

# **HARVESTING AND SOWING**

## **Reflections and testimony on a mathematician's past**

by

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**Presentation of Themes or  
PRELUDE IN FOUR MOVEMENTS**







# 1. En Guise d'Avant- About

□ January 30, 1986

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All that was missing was a foreword to be written, so that I could send *Récoltes et Semailles* to the printer. And I swear I had the best will in the world to write something that would do the job. Something **reasonable** this time. Three or four pages, no more, but well thought-out, to present this enormous "pavé" of over a thousand pages. Something that "hooks" the jaded reader, that makes him or her see that in these unreassuring "more than a thousand pages", there might be things that interest them (or even concern them, who knows?). That's not really my style, the hook, that's not. But I was going to make an exception for once! The "publisher foolish enough to go for it" (to publish this obviously unpublishable monster) had to make ends meet.

And then no, it didn't come. I did my best, though. And not just for one afternoon, as I'd planned, just as quickly as possible. Tomorrow I'll have been at it for three weeks on the dot, the leaves piling up. What's come, that's for sure, is not what one could decently call a "foreword". I've missed it again! You can't do things over again at my age - and I'm not cut out to sell or make people sell. Even when it comes to giving pleasure (to oneself, and to friends. . . ).

What I came up with was a sort of long, commented "walk" through my work as a mathematician. A walk aimed above all at the "layman" - those who "have never understood anything about maths". And for me, too, who had never taken the leisure of such a stroll. One thing led to another, and I found myself bringing out and saying things that until then had always remained unsaid. As chance would have it, these are also the things that I feel are most essential to my work and my oeuvre. There's nothing technical about them. It's up to you to see if I've succeeded in my naïve attempt to "get them across" - an attempt that's surely a little crazy, too. My satisfaction and pleasure would be to have been able to make you feel them. Things that many of my learned colleagues no longer know how to feel. Perhaps they've become too learned and too prestigious. It often makes you lose touch with simple, essential things.

In this "Promenade à travers une oeuvre", I talk a little about my life too. And a little bit, here and here, what we're talking about in *Harvest and Sowing*. I talk about it again, and in greater detail, in the "Letter" (dated □ de May last year) that follows the "Promenade". This Letter was intended for my ex-students p .

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and to my "old friends" in the mathematical world. But there's nothing technical about it either. It can easily be read by any reader interested in learning, through a "first-hand" account, the ins and outs that eventually led me to write *Harvest and Sowing*. Even more than the *Promenade*, it will also give you a taste of a certain atmosphere in the mathematical "big world". And also (like the *Promenade*), of my style of expression, which I'm told is a bit special. And also of the spirit that expresses itself through this style - a spirit that is not appreciated by everyone either.

In the *Promenade* and throughout *Harvest and Sowing*, I talk about **mathematical work**. It's work I know well, first-hand. Most of what I say about it is true, surely, for all creative work, all work of discovery. It's true, at least, for so-called "intellectual" work, that which is mostly done "in the head", and by writing. Such work is marked by the blossoming and blooming of the inner self.

We're also driven by a desire to **understand** the things we're probing. But, to take an example from the opposite end of the spectrum, the passion of love is also an impulse of discovery. It opens us up to "carnal" knowledge, which also renews itself, flourishes and deepens. These two impulses - the one that drives the mathematician at work, let's say, and the one in the lover - are much closer than we generally suspect, or are willing to admit to ourselves. I hope that the pages of *Récoltes et Semailles* will help you to feel this, in your work and in your everyday life.

During the Promenade, I'll be talking mainly about the mathematical work itself. However, I remain virtually silent on the **context** in which this work takes place, and on the **motivations** that come into play outside the work itself. This runs the risk of giving me, or mathematicians or "scientists" in general, a flattering but distorted image. Like "great and noble passion", without any kind of correction. In line, in short, with the great "Myth of Science" (with a capital S, please!). The heroic, "Promethean" myth into which writers and scientists have fallen (and continue to fall) at the drop of a hat. Only historians, perhaps, sometimes resist this seductive myth. The truth is that in the motivations of "the scientist", which sometimes lead him to invest lavishly in

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his work, ambition and vanity play as important and almost universal a role, as in any other profession. It takes on more or less coarse, more or less subtle forms, depending on the person involved. I don't in no way claim to be an exception. I hope that reading my testimonial will leave no doubt on this point.

It's also true that the most devouring ambition is powerless to discover the slightest mathematical statement, or to demonstrate it - just as it is powerless (for example) to "get a hard-on" (in the true sense of the word). Whether you're a man or a woman, what "gives you a hard-on" is not ambition, the desire to shine, to exude power, in this case sexual power - quite the contrary! But it is the acute perception of something strong, very real and very delicate at the same time. You could call it "beauty", and it's one of the thousand faces of this thing. Being ambitious doesn't necessarily mean you can't feel the beauty of a being, or a thing, sometimes. But what's certain is that it's **not** ambition that makes us feel it... .

The man who first discovered and controlled the fire was exactly like you and me. Not at all what we think of as "heroes", "demigods" and so on. Surely, like you and me, he knew the bite of anguish, and the vanity ointment experienced, which makes you forget the bite. But the moment he "knew" the fire, there was neither fear nor vanity. Such is the truth of heroic myth. Myth becomes insipid, it becomes ointment, when it serves to hide from us another aspect of things, just as real and just as essential.

My purpose in *Récoltes et Semailles* was to talk about both aspects - the drive to know, and fear and its vain antidotes. I think I "understand", or at least **know**, the drive and its nature. (Perhaps one day I'll discover, in wonder, just how deluded I've been... .) But as for fear and vanity, and the insidious blocks to creativity that derive from them, I know I haven't got to the bottom of this great enigma. And I don't know if I'll ever get to the bottom of this mystery, in the years I have left to live... .

During the writing of *Récoltes et Semailles*, two images emerged to represent these two aspects of the human adventure. They are **the child** (aka **the worker**), and the **Boss**. In the Promenade we're about to take, we'll be talking almost exclusively about the "child". He's also the one who appears in the subtitle "**L'enfant et la Mère**" ("**The Child and the Mother**"). This name will become clearer, I hope, in the course of the walk.

□ In all the rest of the reflection, it's the Boss on the other hand who mostly takes center stage. He isn't a boss for nothing! In fact, it would be more accurate to say that we're not talking about **a** Boss, but about

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Bosses of competing companies. But it's also true that all bosses are alike in most respects. And when we start talking about Bosses, it also means that there are going to be some "bad guys". In Part I of the reflection ("Fatuity and Renewal", which follows this introductory part, or the "Prelude in Four Movements"), it's mostly me who's "the villain". In the next three parts, it's mostly "the others". Each to his own!

In other words, in addition to profound philosophical reflections and (by no means contrite) "confessions", there will be "vitriolic portraits" (to use the expression of one of my colleagues and friends, who found himself a little mauled... .). Not to mention a number of large-scale "operations". Robert Jaulin<sup>1</sup> assured me (half-jokingly) that in *Récoltes et Semailles* I was doing "the ethnology of the mathematical milieu" (or perhaps the sociology, I couldn't say more). It's flattering, of course, to learn that (without even knowing it) you're doing learned things! It's a fact that, during the "investigation" part of my reflection (and to my chagrin. . .), I saw a good part of the mathematical establishment pass through the pages I was writing, not to mention a number of colleagues and friends of more modest status. And over the last few months, since I sent out the provisional print run of *Récoltes et Semailles* last October, it's been that way again. Decidedly, my testimonial was like a paving stone in the pond. There were echoes in every tone, really (except that of boredom. . .). Almost every time, it was not at all what I would have expected. And there was also a lot of silence, which spoke volumes. Clearly, I had (and still have) a lot to learn about what goes on in people's noggins, among my ex-students and other more or less well-placed colleagues - I mean, about the "sociology of the mathematical milieu"! To all those who have already come to make their contribution to the great sociological work of my old age, I would like to express my gratitude here and now.

Of course, I was particularly sensitive to echoes in warm tones. There were also a few rare colleagues who have shared with me an emotion, or a feeling (unexpressed until now) of crisis, or of degradation□within this mathematical milieu of which they feel a part.

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Outside this milieu, among the very first to give a warm, even emotional, welcome to my testimony, I'd like to mention Sylvie and Catherine Chevalley<sup>2</sup>, Robert Jaulin, Stéphane Deligeorge and Christian Bourgois. If *Récoltes et Semailles* is going to be distributed more widely than the initial provisional print run (intended for a very limited circle), it is above all thanks to them. Thanks, above all, to their shared conviction: that what I had endeavored to grasp and say, had to be said. And that it could be heard in a wider circle than that of my colleagues (who are often sullen, even surly, and not at all disposed to question themselves. . .). So it was that Christian Bourgois didn't hesitate to run the risk of publishing the unpublishable, and Stéphane Deligeorge did me the honor of including my indigestible testimony in the "Epistémé" collection, alongside (for the time being) Newton, Cuvier and Arago. (I couldn't have wished for better company!) To each and every one of you, for your repeated expressions of sympathy and confidence, at a particularly "sensitive" time, I'm happy to express my deepest gratitude.

And here we are, at the start of a Promenade à travers une oeuvre, as an introduction to a journey through a life. A long journey, yes, of a thousand pages or more, and well packed each one. It took me a lifetime to complete this journey, without exhausting it, and more than a year to rediscover it, page after page. Words sometimes

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<sup>1</sup>Robert Jaulin is an old friend. I understand that he finds himself in a somewhat similar situation (as a "white wolf") vis-à-vis the ethnological establishment as I do vis-à-vis the mathematical "beau monde".

<sup>2</sup>Sylvie and Catherine Chevalley are the widow and daughter of Claude Chevalley, the colleague and friend to whom the central part of *Récoltes et Semailles* (ReS III, "La Clef du Yin et du Yang") is dedicated. In several places in this reflection, I mention him and the role he played in my itinerary.

have been reluctant to come, to express all the juice of an experience still evading hesitant comprehension - as ripe, plump grapes piled up in the press seem, at times, to want to evade the force that embraces them. ... But even in those moments when the words seem to jostle and flow, they don't jostle and flow at the drop of a hat. Each one of them has been weighed in passing, or even afterwards, to be carefully adjusted if found too light, or too heavy. So this reflection-testimony-travel is not designed to be read quickly, in a day or a month, by a reader.

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who can't wait for the final word. There are no "final words" or "conclusions" in Récoltes.

and Sowing, any more than there is in my life, or in yours. There is a wine, aged for a lifetime in the barrels of my being. The last glass you drink won't be any better than the first or the hundredth. They're all "the same", and they're all different. And if the first glass is spoiled, so is the whole barrel, so you might as well drink good water (if any), rather than bad wine.

But good wine can't be drunk in a hurry, or at the drop of a hat.

# 2. A walk through a work of art or the Child and Mother

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January 1986

## 2.1. The magic of things

□ When I was a kid, I liked going to school. We had the same teacher to teach us to read and writing, arithmetic, singing (he played a little violin to accompany us), or prehistoric man and the discovery of fire. I don't remember ever being bored at school then. He had the magic of numbers, and the magic of words, signs and sounds. The magic of **rhyme**, too, in songs and little poems. Rhyme seemed to hold a mystery beyond words. And so it was, until one day

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or someone explained to me that there was a simple "trick" to it; that rhyme is simply when you make two consecutive spoken movements end with the same syllable, which then, as if by magic, become **verses**. It was a revelation! For weeks and months on end, at home, where I could find people to talk to, I'd have fun making verses. At one point, I could only speak in rhyme. Fortunately, I got over it. But even today, on occasion, I still write poems - but I don't go looking for the rhyme anymore, unless it comes from me.

At another time, an older friend, who was already in high school, taught me negative numbers. It was another fun game, but one that was quickly exhausted. And then there were crosswords - I spent days and weeks making them, more and more interlocking. This game combined the magic of form with the magic of signs and words. But that passion left me, apparently without a trace.

In high school, first in Germany in the first year, then in France, I was a good student, but not the "brilliant one". I invested myself wholeheartedly in what interested me most, and tended to neglect what interested me less, without worrying too much about the appreciation of the "teacher" concerned. The first year of high school in France, in 1940, I was interned with my mother in the concentration camp at Rieucros near Mende. It was wartime, and we were foreigners - "undesirables", as we called them. But the camp administration kept a close eye on the camp kids, undesirables though they were. We came and went as we pleased.

wanted. I was the oldest, and the only one to go to high school, four or five kilometers away, snow or shine, with makeshift shoes which always took on water.

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I still remember my first "maths composition", when the teacher gave me a bad mark for demonstrating one of the "three cases of equality of triangles". My demonstration wasn't the one in the book, which he followed religiously. However, I knew perfectly well that my demonstration was no more or less convincing than the one in the book, the spirit of which I was following, with the endless traditional "we slide such and such a figure over such and such a figure". Clearly, the man who was teaching me didn't feel capable of judging by his own lights (in this case, the validity of a line of reasoning). He had to refer to an authority, in this case a book. It must have struck me, these dispositions, for me to have remembered this little incident. Subsequently, and to this day, I have had ample opportunity to see that such provisions are by no means the exception, but the almost universal rule. There's a lot to be said for this - a subject I touch on more than once, in one form or another, in Harvest and Sowing. But even today, whether I like it or not, I feel disconcerted every time I come across it again. . .

During the last years of the war, while my mother remained interned in the camp, I was in a "Secours Suisse" children's home for refugee children in Le Chambon sur Lignon. Most of us were Jewish, and when we were warned (by the local police) that the Gestapo would be rounding us up, we would go and hide in the woods for a night or two, in small groups of two or three, without really realizing that our lives were at stake. The region was full of Jews hiding in the Cévennes, and many survived thanks to the solidarity of the local population.

What struck me most at the "Collège Cévenol" (where I was brought up) was how little interest my fellow students took in what they were learning. As for me, I'd devour the textbooks at the start of the school year, thinking that this time we'd finally learn something **really** interesting; and the rest of the year I'd use my time as best I could, while the planned curriculum was churned out inexorably, term after term. But we had some really nice teachers. The natural history teacher, Mr. Friedel, had a remarkable human and intellectual quality. But he was incapable of "cracking down", and would get hounded to death, to the point where towards the end of the year, it became impossible to follow any more, his voice impotent

covered by the general hustle and bustle. Maybe that's why I didn't become a biologist!

□ I spent quite a bit of my time, even during lessons (shhh. . . ), doing math problems.

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Soon, the ones in the book weren't enough for me. Maybe because they tended to resemble each other a little too much; but mostly, I think, because they were falling out of the sky a little too much, just like that, without saying where they came from or where they were going. They were the book's problems, not mine. Yet there was no shortage of really natural questions. Thus, when the lengths  $a$ ,  $b$ ,  $c$  of the three sides of a triangle are known, the triangle is known (apart from its position), so there must be an explicit "formula" to express, for example, the area of the triangle as a function of  $a$ ,  $b$ ,  $c$ . The same goes for a tetrahedron where we know the length of all six edges - what's the volume? That one I think was a bit of a struggle, but I eventually got the hang of it. In any case, when something "got me", I didn't count the hours or days I spent on it, even if it meant forgetting everything else! (And that's how it is even now... )

What I found most unsatisfying in our maths books was the absence of any serious definition of the notion of length (of a curve), area (of a surface) or volume (of a solid). I promised myself to fill this gap as soon as I could. I spent most of my energy on it between 1945 and 1948, when I was a student at the University of Montpellier. I wasn't happy with the courses I was taking. Without ever having said it to myself, I must have had the impression that the teachers were just repeating their books, just like my first maths teacher at the lycée in Mende. So I only went to university from time to time, to keep up to date with the endless "program". The books were enough, but it was also clear that they didn't answer any of my questions. In fact, they didn't even **see** them, any more than my high school books did. As long as they gave recipes for calculating lengths, areas and volumes, using single, double and triple integrals (dimensions greater than three were cautiously avoided. . . ), the question of giving an intrinsic definition didn't seem to arise, either for my teachers or for the authors of the textbooks.

From the limited experience I had at the time, it might well have seemed that I was the only being in the world... gifted with a curiosity for mathematical questions. At any rate, this was my unspoken conviction during those years spent in complete intellectual solitude, which did not weigh heavily on me<sup>1</sup>. To tell the truth, I I don't think I ever thought, in all that time, of □ proving the question whether or not I was indeed the only one. p. P4 no one in the world likely to be interested in what I was doing. My energy was sufficiently absorbed in meeting the challenge I had set myself: to develop a theory that would satisfy me fully.

There was no doubt in my mind that I couldn't fail to get to the bottom of things, if only I took the trouble to scrutinize them, putting down in black and white what they were telling me as I went along. The intuition of the **volume**, let's say, was irrefutable. It could only reflect a **reality**, elusive for the moment, but perfectly reliable. It was this reality that I simply had to grasp.

- a little, perhaps, like that magical reality of "rhyme" had been grasped, "understood" one day.

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<sup>1</sup>Between 1945 and 1948, I lived with my mother in a small hamlet about ten kilometers from Montpellier, Mairargues (by Vendargues), lost in the middle of the vineyards. (My father had disappeared in Auschwitz in 1942.) We lived meagerly on my meager student grant. To make ends meet, I harvested grapes every year, and after the harvest, I made grape wine, which I managed to sell as best I could (in contravention, it seems, of current legislation. . . ) In addition, there was a garden which, without ever having to work it, provided us with an abundance of figs, spinach and even (towards the end) tomatoes, planted by a complaisant neighbor in the middle of a sea of splendid poppies. It was a good life - but sometimes a little tight around the edges, when it came to replacing spectacle frames or a pair of worn-out shoes. Fortunately, my mother, weakened and ill from her long stay in the camps, was entitled to free medical assistance. We would never have been able to afford a doctor...

When I started, at the age of seventeen and fresh out of high school, I thought it would only take a few weeks. I stayed with it for three years. I even found a way of missing an exam at the end of my second year of university - spherical trigonometry (in the "in-depth astronomy" option, sic), because of a silly error in numerical calculation. (I was never very good at calculus, it has to be said, once I got out of high school... ) That's why I had to stay another third year in Montpellier to finish my degree, instead of going to Paris straight away - the only place, I was assured, where I'd have the opportunity to meet people in the know about what was considered important in maths. My informant, Monsieur Soula, also assured me that the last remaining problems in maths had been solved, twenty or thirty years ago, by a man called Lebesgue. He was said to have developed a theory of measurement and integration, which put an end to mathematics.

Monsieur Soula, my "calculus diff" teacher, was a kindly man, well-disposed towards me. I don't think he convinced me though. There must already have been in me the prescience that mathematics is a thing unlimited in extent and depth. Does the sea have an "end point"? The fact remains that at no time was I touched by the thought of unearthing the book by this Lebesgue that Mr. Soula had told me about, and which he must never have held in his hands either. In my mind, there was nothing in common between what a book might contain, and the work I was doing, in my own way, to satisfy my curiosity about such and such things that had intrigued me.

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## 2.2. The importance of being alone

When I finally made contact with the mathematical world in Paris, a year or two later, I learned, among many other things, that the work I'd been doing in my corner with the means at hand was (more or less) what was well known to "everyone", under the name of Lebesgue measure and integral theory. In the eyes of the two or three seniors to whom I spoke about this work (or even showed a manuscript), it was as if I'd simply wasted my time, rehashing what was already known. I don't remember being disappointed. At that point, the idea of getting "credit", or even approval, or simply the interest of others, for the work I was doing, must still have been foreign to me. Not to mention the fact that my energies were well and truly diverted to familiarizing myself with a completely different environment, and above all, to learning what was considered in Paris to be the mathematician's B.A.BA.<sup>2</sup>.

Yet, looking back on those three years now, I realize that they were by no means gas-plundered. Without even realizing it, I learned in solitude what is the essence of a mathematician's profession - what no master can truly teach. Without ever having had to say it to myself, without ever having had to meet anyone with whom to share my thirst for understanding, I nevertheless knew, "from my gut" I'd say, that I was a mathematician: someone who "does" maths, in the full sense of the word - like one "does" love. Mathematics had become my mistress, always welcoming my desire. These years of solitude laid the foundation for a confidence that has never been shaken - not by the discovery (landing in Paris at the age of twenty) of the full extent of my ignorance and the immensity of what I had to learn; nor (more than twenty years later) by the turbulent episodes of my departure without return from the mathematical world; nor, in recent years, by the often rather crazy episodes of a certain "Burial".

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<sup>2</sup>I give a short account of this rough transition period in the first part of Harvest and Sowing (ReS I), in the section entitled "The welcome stranger" (n° 9).

(anticipated and without <sup>□</sup>burrs) of my person and my work, orchestrated by my closest companions p. P6 of yesteryear...

To put it another way: in those crucial years, I learned to **be alone**<sup>3</sup>. By this I mean approaching the things I want to know by my own lights, rather than relying on the ideas and consensuses, expressed or tacit, that would come to me from a more or less extended group of which I would feel a member, or which for any other reason would be invested with authority for me. Mute consensuses had told me, in high school as in university, that there was no need to question the very notion of "volume", presented as "well known", "obvious", "unproblematic". I had ignored it, as a matter of course - just as Lebesgue, a few decades earlier, had had to **ignore it**. It is in this act of "going beyond", of being oneself in short and not simply the expression of the consensus that makes law, of not remaining locked inside the imperative circle they set for us - it is above all in this solitary act that "**creation**" is found. Everything else comes on top.

Later, in the world of mathematicians that welcomed me, I had the opportunity to meet many people, both older and younger, more or less my own age, who were obviously much brighter, much more "gifted" than me. I admired them for the ease with which they learned new notions, as if by playing with them, and juggled with them as if they'd known them since the cradle.

- while I felt heavy and clumsy, slogging my way like a mole through a shapeless mountain of things that it was important (I was assured) for me to learn, and of which I felt incapable of grasping the ins and outs. In fact, I was in no way a brilliant student, passing prestigious competitive exams with flying colors and assimilating prohibitive curricula in no time at all.

In fact, most of my brighter classmates went on to become competent and successful mathematicians. putés. And yet, looking back over thirty or thirty-five years, I can see that they haven't left mathematics behind. of our time□a really deep imprint. They did things, beautiful things sometimes, in a p. P7 context that they would never have thought of touching. They were unknowingly trapped in those invisible, imperious circles that delimit a Universe in a given environment and at a given time. To cross them, they would have had to rediscover within themselves the capacity that was theirs at birth, just as it was mine: the capacity to be alone.

Small children have no trouble being alone. He's solitary by nature, even if he doesn't mind the occasional company, and knows how to claim his mother's totosse when it's time to drink. And he knows, without having to tell himself, that the pod is for him, and that he **knows how to** drink. But we've often lost touch with our inner child. And we constantly miss out on the best, without deigning to see it. ...

If in *Récoltes et Semailles* I'm addressing someone other than myself, it's not an "audience". I'm addressing you, the reader, as a **person**, and a person **alone**. It's to the one in you who knows how to be alone, to the child, that I would like to speak, and to no one else. The child is often far away, I know. He's seen it all, and he's seen it a long time ago. He's hidden himself away God knows where, and it's not easy, often, to get to him. You'd swear he's been dead forever, that he never existed - and yet, I'm sure he's out there somewhere, alive and well.

And I also know what the **sign** is that I'm being heard. It is when, over and above all the differences in

<sup>3</sup>This formulation is a bit of a misnomer. I never had to "learn to be alone", for the simple reason that I never **unlearned**, during my childhood, this innate ability that was in me at birth, as it is in everyone. But those three years of solitary work, in which I was able to give my best to myself, according to my own spontaneous criteria of exigency, confirmed and rested in me, in my relationship this time to mathematical work, a foundation of confidence and trust. quiet assurance, which owed nothing to the consensus and fashions that rule the day. I refer to this again in the note "Racines et solitude" (ReS IV, n° 171<sub>3</sub>, in particular p. 1080).

culture and destiny, what I say about myself and my life finds an echo and resonance in you; when you also find in it **your own life**, your own experience of yourself, in a light perhaps you hadn't paid attention to before. It's not a question of "identification", with something or someone far removed from you. But perhaps, in a small way, you're rediscovering your own life, what's **closest to** you, through the rediscovery I'm making of mine, in the pages of Harvest and Sowing and even in these pages I'm writing today.

## 2.3. The inner adventure - or myth and testimony

Above all, Harvest and Sowing is a **reflection** on myself and my life. At the same time, it is also a **testimony**, in two ways. It's a testament to my **past**, **which** carries the weight of my past.

p. P8 of reflection. But at the same time it's also a testimony to the most immediate **present** - to the very moment when I'm writing, and when the pages of Harvest and Sowing are born ☐ over the course of hours, nights and days. These pages are the faithful witnesses of a long meditation on my life, as it has really gone on (and is still going on at this very moment. . . ).

These pages have no literary pretensions. They are a **document** about myself. I have only allowed myself to touch them (for occasional stylistic alterations, in particular) within very narrow limits<sup>4</sup>. If it has any pretension, it's only that of being true. And that's a lot.

This document, moreover, is not an "autobiography". You won't learn my date of birth (which would only be of interest for astrological charting), or the names of my mother and father or what they did for a living, or the names of my wife and other women who were important in my life, or the names of the children born of these loves, and what they did with their lives. It's not that these things weren't important in my life, and remain important even now. But as this self-reflection began and continued, at no point did I feel prompted to engage in even the slightest description of the things I brush up against here and there, let alone conscientiously line up names and numbers. At no time did it seem to me that this could add anything to the purpose I was pursuing at the time. (Whereas in the few pages that precede this one, I have been led, as if in spite of myself, to include perhaps more material details about my life than in the thousand pages that follow. ... )

And if you ask me what this "purpose" is that I'm pursuing for a thousand pages, I'll answer: it's to tell the story, and thereby **discover**, of **the inner adventure** that was and is my life. This narrative-testimony of an adventure continues at the same time on the two levels I've just mentioned. There's the exploration of an adventure in the past, its roots and origins in my childhood. And there's the continuation and renewal of this "same" adventure, as I write, moment by moment and day by day. Récoltes et Semailles, in spontaneous response to a violent challenge from the outside world<sup>5</sup>.

☐ External facts provide food for thought, only insofar as they prompt and provoke  
p. P9 a twist in the inner adventure, or help to illuminate it. And the burial and plundering of my mathematical work, which will be discussed at length, was such a provocation. It aroused in me a mass outpouring of powerful egotistical reactions, and at the same time revealed to me the deep, unacknowledged links that continue to bind me to the work that grew out of me.

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<sup>4</sup>This means that any errors (material, perspective, etc.) are not corrected in the first draft, but rather in the footnotes, or during a later "review" of the situation under examination.

<sup>5</sup>For details of this "violent interpellation", see "Letter", especially sections 3 to 8.



It's true that the fact that I'm one of the "math majors" isn't necessarily a reason (let alone a good one) for you to be interested in my particular "adventure" - nor is the fact that I've had trouble with my colleagues, after changing my environment and lifestyle. In fact, there's no shortage of colleagues and even friends who find it ridiculous to go public (as they put it) with their "states of mind". What counts are the "results". The "soul", that is to say, that part of us which **experiences** the "production" of these "results", or the repercussions of all kinds (both in the life of the "producer", and in that of his fellow human beings), is the object of scorn, even open derision. While this attitude is intended as an expression of "modesty", I see it as a sign of flight, and a strange derangement, promoted by the very air we breathe. Of course, I don't write for those struck by a kind of latent self-contempt that makes them disdain the best I have to offer. A contempt for what truly makes up his **own life**, and for what makes up mine: the superficial and profound, coarse and subtle movements that animate the psyche, this very "**soul**" that lives experience and reacts to it, that freezes or blossoms, that withdraws or learns. . .

The story of an inner adventure can only be told by the person living it, and by no one else. But even if the story is intended only for oneself, it rarely slips into the rut of **myth-building**, with the narrator as the hero. Such a myth is born, not from the creative imagination of a people and a culture, but from the vanity of one who dares not assume a humble reality, and who likes to substitute a construction, the work of his own mind. But a **true** account (if there is such a thing) of an adventure as it was really lived, is a precious thing. And this, not because of the prestige that (rightly or wrongly) surrounds the narrator, but simply because **it exists**, with its quality of truth. Such a testimony is precious, whether it comes from a man of notoriety or even fame, or from a small employee with no future and a family, or from a common criminal.

If such a story has any virtue for others, it is above all in bringing them face to face with themselves, through this testimony.

of another's experience. Or also (to put it another way) to erase perhaps in him (and if only

p. P10

than the space of time that a reading lasts) this contempt in which he holds his **own adventure**, and this "soul" which is its passenger and captain... .

## 2.4. The painting of

Talking about my past as a mathematician, and then discovering (as if unwillingly) the twists and turns of the gigantic Burial of my work, I was led, without having sought it, to paint a picture of a certain milieu and a certain era - an era marked by the decomposition of some of the values that gave meaning to men's work. It's the "tableau de moeurs" aspect, painted around a "diversionary event" undoubtedly unique in the annals of "Science". What I said earlier makes it clear, I think, that you won't find a "dossier" on a certain unusual "case" in Harvest & Sow, just to bring you up to speed quickly. A friend of mine who was looking for the file missed out on almost everything that makes up the substance and flesh of Harvest and Sowing.

As I explain in much greater detail in the Letter, the "investigation" (or "tableau de moeurs") continues mainly in Parts II and IV, "L' Enterrement (1) - ou la robe de l' Empereur de Chine" and "L' Enterrement (3) - ou les Quatre Opérations". Over the pages, I stubbornly unearth, one after the other, a multitude of juicy facts (to say the least), which I try as best I can to "fit in" as I go along. Little by little, these facts come together to form an overall picture that gradually emerges from the mists,

in increasingly vivid colors, with sharper and sharper outlines. In these day-by-day notes, the "raw facts" that have just appeared are inextricably mixed with personal reminiscences, and with comments and reflections of a psychological, philosophical, even (occasionally) mathematical nature. That's just the way it is, and there's nothing I can do about it!

Based on the work I've done, which has kept me on the edge of my seat for over a year, putting together a dossier, in the style of "investigative conclusions", should represent additional work of the order of a few hours or a few days, depending on the curiosity and requirements of the interested reader. There was a time when I did try to put it together. It was when I started writing a note that was to be called "The Four Operations"<sup>6</sup>. And then no, there was nothing to do. I couldn't do it! That's not my style of expression, ☐decidedly, and on my old age less than ever. And I now estimate, with *Récoltes et Semailles*, in  
p. P11 to have done enough for the benefit of the "mathematical community", to leave without remorse to others (if there are any among my colleagues who feel concerned) the task of putting together the necessary "dossier".

## 2.5. The heirs and the builder

It's time for me to say a few words here about my mathematical work, which has taken and still takes (to my own surprise) an important place in my life. More than once in *Récoltes et Semailles* I come back to this work - sometimes in a way that is clearly intelligible to everyone, and at other times in somewhat technical terms<sup>7</sup>. These latter passages will largely go "over the head" not only of the "layman", but even of the fellow mathematician who might not be more or less "in the know" about the maths involved. Of course, you're welcome to skip over any passages that strike you as a little too "advanced" in nature. You can also skim through them, and perhaps catch a glimpse of the "mysterious beauty" (as a non-mathematician friend wrote to me) of the world of mathematical things, popping up like so many "strange, inaccessible islands" in the vast, shifting waters of reflection. ...

As I said earlier, most mathematicians tend to confine themselves to a conceptual framework, to a "**Universe**" fixed once and for all - essentially, the one they found "ready-made" when they studied. They're like the heirs to a big, beautiful, fully-installed house, with its living rooms and kitchens and workshops, and its cookware and all kinds of tools, with which there's, my goodness, plenty to cook and tinker with. How this house has been built over the generations, and how and why certain tools (and not others. . . ) have been designed and fashioned, why rooms are arranged and laid out in such and such a way here, and in such and such a way there - these are questions that these heirs would never dream of asking themselves. This is the "Universe", the "given" in which we have to live, period! Something that seems large (and we're usually far from having seen all its parts), but at the same time **familiar**, and above all: **unchanging**. When they're busy, it's to maintain

and embellish a heritage: repair a wobbly piece of furniture, plaster a facade, sharpen a tool, or even sometimes, for the most enterprising, make a new piece of furniture from scratch in the workshop. And it happens, ☐when they

They'll do their utmost to ensure that the furniture is beautiful, and that the whole house looks better for it.  
p. P12 Even more rarely, one of them will think of making some modification to one of the tools on hand, or even, under repeated and insistent pressure of need, of imagining and making a new one. This

<sup>6</sup>The planned note to fi nieventually broke out into Part IV (of the same name "The Four Operations") of *Harvest and Sowing*, comprising 70 notes spanning well over four hundred pages.

<sup>7</sup>In addition to mathematical insights into my past work, there are also passages here and there containing new mathematical developments. The longest is "Les cinq photos (cristaux et  $D$ -Modules)" in *ReS IV*, note n° 171 (ix).

In doing so, he's hardly likely to apologize for what he feels is a kind of infringement of the piety due to family tradition, which he feels he's upsetting with an unusual innovation.

In most rooms of the house, windows and shutters are carefully closed - no doubt for fear of a wind blowing in from elsewhere. And when the beautiful new furniture, one here and the other there, not to mention the offspring, start to clutter up the now narrow rooms and invade even the corridors, none of these heirs will want to realize that their cozy, familiar Universe is starting to feel a little cramped around the edges. Rather than come to terms with this, they'll prefer to squeeze and wedge their way as best they can between a Louis XV sideboard and a rattan rocking chair, between a snotty toddler and an Egyptian sarcophagus, or, in desperation, climb as best they can over a crumbling heap of chairs and benches... ..

The little picture I've just painted is not peculiar to the world of mathematicians. It illustrates inveterate and immemorial conditioning, which can be found in all walks of life and in all spheres of human activity, and this (as far as I know) in all societies and at all times. I've already alluded to this, and I don't claim to be free of it myself. As my testimony will show, the opposite is true. It's just that, at the relatively limited level of intellectual creative activity, I've been relatively unaffected<sup>8</sup> by this conditioning, which we might call "cultural blindness" - the inability to see (and move) outside the "Universe" fixed by the surrounding culture.

As for me, I feel part of the lineage of mathematicians whose spontaneous vocation and joy is constantly building new houses<sup>9</sup>. Along the way, they can't help but also invent and de□façonner au fur et à mesure tous les outils, ustensiles, meubles et instruments requis, tant pour construire

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lap.

house from foundation to ridge, than to provide an abundance of space for future kitchens and workshops, and to set up the house for living and comfort. And yet, once everything has been laid, right down to the last oak panel and stool, it's rare for the workman to linger long in these places, where every stone and rafter bears the trace of the hand that worked and laid it. His place is not in the tranquility of ready-made worlds, however welcoming and harmonious they may be - whether they have been arranged by his own hands, or those of his predecessors. Other tasks are already calling him to new sites, under the imperious impulse of needs he is perhaps the only one to feel clearly, or (more often still) anticipating needs he is the only one to sense. His place is in the open air. He's a friend of the wind, and is not afraid to work alone, for months and years and, if need be, for a lifetime, unless a welcome relief comes to the rescue. He has only two hands like everyone else, to be sure - but two hands that always know what they have to do, that don't shy away from the biggest or the most delicate tasks, and that never tire of getting to know and reacquaint themselves with the innumerable things that constantly call out to them. Two hands is not much, perhaps, for the World is infinite. They can never exhaust it! And yet, two hands are a lot... .

I'm not much of a history buff, but if I had to name mathematicians in this lineage, Galois and Riemann (in the last century) and Hilbert (at the beginning of the present century) spontaneously come to mind. If I were to look for a representative among the elders who welcomed me into the mathematical world<sup>10</sup>, Jean Leray's name would come to mind first, even though my contacts with him are very long.

<sup>8</sup>I see the main reason for this in a certain favorable climate that surrounded my childhood until the age of five. On this subject, see the note "Innocence" (ReS III, n° 107).

<sup>9</sup>This archetypal image of the "house" to be built surfaces and is formulated for the first time in the note "Yin le Servant, and the new masters" (ReS III, n° 135).

<sup>10</sup>I talk about these beginnings in the section "The welcome stranger" (ReS I, n° 9).

although they have remained episodic<sup>11</sup>.

p. P14

□I've just sketched out two broad portraits: that of the "homebody" mathematician who is content to be of maintaining and embellishing a heritage, and that of the pioneer-builder<sup>12</sup>, who can't help but constantly cross those "invisible and imperious circles" that delimit a Universe<sup>13</sup>. We might also call them, by names that are a little cookie-cutter but suggestive, the "conservatives" and the "innovators". Both have their *raison d'être* and their role to play, in the same collective adventure continuing over generations, centuries and millennia. In a period of scientific or artistic blossoming, there is no opposition or antagonism between these two temperaments<sup>14</sup>. They are different and they complement each other, just as dough and leaven complement each other.

Between these two extreme (but by no means inherently opposed) types, there is of course a whole range of temperaments in between. A "homebody" who wouldn't dream of leaving a familiar dwelling, let alone going to the trouble of building another one God knows where, will nevertheless not hesitate, when things are getting decidedly cramped, to put his hand to the trowel to fit out a cellar or attic, raise a floor or even, if need be, add to the walls some new outbuilding of modest proportions<sup>15</sup>. Although he's not a builder at heart, he often looks with a sympathetic eye, or at the very least, with an eye for the future.

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at least without secret disquiet or disapproval, another who had shared the same dwelling with him, and here he is, laboring to gather beams□and stones in some impossible outback, with the airs of one who would see already a palace...

## 2.6. Point of view and vision

But let me come back to myself and my work.

If I have excelled in the mathematician's art, it is less through skill and perseverance in solving the problems bequeathed to me by my predecessors, than through that natural propensity in me which pushes me to see **questions**, obviously crucial, that nobody had seen, or to draw out the "**right notions**" that were missing (without anyone often realizing it, before the new notion appeared),

<sup>11</sup>Nevertheless, following in the footsteps of H. Cartan and J.P. Serre, I was one of the main users and promoters of one of the great innovative notions introduced by Leray, that of beam, which has been an essential tool throughout my work as a geometer. It also provided me with the key to broadening the notion of (topological) space into that of topos, discussed below.

Leray differs from my portrait of the "builder", it seems to me, in that he doesn't seem inclined to "build houses from the foundation up". Rather, he could not help but lay vast foundations, in places no one else would have thought of, while leaving it to others to finish them and build on them, and, once the house was built, to move in (if only for a while). ...

<sup>12</sup>I've just surreptitiously added two male-sounding qualifiers ("builder" and "pioneer"), which express very different aspects of the discovery drive, and of a more delicate nature. than these names can evoke. This is what will emerge from the rest of this walk-reflection, in the stage "Discovering the Mother - or the two sides" (n° 17).

<sup>13</sup>At the same time, moreover, and unintentionally, he assigns to this ancient Universe (if not for himself, then at least for his leave-holders) the role of the "new", the "old", the "new", the "new", the "new". nères less mobile than it) new limits, in new circles that are certainly wider, but just as invisible and just as imperious as those they have replaced.

<sup>14</sup>This was particularly the case in the mathematical world, during the period (1948-1969) of which I was a direct witness, when I myself was part of that world. After I left in 1970, there seems to have been a kind of wide-ranging reaction, a sort of "consensus of disdain" for "ideas" in general, and more particularly, for the great innovative ideas I had introduced.

<sup>15</sup>Most of my "elders" (mentioned, for example, in "Une dette bienvenue", Introduction, 10) correspond to this intermediate temperament. I'm thinking in particular of Henri Cartan, Claude Chevalley, André Weil, Jean-Pierre Serre and Laurent Schwartz. With the possible exception of Weil, they all gave a "sympathetic eye", without "secret concern or disapproval", to the solitary adventures they saw me embarking on.

as well as the "**right statements**" that nobody had thought of. Quite often, notions and statements fit together so perfectly that there can be no doubt in my mind that they are correct (apart from a few alterations, at most) - and often then, when it's only a "work in progress" intended for publication, I dispense with going any further, and taking the time to work out a demonstration which quite often, once the statement and its context are well seen, can be little more than a matter of "craft", not to say routine. There are countless things that demand attention, and it's impossible to follow the call of each one to its conclusion! Nevertheless, the propositions and theorems I have duly proved in my written and published work number in the thousands, and I think I can say that, with very few exceptions, they have all become part of the common heritage of things commonly accepted as "known" and commonly used in mathematics.

But even more than the discovery of new questions, notions and statements, my particular genius lies in the discovery of fruitful **points of view**, which constantly lead me to introduce, and more or less develop, entirely new **themes**. It seems to me that this is the most essential contribution I have made to the mathematics of my time. To tell the truth, the innumerable questions, notions and statements I've just mentioned only make sense to me in the light of such a "point of view" - or rather, they **emerge** spontaneously from it, with the force of evidence; in the same way that a light (even a diffuse one) that emerges in the dark of night, seems to bring out of nothingness those more or less blurred or sharp contours that it suddenly reveals to us. Without this light uniting them in a common beam, the ten or hundred or thousand questions, notions, statements would appear to be a motley, amorphous collection of "mental gadgets", each isolated from the next.

one from the other□ - and not as parts of a **Whole** which, to remain perhaps invisible, still evading p . P16 in the recesses of the night, is no less clearly prescient.

The fertile point of view is the one that reveals to us, like so many living parts of the same Whole that encompasses them and gives them meaning, those burning questions that no one had sensed, and (as if in response to these questions) those so natural notions that no one had thought to draw out, and finally those statements that seem to flow from source, and that no one certainly risked asking, as long as the questions that gave rise to them, and the notions that enable them to be formulated, had not yet appeared. Even more than what we call "key theorems" in mathematics, it is the fertile points of view that are, in our art<sup>16</sup>, the most powerful tools of discovery - or rather, they are not tools, but the very eyes of the researcher who, passionately, wants to know the nature of mathematical things.

Thus, the fruitful point of view is none other than that "eye" which both makes us **discover**, and makes us **recognize the unity** in the multiplicity of what is discovered. And this unity is truly the very life and breath that connects and animates these multiple things.

But as its very name suggests, a "point of view" in itself remains fragmentary. It reveals **one aspect of** a landscape or panorama, among a multiplicity of equally valid, equally "real" ones. It is to the extent that complementary points of view of the same reality are combined, that our "eyes" are multiplied, that our gaze penetrates further into the knowledge of things. The richer and more complex the reality we wish to know, the more important it is to have several "eyes"<sup>17</sup> to apprehend it in all its breadth and finesse.

And it sometimes happens that a cluster of converging viewpoints on the same vast landscape, through the

<sup>16</sup>This is certainly not the case in "our art" alone, but (it seems to me) in all work of discovery, at least when this is at the level of intellectual knowledge.

<sup>17</sup>Every point of view requires its own language to express it. Having several "eyes" or "points of view" to apprehend a situation also means (in mathematics at least) having several different languages to define it.

by virtue of this, by enabling us to grasp **the One** through the many, gives shape to a new thing; to a thing that surpasses each of the partial perspectives, in the same way that a living being surpasses □ each of its limbs and organs. This new thing can be called a **vision**. The vision unites the already known points of view that embody it, and reveals to us others hitherto ignored, just as the fruitful point of view makes us discover and apprehend as part of the same Whole, a multiplicity of new questions, notions and statements.

To put it another way: vision is to the points of view from which it appears to emerge and which it unites, as clear, warm daylight is to the various components of the solar spectrum. A vast and profound vision is like an inexhaustible source, made to inspire and enlighten the work not only of the man in whom it was born one day and who made himself its servant, but that of generations, fascinated perhaps (as he was himself) by those distant limits it gives us a glimpse of... .

## 2.7. The "big idea" - or trees and the forest

The so-called "productive" period of my mathematical activity, i.e. the one attested by publications in due form, stretches from 1950 to 1969, i.e. over twenty years. And for twenty-five years, between 1945 (when I was seventeen) and 1969 (when I was about forty-two), I invested practically all my energy in mathematical research. An inordinate investment, to be sure. I paid for it with a long spiritual stagnation, with a progressive "thickening", which I'll have more than one occasion to evoke in the pages of *Récoltes et Semailles*. Yet, within the limited scope of a purely intellectual activity, and through the blossoming and maturation of a vision restricted to the world of mathematical things alone, these were years of intense creativity.

During this long period of my life, almost all my time and energy was devoted to what is known as "**piecework**": the painstaking work of shaping, assembling and honing, required to build from scratch the houses that an inner voice (or demon. . . ) enjoined me to build, according to a master plan that it whispered to me as the work progressed. Taken up with the tasks of the "trade": those of stonemason, bricklayer, carpenter, plumber, joiner and cabinetmaker - I rarely took the time to note down in black and white, even if only in broad strokes, the details of my work.

master-plan invisible to all (as it later appeared. . . ) except to me, who over the days, months and years guided my hand with somnambulistic surety<sup>18</sup>. It □ must be said that the piecework, in which

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<sup>18</sup>The image of the "sleepwalker" was inspired by the title of Koestler's remarkable book "Les somnambules" (Calman Lévy), presenting an "Essay on the history of conceptions of the Universe", from the origins of scientific thought to Newton. One of the aspects of this history that struck Koestler, and which he highlights, is how often the path from a certain point in our knowledge of the world, to some other point that (logically and with hindsight) seems very close, takes sometimes the most academic detours, which seem to defy sound reason ; and yet, through these thousand and one detours that seem destined to lead them astray forever, and with a "sleepwalker's certainty", men who set out in search of the "keys" to the Universe stumble, as if in spite of themselves and often without even realizing it, upon other "keys" that they were far from foreseeing, and which turn out to be "the right ones".

From what I've been able to observe around me, in terms of mathematical discovery, these far-reaching detours in the path of discovery are the work of some large-scale researchers, but by no means all of them. This may be due to the fact that, over the last two or three centuries, research in the natural sciences, and even more so in mathematics, has been freed from the imperative religious or metaphysical presuppositions of a given culture and era, which have been particularly powerful brakes on the unfolding (for better or for worse) of a "scientific" understanding of the Universe. It's true, however, that some of the most fundamental and obvious ideas and notions in mathematics (such as displacement, group, the number zero, literal calculus, the coordinates of a point in space, the notion of set, or that of topological "form", not to mention negative numbers and complex numbers) took millennia to appear. These are all eloquent signs of this inveterate "block", deeply implanted in the psyche, against the conception of totally new ideas, even in cases where these are childishly simple and seem

I liked to take great care with my work, which was by no means to my displeasure. What's more, the mathematical mode of expression professed and practised by my elders gave pre-eminence (to say the least) to the technical aspect of the work, and hardly encouraged "digressions" that would have dwelt on "motivations"; or even those that would have pretended to conjure up some perhaps inspiring image or vision from the mists, but which, because it has not yet been embodied in tangible constructions of wood, stone or pure, unadulterated cement hard, was more akin to dreamy shreds, than to the diligent, conscientious work of the craftsman.

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In quantitative terms, my work during these years of intense productivity took the form above all of some twelve thousand pages of publications, in the form of articles, monographs or seminars<sup>19</sup>, and of hundreds, if not thousands, of new notions, which entered the common heritage, with the very names I had given them when I had identified them<sup>20</sup>. In the history of mathematics, I believe I'm the one who has introduced the greatest number of new notions into our science, and at the same time, the one who has been led, by this very fact, to invent the greatest number of new names, to express these notions with delicacy, and as suggestively as I could.

Of course, these "quantitative" indications only give a rough idea of my work, and miss the real soul, life and vigor of it. As I wrote earlier, the best thing I've contributed to mathematics are the new "**points of view**" that I **first glimpsed**, and then patiently **drew out** and more or less developed. Like the notions I've just been talking about, these new points of view, introduced into a vast multiplicity of very different situations, are themselves almost innumerable.

There are, however, some points of view that are broader than others, and that alone give rise to and encompass a multitude of partial points of view, in a multitude of different particular situations. Such a point of view can rightly be called a "**big idea**". By virtue of its own fecundity, such an idea gives rise to a teeming progeny of ideas, all of which inherit its fecundity, but most (if not all) of which are less far-reaching than the mother idea.

As for **expressing** a great idea, or "saying" it, this is more often than not almost as delicate as its very conception and slow gestation in the person who conceived it - or to put it better, this laborious gestation and formation work **is** precisely that which "expresses" the idea: the work that consists in patiently, day after day, clear away the mists that surround it at birth, and gradually little by little to give it tangible form, in a picture that grows richer, firmer and more refined with each passing week,

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months and years. Simply **naming** the idea, with some striking formula, or with more or less technical keywords, can take a few lines, or even a few pages - but few people who don't already know it well will be able to hear this "name" and recognize a face in it. And when the idea arrives

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impose themselves with the force of evidence, for generations, even millennia. ...

Returning to my own work, I have the impression that in it the "mistakes" (more numerous, perhaps, than among most of my colleagues) are confined exclusively to points of detail, generally quickly spotted by myself. They are mere "accidents along the way", of a purely "local" nature, with no serious impact on the validity of the essential intuitions concerning the situation under examination. On the other hand, at the level of the ideas and the great guiding intuitions, it seems to me that my work is free of any "miss", incredible as it may seem. It's this never-failing ability to grasp, at any given moment, if not the ultimate results of an approach (which more often than not remain hidden from view), then at least the most fertile directions that offer themselves to lead me straight to the essential things - it's this ability that brought back Koestler's image of the "somnambulist".

<sup>19</sup>From the 1960s onwards, some of these publications were written in collaboration with colleagues (especially J. Dieudonné) and students.

<sup>20</sup>The most important of these notions are reviewed in the Thematic Outline, and in the accompanying History Commentary, which will be included in volume 4 of Reflexions. Some of the names were suggested to me by friends or students, such as the term "smooth morphism" (J.Dieudonné) or the panoply of "site, field, sheaf, link", developed in Jean Giraud's thesis.

in full maturity, perhaps a hundred pages will suffice to express it, to the full satisfaction of the worker in whom it was born - as it may also be that ten thousand pages, laboriously worked and weighed, will not suffice<sup>21</sup> .

And in either case, among those who, in order to make it their own, have taken note of the work that at last presents the idea in full bloom, like a spacious forest that has grown there on a deserted moor - it's a safe bet that many will see all these vigorous, slender trees and make use of them (some to climb, some to draw beams and planks from them, some to light fires in their fireplaces...). ), but only a few will know how to see the forest...

## 2.8. Vision - or twelve themes for harmony

Perhaps we could say that the "big idea" is the point of view that not only reveals itself to be new and fecund, but introduces into science a new and vast **theme** that embodies it. And all science, when we understand it not as an instrument of power and domination, but as an adventure in the knowledge of our species through the ages, is nothing other than that harmony, more or less vast and richer from one era to the next, which unfolds over the course of generations and centuries, through the delicate counterpoint of all the themes that appear in turn, as if called from nothing, to join and intertwine within it.

p. P21

□ Among the many new points of view I've opened up in mathematics, there are **twelve**, along with the I'd call them "big ideas"<sup>22</sup> . To see my work as a mathematician, to "feel" it, is to see and "feel" at least some of these ideas, and the great themes they introduce, which form both the fabric and the soul of the work.

<sup>21</sup>By the time I left the mathematical scene in 1970, the totality of my publications (many of them collaborative) on the central theme of schemas must have amounted to some ten thousand pages. Yet this represented only a modest part of the vast program I saw ahead of me concerning schemas. This program was abandoned sine die as soon as I left, despite the fact that very little of what had already been developed and published and made available to the general public, became part of the common heritage of notions and results commonly used as "well known".

The part of my program on the schematic theme and its extensions and ramifications, which I had completed by the time I left, alone represents the most extensive work on foundations ever accomplished in the history of mathematics, and surely one of the most extensive in the history of science too.

<sup>22</sup>Here, for the mathematically curious reader, is a list of the twelve key ideas, or "master themes" of my work (in chronological order of appearance).

1. Topological tensor products and nuclear spaces.
2. Continuous" and "discrete" duality (derived categories, "six operations").
3. Yoga Riemann-Roch-Grothendieck(*K-theory*, relation to intersection theory).
4. Schematics.
5. Topos.
6. Spread and l-adic cohomology.
7. Motifs and motivic Galois group (  $\otimes$ -Grothendieck categories).
8. Crystals and crystal cohomology, yoga "De Rham coefficients", "Hodge coefficient" . . .
9. "Topological algebra":  $\infty$ -fields, derivators; cohomological topos formalism, as inspiration for a new homotopic algebra.
10. Moderate topology.
11. Yoga of Anabelian algebraic geometry, Galois-Teichmüller theory.
12. Schematic" or "arithmetic" point of view for regular polyhedra and regular configurations of all kinds.

Apart from the first of these themes, an important part of which is part of my thesis (1953) and was developed during my period of functional analysis between 1950 and 1955, the eleven others emerged during my period as a surveyor, from 1955 onwards.



By necessity, some of these ideas are "bigger" than others (which, by the same token, are "smaller"! ). In other words, some of these new themes are broader than others, and some plunge deeper into the heart of the mystery of mathematical things<sup>23</sup>. There are three (and no "Officially", they don't even exist, since no formal publication is available to serve as their birth certificate<sup>24</sup>). Of the nine themes that appeared before my departure, the last three, which I had left in full bloom, are still in their infancy, for lack (after my departure) of loving hands to provide for these "orphans", left to fend for themselves in a hostile world<sup>25</sup>. As for the other six themes, which reached full maturity in the two decades preceding my departure, it could be said (with one or two reservations<sup>26</sup>) that by then they had already become part of the common heritage: especially among the geometrician gentry, "everyone" nowadays intones them without even knowing it (as Monsieur Jourdain used to do with prose), all day long and all the time. They're part of the air we breathe when we're "doing geometry", or when we're doing arithmetic, algebra or analysis that's even remotely "geometric".

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These twelve major themes in my work are by no means isolated from one another. They are part of my **unity** of spirit and purpose that runs like a common, persistent background note through all my "written" and "unwritten" work. And as I write these lines, I seem to find the same note again... - like a call! - through these three years of "free", relentless and solitary work, at a time when I hadn't yet bothered to find out whether there were any mathematicians in the world apart from myself, so caught up was I then in the fascination of what was calling me... .

p. P23

This unity is not merely the mark of the same craftsman on the works that emerge from his hands. These themes are linked by innumerable connections, at once delicate and obvious, just as the different themes, each clearly recognizable, that unfold and entwine in

<sup>23</sup>Among these themes, the **widest in scope** seems to me to be that of **topos**, which provides the idea for a synthesis of algebraic geometry, topology and arithmetic. The broadest in terms of **the range of developments** to which it has already given rise, is the theme of schemas. (See b. de p. (\*) on page 20). It provides the framework "par excellence" for eight of the other themes considered (i.e., all the others excluding themes 1, 5 and 10), while at the same time providing the central notion for a fundamental renewal of algebraic geometry, and of the algebraic-geometric language. At the opposite end of the spectrum, the first and last of the twelve themes seem to me to be of more modest dimensions than the others. And yet, as for the last one, which introduces a new perspective to the age-old theme of regular polyhedra and regular configurations, I doubt that a mathematician's lifetime of dedication would be enough to exhaust it. As for the first of all these themes, that of topological tensor products, it played more the role of a new, ready-to-use tool than that of a source of inspiration for further developments. Even in recent years, however, I still receive sporadic echoes of more or less recent work, resolving (twenty or thirty years later) some of the questions I had left in abeyance.

The most profound (to my mind) of these twelve themes are that of **patterns**, and the closely related one of **Anabelian algebraic geometry and Galois-Teichmüller yoga**.

From the point of view of the **power of tools** that I have perfected and honed, and that have been in common use in various "cutting-edge" research sectors over the last two decades, it is the **"schematics"** and **"staggered and l-adic cohomology"** aspects that seem to me to be the most noteworthy. For a well-informed mathematician, I think there can be little doubt that schematics, and the l-adic cohomology that stems from them, are among the few major achievements of the century that have nourished and renewed our science over the last few generations.

<sup>24</sup>The only "semi-official" text in which these three themes are sketched out to any extent is *Esquisse d'un Programme*, written in January 1984 in response to a request for a secondment to the CNRS. This text (also referred to in Introduction 3, "Boussole et Bagages") will in principle be included in volume 4 of *Réflexions*.

<sup>25</sup>After these three orphans had been buried without fanfare on the very day after my departure, two of them were exhumed with great fanfare and no mention of the worker, one in 1981 and the other (given the unmitigated success of the operation) the following year.

<sup>26</sup>The "just about" refers above all to the Grothendieckian yoga of duality (derived categories and six operations), and that of the topos. This is discussed in detail (among other things) in Parts II and IV of *Harvest and Sowing* (Burial (1) and (3)).

a single, vast counterpoint - in a harmony that brings them together, carries them forward and gives each a meaning, a movement and a fullness in which all the others participate. Each of the partial themes seems to emerge from this larger harmony and to be reborn from it moment by moment, rather than appearing as a "sum" or "result" of pre-existing constituent themes. And to tell the truth, I can't help feeling (no doubt absurd... .) that, in a way, it is this harmony that has not yet appeared, but which surely already "existed", somewhere in the obscure bosom of things yet to be born - that it is this harmony that has in turn given rise to these themes that would only take on their full meaning through it, and that it is also this harmony that was already calling me in a low, urgent voice, in those years of ardent solitude, at the end of adolescence. ...

The fact remains that these twelve master themes of my work all, as if by a secret predestination, contribute to the same symphony - or, to use a different image, they embody so many different "points of view", all contributing to the same vast **vision**.

p. P24 This vision only began to emerge from the mists, to show recognizable contours, around the years 1957, 58 - years of intense gestation<sup>27</sup>. Perhaps strangely enough, this vision was for me so close, so "obvious", that until a year ago<sup>28</sup>, I had never thought of giving it a name. (I, whose one of passions, however, has been to constantly **name** the things that discover themselves to me, as a first means of apprehending them. . . .) It's true that I can't point to any particular moment that would have been experienced as the moment of this vision's appearance, or that I could recognize as such in retrospect. A new vision is such a vast thing, that its appearance cannot possibly be situated at a particular moment, but must penetrate and gradually take possession over many years, if not generations, of those who scrutinize and contemplate; as if new eyes had to laboriously form, behind the familiar eyes they are destined to gradually replace. And the vision is too vast for us to "grasp" it, as we would grasp the first notion to appear at the turn of the road. That's why it's probably not surprising that the idea of naming something so vast, yet so close and diffuse, only emerged with hindsight, once it had reached full maturity.

To tell the truth, until two years ago my relationship with mathematics was limited (apart from the task of teaching it) to **make** it - to follow an impulse that kept pulling me **forward**, into an "unknown" that **m'** attirait sans cesse. The idea wouldn't have occurred to me to stop in this momentum, to put down even the space

p. P25

<sup>27</sup>1957 was the year in which I came up with the "**Riemann-Roch**" theme (Grothendieck version) - which, overnight, made me a "big star". It was also the year of my mother's death, which marked a major turning point in my life. It was one of the most intensely creative years of my life, and not just mathematically. For twelve years, all my energy had been invested in mathematical work. That year, I had the feeling that I'd pretty much "come full circle" in terms of mathematical work, and that it might now be time to invest myself in something else. It was a need for inner renewal that surfaced for the first time in my life. At the time, I was thinking of becoming a writer, and for several months I stopped all mathematical activity. In the end, I decided that I would at least put the mathematical work I was already doing in black and white, for a few months probably, or a year at the most... .

The time was not yet ripe, no doubt, for the big leap. Nevertheless, once I had resumed my mathematical work, it was he who took me back. And he didn't let go for another twelve years!

The year that followed this interlude (1958) was perhaps the most fruitful of all in my life as a mathematician. It was in this year that the two central themes of the new geometry emerged, with the strong start of **schema theory** (the subject of my talk at the International Congress of Mathematicians in Edinburgh in the summer of the same year), and the appearance of the notion of "**site**", a provisional technical version of the crucial notion of **topos**. With the benefit of almost thirty years' hindsight, I can now say that this was truly the year when the vision of the new geometry was born, in the wake of the two master tools of this geometry: diagrams (which represent a metamorphosis of the old notion of "algebraic variety"), and topos (which represent an even more profound metamorphosis of the notion of space).

<sup>28</sup>I first thought of giving a name to this vision in the reflection of December 4 1984, in the sub-note (n° 136<sub>1</sub> to the note "Yin the Servant (2) -or generosity" (ReS III, page 637).

(Whether to situate it **in my life**, as something to which deep and long-ignored links continue to connect me; or to situate it in the collective adventure that is "**mathematics**").

Strangely enough, in order for me to finally "put down" and reacquaint myself with this half-forgotten work, or to even think of giving a **name** to the vision that was its soul, I suddenly found myself confronted with the reality of a Burial of gigantic proportions: the burial, through silence and derision, of both the vision and the worker in whom it was born. ...

## 2.9. Form and structure - or the way to things

Without having planned it, this "foreword" ended up becoming a sort of formal presentation of my work, intended (above all) for the non-mathematician reader. I'm already too far along to go back any further, so all that's left for me to do is complete the "introductions"! I'd like to try, as best I can, to say at least a few words about the **substance** of these mirific "big ideas" (or "master themes") that I've hinted at in the preceding pages, and about the nature of the famous "vision" into which these master ideas are supposed to converge. In the absence of any technical language, I'll probably only be able to convey an extremely blurred image (if anything is indeed "conveyed"...)<sup>29</sup>.

Traditionally, we distinguish three types of "qualities" or "aspects" of things in the Universe, which are the object of mathematical reflection: these are **number**<sup>30</sup>, **size** and **shape**. We can also  $\square$  call them the "**arithmetic**" aspect, the "**metric**" (or "analytic") aspect, and the "**geometric**" aspect of things. In most situations studied in mathematics, these three aspects are present simultaneously and in close interaction. More often than not, however, there is a marked predominance of one of the three. It seems to me that in most mathematicians, it's pretty clear (to those who know them, or are familiar with their work) what their basic temperament is, whether they are "arithmeticians", "analysts", or "geometers" - and this, even though they would have many strings to their violin, and would have worked in every conceivable register and pitch.

p. P26

My first, solitary reflections on the theory of measurement and integration fall squarely under the heading of "magnitude" or "analysis". And the same is true of the first of the new themes I introduced into mathematics (which appears to me to be of less vast dimensions than the eleven others). The fact that I entered mathematics through the "medium" of analysis seems to me to be due, not to my particular temperament, but to what might be called a "fortuitous circumstance": the most enormous gap, for my general and rigorous mind, in the teaching offered to me at high school and university, concerned the "metric" or "analytical" aspect of things.

The year 1955 marked a crucial turning point in my mathematical work: the transition from "analysis" to "mathematics".

<sup>29</sup>The fact that this image has to remain "blurred" in no way prevents it from being faithful to, and indeed restoring, something of the essence of what is being looked at (in this case, my work). Conversely, no matter how sharp an image may be, it may well be distorted, and what's more, include only the incidental and miss the essential entirely. So, if you "hang on" to what I see to say about my work (and surely then something of the image in me will indeed "get through"), you'll be able to flatter yourself that you've grasped what's essential in my work better than perhaps any of my learned colleagues!

<sup>30</sup>It's understood here that we're talking about "numbers" known as "natural integers" 0, 1, 2, 3 etc., or (at a pinch) numbers (such as fractional numbers) that can be expressed using them by operations of an elementary nature. Like "real numbers", these numbers are not suitable for measuring a variable that can vary continuously, such as the distance between two variable points on a straight line, in a plane or in space.

to "geometry". I still remember that striking impression (all subjective, of course), as if I'd left the arid, stark steppes, to suddenly find myself in a kind of "promised land" of luxuriant riches, multiplying infinitely wherever it pleases the hand to rest, to pick or to rummage... . And this impression of overwhelming wealth, beyond all measure<sup>31</sup>, has only been confirmed and expanded upon.

over the years, right up to the present day.

p. P27 □ That is to say, if there's one thing in mathematics that has always fascinated me more than

It's neither "number" nor "size", but always **form**. And among the thousand-and-one faces that form chooses to reveal itself to us, the one that has fascinated me more than any other, and continues to fascinate me, is the **structure** hidden in mathematical things.

The structure of a thing is by no means something we can "invent". We can only patiently uncover it, humbly getting to know it, "**discovering**" it. If there is inventiveness in this work, and if we sometimes act as blacksmiths or tireless builders, it is by no means to "shape" or "build" "structures". They didn't wait for us to come into being, and to be exactly what they are! But it is to **express**, as faithfully as we can, these things that we are in the process of discovering and probing, and this structure, reluctant to surrender itself, that we are groping for, and perhaps using a language still in its infancy, to define. In this way, we're constantly "**inventing**" the language capable of expressing in ever finer detail the intimate structure of the mathematical thing, and "constructing" with the help of this language, as we go along and from scratch, the "theories" that are supposed to account for what has been apprehended and seen. There's a continual, uninterrupted back-and-forth movement between **the apprehension of** things and the **expression of** what is apprehended, using a language that is refined and re-created as the work progresses, under the constant pressure of immediate need.

As the reader will no doubt have guessed, these "theories", "built from scratch", are none other than the "**beautiful houses**" mentioned above: those we inherit from our predecessors, and those we are led to build with our own hands, at the call of things. And if I spoke earlier of the "inventiveness" (or imagination) of the builder or blacksmith, I would have to add that the soul and secret nerve of it is not the superbness of the person who says: "I want this, and not that!" and who takes pleasure in deciding as he pleases; like a poor architect who has his plans all ready in his head, before having seen and felt a plot of land, and probed its possibilities and requirements. The quality of the researcher's inventiveness and imagination lies in the **quality of his attention** to the voice of things. Because things

of the Universe never tire of speaking of themselves and revealing themselves, to the one who cares to hear.

p. P28 And the most beautiful house' □ the <sup>one</sup> in which the worker's love appears, is not the one that is larger or higher than others. A beautiful home is one that faithfully reflects the hidden structure and beauty of things.

## 2.10. The new geometry - or the marriage of number and size

But here I am diverging again - I was proposing to speak of master-themes, coming together in the same mother-vision, like so many rivers returning to the Sea whose sons they are... .

<sup>31</sup>I've used the association of words "overwhelming, beyond measure", to render as best I can the German expression "überwältigend", and its English equivalent "overwhelming". In the previous sentence, the (inadequate) expression "striking impression" is also to be understood with this nuance: when the impressions and feelings aroused in us by confrontation with uncommon splendor, grandeur or beauty suddenly overwhelm us, to the point where any attempt to express what we feel seems to be annihilated in advance.

This vast, unifying vision can be described as a **new geometry**. It's the kind of geometry Kronecker dreamed of in the last century<sup>32</sup>. But the reality (which a bold dream sometimes makes us sense or glimpse, and which he encourages us to discover. . . ) is always richer and more resonant than even the boldest or most profound dream. Surely, for more than one facet of this new geometry (if not for all), no one, even the day before it appeared, would have thought of it - the worker himself no more than anyone else.

We could say that "number" is apt to grasp the structure of "discontinuous", or "**discrete**" aggregates: systems, often finite, formed of "elements" or "objects" isolated from one another, so to speak, without some principle of "continuous passage" from one to another. "Magnitude", on the other hand, is the quality par excellence, capable of "**continuous variation**"; in this way, it is able to grasp continuous structures and phenomena: movements, spaces, "varieties" of all kinds, force fields and so on. Thus, arithmetic appears (roughly speaking) as the **science of discrete structures**, and analysis as the **science of continuous structures**,

□ As for geometry, it can be said that for more than two thousand years it has existed as a science in the modern sense of the word, it "straddles" these two types of structure, the "discrete" and the "continuous"<sup>33</sup>. For a long time, moreover, there was no real "**divorce**" between **two** geometries that would have been of different kinds, one discrete, the other continuous. Rather, there were two different points of view in the investigation of the **same** geometric figures: one emphasizing "discrete" properties (and in particular, numerical and combinatorial properties), the other "continuous" properties (such as position in the surrounding space, or "magnitude" measured in terms of the mutual distances of its points, etc.).

It was at the end of the last century that a divorce appeared, with the appearance and development of what was sometimes called "**abstract** (algebraic) **geometry**". Roughly speaking, this consisted in introducing, for each prime number  $p$ , an (algebraic) geometry "of characteristic  $p$ ", modelled on the (continuous) model of the (algebraic) geometry inherited from previous centuries, but in a context that appeared to be irreducibly "discontinuous", "discrete". These new geometric objects have taken on increasing importance since the beginning of the century, particularly in view of their close relationship with arithmetic, the science par excellence of discrete structure. This would seem to be one of the guiding ideas in **André Weil's** work<sup>34</sup>, perhaps even the main thrust (which has remained more or less unspoken).

<sup>32</sup>I only know of this "Kronecker dream" through hearsay, when someone (perhaps it was John Tate) told me I was in the process of realizing it. In the education I received from my elders, historical references were rare, and I was nourished, not by reading ancient or even contemporary authors, but above all by communicating, orally or through letters, with other mathematicians, starting with my elders. The main, perhaps even the only external inspiration for the sudden and vigorous start of scheme theory in 1958, was Serre's well-known paper FAC ("Faisceaux algébriques cohérents"), published a few years earlier. That one aside, my main inspiration in the subsequent development of the theory was to be found to flow from itself, and to be renewed over the years, by the sole demands of simplicity and internal coherence, in an effort to give an account in this new context, of what was "well known" in algebraic geometry (and which I assimilated as it was transformed in my hands), and of what this "known" made me sense.

<sup>33</sup>In fact, traditionally, it was the "continuous" aspect that was the focus of the geometer's attention, while properties of a "discrete" nature, and in particular numerical and combinatorial properties, were passed over in silence or dealt with under the table. It was with wonder that I discovered, some ten years ago, the richness of the combinatorial theory of the icosahedron, whereas this theme is not even touched upon (and probably, not even seen) in Klein's classic book on the icosahedron. I see another striking sign of this (two-thousand-year-old) neglect by geometers of the discrete structures spontaneously introduced into geometry: it's that the notion of groups (of symmetries, in particular) only appeared in the last century, and that, what's more, it was first introduced (by Evariste Galois) in a context that wasn't then considered to belong to "geometry". It's true that even today, many algebraists still haven't understood that Galois' theory is, in essence, a "**geometrical**" vision, renewing our understanding of so-called "arithmetical" phenomena. . .

<sup>34</sup>André Weil, a French mathematician who emigrated to the USA, is one of the "founding members" of the "Bourbaki Group", which will be discussed at length in the first part of Récoltes et Semailles (as well as Weil himself, occasionally).

in his written work, as it should be), that "geometry" (algebraic), and especially the "discrete" geometries associated with the various prime numbers, were to provide the key for a renewal of arithmetic. It was in this spirit that, in 1949, he came up with the famous "**Weil conjectures**". These were absolutely breathtaking conjectures, in fact, which, for these new discrete "varieties" (or "spaces"), opened up the possibility of certain types of constructions and arguments<sup>35</sup> which, until then, had only seemed conceivable within the framework of the only "spaces" considered worthy of the name by analysts - namely, the so-called "topological" spaces (where the notion of continuous variation holds sway).

The new geometry can be seen as, above all else, a synthesis between these two worlds, which until then had been adjacent and closely interwoven, yet separate: the "**arithmetical**" world, in which live (so-called) "spaces" without any principle of continuity, and the **world of continuous magnitude**, where live "spaces" in the proper sense of the term, accessible to the analyst's means and (for this very reason) accepted by him as worthy of dwelling in the mathematical city. **In the new vision, these two previously separate worlds become one.**

The first embryo of this vision of an "arithmetic geometry" (as I propose to call this new geometry) can be found in Weil's conjectures. In the development of some of my main themes<sup>36</sup>, these conjectures remained my main source of inspiration, throughout the years between 1958 and 1969. Even before me, **Oscar Zariski** on the one hand, and **Jean-Pierre Serre** on the other, had developed certain "topological" methods for the lawless spaces of "abstract" algebraic geometry, inspired by those previously used for the "good-natured spaces" of the whole world<sup>37</sup>.

□ Their ideas, of course, played an important role in my first steps in building geometry. arithmetic; more, it's true, as starting points and **tools** (which I had to reshape more or less from scratch, for the needs of a much larger context), than as a source of inspiration that would have continued to nourish my dreams and projects, over the months and years. In any case, it was clear from the outset that, even reshaped, these tools fell far short of what was required to take even the very first steps in the direction of fantastic conjectures.

## 2.11. The magic fan - or innocence

The two key ideas in the launch and development of the new geometry were that of **schema** and **topos**. Appearing more or less simultaneously, and in close symbiosis with each other

<sup>35</sup>(Intended for the mathematician reader.) This section covers "constructions and arguments" related to the cohomological theory of differentiable or complex varieties, and in particular those involving Lefschetz's formula of fixed points and Hodge's theory.

<sup>36</sup>These are the four "median" themes (n°s 5 to 8), i.e. **topos** of *étale* and *l-adic* **cohomology**, **motifs**, and (to a lesser extent) **crystals**. I developed these themes in turn between 1958 and 1966.

<sup>37</sup>(For the mathematician reader.) Zariski's main contribution in this direction seems to me to be the introduction of "Zariski's topology" (which later became an essential tool for Serre in FAC), and his "principle of connectedness" and what he called his "theory of holomorphic functions" - which in his hands became the theory of formal schemes, and the "comparison theorems" between the formal and the algebraic (with, as a second source of inspiration, Serre's fundamental GAGA article). As for Serre's contribution to which I allude in the text, it is, of course, first and foremost his introduction, in abstract algebraic geometry, of the point of view of bundles (introduced by **Jean Leray** a dozen years earlier, in a quite different context), in that other fundamental article already cited FAC ("Faisceaux algébriques cohérents").

In the light of these "reminders", if I were to name the immediate "ancestors" of the new geometrical vision, the names **Oscar Zariski**, **André Weil**, **Jean Leray** and **Jean-Pierre Serre** immediately come to mind. Among them, Serre played a special role, since it was mainly through him that I became aware not only of his own ideas, but also of those of Zariski, Weil and Leray, who had a part to play in the birth and development of the new geometry.

with the other<sup>38</sup>, they were like a single **driving force behind** the spectacular rise of the new geometry, and this from the very year of their appearance. To conclude this overview of my work, I'd like to say a few words about at least these two ideas.

Schema is the most natural, the most "obvious" concept imaginable, to encompass in a single notion the infinite series of notions of "variety" (algebraic) that we previously handled (one such notion for **each**  $p$  . P32 prime number<sup>39</sup> . . . ). What's more, one and the same "pattern" (or new-style "variety") gives rise, for each prime number  $p$ , to a well-defined "(algebraic) variety of characteristic  $p$ ". The collection of these different varieties of different characteristics can then be visualized as a kind of "(infinite) fan of varieties" (one for each characteristic). The "schema" is this magical fan, which links together, like so many different "branches", its "avatars" or "incarnations" of all possible characteristics. At the same time, it provides an effective "principle of passage" for linking together "varieties" of geometries that had hitherto appeared more or less isolated, cut off from one another. Now, they are encompassed by a common "geometry" and linked by it. We could call it **schematic geometry**, the first draft of the "arithmetic geometry" into which it was to blossom in the following years.

The very idea of schematics is childishly simple - so simple, so humble, that no one before me had ever thought of stooping so low. So "silly", in fact, that for years, despite the obvious, many of my learned colleagues thought it was "not serious"! In fact, it took me months of hard, solitary work to convince myself that it really did "work".

- that the silly new language I was incorrigibly naïve enough to insist on testing was indeed adequate to capture, in a new light and with a new finesse, and in a common framework from now on, some of the very first geometrical intuitions attached to the previous "geometries of characteristic  $p$ ". It was the kind of exercise, judged foolish and hopeless in advance by any "well-informed" person, that I was probably the only one, among all my colleagues and friends, to ever have the idea of putting into my head, and even (moved by a secret demon. . . ) to bring to a successful conclusion against all odds!

Rather than allow myself to be distracted by the consensus around me about what is "serious" and what is not, I simply **trusted**, as I had in the past, the humble voice of things, and that part of me that knows how to listen. The reward was immediate, and beyond all expectations. In the space of In just a few months, without even "doing it on purpose", I had put my finger on some powerful and unsuspected tools.

They allowed me not only to find (as if playing) old, reputedly arduous results in  $p$  . P33 a more penetrating light and to go beyond them, but also to finally tackle and solve problems of " $p$ -characteristic geometry" which until then had seemed out of reach by all means then known<sup>40</sup>.

In our knowledge of things in the Universe (mathematical or otherwise), the power

<sup>38</sup>This start, in 1958, is mentioned in the b. de p. note on page 23. The notion of site or "**Grothendieck topology**" (a provisional version of that of topos) appeared in the immediate wake of the notion of schema. It in turn provides the new language of "localization" or "descent", used at every step in the development of the schematic theme and tool. The more intrinsic and geometrical notion of topos, which remained implicit for the next few years, began to emerge from 1963 onwards, with the development of étale cohomology, and gradually imposed itself on me as the most fundamental notion.

<sup>39</sup>The case of  $p = \infty$ , corresponding to algebraic varieties of "zero characteristic", should also be included in this series.

<sup>40</sup>An account of this "strong start" for schema theory is the subject of my presentation at the International Congress.

of Mathematicians in Edinburgh, 1958. The text of this talk seems to me to be one of the best introductions in terms of diagrams, likely (perhaps) to motivate a geometrician reader to familiarize himself or herself as best he or she can with the (later) imposing treatise "Elements of Algebraic Geometry", setting out in a detailed manner (and without mercy to any technical details) the new foundations and techniques of algebraic geometry.

**innocence.** It's the original innocence we were all born with, and which lies within each of us, often the object of our scorn and our most secret fears. It alone unites the humility and boldness that allow us to penetrate to the heart of things, and to let things penetrate and impregnate us.

This power is by no means the privilege of extraordinary "gifts" - of (let's say) uncommon brain power to assimilate and handle, with dexterity and ease, an impressive mass of known facts, ideas and techniques. Such gifts are certainly precious, worthy of envy for anyone (like me) who wasn't blessed with them at birth, "beyond measure".

It's not these gifts, however, nor even the most ardent ambition, served by unfailing willpower, that get us through the "invisible and imperious circles" that enclose our Universe. Only innocence crosses them, unknowingly and unconcernedly, in those moments when we find ourselves alone, listening to things, intensely absorbed in a child's game. ...

## 2.12. Topology - or the survey of mists

As we've just seen, the innovative idea behind the "schema" is that of linking together the different "geometries" associated with the different prime numbers (or different "characteristics"). These geometries, however, were still each essentially "discrete" or "discontinuous" in nature, in contrast to the traditional geometry bequeathed by past centuries (and dating back to Euclid). The new ideas introduced by Zariski and Serre restored, to a certain extent, a "dimension" to these geometries.

p. P34 ☐ continuity, immediately inherited by the "schematic geometry" that had just appeared, for the purpose of uniting them. But

as far as Weil's "fantastic conjectures" were concerned, we were way off the mark. These "Zariski topologies" were, from this point of view, so coarse-grained that it was almost as if we were still at the stage of "discrete aggregates". What was obviously missing was some new principle that would enable us to link these geometric objects (or "varieties", or "schemes") to the usual, or "bon teint" (topological) "spaces"; those, let's say, whose "points" appear to be clearly **separated** from one another, whereas in the spaces-without-law-not introduced by Zariski, the points have an annoying tendency to clump together... ..

It was the appearance of such a decidedly "new principle", and nothing less, that could consummate those "marriages of number and magnitude" or of the "geometry of the discontinuous" with that of the "continuous", a first hint of which emerged from Weil's conjectures.

The notion of "**space**" is undoubtedly one of the oldest in mathematics. It is so fundamental to our "geometric" apprehension of the world, that it has remained more or less unspoken for over two millennia. It's only over the past century that this notion has gradually come to detach itself from the tyrannical grip of immediate perception (of one and the same "space" surrounding us), and from its traditional ("Euclidean") theorization, to acquire its own autonomy and dynamic. Today, it is one of the few notions most universally and commonly used in mathematics, and is familiar to every mathematician without exception. A pro forma notion if ever there was one, with a hundred and a thousand faces, depending on the type of structures we incorporate into these spaces, from the richest of all (such as the venerable "Euclidean" structures, or the "affine" and "projective" structures, or the "algebraic" structures of the "varieties" of the same name, which generalize them and make them more flexible) to the most stripped-down: those where any "quantitative" element of information whatsoever seems to have disappeared without return,



where only the quintessential qualitative notions of "**proximity**" and "**limit**" remain<sup>41</sup>, and the most elusive version of the "topological" intuition of **form**. The most stripped-down of all these notions, the one that<sup>42</sup> up to now, over the past half-century, had taken the place of a kind of vast bosom to encompass all the others, was that of **topological space**. The study of these spaces is one of the most fascinating and lively branches of geometry: **topology**. p. P35

However elusive this structure of "pure quality" embodied by a "space" may seem at first glance, in the absence of any quantitative data (such as the distance between two points, in particular) that would enable us to cling to some familiar intuition of "size" or "smallness", we have nonetheless managed, over the past century, to finely define these spaces in the tight, supple meshes of a carefully "cut-to-size" language. Better still, we've invented and fabricated a whole range of "meters" and "toises" to serve, against all odds, as a way of attaching "me- sures" (called "topological invariants") to these sprawling "spaces" that seemed to elude all attempts at measurement, like elusive mists. It's true that most of these invariants, and the most essential ones at that, are of a more subtle nature than a simple "number" or "magnitude" - rather, they are themselves more or less delicate mathematical structures, attached (by means of more or less sophisticated constructions) to the space under consideration. One of the oldest and most crucial of these invariants, introduced as early as

in the last century (by the Italian mathematician **Betti**), is made up of the various so-called "cohomology" "groups" (or "spaces") associated with the space<sup>42</sup>. These are the ones that intervene (mostly<sup>43</sup> "between the lines", it's true) in . P36

Weil's conjectures, which give them their deepest "raison d'être" and (for me at least, "thrown into the deep end" by Serre's explanations) their full meaning. But the possibility of associating such invariants with the "abstract" algebraic varieties involved in these conjectures, in such a way as to meet the very precise desiderata required for the needs of that cause - that was a mere hope. I doubt that, apart from Serre and myself, nobody else (not even, and above all, André Weil himself!<sup>43</sup>) really believed in it... .

<sup>41</sup>Talking about the notion of "limit", I'm thinking more of "crossing the limit" than of "frontier" (more familiar to the non-mathematician).

<sup>42</sup>In fact, the invariants introduced by Betti were **homology** invariants. **Cohomology** is a more or less equivalent, "dual" version, introduced much later. This aspect took precedence over the initial, "homological" aspect, especially (no doubt) following Jean Leray's introduction of the beam point of view, discussed below. From a technical point of view, one could say that a large part of my work as a geometer consisted in identifying, and developing to a greater or lesser extent, the cohomological theories that were lacking, for spaces and varieties of all kinds, and above all, for "algebraic varieties" and schemes. Along the way, I was also led to reinterpret traditional homological invariants in cohomological terms, and in so doing, to show them in an entirely new light.

There are many other "topological invariants" that have been introduced by topologists, to define one type of property or another of topological spaces. Apart from the "dimension" of a space, and the (co)homological invariants, the first other invariants are the "homotopy groups". I introduced another of these in 1957, the so-called "Grothendieck group"  $K(X)$ , which immediately met with great success, and whose importance (both in topology and arithmetic) continues to be confirmed.

A host of new invariants, of a more subtle nature than the invariants currently known and used, but which I feel are fundamental, are envisaged in my "moderate topology" program (a very rough outline of which can be found in "Esquisse d'un Programme", to be published in volume 4 of *Réflexions*). This program is based on the notion of "moderate theory" or "moderate space", which, like that of topos, constitutes a (second) "metamorphosis of the notion of space". It is far more obvious (it seems to me) and less profound than the latter. I predict, however, that its immediate impact on topology "proper" will be far more far-reaching, and that it will fundamentally transform the geometric topologist's "profession", through a profound transformation of the conceptual context in which he works (as was also the case in algebraic geometry with the introduction of the schema point of view). Incidentally, I sent my "Esquisse" to several of my old friends and illustrious topologists, but it doesn't seem to have interested any of them. . .

<sup>43</sup>Paradoxically, Weil had a tenacious, seemingly visceral "block" against cohomological formalism - whereas it was largely his famous conjectures that inspired the development of the great cohomological theories in algebraic geometry, from the 1950s onwards (with Serre kicking things off, with his seminal paper FAC, already mentioned in a previous footnote).

It seems to me that this "block" is part, in Weil, of a general aversion against all "big stuff", against all that

Shortly before, our conception of these cohomology invariants had been enriched and profoundly renewed by the work of **Jean Leray** (pursued in German captivity during the Second World War).  
 war' □ in the first half of the forties). The essential innovative idea was that of **bundling** (abé-link) on a space, to which Leray associates a series of corresponding "cohomology groups" (with coefficients in this bundle). It was as if the good old standard "cohomological meter" we'd been using until now to "survey" a space had suddenly been multiplied into an unimaginably large multitude of new "meters" of every conceivable size, shape and substance, each one intimately adapted to the space in question, and each one providing us with perfectly precise information about it - information that only it can give us. This was the key idea in a profound transformation in our approach to spaces of all kinds, and surely one of the most crucial ideas to emerge in this century. Thanks largely to the subsequent work of Jean-Pierre Serre, the first fruits of Leray's ideas, already in the decade following their appearance, were an impressive revival in the theory of topological spaces (and in particular, of their so-called "homotopy" invariants, intimately linked to cohomology), and another revival, no less crucial, of so-called "abstract" algebraic geometry (with Serre's fundamental "FAC" article, published in 1955). My own work in geometry, from 1955 onwards, was a continuation of Serre's work, and thus of Leray's innovative ideas.

## 2.13. Les topos - or the double bed

The point of view and language of beams introduced by Leray led us to look at "spaces" and "varieties" of all kinds in a new light. They did not, however, touch on the very notion of space itself, merely helping us to apprehend more finely, with new eyes, those traditional "spaces" already familiar to us all. It turns out that this notion of space is inadequate to account for the most essential "topological invariants" that express the "form" of "abstract" algebraic varieties (such as those to which Weil's conjectures apply), or even that of general "schemes" (generalizing old varieties). For the expected "marriages", "in number and size", it was like a decidedly narrow bed, where only one of the future spouses (i.e., the bride) could find a place to nestle, but never both at once! The "new principle" that remained to be found, to consummate the marriage promised by auspicious fairies, was none other than this spacious "bed" that the future spouses lacked, without anyone having noticed until then... .

This "double bed" appeared (as if by magic wand. . . ) with the idea of the **topos**. This idea encompasses, in a □ common topological intuition, both traditional (topological) spaces, embodying the world of continuous magnitude, than the (so-called) "spaces" (or "varieties") of unrepentant abstract algebraic geometers, as well as countless other types of structures, which until then had seemed irremediably riveted to the "arithmetic world" of "discontinuous" or "discrete" aggregates.

It's the beam's point of view that has been the silent and sure guide, the effective (and by no means secret) key,

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that resembles a formalism (when this can't be summed up in a few pages), or a "construction" that's even remotely interwoven. He was by no means a "builder", and it was clearly against his will that he was forced, in the thirties, to develop the first "abstract" foundations of algebraic geometry, which (given these provisions) turned out to be a veritable "Procrustean bed" for the user.

I don't know whether he blamed me for going beyond that, and for investing myself in building the vast mansions that enabled Kronecker's and his own dreams to be embodied in a language and tools that were both delicate and efficient. The fact remains that at no time did he say a word to me about the work he saw me doing, or the work that had already been done. Nor did I receive any response to *Récoltes et Semailles*, which I had sent him over three months ago, with a warm dedication in my own hand.

leading me without delay or detour to the bridal chamber with its vast marital bed. A bed so vast indeed (like a vast, peaceful, very deep river. . . ), that

"all the king's horses  
could drink together... "

- as an old tune tells us, which I'm sure you must have sung too, or at least heard sung. And whoever was the first to sing it felt the secret beauty and peaceful power of the topos better than any of my learned students and friends of yesteryear... ..

The key was the same, both in the initial, provisional approach (via the very convenient, but not intrinsic notion of "site"), and in that of the topos. It's the idea of the topos that I'd now like to try and describe.

Let's consider the set formed by **all the** beams on a given (topological) space, or, if you like, this prodigious arsenal formed by **all the** "meters" used to survey it<sup>44</sup>. We consider this "set" or "arsenal" to be equipped with its most obvious structure, which appears there, so to speak, "at sight of nose"; namely, a so-called "category" structure. (Let the non-mathematician reader not be troubled by not knowing the technical meaning of this term. He won't need it for what follows). It is this kind of "surveying superstructure", called the "category of beams" (on the space under consideration), that will henceforth be considered as "embodying" what is most essential to space. This is indeed lawful (for the "good mathematical sense"), because it turns out that we can "reconstruct" a topological space<sup>45</sup> from scratch in terms of this associated "beam category" (or **arsenal**  $\square$  **d'**arpentage). (To verify this is a simple exercise - once the question has been asked, of course. . . ) That's all we need to be sure that (if it suits us for whatever reason) we can now "forget" the initial space, and use only the associated "category" (or "arsenal") as the most appropriate embodiment of the "topological" (or "spatial") structure we're trying to express. . P39

As so often in mathematics, we have succeeded here (thanks to the crucial idea of "bundle", or "cohomological metre") in expressing a certain notion (that of "space" in this case) in terms of another (that of "category"). Each time, the discovery of such a **translation** of one notion (expressing a certain type of situation) in terms of another (corresponding to another type of situation), enriches our understanding of both notions, through the unexpected confluence of specific intuitions relating to either one or the other. Thus, a situation of a "topological" nature (embodied by a given space) is here translated by a situation of an "algebraic" nature (embodied by a "category"); or, if you like, the "continuum" embodied by space, is "translated" or "expressed" by the category structure, of an "algebraic" nature (and hitherto perceived as being of an essentially "discontinuous" or "discrete" nature).

But there's more. The first of these notions, that of space, appeared to us as a sort of "maximal" notion - a notion so general already, that it's hard to imagine how we can still find a "reasonable" extension to it. On the other hand, on the other side of the mirror<sup>46</sup>, these "categories" (or "arsenals") that we come across, starting from topological spaces, are of a very particular nature. They

<sup>44</sup>(To the mathematician) Actually, we're talking here about **set** bundles, not **abelian** bundles, introduced by Leray as the most general coefficients for forming "cohomology groups". In fact, I believe I was the first to work systematically with set bundles (from 1955, in my paper "A general theory of fibre spaces with structure sheaf" at the University of Kansas).

<sup>45</sup>(For the mathematician's benefit) Strictly speaking, this is only true for so-called "sober" spaces. However, these include almost all commonly encountered spaces, and in particular all the "separated" spaces so dear to analysts.

<sup>46</sup>The "mirror" referred to here, as in Alice in Wonderland, is the one that gives as an "image" of a space, placed in front of it, the associated "category", considered as a kind of "double" of the space, "on the other side of the mirror". . .

are indeed endowed with a set of strongly typed properties<sup>47</sup>, which make them a kind of "pastiche" of the simplest imaginable one - that obtained by starting from a space reduced to a single point. Having said this, a "new-style space" (or topos), generalizing traditional topological spaces, will be described quite simply as a "category" which, without necessarily coming from an ordinary space, nevertheless possesses all those good properties (explicitly designated once and for all, of course) of such a "category of bundles".

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p. P40

This is the new idea. Its appearance can be seen as a consequence of the almost childish observation that what really counts in a topological space is not its "points" or subsets of points<sup>48</sup>, and the relations of proximity etc. between them, but the **bundles** on this space, and the category they form. In short, all I've done is to take Leray's initial idea to its ultimate conclusion - and, in so doing, **take it a step further**.

Like the very idea of beams (due to Leray), or that of schematics, like any "big idea" that upsets an inveterate view of things, that of topos is disconcerting in its naturalness, its "obviousness", its simplicity (bordering, one might say, on the naïve or simplistic, or even the "silly" - that particular quality that so often makes us exclaim: "Oh, that's all there is to it!" with a tone of half-disappointment, half-envy; with, perhaps, the added undertone of "eccentric", "not serious", which we often reserve for anything that baffles us with an excess of unexpected simplicity. A reminder, perhaps, of the long-buried, long-denied days of our childhood... .

## 2.14. Mutation of the notion of space - or breath and faith

The notion of schema constitutes a vast extension of the notion of "algebraic variety", and as such it has thoroughly renewed the algebraic geometry bequeathed by my predecessors. The concept of topos constitutes an unsuspected extension, or rather, **a metamorphosis of the notion of space**. It holds out the promise of a similar renewal of topology, and beyond topology, of geometry. Indeed, it has already played a crucial role in the development of the new geometry (especially through the *l-adic* and crystalline cohomological themes that emerged from it, and through them, in the demonstration of Weil's conjectures). Like its elder (and near-twin) sister, it possesses the following two complementary characteristics essential to any fertile generalization.

Firstly, the new concept is not **too broad**, in the sense that in the new "spaces" (rather called "topos", so as not to offend delicate ears<sup>49</sup>), intuitions and "geometric" constructions

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p. P41

the most essential       <sup>50</sup> familiar from the good old spaces of yesteryear, can be transposed into more

<sup>47</sup>(For the mathematician) These are properties that I have introduced into category theory under the name of "accuracy properties" (along with the modern categorical notion of general inductive and projective "limits"). See "Sur quelques points d'algèbre homologique", Tohoku math. journal, 1957 (pp. 119-221).

<sup>48</sup>In this way, you can build very "big" topos that have only one "point", or even no "points" at all!

<sup>49</sup>The name "topos" has been chosen (in association with "topology", or "topological") to suggest that it is the "object par excellence" to which topological intuition applies. By virtue of the rich cloud of mental images this name elicits, it should be seen as more or less equivalent to the (topological) term "space", simply with greater emphasis on the "topological" specificity of the notion. (So, there are "vector spaces", but no "vector topos", until further notice!) It's essential to keep the two expressions together, each with its own *spécificité*.

or less obvious. In other words, for the new objects we have at our disposal the whole rich range of mental images and associations, notions and at least some of the techniques that were previously restricted to old-style objects.

And secondly, the new notion is at the same time **broad enough** to encompass a host of situations that, until now, were not considered to give rise to intuitions of a "topologic-geometric" nature.

- intuitions, which in the past were reserved for ordinary topological spaces (and with good reason. . . ).

The crucial thing here, from the point of view of Weil's conjectures, is that the new notion is indeed vast enough, to allow us to associate with any "scheme" such a "generalized space" or "topos" (called the "étale topos" to the scheme under consideration). Certain "cohomological invariants" of this topos (all that "silly" stuff!) then seemed to have a good chance of providing "what we needed" to make sense of these conjectures, and (who knows!) perhaps provide the means to demonstrate them.

It is in these pages that I am writing that, for the first time in my life as a mathematician, I take the liberty of evoking (if only to myself) all the master themes and great guiding ideas in my mathematical work. This leads me to a better appreciation of the place and scope of each of these themes, and of the "points of view" they embody, within the great geometrical vision that unites them and from which they derive. It is through this work that the two innovative ideas central to the creamy and powerful rise of the new geometry have come to the fore: the idea of **schemas**, and that of **topos**.

It is the second of these ideas, that of topos, which now appears to me □ as the most profound of the two. If, in the late fifties, I hadn't rolled up my sleeves and stubbornly developed, day after day, over twelve long years, a "schematic tool" of perfect delicacy and power - it would seem almost unthinkable to me that in the ten or twenty years since, it would seem almost unthinkable, however, that in the ten or twenty years that have followed, anyone other than me would have eventually (albeit unwillingly) introduced the notion that was obviously necessary, and at least erected a few dilapidated "prefab" shacks, in place of the spacious, comfortable mansions that I took it upon myself to assemble stone by stone and build with my own hands. On the other hand, I can't think of anyone else on the mathematical scene, over the past three decades, who might have had the naivety, or innocence, to take (in my place) that other crucial step of all, introducing the oh-so-childish idea of topos (or even that of "sites"). And, even supposing this idea had already been graciously provided, and with it the timid promise it seemed to hold - I can't think of anyone else, either among my old friends or among my students, who would have had the breath, and above all the faith, to bring this humble<sup>51</sup> idea to fruition (so derisory in appearance, when the goal seemed infinitely distant. . . ): from its first stammering beginnings, to the full maturity of the "mastery of staggered cohomology" into which it came to be embodied in my hands, over the years that followed.

p. P42

<sup>50</sup>These "constructions" include all the familiar "topological invariants", including cohomological invariants. For the latter, I had done everything necessary in the article already cited ("Tohoku" 1955), to be able to give them a meaning for any "topos".

<sup>51</sup>(For the benefit of the mathematician reader.) When I speak of "bringing this humble idea to fruition", I mean the idea of étale cohomology as an approach to Weil's conjectures. It was inspired by this idea that I discovered the notion of site in 1958, and that this notion (or the closely related notion of topos), and the étale cohomological formalism, were developed between 1962 and 1966 under my impetus (with the assistance of a number of collaborators who will be mentioned later).

When I speak of "breath" and "faith", I'm referring to qualities of a "non-technical" nature, which seem to me to be the essential qualities here. On another level, I could also add what I would call "cohomological flair", i.e. the kind of flair that had developed in me for building cohomological theories. I thought I'd passed it on to my cohomology students. Looking back seventeen years after my departure from the mathematical world, I can see that it has not been preserved in any of them.

## 2.15. All the king's horses. . .

p. P43

Yes, the river is deep, and vast and peaceful are the waters of my childhood, in a kingdom I thought I'd left long ago. All the king's horses could drink there at ease and all their strength, without exhausting them! They come from the glaciers, fiery as those distant snows, and they have the sweetness from the clay of the plains. I've just been talking about one of these horses, which a kid had brought to drink and drank his content, long. And I've seen <sup>another</sup> □ coming to drink for a while, following in the footsteps of the same kid so but it didn't take long. Someone must have chased him away. And that's all there is to it. Yet I see countless herds of thirsty horses roaming the plains - and just this morning their whinnies roused me from bed at an ungodly hour, me being on my sixtieth birthday and loving the peace and quiet. There was nothing I could do, I had to get up. It pains me to see them like gaunt beasts, even though there's no shortage of good water or green pastures. But it's as if a malevolent spell has been cast over this land that I've known to be welcoming, and condemned access to these generous waters. Or maybe it's a trick pulled by the local horse traders, to drive down prices, who knows? Or maybe it's a country where there are no more children to lead the horses to water, and where the horses are thirsty for lack of a kid to find the way back to the river... .

## 2.16. Patterns - or the heart in the heart

The topos theme grew out of the schematics theme, in the same year that the schematics appeared - but in scope it goes far beyond the mother theme. It is the theme of topos, and not that of schemas, that is the "bed", or "deep river", where geometry and algebra, topology and arithmetic, mathematical logic and category theory, the world of continuity and that of "discontinuous" or "discrete" structures come together. If the theme of schemas is like the **heart of** the new geometry, the theme of topos is its envelope, or **dwelling place**. It's the most far-reaching thing I've conceived, to capture with finesse, through a single language rich in geometrical resonances, an "essence" common to situations as far apart as possible, from one region or another of the vast universe of mathematical things.

The theme of topos, however, is far from having enjoyed the same fortune as that of schemas. I've written on this subject on several occasions in *Récoltes et Semailles*, and this is not the place to dwell on the strange vicissitudes that have befallen this notion. However, two of the master themes of the new geometry stem from that of the topos, two complementary "cohomological theories", both conceived to provide an approach to Weil's conjectures: **the étale** (or "**l-adic**") **theme**, and the **crystalline** theme. The former took shape in my hands as the *l-adic* cohomological tool, which is now one of the most powerful mathematical tools of the century. As for the crystalline theme, reduced after my departure to a quasi-occult existence, it was finally exhumed (under pressure of necessity) in June 1981, in the limelight and under an assumed name, in circumstances even stranger than those surrounding the topos.

p. P44

□ The *l-adic* cohomological tool was, as expected, the essential tool for establishing Weil's conjectures. I've demonstrated quite a few of them myself, and the last step was masterfully taken, three years after I left, by Pierre Deligne, the most brilliant of my "cohomologist" students.

In fact, around 1968, I came up with a stronger and, above all, more "geometric" version of Weil's conjectures. These remained "tainted" (if you can call it that!) by an apparently irreducible "arithmetic" aspect, even though the very spirit of these conjectures is to express and grasp "arithmetic".

(or "the discrete") through the mediation of the "geometric" (or "the continuous")<sup>52</sup>. In this sense, my version of the conjectures seems to me to be more "faithful" than Weil's own to "Weil's philosophy" - to that unwritten and seldom-spoken philosophy, which has perhaps been **the** main tacit motivation in the extraordinary rise of geometry over the past four decades<sup>53</sup>. My reformulation consisted, essentially, in extracting a kind of "quintessence" of what was to remain valid, in the context of so-called "abstract" algebraic varieties, of the classical "Hodge theory", valid for "ordinary" algebraic varieties<sup>54</sup>. I have called this new, entirely geometric version of the famous conjectures "**standard conjectures**" (for algebraic cycles).

In my mind, this was a further step after the development of the *l*-adic cohomological tool, in direction of these conjectures. But at the same time, and above all, it was also one of the principles of the possible to what still seems to me the most profound theme I've introduced into mathematics<sup>55</sup> : p. P45

that of **motifs** (itself born of the "*l*-adic cohomological theme"). This theme is like the **heart** or soul, the most hidden part, best concealed from view, of the schematic theme, which itself is at the heart of the new vision. And the few key phenomena identified in the standard<sup>56</sup> conjectures can be seen as forming a kind of ultimate quintessence of the motivic theme, as the vital "**breath**" of this most subtle of all themes, of this "**heart within the heart**" of the new geometry.

Here's what it's all about. We have seen, for a given prime number  $p$ , how important it is (particularly in view of Weil's conjectures) to be able to construct "cohomological theories" for "(algebraic) varieties of characteristic  $p$ ". Now, the famous "*l*-adic cohomological tool" provides just such a theory, and even an **infinite number of different cohomological theories**, namely one associated with any prime number different from characteristic  $p$ . Here again, there is clearly a "missing theory", which would correspond to the case of an  $l$  equal to  $p$ . To fill this gap, I have deliberately devised another cohomological theory (already alluded to earlier), known as "crystalline cohomology". Moreover, in the important case where  $p$  is infinite, we have three other cohomological theories at our disposal<sup>57</sup> - and there's nothing to prevent us from introducing yet more cohomological theories with similar formal properties sooner or later. Contrary to the situation in ordinary topology, we find ourselves faced with a bewildering abundance of different cohomological theories. We had the impres-

sion that, in a sense that initially remained rather vague, all these theories had to "come back to the same thing", that they "gave the same results"<sup>58</sup>. It was in order to express this intuition of "kinship" between p . P46

<sup>52</sup>(For the mathematician) Weil's conjectures are conditional on assumptions of an "arithmetical" nature, due in particular to the fact that the varieties under consideration must be defined over a finite body. From the point of view of cohomological formalism, this leads us to give a special place to the **Frobenius endomorphism** associated with such a situation. In my approach, the crucial properties (of the "generalized index theorem" type) concern **any** algebraic correspondences, and make no arithmetical assumptions about a given base field.

<sup>53</sup>However, after I left in 1970, there was a clear reaction, which resulted in a situation of relative stagnation, which I've mentioned more than once in the pages of Récoltes et Semailles.

<sup>54</sup>"Ordinary" here means: "defined sur le corps des complexes". Hodge's theory of harmonic integrals was the most powerful cohomological theory known in the context of complex algebraic varieties.

<sup>55</sup>This is the most profound theme, at least in the "public" period of my mathematical activity, between 1950 and 1969, i.e. until I left the mathematical scene. I consider the theme of Ana-Babelian algebraic geometry and Galois-Teichmüller theory, developed from 1977 onwards, to be of comparable depth.

<sup>56</sup>(For the benefit of the algebraic geometer reader) These conjectures may need to be reformulated. For more detailed comments, see "Le tour des chantiers" (ReS IV note n° 178, p. 1215-1216) and the note de b. de p. p 769 in "Conviction et connaissance" (ReS III, note n° 162).

<sup>57</sup>(For the mathematician reader) These theories correspond respectively to Betti cohomology (defined by way of transcendental, using a plunge of the base body into the body of complexes), Hodge cohomology (challenged by Serre) and De Rham cohomology (challenged by me), the latter two dating back to the 1950s (and Betti's to the last century).

<sup>58</sup>(For the mathematical reader) For example, if  $f$  is an endomorphism of the algebraic variety  $X$ , inducing a

different cohomological theories, that I derived the notion of a "**motif**" associated with an algebraic variety. By this term, I mean to suggest that this is the "common motif" (or "common **reason**") underlying this multitude of different cohomological invariants associated with the variety, using the multitude of all possible cohomological theories a priori. These different cohomological theories would be like so many different thematic developments, each in its own "tempo", "key" and "mode" ("major" or "minor"), of the same "basic motif" (called a "**mo- tivic** cohomological theory"), which would at the same time be the most fundamental, or "finest", of all these different thematic "incarnations" (i.e., of all these possible cohomological theories). Thus, the motif associated with an algebraic variety would constitute the "ultimate" cohomological invariant, "par excellence", from which all the others (associated with the different possible cohomological theories) would be deduced, like so many different musical "incarnations", or "realizations". All the essential properties of **the** variety's "cohomology" would already be "read" (or "heard") on the corresponding motif, so that the familiar properties and structures on particularized cohomological invariants (*l-adic* or crystalline, for example), would simply be the faithful reflection of the **internal** properties and structures **of the motif**<sup>59</sup>.

p. P47

□ This, expressed in the non-technical language of a musical metaphor, is the quintessential idea of a childlike simplicity, delicate and audacious at the same time. I developed this idea, on the bangs of the foundational tasks I considered more urgent, under the name of "theory of motifs" or "philosophy (or "yoga") of motifs", throughout the years 1963-69. It's a theory of fascinating structural richness, much of which is still conjectural<sup>60</sup>.

I've written several times in *Récoltes et Semailles* about this "yoga of motives", which is very important to me.

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endomorphism of the cohomology space  $H^i(X)$ , the "characteristic polynomial" of the latter had to have **integer** coefficients, not depending on the particular cohomological theory chosen (e.g.: *l-adic*, for *l* variable). The same applies to general algebraic correspondences, when *X* is assumed to be clean and smooth. The sad truth (and one that gives an idea of the lamentable state of abandonment of the cohomological theory of algebraic varieties in characteristic  $p > 0$ , since I left) is that the thing still hasn't been demonstrated to this day, even in the particular case where *X* is a projective and smooth **surface** and  $i = 2$ . In fact, to the best of my knowledge, no one after my departure has yet deigned to address this crucial question, typical of those that appear subordinate to standard conjectures. The decree of fashion is that the only endomorphism worthy of attention is the Frobenius endomorphism (which Deligne was able to treat separately, using whatever means at his disposal. . . ).

<sup>59</sup>(For the mathematical reader) Another way of looking at the category of patterns over a *k-body* is to visualize it as follows as a kind of "wraparound abelian category" of the category of separate finite type patterns over *k*. The associated pattern to such a scheme *X* (or "motivic cohomology of *X*", which I call  $H^*_{\text{wor}}(X)$ ) thus appears as a kind of "avatar" of *X*. The crucial thing here is that, just as an algebraic variety *X* is susceptible to "continuous variation" (its isomorphism class therefore depends on continuous "parameters", or "modules"), so too is the pattern associated with *X*, or more generally, a "variable" pattern, susceptible to continuous variation. This is an aspect of motivic cohomology that stands in stark contrast to all classical cohomological invariants, including *l-adic* invariants, with the sole exception of the Hodge cohomology of complex algebraic varieties.

This gives an idea of the extent to which "motivic cohomology" is a more fine invariant, encircling the "arithmetic form" (if I may venture this expression) of *X* much more tightly, than the traditional purely topological invariants. In my vision of patterns, these constitute a kind of very hidden and delicate "cord", linking the algebraic-geometric properties of an algebraic variety to properties of an "arithmetic" nature embodied by its pattern. The latter can be considered a "geometric" object in spirit, but one in which the "arithmetical" properties subordinate to geometry are, so to speak, "laid bare".

Thus, the motif appears to me as the most profound "invariant of form" that we have yet been able to associate with an algebraic variety, apart from its "motivic fundamental group". For me, both invariants are like the "shadows" of an algebraic variety.

of a "type of motivic homotopy" yet to be described (and about which I say a few words in passing in the note "Le tour des chantiers - ou outils et vision" (ReS IV, n° 178, see chantier 5 (Motifs), and in particular page 1214)). It is this last object that seems to me to be the most perfect embodiment of the elusive "arithmetic form" (or "motivic") intuition of a any algebraic variety.

<sup>60</sup>Over the years, I've explained my vision of the motives to anyone who would listen, without bothering to publish anything about it in black and white (not lacking other tasks at the service of all). This later enabled some of my students to plunder more at ease, under the watchful eye of all my old friends, who were well aware of the situation. (See b. de p. note below. )



particularly close to my heart. This is not the place to go into what I've said elsewhere. Suffice it to say that the "standard conjectures" flow most naturally from this yoga of motifs. At the same time, they provide a principle of approach for one of the possible formal constructions of the notion of motif.

These conjectures appear to me, and still do today, as one of the two most fundamental questions in algebraic geometry. Neither this question, nor the other equally crucial one (the so-called "resolution of singularities") has yet been resolved. But while

the second of these questions appears, today as it did a hundred years ago, as a prestigious and formidable one, the one I had the honor of *dégager s'est vu classer par les péremptoires décrets de la mode* (dès p. 48 the years following my departure from the mathematical scene, and just like the motivational theme itself<sup>61</sup>) as a kind of Grothendieckian smoke and mirrors. But once again I anticipate...

## 2.17. Discovering the Mother - or the two slopes

To tell the truth, my reflections on Weil's conjectures themselves, with a view to establishing them, remained sporadic. The panorama that had begun to open up before me, and which I was striving to scrutinize and capture, far exceeded in breadth and depth the hypothetical needs of a demonstration, and even all that these famous conjectures had initially been able to hint at. With the emergence of the schematic theme and that of topos, a new and unsuspected world had suddenly opened up. "Conjectures" occupied a central place, not unlike that of the capital of a vast empire or continent, with its countless provinces, most of which have only the most distant connections with this brilliant and prestigious place. Without ever having to admit it to myself, I knew I was now the servant of a great task: to explore this immense and unknown world, to grasp its contours right up to its most distant frontiers; and also, to travel in all directions and inventory with tenacious and methodical care the nearest and most accessible provinces, and draw up maps of scrupulous fidelity and precision, where the smallest hamlet and thatched cottage would have its place. ...

It was this latter work in particular that absorbed most of my energy - a patient and vast work on the foundations that only I could see clearly and, above all, "feel with my gut". It took up by far the largest part of my time, between 1958 (the year in which the schematic and topos themes appeared, one after the other) and 1970 (the year of my departure from the mathematical scene).

I often gnawed at myself for being held back like that, as if by a tenacious, sticky weight, with those interminable tasks which (once I'd got the gist of them) seemed to me more akin to "stewardship", than to a "job".

launched into the unknown. Constantly I had to hold back this impulse to rush forward - *celle du pionnier* p. P49 or the explorer, out to discover and explore unknown and nameless worlds, constantly calling me to know and name them. This impulse, and the energy I invested in it (as if by stealth, almost!), were constantly at a premium.

Yet I knew deep down that it was this energy, stolen (so to speak) from that which I owed to my "tasks", that was of the rarest and most delicate essence - that the "creation" in my work as a mathematician was above all to be found **there**: in this intense attention to apprehend, in the "creation" of my work.

<sup>61</sup>In fact, this theme was exhumed in 1982 (one year after the crystalline theme), this time under its original name (and in a narrowed form, in the sole case of a base body of zero characteristic), without the worker's name being mentioned. This is just one example, among many, of a notion or theme that was buried as a Grothendieckian phantasmagoria in the aftermath of my departure, only to be exhumed one after the other by some of my students over the following ten or fifteen years, with modest fidelity and (need I say it again) without any mention of the worker... .

the dark, shapeless, clammy folds of a warm, inexhaustible nurturing matrix, the first traces of form and outline of that which had not yet been born and which seemed to be calling me, to take shape and become incarnate and born. ... In the work of discovery, this intense attention, this ardent solicitude, is an essential force, like the warmth of the sun for the obscure gestation of seeds buried in the nourishing earth, and for their humble and miraculous blossoming in the light of day.

In my work as a mathematician, I see at work above all these two forces or impulses, equally profound, of (it seems to me) different natures. To evoke both, I've used the image of the **builder**, and that of the **pioneer** or explorer. Placed side by side, both suddenly strike me as very "yang", very "masculine", even "macho"! They have the haughty resonance of myth, or that of "special occasions". Surely they're inspired by the remnants, in me, of my old "heroic" vision of creative work, the super-yang vision. As they are, they give a strongly tinted, not to say fixed, "at attention" vision of a much more fluid, humble, "simple" reality - a **living** reality.

In this masculine "builder's" impulse, which seems to be constantly pushing me towards new building sites, I can also discern the **homebody's** impulse: that of someone deeply attached to "home". First and foremost, it's "**his**" house, the home of "**loved ones**" - the place of an intimate living entity of which he feels a part. Only then, as the circle of what is felt to be "close" widens, is it also a "home for all". And in this impulse to "make homes" (as one would "make" love. . . ) there is also, and above all, **tenderness**. There's the impulse of **contact** with these materials that we shape one by one,

p. P50 with loving care, and that we only really know through this loving contact. And, once the walls are up and the beams and roof have been laid, there's the profound satisfaction of installing one room after another, and seeing little by little

the harmonious order of the living home - beautiful, welcoming, good to live in. For **the house**, first and foremost and secretly in each of us, is also **the mother** - that which surrounds us and shelters us, both refuge and comfort; and perhaps (even more profoundly, and even while we're in the process of building it from scratch) it's also that from which we ourselves came, that which sheltered and nourished us, in those forever-forgotten times before our birth. ... It's also the **Giron**.

And the image that spontaneously appeared earlier, to go beyond the prestigious appellation of "pioneer" and identify the more hidden reality it covered, was also stripped of any "heroic" accent. Here again, it was the archetypal maternal image that came to the fore - that of the nurturing "matrix" and its shapeless, obscure labors. ...

These two impulses, which seemed to me to be "different in nature", are actually closer than I thought. Both are in the nature of a "**contact impulse**", leading us to meet "**the Mother**": the One who embodies **both what is close, "known", and what is "unknown"**. To abandon myself to one or other of these impulses is to "find the Mother". It means renewing contact with both the near, the "more or less known", and the "**far-off**", with what is "unknown" but at the same time prescient, on the verge of becoming known.

The difference here is one of tonality, of dosage, not of nature. When I "build houses", it's the "known" that dominates, and when I "explore", it's the unknown. These two "modes" of discovery, or better said, these two aspects of the same process or work, are indissolubly linked. They are both essential and complementary. In my mathematical work, I see a back-and-forth movement

constant between these two modes of approach, or rather, between the moments (or periods) when one predominates, and those when the other predominates<sup>62</sup>. But it's also clear that in every moment, both one and the other mode is present.

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<sup>62</sup>What I say here about mathematical work is also true of "meditative" work (which will be discussed throughout Harvest and Sowing). There's little doubt in my mind that this is something that appears in every work of

When I'm building, landscaping, clearing, cleaning, ordering, it's the "mode" or "yang" or "masculine" side of my work that sets the tone. When I'm groping for the elusive, the formless, the nameless, I'm the "yin" or "feminine" side of my being.

There's no question of me minimizing or denying either side of my nature, both are essential - the "masculine" that builds and begets, and the "feminine" that conceives and shelters the slow, obscure gestations. I "am" both - "yang" and "yin", "male" and "female". But I also know that the most delicate, delicate essence in creative processes lies on the side of the "yin", "feminine" side - the humble, obscure, and often poor-looking side.

I think it's this side of work that has always held the most powerful fascination for me. The prevailing consensus, however, encouraged me to invest most of my energy in the other side, that which is embodied and asserted in tangible, not to say finished and completed "products" - products with clear-cut contours, attesting to their reality with the evidence of carved stone... .

Looking back, I can see how this consensus weighed on me, and also how I "bore the brunt" of it. - flexible! The "conception" or "exploration" part of my work was kept to a minimum, right up until the moment of my departure. And yet, as I look back on my work as a mathematician, it's strikingly clear that the essence and power of this work lies in the neglected, if not derided or condescended-to side of the equation: that of "**ideas**", or even "**dreams**", not "results". In these pages, as I try to pinpoint the most essential contribution I have made to the mathematics of my time, with a view that embraces a forest, rather than lingering on trees - I have seen, not a list of "great theorems", but a lively array of fertile ideas<sup>63</sup>, all contributing to the same vast vision.

## 2.18. The child and Mother

□ When this "foreword" began to turn into a stroll through my work as a mathematician, p. p52 with my little talk about "heirs" (good-natured) and "builders" (incorrigible), a name also began to emerge for this missed foreword: it would be "L'enfant et le bâtisseur" ("The child and the builder"). Over the following days, it became increasingly clear that "the child" and "the builder" were one and the same character. The name thus became, more simply, "L'enfant bâtisseur". A name, I must say, that was not lacking in allure, and one that had everything to please me!

But reflection reveals that this haughty "builder", or (more modestly) the child-who-played-at-making-houses, was just one face of the famous child-who-played, who had **two**. There's also the child-who-likes-exploring-things, snooping and burrowing in sands and mud.

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discovery, including that of the artist (writer or poet, let's say). The two "sides" I'm describing here can also be seen as being, on the one hand, that of **expression** and its "technical" demands, and on the other, that of **reception** (of perceptions and impressions of all kinds), becoming **inspiration** through the effect of intense attention. Both are present at every moment of the work, and there is a constant to-and-fro between the "times" when one predominates, and those when the other predominates.

<sup>63</sup>It's not that what might be called "great theorems" are lacking in my work, including theorems that solve questions posed by others than myself, that no one before me had been able to solve. (I review some of them in the b. de p. note (\*\*\*) page 554, from the note "The rising sea. ... " (ReS III, n° 122).) But, as I pointed out at the outset of this "walk" (in the "Points of view and vision" stage, n° 6), these theorems only make sense to me through the nourishing context of a great theme, initiated by one of these "fertile ideas". Their demonstration then follows, as from The waves of the river seem to spring gently from the very depth of its waters, without rupture or effort. I express myself in a very similar sense, but with other images, in the note already quoted "La mer qui monte. ...".

## 2. A walk through a work or the Child and Mother

muddy and nameless, the most impossible and bizarre places. . . No doubt to give the impression (if only to myself. . . ), I began by introducing him under the flamboyant name of "pioneer", followed by the more down-to-earth but still prestigious "explorer". It made you wonder, between the "builder" and the "pioneer-explorer", which was the more masculine, the more alluring of the two! Heads or tails? And then, on closer inspection, our intrepid "pioneer" turned out to be a **girl** (whom I'd been happy to dress as a boy) - a sister of ponds, rain, drizzle and night, silent and almost invisible by dint of fading into the shadows - the one we always forget (if we don't pretend to laugh at her. . . ). And I too found a way, for days and days, to forget her - to forget her doubly, I might add: at first, I'd only wanted to see the boy (the one who plays at making houses...) - and even when I couldn't stop myself from seeing **the other one**, I still saw her as a boy too...

As for the beautiful name for my walk, well, it doesn't hold up at all. It's an all-in-one name. yang, all macho, a nom-qui-boite. To keep it straight, you'd have to include **the other**

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as well. But, strangely enough, "**the other**" **doesn't really have a** ☐ **name**. The only one that fits at all is "explorer", but it's still a boy's name, nothing to do with it. Language is a bitch here, trapping us without our even realizing it, obviously in cahoots with age-old prejudices.

Perhaps we could get away with "L'enfant-qui-bâtît et l'enfant-qui-explore". Leaving unsaid that one is a "boy" and the other a "girl", and that it's one and the same boy-girl child who, in building, explores, and in exploring, builds... . But yesterday, in addition to the double yin-yang aspect of what contemplates and explores, and what names and builds, another aspect of things had appeared.

The Universe, the World, even the Cosmos, are basically foreign and very distant things. They don't really concern us. It's not towards **them** that our deepest impulse for knowledge leads us. What attracts us is their tangible and immediate **Incarnation**, the closest, the most "carnal", charged with deep resonance and rich in mystery - the One who merges with the origins of our being of flesh, as with those of our species - and the One who has always waited for us, silent and ready to welcome us, "at the other end of the road". It is **from her**, the Mother, the One who gave birth to us as she gave birth to the world, that the impulse is born and that the paths of desire take off - and it is to **her** that they lead us, to **her** that they take off, to return unceasingly and to sink into her.

And so, at the bend in the path of an unplanned "walk", I unexpectedly come across a parable that was once familiar to me, but which I had somewhat forgotten - the parable of **the Child and the Mother**. It can be seen as a parable for "**Life, in search of itself**". Or, on the more humble level of individual existence, a parable for "**Being, in search of things**".

It's a parable, but it's also the expression of an ancestral experience, deeply rooted in the psyche - the most powerful of the original symbols that nourish the deepest creative layers. I believe I recognize in it, expressed in the immemorial language of archetypal images, the very breath of creative power in man, animating his flesh and spirit, in its humblest and most ephemeral manifestations, as well as its most dazzling and enduring ones.

This "breath", like the carnal image that embodies it, is the most humble thing in the world. It is also the most fragile thing, the most ignored by all and the most despised... ..

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☐ And the story of the vicissitudes of this breath over the course of your existence is none other than **your** adventure, the "adventure of knowledge" in **your** life. The wordless parable that expresses this is that of the Child and the Mother.

You are the child, born of the Mother, sheltered in Her, nourished by Her power. And the child rises from the Mother, the

Nearby, the Well-Known - to meet the Mother, the Unlimited, forever Unknown and full of mystery. ...

End of the "Walk through a work of art" tour

## Epilogue: Invisible circles

### 2.19. Death is my cradle (or three little ones for a dying )

Until the emergence of the topos point of view, towards the end of the 1950s, the evolution of the notion of space seems to me to have been essentially "**continuous**". It seems to have proceeded smoothly, starting with the Euclidean theorization of the space that surrounds us, and the geometry bequeathed to us by the Greeks, focusing on the study of certain "figures" (straight lines, planes, circles, triangles etc.) living in this es- pace. Admittedly, there have been profound changes in the way the mathematician or "natural philosopher" conceives of "space"<sup>64</sup>. But these changes all seem to me to be in the nature of an essential "continuity" - they never placed the mathematician, attached (like everyone else) to familiar mental images, before a sudden **disorientation**. They were like the changes, profound perhaps but pro- gressive, that take place over the years in a being we would have known as a child, and whose evolution we would have followed from its first steps to adulthood and full maturity. Changes that are imperceptible in certain long periods of calm, and tumultuous perhaps in others. But even in the most intense periods of growth or ripening, and even when we've lost sight of it, we're still able to see it.

months, even years, at no time could there be the slightest doubt, however, that the

□no hesitation: it was indeed him again, a well-known and familiar being, that we were meeting up with, albeit with p .

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changed features.

In fact, I think I can say that by the middle of this century, this familiar creature had already aged considerably. - like a man who had finally worn himself out, overwhelmed by an influx of new tasks for which he was in no way prepared. Perhaps he'd already died a natural death, without anyone bothering to take note and acknowledge it. "Everyone" was still so busy in the house of a living person, that it was almost as if he were still very much alive indeed.

And so, judge the unfortunate effect, for the regulars of the house, when in place of the venerable old man, frozen, straight and stiff in his armchair, we suddenly see a vigorous kid, no taller than three apples, and who claims in passing, without laughing and as a matter of course, that Monsieur Espace (and you can even drop the "Monsieur", at your ease. . . ) is **him**! If only he looked like he had the family traits, a natural child perhaps, who knows... but not at all! From the looks of it, nothing like the old Father Espace we'd known so well (or thought we'd known...), and of whom we were sure, in any case (and that was the least we could do...) that he was eternal...

**This is** the famous "mutation of the notion of space". This is what I must have "seen", as a matter of course, from at least the early sixties, without ever having had the opportunity to formulate it to myself before this very moment when I'm writing these lines. And I suddenly see with new clarity, by the very virtue of this

<sup>64</sup>My initial intention in writing the Epilogue had been to include a very brief sketch of some of these "profound changes", and to bring out the "essential continuity" I see in them. I've decided against it, so as not to overextend this Promenade, which is already much longer than planned! I intend to come back to it in the Historical Commentaries planned for volume 4 of "Reflections", this time for a mathematician reader (which totally changes the task of exposition).

The traditional notion of "space", like the closely related "variety" (of all kinds, including "algebraic variety"),

had become so old by the time I came around, it was as if they were dead. <sup>65</sup>. And I could say that it's with the appearance of stroke after stroke from the point of view of the

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schemas (and its offspring<sup>66</sup>, plus ten thousand pages of foundations), and then of topos, that a situation of crisis-which-doesn't-say-its-names was finally resolved.

In today's image, we're not talking about one kid, as the product of a sudden mutation, but two. Two kids, moreover, who have an unmistakable "family resemblance" to each other, even if they bear little resemblance to the late old man. And if you look closely, you could even say that the Schémas toddler is like a "kinship link" between the late Père Espace (aka Variétés-en-tous-genres) and the Topos toddler<sup>67</sup>.

## 2.20. A look at our neighbors face

The situation seems to me very similar to that which arose at the beginning of this century, with the appearance of Einstein's theory of relativity. There was an even more glaring conceptual cul-de-sac in the form of a sudden **contradiction**, which seemed irresolvable. Fittingly, the new idea that would bring order to the chaos was one of childlike simplicity. The remarkable thing (and in keeping with a most repetitive scenario. . . ) was that, of all the brilliant, eminent, prestigious people who were suddenly on their toes, trying to "save the furniture", no one had thought of the idea. It had to be an unknown young man, fresh (if that's possible) from the benches of the student amphitheatres, who came (perhaps a little embarrassed by his own audacity. . . ) to explain to his illustrious elders what had to be done to "save the furniture".

"save the phenomena": just separate space from time<sup>68</sup> ! Technically, everything was in place for this idea to emerge and be accepted. And it's to the credit of Einstein's predecessors that they knew how to do it.

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Indeed, they welcomed the new idea without too much mortification. A sign that it was still a great time... .

From a mathematical point of view, Einstein's new idea was trivial. From the point of view of our conception of **physical space**, on the other hand, it was a profound mutation, and a sudden "change of scenery". The first

<sup>65</sup>This assertion (which will seem preemptory to some) should be taken with a "grain of salt". It is no more or less valid than the assertion (which I take up again below) that the "Newtonian model" of mechanics (terrestrial or celestial) was "moribund" at the beginning of this century, when Einstein came to the rescue. It's a fact that even today, in most "common" situations in physics, the Newtonian model is perfectly adequate, and it would be madness (given the admitted margin of error in the measurements made) to go looking for relativistic models. Similarly, in many mathematical situations, the old familiar notions of "space" and "variety" remain perfectly adequate, without going in search of nilpotent elements, topos or "moderated structures". But in both cases, for a growing number of contexts involved in cutting-edge research, the old conceptual frameworks have become inadequate to express even the most "common" situations.

<sup>66</sup>(For the mathematician's benefit) In this "progeny", I include formal diagrams, "multiplicities" of all kinds (and in particular, schematic or formal multiplicities), and the so-called "rigid-analytic" spaces (introduced by Tate, following a "master plan" provided by me, inspired by the new notion of topos, as well as that of formal diagram). This list is by no means exhaustive. ...

<sup>67</sup>In addition to these two toddlers, there's a third, younger one, who has appeared in less clement times: the Espace Modéré brat. As I've pointed out elsewhere, he wasn't entitled to a birth certificate, and it's in total illegality that I've nevertheless included him among the twelve "master themes" I've had the honor of introducing into mathematics.

<sup>68</sup>It's a bit short, of course, as a description of Einstein's idea. On a technical level, it was necessary to show what structure to put on the new space-time (although this was already "up in the air", with Maxwell's theory and Lorenz's ideas). The essential step here was not technical, but "**philosophical**": the realization that the notion of simultaneity for distant events had no experimental reality. This was the "childish realization", the "but the Emperor is naked!", that broke through the famous "imperious and invisible circle that limits a Universe". . .

mutation of its kind, since the mathematical model of physical space devised by Euclid 2,400 years ago, and taken up unchanged for the needs of mechanics by all physicists and astronomers since antiquity (including Newton), to describe terrestrial and stellar mechanical phenomena.

Einstein's initial idea subsequently deepened, becoming embodied in a subtler, richer and more flexible mathematical model, drawing on the rich arsenal of mathematical notions already in existence<sup>69</sup>. With the "theory of generalized relativity", this idea expanded into a vast **vision** of the physical world, embracing in a single view the subatomic world of the infinitely small, the solar system, the Milky Way and distant galaxies, and the path of electromagnetic waves in a space-time curved at every point by the matter within it<sup>70</sup>. This is the second and last time in the history of cosmology and physics (following Newton's first great synthesis three centuries ago) that a vast unifying vision has emerged, in the language of a mathematical model, of all physical phenomena in the Universe.

This Einsteinian vision of the physical Universe was in turn overtaken by events.

The "set of physical phenomena" we're trying to account for has had time to grow, since the beginning of the century! A multitude of physical theories have appeared, each one to account for, □with more p. P58 or less successful, of a limited set of facts, in the immense hodgepodge of all the "observed facts". And we're still waiting for the daring kid who will playfully find the new key (if there is one. . .), the dreamed-of "model-cake" willing to "work" to save all the phenomena at once. . .<sup>71</sup>

<sup>69</sup>It's all about the notion of "Riemannian variety", and tensor calculus on such a variety.

<sup>70</sup>One of the most striking features that distinguishes this model from the Euclidean (or Newtonian) model of space and time, and also from Einstein's very first model ("special relativity"), is that the **overall topological form** of space-time remains undetermined, rather than being imperatively prescribed by the very nature of the model. As a mathematician, the question of what this global form is seems to me to be one of the most fascinating in cosmology.

<sup>71</sup>Such a hypothetical theory, which would "unify" and reconcile the multitude of partial theories mentioned above, has been called a "unitary theory". I have the feeling that the fundamental reflection that awaits us will have to take place on two different levels.

1° ) A "philosophical" reflection on the very notion of a "mathematical model" for a portion of reality. Since the success of Newtonian theory, it has become a tacit axiom for physicists that **there is** such a thing as a mathematical model. (indeed, a single model, or "**the**" model) to express physical reality perfectly, without "detachment" or burrs. This consensus, which has been the law for over two centuries, is a kind of fossilized vestige of Pythagoras' vivid vision that "All is number". Perhaps this is the new "invisible circle", which has replaced the old metaphysical circles to limit the physicist's Universe (while the race of "philosophers of nature" seems defiantly extinct, superseded hands down by that of computers... ). However, it's clear that the validity of this consensus is far from self-evident. There are even very serious philosophical reasons to doubt it a priori, or at least to set very strict limits on its validity. This would be the perfect time to subject this axiom to a close critique, and perhaps even to "demonstrate", beyond any possible doubt, that it is unfounded: that there is no single rigorous mathematical model that accounts for all the so-called "physical" phenomena recorded to date.

Once the very notion of a "mathematical model" has been satisfactorily defined, as has the "validity" of such a model (within the limits of such "margins of error" admitted in the measurements made), the question of a "unitary theory" or at least that of an "optimum model" (in a sense yet to be specified) will be clearly posed. At the same time, we'll no doubt have a clearer idea of the degree of arbitrariness involved (by necessity, perhaps) in choosing such a model.

2° ) It seems to me that it's only **after** such reflection that the "technical" question of identifying an explicit model, more satisfactory than its predecessors, takes on its full meaning. It would be a good time, then, perhaps, to take up a second axiom. The physicist's unspoken assumption, dating back to antiquity, is deeply rooted in our very way of perceiving space: it's the **continuous nature** of space and time (or space-time), the "place" where "physical phenomena" take place.

Fifteen or twenty years ago, while leafing through the modest volume constituting Riemann's complete works, I was struck by a remark he made "in passing". In it, he observed that the ultimate structure of space may well be "discrete", and that the "continuous" representations we make of it may well be a simplification (excessive, perhaps, in the long run. . . ) of a more complex reality; that for the human mind, "the continuous" was easier to grasp than "the discontinuous", and that it serves us, therefore, as an "approximation" to apprehend the discontinuous. This is a remark of surprising penetration from a mathematician, at a time when the Euclidean model of physical space

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□The comparison between my contribution to the mathematics of my time, and Einstein's to physics, came to me for two reasons: both works are accomplished through a **mutation of our conception of "space"** (in the mathematical sense in one case, in the physical sense in the other); and both take the form of a **unifying vision**, embracing a vast multitude of phenomena and situations that until then had appeared as separate from one another. I see an obvious **kinship of spirit** between his work<sup>72</sup> and mine.

I don't think this kinship is in any way contradicted by an obvious difference in "**substance**". As I suggested earlier, the Einsteinian mutation concerns the notion of physical space, whereas Einstein drew on the arsenal of mathematical notions already known to him, without ever needing to expand or even overturn it. His contribution consisted in identifying, from among the mathematical structures known at the time, those that were best suited to<sup>73</sup> serving as "models" for the world of physical phenomena, in place of, and in addition to, the mathematical structures of his time.

the moribund model bequeathed by his predecessors. In this sense, his work was indeed that of a **physicist**, and beyond that, that of a "**philosopher of nature**", in the sense understood by Newton and his contemporaries

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This "philosophical" dimension is absent from my mathematical work, where I've never been led to question the possible relationships between the "ideal" conceptual constructions taking place in the Universe of mathematical things, and the phenomena taking place in the physical Universe (or even, the lived events taking place in the psyche). My work has been that of a **mathematician**, deliberately turning away from the question of "applications" (to other sciences), or the "motivations" and psychic roots of my work. A mathematician, moreover, driven by his very particular genius to constantly expand the arsenal of notions at the very basis of his art. In this way, I was led, without even realizing it and as if at play, to overturn the most fundamental notion of all for the geometer: that of **space** (and that of "variety"), i.e. our conception of the very "**place**" where geometric beings live. The new notion of space (as a kind of "generalized space", but where the points that are supposed to form "space" have more or less disappeared) bears no resemblance in substance to the notion brought by Einstein to physics (which is by no means confusing for the mathematician). By contrast, the comparison is with **quantum mechanics** as discovered by **Schrödinger**<sup>74</sup>. In this new mechanics, the traditional "material point" disappears, to be replaced by a kind of "probabilistic cloud", more or less dense from one region of the surrounding space to another, depending on the "probability" that the point is in that region.

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had never yet been called into question; in a strictly logical sense, it is rather the discontinuous that has traditionally served as the technical approach to the continuous.

Developments in mathematics over the last few decades have shown a much closer symbiosis between continuous and discontinuous structures than was imagined in the first half of this century. The fact remains that finding a "satisfactory" model (or, if necessary, a set of such models, "connecting" as satisfactorily as possible. . . ), whether "continuous", "discrete" or of a "mixed" nature - such a task will surely require a great deal of conceptual imagination, and a consummate flair for apprehending and uncovering mathematical structures of a new kind. This kind of imagination or "flair" seems to me to be a rare thing, not only among physicists (where Einstein and Schrödinger seem to have been among the rare exceptions), but even among mathematicians (and here I speak with full knowledge of the facts).

To sum up, I predict that the expected renewal (if it is still to come. . . ) will come more from a mathematician at heart, well-informed about the great problems of physics, than from a physicist. But above all, it will require a man with the "philosophical openness" to grasp the crux of the problem. This is by no means a technical problem, but a fundamental one of "natural philosophy".

<sup>72</sup>I make no claim to be familiar with Einstein's work. In fact, I haven't read any of his work, and know his ideas only by hearsay and very roughly. Yet I seem to be able to discern "the forest", even though I've never made the effort to peer into any of its trees... ..

<sup>73</sup>For comments on the qualifier "moribund", see a previous footnote (note page 55 ).

<sup>74</sup>I understand (from echoes that have come back to me from various quarters) that this century is generally considered to have seen three "revolutions" or major upheavals in physics: Einstein's theory, the Curies' discovery of radioactivity, and Schrödinger's introduction of quantum mechanics.



region. From this new perspective, we can sense an even more profound "mutation" in the way we conceive of mechanical phenomena, than that embodied by Einstein's model - a mutation that doesn't simply consist of replacing a mathematical model that's a little tight around the edges, with another similar one, but cut wider or with a better fit. This time, the new model bears so little resemblance to the good old traditional models, that even the mathematician who is a great specialist in mechanics must have felt suddenly out of place, even lost (or outraged. . . ). For a mathematician, switching from Newtonian mechanics to Einstein's must be a bit like switching from the good old Provençal dialect to the latest Parisian slang. On the other hand, switching to quantum mechanics, I imagine, is like going from French to Chinese.

And these "probabilistic clouds", replacing the reassuring material particles of yesteryear, remind me strangely of the elusive "open neighborhoods" that ☐ populate the topos, like evanescent ghosts, to surroundp . P61  
imaginary "points" to which a recalcitrant imagination continues to cling against all odds... .

## 2.21. "L'unique" - or the gift of solitude

This brief excursion to the "neighbors across the street", the physicists, may serve as a point of reference for a reader who (like most people) knows nothing of the world of mathematicians, but who has surely heard of Einstein and his famous "fourth dimension", or even of quantum mechanics. After all, even if it wasn't foreseen by the inventors that their discoveries would result in Hi- roshimas, and later in both military and (supposedly) "peaceful" atomic bombs, the fact remains that discoveries in physics have a tangible, almost immediate impact on the human world in general. The im- pact of mathematical discoveries, especially in so-called "pure" mathematics (i.e., with no "applications" in view), is less direct, and certainly trickier to pin down. I'm not aware, for example, that my contributions to mathematics have been "used" to build anything, let's say any kind of machine. I don't deserve any credit for that, of course, but it does reassure me. As soon as there are applications, you can be sure that it's the military (and after them, the police) who are the first to get hold of them - and as far as industry (even so-called "peaceful" industry) is concerned, it's not always so much better. ...

Certainly for my own sake, or for that of a mathematician reader, it would be better to try and situate my work by "landmarks" in the history of mathematics itself, rather than looking for analogies elsewhere. I've been thinking about this over the last few days, within the limits of my rather vague knowledge of the history in question<sup>75</sup> . Already during the "Promenade", I had the opportunity to mention a "lineage" of mathematicians, of a temperament in which I recognize myself: Galois, Riemann, Hilbert. If I were better acquainted with the history of my art, chances are I'd find it ☐ prolong this lineage more

p. P62

The only thing that struck me was that I can't recall ever hearing of a mathematician, even by hint, from friends or colleagues better versed in history than myself. The thing that struck me is that I don't recall being aware, even if only by hint from friends or colleagues better versed in history than myself, of a mathematician apart from myself who contributed a multiplicity of innovative ideas, not more or less disjointed from one another, but as part of a vast unifying vision (as was the case with Newton and Einstein in physics and cosmology),

<sup>75</sup>Ever since I was a kid, I've never been too keen on history (or geography, for that matter). (In the fifth part of *Récoltes et Semailles* (written only in part), I have the opportunity "en passant" to detect what seems to me the deepest reason for this partial "block" against history - a block that is being resolved, I believe, in recent years). The mathematical education received by my elders in the "Bourbach circle" didn't help matters any.  
- the occasional historical reference was more than rare.

and for Darwin and Pasteur in biology). I'm only aware of two "moments" in the history of mathematics when a vast new vision was born. One is the birth of mathematics as a science in the sense we understand it today, 2500 years ago in ancient Greece. The other is, above all, the birth of infinitesimal and integral calculus, in the seventeenth century, a period marked by the names of Newton, Leibnitz, Descartes and others. As far as I know, the vision born at either moment was not the work of a single individual, but the collective work of an era.

Of course, between the time of Pythagoras and Euclid and the beginning of the seventeenth century, mathematics had had time to change its face, and likewise between that of the "Calcul des infiniments petits" created by the mathematicians of the seventeenth century, and the middle of the present nineteenth. But as far as I know, the profound changes that took place during these two periods, one over two thousand years and the other three centuries, never materialized or condensed into a new vision expressed in a given work<sup>76</sup>, in a similar way to what happened in physics and cosmology with the great syntheses of Newton, then Einstein, at two crucial moments in their history.

p. P63 □ It would seem that as the servant of a vast unifying vision born in me, I am "unique in my kind" in the history of mathematics from its origins to the present day. I'm sorry if I seem to be trying to single myself out more than I should! To my own relief, however, I think I can discern a sort of potential (and providential!) **brother**. I've already had occasion to mention him, as the first in the line of my "temperament brothers": **Evariste Galois**. In his short and dazzling life<sup>77</sup>, I think I can discern the beginnings of a great vision - that of the "marriage of number and greatness", in a new geometric vision. Elsewhere in *Récoltes et Semailles*<sup>78</sup>, I describe how, two years ago, this sudden intuition appeared in me: that in the mathematical work that at the time held the most powerful fascination for me, I was in the process of "taking up the legacy of Galois". This intuition, rarely mentioned since, has nevertheless had time to mature in silence. The retrospective reflection on my work that I've been pursuing for the past three weeks will surely have contributed to this. The most direct filiation I now recognize with a mathematician of the past is with Evariste Galois. Rightly or wrongly, it seems to me that the vision I developed over fifteen years of my life, and which has continued to mature within me and to grow richer in the sixteen years since I left the mathematical scene - that this

<sup>76</sup>Hours after writing these lines, it struck me that I hadn't thought here of the vast synthesis of contemporary mathematics that Mr. Bourbaki's (collective) treatise endeavors to present. (The Bourbaki group will be discussed at length again in the first part of *Récoltes et Semailles*). There are, it seems to me, two reasons for this.

On the one hand, this synthesis is limited to a sort of "tidying-up" of a vast body of ideas and results already known, without adding any new ideas of its own. If there is a new idea, it would be that of a precise mathematical definition of the notion of "structure", which has proved to be a valuable thread running through the entire treatise. But this idea seems to me to be more akin to that of an intelligent and imaginative lexicographer, than to an element of language renewal, giving a renewed apprehension of reality (here, that of mathematical things).

On the other hand, from the 1950s onwards, the idea of structure was overtaken by events, with the sudden influx of "categorical" methods into some of the most dynamic parts of mathematics, such as topology and algebraic geometry. (Thus, the notion of "topos" refuses to fit into the "Bourbachian bag" of structures, which is decidedly narrow to the entours!) In deciding, admittedly with full knowledge of the facts, not to embark on this "galley", Bourbaki thereby renounced his initial ambition, which was to provide the foundations and basic language for the whole of contemporary mathematics.

He did, however, fix a language and, at the same time, a certain style of writing and approach to mathematics. This style was originally a (very partial) reflection of a certain spirit, a living and direct inheritance from Hilbert. Over the course of the fifties and sixties, this style came to the fore - for better and (above all) for worse. In the last twenty years or so, it has become a rigid "canon" of purely facade "rigor", whose original spirit seems to have disappeared without return.

<sup>77</sup>Evariste Galois (1811-1832) died in a duel at the age of twenty-one. There are, I believe, several biographies of him. As a young man, I read a fictionalized biography written by the physicist Infeld, which really struck me at the time.

<sup>78</sup>See "L'héritage de Galois" (ReS I, section 7).

vision is also one that Galois could not have refrained from developing<sup>79</sup>, had he been in the vicinity at my place, and without an early death brutally cutting short a magnificent momentum.

There's another reason, surely, that contributes to my feeling of "essential kinship".

- of a kinship that cannot be reduced to a mere "mathematical temperament", nor to the striking aspects of a work. Between his life and mine, I also feel a kinship of destinies. Of course, Galois died stupidly, at the age of twenty-one, whereas I'm going on sixty, and determined to live to a ripe old age. However, Evariste Galois remained a **"marginal"** in the official mathematical world during his lifetime, just as I did a century and a half later. In Galois's case, it might seem to a superficial observer that this marginality was "accidental", that he simply hadn't yet had the time to "make his mark" with his innovative ideas and work. In my case, my marginality, during the first three years of my life as a mathematician, was due to my ignorance (deliberate perhaps. . . ) of the very existence of a world of mathematicians, with which I would have to confront myself; and since my departure from the mathematical scene, sixteen years ago, it has been the consequence of a deliberate choice. It is this choice, surely, that has provoked in retaliation an "unfailing collective will" to erase from mathematics all trace of my name, and with it the vision too, of which I had made myself the servant.

But beyond these accidental differences, I believe I can discern a common cause to this "marginality", which I feel is essential. I don't see this cause in historical circumstances, nor in particularities of "temperament" or "character" (which are undoubtedly as different from him to me as they can be from one person to another), and even less in "gifts" (obviously prodigious in Galois, and comparatively modest in me). If there is indeed an "essential kinship", I see it at a much more humble, elementary level.

I've felt such a kinship on a few rare occasions in my life. It is also through her that I feel "close" to yet another mathematician, who was my senior: **Claude Chevalley**<sup>80</sup>. The link I mean is that of a certain "naïveté", or "innocence", which I've had occasion to talk about. It is expressed by a propensity (often unappreciated by those around us) to look at things through our own eyes, rather than through patented glasses, graciously offered by some larger or smaller human group, invested with authority for one reason or another.

This "propensity", or inner attitude, is not the privilege of maturity, but of childhood. It's a gift received at birth, along with life - a gift both humble and formidable. It's a gift that's often buried deep down, but which some people have managed to preserve, or perhaps rediscover. ...

It can also be called **the gift of solitude**.

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<sup>79</sup>I'm convinced that a Galois would have gone much further than I did. On the one hand, because of his exceptional gifts (which I did not share). On the other hand, it's likely that he wouldn't have allowed most of his energy to be distracted, as I did, by endless tasks of meticulously shaping, as we go along, what is already more or less acquired...

<sup>80</sup>I mention Claude Chevalley here and there in *Récoltes et Semailles*, particularly in the section "Rencontre avec Claude Chevalley - ou liberté et bons sentiments" (ReS I section 11), and in the note "Un adieu à Claude Chevalley" (ReS III, note n° 100).

## 2. A walk through a work or the Child and Mother

# 3. A LETTER

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May 1985

## 3.1. The thousand-page letter

□ The text I'm sending you here, typed and printed in a limited number of copies by my université, is neither a separate edition nor a preprint. Its name, Récoltes et Semailles, makes that clear enough. I'm sending it to you as I would send a long letter - a very personal letter, at that. If I'm sending it to you, instead of being content for you to read it one day (if you're curious enough) in some volume on sale in bookshops (if there's a publisher foolish enough to go for it. . . ), it's because I'm addressing it to you more than to others. More than once I've thought of you as I've written this letter - I must say, I've been writing it for more than a year, putting my heart and soul into it. It's a gift I'm giving you, and I've taken great care in writing it to give you the best I have (at any given moment) to offer. I don't know if the gift will be received - your answer (or non-answer. . . ) will let me know. . .

p. L1

Along with you, I'm sending Harvest and Sowing to all my colleagues, friends and (ex-) students in the mathematical world, to whom I've been closely linked at some point, or who figure in my thinking in one way or another, by name or not. Chances are, you're among them, and if you read this with

your heart and not just with your eyes and head, surely you'll recognize yourself even where you're not named. I'm also sending Harvest and Sowing to a few more friends, scientific and otherwise.

This "introductory letter" you are now reading, which announces and introduces you to a "thousand-page letter" (for starters. . . ), will also serve as a Foreword. The latter has not yet been written at the time of writing. *Récoltes et Semailles* consists of five parts (not counting an introduction with "drawers"). I'm sending you parts I (Fatuity and Renewal), II (Burial (1) - or the Robe of the Chinese Emperor), and IV (Burial (3) - or the Four Operations)<sup>1</sup>. These are the ones that I felt particularly concerned you. Part III (Burial (2) - or the Key to Yin and Yang) is undoubtedly the most personal part of my testimony, and the one which, even more than

p. L2 □ les autres, paraît moi avoir une valeur "universelle", au delà des circonstances particulières qui ont entouré sa birth. I refer to this part here and there in Part IV (The Four Operations), which however can be read independently, and even (to a large extent) independently of the three preceding parts<sup>2</sup> (\*). If reading what I've sent you here prompts you to reply (as is my wish), and if it makes you want to read the missing part too, please let me know. I'll be happy to send it to you, provided your reply makes me feel that your interest goes beyond superficial curiosity.

### 3.2. Birth of *Récoltes et Semailles* (a retrospective - )

In this pre-letter, I'd now like to tell you in just a few pages (if I can) what *Récoltes et Semailles* is all about - telling you in more detail than the subtitle alone suggests: "Reflections and testimony on a mathematician's past" (mine, as you may have guessed. . . ). There are many things in *Récoltes et Semailles*, and some will no doubt see in it many different things: a **voyage** of discovery into a past; a **meditation** on existence; a **portrait of the mores of a milieu** and an era (or the picture of the insidious and implacable slide from one era to another. . . ); an **investigation** (at times almost detective-like, at others verging on the cloak-and-dagger novel in the underbelly of the mathematical megalopolis. . . ); a vast **mathematical rambling** (which will sow more than one seed. . . ); a practical treatise on applied psychoanalysis (or, as the case may be, a book of "**psychoanalysis-fiction**"); a panegyric of **self-knowledge**; "**My Confessions**"; a diary; a psychology of **discovery** and **creation**; an **indictment** (merciless, as it should be. . . ), even a **settling of scores** in "the beautiful mathematical world" (and without giving any gifts. . . ). What's certain is that at no time did I feel

bored writing it, when I've learned and seen so much. If your important tasks leave you the leisure to read it' □ I'd be surprised if you got bored reading me. Unless you force yourself, who knows...

p. L3 Obviously, it's not just for mathematicians. It's also true that, at times, it's aimed more at mathematicians than at others. In this pre-letter to the "Harvest and Sowing Letter", I'd like to summarize and highlight what may be of particular interest to you as a mathematician.

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<sup>1</sup>I'd like to single out those colleagues who feature in my reflections in one capacity or another, but whom I don't know personally. I'm simply sending them "Les Quatre Opérations" (which concerns them in particular), together with the "fascicule O" consisting of this letter, and the Introduction to *Récoltes et Semailles* (plus the detailed table of contents of all the first four parts).

<sup>2</sup>(\*)Generally speaking, you'll find that each "section" (in Fatuity and Renewal) or "note" (in any of the following three parts of Harvest and Sowing) has its own unity and autonomy. It can be read independently of the rest, just as we can find interest and pleasure in looking at a hand, a foot, a finger or a toe, or any other large or small portion of the whole body, without forgetting that it is part of a Whole, and that it is this Whole alone (which remains unsaid) that gives it its full meaning.

tician. The most natural way to do this is to tell you how I came, one thing leading to another, to write these four or five "pavés" one after the other.

As you know, I left the mathematical "big world" in 1970, following a story about military funds in my home institution (the IHES). After a few years of anti-militarist and ecological activism, "cultural revolution" style, which you've no doubt heard about here and there, I practically disappeared from circulation, lost in a provincial university God knows where. Rumor has it that I spend my time herding sheep and drilling wells. The truth is that, apart from many other occupations, I bravely went, like everyone else, to do my lectures at the University (that was my little original livelihood, and still is today). Occasionally, for a few days, weeks or months at a time, I'd even do a bit of zinc-studded math - I've got boxes full of my scribbles, which I'm probably the only one who can decipher. But it was very different, at first sight at least, from what I'd done in the past. Between 1955 and 1970, my favorite theme had been cohomology, and more specifically, the cohomology of varieties of all kinds (algebraic, in particular). I felt I'd done enough in that direction for others to manage without me, and as long as I was doing maths, it was time to change discs... ..

In 1976, a new passion appeared in my life, as strong as, and closely related to, my former passion for mathematics. It was a passion for what I called "meditation" (since things need names). This name, like any other name here, is bound to give rise to innumerable misunderstandings. As in mathematics, this is a work of discovery. I've written about it here and there in Récoltes et Semailles. Still, it was clear that there was plenty to keep me busy for the rest of my life. And more than once, in fact, I thought that mathematics was a thing of the past and that from now on, I was only going to occupy myself with more serious things - that I was going to "meditate".

□ I finally came to the conclusion (four years ago) that mathematical passion was not p . L4 extinguished. And even, without really knowing how and to my own surprise, I, who (for almost fifteen years) hadn't thought of publishing a line of maths in my life, suddenly found myself embarking on the writing of an obviously never-ending maths book that was going to have volumes and volumes; and while I was at it, I was going to throw what I thought I had to say about maths into an (infinite?) of books called "Réflexions Mathématiques", and leave it at that.

It was two years ago, spring 1983. At the time, I was too busy writing (volume 1 of) "A la Poursuite des Champs", which was also to be volume 1 of "Réflexions" (mathematics), to wonder what was happening to me. Nine months later, as it should be, this first volume was finished, so to speak, there was only the introduction to write, reread the whole thing, annotations - and to print... .

The volume in question is still not finished - it hasn't moved a muscle in a year and a half. The remaining introduction has passed the twelve-hundred-page mark (typed), so when it's actually finished it will be fourteen hundred. As you may have guessed, the "introduction" is none other than Harvest and Sowing. At last count, it's supposed to form volumes 1 and 2, plus part of volume 3 of the planned "series". The latter is now called "Réflexions" (for short, not necessarily mathematical). The rest of volume 3 will be made up mainly of mathematical texts, which are now more burning issues for me than the Poursuite des Champs. The latter will have to wait until next year, for annotations, indexes and, of course, an introduction... .

End of Act One!

### 3.3. The death of the boss - building sites at abandonment

It's time, I feel, to give some explanations: why I left so abruptly a world in which, apparently, I had felt at ease for more than twenty years of my life; why I had the strange idea of "coming back" (like a revenant. . .) when I had been quite happily dispensed with for those fifteen years; and why, finally, an introduction to a mathematical work of six or seven hundred pages has ended up being twelve (or fourteen) hundred. And here, too, as I get to the heart of the matter, I'm bound to upset you (sorry!), or even make you angry. Because no doubt, like me once upon a time, you like to see the world you're part of, where you belong, your name and all that, as "rosy". I know what it's like... . And now it's going to squeak a little... .

p. L5     □ I talk here and there in Harvest and Sowing about the episode of my departure, without dwelling on it too much. This "departure"

It is in relation to this "point" that the events of my life as a mathematician are constantly situated, as "before" and "after". It took a powerful **shock** to tear me away from an environment in which I was firmly rooted, and from a "trajectory" that had been firmly mapped out for me. This shock came when I was confronted, in an environment with which I was strongly identified, with a certain form of corruption<sup>3</sup> to which, until then, I had chosen to turn a blind eye (by simply refraining from participating). Looking back, I realize that beyond the event, there was a deeper force at work within me. It was an intense **need for inner renewal**. Such a renewal could not be achieved and sustained in the lukewarm scientific incubator of a prestigious institution. Behind me lay twenty years of intense mathematical creativity and inordinate mathematical investment - and, at the same time, twenty long years of spiritual stagnation, in "isolation"... . Without realizing it, I was suffocating - what I needed was fresh air! My providential "departure" marked the sudden end of a long stagnation, and a first step towards balancing the deep forces in my being, bent and screwed into a state of intense, frozen imbalance... . This departure was, truly, a **new beginning** - the first step on a new journey. ...

As I said, my mathematical passion was not extinguished for all that. It found expression in reflections that remained sporadic, in paths quite different from those I had mapped out for myself "before". As for **the work** I'd left behind, the work "before" - both the work published in black and white and the more essential work, perhaps, that hadn't yet found its way into writing or published text - it could well seem, and indeed did seem, that it had detached itself from me. Before last year, with Récoltes et Semailles, I'd never had the idea of "putting down" any of the scattered echoes that I'd heard of it.

p. L6     came back, here and there. I was well aware that everything I had done in maths, and more particularly, in my □ "geometric" period from 1955 to 1970, were things that **had to be** done - and the things I had things that **had to be** brought out into the open. And also, that the work I'd done, and the work I'd had done, was work well done, work into which I'd put my whole self. I had put all my strength and all my love into it, and (so it seemed to me) it was now autonomous - a living, vigorous thing - that no longer needed me to mother it. From that point of view, I left with perfect peace of mind. I had no doubt that these written and unwritten things I was leaving behind were in good hands, who would see to it that they unfolded, grew and multiplied according to their proper nature as living, vigorous things.

In these fifteen years of intense mathematical work, a vast **vision** had blossomed, matured and grown within me.

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<sup>3</sup>We're talking here about the unreserved collaboration of the entire scientific establishment in all countries with the military apparatus, as a convenient source of funding, prestige and power. This issue is barely touched upon in once or twice in Récoltes et Semailles, for example in the note "Le respect" of April 2 (n° 179, pages 1221 - 1223).



The vision was of an "arithmetic geometry", a synthesis of topology, geometry (algebraic and analytic) and arithmetic. The vision was that of an "arithmetic geometry", a synthesis of topology, geometry (algebraic and analytic) and arithmetic, the first embryo of which I found in Weil's conjectures. She has been my main source of inspiration in these years, which for me are above all those in which I have developed the key ideas of this new geometry, and shaped some of its principal tools. This vision and these key ideas have become second nature to me. (And after having ceased all contact with them for almost fifteen years, I can see today that this "second nature" is still alive in me!) They were so simple and obvious to me, that it went without saying that "everyone" had assimilated them and made them their own, along with me. It's only recently, in these last few months, that I've come to realize that neither the vision, nor these few "key ideas" that had been my constant guide, are to be found written out in full in any published text, except at most between the lines. And, above all, that the vision I thought I was communicating, and the key ideas that underpinned it, remain unheard of even today, twenty years after reaching full maturity. It is I, the worker and servant of these things I had the privilege of discovering, who am also the only one in whom they are still alive.

This and that tool I'd fashioned is used here and there to "fracture" a reputedly difficult problem, like breaking into a safe. The tool is apparently solid. Yet I know it to have even more "strength" than a pair of monseigneur pliers. It's part of a Whole, like a limb □ is part of the body - p .L7 a Whole from which it springs, which gives it meaning and from which it draws strength and life. You can use a bone (if it's big) to fracture a skull, that's a given. But that's not its real function, its *raison d'être*. And I see these scattered tools that some people have seized upon, rather like bones, carefully butchered and cleaned, that they have torn from a body - from a living body that they pretend to ignore... .

What I'm saying here in carefully considered terms, at the end of a long reflection, must have been perceived by me little by little and diffusely, over the years, at the level of the unformulated which is not yet trying to take shape in conscious thought and images, and through clearly articulated speech, I had decided that this past, basically, no longer concerned me. The echoes that came back to me from afar, filtered as they were, were nevertheless eloquent, if I dwelt on them just a little. I had thought of myself as just another worker, busying myself on five or six<sup>4</sup> "construction sites" in full swing - a more experienced worker perhaps, the older one who had once worked alone on these very premises for many years, before a welcome relief arrived; the older one, yes, but basically no different from the others. And now, with him gone, it was like a masonry firm declaring bankruptcy, following the unexpected death of the boss: overnight, as it were, the building sites were deserted. The "workers" left, each taking under his arm the odds and ends he thought he could use at home. The cash register was gone, and there was no longer any reason for him to go on working himself to exhaustion. ...

Once again, it's a formulation that has emerged from more than a year's reflection and investigation. But surely, it was something I had already perceived "somewhere", from the very first years after my departure. Leaving aside Deligne's work on the absolute values of Frobenius eigenvalues (the "prestige question", as I've come to understand it lately. . . ) - when I would come across one of my old acquaintances, with whom I'd worked on the same sites, and that I'd asked him and so...?", it was always the same eloquent gesture, arms up □ as if to ask for mercy... . Visible- p .L8 ment, everyone was busy with more important things than I cared about - and obviously, too, while everyone was busy looking busy and important, not much was getting done. The main thing

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<sup>4</sup>I write about these deserted "building sites", and review them in detail, in the series of notes entitled "Les chantiers désolés" (n° s 176' to 178), dated three months ago. A year earlier, and before the discovery of L'Enterrement, I had already mentioned them, in the first note in which I reconnected with my work and its fate, in the note "Mes orphelins" (n° 46).

had disappeared - a **unity** that gave meaning to the partial tasks, and a **warmth** too, I think. What remained was a scattering of tasks detached from a whole, each in his own corner, brooding over his little hoard, or making it grow as best he could.

As much as I would have liked to defend myself, it pained me to realize that everything had come to a screeching halt; to hear no more about motives, topos, the six operations, De Rham's coefficients, Hodge's coefficients or the "mysterious functor" that was to link them together in a single fan around De Rham's coefficients, the  $l$ -adic coefficients for all primes, nor the crystals (except to learn that they are still at the same point), nor the "standard conjectures" and others that I had identified and which clearly represented crucial questions. Even the extensive groundwork begun with the *Eléments de Géométrie Algébrique* (with Dieudonné's tireless assistance), which it would have been almost enough to continue on the momentum already gained, was left to one side: everyone was content to settle into the walls and furniture that someone else had patiently assembled, mounted and bricked up. With the worker gone, it would never have occurred to anyone else to roll up their sleeves and get their hands on a trowel, to build the many buildings that remained to be constructed - **houses**, good to live in, for themselves and for everyone else... .

Once again, I couldn't help but follow up with fully conscious images, which emerged and surfaced by virtue of reflective work. But there's no doubt in my mind that these images must already have been present, in one form or another, in the deepest layers of my being. I must have already sensed the insidious reality of a **Burial** of my work as well as of myself, which imposed itself on me suddenly, with irrefutable force and with this very name, "L' Enterrement", on April 19 last year. On a conscious level, however, I would hardly have thought of taking offence, or even grieving. After all, "close" or not, it was up to the individual to decide what to do with his time. If what had once seemed to motivate or inspire him no longer did, that was his business, not mine.

mine. If the same thing seemed to happen, in perfect unison, to all my ex-students without exception, that was still a matter for each of them separately and I had other things to worry about than going looking for what sense it made, period! As for the things I'd left behind, to which a deep, unacknowledged link continued to bind me - even though they were visibly abandoned on those desolate building sites - I knew they weren't the kind of things that fear "the ravages of time" or the fluctuations of fashions. If they hadn't yet become part of our common heritage (as I had once thought), they were bound to take root there sooner or later, in ten years or a hundred, it really didn't matter. ... .

p. L9

### 3.4. A burial wind. . .

And yet, while I've been happy to evade the diffuse perception of a large-scale funeral over the years, it has stubbornly come back to haunt me, in other and less anodyne guises than that of simple disaffection with a work of art. Little by little, I came to realize that several notions that were part of the forgotten vision had not only fallen into disuse, but had become, in a certain beau monde, the object of condescending disdain. Such was the case, in particular, of the crucial unifying notion of topos, at the very heart of the new geometry

- the same one that provides the common geometric intuition for topology, algebraic geometry and arithmetic - the same one that enabled me to develop both the étale and  $l$ -adic cohomological tools, and the key ideas (more or less forgotten since, it's true. . . ) of crystalline cohomology. To tell the truth,

it was my very name, over the years, that had insidiously, mysteriously, become an object of derision - like a synonym for endless muddy bombast (such as the famous "topos", or the "motifs" he was raving about that no one had ever seen. . . ), of splitting hairs over a thousand pages, and of gigantic chatter about what, in any case, everyone had already known all along and without having expected it... . A little in those tones, but muted, in undertones, with all the delicacy that is appropriate "among people of high flight and exquisite company".

In the course of my reflections in *Récoltes et Semailles*, I think I've put my finger on the deep forces at work in some and others, behind those airs of derision and condescension in the face of a work whose scope, life and breath escape them. I also discovered (apart from the traits that have marked my work and my destiny) the secret "**catalyst**" that prompted

□ces forces to manifest themselves in this form of casual contempt before the eloquent signs of a creative p. L10

He was, in short, the Grand Officiant at the funeral, a funeral muffled by derision and contempt. Strangely enough, he's also the one who's been closest to me - the only one who's ever assimilated and made his own a certain vision, full of life and intense strength. But I anticipate...

To tell the truth, these "puffs of discreet derision" that came back to me here and there, didn't affect me too much. Until three or four years ago, they remained somewhat anonymous. I certainly saw them as an unhappy sign of the times, but they didn't really call me into question, nor did they arouse any anxiety or concern in me. What did affect me more directly, however, were the signs of distance from myself that came here and there from a good number of my old friends in the mathematical world, friends to whom (notwithstanding my departure from a world we once shared) I continued to feel linked by bonds of sympathy, in addition to those created by a common passion and a certain shared past. Here again, although each time I was saddened by this, I hardly ever stopped to think about it, and the thought never occurred to me (as far as I can remember) to make a connection between these three series of signs: the abandoned building sites (and the forgotten vision), the "wind of derision", and the distancing of many of those who had been friends. I wrote to all of them, and received no reply from any of them. In fact, it was not uncommon from then on for letters I wrote to former friends or students, on matters close to my heart, to go unanswered. New times, new customs - what could I do? I simply refrained from writing to them again. And yet (if you're one of them) this letter I'm writing will be the exception - a word offered to you once again - it's up to you to see whether you welcome it this time, or close yourself off to it once more... ..

If I'm not mistaken, the first signs of some old friends distancing themselves from me date back to 1976. That was also the year when yet another "series" of signs began to appear, which I'd like to talk about before returning to Harvest and Sowing. In other words, these last two series of signs appeared at the same time. As I write, it seems to me that they are in fact inseparable, that they are in fact two different aspects or "faces" of the same reality, bursting into the field of my own experience that year. For the aspect I was about to discuss speaking just now, this is a systematic, □discrete and unanswerable "fin de non recevoir", reserved by p. L11 a "flawless consensus"<sup>5</sup> to the few post-1970 students and assimilates who, through their work, their style

<sup>5</sup>This "flawless consensus" is sporadically evoked here and there in *Fatuité et Renouvellement*, and eventually becomes the subject of detailed testimony and reflection in the following section, *L'Enterrement* (1), with the "Cortège X" or "Le Fourgon Funèbre", made up of the "coffin notes" (n° s 93-96) and the note "Le Fossoyeur - ou la Congrégation toute entière". This concludes this part of Harvest and Sowing, and at the same time marks the first culmination of this "second wind" of the reflection.

and their inspiration, clearly bore the stamp of my influence. Perhaps it was also on this occasion that, for the first time, I perceived this "breath of discreet derision" that, through them, was aimed at a certain style and **approach** to mathematics - a style and vision that (according to a consensus that had apparently already become universal by then in the mathematical establishment) **had no place**.

Here again, it was something clearly perceived on an unconscious level. It even came to my conscious attention that same year, after the same aberrant scenario (illustrating the impossibility of getting an obviously brilliant thesis published) had been repeated five times in a row, with the burlesque obstinacy of a circus gag. Thinking back on it now, I realize that a certain reality was "beckoning" me at the time with benevolent insistence, while I pretended to turn a deaf ear: "Hey, look, big guy, pay a little attention to what's going on right under your nose, it concerns you, but yes ... . !". I shook my head a little, looked (for a moment), half bewildered and half distracted: "ah yes, well, a bit strange, it looks like someone's holding a grudge here, something that's definitely not going down well, and with such a perfect ensemble, it's scarcely believable!

It was so unbelievable, in fact, that I was quick to forget both the gag and the circus. True, I had plenty of other interesting things to do. That didn't stop the circus from coming back to my memory in the years that followed - no longer in the tones of a gag now, but in those of a secret delight in humiliating, or that of a punch in the face; except that we're among distinguished people here, and the punch takes more distinguished forms too, of course, but all equally effective, left to the inventiveness of the distinguished people in question... .

p. L12 □ The episode that felt like "a punch in the face" (from another) was in October 1981<sup>6</sup>. That time, and for the first time since the insistent signs of a new mind had reached me, I was hit - more strongly, no doubt, than if it had hit me, instead of someone else, whom I'd grown fond of. He was a bit of a student, and a remarkably gifted mathematician, who had just done some great things - but that's a detail, after all. What wasn't a detail, however, was the fact that three of my "former" students were directly involved in an act that was received (and not without reason) as a humiliation and an affront. Two of my former pupils had already had occasion to treat him with condescension, as well-to-do people who would send a scholarly tramp packing<sup>7</sup>. Yet another pupil was to follow suit three years later (and in the "punch in the face" style again) - but I didn't know that yet, of course. What appealed to me then was more than enough. It was as if my mathematical past, never examined, suddenly taunted me with a hideous sneer, in the person of five of my students, who had become important, powerful and disdainful figures... ..

It would have been the perfect moment to ask, to probe the meaning of what was suddenly calling out to me with such violence. But somewhere inside me, it had been decided (without ever having to be said. . . ) that this past "before" no longer concerned me, that there was no need for me to dwell on it; that if it seemed to be calling out to me now in a voice I recognized only too well - that of the time of contempt - it was decidedly wrong. And yet, for days, perhaps weeks, I was tied up in anguish, without even acknowledging it. (It was only last year, when I wrote *Récoltes et Semailles*, which brought me back to this episode, that I finally became aware of this anguish, which had been taken under control as soon as it appeared). Instead of taking stock of it and probing its meaning, I became agitated, writing to

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<sup>6</sup>This episode is recounted in the note "Cercueil 3 - ou les jacobiennes un peu trop relatives" (n° 95), in particular pages 404-406.

<sup>7</sup>It is mentioned in passing in the note cited in the previous footnote.

right and left, "the right letters". Those concerned even took the trouble to reply to me - evasive letters, of course, which got to the bottom of nothing. Eventually the waves subsided, and everything returned to normal. I didn't have to think about it again until last year. This time, however, it was as if a wound, or ☐like a painful splinter, rather, that one avoids touching; a splinter that **maintains p** . L13 this wound that just wants to close...

It was surely the most painful and distressing experience I've had in my life as a mathematician - when I saw (without really consenting to **be aware** of what my eyes were seeing) "one of my former students or companions, whom I loved, taking pleasure in discreetly crushing another whom I love and in whom he recognizes me". It left a stronger impression on me then, surely, than the crazy discoveries I made last year, which (to the casual observer) may seem quite different... .... It's true that this experience had resonated with several others, in the same vein but less violent, and which at the time had been a little "forgotten".

It reminds me, too, that 1981 was also the year of a drastic turning point in my relationship with the only one of my former students with whom I remained in regular contact after my departure, and the one who for some fifteen years had been a "privileged interlocutor" for me, mathematically speaking. That was the year when "the signs of disdain" that had been apparent for some years<sup>8</sup> "suddenly became so brutal" that I ceased all mathematical communication with him. This was a few months before the episode that took place earlier. In retrospect, the coincidence seems striking, but I don't think I made the slightest connection at the time. It was stored in separate "lockers"; lockers which, incidentally, someone had said were of no real consequence - the case was made!

And it reminds me, too, that as long ago as June 1981, a certain brilliant **colloquium** had already taken place, memorable in more ways than one - a colloquium that has well deserved to go down in history (or in what's left of it. . . ) under the indelible name of "Colloque Pervers". I made his acquaintance (or rather, he came down on me!) on May 2 last year, two weeks after the discovery (on April 19) of l'Enter- rement in the flesh - and I immediately understood that I had just stumbled upon "**l'Apothéose**". The apotheosis of a funeral, certainly, but also, an **apotheosis** of **contempt for** what, for over two thousand years of our science's existence, has been the tacit and immutable foundation of the mathematician's ethic: namely, this rule

elementary, not to present as one's own the ideas and results taken from another. And taking note just now of ☐this remarkable coincidence in time, between two events that may seem of na- p . L14

I'm struck by the deep and obvious link between **respect** for the **individual** and respect for the basic ethical rules of an art or science, which make its practice something other than a "jockeying match", and the group of people known to excel in it and who set the tone, something other than an unscrupulous "mafia". But again, I anticipate...

### 3.5. Le voyage

I think I've pretty much covered the background to my "return to maths" and, one thing leading to another, to the writing of Récoltes et Semailles. It was at the end of March last year, in the very last section of Fatuité et Renouvellement ("Le poids d'un passé" (n° 50)), that I finally thought about the reasons for and the meaning of this unexpected return. As for the "reasons", surely the strongest of all was the impression, diffuse and compelling at the same time, that these strong and vigorous things, which I had believed

<sup>8</sup>This episode is mentioned in the note "Two turning points" (n° 66).

once entrusted to loving hands - it was in a tomb, cut off from the blessings of wind, rain and sun that they languished for the fifteen years I'd lost sight of them"<sup>9</sup>. I must have understood, little by little and without ever having thought of telling myself before today, that it would be none other than myself who would finally blow up these worm-eaten boards, holding captive living things made, not to rot in closed coffins, but to flourish in the open air. And these airs of false compunction and insidious derision around these padded and plethoric coffins (in the image of the late lamented, no doubt. . . ), must also have "ended up awakening in me a fighting spirit that had become somewhat dormant over the last ten years" and the desire to throw myself into the fray. . .<sup>10</sup>.

And so, two years ago, what was initially intended as a quick survey, lasting a few days or a few weeks at the most, of one of those "building sites" left to one side, became a major mathematical serial in N volumes, part of the famous new series of "Réflexions" ("mathématiques", until that useless qualifier is pruned). From the moment I found out I was

p. L15 writing a mathematical work for publication, I also knew that I was going to include, in addition to a more or less standard "mathematical" introduction, another "introduction" <sup>"□ again"</sup>, from more personal nature. I felt it was important to explain my "return", which was by no means a return to a **milieu**, but only a "return" to an intense mathematical investment and to the publication of mathematical texts from my pen, for an indeterminate period of time. I also wanted to explain the spirit in which I was now writing maths, very different in some respects from the spirit of my writings before I left - the "logbook" spirit of a voyage of discovery. Not to mention that there were other things on my mind, related to these no doubt, but which I felt were even more essential. It was understood that I would take my time to say what I had to say. These things, still diffuse, were inseparable for me from the meaning of the volumes I was about to write, and the "Reflections" into which they were to be inserted. There was no question of slipping them in there on the sly, as if to excuse myself from taking up the precious time of a hurried reader. If there were things in "A la Poursuite des Champs" of which it was good, for him and for everyone, that he should become acquainted, they were precisely those that I reserved to say in this introduction. If twenty or thirty pages weren't enough, I'd write forty or even fifty - and I didn't force anyone to read me...

Thus was born Récoltes et Semaines. I wrote the first pages of the planned introduction in June 1983, at a low point in the writing of the first volume of La Poursuite des Champs. Then I did it again in February last year, when my volume had been practically finished for several months<sup>11</sup>. I was counting on this introduction as an opportunity to clarify a couple of things that were still a little hazy in my mind. But I had no inkling that it was going to be, like the volume I'd just written, a **voyage of discovery**; a voyage into a world even richer and wider in scope than the one I was about to explore, in the volume I'd written and in those that were to follow. As the days, weeks and months went by, without really realizing what was happening,

that this new journey continued, with the discovery of a certain past (stubbornly eluded for more than three decades. . . ) and of myself and the links that connect me to this past; with <sup>"□"</sup>the discovery also of some of those who were close to me in the mathematical world, and whom I have known so poorly; and lastly, even, in the

p. L16

<sup>9</sup>Quoted from the note "La mélodie au tombeau - ou la suffi sance" (n° 167), page 826.

<sup>10</sup>See "The weight of a past" (section n° 50), especially p. 137. (\*\*).

<sup>11</sup>In the meantime I'd spent a good month thinking about the "structural surface" for a system of pseudo-straight lines, obtained in terms of

of all the possible "relative positions" of a pseudo-right in relation to such a system. I have also written "L'Esquisse d'un Programme", which will be included in volume 3 of Réflexions.

a journey of mathematical discovery, while for the first time in fifteen or twenty years<sup>12</sup>, I took the time to revisit some of the questions I had left burning at the time of my departure. In short, I can say that these are **three** closely intertwined journeys of discovery that I'm pursuing in the pages of *Récoltes et Semailles*. And none of them ends with the period, on page twelve hundred and something. The echoes that my testimony will gather (including the echo of silence. . . ) will be part of the "continuation" of the journey. As for its completion, this journey is surely one of those that are never completed - not even, as it happens, on the day we die....

And here I am, back where I started: telling you in advance, if I can, "what Harvest and Sowing is all about". But it's also true that, without even looking for it, the preceding pages have already told you more or less what it's all about. Perhaps it will be more interesting to continue on my path and **tell**, rather than "announce".

June 1985

### 3.6. The shadow side - or creation and contempt

The preceding pages were written during a brief "lull" last month. In the meantime, I've finally put the finishing touches to the "Four Operations" (the fourth part of Harvest and Sowing).

- All that's left is to finish this letter or "pre-letter" (which is also pretending to be prohibitively large. . . ) so that everything is finally ready for typing and duplication. I couldn't believe it any more, after nearly a year and a half of being "on the verge of finishing" these famous notes! When I set about this somewhat unusual "introduction" to a mathematical work in February last year (and the year before that, in June), there were (I think) three main things I wanted to say. First of all, I wanted to explain my intentions and the spirit in which I wrote the first volume of "A

la Poursuite des Champs" (which I had just declared finished), and on the spirit  also in which I intended to continue on an even wider journey of mathematical exploration and discovery, with "Réflexions". From now on, it would no longer be a matter of meticulously laying the foundations for some new mathematical universe in the making. Rather, they would be "logbooks", in which the work would be carried on day by day, as it **really** is, with its missteps and blunders, its insistent backtracking and also its sudden leaps forward - a work drawn forward irresistibly day after day (and notwithstanding innumerable incidents and unforeseen events), as if by an invisible thread - by some elusive, tenacious and sure vision. It's often a groping process, especially in those "sensitive moments" when some as yet nameless and faceless intuition emerges, barely perceptible; or at the start of some new journey, in the call and pursuit of some first ideas and intuitions, often elusive and reluctant to let themselves be grasped in the meshes of language, when it's precisely the language adequate to grasp them delicately that is often still lacking. It is this kind of language, above all else, that needs to be condensed out of an apparent nothingness of impalpable mists. What is still only hinted at, before it is even glimpsed, let alone "seen" and touched with the finger, gradually decants from the imponderable, emerges from its cloak of shadows and mists to take shape and flesh and weight... .

It's this part of the work, which looks shoddy if not (quite often) shoddy, that's the most important.

<sup>12</sup>In the fifties and sixties, I had often suppressed my urge to launch myself into the pursuit of such juicy and burning questions, preoccupied as I was with interminable foundation tasks, which nobody would have known or wanted to pursue in my place, and which nobody after my departure had the heart to continue either. ...

It's also the most delicate and essential part - the part where, truly, something new comes into being, through intense attention, concern and respect for this fragile, infinitely delicate thing about to be born. This is the creative part of all - that of conception and slow gestation in the warm darkness of the nurturing womb, from the invisible original double gamete, becoming formless embryo and transforming over the days and months, through obscure and intense, invisible and seemingly invisible work, into a new being in flesh and blood.

This is also the "dark", "yin" or "**feminine**" part of the work of discovery. The complementary aspect, the "light" or "yang" or "**masculine**" part, is more akin to working with a hammer or sledgehammer, a well-sharpened chisel or a wedge of good, hardened steel. (Tools already ready for use, and a proven efficiency. ... ) Both aspects have their *raison d'être* and their function, in

p. L18 □ symbiosis inseparable from each other - or, to put it better, they are the **wife** and **husband** of the indissoluble couple.

luble of the two original cosmic forces, whose ceaselessly renewed embrace constantly resurrects the obscure creative labors of conception, gestation and birth - the birth of **the child**, of the new thing.

The second thing I felt the need to talk about, in my famous personal and "philosophical" "introduction" to a mathematical text, was the very nature of creative work. I'd been aware for years that this nature was generally ignored, obscured by all kinds of clichés, repressions and ancestral fears. How true this is, I discovered only afterwards, gradually, over the course of days and months, throughout the reflection and "investigation" pursued in *Récoltes et Semailles*. It was at the very "kick-off" of this reflection, in the course of the few pages dated June 1983, that I was first seized by the significance of this seemingly trivial, yet astonishing fact, if only you stop to think about it for a moment: that this "most creative of all" parts of the work of discovery I've just been talking about, **is practically nowhere to be found** in the texts or speeches that are supposed to present such work (or at least, its most tangible fruits); be they textbooks and other didactic texts, or original articles and dissertations, or oral lectures and seminar presentations and so on. For millennia, it would seem, since the very origins of mathematics and the other arts and sciences, there has been a kind of "conspiracy of silence" surrounding the "**unavowable labours**" that prelude the birth of any new idea, large or small, that renews our knowledge of a portion of this world, in perpetual creation, in which we live.

In fact, it would seem that the repression of knowledge of this aspect or this stage, the most crucial of all in any work of discovery (and in creative work in general), is so effective, so internalized by those who know such work first-hand, that one would often swear that even they have eradicated all trace of it from their conscious memory. Rather as in a puritanical society, a woman would have eradicated from her memory, in relation to each of those children she makes it her duty to wipe and wipe, the moment of the embrace (reluctantly endured) that made him conceive, the long months of pregnancy (experienced as an impropriety), and the long hours of childbirth (endured as an unpleasant ordeal, followed at last by deliverance).

p. L19 □ This comparison may seem outrageous, and perhaps it is indeed, if I apply it to what I'm I'm reminded today of the spirit I experienced in the mathematical milieu of which I was a part, even twenty years ago. But in the course of my reflections in *Récoltes et Semailles*, I've come to realize - most strikingly in these last few months (with the writing of "Quatre Opérations") - that since my departure from the mathematical scene, there has been an astonishing **deterioration in the spirit** that today prevails in the circles I knew, and (it seems to me, to a large extent at least) in the world of mathematics.



mathematics in general<sup>13</sup>. It's even possible, both because of my very particular mathematical personality and because of the conditions surrounding my departure, that it acted as a catalyst in an evolution that was already taking place<sup>14</sup> - an evolution of which I was then unaware (nor were any of my colleagues and friends, with the possible exception of Claude Chevalley). The aspect of this degradation that I'm thinking of here (which is just one aspect among many<sup>15</sup>) is the **tacit contempt**, if not unequivocal derision, of what (in mathematics, in this case) is not akin to a "scientific" approach.

not the pure work of hammer on anvil or chisel - disregard for the most delicate creative processes

(and <sup>□</sup> often of lesser appearance); of all that is **inspiration, dream, vision** (so powerful and fertile p. L20

This applies to everything that is not written and **published in black and white**, in the form of pure and hard statements, indexable and indexed, ripe for the "data banks" engulfed in the inexhaustible memories of our mega-computers.

There has been (to borrow a phrase from C.L. Siegel<sup>16</sup>) an extraordinary "**flattening**", a "**narrowing**" of mathematical thought, stripped of an essential dimension, of its "shadow side", its "feminine" side. It's true that, thanks to an ancestral tradition, this side of the work of discovery remained largely hidden, and nobody (as far as I know) ever **talked about** it - but living contact with the deep sources of dreams, which nourish great visions and grand designs, had never yet (as far as I know) been lost. It would seem that we have already entered an **era** of **desiccation**, in which this source is not dried up, but access to it is condemned, by the final verdict of general contempt and the reprisals of derision.

We are approaching the time, it seems, when the **memory of** any work close to the source, of "feminine" work (ridiculed as "muddy", "soft", "inconsistent" - or at the opposite end as "trivialities", "childishness", "bombast" . . .), but also where the work itself and its fruits will be extirpated: where new notions and visions are conceived, elaborated and born. It will also be the time when the exercise of our art will be reduced to arid and futile displays of cerebral "weights and dumbbells", to one-upmanship to "crack" the problems in the competition ("of proverbial difficulty") - the time of a feverish and sterile "supermacho" hypertrophy, taking over from more than three centuries of creative renewal.

## 3.7. Respect and fortitude

But again I digress, anticipating what reflection has taught me. I had started with a double purpose, clearly present in me even before the beginning of this one: the purpose of a "declaration of in-

<sup>13</sup>This deterioration is by no means confined to the "mathematical world". It can also be observed in scientific life as a whole, and even beyond that, in the contemporary world on a planetary scale. A starting point for observation and reflection along these lines can be found in the note entitled "Muscle and gut", which opens the discussion on yin and yang (note no. ° 106).

<sup>14</sup>This is the development examined in the note cited in the previous b. de p. note. Links between this and Burial (from my person and my work) make their appearance and are examined in the notes "Les Obsèques du Yin (yang enterre yin (4))", "La circonstance providentielle - ou l'Apothéose", "Le désaveu (1) - ou le rappel", "Le désaveu (2) - ou la métamorphose" (n° s 124, 151, 152, 153). See also the more recent notes (in RS IV) "Les détails inutiles" (n° 171 (v), part (c) "Des choses qui ressemblent à rien - ou le dessèchement") and "L'album de famille" (n° 173, part c. "Celui entre tous - ou l'acquiescement").

<sup>15</sup>The aspect that is most often at the center of attention in Harvest and Sowing, and more particularly in the two sections "investigation" (RS II or "The Robe of the Chinese Emperor", and RS IV or "The Four Operations"), and the one too, perhaps, that most "flabbergasted" me, was the degradation of the ethics of the profession, expressed in shameless plundering, debunking and scheming, practiced among some of the most prestigious and brilliant mathematicians of the day, and this (to a very large extent) in full view of everyone. For other more delicate aspects, directly related to this one, I refer you to the note already quoted (n° 173 part c.) "Things that look like nothing - or desiccation".

<sup>16</sup>This expression is quoted and commented on in the note just quoted in the previous b. de p. note.

tentions", and (intimately linked to this, as has just become clear) that of expressing myself on the subject of: the nature of creative work. There was, however, a third purpose, less clearly present at the level of the "creative work".

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conscious, but responding to a deeper, more essential need<sup>17</sup>. It was aroused by these "interpellations" sometimes disconcerting, coming to me from my mathematical past through the voices of those who had been my students or friends (or at least, of many of them). On an epidermal level, this need translated into an urge to "speak my mind", to tell a few "unpleasant truths". But more profoundly, surely, there was the need to finally **come to terms** with a certain past that I'd chosen to evade until then. It was from this need, above all, that *Récoltes et Semailles* was born. This long reflection was my "response", day by day, to this impulse for knowledge within me, and to the constantly renewed interpellation that came to me from the outside world, from the "mathematical world" that I had left with no spirit of return. Apart from the very first pages of "Fatuité et Renouveau", the first two chapters ("Travail et découverte" and "Le rêve et le Rêveur"), and right from the chapter that follows "Naissance de la crainte" (p. 18), with a "testimony" that was by no means part of the program, it was this need to get to know my past and to come to terms with it fully, that (I believe) was the main force at work in the writing of *Récoltes et Semailles*.

The interpellation that had come to me from the world of mathematicians, and which came back at me with renewed force throughout Harvest and Sowing (and above all, during the "investigation" pursued in Parts II and IV), had from the outset taken on the mask of smugness, if not that of disdain ("delicately dosed"), derision or contempt, whether towards me (sometimes) or (above all) towards those who had dared to draw inspiration from me (without suspecting, of course, what was in store for them) and who were "classified" as having ties with me, by some tacit and implacable decree. And here again I see the "obvious" and "profound" link between **respect** (or lack of respect) for the person of others; respect for the act of creation and for some of its most delicate and essential fruits; and respect for the most obvious rules of scientific ethics: those rooted in a basic respect for oneself and for others, and which I would be tempted to call the "**rules of decency**" in the exercise of our art. These are all aspects, surely, of a basic and essential "**self-respect**". If I try, in a single lapidary formula, to put it in a nutshell

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the assessment of what Harvest and Sowing taught me about a certain world that was mine, a world with which I had identified myself for more than twenty years of my life, I would say: <sup>□</sup>it's a world that has **lost** the **respect**<sup>17</sup>.

This was something that had already been strongly sensed, if not formulated, in the preceding years. It was only confirmed and clarified, always in unexpected and sometimes astonishing ways, throughout the course of *Récoltes et Semailles*. It is clearly apparent from the very moment when a "philosophical" and general reflection suddenly becomes a personal testimony (in the section "L'étranger bienvenu" (n° 9, p. 18) opening the aforementioned chapter "Naissance de la crainte").

Yet this perception does not appear in the tone of bitter or acerbic recrimination, but (through the internal logic of the writing and the different attitude it elicits) in that of a **question**: what has been my own part in this degradation, in this loss of respect that I see today? This is the main interrogation that runs through and carries this first part of Harvest and Sowing, until it finally resolves itself into a clear and unequivocal observation<sup>18</sup>. Previously, this degradation had been

<sup>17</sup>Here again, it's a formulation that doesn't just apply to a certain limited milieu, where I've had ample opportunity to see the thing up close, but it seems to me to sum up a certain degradation in the contemporary world as a whole. (Compare with the b. de p. note on page L 19.) In the more limited framework of the assessment of an "investigation" pursued in *Récoltes et Semailles*, this

wording appears in the April 2 note, "Respect" (n° 179).

<sup>18</sup>In the "Sports math" and "No more merry-go-round" sections (n° s 40, 41).

appeared as if it had suddenly "fallen from the sky", inexplicably and all the more outrageously, intolerably. In the course of reflection, I discovered that it had continued insidiously, undetected by anyone around him or in himself, throughout the fifties and sixties, **including in my own person**.

The realization of this humble fact, surely quite obvious and without appearance, marked a first crucial turning point in the testimony, and an immediate qualitative change<sup>19</sup>. This was the first essential thing I had to learn, about my past as a mathematician and about myself. This knowledge of my **share of responsibility** for the general deterioration (a knowledge that varies in severity depending on the type of mathematics involved) was the first essential step.

moments of reflection) remained as a background note and a reminder, throughout Récoltes and Semailles. It was so, especially, at times when my reflection□took on the allure of an investigation into p. L23 disgraces and iniquities of an era. Together with the desire to understand, the curiosity that drives and drives forward all true work of discovery, it is this humble knowledge (often forgotten along the way and resurfacing in spite of everything, where it was least expected. . . ) that has kept my testimony from ever turning (I believe) into sterile recrimination about the ungratefulness of the world, or even into a "settlement". ) that kept my testimony from ever veering (I think) into sterile recrimination about the world's ingratitude, or even into "settling scores" with some of those who had been my students or friends (or both). This lack of self-indulgence also gave me an inner calm, or fortitude, that preserved me from the pitfalls of complacency towards others, or even those of false "discretion". Whatever I felt I had to say, at one time or another, about myself, or about one of my colleagues, ex-students or friends, or about an environment, or about an era, I said it, without ever having to shake my reticence. As for the latter, all I had to do was examine them carefully, and they vanished without a trace.

### 3.8. "Mes proches" - or connivance

It is not my intention in this letter to review all the "highlights" (or all the "sensitive moments") in the writing of Récoltes et Semailles, or in any of its stages<sup>20</sup>. Suffice it to say that there were, in this work, four clearly marked stages, or four "breaths" - like the breaths of a breath, or like the successive waves in a train of waves arising, I cannot say how, from those vast, mute masses, motionless and moving, boundless and nameless, from an unknown and bottomless sea that is "me", or rather, from a sea infinitely vaster and deeper than the "me" it carries and nourishes. These "breaths" or "waves" have materialized in the four parts of Récoltes et Semailles now written. Each wave came without my calling it or planning it in the least, and at no time could I have said where it would take me or when it would end. And when it had ended and a new wave had already taken its place, for a while I thought I was still at the end of a journey (which would also be, at the end of all ends, the end of Harvest and Sowing!

lifted and carried towards another breath of the same vast movement. It's only with hindsight that

□celui apparaît clairement et que se révèle sans équivoque une **structure** dans ce qui avait été vécu comme p. L24 act and as a movement.

And it certainly didn't end with my (very provisional!) finale to Récoltes et Se-.

<sup>19</sup>From the very next day, the testimony deepens into a meditation on myself, and retains this particular quality in the weeks that follow, right up to the end of this "first breath" of Harvest and Sowing (with the section "The weight of a past", n° 50).

<sup>20</sup>You'll find a brief retrospective of the first three parts of Harvest and Sowing in the two groups of notes "Evening fruits" (n° s 179-182) and "Discovery of a past" (n° 183-186).

Nor will it end with the completion of this letter to you, which is one of the "times" of this movement. And it wasn't born on a day in June 1983, or February 1984, when I sat down at my typewriter to write (or rewrite) a certain introduction to a certain mathematical work. It was born (or rather, re-born. . . ), the day meditation came into my life. . .

But once again I digress, letting myself be carried (and carried away. . . ) by the images and associations born of the moment, instead of wisely sticking to the thread of a "propos", of the planned. My intention today had been to follow up with an account, however succinct, of the "discovery of the Burial" last April, at a time when two weeks ago I thought I'd finished *Récoltes et Semailles* - how they came cascading down on me, in the space of just three or four weeks, discoveries, some bigger and more incredible than the others - so big and so crazy, in fact, that for months afterwards, I had the greatest difficulty "to believe the testimony of my healthy faculties", to free myself from an insidious **incredulity** in the face of the obvious<sup>21</sup>. This secret, tenacious incredulity only finally dissipated last October (six months after the discovery of "L'Enterrement dans toute sa splendeur"), following a visit to my home by my friend and ex-student (admittedly occult) Pierre Deligne<sup>22</sup>. For the first time, I was confronted with L'Enterrement not through **texts**, telling me (in eloquent terms!) of the debunking, pillage and massacre of a work, and of the burial (in the person of the absent master) of a certain style and approach to mathematics - but in a direct and tangible way this time, under familiar features and by a well-known voice, with affable and ingenuous intonations. L'Enterrement was there before me at last, "in the flesh", under those busy, anodyne features that I now well recognized, but which I'd never seen before.

p. L25

for the first time I was looking with new eyes, new attention. Here, then, unfolded before me the one who, in the course of my reflection over the preceding months, had revealed himself as the Grand Officiant at my solemn funeral, as the "Priest in a pinafore" as well as the main architect and "beneficiary" of an unprecedented "operation", the hidden heir to a work of art handed over to derision and pillage...

This encounter took place at the beginning of the "third wave" in *Récoltes et Semailles*, when I had just embarked on the long meditation on yin and yang, in pursuit of an elusive and tenacious association of ideas. At the time, this short episode only left the trace of an echo of a few lines, in passing. Yet it marked an important moment, the fruits of which would only become clear months later.

There was a second such moment of confrontation at "L'Enterrement en chair et en os". This time, it was a simple phone call to Jean-Pierre Serre<sup>23</sup>. This "off-the-record" conversation confirmed, in a striking way and beyond all expectations, what (just a few days before) I had just explained to myself at length<sup>24</sup>, and almost unwillingly, about Serre's role in my Burial and about his "secret acquiescence" to what was happening "right under his nose", without him pretending to see or feel anything.

Here again, as usual, the conversation was cool and friendly, and obviously Serre's friendliness towards me was sincere and genuine. All the same, this time I was able to really see, or "touch" I would have liked to write, this "acquiescence" that I had finally admitted to myself; "secret" no doubt (as I had written earlier) but above all

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<sup>21</sup>I try to express this difficulty, through the tale "The robe of the Chinese Emperor", in the note of the same name (n° 77'), and come back to it again in the note "Duty done - or the moment of truth" (n° 163).

<sup>22</sup>I give an account of this visit in the note I just quoted (in the previous b. de p. note).

<sup>23</sup>This is a quote from the note "Le Fossoyeur - ou la Congrégation toute entière" (n° 97, page 417).

<sup>24</sup>In part c. ("The one among all - or acquiescence") of the same note (n° 173).

as I was then able to see without any possibility of doubt. An eager and unreserved acquiescence, so that what needs to be buried can be buried, and so that, wherever desirable and whatever the means, real (which Serre knows first-hand) and undesirable paternity can be replaced.

<sup>25</sup> □ This was a striking confirmation of an intuition that had appeared one p. L26 year ago, when I wrote<sup>26</sup> :

"Seen in this light<sup>27</sup> , the principal official Deligne appears no longer as one who would have fashioned a fashion in the image of the profound forces that determine his own life and actions, but rather as the all-designated **instrument** (by virtue of his role as "legitimate heir"<sup>28</sup> ) of a **collective will** of flawless consistency, committed to the impossible task of erasing both my name and my personal style from contemporary mathematics."

If Deligne appeared to me at the time as the designated "instrument" (as well as the first and principal "beneficiary") of an "unfailing coherent collective will", Serre now appears to me as **the embodiment of** this same collective will, and as the **guarantor of** its unreserved acquiescence; acquiescence to all the innumerable schemes and swindles and to the vast "operations" of collective mystification and shameless appropriation, as long as they contribute to this "impossible task" with regard to my modest and deceased person, or with regard to any other<sup>29</sup> who dared to claim to be the "beneficiary" of this "collective will". I'd like to be seen, against all odds, as a "Grothendieck continuator".

□ It is one of the paradoxical and disconcerting aspects, among many others in Burial, that it- p. L27

This is the work above all, not to say exclusively, of those who had been my friends or my pupils, in a world where I had never known any enemies. It is for this reason above all, I believe, that Récoltes et Semailles concerns you more than anyone else, and that this letter I am writing to you is intended as an **interpellation** in its own right. For if you're a mathematician, and if you're one of my students or friends, you're no doubt no stranger to the Burial, whether by deed or connivance, and if only by your silence towards me about something that's taking place on your doorstep. And if (extraordinarily) you welcome my humble words and the testimony they bear to you, rather than remaining closed behind your closed doors and sending away these unwelcome messengers, you will then learn, perhaps, that what has been buried by all and with your participation (active, or by tacit acquiescence), is not only the work of another, the fruit and living testimony of streets in love with mathematics; but that at an even more secret level than this burial (which never says its name . . . ) and deeper, it's a living and essential part of your own being, of your original power to know, to love and to create, that you were pleased to bury with your own hands in the person of another.

<sup>25</sup>This is a quote from the note "Le Fossoyeur - ou la Congrégation toute entière" (n° 97, page 417).

<sup>26</sup>This quotation is taken from the same note (see previous b. de p. note), on the same page 417.

<sup>27</sup>"In light" of this deliberate intention, just mentioned, to eliminate "undesirable" (indeed, "intolerable", to use the expression employed in the quoted note) paternities at all costs.

<sup>28</sup>This role of Deligne's "heir" is a role that is both concealed (while not a single published line of Deligne's could give rise to the suspicion that he might have learned something from me), and at the same time clearly felt and accepted by all. It's one of the typical aspects of Deligne's double game and his particular "style" that he was able to masterfully play on this ambiguity, cashing in on the benefits of this unspoken role of heir, while disowning the late master and taking the lead in large-scale burial operations.

<sup>29</sup>I'm thinking here of **Zoghman Mebkhout**, first mentioned in the Introduction, 6 ("L'Enterrement"), then in the note "Mes orphelins" (n° 46), and in the notes (written later, after the discovery of L'Enterrement) "Echec d'un enseignement (2) - ou création et fatuité" and "Un sentiment d'injustice et d'impuissance" (n° s 44', 44"). I discover the iniquitous operation to conceal and appropriate Mebkhout's pioneering work, at the end of the eleven notes forming Cortère VII of L'Enterrement, "Le Colloque - ou faisceaux de Mebkhout et Perversité" (n° s 75-80). A more detailed investigation and account of this (fourth and final) "operation" forms the most substantial part of the investigation "The Four Operations", under the appropriate title "**The Apotheosis**" (notes n° s 171 (i) to 171).

Of all my pupils, Deligne occupied a very special place, which I'll expand on at length in the course of my reflection<sup>30</sup>. He was, by far, the "closest", the only one (student or not) to have intimately assimilated and made his own<sup>31</sup> a vast vision that had been born and grown within me long before we met. And of all my friends who shared a common passion for mathematics, it was Serre, who had also been a bit of an elder, who was the closest (also by far),

p. L28

as the one (in particular) who for a decade had played a unique "detona- teur" role in my work for some of my major investments, and for the □ plurality of the great ideas-force that inspired my mathematical thinking in the fifties and sixties, right up to the time of my departure. This very special relationship that both of them had with me is not unrelated, of course, to their exceptional means, which ensured them an equally exceptional ascendancy over the mathematicians of their generation and those that followed. Apart from these points in common, Serre's and Deligne's temperaments and ways of doing things seem to me to be as dissimilar as it is possible to be, and in many ways poles apart.

In any case, if ever there were mathematicians who, in one way or another, were "close" to me and my work (and, what's more, known for it), it's Serre and Deligne: one, an elder and a source of inspiration in my work during a crucial period of gestation of a vision; the other, the most gifted of my students, for whom I was in turn (and have remained, Burial or not. . . ) his main (and secret. . . ) source of inspiration. . . ) his main (and secret. . . ) source of inspiration<sup>32</sup>. If a Burial was set in motion in the aftermath of my departure (which became a "death" in due form), and materialized in an interminable procession of "operations" large and small in the service of the same end, this could only have happened with the combined and closely interdependent assistance of one and the other, the ex-senior and the ex-pupil (see, ex-"disciple"): one taking the discreet and effective lead in operations, while sounding the rallying call for some of my pupils<sup>33</sup>, craving the slaughter of the **Father** (under the grotesque and derisory effigy of a plethoric and bombastic **super-nana**); and the other giving an unreserved, unconditional and unlimited "green light" to the continuation of the (four) operations (of debunking, carnage, butchering and sharing of an inexhaustible corpse. . . ).

### 3.9. counting

As I've already hinted, I had to overcome considerable inner resistance - or rather, to overcome it through patient, meticulous and tenacious work - in order to separate myself from certain familiar, solidly-based images of considerable inertia, which for decades had taken the place of a direct, unbiased perception in me (as in everyone else, and surely in you too).

p. L29

nuanced reality - in this case, that of a certain mathematical world, to which I continue to be linked □ by a past and by a work. One of the most strongly anchored of these images, or ready-made ideas, is that it seems out of the question for an internationally-renowned scientist, or even a man with a reputation as a great mathematician, to be able to afford (even if only exceptionally, and even less so as an expensive

<sup>30</sup>On this subject, see above all the group of seventeen notes "Mon ami Pierre" (n° s 60-71) in RS II.

<sup>31</sup>This "vast vision", which Deligne had indeed "intimately assimilated and made his own", had exerted a powerful fascination on him.

He was fascinated by his own work, and continues to be fascinated by it in spite of himself, while at the same time a compelling force drives him to destroy it, to shatter its fundamental unity and seize the scattered pieces. Thus, his occult antagonism towards a disowned and "dead" master is the expression of a division in his being, which profoundly marked his work after my departure - work that remained far below the rather prodigious means I had known him to have.

<sup>32</sup>On this subject, see the previous b. de p. note.

<sup>33</sup>These are exactly the five other students who (like Deligne) have chosen cohomology of varieties as their main theme.

habit. . . ) of swindles large and small; or if he refrains (out of old habit again) from dipping his hand in it himself, let him nevertheless welcome with open arms such operations "(defying all sense of decency, at times)" set up by another, and where, for one reason or another, he finds his profit.

Such was the inertia of my mind, that it was only less than two months ago, at the end of a long reflection that had already lasted a whole year, that I finally came to the timid realization that Serre might also have had something to do with this Burial - something that now seems obvious to me, even independently of the eloquent conversation I had with him recently. As with all the members of the "Bourbaki milieu" that had welcomed me so graciously in my early days, and especially in his case, there was for me a kind of unspoken "taboo" around his person. He represented the very embodiment of a certain "elegance" - an elegance that is by no means limited to form, but also includes rigor and scrupulous probity.

Before I discovered l'Enterrement on April 19 last year, I could not have dreamt that any of my students would be capable of dishonesty in the exercise of their profession, whether towards me or anyone else. And it was for the most brilliant of them, the one who had been closest to me, that such a supposition would have seemed the most aberrant! And yet, from the very moment of my departure and throughout the years that followed, right up to the present day, I'd had ample opportunity to realize just how divided his relationship with me was. More than once, too, I've seen him use (for the sheer pleasure of it, it would seem) the power to discourage and humiliate, when the occasion was right. Each time, I was deeply affected (more, no doubt, than I would have liked to admit to myself. . . ). These were eloquent signs of a deep-seated disturbance, which (I'd had ample opportunity to observe) was by no means confined to him alone, even in the most limited circle of those who had been my pupils. The loss of respect for the person of others is no less flagrant, and no less obvious, than the loss of respect for the person of others.

that which manifests itself in so-called "professional dishonesty". Still, the discovery of such dishonesty came as a complete surprise and shock to me. p. L30

In the weeks following this breathtaking revelation, followed by a whole "cascade" of others of the same kind, I gradually came to realize that a certain amount of scheming, among some of my students<sup>34</sup>, had already begun in the years leading up to my departure. This was particularly obvious in the case of the most brilliant of them - the one who, after my departure, set the tone and (as I wrote earlier) "quietly and efficiently took the lead". With the benefit of almost twenty years' hindsight, this scheming is now obvious to me - it was "plain to see". While I chose to turn a blind eye to what was going on, all in pursuit of the "white whale" in a world "where all is order and beauty" (as I liked to imagine it), I realize today that I failed to assume the responsibility that was incumbent on me at the time, with regard to students learning from me a trade that I love; a trade that is something other than simple know-how, or the development of a certain "flair". Through my complacency towards brilliant students, whom I was pleased (by tacit decree) to treat as "beings apart" and above suspicion, I contributed my share<sup>35</sup> to the corruption (unprecedented, it seems to me) that I see spreading today in a world and among people who were once dear to me.

Admittedly, given their immense inertia, it took intense and sustained work to separate myself from what we used to call "illusions" (not without a hint of regret. . . ), and which I'd rather call "illusions".

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<sup>34</sup>See previous b. de p. note.

<sup>35</sup>This "contribution" appears notably in the note "L'être à part" (n° 67'), as well as in the two notes "L'ascension" and "L'ambiguïté" (n° s 63', 63"), and again (in a slightly different light) at the end of the note "L'éviction" (n° 169). Another type of "contribution" appears in "Fatuité et Renouveau", with attitudes of fatuity towards young less brilliantly gifted mathematicians. This awareness of a share of responsibility for a general deterioration culminates in the section on "Sports mathematics" (n° 40).

about myself, about an environment with which I'd once identified, about people I've loved and perhaps still love - to "separate" myself from these ideas, or rather, to **let them detach themselves from me**. It's been work, yes, but never a struggle - work that has brought me, among many other things

of price, moments of sadness sometimes, but never a moment of regret or bitterness. Bitterness is one of the ways to evade ☐ une knowledge, to evade the message of an experience; to maintain oneself in a certain

p. L31

A tenacious illusion about oneself, at the cost of another "illusion" (in negative, as it were) about the world and others.

It's without bitterness or regret that I see these ready-made ideas that had been "dear" to me, out of old habit and because they'd been there "forever", being detached from me one by one, like so many cumbersome, even crushing, weights. They had become, of course, like second nature. But this "second nature" is not "me". Separating myself from them piece by piece is not the heartbreak or even frustration of someone who would see himself stripped of things that are precious to him. The "stripping" of which I speak comes as the reward and fruit of **work**. Its sign is an immediate and beneficial relief, a welcome **liberation**.

### 3.10. Four waves in one movement

As expected, this letter is not at all what I had planned when I started writing it. Here's what happened in broad outline, you'll believe me or not (I found it hard to believe myself. . . ), but that's what it is, unmistakable, even, whether you like it or not, black publications. ), but that's what it is, unmistakable, even, whether you like it or not, published in black and white in such and such a periodical or book, such and such a date, such and such a page, all you have to do is look - in fact, everything in *Récoltes et Semailles* is stripped down to the bare essentials; see "Quatre Opérations" such and such notes - take it or leave it! And if you prefer not to read me, others will do it for you... . In the end, none of this happened - and yet this letter is already at the thirty-page mark, although I had planned for five or six pages in all.

Unintentionally, these are the essential things I've been led to tell you, as the pages go by, while the "bag"

I've been so eager to empty (right there in plain sight, on the first pages!) still hasn't been unpacked! It doesn't even tickle me anymore

in my fingers, the desire dissipated along the way. I realized that this was not the place. . .

In fact, Part IV of *Harvest and Sowing* (and the longest of all), entitled "The Burial", is the longest of all. (3)" or "The Four Operations", is the result of a "note" originally intended as a "brief summary" of what last year's surprise investigation had revealed to me, and continued in Part II ("The Burial (1)", or "The robe of the Chinese Emperor"). I thought

p. L32

that there would be enough for a "note" of five or ten pages, no more. Eventually, one thing led to another, and the investigation got under way again, and there were nearly four hundred pages of it - almost double ☐ the part I was supposed to have written.

to summarize or take stock! As a result, there's still no report on the subject, even though the six hundred pages of *Récoltes et Semailles* are devoted to the investigation of the Burial. It's a bit silly, it's true. But there's always time to add it in a third part to the Introduction (which is no more than ten or twenty pages long), before handing over my notes to a printer.

The five parts of *Récoltes et Semailles* (the last of which is not yet finished, and probably won't be for a few months) represent an alternation of (three) waves of "meditation" and (two) waves of "investigation". It's like an abbreviated reflection of my life over the last nine years, which has also consisted of alternating "waves" arising from the two passions that now dominate my life, the passion for meditation and the passion for mathematics. In fact, the two parts (or "waves") of *Récoltes et*



The seeds I've just called "investigation" are those that have sprung directly from my roots in my mathematical past, driven by my mathematical passion and the egotistical attachments that have taken root in it.

The first wave, "Fatuity and Renewal", is a first encounter with my past as a mathematician, leading to a meditation on my present, whose roots I have just discovered. Although not in the least premeditated, this part sets the "basic tone" for the rest of *Récoltes et Semailles*, and is a providential and indispensable inner preparation for the discovery of "L'Enterrement dans toute sa splendeur" (Burial in all its splendor), which follows closely on its heels in the second wave, "L'Enterrement (1) - ou la robe de l'Empereur de Chine" (Burial (1) - or the dress of the Chinese Emperor). More than an "investigation", in fact, it's the story of this day-to-day discovery, of its impact on my being, of my efforts to face up to what was tumbling down on me without warning, to situate the unbelievable in terms of my experience, of what ended up becoming familiar to me, making it intelligible as best I could. This movement led to a first provisional outcome, in the note "Le Fossoyeur - ou la Congrégation toute entière" (n° 97), a first attempt to discern an explanation and a meaning in something which, for years already and now more acutely than ever, was taking on the appearance of a formidable challenge to common sense!

This same second movement also leads to a "sick episode"<sup>36</sup>, □me forcing a absolute rest and put an end to all intellectual activity for more than three months. This was at a time when I thought I was once again on the verge of completing Harvest and Sowing (apart from some final "housekeeping" tasks. . . ). When I resumed normal activity towards the end of September last year, and was about to put the finishing touches to my notes, I still thought I'd have two or three final notes to add, including one about the "health incident" I'd just been through. In fact, week after week and month after month, another thousand pages were added - more than double what had already been written - and this time, it's quite clear that I still haven't finished<sup>37</sup> ! In fact, this long interruption, during which I had practically lost contact with a substance that was hot (and even burning!) when I left it, practically forced me to return to this substance with new eyes, if I didn't want to limit myself to foolishly "wrapping up" the final end of a "program" with which I had lost living contact.

p. L33

Thus is born the third wave in the vast movement that is *Récoltes et Semailles* - a long "wave-meditation" on the theme of yin and yang, the "shadow" and "light" sides in the dynamics of things and in human existence. Stemming from a desire for a deeper understanding of the profound forces at work in Burial, this meditation nonetheless acquires an autonomy and unity of its own from the outset, and focuses from the outset on what is most universal, as well as what is most intimately personal. It was during this meditation that I discovered something (quite obvious, in fact, if you ask me): that in my spontaneous approach to the discovery of things, whether in mathematics or elsewhere, the "basic tone" is "yin", "feminine"; and also, and above all, that contrary to what most often happens, I have remained faithful to this original nature in me<sup>38</sup>, without ever bending or correcting it in order to "make the most of it".

<sup>36</sup>This episode is the subject of two notes, "L'incident - ou le corps et l'esprit" and "Le piège - ou facilité et épuisement" (n° s 98, 99), opening the "Cortège XI" entitled "Le défunt (toujours pas décédé)".

<sup>37</sup>"Still not finished" - if only because there's still a Part V to come, unfinished at the time of writing.

<sup>38</sup>This "fidelity to my original nature" was by no means total. For a long time, it was confined to my mathematical work, while everywhere else, and particularly in my relationships with others, I followed the general trend of valuing and giving primacy to traits in me felt to be "virile", and repressing "feminine" traits. It's all about quite detailed in the group of notes entitled "Histoire d'une vie: un cycle en trois mouvements" (n° 107-110), which practically opens the *Clef du Yin et du Yang*.

to conform to the dominant values of the surrounding environment. This discovery

p. L34

□d'abord comme une simple curiosité. Only gradually, however, does it reveal itself as a key to the future. essential for an understanding of the Burial. Moreover - and this is something that seems to me of even greater significance - I now see very clearly and without residue of the slightest doubt this: that if, with by no means exceptional intellectual gifts, I have nonetheless been able to consistently give my full measure in my mathematical work, and to produce a work and give birth to a vision that is vast, powerful and fertile, it is to nothing other than this fidelity that I owe it, to this absence of any concern to conform to standards, thanks to which I abandon myself with total confidence to the original impulse for knowledge, without carving it up or amputating it in any way from what makes its strength and finesse and its undivided nature.

However, it's not creativity and its sources that are at the center of attention in this meditation "Burial (2) - or The Key to Yin and Yang", but rather "conflict", the state of blocked creativity, or the dispersion of creative energy by the confrontation, in the psyche, of antagonistic (usually occult) forces. The aspects of **violence**, of (seemingly) "gratuitous" violence, "for the sake of it", had disconcerted me more than once in Burial, and brought back a host of similar real-life situations. The experience of this violence has been in my life like "the hard, irreducible core of the experience of conflict". Never before had I confronted the formidable mystery of the very existence and universality of this violence in human existence in general, and in my own in particular. It is this mystery that is the focus of attention throughout the second half (the "yin" or "decline" side) of the meditation on yin and yang. It is during this part of the meditation that a deeper insight into the meaning of the Burial, and the forces at work within it, gradually emerges. It's also the part of Harvesting and Sowing that has been the most fruitful, it seems to me, in terms of self-knowledge, bringing me into contact with neuralgic questions and situations, and making me feel precisely this "neuralgic" character, which until last year had remained elusive.

Once I'd finished this interminable "digression" on yin and yang, I was still left, more or less, with my "two or three notes" to write (plus one or two more, at most, one of which already had its ready-made name "The four operations". . . ), to finish with Récoltes et Semailles.

p. L35

We know the rest: these "last few notes" ended up making up the longest part of Récoltes et Semailles, running to almost □five hundred pages. This, then, is the "fourth wave" of the movement. It is also the third and final part of L'Enterrement, and I've given it the name "Les Quatre Opérations" (The Four Operations), which is also the name of the group of notes ("Les quatre opérations (sur une dépouille)") that form the heart of this fourth breath of reflection. Here, in Récoltes et Semailles, is the "investigation" part in the strictest sense of the term - with the added twist, however, that this investigation is not confined to the purely "technical" aspect, to the "detective" aspect in short, but that the reflection here is driven above all, as elsewhere in Récoltes et Semailles, by the desire to know and understand. The tone is more "muscular" than in the first part of Burial, where I was still rubbing my eyes and wondering if I was dreaming or what! Nevertheless, the facts uncovered in the course of the pages often come at just the right moment, to illustrate on the spot many things that had only been hinted at in passing here and there, embodied in precise and striking examples. It's in this section, too, that the mathematical digressions take on an important role, stimulated by renewed contact (through the necessities of inquiry) with a substance I'd lost sight of for fifteen years. At the other end of the spectrum, there are also first-hand accounts of the misadventures of my friend Zoghman Mebkhout (to whom this section is dedicated), at the hands of a high-flying, unscrupulous "mafia" that he had never dreamed of when he embarked on the (admittedly fascinating, and seemingly innocuous) subject of the cohomology of varieties of all kinds. For a thread

For a brief guide through the intricate maze of notes, sub-notes, sub-sub-notes... of this entire "investigation" section, I refer you to the table of contents (notes 167' to 176<sub>7</sub>), and to the first of the notes in the package, "Le détective - ou la vie en rose" (n° 167'). I should point out, however, that this note, dated April 22, was then a little "overtaken by events", since, from one twist to another, this investigation, which I then thought (practically) completed, continued to brin de zinc for another two months.

This fourth breath lasted over four months in a row, from mid-February to the end of June. It's in this part of the reflection above all, through meticulous and obstinate "work on parts", that I've gradually established, as the days and pages go by, a concrete, tangible contact with the reality of the Burial; that I've managed to "familiarize" myself with it, in short, to some extent, notwithstanding the visceral reactions of refusal that it had mentioned above (and that it continues to arouse) in me, standing in the way of a true acquaintance. This lengthy reflection begins with a retrospective on Deligne's visit (mentioned above).

already in this <sup>□</sup>lettre), and it ends with the "last-minute" reflection on my relationship with Serre and on Serre's role in l'Enterrement<sup>39</sup>. It was to have tacitly put Serre "off the hook", in favor of that "taboo" I've already mentioned, which now seems to me perhaps the most serious gap remaining in my understanding of l'Enterrement, until last month - and it's this "last-minute" reflection that now seems to me to be the most important thing that this "fourth breath" of Harvest and Sowing has given me, for a less tenuous, more fleshed-out apprehension of Burial and the forces that express themselves within it. p. L36

### 3.11. Movement and structure

I think I've covered the most important things I wanted to tell you about Harvest and Sowing, so that you already know "what it's all about". Surely, I've said more than enough to enable you to judge whether you consider the (more than) thousand-page letter that follows to be "about you", or not - and consequently, whether or not you're going to read on. In the event of a "yes", I think it would be useful to add a few more explanations (of a practical nature, in particular) about the **form of Récoltes et Semailles**.

This form is the reflection and expression of a certain **spirit**, which I have tried to "convey" in the preceding pages. Compared with my previous publications, if there's one new quality that stands out in Récoltes et Semailles, and also in A la Poursuite des Champs from which it stems, it's undoubtedly **spontaneity**. Admittedly, there are some common threads, and some big questions, which give coherence and unity to the whole reflection. However, this process continues from day to day, without any pre-established "program" or "plan", without there ever being any question of fixing for me in advance "what had to be demonstrated". My aim is not to demonstrate<sup>□</sup> but to **discover**, to penetrate further into an unknown substance, to condense<sup>□</sup> what is still only sensed, suspected, glimpsed. I can say, without any exaggeration really, that in this work, there is not a single day or night of reflection that has taken place in the field of the "foreseen", in terms of the ideas, images, associations that were present at the moment I sat down. . L37

<sup>39</sup>In parts c, d, e, of the note "L'album de famille" (n° 173), the last of which is dated June 18 (exactly ten days ago). There is only one note or part of a note with a later date (namely, "Cinq thèses pour un massacre - ou la piété filiale", n° 176<sub>7</sub>, dated the following day, June 19). You will note that in this fourth part of Récoltes et Semailles, or "investigation part", unlike the others, the notes often follow each other in a logical rather than chronological order. For example, the last two notes of l'Enterrement (forming the final "De Profundis") are dated April 7, two and a half months before the note I just quoted. I would point out, however, that apart from the "investigation" part of l'Enterrement (3) (notes n° s 167'-176<sub>7</sub>), forming the "fifth beat" of the funeral ceremony (of which the Key of Yin and Yang is the second), the notes follow each other in the order in which they were written, with rare exceptions.

in front of a blank sheet of paper, stubbornly pursuing a stubborn "thread", or picking up another that has just appeared. Each time, what emerges from the reflection is different from what I would have predicted, had I ventured to try and describe in advance what I thought I saw before me. More often than not, reflection takes paths that are entirely unforeseen at the outset, leading to new, equally unforeseen landscapes. But even if it sticks to a more or less planned itinerary, what the journey reveals to me as the hours go by differs as much from the image I had when I set off as a real landscape, with its play of cool shade and warm light, its delicate perspective changing with the hiker's footsteps, and its countless sounds and nameless fragrances carried by a breeze that makes the grasses dance and the trees sing... - that such a living, elusive landscape differs from a postcard, however beautiful and successful, however "right" it may be.

p. L38 It's the thought pursued in one go, over the course of a day or night, that constitutes the undivided unit, the living, individual cell, as it were; in the whole of the reflection (Harvest and Sowing, in this case). The latter is to each of these units (or "notes"<sup>40</sup>, forming a melody . . . ) what the body of a The living organism is each of its individual cells, infinitely diverse, each fulfilling a place and function that belongs to it alone. Sometimes, however, in a single reflection, after the fact, we perceive important breaks, which make it possible to distinguish several such units or messages, each of which then receives its own name and thus acquires its own identity and autonomy. At other times, on the other hand, a reflection that had been cut short for one reason or another (usually fortuitous), is spontaneously extended the next day or the day after that; or a reflection pursued over two or more consecutive days appears, in retrospect, as if it had been continued in one go; it seems as if only the need to sleep has forced us, unwillingly, to include some (as it were "physiological") caesura, marked only by a lapidary indication of the date.

<sup>40</sup>Originally, when I wrote *Fatuité et Renouveau*, the word "note" was synonymous with "annotation", playing the role of a footnote. For reasons of typographical convenience, I preferred to reject these annotations at the end of the text (notes 1 to 44, pages 141 and 171). One of the reasons for this was that some of these "notes" or "annotations" extend over one or more pages, and become longer even than the text they are supposed to comment on. As for the undivided "units" of the "first draft" of the reflection, for want of a better name I called them "sections" (less off-putting than "paragraphs").

This situation, and the structure of the text, changes with the next part, which was originally called "L'Enterrement", and became "L'Enterrement (1)" (or "La robe de l'Empereur de Chine"). This reflection led to the double-note "Mes orphans" and "Refusal of an inheritance - or the price of a contradiction" (notes n°s 46, 47, pages 177, 192), coming as annotations to the ultimate "section" of Harvest and Sowing (or rather, of what was to be its Part I, or Fatuity and Renewal), "The weight of a past" (n° 50, p. 131). Subsequently, other annotations were added to this same section (notes n°s 44' and 50), and still other notes were added as annotations to "Mes orphelins", which in turn gave rise to new notes. not to mention, this time, real footnotes, when the planned annotations were (and remained, once black on white) of modest dimensions. So, theoretically, this whole part of *Récoltes et Semailles* (which was then supposed to constitute the second and final part) appeared as a set of "notes" to the "Weight of a Past" "section". Through acquired inertia, this subdivision into "notes" (instead of "sections") was maintained in the following three parts, where I use both the footnote (when its size allows) and the subsequent note referred to in the text, as a means of annotating a "first draft" of the reflection.

Typographically, the "note" is distinguished from the "section" (used in RS I as the basic unit of the "first draft" of the reflection) by a sign such as (1), (2) etc. (including the number of the note placed between brackets and "in the air", following a widespread usage for cross-references to annotations), placed either at the beginning of the note in question, or as a cross-reference to the appropriate place in the text that refers to it. Sections are designated by Arabic numerals from 1 to 50 (to the exclusion of annoying subscripts and superscripts, as I have been led to use for notes, by imperatives of a practical nature). That said, there is no essential difference between the function of the "sections" in the first part of Harvest and Sowing and that of the "notes" in the later parts. The comments I make about this function in the present part of my letter ("Spontaneity and structure") apply equally well to the "sections" of RS I, even though I use the common noun "notes".

For further details and conventions, including how to read the table of contents of L'Enterrement (1), please refer to Introduction, 7 (L'Ordonnement des Obsèques), especially pages xiv - xv.

(or even several) between consecutive paragraphs of the "note" in question, which is then distinguished as such by a unique name.

□ And so, each of the notes in *Récoltes et Semailles* has its own individuality, a face and a function that distinguish it from any other. For each one, I've tried to express its own particularity through its **name**, which is supposed to capture or evoke the essence, or at least something essential, of what it "has to say". I truly recognize each one, above all else, by name, and it's by that name that I call her whenever I need her assistance.

p. L39

Often the name has presented itself to me spontaneously, before I've even thought about it. It's its unexpected appearance that signals to me that this note I'm still writing is about to be finished - that it has said what it had to say, by the time I finish the paragraph I'm writing. ... Often, too, the name appears, just as spontaneously, as I reread the notes from the day before or the day before, before continuing my reflection. Sometimes, it changes somewhat in the days or weeks following the appearance of the new note, where it is enriched by a second name I hadn't thought of at first. Many notes have a double name, expressing two different, sometimes complementary, perspectives on their message. The first of these double-names that presented itself to me, right from the start of "*Fatuité et Renouveau*", was "*Rencontre avec Claude Chevalley - ou liberté et bons sentiments*" (n° 11).

Only twice have I already had a name in mind before starting a note - and both times, moreover, it was jostled by what happened next!

It's only with the benefit of hindsight, the hindsight of weeks or even months, that an **overall movement** and **structure** emerges in the set of notes that follow one another from day to day. I've tried to capture both through various groupings and sub-groupings of notes, each with its own name, giving it its own existence and function or message; a bit like the organs and limbs of the same body (to use the image from earlier), and such and such parts of its limbs. Thus, in "the Whole" of Harvest and Sowing, there are has the five "parts" I've already mentioned, each with its own structure: *Fatuity and Renewal*

is grouped into eight "chapters" I to VIII<sup>41</sup>, and all three parts forming □ I, *Enterrement* (which, themselves ...which have also gradually emerged over the months... .) consists of a long, solemn Procession of twelve "Cortèges" I to XII. The last of these, or rather the "**Funeral Ceremony**" (that's its name) towards which the eleven preceding Processions had been heading (without really suspecting anything, surely. . .) of the eleven preceding Cortèges, is of truly gigantic dimensions, commensurate with the Work whose solemn funeral it consecrates: it encompasses almost the entirety of RS III (L' *Enterrement* (2)) and the entirety of RS IV (L' *Enterrement* (3)), with its nearly eight hundred pages and one hundred and fifty notes (although initially, this famous ceremony was only intended to comprise two!). Tactfully conducted (and with his well-known modesty. . .) by the great officiant himself, the ceremony continues in nine separate "beats" or liturgical acts, opening with the **Funeral Eulogy** (as one would have expected), and concluding (as it should) with the final **De Profundis**. Two other of these "times", one called "**The Key of Yin and Yang**", the other "**The Four Operations**", each constitute (by far) the largest part of the part (III or IV) of Harvest and Sowing into which it is inserted, and indeed give the latter its name.

p. L40

Throughout Harvest and Sowing, I took great care (as if it were the apple of my eye!) with the table of contents, constantly reworking it to accommodate the ever-renewed influx of unexpected notes<sup>42</sup>, and

<sup>41</sup>In *Fatuity and Renewal*, I occasionally refer to these chapters as "parts" of Harvest and Sowing, not to be confused, of course, with the five parts already mentioned, which only appeared later.

<sup>42</sup>Among these unexpected notes are those that are "derived from a footnote that has grown prohibitively large". In most cases, I've placed it immediately after the footnote to which it refers, giving it the same number.

to reflect as accurately as I could the overall movement of thought and the delicate structure that emerges. It is in Parts III and especially IV (just mentioned), "The Key" and "The Four Operations", that this structure is at its most complex and interwoven.

p. L41 To preserve the spontaneity of the text, and the unforeseen aspects of thinking as it is continued and that it was actually experienced, I didn't want to preface the notes with their names, even though the latter each time only appeared after the fact. This is why I ☐te advise, at the end of When reading each note, refer to the table of contents to learn what the note is called; and also, on occasion, to be able to appreciate at a glance how it fits into the thinking already pursued, or even, into that yet to come. Otherwise, you run the risk of getting hopelessly lost in a seemingly indigestible and heterogeneous collection of notes with sometimes bizarre, not to say off-putting, numbering<sup>43</sup> ; like a traveller lost in a foreign city (pushed there strangely at the whim of generations and centuries... . ), without a guide or even a map to help him find his way. In the manuscript intended for printing, I intend to include the names of "chapters" and other groupings of notes and sections throughout the text, to the exclusion of the notes (or sections) themselves. But even then, the occasional recourse to the table of contents seems indispensable, to avoid getting lost in a jumble of hundreds of notes, following each other like a tail over more than a thousand pages... .

## 3.12. Spontaneity and rigor

Spontaneity and rigor are the two "shadow" and "light" sides of the same undivided quality. It is only from their marriage that that particular quality of a text, or of a being, is born, which we can try to evoke by an expression like "quality of truth". If spontaneity has been (if not absent, at least) at a minimum in my past publications, I don't think that its late blossoming in me has made me any less rigorous. Rather, the full presence of its yin companion gives rigor a new dimension, a new fruitfulness.

This rigor is exercised in relation to itself, ensuring that the delicate "sorting" it must perform in the multitude of things that pass through the field of consciousness, to constantly decant the significant or essential from the incidental or accessory, does not thicken and congeal into automorphisms of censorship and complacency. Only curiosity, only our thirst for knowledge, can awaken and stimulate such vigilance without heaviness, such vivacity, against the immense, omnipresent inertia of "natural" slopes, carved out by ready-made ideas, expressions of our fears and conditioning.

p. L42 And this same rigor, this same vigilant attention is also directed towards spontaneity as towards this which takes on its aspects, in order to take into account, here again, these natural "slopes", and to distinguish them ☐ from what truly springs from the deepest layers of being, from the original impulse of knowledge and action, taking us out into the world.

In terms of writing, rigor manifests itself in a constant concern to use language to define as precisely and faithfully as possible the thoughts, feelings, perceptions, images, intuitions... ... that need to be expressed, without settling for a vague or approximate term where the thing to be expressed has clear-cut contours, or for a term of artificial precision (and therefore just as distorting) to express something that is not clear-cut.

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with a superscript ' or ", or even "" if necessary - thus avoiding the prohibitive task of renumbering all subsequent notes already written at the same time! These notes, from one footnote to another, are preceded in the table of contents by the sign ! (at least in L'Enterrement (1)).

<sup>43</sup>For the rationale behind such seemingly bizarre numbering, I refer you to the previous footnote to this inexhaustible letter.

thing that remains shrouded in the mists of what is still only presentiment. When we try to capture it as it is in the moment, and only then, the unknown thing reveals its true nature to us, and into the full light of day perhaps, if it is made for the day and our desire incites it to strip itself of its veils of shadow and mist. Our role is not to pretend to describe and fix what we don't know and what escapes us, but to humbly, passionately take note of the unknown and the mystery that surround us on all sides.

In other words, the role of writing is not to record the results of a search, but the very process of the search - the labors of love and the works of our loves with Our Mother the World, the Unknown, who unceasingly calls us into herself to know her again in her inexhaustible Body, wherever in her the mysterious ways of desire take us.

In order to render this process, backtracking, which nuances, clarifies, deepens and sometimes corrects the "first draft" of the writing, or even a second or third, is part of the very process of discovery. They form an essential part of the text and give it meaning. This is why the "notes" (or "annotations") placed at the end of *Fatuité et Renouveau*, and referred to here and there throughout the fifty "sections" that make up the "first draft" of the text, are an inseparable and essential part of it. I urge you to refer to them as you go along, and at least at the end of each section where there are one or more references to such "notes". The same applies to footnotes in other parts of *Harvest and Sowing*, or references in a given "note" (here constituting the "main text") to a subsequent note, which then acts as a "return" to it, or as an annotation. This, along with my advice not to separate yourself from the table of contents while reading, is the main reading recommendation I have for you.

□ One last, practical question, which will close (somewhat prosaically) this letter that it's time to end. p. L43  
There's been a bit of "panic" at times, to get the various *Harvest and Seed* fascicles ready for printing by the Duplication Department at the Fac, in time for the print run to take place (if possible) before the summer vacations. In the rush, a whole sheet of last-minute footnotes to fascicule 2 (*L'Enterrement* (1) - or *La robe de l'Empereur de Chine*) was "skipped". It was mainly a matter of rectifying certain material errors that had only recently come to light, in the course of writing the *Four Operations*. One of these footnotes is more substantial than the others, and I'd like to mention it here. It's an annotation to the note "*La victime - ou les deux silences*" (n° 78', page 304). This note, in which I tried, among other things, to define my impressions (all subjective, admittedly) of the way in which my friend Zoghman Mebkhout "internalized" at the time the iniquitous spoliation of which he was the victim, was felt by him to be unfair to him, whereas I seemed almost to be putting him "in the same bag" with his spoliators. What is certain is that in this note, which does not pretend to give anything other than impressions linked to a particular "moment", I am only presenting one sound of the bell, leaving unsaid (and as a matter of course, no doubt) certain other sounds just as real (and less debatable perhaps). The fact remains that, a year on, the reflections on this delicate subject are considerably deepened in the note "*Racines et Solitude*" (n° 171). Zoghman had no reservations about it. Further food for thought on the same subject can also be found in the two notes "*Trois jalons - ou l'innocence*", and "*Les pages mortes*" (n° s 171 (x) and (xii)). These three notes are part of "*L'Apothéose*", which is the section of *Quatre Opérations* devoted to the appropriation and misappropriation of Zoghman Mebkhout's work.

All that's left for me to do is wish you happy reading - and I look forward to reading you too!

Alexandre Grothendieck

## Postscript epilogue - or context and prerequisites for a debate

February 1986

### 3.13. The spectograph at bottles

p. L44     □ It's been a full seven months since this Letter was written, and nearly four months since it was sent, with the "pavement" that goes with it. And with a dedication from my hand in each<sup>44</sup>. Like a "bottle to the sea", or rather, like a whole flock of such wandering bottles, my message landed and circulated to the furthest corners of this mathematical microcosm with which I was once familiar. And through the direct and indirect echoes that come back to me over the days, weeks and months, here I am, unexpectedly, like a vast X-ray of the mathematical milieu, taken by a sprawling spectograph, of which my innocent "bottles" are so many travelling antennae. And so (noblesse oblige!), even though I'm not short of things to do, I find myself faced with the new task of deciphering the radio and reporting, as best I can, on what I've read there. This will be the sixth (and last, I promise!) part of Récoltes et Semailles. This will crown, God willing, "the great sociological work of my old age". For now, a few initial comments.

When it comes to greeting my modest, hand-crafted flotilla, what seems to dominate by far is the half-snidey, half-snarky tone, along the lines of "here's Grothendieck getting paranoid in his old age", or "here's one who takes himself very seriously" - and there you have it! However, I've only had one letter in that style<sup>45</sup>, plus two more in the vein of hushed, self-pleasured derision<sup>46</sup>. Most of my mathematician addressees, including some of my former students, responded with silence<sup>47</sup> - a silence that tells me a lot.

p. L45     □ That doesn't stop me from having already had a voluminous correspondence. Most of the letters are in the tones of polite embarrassment, which is often intended to be friendly, as if for the sake of propriety. Two or three times I've felt, behind this embarrassment and as if sifted by it, the warmth of an ever-living feeling. More often than not, when the embarrassment isn't expressed by protestations of good feeling (on one's own behalf, or on behalf of others), it's expressed by compliments - I've never received so many in my life! To the tune of "great mathematician", "superb pages" (on creativity "and all that". . .), "indisputable writer", and so on. For good measure, I even received a heartfelt (and by no means ironic) compliment on the richness of my inner life. Needless to say, in all these letters, my correspondent is careful not to go into the heart of any matter, and even less, to get personally involved; the tone is more that of one who has been "asked to give his opinion" (to use the words of one of these letters), on a rather scabrous and, what's more, hypothetical or even imaginary affair, and in any case and above all, one that doesn't **concern him personally**. Whenever he even pretends to touch on one of these issues, it's with his fingertips and in order to keep it as far away from him as he can - whether it's with the help of some good advice I've given myself, or with cautious conditionals, or with the usual commonplaces when one doesn't know too much.

<sup>44</sup>There are a few rare exceptions, mostly comprising colleagues I don't know personally, who received only issues 0 and 4 of the provisional print run, as a bonus for their active participation in my Funeral.

<sup>45</sup>This letter comes from one of my former students, and also one of my co-internees.

<sup>46</sup>From two of my former colleagues at Bourbaki, one of whom is one of the elders who warmly welcomed me when I started out.

<sup>47</sup>For one hundred and thirty-one items sent to mathematicians, fifty-three of the recipients have so far given some sign of life, even if only to acknowledge receipt. These include six of my ex-students - I haven't heard from any of the other eight.



what to say, or in any other way. Still, some of them hinted that there might have been some not-so-normal things going on - all the while taking care to leave what and who they were about as vague as possible. ...

I also received some frankly warm feedback from fifteen or sixteen of my old and new friends. Some of them expressed emotion, with no desire to hide it or keep it quiet. These echoes, and others just as warm from outside the mathematical world, have been my reward for a long and solitary work, done not only for myself, but for all.

And of the hundred and thirty or so colleagues who received my Letter, three responded to it, in the full sense of the word, by getting involved themselves, rather than confining themselves to a distant commentary on the events of the century. I received yet another such response from a non-mathematical correspondent. These were real **responses** to my message. And that, too, was my greatest reward.

### 3.14. Three feet in a flat

□ Many of my mathematician colleagues and friends have expressed the hope that Harvest and Sowing will open up p.  
L46

a wide-ranging **debate** in the mathematical community, on the state of mores in this milieu, on the ethics of the mathematician, and on the meaning and purpose of his work. For the moment, the least we can say is that it's not going that way. As of now (and to make the pun de rigueur), the debate on a funeral looks set to be replaced by the burial of a debate!

However, whether we like it or not, and despite the silence and apathy of many, a debate is well and truly underway. It is unlikely that it will ever take on the scale of a real public debate, or even (heaven forbid!) the pomp and stiffness of the "official" debate. In any case, there are many who have already jumped the gun, shutting it down in their own minds before they've even heard about it, on the strength of the eternal, unchanging consensus that "all's for the best in the best of all possible worlds" (mathematics, in this case). Perhaps, however, a challenge will eventually come from the **outside**, gradually, from "witnesses" who, not being part of the same milieu, are not prisoners of its group consensus, and who therefore don't feel (even inwardly) personally challenged.

In almost all the feedback I've received, I've noticed the same confusion over the two preliminary questions: **what does** the "debate" posed (at least tacitly) by Récoltes et Semailles concern; and who is qualified to take cognizance of it and give an opinion on it, or else: to form an opinion in full knowledge of the facts. In this connection, I'd like to make **three points of reference**. Of course, this will not prevent those who want to remain confused from continuing to do so. At least, for those who would like to know what it's all about, perhaps it will help them not to be distracted by all kinds of noise (even the best-intentioned. . . ).

a) Sincere friends assure me that "everything will work out in the end" (or "everything", I suppose, means "things" that have been damaged... . ); all I had to do was go back to school, "make my mark with new work", give lectures etc. - and the others would do the rest. We'll generously say "We has been a bit unfair with this Grothendieck fellow", and to discreetly rectify the situation with more or less convincingly<sup>48</sup> (\*); □ voire, patting him on the shoulder with a paternal air and giving him "big p. L47 mathematician", just to calm down a respectable quidam who, alas, is pretending to get angry and make unwanted waves.

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<sup>48</sup>(\*) I've already noticed several such discreet signs, showing that the lion has woken up. ...

It's not a question, as these friends suggest, of "letting go of ballast" or making others let go of ballast. I, for one, have no need of compliments or even of sincere admirers, nor of "allies", for "my" cause or for any cause whatsoever. It's not about me, who's doing just fine, nor about my work, which speaks for itself, even to the deaf. If this debate also concerns, among others, my person and my work, it's simply as **revelations of** something else, through the reality of a Funeral (most revelatory indeed).

If there is "someone" who I feel should inspire a sense of alarm, concern and urgency, it is by no means myself, nor any of my "co-interred". But it is a collective being, at once elusive and very tangible, often spoken of but never examined, who is not "**the mathematical community**".

Over the past few weeks, I've come to see her as a flesh-and-blood person, whose body is stricken with a deep-seated gangrene. The best food, the choicest dishes, in her turn into poison, causing the evil to spread and become more entrenched. And yet, there's an irresistible bulimia to gorge herself on more and more, as a way of giving herself the slip, surely, about an evil she wouldn't want to know anything about. Anything we say to her is wasted - even the simplest words have lost their meaning. They cease to carry a message, and serve only to trigger the triggers of fear and refusal. . .

b) Most of my colleagues or former friends, however well-disposed, when they hazard an opinion, surround themselves with cautious conditionals, like "if it were true that... .. it would indeed be inadmissible" - just so they can sleep soundly again. I thought I'd made myself clear...

With the benefit of seven months' hindsight, I can now state that **for almost all the facts** reported and commented on in Récoltes et Semailles, **their reality is uncontroversial**. I'll come back later to the few exceptions, which will be noted as such, each in its own place, for all the other facts, after writing the primitive version of □ Récoltes et Semailles, a confrontation careful discussions with some of the main people involved (namely, Pierre Deligne, Jean-Pierre Serre and Luc Illusie) enabled us to eliminate errors of detail, and reach unambiguous agreement on the material facts themselves<sup>49</sup> (\*).

p. L48

Thus, the debate is not about the reality of the facts, which is not in question, but about **whether the practices and attitudes described by these facts should be considered as accepted and as "normal", or not**.

We're talking here about practices that in my testimony I describe (perhaps wrongly. . . ) as scandalous; as abuses of trust or power and as blatant dishonesty, reaching on more than one occasion the dimension of the iniquitous and the shameless. The unimaginable thing I had yet to learn, after having been made aware of these facts (unthinkable even fifteen years ago), was that the vast majority of my mathematical colleagues, and even those who were once my students or friends, now consider these practices to be normal and perfectly honourable.

c) There is a second way in which many of my colleagues and former friends maintain confusion. It's to the tune of "sorry, but we're not specialists in this field - don't ask us to take cognizance of facts, which (providentially. . . ) go right over our heads. . .".

On the contrary, I assert that you don't need to be a "specialist" (sorry about that!), or even know your multiplication table or the Pythagorean theorem, to get the main facts.

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<sup>49</sup>(\*) I am pleased to express my gratitude to all three for the goodwill they have shown on this occasion, and acknowledge their complete good faith in all matters of material fact.

Not even having read "Le Cid" or the Fables de la Fontaine. A normally-developed ten-year-old is just as capable of this as the most renowned specialist (or even better than him. . . )<sup>50</sup> (\*\*).

Allow me to illustrate this point with just one example, the "first come" from l'Enterrement<sup>51</sup> (\*\*\*) .

There's no need to know the ins and outs of the multifaceted and highly delicate mathematical concept of p . L49 of "motive", nor of having only his school certificate, to take note of the following few facts, and to pass judgment on them.

1° ) Between 1963 and 1969, I introduced the notion of "pattern" and developed a "philosophy" and "theory" around it, which have remained partially conjectural. Rightly or wrongly (it doesn't matter here), I consider motif theory to be the most profound contribution I have made to the mathematics of my time. Indeed, the importance and depth of "motivic yoga" is no longer disputed by anyone (after ten years of almost complete silence on the subject, immediately after my departure from the mathematical scene).

2° ) In my first and only book (published in 1981), devoted essentially to the theory of motives (and in which this name, introduced by me, appears in the title of the book), the one and only passage that could lead the reader to suspect that my modest person is in any way connected with any theory that might resemble the one developed at length in this book, is on page 261. This passage (two and a half lines long) consists of explaining to the reader that the theory developed there has nothing to do with that of a man called Grothendieck (a theory mentioned there for the first and last time, without any further reference or precision).

3° ) There's a famous conjecture, the so-called "Hodge conjecture" (never mind what it's actually about), whose validity would imply that the so-called "other" pattern theory developed in the brilliant volume, is identical to (a very special case of) the one I had developed, in plain sight, nearly twenty years before. I

might add a 4° ) that the most prestigious of the book's four co-authors was my student,

and that it is from none other than me that he has learned over the years the brilliant ideas he presents here

as if he had just found them<sup>52</sup> (\*), and 5° ) that these two circumstances are public knowledge among well-informed people, but that it would be futile to look in literature for a written trace attesting

that said brilliant author might have learned something from my mouth<sup>53</sup> (\*), and that 6° ) the delicate issue p. L50 of arithmetic which (according to what the main author himself explained to me) constitutes the central problem of the book (and without my name being mentioned), had been worked out by me in the sixties, in the wake of the "yoga of patterns", and that it was through me that the author became aware of it; and I could pile on more 7° and 8° etc (which I certainly don't fail to do in its place ).

The foregoing will suffice for my purpose, which is this. You don't need any special "skills" to take note of such facts and pass judgment on them - **that's not "where it's at"**. The faculty at stake here, apart from sound reason (which in principle is available to everyone), is what I would call a **sense of decency**.

The book in question is now one of the most cited in mathematical literature, and its "principal author" one of the most prestigious mathematicians of the time. That said, the most remarkable thing about this story, in my view, is that no one among the countless readers of

---

<sup>50</sup>(\*\*) Of course, I didn't write Récoltes et Semailles for ten-year-olds, and I'd use language they're familiar with.

<sup>51</sup>(\*\*\*) This was the first "great operation" of Burial that I discovered, on a certain April 19, 1984, when the name "l'Enterrement" also imposed itself on me. On this subject, see the two notes written on the same day, "Souvenir d'un rêve - ou la naissance des motifs", and "L'Enterrement - ou le Nouveau père" (Res III, n° s 51, 52). The full reference of the book is also included.  
we're going to talk about.

<sup>52</sup>(\*) I don't mean to say that there aren't ideas in this book, and even fine ones, due to this author or the other co-authors. But the whole problematic of the book, and the conceptual context that gives it its meaning, up to and including the delicate theory of *X-categories* (wrongly called "Tannakian"), which technically constitutes the heart of the book, are my work.

<sup>53</sup>(\*) With the exception of one line in a report by Serre in 1977, which will be discussed later.

I'm sure that I've **never seen anything wrong with** this book, even among those who know first-hand what it's about, and who were my students or friends. In any case, not one of them, even today as I write these lines, has made himself known to me to express the slightest reservation about this prestigious book<sup>54</sup> (\*\*).

As for those of my colleagues and former friends who have never held this book in their hands and use it to plead incompetence, I say to them: you don't need to be a "specialist" to ask for the volume in the first mathematical library you come across, leaf through it, and see for yourself what nobody disputes... .

### 3.15. Gangrene - or the zeitgeist (1)

p. L51

15. This "pattern operation" is only one of four "large operations" of the same water, and among them a host of others on a lesser scale and in the same spirit. This is by no means the "biggest" of the collective mystifications that fill out my "tableau de moeurs" of an era, nor especially the most iniquitous.

It only consisted in plundering the rich man's flock, in his absence (or death. . . ), and not in coming (with general indifference) to strangle the poor man's sheep for his own pleasure and before his very eyes. And even in the mathematical language now in common use, the seemingly innocuous names of books, notions or statements quoted at any given moment are in themselves already a mystification or imposture<sup>55</sup> , and bear witness in their own way to the disgrace of an era.

If I think I've ever done anything useful for the "mathematical community", it's that I've brought to the light of day a number of less-than-glamorous facts that were lurking in the shadows. The kind of facts, surely, that everyone comes into contact with every day, from near or far. But how many of them took the time to stop, even for a moment, to smell the air and look around?

He who found himself the victim of some people's arrogance and others' (or the same people's) dishonesty, perhaps flattered himself that this was a very special misfortune that had fallen to him. If he were to compare his experience with my own, he might well realize that this "misfortune" is also a name he has given to a **zeitgeist** that weighs on him as it does on everyone else. And (who knows!) perhaps this will encourage him to get involved in a debate that concerns him as much as it concerns me.

p. L52

But if this "dirty laundry" that I'm "airing in the public square" elicits nothing more than mirthless sneers of some and the polite embarrassment of others, in the indifference of all, a situation that was murky will have become very clear. (At least for those who still care to use their eyes.) Traditional consensus of good faith and decency<sup>56</sup> , in the relationship between mathematicians and in that of the mathematician to his art, are now things of the past, "outdated". Although it has yet to be solemnly proclaimed by any international association of mathematicians, the fact is that it is now a matter of course, and virtually official: the "cooptation fraternity" of those who wield power in the mathematical world **is now allowed to pull all the stops**, without any reservations or limitations. All the shenanigans of ideas

<sup>54</sup>(\*\*) In all, two colleagues (including Zoghman Mebkhout) have expressed such "reservations" to me. Neither of them can be considered "readers" of this book. They looked at it out of curiosity, just to see for themselves... ..

<sup>55</sup>I'm thinking here, in particular, of the unusual acronym "SGA 4<sup>1</sup>" (fractional numbers are useful!), which is a double imposture in itself (and one of the most quoted acronyms in contemporary mathematical literature), and of the names "Verdier duality" or "dual".

de Verdier", "Deligne-Grothendieck conjecture", or "Tannakian categories" (where Tannaka is not involved, as he was never consulted). . . ). We'll discuss this in more detail later.

<sup>56</sup>When I speak of these "consensuses of good faith and decency", I do not mean to imply that they were never transgressed. But even when they were transgressed, they were indeed "transgressions", and the consensus itself was nonetheless accepted.

to lead the apathetic reader, who only wants to believe, by the nose, all the paternity deals, and the bogus quotations between accomplices and the silence for those condemned to silence, and the cronyism and falsifications of all kinds and even the crudest plagiarism in plain sight - **yes and amen to everything**, with the blessing, by word or by silence (if not by active and eager participation), of all the "big names" and all the big and small bosses in the mathematical public arena. Yes and amen to the "**new style**" that's all the rage! What was once an art form has now become, with (almost) unanimous assent, a muddle-headed fair under the paternal eye of the bosses.

There was a time when the exercise of power in the world of mathematicians was limited by unanimous, intangible consensus, the expression of a collective sense of **decency**. Today, these consensuses and this sentiment are outdated and unworthy of the glorious era of computers, space cells and the neutron bomb.

It would be a foregone conclusion: power, for the brotherhood of those who wield it, is a means to an end. **discretionary power**.

### 3.16. Amende honorable - ou l'esprit du temps (2)

16. In the Lettre, I explained sufficiently the spirit in which I wrote Récoltes et Semailles, to make it clear that I make no claim to be a historian. It is an honest account of first-hand experience, and a reflection on that experience. Testimony and reflection are available to all, including the historian, who can use it as one of several types of material. It is then up to the historian to subject this material to critical analysis, in accordance with the rigorous canons of his art.

We must, of course, distinguish between facts in the narrow sense ("brute **facts**" or "material facts"), and the "evaluation" or ☐ "**interpretation**" of these facts, which gives them a **meaning**, which is not the same, for p.

L53

one observer (or co-actor) and for another. Roughly speaking, we can say that the "testimony" aspect of Récoltes et Semailles concerns the facts, and that its "reflection" aspect concerns their interpretation, i.e. my work to give them meaning. Among the "facts" that make up the testimony, I also include the "psychic facts", and in particular the feelings, associations and images of all kinds that my testimony reflects, whether these took place in the more or less distant past, or at the very moment of writing.

For the events I describe or report in Récoltes et Semailles, I distinguish three kinds of **sources**. There are the facts recounted to me by **memory**, more or less precise or blurred from one occasion to the next, and sometimes distorted. I can vouch for their truthfulness at the time of writing, but by no means for their absence of error. On the contrary, I have had occasion to point out a number of them, errors of detail which I point out in their place in later footnotes. On the other hand, there are **written documents**, in particular letters and, above all, scientific publications in due form, to which I refer from time to time with all due precision. Finally, there is the **testimony of third parties**. Sometimes they complement my own memories, allowing me to revive, clarify and, sometimes, correct them. On rare occasions (to which I'll return shortly), this testimony brings me entirely new information compared to what I already knew. When I happen to echo such testimony, it doesn't mean that I've had the opportunity to verify its accuracy and validity down the line, but simply that it fits plausibly enough into the rich tapestry of facts that were known to me first-hand, to lead me to believe (rightly or wrongly. . . ) that it was essentially true.

For an attentive reader, I don't think there will be any difficulty, at any point, in "distinguishing" between an account of the facts and an interpretation of them, and (in the former case) in discerning which of the three sources I've just described comes into play.

\*       \*

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□  
p. L54 When I alluded a moment ago to the testimony of a third party, which I echoed without having been able to "verify its validity on all counts", I was referring to that of **Zoghman Mebkhout**, concerning the vast cover-up operation surrounding his work. Of the "material facts" I mention in *Récoltes et Semailles*, the only ones that are now subject to controversy, or which, in my own judgment at present, require rectification, are some of the facts attested by Mebkhout's testimony alone. To conclude this postscript, I would like to present here some critical comments on the version of the "Mebkhout affair" presented in the provisional printing of *Récoltes et Semailles*. More detailed comments and corrections will be included, each in his or her own place, in the printed edition (constituting the definitive text of *Récoltes et Semailles*).

The "Mebkhout version", which I have tried to interpret, seems to me to consist essentially of the following two theses:

1. Between 1972 and 1979, Mebkhout was the only<sup>57</sup>, in the general indifference and inspired by my work, to develop the "philosophy of  $D$ -Modules", as a new theory of "cohomological coefficients" in my sense.
2. There would have been a unanimous consensus, both in France and internationally, to disregard his name and his role in this new theory, once its scope began to be recognized.

This version was well documented, firstly by Mebkhout's own convincing publications, and secondly by numerous publications by other authors (in particular, by the **Actes** du Colloque de Luminy of June 1981), where the deliberate intention of concealment cannot be doubted. Finally, the more detailed details Mebkhout provided me with later (and which I echo in the section "L'Enterrement (3) - ou les Quatre Opérations"), while not directly verifiable, were nonetheless concordant entirely with a certain general mood, the reality of which I could no longer doubt.

p. L55 □ I have just become aware of several new facts<sup>58</sup>, which show that there is reason to nuance strongly- See point 1°) above. The isolation in which Mebkhout found himself<sup>59</sup> was indeed real, but it was a relative isolation. In France, there was the work of **J.P. Ramis** on the same subject (work of which Mebkhout said nothing to me), and above all, it appears that certain important ideas developed and brought to fruition by Mebkhout, and of which he takes credit, could be due to Kashiwara<sup>60</sup>. This, in turn, renders the

<sup>57</sup>With the exception of Kashiwara's 1975 constructibility theorem, whose importance in the theory is by no means disputed. But according to Mebkhout's version, this was Kashiwara's one and only contribution to the emerging theory. This (inaccurate) version was corroborated by the absence of other publications by Kashiwara, in which he would have alluded to at least some of the key ideas.

<sup>58</sup>I am grateful to Pierre Schapira and Christian Houzel for drawing my attention to these facts, and to the tendentious nature of my presentation of the Mebkhout-Kashiwara dispute.

<sup>59</sup>This isolation stemmed above all from my ex-students' indifference to Mebkhout's ideas and work, which stubbornly pretended to draw inspiration from an "ancestor" doomed to oblivion by unanimous consensus... ..

<sup>60</sup>The most important of these ideas is the so-called "Riemann-Hilbert correspondence" (to use the new-style jargon) for  $D$ -Modules. The relevant conjecture was proved by Mebkhout, and also (as Schapira tells me) by Kashiwara (although Mebkhout assured me that his was the only published proof). The question of priority for

blable or dubious some of the episodes in the Kashiwara-Mebkhout dispute, as recounted in the Mebkhout version whose (overly) faithful interpreter I have made my own.

There's no doubt that Mebkhout was one of the main pioneers of the new D-Modules theory, perhaps even **the** main pioneer, in terms of his "work on the pieces" and the conception of some of the ideas he was able to bring to fruition; in any case, he was the only one to put his heart and soul into this task, the true scope of which still eluded him, just as it eluded everyone else. And it's also true that the cover-up that took place around this work, culminating in the Colloque de Luminy, remains for me one of the great disgraces of the century in the mathematical world. But it would be wrong to claim (as I have done in good faith) that Mebkhout was alone in his task. On the other hand, he was the only one to have the honesty and courage to state clearly the importance of my ideas and my work in his work and in the emergence of the new theory.

This postscript is not the place to go into more detail on this affair - I shall do so in its place, including comments that may shed light on the psychological context of the "Mebkhout version". If the "Mebkhout-Kashiwara dispute" is of interest to me, it is only insofar as it sheds light on the general atmosphere of an era. And for me, the "Mebkhout version", right down to its very distortions and the forces that played a part in bringing them about, also appears to be, among other lesser-known materials, the most important one. to the "record of an era", an eloquent "sign of the times".

□ It remains for me to make amends for levity, presenting of the Mebkhout-Kashiwara dispute a picture that took into account only the testimony and documents provided by Mebkhout, as if there could be no doubt about this version. This version presented a third party in a ridiculous, even odious light, all the more reason to exercise caution. For my thoughtlessness and for this lack of healthy caution, I gladly offer Mr. Kashiwara my sincerest apologies.

p. L56

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demonstration is still a mystery to me, and I've given up on spending the rest of my life trying to get to the bottom of it. ...

As for the sister-statement in terms of  $D^\infty$  Modules, there doesn't seem to be the slightest doubt that Mebkhout is responsible for both the idea and the demonstration.

## 2. A walk through a work or the Child and Mother



# 4. Introduction

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## 4.1. (I) The five-leaf clover

### 4.1.1. 1. Dream and fulfillment

□ Three years ago in July, I had an unusual dream. When I say "unusual", I meanp  
I'm talking about an impression that came to me only after the fact, when I thought about it when I woke up. The dream itself came to me as the most natural, obvious thing in the world, without fanfare - so much so that when I woke up, I almost didn't pay any attention to it, just pushed it into oblivion and moved on to "the order of things".  
of the day". Since the day before, I had been reflecting on my relationship with mathematics. It was the first time in my life that I'd taken the trouble to look into it - and even then, if I'd gone into it at all, it was because I'd really been forced to! So many strange, not to say violent, things had happened in the preceding months and years - explosions of mathematical passion bursting into my life out of the blue - that it was no longer possible to ignore what was going on.  
The dream I'm talking about had no scenario or action of any kind. It consisted of a single image, motionless but at the same time very much alive. It was the head of a person, seen in profile. He could be seen looking from right to left. It was a middle-aged man, beardless, his wild hair forming a halo of strength around his head. The impression given off by this head was that of a youthful, joyful strength, which seemed to spring from the supple, vigorous arch of the nape of the neck (which we guessed more than we saw). The expression on his face was more that of a mischievous little boy, delighted by whatever blow he might come upon.

or meditate to do, than that of a mature man, or of one who has grown in stature, mature or not. Above all, it exuded an intense, restrained joie de vivre, bursting into play. . .

There was no second person present, no "I" looking at or contemplating this other person, whose head was all we could see. But there was an intense perception of this head, of what emanated from it. Nor was there anyone to feel impressions, to comment on them, to name them, or to stick a name to the perceived person, designating him or her as "so-and-so". There was only this living thing, this man's head, and an equally vivid, intense perception of this thing.

p. II      As for waking up, without deliberate purpose, I remembered the dreams of the night gone by, the vision of that head

man didn't stand out on the number with any particular intensity, it didn't thrust itself forward to shout at me or whisper to me: it's me you've got to look at! When this dream appeared in the field of my quick glance at the night's dreams, in the warm stillness of the bed, I naturally had the reflex of the waking mind to put a name to what had been seen. All I had to do was ask, and I knew at once that the man's head that had been there in the dream was none other than my own. That's not a bad one, I thought, it's got to be done, seeing yourself in a dream like that, as if it were someone else! This dream came to me a little as if I'd stumbled across a four-leaf clover, or even a five-leaf clover, while out for a walk, and I'd marvelled at it for a few moments.

and carry on as if nothing had happened.

At least, that's how it almost happened. Fortunately, as has happened to me many times in situations of this kind, I made a mental note of this little "not bad" incident, starting a reflection that was supposed to build on the previous day's. Then, one thing led to another, and that day's reflection was confined to exploring the meaning of this unpretentious dream, this "not bad" incident. Then, one thing leading to another, that day's reflection was limited to immersing myself in the meaning of this unpretentious dream, this unique image, and the message about myself that it brought me.

This is not the place to dwell on what this one-day meditation taught and brought me. Or rather, what the **dream** taught me and brought me, once I'd put myself in the mood of attention and listening that enabled me to accept what it had to say. The first immediate fruit of the dream and this listening was a sudden influx of new energy. This energy supported the long-term meditation that continued over the following months, against stubborn inner resistance, which I had to dismantle one by one through patient, obstinate work.

In the five years since I started paying attention to some of the dreams that came to me, this was the first "messenger dream" that didn't present itself in the now-recognizable guise of such a dream, with impressive scenic means and an exceptional, sometimes overwhelming intensity of vision. This one was all about "cool", with nothing to force attention, discretion even - take it or leave it, with no fuss.

p. III      □A <sup>few</sup> weeks earlier, a messenger dream had come to me in the old style, on the dramatic diapason and even savage, which put a sudden and immediate end to a long period of mathematical frenzy. The only similarity between the two dreams was that in neither was there an observer. In a parable of lapidary force, this dream showed something that was going on in my life at the time, without my taking the trouble to pay attention to it - something I was even taking great care to ignore, to tell the truth. It was this dream that made me realize the urgency of a process of reflection, which I embarked upon a few weeks later, and which then continued for almost six months. I'll have a chance to talk a little about it in the last part of this reflection-testimony, "**Récoltes et Semailles**", which opens the present volume and gives it its name<sup>1</sup>.

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<sup>1</sup>See section 43, "The troublemaker boss - or the pressure cooker".

If I have begun this introduction by evoking this other dream, this image-vision of myself ("Traumgesicht meiner selbst" as I called it in my German notes), it's because in recent weeks the thought of this dream has come back to me more than once, as the meditation "on a mathematician's past" drew to a close. In retrospect, the three years since the dream appear to me as years of decantation and maturation, towards a fulfillment of its simple, limpid message. The dream showed me "**as I am**". It was also clear that in my waking life I was not fully who the dream showed me to be - weights and stiffnesses from afar often prevented (and still prevent) me from being fully and simply myself. During those years, when the thought of that dream rarely came back to me, it must have acted in some way. It was by no means a kind of model or ideal to which I would have strived to resemble, but a discreet reminder of a joyful simplicity that "was me", that manifested itself in many ways, and that was called upon to free itself from what continued to weigh upon it and to blossom fully. This dream was both a delicate and vigorous link, between a present still weighed down by many past burdens, and a very near "tomorrow" that this present contains in germ, a "tomorrow" that is me right now, and that has surely always been in me. ...

Surely, if in these last few weeks this rarely evoked dream has been very present again, it is because at some level that is not that of a ☐thought that probes and analyzes, I must have "known" that the work I wasp . iv  
work, which took up and deepened that other work of three years ago, was a new step towards fulfilling the message about myself that it brought me.

For me, this is now the main meaning of Harvest and Sowing, of this intense work of almost two months. Only now that it's finished, do I realize how important it was for me to do it. During the course of this work, I experienced many moments of joy, often mischievous, joking, exuberant joy. And there have also been moments of sadness, and times when I've relived frustrations or sorrows that have hit me painfully in recent years - but there hasn't been a single moment of bitterness. I leave this work with the complete satisfaction of one who knows he has completed a job. There is nothing, no matter how "small", that I have avoided, or that I would have cared to say but didn't, and which at this moment would leave me with the residue of dissatisfaction, of regret, no matter how "small".

When I wrote this testimonial, it was clear to me that it would not please everyone. It's even possible that I've found a way to displease everyone, without exception. But that wasn't my intention at all, or even to upset anyone. My purpose was simply to look at the simple and important things, the everyday things, from my past (and sometimes from my present too) as a mathematician, to discover at last (better late than never!) and without the shadow of a doubt or reservation, what they were and what they are; and, along the way, to say in simple words what I saw.

#### 4.1.2. 2. The spirit of a trip

This reflection, which eventually became "Récoltes et Semailles", had begun as an "introduction" to the first volume (now nearing completion) of "**A la Poursuite des Champs**", the first mathematical work I've been intending to publish since 1970. I had written the first few pages at a low point, in June last year, and resumed this reflection less than two months ago, at the point where I had left it. I realized that there was a lot to look at and a lot to say, so I expected an introduction

thirty or forty pages. Then, for the nearly two months that followed, up to now even ☐o where I'm writing this new introduction to what was first an introduction, I believed everyp . v

day that it was the day I'd finish this work, or that it would be the next day, or the day after that at worst. When, after a few weeks, I began to approach the hundred-page mark, the introduction was promoted to "introductory chapter". After a few more weeks, when the dimensions of this "chapter" far exceeded those of the other chapters in the volume in preparation (all completed at the time of writing, except for the last one), I finally understood that it didn't belong in a maths book, that this reflection and this testimony would definitely be cramped. Their true place was in a separate volume, which will be volume 1 of these "**Mathematical Reflections**" that I intend to continue in the years to come, following on from the *Poursuite des Champs*.

I wouldn't say that *Récoltes et Semailles*, this first volume in the *Réflexions Mathématiques* series (to be followed by two or three volumes of *Poursuite des Champs*, to start with) is an "introduction" volume to the *Réflexions*. Rather, I see this first volume as the foundation for what's to come, or rather, as the one that sets the tone, the **spirit** in which I'm embarking on this new journey, which I intend to pursue in the years to come, and which will take me I can't say where.

To conclude these clarifications concerning the main part of the present volume, a few indications of a practical nature. The reader will not be surprised to find in the text of *Récoltes et Semailles* occasional references to the "present volume" - by which I mean the first volume (*Histoire de Modèles*) of *Poursuite des Champs*, whose introduction I believe I am still writing. I didn't want to "correct" these passages, as I wanted above all to preserve the text's spontaneity, and its authenticity as a testimony not only to a distant past, but also to the very moment I'm writing.

It's for the same reason, too, that my reworking of the first draft of the text was limited to correcting stylistic clumsiness or a sometimes confused expression that interfered with the understanding of what I wanted to express. In some cases, these alterations led me to a clearer or more refined understanding than when I wrote the first draft. Slightly substantial modifications to the text, to nuance, clarify, complete or (sometimes) correct it, are the subject of some fifty numbered **notes**, grouped together at the end of the reflection, and which make up more than a quarter of the text<sup>2</sup>. I refer to them by acronyms such as (1) etc. . Among these notes, I have singled out twenty or so that seemed to me of comparable importance (in length or substance) to any of the fifty "sections" or "paragraphs" into which the reflection was spontaneously organized. These longer notes have been included in the table of contents, after the list of fifty sections. Not surprisingly, for some of the longer notes, there was a need to add one or more notes to the note. These are then included following the note, with the same type of cross-references, except for shorter notes, which then appear on the same page as "footnotes", with cross-references such as or.

I took great pleasure in naming each of the sections of the text, as well as each of the more substantial notes - not to mention the fact that, later on, this proved to be indispensable for finding my way around. It goes without saying that these names were found after the fact, whereas when I began a section or a note that was a little long, I wouldn't have been able to tell for any of them what the essential substance would be. The same applies a fortiori to the names (such as "Work and Discovery", etc . . . ) by which I have designated the eight parts I to VIII, into which I have grouped the fifty sections that make up the text.

I'll confine myself to brief comments on the content of these eight parts. The first two, I (Work and Discovery) and II (The Dream and the Dreamer), contain elements of a reflection on mathematical work, and on the work of discovery in general. I'm involved in them in a very personal way.

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<sup>2</sup>(May 28) This is the text of the first part of *Récoltes et Semailles*, "Fatuité et Renouveau". The second part had not been written at the time of writing.

more episodic and much less direct than in the following sections. It is these parts in particular that have the quality of testimony and meditation. Parts III to VI are above all a reflection and testimony on my past as a mathematician "in the mathematical world", between 1948 and 1970. The motivation behind this meditation was above all the desire to understand this past, in an effort to understand and assume a present in which certain aspects are sometimes disappointing or confusing. Parts VII (*L'Enfant s'amuse*) and VIII (*L'aventure solitaire*) are more concerned with the evolution of my relationship with mathematics from 1970 to the present day, i.e. since I left "the world of mathematicians" and never returned. I

examines in particular the motivations, and the forces and circumstances, that led me (to my own surprise) to resume "public" mathematical activity (writing  $\square$  and having *Réflexions Mathématiques* published), p. vii after an interruption of more than thirteen years.

### 4.1.3. 3. Compass and luggage

I'd like to say a few words about the other two texts that, along with *Harvest* and *Sowing*, make up the present volume of the same name.

**Esquisse d'un Programme**" outlines the main themes of mathematical reflection that I have pursued over the last ten years. I intend to develop at least a few of them in the years to come, in a series of informal reflections that I've already had occasion to mention, the "Mathematical Reflections". This outline is the verbatim reproduction of a report I wrote last January in support of my application for a research post at the CNRS. I have included it in the present volume, because obviously this program far exceeds the possibilities of my modest person, even if I were given another hundred years to live and chose to use them to pursue the themes in question as far as I can.

The "**Esquisse thématique**" was written in 1972 to coincide with another application (for a professorship at the Collège de France). It contains a thematic sketch of what I then considered to be my main mathematical contributions. This text reflects the circumstances in which it was written, at a time when my interest in mathematics was marginal, to say the least. So this sketch is little better than a dry, methodical enumeration (though fortunately not intended to be exhaustive. . . ). It doesn't seem to be carried by a vision or the breath of a desire - as if the things I review here as if by conscience (and these were indeed my dispositions) had never been touched by a living vision, nor by a passion to bring them to light when they were still only sensed behind their veils of mist and shadow... .

If I've decided to include this uninspiring report here, I'm afraid, it's mainly to put an end (if that's even possible) to certain high-flying colleagues and a certain fashion, who since my departure from a world we once shared, seem to look down their noses at what they kindly call

grothendieckeries". This, it seems, is synonymous with bombast about things too trivial for a serious mathematician  $\square$  of good taste to consent to wasting admittedly precious time on them. Perhaps this "digest" p. viii will seem more "serious" to them! As for the texts from my pen, which are driven by a vision and a passion, they are not for those whom a fashion maintains and justifies in complacency, making them insensitive to the things that enchant me. If I write for others than for myself, it's for those who don't find their time and their person too precious to pursue without ever tiring of the obvious things that no one deigns to see, and to rejoice in the intimate beauty of each of the things discovered, distinguishing it from any other that was known to us in its own beauty.

If I wanted to situate the three texts that make up this volume in relation to one another, and the

The role of each in this journey on which I've embarked with *Réflexions Mathématiques*, I could say that the reflection-testimony *Récoltes et Semailles* reflects and describes the **spirit** in which I'm undertaking this journey and which gives it meaning. The Program Outline describes my sources of inspiration, which set a **direction**, if not a destination, for this journey into the unknown, rather like a compass, or a strong Ariadne's thread. Finally, the *Esquisse thématique* (Thematic Outline) briefly reviews the **baggage** I acquired as a mathematician before 1970, at least some of which will be useful and welcome in this or that stage of the journey (just as my cohomological and topological algebra reflexes are indispensable to me right now in the *Poursuite des Champs*). And the order in which these three texts follow one another, as well as their respective lengths, reflect well (without any deliberate intention on my part) the importance and weight I attach to them in this journey, the first stage of which is nearing its end.

#### 4.1.4. 4. A journey in pursuit of the obvious

I'd still have to say a few more detailed words about this journey I've been on for just over a year, the *Réflexions Mathématiques*. I explain myself in some detail, in the first eight sections of *Récoltes et Semailles* (i.e. in parts I and II of the *Réflexions*), about the **spirit** in which I'm undertaking this journey, and which, I think, is already apparent in the present first volume, as well as in the one that follows it (the *Histoire de Modèles*, which is volume 1 of the *Poursuite des Champs*), now nearing completion. I don't think it's worth expanding on this in this introduction.

p. IX

□ I certainly can't predict what the journey will be like, something I'll discover as it unfolds. will continue. At present, I don't have an itinerary planned, even in outline, and I doubt that one will emerge any time soon. As I said earlier, the main themes that will undoubtedly inspire my thinking are sketched out more or less in the "Esquisse d'un Programme", the "texte-boussole". Among these themes, there's also the main theme of *La Poursuite des Champs*, i.e. the "fields", which I hope to cover (and leave it at that) again this year, in two or perhaps three volumes. On this theme, I write in the *Esquisse*: ". . . it's a bit like a debt I owe to a scientific past in which, for some fifteen years (between 1955 and 1970), the development of cohomological tools was the constant leitmotiv in my work on the foundations of algebraic geometry". So, of all the planned themes, this is the one most strongly rooted in my scientific "past". It is also the one that has remained as a regret throughout these past fifteen years, as perhaps the most glaring gap of all in the work I left to do when I left the mathematical scene, and which none of my students or friends of yesteryear bothered to fill. For more details on this work-in-progress, the interested reader may refer to the relevant section in *Esquisse d'un Programme*, or to the introduction (the real one this time!) to the first volume, now nearing completion, of *Poursuite des Champs*.

Another legacy of my scientific past that is particularly close to my heart is the notion of **motif**, which is still waiting to emerge from the darkness in which it has been kept, even though it has been around for a good fifteen years. It's not out of the question that I'll end up working on the foundations that need to be laid here, if nobody better placed than me (by virtue of my younger age, as well as the tools and knowledge at my disposal) decides to do so in the next few years.

I'd like to take this opportunity to point out that the fortune (or rather, misfortune. . . ) of the notion of motif, and of a few others among those I've uncovered and which seem to me to be (potentially) the most fruitful of all, are the subject of a retrospective reflection of almost twenty pages, forming the most

long (and one of the very last) "notes" to Récoltes et Semailles<sup>3</sup>. I have subsequently subdivided this note in two parts ("My orphans" and "Refusal of an inheritance - or the price of a contradiction"), in addition to the three p. x

"sub-notes" that follow<sup>4</sup>. Taken together, these five consecutive notes are the only part of Récoltes et Semailles where mathematical notions are evoked in anything other than passing allusions. These notions become the occasion for illustrating certain contradictions within the world of mathematicians, which themselves reflect contradictions within the people themselves. At one point, I considered separating this sprawling note from the text from which it comes, and attaching it to the Thematic Outline. This would have had the advantage of putting the latter into perspective, and breathing a little life into a text that looks a little too much like a catalog. I have refrained from doing so, however, out of a concern to preserve the authenticity of a testimony of which this meganote, whether I like it or not, is indeed a part.

To what is said in Récoltes et Semailles about the dispositions in which I approach the "Réflexions", I would like to add just one thing here, which I have already expressed in one of the notes ("Le snobisme des jeunes - ou les défenseurs de la pureté"), when I write: "My lifelong ambition as a mathematician, or rather my joy and passion, have been constantly to discover the obvious things, and this is my only ambition also in the present work" (A la Poursuite des Champs). This is also my sole ambition for this new journey I've been pursuing for the past year with Réflexions. It was no different in Récoltes et Semailles, which (for my readers at least, if there are any) opens this journey.

#### 4.1.5. 5. A welcome debt

I'd like to conclude this introduction with a few words about the two dedications to the present volume "Récoltes et Semailles".

The dedication "to those who were my students, to whom I gave the best of myself - and also the worst" has been present in me at least since last summer, and in particular when I wrote the first four sections of what was still supposed to be an introduction to a mathematical book. In other words, I'd known for some years that there was a "worst" to consider - and now was the time! (But I had no idea that this "worst" would eventually lead me through a meditation of almost two hundred pages).

On the other hand, the dedication "to those who were my elders" only appeared along the way, as did the very name of this reflection (which also became the name of a volume). It revealed to me the important role p. xi that they played in my life as a mathematician, a role whose effects are still very much alive today. This will no doubt become clear enough in the pages that follow - so there's no need to dwell on it here. These "elders", in (approximate) order of appearance in my life when I was twenty, are Henri Cartan, Claude Chevalley, André Weil, Jean-Pierre Serre, Laurent Schwartz, Jean Dieudonné, Roger Godement and Jean Delsarte. The ignorant newcomer that I was was welcomed with kindness by each of them, and many of them later gave me lasting friendship and affection. I must also mention Jean Leray, whose kind welcome during my first contact with the "world of mathematicians" (in 1948/49) was also a precious encouragement. My reflection has revealed a debt of gratitude to each of these men "from another world and another destiny". This debt is by no means a burden. Its discovery came as a joy, and made me feel lighter.

Late March 1984

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<sup>3</sup>This double note (n°s 46, 47) and its sub-notes have been included in the second part "L'Enterrement" of Récoltes et Semailles, which is a direct continuation of it.

<sup>4</sup>These are sub-notes n°s 48, 49, 50 (note n° 48' was added later).

## 4.2. (II) An act of respect

(- May 4 - . . . June)

### 4.2.1. 6. The Funeral

An unforeseen event rekindled a process of reflection that had already been completed. It inaugurated a cascade of discoveries, large and small, over the course of the past few weeks, gradually revealing a situation that had remained unclear and sharpening its contours. In particular, this led me to enter in detail and in depth into events and situations that had previously only been mentioned in passing or by allusion. As a result, the "fifteen-page retrospective reflection" on the vicissitudes of a work, mentioned earlier (Introduction, 4), has taken on unexpected dimensions, expanding by some two hundred additional pages.

By force of circumstance and by the inner logic of a reflection, I was led along the way to involve others as much as myself. The one who is involved more than anyone else (apart from myself) is a man to whom I have had a friendship for nearly twenty years. I wrote of him (euphemistically<sup>5</sup>) that he had "made  $\square$  un peu figure

p. XII

student", in the very early years of this affectionate friendship rooted in a shared passion, and for a long time in my heart of hearts I saw in him a kind of "legitimate heir" to what I believed I could contribute in mathematics, beyond a fragmentary published body of work. Many of you will already have recognized him: he's **Pierre Deligne**.

I make no apology for making public with these notes, among others, a personal reflection on a personal relationship, and thus implicating him without having consulted him. I think it's important, and healthy for everyone, that a situation that has long remained hidden and confused should finally be brought out into the open and examined. In so doing, I am providing a subjective account, which does not claim to exhaust a delicate and complex situation, nor to be free from error. Its first merit (like that of my past publications, or those on which I am currently working) is that it exists, available to those who may find it of interest. My concern has been neither to convince, nor to shield myself from error or doubt behind the only so-called "patent" things. My concern is to be true, by saying things as I see or feel them, in each moment - as a means to deepen them and to understand.

The name "**L' Enterrement**", for all the notes relating to the "Weight of a past", came to the fore with increasing force in the course of reflection<sup>6</sup>. In it, I play the role of the anticipated deceased, in the mournful company of the few (much younger) mathematicians whose work takes place after my "departure" in 1970 and bears the mark of my influence, through a certain style and approach to mathematics. Foremost among these is my friend **Zoghman Mebkhout**, who had the heavy privilege of having to face all the handicaps of being treated as Grothendieck's "pupil after 1970", without having had the benefit of contact with me and my encouragement and advice, whereas he was only a "pupil" of my work through my writings. This was at a time when (in the world he haunts) I was already such a "defunct" figure that for a long time the very idea of a meeting apparently did not present itself, and an ongoing relationship (both personal and mathematical) was only finally established last year.

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<sup>5</sup>On the meaning of this "euphemism"; see the note "Being apart", n° 57'.

<sup>6</sup>Towards the end of this reflection, another name came to the fore, expressing another striking aspect of a certain painting that had come to the fore.

gradually revealed to my eyes over the past five weeks. It's the name of a tale, to which I'll return in its place: "The robe of the Emperor of China". . .

<sup>7</sup>Towards the end of this reflection, another name came to the fore, expressing another striking aspect of a certain painting that had come to the fore.



□ This didn't stop Mebkhout, against a tyrannical fashion and the disdain of his elders (quip were my students) and in near-complete isolation, from making an original and profound work, through an unforeseen synthesis of the ideas of the Sato school and my own. This work provides a new insight into the cohomology of analytic and algebraic varieties, and holds the promise of a far-reaching renewal in our understanding of this cohomology. There can be no doubt that this renewal would have been accomplished now and years ago, if Mebkhout had found the warm welcome and unreserved support he once received from me. At least, since October 1980, his ideas and work have provided the inspiration and technical means for a spectacular revival of the cohomological theory of algebraic varieties, finally emerging (apart from Deligne's results on Weil's conjectures) from a long period of stagnation.

Incredibly, his ideas and results have been used by "everyone" for almost four years now (just like mine), while his name remains studiously ignored and hushed by the very people who know his work first-hand and use it as an essential part of their work. I don't know of any other time when mathematics has been so disgraced, when some of the most influential or prestigious of its followers set an example, in the midst of general indifference, of disregard for the most universally accepted rule in the ethics of the mathematical profession.

I see four men, mathematicians of brilliant means, who had and have the right with me to the honors of this burial through silence and disdain. And I see in each of them the bite of contempt on the beautiful passion that had animated him.

Apart from these, I see two men in particular, both in the limelight in the mathematical public arena, who officiate at funerals in large company and who at the same time (in a more hidden sense) are buried with their own hands, along with those they deliberately bury. I've already named one of them. The other is also a former student and friend, **Jean-Louis**.

**Verdier**. After my "departure" in 1970, contact between him and me was not maintained, apart from a few hasty meetings on a professional level. This is probably why he □ only appears in this reflection atp . XIV through certain acts in his professional life, while the possible motivations for these acts, in terms of his relationship with me, are not examined and, moreover, escape me entirely.

If there's one pressing question that has imposed itself on me over the last few years, which was a profound motivation for *Récoltes et Semailles* and which has also followed me throughout this reflection, it's that of my part in the advent of a certain spirit and certain mores that make possible disgraces such as the one I've mentioned, in a world that was once mine and with which I had identified myself for more than twenty years of my life as a mathematician. Reflection has led me to discover that certain attitudes of fatuity within me, expressed in a tacit disdain for colleagues of modest means, and in complacency towards myself and certain mathematicians with brilliant means, were no strangers to this spirit that I see spreading today among the very people I had loved, and among those too whom I taught a profession I loved; those whom I disliked and taught badly, and who today set the tone (when they don't make the law) in this world that was dear to me and which I have left.

I feel a wind of smugness, cynicism and contempt. "It blows without concern for "merit" or "demerit", burning with its breath humble vocations as well as the most beautiful passions... .". I've realized that this wind is the prolific harvest of careless, blind sowing that I've helped to sow. And if its breath comes back on me and on what I had entrusted to other hands, and on those I love today and in the future, I'm sure that it will be a great help.

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gradually revealed to my eyes over the past five weeks. It's the name of a tale, to which I'll return in its place: "The robe of the Emperor of China". . .

who have dared to claim me as their own, or even to draw inspiration from me, I have no reason to complain, and much to learn from it.

### 4.2.2. 7. Scheduling Funerals

Under the name "L'Enterrement" (Burial), I have therefore grouped together in the table of contents the imposing procession of the main "notes" relating to this seemingly innocuous section "Le poids d'un passé" (s.50), thus giving full meaning to the name that had immediately imposed itself on me for this ultimate section of the "first draft" of Récoltes et Semailles.

In this long procession of related notes<sup>8</sup>, those that have joined over the past four<sup>9</sup> weeks elapsed (notes (51) to (97)) stand out as the only dated ones (from April 19 to May 24). It p. XV I thought it most natural to list them in the chronological order in which they follow one another in the<sup>10</sup> reflection, rather than in any other "logical" order; or in the order in which references to these notes appear in earlier notes. In order to trace this latter (by no means linear) order of filiation between participating notes, I have followed (in the table of contents) the number of each note by that of the note (among those preceding it) where it is first referred to<sup>11</sup>, or (failing that) by the number of the note of which it constitutes an immediate continuation<sup>12</sup>. (This latter relationship is indicated in the text itself by a reference abbreviation placed at the end of the first note, such as (⇒ 47) placed at the end of the last line of the note (46), which refers to the note (47) that continues it). Lastly, certain technical details are grouped together at the end of a note in sub-notes numbered by consecutive indices in the number of the original note - as in sub-notes (46<sub>1</sub>) to (46<sub>9</sub>) of note (46) "My orphans". p. XVI

□ To give some structure to the overall order of the Burial, and to enable re-

In order to make the most of the multitude of notes that crowd together, I thought it would be appropriate to include a few highly suggestive subtitles in the procession, each preceding and leading a long or short procession of consecutive notes linked by a common theme.

I had the pleasure of seeing ten<sup>13</sup> processions gather one by one, in a long, solemn procession to honor my funeral - some humble, others imposing, some contrite and others secretly jubilant, as cannot be otherwise on such an occasion. Here come the **posthumous pupil** (whom everyone makes a point of ignoring), the **orphans** (freshly exhumed for the occasion), **Fashion** and its **Illustrious Men** (I've earned that), the **motifs** (the last born and last exhumed of all my

<sup>8</sup>To these should be added note no.° 104, dated May 12, 1984. Notes n° 98 and following (with the exception of the previous note n° 104) constitute the "third breath" of reflection, from September 22 1984 onwards. They are also dated.

<sup>9</sup>In a series of consecutive notes written on the same day, only the first is dated. The other undated notes are n° s 44' to 50 (forming cortèges I, II, III). Notes n° s 46, 47, 50 are from March 30 or 31, notes n° s 44', 48, 48', 49 from the first half of April, and note n° 44" is dated (May 10).

<sup>10</sup>I've sometimes made a small inversion in this chronological order, in favor of a "logical" order, when it's been I felt that this did not distort the overall impression of the thought process. As the only exceptions, however, I would like to point out eleven notes (preceded by the number !) taken from b. de p. notes subsequent to a note and which have grown to prohibitive dimensions, and which I have placed each following the note to which it refers (except note n° 98, see see° 47).

<sup>11</sup>When the reference to a note (such as (45)) is found in the "Weight of a past" section itself; it's number (50) of which is **placed in brackets** after the note, as in 46 (50).

<sup>12</sup>The number of a note that is an immediate continuation of a previous note (which numbers then follow each other) is preceded by the sign \* in the table of contents. Thus \*47, 46 indicates that note n° 47 is an immediate continuation of note n° 46 (which in this case is not the note that immediately precedes it, which is note n° 46 ).<sup>9</sup>

I have **underlined** in the t. des m. the numbers of the notes which are not followed by another number, i.e. those which are not

represent a "new departure" in our thinking, and do not fit into any particular part of our existing thinking.

<sup>13</sup>(September 29) In fact, there are finally twelve processions, including the Funeral Van (x), and "The deceased (still not deceased)." (XI), who has just managed to sneak back into the procession...

orphans), **my friend Pierre** modestly leading the largest of the processions, closely followed by **the Accord Unanime** des notes (silencieusement) concertantes and by the **Colloque** (known as the "Pervers") in its entirety (distinguishing itself from the posthumous pupil, alias the Unknown Pupil, by interposed funeral processions bearing flowers and wreaths) ; finally, to bring the imposing procession to a fitting close, here comes **the Pupil** (by no means posthumous and even less unknown) aka the **Boss**, followed by the bustling troop of my **pupils** (armed with shovels and ropes) and finally the **Funeral Van** (bearing four beautiful oak coffins firmly screwed together, not to mention the Gravedigger). . . ten processions at last in full force (it's about time), slowly making their way to the **Funeral Ceremony**.

The highlight of the Ceremony is the Eulogy, delivered with perfect skill by none other than my friend Pierre himself, presiding over the funeral in response to everyone's wishes and general satisfaction. The Ceremony concludes with a final and definitive De Profundis (or so we hope), sung as a heartfelt thanksgiving by the deceased himself, who unbeknownst to all has survived his impressive funeral and leads from it, to his **complete satisfaction** - which satisfaction forms the final note and final chord of the memorable Funeral.

### 4.2.3. 8. The end of a secret

In the course of this (hopefully) final stage of reflection, it occurred to me that it would be worthwhile to add the following in an "Appendix".

to the present volume 1 of *Réflexions Mathématiques* □ two other texts, of a mathematical nature, in addition to p. xvii three previously mentioned<sup>14</sup>.

The first is the reproduction of an annotated two-part **report** I wrote in 1968 and 1969 on P. Deligne's work (some of which remains unpublished to this day), corresponding to mathematical activity at the IHES during the three years 1965/67/68.

The other text is a sketch of a "**six-variance form**", bringing together the features common to a duality formalism (inspired by Poincaré's and Serre's duality) that I had worked out between 1956 and 1963, a form that proved to be "universal" for all cohomological duality situations encountered to date. This formalism seems to have fallen into disuse with my departure from the mathematical scene, to the extent that to my knowledge no one (apart from myself) has yet taken the trouble to write down even the list of fundamental operations, the fundamental canonical isomorphisms to which these operations are linked and the fundamental isomorphisms to which they are linked. and the essential compatibilities between them.

□ This sketch of a coherent form will be for me the first obvious step towards this "vast array of en- p. XVIII  
of the **dream of motives**", which for more than fifteen years "has been waiting for the bold mathematician who will

<sup>14</sup>In addition, I'm thinking of adding a "commentary" to the *Esquisse Thématique* (see "Boussole et bagages", Introduction, 3), giving some details about my contributions to the "themes" that are summarily reviewed there, and about the influences that played a part in the genesis of the main ideas-force in my mathematical work. The retrospective of the last six weeks has already revealed (to my own surprise) Serre's role as "detonator" for the start of most of these ideas, as well as for some of the "big tasks" I set myself between 1955 and 1970.

As a further text of a mathematical nature (in the usual sense), and the only one to appear (incidentally) in the non-technical text "Harvest and Sowing", I refer to sub-note no.° 87 to the note "The Massacre" (no.° 87), where I explain with the care it deserves a "discrete" (conjectural) variant of the Riemann-Roch-Grothendieck theorem familiar in the context.

coherent. This conjecture appeared (among many others) in the closing lecture of the SGA 5 seminar in 1965/66, a lecture of which no trace remains (any more than many others) in the volume published eleven years later under the name SGA 5. The vicissitudes of this crucial seminar in the hands of some of my students, and their links with a certain "operation

SGA 4<sub>2</sub>", are gradually revealed in the course of the reflections in notes n° s 63''', 67, 67', 68, 68', 84, 85, 85', 86, 87, 88.

As a further note giving fairly extensive mathematical comments on the desirability of a common "topossical" framework (as far as possible) for known cases where a duality formalism known as "six operations" is available, I would also point out sub-note no.° 81<sub>2</sub> to the note "Credit thesis and comprehensive insurance", no.° 81.

brush". To all appearances, this mathematician will be none other than myself. It's high time that what was born and entrusted intimately almost twenty years ago, not to remain the privilege of one but to be available to **all**, finally emerges from the night of secrecy, and is born once again into the full light of day.

It's true that only one of them, apart from me, had an intimate knowledge of this "yoga of patterns", having learned it from me in the days and years leading up to my departure. Of all the mathematical things I'd had the privilege of discovering and bringing to light, this reality of patterns still appears to me as the most fascinating, the most charged with mystery - at the very heart of the profound identity between "geometry" and "arithmetic". And the "yoga of patterns" to which this long-ignored reality has led me is perhaps the most powerful instrument of discovery I have unleashed in this first period of my mathematical life.

But it's also true that this reality, and the "yoga" that strives to capture it as closely as possible, had by no means been kept secret from me. Absorbed by the imperative task of writing the basics (which everyone has since been happy to use as they are in their everyday work), I didn't take the few months necessary to write a vast outline of this yoga of motives, and thus make it available to everyone. Nevertheless, in the years leading up to my sudden departure, I spoke about it at random and to anyone who would listen, starting with my students, who (apart from one) have forgotten it, as they all have. My reason for mentioning it was not to put forward "inventions" that would bear my name, but to draw attention to a reality that manifests itself at every step, as soon as one becomes interested in the cohomology of algebraic varieties and, in particular, in their "arithmetic" properties and the relationships between them of the various cohomological theories known to date. This reality is just as tangible

as that of the "infinitely small" was, perceived long before the appearance of the rigorous language that allowed us to apprehend it perfectly and "establish" it. And to apprehend the reality of  $\square$  motifs, we

p. XIX

Today, we are by no means short of a flexible and adequate language, nor of consummate experience in the construction of mathematical theories, which our predecessors lacked.

If what I once shouted from the rooftops has fallen on deaf ears, and if the disdainful silence of one has echoed the silence and lethargy of all those who pretend to be interested in cohomology (and yet have eyes and hands just like me. . . ), I can't hold the one person responsible who chose to keep the "benefit" of what I had entrusted to him for the benefit of all. It has to be said that our age, whose unbridled scientific productivity rivals that invested in armaments or consumer goods, is a long way from the "bold dynamism" of our seventeenth-century predecessors, who "didn't beat about the bush" to develop a calculation of the infinitely small, without worrying whether it was "conjectural" or not ; nor did they wait for some prestigious man among them to deign to give them the green light, before grabbing hold of what everyone could see with their own eyes and feel at first hand.

#### 4.2.4. 9. The stage and Actors

By virtue of its own internal structure and particular theme, "L'Enterrement" (which now forms over half the text of Récoltes et Semailles) is to a large extent and logically independent of the long reflection that precedes it. But this independence is superficial. For me, this reflection on a "funeral" gradually emerging from the mists of the unspoken and the prescient, is inseparable from the one that preceded it, from which it springs and which gives it its full meaning. Begun as a quick glance "en passant" at the vicissitudes of a work I'd somewhat (very much) lost sight of, it's a work I'd never seen before.

As I saw it, without planning or seeking it, it became a meditation on an important relationship in my life, leading me in turn to reflect on the fate of this work at the hands of "those who were my students". To separate this reflection from the one from which it spontaneously sprang seems to me a way of reducing it to a mere "tableau de mœurs" (or even, a settling of scores in the mathematical "beau monde").

It's true that, if we insist on it, the same reduction to a "tableau de mœurs" can be made for *Récoltes et Semailles* as a whole. Certainly, the mores that □ prevalent in a given time and milieu and that contribute to shape the lives of the men who are part of it, have their importance and deserve to be described. However, it will be clear to an attentive reader of *Harvest and Sowing* that my aim is not to describe mores, i.e. a certain **scene**, changing over time and from place to place, on which our actions take place. To a large extent, this stage defines and delimits the **means** available to the various forces within us, enabling them to express themselves. While the stage and the means it provides (and the "rules of the game" it imposes) vary ad infinitum, the nature of the forces deep within us that (at the collective level) shape the stages and that (at the level of the individual) express themselves on them, seems to be the same from one milieu or culture to another, and from one era to another. If there's one thing in my life, apart from mathematics and the love of a woman, whose mystery and attraction I've sensed (admittedly late in life), it's the hidden nature of those few forces that have the power to make us act, for "better" or "worse", to bury and to create.

. XX

#### 4.2.5. 10. An act of respect

This reflection, which eventually took on the name "L'Enterrement", had begun as an **act of respect**. A respect for things that I had discovered, that I had seen condense and take shape in a void, whose taste and vigor I was the first to know, and to which I gave a name, to express both the knowledge I had of them, and my respect. To these things, I gave the best of myself. They have fed on the strength that lies within me, they have grown and flourished, like multiple, vigorous branches sprouting from a single living trunk with multiple, vigorous roots. These are living and present things, not inventions that can be made or not made - things closely interwoven in a living unity that is made of each of them and gives each its place and meaning, an origin and an end. I had left them a long time ago and without any worry or regret, because I knew that what I was leaving behind was healthy and strong and didn't need me to grow and flourish and multiply, according to its own nature. It wasn't a bag of gold I was leaving behind that could be stolen, nor a pile of tools that could rust or rot.

And yet, as the years went by and I thought I was far away from a world I'd left behind, I found myself back here. and there even in my retreat like □ bouts of insidious disdain and discreet derision, pointing such things I knew to be strong and beautiful, to have a unique place and function that nothing else could ever fulfill. I felt they were orphans in a hostile world, a world sick with the disease of contempt, lashing out at the unarmored. It was with this in mind that I began this reflection, as an act of respect for these things, and thus for myself.

p. XXI

- as a reminder of the deep connection between these things and me: whoever delights in showing disdain for any of these things that have been nurtured by my love, it is **I** whom he delights in scorning, and all that comes from me.

And the same applies to the person who, knowing at first hand the link that connects me to such and such a thing that he has learned from none other than me, pretends to consider this link negligible or to ignore it, or to claim (albeit tacitly and by omission) for himself or for others a fictitious "paternity". I see

clearly an act of contempt for a thing born of the worker, as well as for the obscure and delicate work that brought it into being, **and** for the worker, and above all (in a more hidden and essential way) for himself.

If my "return to maths" was only to serve as a reminder of this link and to arouse in me this act of respect in front of all - in front of those who affect to disdain and in front of indifferent witnesses - this return will not have been in vain.

It's true that I'd really lost touch with the written and unwritten (or at least unpublished) work I'd left behind. As I began this reflection - I could see the branches quite clearly, but without really remembering that they were all part of the same tree. Strangely enough, it took the picture of the **ransacking of** what I'd left behind to gradually reveal itself to me, for me to regain a sense of the living unity of what was thus ransacked and dispersed. One took away a few écus, the other a tool or two to use - but the unity that makes up the life and true strength of what I had left eluded each and every one of them. I do know one person, however, who deeply felt this unity and strength, and who at the

It is in this living unity that we find beauty and beauty, and in this living unity, that we find ourselves. It is in this living unity that beauty and

the creative virtue of the work. Despite the destruction, I find them as intact as if I'd just left them - except that I've matured and now see them with new eyes.

If anything, however, is ransacked and mutilated, and disarmed of its original power, it is in those who forget the power that lies within themselves and imagine they are ransacking something at their mercy, when they are only cutting themselves off from the creative virtue of that which is at their disposal as it is at the disposal of all, but by no means at their mercy or in anyone's power.

So this reflection, and through it this unexpected "return", will also have put me back in touch with a forgotten beauty. It's having fully felt this beauty that gives full meaning to the act of respect awkwardly expressed in the note "My orphans"<sup>15</sup>, and which I have just reiterated here in full knowledge of the facts.

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<sup>15</sup>This note (n° 46) is chronologically the first of all those that appear in the Burial.

## **Part 1.**

# **FATUITE AND RENEWAL**





To those who were my elders  
who welcomed me fraternally into this  
world of theirs  
and became mine

To those who were my  
students, to whom I gave the  
best of myself  
and also the worst...



# 5. Work and discovery

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June 1983

## 5.1. (1) the child and the good God

□The mathematical notes on which I am now working are the first in thirteen years that I intended for publication. The reader will not be surprised that, after a long silence, my style of expression has changed. This change of expression is not, however, the sign of a change in style or working method<sup>1</sup> (1), still less that of a transformation that has taken place in the very nature of my work. of my mathematical work. Not only did it remain the same - I became convinced that the nature of the work of discovery is the same from one discoverer to the next, that it is beyond the differences created by infinitely varying conditioning and temperament.

Discovery is a child's privilege. It's the little child I'm talking about, the child who isn't yet afraid of making mistakes, of looking stupid, of not being serious, of not being like everyone else. Nor is he afraid that the things he looks at have the bad taste of being different from what he expects them to be, from what they should be, or rather: from what it is well understood that they **are**. He ignores the mute, flawless consensus that is part of the air we breathe - the air of all right-thinking people, well known as such. God knows, there have been many sensible people, well known as such, since the dawn of time!

Our minds are saturated with a heterogeneous "knowledge", a tangle of fears and laziness, cravings and prohibitions; of information at every turn and push-button explanations - a closed space where information, cravings and fears pile up without ever letting in the wind from the open sea. With the exception of routine know-how, it would seem that the main role of this "knowledge" is to evacuate a living perception, an awareness of the things of this world. Its effect is above all one of immense inertia, of an often crushing weight.

<sup>1</sup>(1)  
(Added in March 1984) It's probably an overstatement to say that my "style" and "method" of working have not changed, whereas my style of expressing myself in mathematics has been profoundly transformed. Most of the time devoted over the last year to "La Poursuite des Champs" has been spent on my typewriter, typing out reflections that are destined to be published virtually as they are (apart from the addition of relatively short notes added later to facilitate reading by cross-referencing, error correction, etc. . ). No scissors or glue to painstakingly prepare a "definitive" manuscript (which, above all, must reveal nothing of the process that led to it) - that's a lot of changes in "style" and "method"! Unless you dissociate the mathematical work itself from the writing and presentation of results, which is artificial, because it doesn't correspond to the reality of things, since mathematical work is indissolubly linked to writing.

The little child discovers the world as it breathes - the ebb and flow of its breath makes it welcome the world into its delicate being, and projects itself into the world that welcomes it. Adults also discover, in those rare moments when they have forgotten their fears and their knowledge, when they look at things or themselves with eyes wide open, eager to know, new eyes - the eyes of a child.

\*       \*

\*

p. 2

□ God created the world as he discovered it, or rather he **creates** the world eternally, as he discovers it - and he discovers it as he creates it. He created the world and creates it day after day, repeating himself millions and millions of times, without respite, groping his way, making millions and millions of mistakes and then rectifying his aim, without tiring. ... Each time, in this game of probing into things, responding to things ("that's not bad", or: "you're really messing up", or "it's going like clockwork, keep it up"), and the new probe rectifying or taking up the previous probe, in response to the previous response... . . . every time we go back and forth in this infinite dialogue between Creator and Things, which takes place at every moment and in every place of Creation, God learns, discovers and becomes more intimately acquainted with things, as they take on life and form and are transformed in His hands.

Such is the process of discovery and creation, such has it been from all eternity it seems (as far as we can tell). It has been like this, without man having to make his late entrance on the scene, barely a million or two years ago, and get his hands dirty - with, of late, the unfortunate consequences we know about.

Occasionally, one or other of us discovers one thing or another. Sometimes we rediscover in our own lives, with wonder, what it is to **discover**. Each of us has everything it takes to discover whatever attracts us in this vast world, including this marvellous capacity within us - the simplest, most obvious thing in the world! (A thing, however, that many have forgotten, just as we have forgotten to sing, or to breathe as a child breathes... .)

Everyone can rediscover what it is to discover and create, and no one can invent it. They were there before us, and they are what they are.

## 5.2. (2) Error and discovery

To return to the style of my mathematical work itself, or its "nature" or "approach", they are now as before those that the good Lord himself taught us all wordlessly, God knows when, long before we were born perhaps. **I do as he did**. It's also what everyone does instinctively, as soon as curiosity pushes them to know such and such a thing of all things, a thing invested from that moment on in the knowledge of all things. by this desire, this thirst...

p. 3

□ When I'm curious about something, mathematical or otherwise, I **question it**. I question it, without worrying about

if my question is perhaps stupid, or if it will appear so, without being carefully weighed. Often the question takes the form of an assertion - an assertion which, in truth, is a sounding board. I believe my assertion more or less, depending of course on where I am in my understanding of the things I'm looking at. Often, especially in the early stages of a research project, the assertion is downright false - but you had to make it to be convinced. Often, all I had to do was write it down.

It's obvious that it's not true, whereas before writing it there was a blur, like a malaise, instead of this obviousness. Now it's possible to come back to the charge with less ignorance, with a question-assertion that's perhaps a little less "off the mark". Even more often, the assertion taken literally turns out to be wrong, but the intuition that, clumsily still, tried to express itself through it is right, though still vague. Little by little, this intuition will be decanted from an equally shapeless gangue of false or inadequate ideas, it will gradually emerge from the limbo of the misunderstood that only asks to be understood, of the unknown that only asks to be known, to take on a form of its own, refining and sharpening its contours, as the questions I ask these things in front of me become more precise or more pertinent, to define them more and more closely.

But it can also happen that, through this process, repeated probing converges on a certain image of the situation, emerging from the mists with sufficiently marked features to give rise to the beginnings of a conviction that this image does indeed express reality - when, however, it does not, when this image is tainted by a major error, likely to distort it profoundly. The work, sometimes laborious, which leads to the detection of such a misconception - starting with the first "take-offs" observed between the image obtained and certain obvious facts, or between this image and others which we also trusted - is often marked by a growing tension, as we approach the crux of the contradiction, which at first becomes more and more vague - until finally it explodes, with the discovery of the error.

and the collapse of a certain vision of things, coming as an immense relief, as a liberation. **The discovery of error is one of the most crucial moments** <sup>□</sup> **ciaux, a creative moment of all, in p** . 4  
**all work of discovery**, whether mathematical or self-discovery. It's a moment when our knowledge of the thing being probed is suddenly renewed.

**Fear of error and fear of truth are one and the same thing.** He who fears error is powerless to discover. It's when we fear being wrong that the error within us becomes immovable as a rock. For in our fear, we cling to what we once thought "true", or to what has always been presented to us as true. When we are moved, not by the fear of seeing an illusory security vanish, but by a thirst for knowledge, then error, like suffering or sadness, passes through us without ever becoming fixed, and the trace of its passage is renewed knowledge.

### 5.3. (3) The unspeakable labours

It's surely no coincidence that the spontaneous process of true research never appears in the texts or discourse that are supposed to communicate and transmit the substance of what has been "found". More often than not, texts and speeches confine themselves to recording "**results**", in a form that must appear to ordinary mortals as so many austere and immutable laws, inscribed from all eternity on the granite tables of some sort of giant library, and dictated by some omniscient God to the initiated-scribes-savants and assimilated; to those who write learned books and no less learned articles, those who transmit knowledge from the pulpit, or in the more restricted circle of a seminar. Is there a single textbook, a single manual for schoolchildren, high-school students, university students, or even "our researchers", that can give the unfortunate reader the slightest idea of what research is all about - apart from the universally accepted idea that research is something you do when you're really good at it, when you've passed lots of exams and even competitions, the big shots, Pasteur and Curie and the Nobel Prize winners and all that...? The rest of us, readers or listeners, are just doing our best to swallow up the knowledge that these great men have so willingly consigned to the good of humanity, and we're just good enough (if we work hard) to pass our exams at the end of the year.

again...

p. 5 How many there are, including among the unfortunate "researchers" themselves, in need of theses or articles, including even among the most "learned" □ the most prestigious among us - who then has the simplicity to see that "to seek" is no more and no less than to **question** things, passionately - like a child who **wants to know** how he or his little sister came into the world. That seeking and finding, that is, questioning and listening, is the simplest, most spontaneous thing in the world, and one that no one in the world has the privilege of doing. It's a "gift" we've all been given from the cradle - made to express itself and blossom under an infinite number of faces, from one moment to the next and from one person to the next... .

When you dare to say such things, you'll get the same half-sad, half-understanding smiles from all sides, from the dunce who's sure he's a dunce, to the scholar who's sure he's a scholar and well above the common man, as if you've just made a joke that's a bit too big for words, as if you're displaying a naivety that's been stitched together with white thread; It's all very well to spit on no-one, of course - but don't push it - a dunce is a dunce, and not Einstein or Picasso!

In the face of such unanimous agreement, I'd be remiss if I didn't insist. Incurable as I am, I've lost yet another opportunity to keep quiet...

No, it's surely no coincidence that, in perfect harmony, instructive or edifying books and manuals of all kinds present "Knowledge" as if it had emerged fully dressed from the brilliant minds that recorded it for our benefit. Nor can this be said to be bad faith, even in those rare cases where the author is "in the know" enough to know that this image (which his text cannot fail to suggest) in no way corresponds to reality. In such cases, the presentation may be more than a collection of results and recipes, it may be infused with a breath of fresh air, animated by a living vision that is sometimes communicated from the author to the attentive reader. But an unspoken consensus, of considerable force it seems, ensures that the text leaves no trace of the **work of** which it is the product, even when it expresses with lapidary force the sometimes profound vision of things that is one of the true fruits of this work.

p. 6 To tell the truth, there have been times when I myself have felt the weight of this force, of this silent consensus, on the occasion of my project to write and publish these "Mathematical Reflections". If I try to fathom the tacit form that this consensus takes, or rather the one taken by the resistance within me to my □ project, triggered

by this consensus, the term "indecent" immediately comes to mind. The consensus, internalized in me I can't say for how long, tells me (and this is the first time I've taken the trouble to draw into the light of day, into the field of my gaze, what it's been mumbling to me with some insistence for weeks, if not months): "It's indecent to flaunt before others, even publicly, the ups and downs, the messy gropings around the edges, the "dirty laundry" in short, of a work of discovery. It's a waste of the reader's precious time. What's more, it's going to add up to pages and pages and pages of typesetting and printing - what a waste, at the price of scientific printed paper! You've got to be really vain to flaunt things that are of no interest to anyone, as if my own screw-ups were something remarkable - an opportunity to strut your stuff, in short". And even more secretly: "It's indecent to publish the notes of such a reflection, as it **really** goes on, just as it would be indecent to make love in a public square, or to expose, or just leave lying around, the blood-stained sheets of the labors of childbirth.... ".

The taboo here takes the insidious yet imperious form of the sexual taboo. It is as I write this introduction that I begin to glimpse its extraordinary force, and the scope of this extraordinary fact itself, attesting to this force: that the true process of discovery, so simple in its simplicity, is to be found in the very fact of the taboo.

This is the case even in the relatively innocuous field of scientific discovery, not that of his willy or anything like that, thank God. This is the case even in the relatively innocuous field of scientific discovery, not that of his willy or anything like that, thank God - a "discovery" in short, fit to be placed in everyone's hands, and which (one might think) has nothing to hide... ..

If I wanted to follow the "thread" that's there - a thread that's by no means tenuous, but very thick and strong - surely it would take me much further than the few hundred pages of homologic-homotopic algebra that I'll eventually finish and deliver to the printer.

## 5.4. (4) Infallibility (of others) and contempt (of oneself)

Decidedly this was an understatement, when at times I cautiously observed that "my style of expression" had changed, even suggesting that there <sup>□</sup>was nothing there to be surprised about: you understand well, p. 7  
when you haven't written for thirteen years, it's not the same as before, your "style of expression" has to change, of course. ... The difference is that I used to "express myself" (sic) like everyone else: I'd do the work, then do it again in reverse, carefully erasing all the erasures. Along the way, I'd make new erasures, disrupting the work and sometimes making it worse than the first draft. So it had to be done again - sometimes three or even four times, until everything was perfect. Not only were there no dubious corners or sweepings surreptitiously pushed under a suitable piece of furniture (I've never liked sweepings in corners, as long as you take the trouble to sweep); but above all, when reading the final text, the admittedly flattering impression that emerged (as with any other scientific text) was that **the author** (my modest self in this case) **was infallibility incarnate**. Infallibly, he would come across "the" right notions, then "the" right statements, with a well-oiled engine purring along, with demonstrations that "fell" with a dull noise, each at exactly the right moment!

Let's judge the effect on an unsuspecting reader, a high-school pupil learning Pythagoras' theorem or equations of the second degree, or even one of my colleagues in research institutions or so-called "higher education" (hello!) shouting (let's say) about reading such and such an article by such and such a prestigious colleague! As this kind of experience is repeated hundreds and thousands of times over the course of a pupil's, or even a student's or researcher's, life, amplified by the appropriate concert in the family and in the media of every country in the world, the effect is what you'd expect. You can see it in yourself as well as in others, if you take the trouble to pay attention: **it's the intimate conviction of your own nullity**, in contrast to the

competence and importance of people "who know" and people "who do". This intimate conviction is sometimes compensated for, but by no means resolved or defused, by the development of an ability to memorize things that are misunderstood, or even by the development of a certain operative skill: multiplier matrices, "editing" a French composition with "thesis" and "antithesis"... . It's the ability of the parrot or the learned monkey, more popular today than ever before, sanctioned by

coveted diplomas, rewarded by comfortable careers' □ But the very one sewn with diplomas p. 8  
and well-connected, covered in honors perhaps, is not fooled, deep down, by these factitious signs of importance, of "value". Nor, more rarely, is the person who has invested his or her all in the development of some genuine gift, and who in his or her professional life has been able to give his or her all and be creative - he or she is not convinced, deep down, by the brilliance of his or her notoriety, by which he or she often wants to give the impression to himself or herself and to others. The same unexamined doubt inhabits both of them, just as it does the first dunce who comes along, the same conviction they may never dare to acknowledge.

It's this doubt, this deep, unspoken conviction, that drives both of them to constantly surpass themselves.

in the accumulation of honors or works, and to project onto others (above all those over whom they have some power. . . ) the self-contempt that gnaws at them in secret - in an impossible attempt to escape it, by accumulating "proofs" of their superiority over others<sup>2</sup> (2).

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<sup>2</sup>(2)

(Added in March 1984) When I reread these last two paragraphs, I had a certain feeling of unease, due to the fact that in reading them

In writing, I involve others, not myself. Obviously, the thought that my own person might be involved didn't occur to me when I was writing. I certainly didn't learn anything when I confined myself to putting down in black and white (no doubt with a certain satisfaction) things that for years I have perceived in others, and seen confirmed in many ways. As I continue my reflection, I'm led to remember that there has been no shortage of contemptuous attitudes towards others in my life. It would be strange if the link I've grasped between contempt for others and contempt for oneself were absent in the case of myself; sound reason (and also the experience of similar situations of blindness towards myself, which I've come to realize) tell me that this must surely not be the case! For the time being, however, this is no more than a deduction, the only possible use of which would be to encourage me to see with my own eyes what's going on, and to see and examine (if it does indeed exist, or has existed) this as yet hypothetical contempt for myself, so deeply buried that it has totally escaped my gaze until now. It's true that there's been no shortage of things to look at! This one suddenly strikes me as one of the most crucial, because precisely because it is so hidden... . [(August 1984) *On this subject, however, see the last two paragraphs of the note "The Massacre", n° 87.*].



# 6. Dreams and the Dreamer

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February 1984

## 6.1. (5) The forbidden dream

I'm taking the opportunity of a three-month break in the writing of *La Poursuite des Champs*, to take the Introduction back to where I left it last June. I've just reread it carefully, more than six months later, and added a few subtitles.

When I wrote this Introduction, I was well aware that such reflections would inevitably give rise to many "misunderstandings" - and it would be pointless to try to anticipate them, as that would simply mean piling more on top of the first! The only thing I would add in this connection is that I have no intention of going to war against the scientific writing style that has been established by millennia of usage, and which I myself have practised assiduously for over twenty years of my life, and taught to my students as an essential part of the mathematician's craft. Rightly or wrongly, I still regard it as such and continue to teach it. I'd probably even be old-fashioned, with my insistence on a job done right to the end, hand-sewn from start to finish, and with no mercy shown to anyone.

dark corner. If I've had to put water in my wine over the last ten years or so, it's by necessity! For me, "writing in form" remains an important stage in mathematical work, both as an instrument of discovery, to test and deepen an understanding of things that would otherwise remain approximate and fragmentary, and as a means of communicating such an understanding. From a didactic point of view, the deductive mode of exposition, which in no way excludes the possibility of painting vast pictures, offers obvious advantages in terms of conciseness and ease of reference. These are real advantages, and significant ones at that, when it comes to presentations aimed at the general public.

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mathematicians, let's say, and more specifically, to mathematicians who are already sufficiently familiar with some of the ins and outs of the subject at hand, or others close by.

These advantages, on the other hand, become entirely illusory when the presentation is aimed at children, young people or adults who are not at all "in the loop" beforehand, whose interest is not yet aroused, and who, moreover, are more often than not (and will remain, and for good reason. . . ) in total ignorance of what the real process of discovery is. Readers, to put it better, who are unaware **of the** very **existence of** such work, which is within **the reach of anyone** gifted with curiosity and common sense - the work from which our intellectual knowledge of the Universe is constantly born and reborn, including that expressed in such imposing ordinances as Euclid's "Elements", or Darwin's "Origin of Species". Complete ignorance of the existence and nature of such work is almost universal, including among teachers at all levels of education, from schoolteacher to university professor. It's an extraordinary fact, and one that first came to light in the reflection I began last year with the first part of this Introduction, at the same time as I glimpsed the deep roots of this puzzling fact. ...

Even though it's aimed at readers who are perfectly "in the know" in every respect, there's still one important thing that the "de rigueur" mode of exposure refuses to communicate. It's also something that's totally frowned upon in the circles of serious people, like us scientists in particular! I

want to talk about **dreams**. The dream, and the visions it breathes into us - impalpable like itself at first, and often reluctant to take shape. Long years, even a lifetime of intense work may not be enough ☐ pas perhaps

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to see a dream vision fully manifest, to see it condensed and polished to the hardness and brilliance of a diamond. This **is** our work, done by hand or by mind. When the job is done, or a part of it, we present the tangible result in the brightest light we can find, rejoice in it, and often take pride in it. Yet it's not in this diamond, which we've cut at great length, that we find the inspiration for cutting it. We may have fashioned a tool of great precision, an efficient tool - but the tool itself is limited, like all things made by human hands, even when they seem great to us. A vision, unnamed and unoutlined at first, tenuous as a shred of mist, has guided our hand and kept us bent over the work, without feeling the passing hours or perhaps years. A flap that noiselessly detached itself from a bottomless sea of mist and gloom. ... What is limitless in us is Her, this Sea ready to conceive and give birth unceasingly, when our thirst fecundates Her. The Dream is born of these marriages, like the embryo nestled in the womb, awaiting the obscure labours that will lead it to a second birth, in the light of day.

Woe betide a world where dreams are scorned - it's also a world where what's deep inside us is scorned. I don't know if any other culture before ours - the culture of television, computers and transcontinental rockets - has professed such contempt. It must be one of the many ways in which we distinguish ourselves from our predecessors, whom we have so radically supplanted, eliminated as it were from the face of the planet. I know of no other culture where the dream is not respected, where its deep roots are not felt by all and recognized. And is there any major work in the life of a person or a people that was not born of a dream and nourished by it before coming to light? In our country (or should we say everywhere?), respect for dreams is called "superstition", and it's well known that our psychologists and psychiatrists have taken the measure of dreams in great length and breadth - hardly enough to clutter the memory of a small computer, surely. It's also true that no-one "back home" knows how to light a fire, or dares to watch their child being born or their mother or father dying in their own home.

have clinics and hospitals for that. Thank God for that. ... Our world, so proud of its power in atomic megatons and in the amount of information stored in its libraries ☐ and in its computers, is undoubtedly also the one in which everyone's **powerlessness**, fear and contempt for the simple, essential things in life reached its peak.

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Fortunately, dreams, like the original sex drive in even the most repressive society, have a way of enduring! Superstition or not, they continue to obstinately whisper to us knowledge that our waking minds are too heavy, or too pusillanimous, to grasp, and to give life and wings to the projects they have inspired.

I suggested earlier that dreams are often reluctant to take shape, but that's just an appearance, and doesn't really get to the heart of the matter. The "reluctance" would rather come from our waking mind, in its ordinary "plate" - and "reluctance" is a euphemism! Rather, it's a deep-seated mistrust, covering up an ancestral fear - **the fear of knowing**. Speaking of dreams in the true sense of the word, this fear is all the more powerful, all the more effective as a screen, because the message of the dream touches us more closely, because it carries with it the threat of a profound transformation of our very being, should it happen to be heard. But it seems that this mistrust is present and effective even in the relatively innocuous case of the mathematical "dream"; so much so that all dreams seem to be banned not only from texts (I don't know of any in any case where there is any trace of them), but also from discussions between colleagues, in small groups, or even one-on-one.

The reason for this apparent absence, this conspiracy of silence, is certainly not that the mathematical dream doesn't exist or no longer exists - our science would then have become sterile, which is by no means the case. Surely, the reason for this apparent absence, this conspiracy of silence, is closely linked to that other consensus - that of carefully erasing all trace and mention of the work through which our knowledge of the world is discovered and renewed. Or rather, **it is one and the same silence that surrounds both the dream and the work it inspires and nourishes**. So much so, in fact, that the very term "mathematical dream" will seem nonsensical to many of us, so often driven by push-button clichés, rather than by the direct experience we can have of a simple, everyday, important reality.

## 6.2. (6) The Dreamer

☐ In fact, I know from experience that when the mind is eager to get to know it, instead of running away from it (or approaching it with a patented grid in hand, which amounts to the same thing), the dream is in no way reluctant "to take shape" - to let itself be delicately described and deliver its message, always simple, never silly, and sometimes overwhelming. On the contrary, the Dreamer in us is an incomparable master at finding, or creating from one occasion to the next, the most appropriate language to circumvent our fears, to shake our torpor, with scenic means varying infinitely, from the absence of any visual or sensory element whatsoever, to the most breathtaking stagings. When He shows up, it's not to evade Himself, but to encourage us (almost always to no avail, but His benevolence never tires...) to get out of ourselves, out of the heaviness in which He sees us stuck, and which He sometimes takes the slightest pleasure in parodying in comical colors. Lending an ear to the Dreamer within us means communicating with ourselves, against the powerful barriers that would like to prevent us from doing so at all costs.

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But who can do more, can do less. If we can communicate with ourselves through the dream, revealing ourselves to ourselves, surely it must be possible in an equally simple way to communicate to others the by no means intimate message of the mathematical dream, let's say, which does not involve forces of

resistance of comparable power. And to tell the truth, what else have I done in my mathematical past, if not follow, "dream" to the end, until their most manifest, most solid: irrefutable manifestation, shreds of dream detaching themselves one by one from a heavy, dense fabric of mists? And how many times did I tremble with impatience at my own obstinacy in jealously polishing to the last facet each precious or semi-precious stone in which my dreams condensed - rather than following a deeper impulse: that of following the multiform arcana of the mother fabric - to the indecisive confines of the dream and its patent incarnation, "publishable" in short, according to the canons in force! In fact, I was about to follow this impulse, to embark on a work of "mathematical science fiction", "a kind of daydream" about a theory of "patterns" that remained purely hypothetical at the time - and has remained so to this day.

p. 13 even today, and for good reason, for want of another "daydreamer" to embark on this adventure. It was towards the end of the sixties, when my life  $\square$  (without my suspecting it in the slightest) was about to change. took a completely different turn, which for the next ten years or so would relegate my mathematical passion to the sidelines.

But all things considered, "A la Poursuite des Champs", this first publication after fourteen years of silence, is very much in the spirit of that "waking dream" which was never written, and of which it seems to have been the provisional sequel. Admittedly, the themes of these two dreams are as dissimilar, at least at first glance, as it is possible for two mathematical themes to be; not to mention that the first, that of motifs, would seem to lie more on the horizon of what might be "feasible" with the means at hand, whereas the second, the famous "fields" and consorts, appear to be entirely within reach. These are dissimilarities that could be called fortuitous or accidental, and which will perhaps fade away much sooner than we expect<sup>1</sup> (3). They have relatively little impact, it seems to me, on the kind of work to which the two themes can give rise, since it's all about "daydreaming", or, to put it in less provocative terms: continuing the work of conceptual rough-casting until an overall vision of sufficient coherence and precision is achieved, to bring about the more or less complete conviction that the vision does indeed correspond, in essence, to the reality of things. In the case of the theme developed in this book, this should mean, more or less, that the detailed verification of the validity of this vision becomes a matter of pure craft. This may well require a considerable amount of work, with its share of astuteness and imagination, and no doubt also unexpected twists and perspectives, which will make it something other than a purely routine task (a "long exercise", as André Weil would say).

It's the kind of work, in short, that I've done over and over again in the past, that I've got at my fingertips and that there's no need for me to do again in the years still ahead of me. Insofar as I'm once again investing myself in mathematical work, it's on the fringes of "daydreaming" that my energy will surely be best employed. In this choice, it's not a concern for profitability that inspires me (assuming that such a concern could inspire anyone), but a dream, or dreams. If this new impetus within me is to prove a source of strength, it will have been drawn from the dream!

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<sup>1</sup>(3)

I am thinking in particular of the famous conjectures of Mordell, Tate and Shafarevich, all of which have been proven. three last year in a forty-page manuscript by Faltings, at a time when the well-established consensus of those "in the know" was that these conjectures were "out of reach"! As it happens, "the" fundamental conjecture that serves as the keystone of the "Anabelian algebraic geometry" program I'm so fond of, is close to Mordell's conjecture. (It would even seem that the latter is a consequence of the former, which just goes to show that this program is not a story for serious people. ...). )

## 6.3. (7) The legacy of Galois

□ It would seem that of all the natural sciences, it is only in mathematics that what I have called "dreaming", or "daydreaming", is subject to a seemingly absolute ban, more than twice a millennium old. In the other sciences, including reputedly "exact" sciences such as physics, dreaming is at least tolerated, even encouraged (depending on the era), under admittedly more "outlandish" names such as: "speculations", "hypotheses" (such as the famous "atomic hypothesis", the result of a dream, pardon the pun, of a speculation by Democritus), "theories"... . The transition from the status of a dream-which-dare-not-say-its-name to that of "scientific truth" takes place by insensible degrees, through a gradually widening consensus. In mathematics, on the other hand, it is almost always (these days at least) a sudden transformation, by virtue of the magic wand of a **demonstration**<sup>2</sup> (4). At a time when the notion of mathematical definition and demonstration was not, as it is today, clear and the object of a (more or less) general consensus, there were nonetheless visibly important notions that had an ambiguous existence - such as that of a "negative" number (rejected by Pascal) or that of an "imaginary" number. This ambiguity is reflected in the language still in use today.

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The gradual clarification of the notions of definition, statement, demonstration and mathematical theory has been extremely beneficial in this respect. It has made us aware of the power of the tools we have at our disposal to formulate with perfect precision what might otherwise have seemed unformulate - by virtue of a sufficiently rigorous use of everyday language, that is. If there's one thing that has fascinated me about mathematics since I was a child, it's precisely this power to capture in words, and to express perfectly, the essence of such mathematical things that at first glance present themselves in such elusive, or mysterious, form that they seem beyond words. ...

However, an unfortunate psychological side-effect of this power, of the resources offered by perfect precision and demonstration, is that they have further accentuated the traditional taboo on the "thematic dream"; that is, on anything that does not present itself under the conventional aspects of precision (albeit at the expense of a broader vision), guaranteed to be "right" thanks to well-designed demonstrations, or else (and increasingly so □ plus these days. . . ) by sketches of demonstrations, supposedly able to form. Occasional **conjectures** are tolerated in a pinch, as long as they satisfy the conditions of precision of a questionnaire, where the only accepted answers would be "yes" or "no". (And, needless to say, on condition that the person who makes them is well known in the mathematical world). To my knowledge, there has been no example of the development, on an "experimental" basis, of a mathematical theory that would be explicitly conjectural in its essential parts. It's true that, according to modern standards, the entire calculus of the "infinitely small" developed from the seventeenth century onwards, which has since become the differential and integral calculus, would appear to be a daydream, eventually transformed into a

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<sup>2</sup>(4)

Even today, we come across "demonstrations" of uncertain status. For years, this was the case with Grauert's demonstration of the fi nite theorem that bears his name, which nobody (and there was no shortage of good-willers!) could read. This perplexity was resolved by other, more transparent demonstrations, some of which went further, taking over from the initial one. A similar, more extreme situation is the "solution" to the so-called "four-color problem", the computational part of which was solved with a computer (and a few million dollars). This is a "demonstration" that is no longer based on the intimate conviction that comes from understanding a mathematical situation, but on credit given to a machine devoid of the faculty of understanding, and whose structure and operation the mathematician user is unaware of. Even supposing that the calculation is confirmed by other computers, following other calculation programs, I don't consider the four-color problem to be closed. It will only have changed its face, in the sense that it will no longer be a question of looking for a counter-example, but only a demonstration (readable, of course!).

serious mathematics only two centuries later, thanks to the magic wand of Cauchy. And I'm reminded of **Evariste Galois'** daydream, which had no luck with Cauchy himself; but this time it took less than a hundred years for another wand, this time from Jordan (if I remember correctly), to give the dream a new name, "Galois theory".

The observation that emerges from all this, and which is not to the advantage of "Mathematics 1984", is that it's fortunate that people like Newton, Leibnitz, Galois (and I'm sure many others, as I'm not well versed in history...) were not encumbered by our current canons, at a time when they were content to discover without taking the trouble to canonize!

The example of Galois, who came here without my calling him, strikes a chord with me. I seem to recall that a feeling of fraternal sympathy for him was awakened the first time I heard about him and his strange destiny, when I was still a high school or university student, I think. Like him, I felt a passion for mathematics - and like him, I felt like an outsider, a stranger in the "beautiful world" that (it seemed to me) had rejected him. Yet I ended up being part of this beautiful world myself, only to leave it one day, without regret... . This somewhat forgotten affinity reappeared to me quite recently, and in a whole new light, as I was writing "Esquisse d'un Programme" (on the occasion of my request for a

admission as a researcher at the Centre National de la Recherche Scientifique). This report is mainly devoted to a sketch of my main themes of reflection over the last ten years or so. Of all

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Of these themes, the one that fascinates me the most, and which I intend to develop in particular over the next few years, is the very type of mathematical dream, which, incidentally, is similar to the "dream of patterns", for which it provides a new approach. In writing this sketch, I was reminded of the longest mathematical reflection I've pursued in one go in the last fourteen years. It lasted from January to June 1981, and I called it "The Long Walk through Galois Theory". One thing led to another, and I realized that the daydream I'd been pursuing sporadically for several years, which had come to be known as "Anabelian algebraic geometry", was nothing other than a continuation, "an ultimate culmination of Galois' theory, and no doubt in the spirit of Galois".

When this continuity became apparent to me, as I was writing the passage from which the quoted line is taken, a joy came over me that has not dissipated. It was one of the rewards of working in complete solitude. Its appearance was as unexpected as the more than fresh welcome I had received from two or three colleagues and old friends who were well "in the know", one of whom, incidentally, was my pupil, and to whom I had had the opportunity to talk, still "hot off the press" and in the joy of my heart, about these things I was in the process of discovering. ...

It reminds me that to take up Galois' legacy today is surely also to accept the risk of the solitude that was his in his time. Perhaps times are changing less than we think, but this "risk" often doesn't feel like a threat to me. While I am sometimes saddened and frustrated by the indifference or disdain of those I have loved, I have never been weighed down by loneliness, mathematical or otherwise, for many years. If ever there was a faithful friend I long to find again when I leave her, it's her!

## 6.4. (8) Dream and demonstration

But let's get back to the dream, and the prohibition that has plagued mathematics for millennia. This is perhaps the most inveterate of all the a-priorities, often implicit and rooted in habit, decreeing that one thing is "math" and another is not. It took millennia for such childish things to become accepted.

and ubiquitous that the symmetry groups of certain geometric figures, the topological forms of certain others, the number zero, the sets find admission in the sanctuary! re!

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hen I talk to desp

students of the topology of a sphere, and the shapes that can be deduced from a sphere by adding coves - things that don't surprise young children, but which baffle them because they think they know what "maths" is - the first spontaneous echo I get is: but that's not maths! Maths, of course, is the Pythagorean theorem, the heights of a triangle and second-degree polynomials... . These students are no more stupid than you or I. They're reacting as all the world's mathematicians have reacted from time immemorial to the present day, except for people like Pythagoras or Riemann and maybe five or six others. Even Poincaré, who wasn't the first to come along, managed to prove with a well-felt philosophical A plus B that infinite sets weren't math! Surely there must have been a time when triangles and squares weren't maths - they were drawings that kids or craftsmen traced on the sand or in the clay of vases, not to be confused...

This fundamental inertia of the mind, suffocated by its "knowledge", is certainly not peculiar to mathematicians. I'm straying a little from the point I'm trying to make: **the ban on mathematical dreams**, and through them, on anything that doesn't present itself in the usual guise of a finished product, ready for consumption. The little I've learned about the other natural sciences is enough to make me realize that a similarly rigorous ban would have condemned them to sterility, or to a tortoise's progress, rather like in the Middle Ages when there was no question of dehornifying the letter of the Holy Scriptures. But I'm also well aware that the deep source of discovery, just as the process of discovery in all its essential aspects, is the same in mathematics as in any other region or thing in the universe that our body and mind can experience. **To banish the dream is to banish the source** - to condemn it to an occult existence.

And I'm also well aware, from an experience that has not wavered since my first, juvenile love affair with mathematics, that in the unfolding of a vast or profound vision of mathematical things, it is this unfolding of a vision and understanding, this progressive penetration, that constantly **precedes** the demonstration, that makes it possible and gives it meaning. When a situation, from the humblest to the broadest, has been understood in its essential aspects, the demonstration of what is understood (and of the rest) falls like a ripe fruit at the right moment. Whereas the demonstration **plucked like** a still-green fruit from the treep . 18  
of knowledge leaves an aftertaste of dissatisfaction, a frustration of our unquenchable thirst. Two or three times in my life as a mathematician, for want of anything better, I've had to pluck the fruit rather than pick it. I'm not saying I did wrong, or that I regret it. But what I did best and what I liked best, I took willingly, not by force. If mathematics has given me joy in profusion and continues to fascinate me in my middle age, it's not because of the demonstrations I've been able to wring out of it, but because of the inexhaustible mystery and perfect harmony I sense in it, always ready to reveal itself to a loving hand and gaze.

## 6.5. (9) Strangers welcome

I think the time has come to say something about my relationship with the world of mathematicians. This is quite different from my relationship with mathematics. The latter existed and was strong from an early age, long before I even suspected that there was a world and a milieu of mathematicians. A complex world, with its learned societies, periodicals, meetings, colloquia, congresses, primas-donnes and tâcherons, its power structure, its eminences grises, and the no less grey mass of the taillables et corvéables, in need of a thesis or articles, and of those too, rarer still, who are rich in means and ideas and

come up against closed doors, desperate to find the support of one of those powerful, pressurized and feared men who have that magic power: to get an article published... ..

I discovered the existence of a mathematical world when I arrived in Paris in 1948, at the age of twenty, with a Licence es Sciences from the University of Montpellier in my meagre suitcase, and a tightly-written, double-sided manuscript with no margins (paper was expensive!), representing three years of solitary reflection on what (I later learned) was then well known as "measurement theory" or "Lebesgue's integral". Since I'd never met anyone else, until the day I arrived in Paris, I thought I was the only person in the world "doing math", the only **mathematician**. (It was the same for me, and remains so to this day). I had juggled with sets that I called measurable (without ever having met a set that wasn't. . . ) and with the almost everywhere, but didn't know what a topological space was. I remained a little lost in a dozen non-equivalent notions of "abstract space" and compactness, sinned in a little booklet (by someone called Appert

p. 19 I think, in *Actualités Scientifiques et Industrielles*), on which I had stumbled: God knows how. I'd never heard before, in a mathematical context at least, such strange or barbaric words as group, body, ring, module, complex, homology (and the list goes on!), which suddenly, without warning, came crashing down on me all at once. It was a rude shock!

If I "survived" this shock, and went on to do maths and even make a living of it, it's because in those early days, the mathematical world hardly resembled what it has since become. It's also possible that I'd been lucky enough to land in a more welcoming corner of this unsuspected world. I had a vague recommendation from one of my professors at the Faculty of Montpellier, Monsieur Soula (no more than any of his colleagues, he had not seen me often in his classes!), who had been a pupil of Cartan (father or son, I couldn't say). As Elie Cartan was already "out of the game" by then, his son Henri Cartan was the first "fellow student" I had the pleasure of meeting. I had no idea how auspicious this was! I was greeted by him with the kindly courtesy that distinguishes him, well known to the generations of Normaliens who had the good fortune to make their very first acquaintance with him. He must not have realized the extent of my ignorance, judging by the advice he gave me to guide my studies. Be that as it may, his benevolence was obviously directed at the person, not at the background or potential gifts, nor (later) at a reputation or notoriety. . .

In the year that followed, I was host to one of Cartan's lectures at "l'Ecole" (on differential formalism on varieties), to which I clung firmly; and to the "Séminaire Cartan", witnessing in amazement the discussions between him and Serre, with "Suites Spectrales" (brr!) and drawings (called "diagrams") full of arrows covering the whole board. Those were the heroic days of the theory of "beams", "cara-paces" and a whole arsenal whose meaning totally escaped me, even though I was trying so hard to make sense of it.

to swallow definitions and statements and check demonstrations. At the Séminaire Cartan there were also periodic appearances by Chevalley, Weil, and the days of the Bourbaki Seminars (bringing together une

p. 20 In the early hours of the morning, when the crowd was still in its early twenties or thirties (participants and listeners at most), the other members of the famous Bourbaki gang - Dieudonné, Schwartz, Godement and Delsarte - would turn up like a group of noisy friends. They were all on a first-name basis, spoke the same language that almost totally escaped me, smoked a lot and laughed a lot, and the only thing missing was the crates of beer to complete the atmosphere - which was replaced by chalk and sponge. It was a very different atmosphere from Leray's lectures at the Collège de France (on Schauder's theory of topological degree in infinite-dimensional spaces, poor me!), which I went to listen to on Cartan's advice. I had gone to see Monsieur Leray at the Collège de France to ask him (if I remember correctly) what his lecture would be about. I don't remember what he told me,



or whether I understood anything at all - only that here, too, I felt a benevolent welcome, addressed to the first stranger who came along. It was this and nothing else, surely, that made me go to this course and bravely hold on to it, as I had done at the Cartan Seminar, even though the meaning of what Leray was expounding there almost totally escaped me at the time.

The strange thing was that, in this world to which I was a newcomer and whose language I hardly understood and spoke even less, I didn't feel like a stranger. While I hardly ever had the opportunity to speak (and with good reason!) with one of those cheerful fellows like Weil or Dieudonné, or with one of those more distinguished gentlemen like Cartan, Leray or Chevalley, I nevertheless felt **accepted**, I'd almost say: **one of them**. I can't recall a single occasion when I was treated with condescension by one of these men, or when my thirst for knowledge, and later, again, my joy in discovery, was dismissed with smugness or disdain<sup>3</sup> (5). If it hadn't been so, I wouldn't have "become a mathematician" as they say - I'd have chosen another profession, where I could give my all without having to face scorn. ...

Although I was "objectively" a stranger to this world, just as I was a stranger to France, a link united me to these men from another background, another culture, another destiny: a shared passion. I doubt that, in that crucial year when I was discovering the world of mathematicians, any one of them, not even Cartan, of whom I was a bit of a pupil but who had many others (and some who weren't so clueless!), perceived in me the same passion that <sup>them</sup>habited. For them, I had to be one among a mass of course listeners and seminars, taking notes and obviously not quite up to speed. If I perhaps stood out in any way from the other listeners, it was that I wasn't afraid to ask questions, which more often than not had to do with my phenomenal ignorance of both language and mathematics. The answers could be brief, or even astonished, but never did the bemused oddball that I was then encounter a rebuff, a "putting in my place", either in the informal milieu of the Bourbaki group, or in the more austere setting of the Leray course at the Collège de France. In those years, ever since I arrived in Paris with a letter to Elie Cartan in my pocket, I've never had the impression of finding myself in front of a clan, a closed or even hostile world. If I've ever experienced this inner contraction in the face of contempt, it wasn't in that world; at least not in those days. Respect for the individual was part of the air I breathed. You didn't have to earn respect, prove yourself before you were accepted, and treated with any kind of kindness. Strangely enough, it was enough to be a person, to have a human face.

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<sup>3</sup>(5)

This fact is all the more remarkable given that up until 1957, I was regarded with a certain reserve by more than one member of the company.

of the Bourbaki Group, which, I believe, co-opted me with some reluctance. A good-natured quip put me among the "dangerous specialists" (in Functional Analysis). At times, I sensed a more serious, unspoken reserve in Cartan - for some years, I must have given him the impression of someone inclined towards gratuitous, superficial generalization. I saw him quite surprised to find in the first (and only) rather long essay I wrote for Bourbaki (on differential formalism on varieties) a reflection of any substance - he hadn't been too keen when I'd offered to take it on. (This reflection came in handy again years later, when I developed the residue formalism from the point of view of coherent duality). I was more often than not left in the lurch during Bourbaki congresses, especially during the joint readings of the essays, being quite unable to keep up with the readings and discussions at the rate they were going. Maybe I'm just not cut out for collective work. In any case, the difficulty I had in integrating myself into the common work, or the reservations I may have aroused for other reasons in Cartan and others, never drew sarcasm or rebuff, or even a shadow of condescension, except at most once or twice from Weil (definitely a case apart!). At no time did Cartan deviate from an equal kindness towards me, imbued with cordiality and also with that distinctive touch of humor that for me remains inseparable from his person.

## 6.6. (10) The "Mathematical Community": fiction and reality

So it's hardly surprising that, perhaps from that year onwards, and more and more clearly in any case over the years that followed, I felt part of this world, to which I was happy to refer under the meaningful name of "**mathematical community**". Before writing these lines, I never had the opportunity to examine the meaning I gave to this name, even though I identified with this "community" to a large extent. It's clear now that this community represented for me nothing more and nothing less than a kind of ideal extension, in space and in time, of the benevolent world that had welcomed me and accepted me as one of their own; a world, moreover, to which I was linked by one of the great passions that have dominated my life.

p. 22 This "community", with which I gradually became identified, was not an entirely fictitious extrapolation of the mathematical milieu that had initially welcomed me. The initial milieu gradually expanded, by which I mean: the circle of mathematicians I was led to frequent regularly, driven by themes of common interests and personal affinities, has gone from strength to strength in the ten or twenty years since that first contact. In concrete <sup>termes</sup>, it's the circle of colleagues and friends, or rather this structure concentric, ranging from the colleagues with whom I was most closely associated (first Dieudonné, Schwartz, Godement, later above all Serre, later still people like Andreotti, Lang, Tate, Zariski, Hironaka, Mumford, Bott, Mike Artin, not to mention the people in the Bourbaki group, which was also gradually expanding, and the students who came to me from the 1960s onwards...), to other colleagues I'd met here and there and with whom I had more or less close affinities - it was this microcosm, formed by chance encounters and affinities, that represented the concrete content of this name, charged with warmth and resonance for me: the mathematical community. When I identified with it as a warm, living entity, it was in fact this microcosm with which I identified.

It was only after the "great turning point" of 1970, the first **awakening** I should say, that I realized that this cozy, friendly microcosm represented only a tiny portion of the "mathematical world", and that the traits I liked to attribute to this world, which I continued to ignore and had never thought of taking an interest in, were fictitious traits.

Over the past twenty-two years, this microcosm itself had changed its face, in a world that was also changing. Certainly, over the years, I too had changed, without suspecting it, as had the world around me. I don't know if my friends and colleagues were more aware of this change than I was, in the world around them, in their own microcosm, and in themselves. Nor can I say when or how this strange change came about - it probably came about insidiously, in fits and starts: **the man of notoriety was feared**. I myself was feared - if not by my students, then by my friends, or by those who knew me personally, at least by those who knew me only through notoriety, and who did not feel themselves protected by comparable notoriety.

p. 23 I only became aware of the fear that is rife in the mathematical world (and just as much, if not more, in other scientific circles) than in the aftermath of my "awakening" nearly fifteen years ago. Over the preceding fifteen years, I had gradually and unsuspectingly assumed the role of the "great boss", in the world of mathematical Who is Who. <sup>Without</sup> also suspecting it, I was a prisoner of this role, which isolated me from all but a few "peers" and a few students (and still... ) who decidedly "wanted it". It was only when I stepped out of this role that at least some of the fear surrounding it fell away. Tongues were loosened that had been silent before me for years.

The testimony they brought me was not only one of fear. It was also the testimony

**taken.** The contempt of those in power towards others, a contempt that creates and fuels fear.

I didn't have much experience of fear, but of contempt, in times when a person's persona and life didn't carry much weight. It had pleased me to forget the time of contempt, and here it was again! Perhaps it had never stopped, when I'd simply moved on to another world (as it seemed to me), or looked the other way, or simply pretended not to see or hear anything, apart from the fascinating and interminable mathematical discussions? These were the days when I finally accepted the fact that contempt was rampant all around me, in the world I'd chosen as my own, with which I'd identified, which I'd vouched for and which had pampered me.

## 6.7. (11) Meeting with Claude Chevalley, or: freedom and good feelings

Perhaps the preceding lines give the impression that I was overwhelmed by the testimonials that poured in almost overnight. Not so. These testimonies were recorded at a superficial level. They were simply added to other facts that I had just learned, or that I had known but avoided paying attention to until then. Today, I would ex-prime the lesson I learned then as follows: "scientists", from the most illustrious to the most obscure, are people just like everyone else! I had deluded myself into thinking that "we" were something better, that we had something extra - it took me a good year or two to get rid of that stubborn illusion!

Among the friends who helped me, only one was part of the milieu I'd just left, with no desire to return.<sup>4</sup>

(6). It's Claude Chevalley. While he didn't make speeches and wasn't interested in mine, I think I can say that I learned more important and more hidden things from him than the one I've just said.

In the days when I used to see him quite regularly (the days of the "Survivre" group, which he had joined with a mixed conviction), he often baffled me. I don't know how, but I felt he had a special gift for me. p. 24

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### <sup>4</sup>(6) My friends from Survivre et Vivre

Among these friends, I should probably also count Pierre Samuel, whom I had previously known mainly through Bourbaki, just like Chevalley, and who (like Chevalley) played an important role in the Survivre et Vivre group. It doesn't seem to me that Samuel was so much into this illusion of the superiority of the scientific queue. Above all, I feel he contributed a great deal through the common sense and smiling good humor he brought to joint work, discussions and relations with others, as well as gracefully taking on the role of "ugly reformer" in a group inclined towards radical analyses and options. He stayed with Survivre et Vivre for some time after I withdrew, acting as editor of the newsletter of the same name, and left with good grace (to join Friends of the Earth) when he felt that his presence in the group had ceased to be useful.

Samuel belonged to the same restricted milieu as I did, which didn't prevent him from being one of the friends I made during those bubbling years, from whom I believe I learned something (as bad a student as I was. . . ). These ways of being, just like Chevalley's even though they hardly resemble each other, were a better antidote for my "meritocratic" inclinations than the sharpest analysis!

It now seems to me that for all the friends from that period from whom I learned something, it was more through their way of being and their sensitivity, which differed from mine, and from whom "something" was communicated, than through explanations, discussions, etc. ... . In this respect, I especially remember, in addition to Chevalley and Samuel, Denis Guedj (who had a great influence on the Survivre et Vivre group), Daniel Sibony (who kept his distance from this group, while pursuing its evolution with a half-disdainful, half-narcotic eye), Gordon Edwards (who was a co-actor in the birth of the "movement" in June 1970 in Montreal, and who for years did prodigious feats of energy to maintain an "American edition" of the Survivre et Vivre newsletter in English), Jean Delord (a physicist about my age, a fine and warm-hearted man, who took a liking to me and the Survivien microcosm), Fred Snell (another US-based physicist from Buffalo, whose country house I stayed in for a few months in 1972).

Of all these friends, five are mathematicians, two are physicists, and all are scientists - which seems to show that the milieu closest to me in those years remained a milieu of scientists, and especially mathematicians.

knowledge that eluded me, an understanding of certain essential and simple things, which can certainly be expressed in simple words, but without the understanding "passing" from one to the other. I realize now that there was a difference in maturity between him and me, which meant that I often felt at odds with him, in a kind of dialogue of the deaf that was not due to a lack of mutual sympathy or esteem. Although he didn't express himself in these terms (as far as I can remember), it must have been clear to him that the "questioning" (of the "social role of the scientist", of science, etc. . . ) to which I was then arriving, either on my own, or through the logic of joint reflection and activity within the "Survivre" group (which later became "Survivre et Vivre")-that questioning remained basically superficial. They concerned the world in which I lived, and even the role I played in it - but they didn't really involve me in any profound way. My vision of myself, during those heady years, didn't change a bit. It wasn't then that I began to get to know myself. It was only six years later that, for the first time in my life, I got rid of a persistent illusion, not about others or the world around me, but about myself. It was another awakening, more far-reaching than the first that had prepared it. It was one of the first in a whole "cascade" of successive awakenings, which I hope will continue in the years that remain to me.

I don't recall Chevalley ever alluding to self-knowledge, or "self-discovery" for that matter. In retrospect, however, it's clear that he must have started getting to know himself a long time ago. He sometimes spoke about himself, just a few words on the occasion of this or that, with disconcerting simplicity. He's one of the two or three people I've never heard come up with a cliché. He spoke very little, and what he said expressed not ideas that he had adopted and made his own, but a personal perception and understanding of things.

That's why I'm sure he often baffled me, even back in the days when we were still meeting in the Bourbaki group. What he was saying often upset ways of seeing that were <sup>dear</sup> to me, and that for That's why I considered them "true". There was an inner autonomy in him that I lacked, and which I began to perceive obscurely at the time of "Survivre et Vivre". This autonomy is not a matter of intellect or discourse. It's not something you can "adopt", like ideas, points of view, etc. ... . Fortunately, the idea would never have occurred to me to want to "make my own" this autonomy perceived in another person. I had to find my own autonomy. That also meant learning (or relearning) to be myself. But in those years, I had no idea of my lack of maturity, of inner autonomy. If I eventually discovered it, it must have been the encounter with Chevalley that was one of the leavenings that silently worked within me, while I was embarking on major projects. It wasn't speeches or words that sowed that seed. To sow it, it was enough for a person I met along the way to dispense with speeches, and just be himself.

It seems to me that in those early seventies, when we met regularly to publish the bulletin "Survivre et Vivre", Chevalley was trying, without insistence, to communicate a message to me that I was then too clumsy to grasp, or too wrapped up in my militant tasks. I was dimly aware that he had something to teach me about freedom - about inner freedom. Whereas I tended to operate on the basis of high moral principles, and had begun to sound that trumpet in the early issues of Survivre as a matter of course, he had a particular aversion to moralizing. I think that was the thing that most baffled me about him in the early days of Survivre. For him, such discourse was just an attempt at constraint, superimposed on a multitude of other external constraints stifling the individual. Of course, you can spend your whole life discussing a

such a way of seeing, the pros and cons. It completely overturned my own, which (as you might imagine) was driven by the noblest and most generous feelings. It was incomprehensible to me that Chevalley, for whom I had the highest esteem and with whom I felt a bit like a comrade-in-arms, should take such malicious pleasure in not sharing these feelings! I didn't understand that the truth, the reality of things, is only one thing.

It's not a question of good feelings, points of view or preferences. Chevalley **saw** one thing, everything simple and real, and I couldn't see it. It wasn't that he'd read it somewhere; there's nothing

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What do seeing something and reading about it have in common? You can read a text with your hands (in Braille) or with your ears (if someone reads it to you), but you can only see the thing itself with your own eyes. I don't think Chevalley had better eyes than me. But he used them, and I didn't. I was too caught up in my good feelings and everything else to have the leisure to look at the effect of my good feelings and principles on myself and others, starting with my own children.

He must have seen that I often didn't use my eyes, that I didn't even want to. Strange that he never let me know. Or did he, without my hearing? Or did he refrain, judging it a waste of time? Or maybe the idea didn't even occur to him - it was my business after all, not his, whether I used my eyes or not!

## 6.8. (12) Merit and contempt

I'd like to take a closer look, in the light of my own limited experience, at when and how contempt took hold in the world of mathematicians, and more particularly in that "microcosm" of colleagues, friends and students that had become like my second home. And at the same time, to see what part I played in this transformation.

It seems to me I can say, without reservation, that in 1948-49, in the circle of mathematicians I mentioned earlier (whose center for me was the original Bourbaki group), I didn't encounter the slightest trace of contempt, or simply disdain or condescension, towards myself or any of the other young people, French or foreign, who came there to learn the mathematical profession. The men who played a leading role, through their position or prestige, such as Leray, Cartan and Weil, were not feared by me, nor I believe by any of my fellow students. With the exception of Leray and Cartan, who were very "distinguished gentlemen", it took me quite a while to realize that each of these louts, who barged in with Cartan like a friend and obviously "in the know", was a university professor just like Cartan himself, didn't aim from hand to mouth like me, but received what I considered astronomical emoluments, and was, what's more, a mathematician of international stature and influence.

Following a suggestion from Weil, I spent the next three years in Nancy, which at that time was a Bourbaki's headquarters, along with Delsarte, Dieudonné, Schwartz, Godement (and a little later also Serre) taught at the University. There was only a handful of four or five young people there with me (including Lions, Malgrange, Bruhat and Berger, unless I'm mistaken), so we were much less "drowned in the crowd" than in Paris. The atmosphere was all the more familiar, everyone knew each other personally, and I think we were all on first-name terms. When I look back, however, this is the first and only time I saw a mathematician treat a student with undisguised contempt. The unfortunate fellow had come for the day from another town to work with his boss (he was to prepare a doctoral thesis, which he eventually passed with flying colors, and has since acquired a degree in mathematics).

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a certain notoriety, I think). I was quite taken aback by the scene. If someone had used that kind of tone with me for even a second, I'd have slammed the door in their face! As it was, I knew the "boss" well, I was even up to you with him, not the student I only knew by sight. My eldest had, in addition to an extensive culture (not only mathematical) and an incisive mind, a kind of peremptory authority that impressed me at the time (and for quite a long time afterwards, right into the early 70s). He exerted a certain ascendancy over me. I don't remember whether I asked him a question about his attitude, only the conclusion I drew from the scene: this unfortunate pupil must really be a loser, to deserve to be treated like that - something like that. It didn't occur to me then that if the student did indeed suck, that was a reason to advise him to do something else, and to stop working with him, but by no means to treat him with contempt. I had identified myself with the "math whizzes" such as this prestigious elder, at the expense of the "nobodies" whom it would be licit to despise. So I followed the ready-made path of connivance with contempt, which suited me, by emphasizing the fact that I was accepted into the brotherhood of deserving people, the math whizzes!<sup>5</sup> (7)

Of course, I wouldn't have said to myself any more than anyone else: people who try to make maths without getting there are good to despise! I would have heard someone say something of this water, around this time or any other, I would have taken it back beautifully, sincerely sorry for a□ignorance spiritual.

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phenomenal. The fact is, I was bathed in ambiguity, playing both sides of the fence: on the one hand, fine principles and feelings, on the other: poor guy, you'd have to be a real loser to be treated like that (the implication being: this kind of misadventure couldn't happen to me, that's for sure!).

In the end, it seems to me that the incident I reported, and above all the (seemingly harmless) role I played in it, is in fact typical of an ambiguity in me that has followed me throughout my life as a mathematician in the twenty years that followed, and which only dissipated in the aftermath of the "awakening" of 1970<sup>6</sup> (8), without me clearly detecting it until today, when I'm writing these lines. It's a pity I didn't notice it then. Perhaps the time wasn't ripe for me. In any case, the evidence I was receiving at the time about the reign of contempt to which I had chosen to turn a blind eye, did not implicate me personally, nor indeed any of my colleagues and friends in the part of my dear microcosm closest to me<sup>7</sup> (9). It was more to the tune of: ah! how sad it is to have to

<sup>5</sup>(7)

The preceding paragraph is the first in the entire introduction to be heavily crossed out on my original manuscript, and provided with

numerous overloads. At first, the description of the incident and the choice of words went against the grain.

- a force was clearly pushing him to get over the incident as quickly as possible, as if to clear his conscience and "get down to business". These are the familiar signs of **resistance**, here against the elucidation of this episode, and its significance as a revelation of an inner attitude. The situation is very similar to that described at the beginning of this introduction (par. 2), that of the "crucial" moment of discovery of a contradiction and its meaning, in mathematical work: it is then **the inertia** of the mind, its reluctance to part with an erroneous or insufficient vision (but in which our person is in no way involved), which plays the role of "resistance". Resistance is active in nature, inventive if necessary to succeed in drowning a fish even without water, whereas the inertia I spoke of is simply a passive force. In this case, even more than in the case of mathematical work, the discovery that has just appeared in all its simplicity, in all its obviousness, is followed in the instant by a feeling of relief from a weight, a feeling of **liberation**. It's not just a feeling - it's rather an acute, grateful perception of what has just happened, which **is** a liberation.

<sup>6</sup>(8)

As will become clear in the following pages, this ambiguity in no way "dissipated in the aftermath of the 1970 awakening". This is typical of the strategic retreat of the "self", which abandons to profit and loss the period "before the awakening", which immediately becomes the demarcation line for an irreproachable "after"!

<sup>7</sup>(9)

That's not entirely true; there is at least one exception among my closest colleagues, as will become clear later on. There was a typical "laziness" of memory, which often tends to "pass over" facts that don't "fit" with a familiar, long-established view of things.

To learn (or: to teach you) such things, who'd have thought it, you'd really have to be a bastard (I was going to say: null, sorry!) to treat living beings that way! Not so different from the other air after all, just replace "nul" by "salaud" and "se faire traître" by "traître" and you're done! And honor, of course, is intact, for the champion of good causes!

What's clear from this is my connivance with attitudes of contempt. At the very least, it goes back to the very beginnings of the 1950s, in other words, to the years following the warm welcome I received from Cartan and his friends. If I didn't "see anything" later on, when contempt was becoming commonplace just about everywhere, it's because I didn't want to see - any more than in this isolated and particularly flagrant case, where you really had to pull out all the stops to pretend not to see or feel anything!

This connivance was in close symbiosis with my new identity, that of a respected member of a group, the group of deserving people, the math whizzes. I remember feeling particularly satisfied, proud even, that in this world I'd chosen for myself, which had co-opted me, it wasn't social position, nor the fact that I'd become a member of it, that I'd become a member of it.

even (but no!) the only reputation that counted, still had to be deserved - you could be a University professor or academician or whatever, if you were just a mediocre mathematician (poor p. 29

guy!) we were nothing, what counted was merit, profound, original ideas, technical virtuosity, vast visions and all that!

This ideology of merit, with which I had identified wholeheartedly (even though it remained, of course, implicit and unspoken), still took a heavy toll on me in the aftermath, as I said, of the famous awakening of 1970. I'm not sure it disappeared without a trace from that moment on. For that to have happened, I would probably have had to detect it clearly in myself, whereas it seems to me I was mostly denouncing it in others. In fact, it was Chevalley who was one of the first, along with Denis Guedj, whom I also met through Survivre, to draw my attention to this ideology (they called it "**meritocracy**", or something like that), and its violence and contempt. It was because of this, Chevalley told me (it must have been at the time of our first meeting at his place, about Survivre), that he could no longer stand the atmosphere in Bourbaki and had stopped going there. Looking back, I'm sure he must have realized that I'd been part of that ideology, and maybe even that traces of it still remained in some corners. But I don't remember him ever suggesting it. Perhaps he preferred to leave it to me to dot the i's and cross the t's, and I waited until now to do so. Better late than never!

## 6.9. (13) strength and thickness

It's quite possible that the incident I've reported also marks the moment of an inner shift within me, towards a more or less unconditional identification with the brotherhood of merit, at the expense of people considered to be worthless, or simply "without genius" as we would have said a few generations before (this term was no longer in vogue even in my day): dull, mediocre people - at best "resonance boxes" (as Weil wrote somewhere) for the great ideas of those who really matter... . The mere fact that my memory, which so often acts as a gravedigger even for episodes that at the time mobilize considerable psychic energy, retained this episode, is not linked to any other directly related memory, and appears so innocuous, makes this feeling of a "tipping point" that would have taken place then plausible.

□ In a meditation less than five years ago, I came to the realization that this ideology of "we great and noble minds... .", in a particularly extreme and virulent form, had reigned supreme.

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in my mother since she was a child, and dominated her relationship with others, whom she liked to look down on from her lofty height with an often disdainful, even contemptuous commiseration. In fact, I admired my parents unreservedly. The first and only group with which I identified myself, before the famous "mathematical community", was the family group reduced to my mother, my father and myself, who had had the honor of being recognized by my mother as worthy of having them as parents. In other words, the seeds of contempt must have been sown in me from childhood. The time might be ripe to follow the vicissitudes, through my childhood and adult life, of these seeds, and the harvests of delusion, isolation and conflict into which some of them have risen. But that's not the place here, where I have a more limited purpose. I think I can say that this attitude of contempt has never in my life taken on a vehemence and destructive force comparable to those I saw in my mother's life, (when I bothered to look at my parents' lives, twenty-two years after my mother's death, and thirty-seven years after my father's). But now is as good a time as any to examine carefully, here, at least, what place this attitude has had in my life as a mathematician.

Before doing so, to put the incident reported in the preceding paragraph into its general context, I'd like to emphasize that it stands entirely alone among my memories of the fifties, and even later. Even today, when I note a sometimes disconcerting erosion of certain elementary forms of courtesy and respect for others in my own milieu<sup>8</sup> (10), the direct and undisguised expression of contempt from boss to pupil must be a rare enough occurrence. As far as the fifties are concerned, I have very few recollections of fear surrounding a figure of notoriety, or of a contemptuous or simply disdainful attitude. If I dig in this direction, I can say that the first time I was received at Dieudonné's home in Nancy, with the delicate friendliness he always showed me, I was a little taken aback by the way this refined and affable man talked about his students.

- they were all morons! It was a chore to give them lectures, which it was obvious they didn't understand. ... After 1970 I heard the echoes□ coming from the amphitheater side, and I knew that Dieudonné was well and truly feared by students. And yet, while he was renowned for having strong opinions and for delivering them with a sometimes thunderous frankness, I never saw him behave in a hurtful or humiliating way, even in the presence of colleagues whom he held in low esteem, or at times of his legendary temper tantrums, which subsided as quickly and easily as they had arisen.

I didn't associate myself with Dieudonné's feelings about his students, but neither did I distance myself from his attitude, which was presented as the most obvious thing in the world, as almost self-evident from someone with a passion for mathematics. Thanks to the benevolent authority of my elder brother, this attitude seemed to me to be at least one of the possible attitudes one could reasonably have towards students and teaching tasks.

It seems to me that for Dieudonné and myself, both imbued with the same ideology of merit, its isolating effect was largely neutralized when we found ourselves in front of a real person, whose very presence silently reminded us of realities more essential than those of so-called "merit", and re-established a forgotten link. The same must have been true for most of our colleagues and friends, no less imbued than Dieudonné or myself with the widespread superiority syndrome. This is no doubt still the case today for many of them.

<sup>8</sup>(10)

For example, I've lost count of the number of letters, on mathematical as well as practical and personal matters, sent to colleagues or ex-students whom I once considered friends, and who have never received a reply. This doesn't seem to be just preferential treatment reserved for me, but a sign of a change in mores, according to echoes in the same vein. (Admittedly, these concern cases where the person sending a mathematical letter was not known to the recipient, a prominent mathematician... .)



Weil also had a reputation for being feared by his students, and he's the only one in my microcosm, in the fifties, whom I had the impression was feared even among colleagues of more modest status (or simply temperament). At times, he would display an attitude of unrelenting haughtiness, which could disconcert even the most hardened self-confidence. My susceptibility helped, and once or twice this led to a passing quarrel. I didn't perceive in his manner a hint of contempt or a deliberate intention to hurt or crush; rather, he had the attitude of a spoiled child, taking pleasure (sometimes maliciously) in making people uncomfortable, as if to convince himself of the power he wielded. In fact, he had a truly astonishing ascendancy over the Bourbaki group, which he sometimes gave me the impression of bossing around, rather like a kindergarten teacher bossing around a group of well-behaved children.

□ I can recall only one other occasion in the fifties when I felt a brutal expression, undisguised contempt. It came from a foreign colleague and friend, about my age. He had an uncommon mathematical power. A few years earlier, when this power was already quite evident, I had been struck by his submission (which seemed to me almost obsequious) to the great professor whose modest assistant he still was. His exceptional abilities soon earned him an international reputation, and a key position at a particularly prestigious university. There, he ruled over a small army of student assistants, apparently just as absolutely as his boss had ruled over him and his fellow students. To my question (if I remember correctly) whether he had any students (by which I mean: who did a good job with him), he replied, with an air of false casualness (I'm translating into French): "douze pièces!" - where "pièces" was the name by which he referred to his pupils and assistants. It's certainly rare for a mathematician to have such a large number of students at the same time doing research under his direction - and surely my interlocutor took a secret pride in this, which he tried to hide under that careless air, as if to say: "oh, just twelve pieces, not even worth talking about!". It must have been around 1959, I already had a good shell so surely, I did get a gutful though! I had to tell him on the spot one way or another, and I don't think he resented it. Perhaps even his relationship with his students wasn't as sinister as his expression might suggest (I didn't get a testimonial from one of his students), and he'd simply been caught up in his childish desire to strut before me in all his glory. Looking back, I can see that this incident must have marked a turning point in our relationship, which had been one of friendship - I sensed in him a kind of fragility, a finesse too, which attracted my affectionate sympathy. These qualities had become blunted, corroded by his position as an important man, admired and feared. After this incident, I still felt uneasy about him - I definitely didn't feel part of the same world as him...

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Yet we were part of the same world - and without realizing it any more than he did, I was probably getting thicker too. In this respect, I still have a vivid memory of the International Congress of Edinburgh, in 1958, Since the previous year, with my work on the Riemann-□Roch theorem, I was promoted to superstar, and (although I didn't have to tell myself this in no uncertain terms at the time) I was also one of the stars of the Congress (where I gave a talk on the vigorous start made by scheme theory in that same year). Hirzebruch (another star of the day, with his very own Riemann-Roch theorem) was giving an opening speech, in honor of Hodge, who was retiring this year. At one point, Hirzebruch suggested that mathematics was made by the work of young people in particular, rather than that of mature mathematicians. This triggered a general outcry of approval in the Congress hall, where young people formed the majority. I was thrilled and very much in agreement, of course - I was thirty years old, which could still pass for young, and the world belonged to me! In my enthusiasm, I had to shout out loud and bang my head on the table. I happened to be sitting next to Lady Hodge, the wife of the

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eminent mathematician who was supposed to be honored on this occasion, as he was about to retire. She turned to me with wide eyes and said a few words, which I no longer remember - but I must have seen reflected in her astonished eyes the tactless thickness that had just been unleashed without restraint in front of this lady at the end of her life. I felt something then, of which the word "shame" perhaps gives a distorted image - rather, a humble truth about who I was then. I didn't have to bang on any more tables that day. ...

## 6.10. (14) Birth of fear

It was around this time, I suppose, when (without having sought it out) I began to be seen as a star in the mathematical world, that a certain fear must also have begun to surround my person, for many unknown or lesser-known colleagues. I suppose so, without being able to place it in a precise memory, in an image that would have struck me and become fixed in my memory, like that incident reported earlier (which undoubtedly marked my first encounter with contempt in my adopted environment). It must have happened insensitively, without attracting my attention, without manifesting itself in some particular, typical incident that memory would have retained, perhaps just as deliberately anodyne as that other incident. What my memory of these transitional years gives me "en bloc" is that it

p. 34 it was not uncommon for people who approached me, whether after my seminar, or during a gathering such as the Bourbaki seminar or some ☐ colloque or congress, to have to overcome a kind of stage fright, which remained more or less apparent during our discussion, if any. When the discussion lasted more than a few minutes, this discomfort usually disappeared gradually as we talked and the conversation became more animated. Occasionally, though rarely, the discomfort would persist to the point of becoming a real obstacle to communication, even at the impersonal level of a mathematical discussion, and I would then feel a confused sense of helpless suffering in front of me, exasperated with itself. I'm talking about all this without really "remembering", as if through a fog that nonetheless restores to me impressions that must have been recorded, and no doubt evacuated as I went along. I'm unable to place in time, other than by supposition, the appearance of this discomfort, an expression of fear.

I don't believe that this fear emanated from me personally, and that it was limited to an attitude, to behaviors that would have set me apart from my colleagues. If this had been the case, it seems to me that I would have started to hear echoes of it in the early seventies, when I stepped out of a role to which I had previously lent myself, the role of the star, the "big boss". I think it was this role, and not myself, that was surrounded by fear. And this role, it seems to me, with this halo of fear that has nothing in common with respect, did not exist, not yet, in the early fifties, at least not in the mathematical milieu that had welcomed me from the very moment I met it, in 1948.

Before this "awakening" in 1970, I wouldn't have thought of describing as "fear" the stage fright and embarrassment I was sometimes confronted with by colleagues who weren't part of my most familiar environment. I was embarrassed by it myself when it manifested itself, and did my utmost to dispel it. A remarkable thing, typical of the lack of attention paid to this sort of thing in my beloved microcosm: I can't recall a single time, in the twenty years I've been part of this milieu, when the question was broached between a colleague and myself, or by others in front of me!<sup>9</sup> (11) This "fog" that takes the place of my memory does not give me

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<sup>9</sup>(11) Aldo Andreotti, Ionel Bucur

Of course, it's not impossible that I've forgotten - not to mention that my particularly "polar" disposition at the time would hardly encourage anyone to talk to me about such things, nor would it lead me to remember any such conversation that might well have taken place. What is certain is that it must have been very exceptional, to say the least, for the question to have been raised.

nor any impression of conscious or unconscious gratification that such situations might have aroused in me. I don't think there was any on a conscious level, but I wouldn't venture to say that I wasn't touched by it occasionally □ unconsciously, in the early years. If so, this must have been fleeting, and not reflected in behavior that would have acted as a fixative for discomfort. It's certainly not that my fatuity wasn't committed to the role I was playing! But if I invested in this role without counting the cost, then what motivated my ego was not the ambition to impress the "rank colleague", but to constantly surpass myself in order to force the ever-renewed esteem of my "peers" - and above all, perhaps, of the elders who had given me credit and accepted me as one of their own even before I had had a chance to show my worth. It seems to me that the inner attitude I had towards the fear I was the object of, which I tried my best to ignore while at the same time dispelling it as best I could wherever it manifested itself - that this attitude can be considered typical throughout the sixties in the milieu (the "microcosm") of which I was a part.

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The situation has deteriorated considerably in the ten or fifteen years since then, at least judging by the signs that reach me from time to time from this world, and the situations of which I have been a close witness, even sometimes a co-actor. More than once, among those of my former friends or pupils who had been dearest to me, I have been confronted with the familiar, unmistakable signs of contempt; with the (seemingly "gratuitous") desire to discourage, humiliate and crush. A wind of contempt blew through this world that had been dear to me. It blows, regardless of "merit" or "demerit", burning with its breath the humblest vocations as well as the most beautiful passions. Is there a single one of my old companions, each protected by solid walls with "his own", settled (as I once was) in the hushed fear that surrounds his person - is there a single one who feels this breath? I know one and only one, among my old friends, who felt it and told me about it, without calling it by name. And I know another who felt it one day, as if against his will, only to forget it the very next day<sup>10</sup> (12). For one of my old friends, feeling this breath and taking it on

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of fear be addressed (without even calling it by that name. . .), and it needs to be addressed just as much today, especially in the "beau monde".

Of my many friends in this world, apart from Chevalley, who must have been aware of this atmosphere of fear at least during the sixties, the only other one I can think of who must have perceived it clearly was Aldo Andreotti. I had met him, his wife Barbara and their twin children (still very small) in 1955 (at a party at Weil's in Chicago, I believe). We remained close friends until the "great turning point" of 1970, when I left the milieu that had been ours and lost touch with them. Aldo had a very keen sensitivity, which hadn't been dulled by his dealings with mathematics and detective stories like mine. He had a gift for spontaneous sympathy for those he came into contact with. This set him apart from all the other friends I knew in the mathematical world, or even outside it. With him, friendship always took precedence over shared mathematical interests (of which there were plenty), and he was one of the few mathematicians with whom I talked a little about my life, and he about his. His father, like mine, was Jewish, and had suffered in Mussolini's Italy, as I had in Hitler's Germany. I saw him always available to encourage and support young researchers, in a climate where it was becoming difficult to be accepted by the establishment. His spontaneous interest was always in people, not in mathematical "potential" or fame. He was one of the most engaging people I've ever had the good fortune to meet.

This evocation of Aldo brings back memories of Ionel Bucur, who was also taken from us unexpectedly and before his time, and like Aldo, missed even more (I think) as a friend whom we like to meet again, than as a partner in mathematical discussions. We sensed in him a kindness, alongside an uncommon modesty, a propensity for constantly stepping aside. It's a mystery how a man so little inclined to think of himself as important, or to impress anyone, ended up as Dean of the Faculty of Sciences in Bucharest; no doubt because he didn't feel like challenging the responsibilities that he was far from coveting, but which his colleagues or the political authorities were placing on his shoulders, which were, it must be said, robust. He was the son of peasants (something that must have played a role in a country where "class" is an important criterion), and had the common sense and simplicity of one. Surely he must have been aware of the fear that surrounds the man of notoriety, but surely it must also have seemed to him a matter of course, the natural attribute of a position of power. I don't think, however, that he himself ever inspired fear in anyone, certainly not in his wife Florica or their daughter Alexandra, nor in his colleagues or students - and the echoes I've been able to gather point in this direction.

<sup>10</sup>(12)

as it is for me, means accepting to take a look at ourselves.

## 6.11. (15) Harvesting and sowing

I don't think, I wouldn't dream of being indignant about a wind that blows, when I've clearly seen that I'm not the only one.

p. 36 am no stranger to this wind, as a fatuity in me would have me believe. And even though I au□rais been a stranger to it, my indignation would have been a paltry offering to those who are humiliated. and those who humiliate, whom I have loved one and all.

I was no stranger to this wind, because of my connivance with the contempt and fear in the world I had chosen. It suited me to turn a blind eye to these and many other blunders, both in my professional life and in my family life. In both, I reaped what I sowed - and what others sowed before me or with me, from my parents (and my parents' parents. . . ) to my new friends of yesteryear. And others besides me are reaping today from the seeds that were sown: my children (and my children's children), as well as one of my pupils today, treated with contempt by another of my pupils of yesteryear.

And there is neither bitterness nor resignation in me, nor self-pity, when I speak of sowing and reaping. For I have learned that in even the bitter harvest there is substantial flesh which it is up to us to feed on. When this substance is eaten and becomes part of our flesh, the bitterness has disappeared, which was merely a sign of our resistance to a food destined for us.

And I also know that there are no harvests that are not also the sowing of other harvests, often more bitter than those that preceded them. There are still times when something in me tightens at the seemingly endless chain of carefree sowing and bitter reaping, handed down and repeated from generation to generation. But I'm no longer overwhelmed or revolted by it, as if it were a cruel and inescapable fate, and even less am I its complacent and blind prisoner, as I once was. For I know that there is a nourishing substance in everything that happens to me, whether the seeds are sown by me or by someone else.

- it's up to me to eat and see it turn into knowledge. And it's no different for my children and all those I've loved and those I love at this moment, when they reap what I sowed in times of fatuity and carelessness, or what I still happen to sow today.

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The word "tomorrow" is to be taken literally, not as a metaphor.

# 7. The double face

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## 7.1. (16) Marshes and first rows

But I haven't yet come to the end of this reflection, on my part in the appearance of contempt and its progression, in this world to which I blithely continued to refer by the name of "ma<sup>thematic</sup> community". It's this reflection, I feel now, who do I have better to offer to those I have loved in this world, as I prepare, not to return, but to express myself once again.

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Above all, I think I need to examine the kind of relationships I had with the people who were part of that world, when I was still part of it like them.

Thinking about it now, I'm struck by the fact that there was a whole part of this world that I used to come into contact with regularly, but which escaped my attention as if it hadn't existed. I must have perceived it at the time as a kind of "swamp" with no clearly defined function in my mind, not even that of a "sounding board" I suppose - as a kind of grey, anonymous mass of those who in seminars and colloquia invariably sat in the back rows, as if they'd been assigned there by birth, those who never opened their mouths during a talk to hazard a question, certain as they must have been in advance that their question could only be off the mark. If they asked a question of people like me, reputed to be "in the know", it was in the corridors, when it was obvious that "the skills" weren't pretending to want to talk amongst themselves - they then asked their question quickly and as if on tiptoe, ashamed of abusing the precious time of important people like us. Sometimes the question seemed to be off the mark, and I'd try (I guess) to say in a few words why; often it was relevant, and I'd answer it as best I could, I think. In both cases, it was rare for a question asked in such a mood (or, should I say, in such an atmosphere) to be followed up by a second question, which would have clarified or deepened it. Perhaps we, the people in the front rows, were indeed in too much of a hurry in such cases (even as we were applying ourselves to the task).

I felt, of course, as did my interlocutor, that there was something false and artificial about the situation we were in. Of course, I sensed, as did my interlocutor, what was false and artificial about the situation in which we were involved - without my ever having formulated it to myself, and without his ever having formulated it to himself either. We both functioned as

p. 38 strange automatons, and a strange connivance bound us together: that of pretending to ignore the anguish that embraced one of us, obscurely perceived by the other - that <sup>particle</sup>□d'angoisse dans l'air chargé d'angoisse that saturated the place, that everyone surely perceived as we did, and that everyone chose to ignore by mutual agreement<sup>1</sup> (13).

This confused perception of anguish didn't become conscious in me until after the first "awakening", in 1970, when this "swamp" came out of the half-light in which I'd been happy to keep it in my mind until then. Suddenly, most of my new friends were precisely those whom, a year earlier, I would have tacitly placed in that nameless, contours-less land. Suddenly, the so-called marsh was coming alive in the faces of friends linked to me by a shared adventure - another adventure!

## 7.2. (17) Terry Mirkil

To tell the truth, even before this crucial turning point, I'd been friends with comrades (who later became "colleagues") whom I'd probably have located in the "swamp", had the question been put to me (and had they not been my friends. . . ). It took this reflection, and a lot of digging into my memories, to bring me back to mind, and for scattered memories to come together. I met these three friends in the very early days, when I was learning the trade in Nancy like them - at a time when we were still in the same boat, when nothing designated me as an "eminence". It's probably no coincidence that there were no other such friendships in the twenty years that followed. The four of us were foreigners, which was certainly a significant bond - my relations with the young "normaliens", parachuted into Nancy like me, were far less personal, and we hardly saw each other except at university. One of my three friends emigrated to South America a year or two later. Like me, he was a research associate at the CNRS, and I had the impression that he didn't really know himself what he was "looking for". We continued to see or write to each other from time to time, and eventually lost touch. My relationship with the other two friends was longer-lasting, stronger and much less superficial. Our mathematical interests played little or no part.

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<sup>1</sup>(13)

Clearly, the foregoing description has no pretension other than to try and render as best I can, in concrete words, what this "fog" of memory delivers to me, which has not been condensed into any kind of case that is even remotely precise, of which I could have given here a description that was even remotely "realistic" or "objective". It would be a misrepresentation to suggest that colleagues who are reluctant to sit in the front rows, or who lack star or eminence status, are necessarily tied up in anguish when talking to one of the latter. This was clearly **not the** case for most of the friends I knew in this milieu, even among those who sometimes haunted conferences and seminars. What is unreservedly true is that the status of "eminence" creates a barrier, a gulf vis-à-vis those without such status, and that this gulf rarely disappears, even if only for the space of a discussion. I would add that the subjective distinction (which nevertheless seems very real to me) between the "front ranks" and the "marshes" can in no way be reduced to sociological criteria (of social position, posts, titles, etc. . . ) or even of "status" or renown, but that it also reflects psychological particularities of temperament or dispositions that are more delicate to pin down. When I arrived in Paris at the age of twenty, I knew that I was a mathematician, that I had **done** maths, and despite the disorientation I've already mentioned, I basically felt "one of them", although I was the only one to know it, and I wasn't even sure that I would continue to do math. Today, I'd be more inclined to sit in the back rows (on the rare occasions when the question arises).

With Terry Mirkil and his wife Presocia, slim and fragile as he <sup>was</sup>□<sup>rable</sup>, with an air of gentleness in both of us, we often spent evenings, and sometimes nights, in Nancy, singing, playing the piano (Terry played then), talking about music, which was their passion, and about other important things

in our lives. Not the **most** important ones, it's true - not the ones that are always so carefully hushed up. ... This friendship has given me a lot, though. Terry had a finesse and discernment that I lacked, when most of my energy was already focused on mathematics. Much more than me, he had retained a sense of the simple, essential things - the sun, the rain, the earth, the wind, the song, friendship ... . After Terry found a position to his liking at Dartmouth College, not so far from Harvard, where I made frequent visits (from the late fifties onwards), we continued to meet and write to each other. In the meantime, I knew he was prone to depressions, which led to long stays in "madhouses", as he called them in the only laconic letter he ever wrote to me, following one of those "horrible stays". When we met, there was never any mention of them - except once or twice, very incidentally, to answer my astonishment that he and Presocia weren't adopting children. I don't think it ever occurred to me that he and I could talk about the substance of the problem, or even touch on it - certainly not even the idea that there might be problems to look at, in my friend or mine. ... There was an unspoken, impenetrable taboo about these things.

Gradually, the meetings and letters became less frequent. It's true that I was becoming more and more a prisoner of tasks and a role, and above all of this desire, which had become like a fixed idea, an escape perhaps from something else, to constantly surpass myself in the accumulation of works - while my family life was mysteriously, inexorably deteriorating. ...

When I learned one day, through a letter from one of Terry's colleagues at Dartmouth, that my friend had committed suicide (this was long after he was already dead and buried. . . ), this news came to me as if through a fog, like an echo from a very distant world that I would have left, God knows when. A world inside me, perhaps, that had died long before Terry ended his life, devastated by the violence of an anguish he hadn't known or wanted to resolve, and that I hadn't known or wanted to guess... .

### 7.3. (18) Vingt ans de fatuité, or: l'ami tireless

□My relationship with Terry was not distorted, at any time I believe, by the difference in our statuses in the mathematical world, or by any feeling of superiority I might have derived from it. This friendship, and one or two others that life gave me in those days (regardless of whether I "deserved" it!) was surely one of the few antidotes then against a secret fatuity, fuelled by social status and, even more so, by the awareness I'd gained of my mathematical power and the value I myself placed on it. The same could not be said of my relationship with my third friend. Over the years, he and his wife (whom he'd met around the time we met in Nancy) showed me a warm friendship, marked by delicacy and simplicity, whenever we met, in their home or mine. In this friendship there was clearly no ulterior motive, linked to status or cerebral abilities. And yet, for over twenty years, my relationship with them remained marked by that deep ambiguity within me, that division I spoke of, which marked my life as a mathematician. In their presence, each time anew, I couldn't help feeling their affectionate friendship and responding to it, almost unwillingly! At the same time, for more than twenty years, I managed the feat of looking at my friend with disdain, from the height of my greatness. This must have been the case from the very first years in Nancy, and for a long time, my prejudice extended to his wife, as well.

if it could only be understood in advance that his wife could only be as "insignificant" as he was. Between my mother and me, we were fond of referring to him only by a mocking nickname, which must have stayed with me long after my mother's death in 1957. It now appears to me that at least one of the forces behind my attitude was the ascendancy that my mother's strong personality exerted over me throughout her life, and for almost twenty years after her death, during which I continued to be imbued with the values that had dominated her own life. My friend's gentle, affable, non-combative nature was tacitly classified as "insignificant", and became the object of mocking disdain. It's only now, taking the trouble for the first time to examine what that relationship had been, that I'm discovering

the full extent of this forced isolation from the warm sympathy of others, which had marked her for so long.

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My friend Terry, no more combative or forceful than this other friend, had ☐ l'heure, lui, d'être agréé by my mother and was not the object of her mockery - and I suspect that this is why my relationship with Terry was able to blossom without inner resistance within me. His investment in mathematics was no more fervent, nor were his "gifts" any more promising, without my using this as an excuse to cut myself off from him and his wife by this shell of disdain and smugness!

What still remains incomprehensible to me in this other relationship is that my friend's affectionate friendship was never discouraged by the reticence he could not fail to sense in me, with each new encounter. And yet, today I know that I was **something other** than that shell and that disdain, something other than a cerebral muscle and a fatuity that drew vanity from it. As in them, there was the child in me - the child I ignored, the object of disdain. I had cut myself off from him, and yet he lived somewhere inside me, healthy and vigorous as the day I was born. The affection of my friends, less cut off from their roots than I was, surely went to the child. And it was surely he, too, who responded in secret, on the sly, when the Big Chief's back was turned.

## 7.4. (19) The world without love

As for this relationship with these truly enduring friends, it seems to me that I've put my finger on the most blatant, the most grotesque case in my life of the effects of a certain fatuity (among other things) in a personal relationship. Maybe I'm just fooling myself again, but I think it's also the only case where my relationship with a colleague or friend in the mathematical world (or indeed anywhere else) has been permanently invested by fatuity, instead of just appearing occasionally, discreetly and fleetingly. In fact, it seems to me that among the many friends I had in the mathematical world at the time, and whom I liked to keep company with, there was not one for whom I could imagine they had experienced a similar misguidance, in a relationship with a colleague, friend or not. Of all my friends, I was perhaps the least "cool", the most "polite", the least inclined to show a hint of humor (it only came to me later), the most inclined to take myself terribly seriously. In fact, I probably wouldn't have sought out the company of people like me (assuming there ever were any)!

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☐ The amazing thing was that my friends, "swamp" or not "swamp," put up with me and even took affection. It's a good and important thing to say here - even though we often saw each other only to discuss maths for hours and days on end : affection flowed, as it still does today, between the friends of the time (according to sometimes fortuitous affinities) and me, from that first moment when I was received with affection in Nancy, in 1949, in the home of Laurent and Hélène Schwartz (where I was somewhat part of the family), that of Dieudonné, that of Godement (which at one time I also haunted regularly



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This affectionate warmth that surrounded my first steps in the mathematical world, and which I have tended to forget, has been important throughout my life as a mathematician. It must surely have given a similar warm tone to my relationship with the environment that my elders embodied for me. It gave all its strength to my identification with this environment, and all its meaning to the name "mathematical community".

Clearly, for many young mathematicians today, it's being cut off during their apprenticeship, and often well beyond, from any current of affection or warmth; seeing their work reflected in the eyes of a distant boss and his parsimonious comments, rather as if they were reading a circular from the Ministry of Research and Industry, that clips the wings of work and robs it of any deeper meaning than that of a dull, uncertain livelihood.

But I'm anticipating this disgrace, perhaps the most profound of all, in the mathematical world of the 70s and 80s - the mathematical world in which those who were my students, and the students of my friends of yesteryear, set the tone. A world where, often, the boss assigns his subject of work to the student, as one throws a bone to a dog - that or nothing! Like assigning a cell to a prisoner: this is where you will purge your solitude! Where such meticulous, solid work, the fruit of years of patient effort, is dismissed with the smiling contempt of the all-knowing, all-powerful: "this work doesn't amuse me!" and the matter is closed. Good for the garbage can, let's say no more about it. . .

Such disgraces, I know, did not exist in the milieu I knew, among the friends I haunted, in the fifties and sixties. It's true that, in 1970, I learned that it was rather the daily bread □ in the scientific world outside maths - and even in maths it wasn't so

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apparently rare, open contempt, blatant abuse of power (and no recourse), even among some renowned colleagues whom I had had the opportunity to meet. But in the circle of friends I had naively taken to be "the" mathematical world, or at least a faithful miniature expression of it, I knew nothing of the sort.

And yet, the seeds of contempt must already have been there, sown by my friends and me, and sprouted in our pupils. And not only in our pupils, but also in some of my former companions and friends. But my role is not to denounce or even to fight: you can't fight corruption. To see it in such and such of my students whom I once loved, or in such and such of the companions of yesteryear, something in me tightens - and rather than accept the knowledge that a pain brings me, I often refuse the pain and struggle and take refuge in refusal and a fighting attitude: such a thing has no place! And yet it is - and I even know deep down what it means. In more ways than one, I'm no stranger to it, if one of my former students or companions, whom I've loved, likes to discreetly crush another whom I love and in whom he recognizes me.

Once again I digress, doubly so I might say - as if the wind of contempt only blew around my home! Yet it's by blowing on me above all, and on those who are close and dear to me, that I am touched by it and know it. But the time is not ripe to talk about it, except to myself alone, in silence. Instead, it's time for me to resume the thread of my reflection-testimony, which could well be called "In pursuit of contempt" - contempt in myself and around me, in the mathematical milieu that was mine in the fifties and sixties.

## 7.5. (20) A world without conflict ?

I'd thought I'd mention the "marsh" in a few lines, just to say that it was there but that I didn't frequent it - and as is so often the case in meditation (and also in mathematical work), the "nothing" we look at turns out to be rich in life and mystery, and in hitherto neglected knowledge. Like that other "nothing", which also happened to be located in Nancy (decidedly the cradle of my new identity!), the "nothing" of that student who was surely a bit of a loser, who was being treated like... I thought about it again

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in a flash earlier, when I wrote (perhaps a little hastily?) that "these disgraces" didn't yet exist "back home". Let's just say that this is the only□and only incident of its kind that I can report, which resembles (we must to the "disgrace" to which I alluded, without dwelling too much on a detailed description. Those who have experienced it know what I mean, without having to draw a picture. And also those who, without having experienced it, are not so quick to close their eyes whenever they are confronted with it. As for the others, those who despise to their heart's content as well as those who are content to close their eyes (as I myself successfully did for twenty years), even an album of drawings would be a wasted effort... .

It remains for me to examine my personal and professional relationships with my colleagues and students over these two decades, and incidentally also what I may have known about the relationships of my closest colleagues with each other, and with their students. The thing that strikes me most today is how **conflict** seems to **have been absent from all these relationships**. I should add at once that this is something that in those days seemed quite natural to me - like the least of things. Conflict, between people of good will, mentally and spiritually mature and all that (the least of things, again!), had no place. Where there was conflict, I looked on it as a kind of unfortunate misunderstanding: with the right amount of goodwill and explanation, it could only be resolved as quickly as possible and without a trace! If I chose mathematics as my preferred activity from an early age, it's surely because I felt that this was the path where my vision of the world had the best chance of not coming up against disturbing denials at every step. Once you've **demonstrated** something, after all, everyone agrees, i.e. people of good will and all that.

As it happens, I was right. And the story of those two decades spent in the quiet, "conflict-free" (?) world of my beloved "mathematical community", is also the story of a long inner stagnation within me, eyes and ears plugged, learning nothing except maths or little else - while in my private life (first in the relationship between my mother and me, then in the family I founded immediately after her death) a silent destruction was rampant that at no time during those years did I dare

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look. But that's another story. ... The "awakening" of 1970, of which I have spoken often in these lines, was a turning point not only in my life as a mathematician□and a radical change of milieu, but a turning point (give or take a year) in my family life. It was also the year when, for the first time, in contact with my new friends, I risked an occasional glance, still quite furtive, at the conflict in my life. It was the moment when a doubt began to grow in me, which matured over the years that followed, that the conflict in my life, and the conflict I sometimes feared in the lives of others, was not just a misunderstanding, a "burr" that could be wiped away with a sponge.

This (at least relative) absence of conflict, in the environment I had chosen as my own, seems to me in retrospect a rather remarkable thing, when I've come to learn that conflict rages wherever humans live, in families as much as in workplaces, be they factories, laboratories or teachers' or assistants' offices. It's almost as if I'd stumbled, in September or October 1948, unsuspectingly landing in Paris on the only heavenly island in the Universe where people live without conflict with each other!

All of a sudden, after everything I've learned since 1970, it seems really extraordinary. Surely it deserves a closer look - is it myth, or reality? I can see the affection that flowed between so many of my friends and me, and later between students and me, I don't have to invent it - but it almost seems as if I'm obliged to invent conflict, in this heavenly world from which conflict seems banished!

It's true, in the course of this reflection I've had the opportunity to touch on two situations of conflict, each revealing an inner attitude within me: One is the "l'élève nul" incident in Nancy, the ins and outs of which I don't know between the direct protagonists. The other is a situation of conflict within myself, a division, in my relationship with the "indefatigable friend" - but this never expressed itself as a conflict between people, the only form of conflict generally recognized. Remarkably, in the conventional sense of the word, the relationship between these friends and myself was entirely free of conflict - at no time was there the slightest cloud. The division was in me, not in them.

I continue the census. One of the first thoughts: the Bourbaki group! During the years I was participate more or less regularly, so until the late fifties, this group embodied for me, the ideal of a collective work respecting both the minute appa  rence of detail in this work p. 46

The Bourbaki Group is a group of people who share the same values, not of themselves, but of the freedom of each and every one of its members. At no time did I sense among my friends in the Bourbaki group any hint of coercion, either on myself or on anyone else, veteran member or guest, who had come on a trial basis to see if things would "click" between him and the group. At no time was there any hint of a struggle for influence, be it over differences of opinion on this or that agenda item, or a rivalry for hegemony over the group. The group functioned without a leader, and apparently no one in their right mind, as far as I could see, aspired to that role. Of course, as in any group, one member exerted a greater influence on the group, or on other members, than another. Weil played a special role in this respect, which I've already mentioned. When he was present, he was a bit of a "playmaker"<sup>2</sup> (14). Twice, I think, my sensibilities were offended, and I left - these are the only signs of "conflict" of which I am aware. Gradually, Serre exerted an ascendancy over the group comparable to that of Weil. While I was a member of Bourbaki, this did not give rise to any rivalry between the two men, nor was I aware of any enmity developing between them later on. With the benefit of twenty-five years' hindsight, Bourbaki, as I knew him in the 1950s, still seems to me a remarkable example of success in terms of the quality of relationships within a group formed around a common project. This quality of the group strikes me as even rarer than the quality of the books that came out of it. It was one of the many privileges of my life, full of privileges, to have met Bourbaki, and to have been part of it for a few years. If I didn't stay, it wasn't because of conflicts or because the quality I mentioned had deteriorated, but because more personal tasks attracted me even more strongly, and I devoted all my energy to them. Nor did this departure cast a shadow over my relationship with the group or any of its members.

I'd have to review the conflict situations I've been involved in, which have pitted me against

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<sup>2</sup>(14)

You'd think this would contradict the assertion that there was no leader, but it's not so. For Bourbaki alumni, it seems to me that Weil was perceived as the soul of the group, but never as a "leader". When he was there and when he liked it, he became the "ringleader" as I said, but he didn't lay down the law. When he was in a bad mood, he could block discussion on a subject he disliked, even if it meant taking up the subject again at another congress when Weil wasn't there, or even the next day when he was no longer obstructing. Decisions were taken unanimously by the members present, given that it was by no means out of the question (nor even rare) for one person to be in the right against the unanimity of all the others. This may seem an aberrant principle for group work. The extraordinary thing is that it actually worked!

p. 47 to one of my colleagues or students, between 1948 and 1970. The only thing that stands out at all are the two passing quarrels with Weil, which have already been mentioned. A few passing shadows, my relations with Serre, because of my susceptibility to a certain casualness, sometimes disconcerting way in which he would cut things short when a conversation had finished interesting him, or express his lack of interest in, or even aversion to, a particular piece of work I was involved in, or a particular view of things I insisted on, perhaps a little too much and too often! It never got to the point of a falling out. Temperamental differences aside, our mathematical affinities were particularly strong, and he must have felt as I did that we complemented each other.

The only other mathematician with whom I felt a comparable or even stronger affinity was Deligne. In this respect, I remember that the question of Deligne's appointment to the IHES in 1969 gave rise to tensions, which I didn't perceive as a "conflict" (which would have been expressed by a quarrel, or by a turning point in a relationship between colleagues).

It seems to me that I've come full circle - that in terms of conflict between people, visible through tangible manifestations, in relations between colleagues or between colleagues and students in the environment I haunted, that's all during those twenty-two years, incredible as it may seem. In other words, no conflict in the paradise I'd chosen - so, are we to believe, no contempt? Another contradiction in mathematics?

I'll definitely have to take a closer look!

## 7.6. (21) A well-kept secret

p. 48 I'm sure I forgot a few minor episodes yesterday, such as a temporary "chill" in my relationship with a colleague, due in particular to my susceptibility. I should also add three or four occasions when my self-esteem was disappointed, when colleagues and friends failed to remember, in one of their publications, that an idea or result I had shared with them must have played a role in their work (so it seemed to me). The fact that I still remember it shows that it was a sensitive point, and one that perhaps hasn't entirely disappeared with age! Except on one occasion, I refrained from mentioning it to those concerned, whose good faith was certainly above suspicion. The opposite situation must surely have occurred as well, without that I receive any echo of it. I am not aware of any case, in my "microcosm", where a question of priority is the occasion of a brouille or enmity, or even of bitter-sweet remarks between the interested parties. Even so, the only time I've ever had such a discussion (in what seemed to me an egregious case) there was a spat of sorts, which cleared the atmosphere without leaving a residue of resentment. It concerned a particularly brilliant colleague, whose abilities included the ability to assimilate everything he heard with impressive speed, and it seems to me that he often had an unfortunate tendency to take as his own the ideas of others that he had just learned from their mouths.

There's a difficulty here that all mathematicians (and not just mathematicians) have to deal with to a greater or lesser degree, and it's not just due to the egotistical drive that pushes most of us (and I'm no exception) to attribute "merits" to ourselves, whether real or supposed. The understanding of a situation (mathematical or otherwise), however we achieve it, with or without the assistance of others, is in itself something of a personal essence, a personal experience whose fruit is a vision, also necessarily personal. A vision can sometimes be communicated, but the vision communicated is different from the initial vision. This being the case, we need to be extremely vigilant when it comes to identifying the part played by others in shaping our vision. I'm sure I didn't always have this vigilance, which was the least of my worries, even though I expected it of others in relation to my own vision.

screw me! Mike Artin was the first and only person to suggest to me, with the joking air of someone revealing an open secret, that it was both impossible and perfectly pointless to bother trying to discern which part was "one's own" and which was "someone else's", when you manage to take a substance head-on and make sense of it. I was a little taken aback by this, even though it was not at all part of the deontology that had been taught to me by example by Cartan, Dieudonné, Schwartz and others. Yet I had a vague feeling that there was a truth in his words, and just as much in his laughing gaze, that had eluded me until then<sup>3</sup>. My relationship with mathematics (and above all, with mathematical production) was heavily invested by the ego, and this was not the case with Mike. He really gave the impression of doing math like a kid having fun, without forgetting to eat and drink.

## 7.7. (22) Bourbaki, or my great luck - and its downside

Even before plunging a little further below the visible surface, there is a realization that is self-evident. The **mathematical milieu I inhabited for two decades, in the 50s and 60s, was indeed a "world without conflict"**, so to speak! That's a pretty extraordinary thing in itself, and one that deserves a few words of reflection.

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I should make it clear right away that this was a very restricted milieu, the central part of my mathematical microcosm, limited to my immediate "environment" - the twenty or so colleagues and friends I met regularly, and to whom I was most closely linked. Reviewing them, I was struck by the fact that more than half of these colleagues were active Bourbaki members. Clearly, **the core and soul of this microcosm was Bourbaki**. It was, pretty much, Bourbaki and the mathematicians closest to Bourbaki. In the '60s, I was no longer part of the group myself, but my relationship with some of the members remained as close as ever, notably with Dieudonné, Serre, Tate, Lang and Cartier. I continued to be a regular at the Séminaire Bourbaki, or rather, I became one at that time, and it was there that I gave most of my talks (on schema theory).

It was undoubtedly in the sixties that the "tone" in the Bourbaki group shifted towards an increasingly pronounced elitism, of which I was certainly a part at the time, and which for that reason I was unlikely to notice. I still remember how astonished I was, in 1970, to discover how unpopular the very name of Bourbaki had become in large sections (of me hitherto unaware) of the mathematical world, as a synonym more or less for elitism, narrow dogmatism, the cult of "canonical" form at the expense of living understanding, hermeticism, castrating antispontaneity and so on! And it wasn't just in the "swamp" that Bourbaki got a bad press: in the sixties, and perhaps even earlier, I'd heard occasional echoes of it from mathematicians with a different turn of mind, allergic to the "Bourbaki style"<sup>4</sup> (15). As an unconditional adherent, I had been surprised

<sup>3</sup>(September 30) For another aspect of things, however, see the note of June 1 (three months after the present text), "Ambiguity" (n° 63"), examining the pitfalls of a certain complacency with oneself and others.

<sup>4</sup>(15)

I didn't get the impression that this "allergy" to the Bourbaki style gave rise to any communication difficulties between these mathematicians and myself or other Bourbaki members or sympathizers, as would have been the case if the spirit of the group had been that of a chapel, of an elite within the elite. Above and beyond styles and fashions, there was in all members of the group a keen sense of mathematical substance, wherever it came from. It was only in the sixties that I remember one of my friends referring to mathematicians whose work he wasn't interested in as "pain in the ass". When it came to things about which I otherwise knew virtually nothing, I tended to take such assessments at face value, impressed by such casual assurance - until one day I discovered that such a "pain in the ass" was an original and profound mind, which had not pleased my brilliant friend. It seems to me that among certain Bourbaki members, an attitude of modesty (or at least reserve) towards the work of others, when one ignores that work or understands it imperfectly, has eroded.

p. 50 and a little pained - I thought mathematics made minds agree! Yet I should have remembered that when I first started out, it wasn't always easy or inspiring to swallow a text□Bourbaki, even if it was expedient. The canonical text hardly gave an idea of the mood in which it was written, to say the least. It now seems to me that this is precisely the main flaw in the Bourbaki texts - that not even the occasional smile can give any hint that these texts were written by **people**, and people bound by anything other than some oath of unconditional loyalty to ruthless canons of rigor... .

But the question of the slide towards elitism, like that of Bourbaki's writing style, is a digression here. What strikes me here is that the "Bourbakian microcosm" I had chosen as my professional milieu **was a world without conflict**. This seems all the more remarkable given that the protagonists in this milieu each had a strong mathematical personality, and many are considered to be "great mathematicians", each of whom undoubtedly had the weight to form his own microcosm, of which he would have been the center and undisputed leader!<sup>5</sup> (16) It's the cordial and even affectionate conviviality, over two decades, of these strong personalities in the same microcosm and in the same working group, that strikes me as so remarkable, perhaps unique. This ties in with the impression of "exceptional success" that was already expressed yesterday about Bourbaki.

In the end, it would seem that I was exceptionally fortunate, when I first came into contact with the mathematical world, to stumble upon **the** privileged place, in time and space, where a mathematical milieu of exceptional quality, perhaps unique in its own right, had been forming for some years. This environment became mine, and has remained for me the embodiment of an ideal "mathematical community", which probably did not exist at that time (beyond the environment that for me embodied it) any more than at any other time in the history of mathematics, except perhaps in a few equally restricted groups (such as the one that had formed around Pythagoras in a quite different spirit).

p. 51 My identification with this milieu was very strong, and inseparable from my new identity as a mathematician, born in the late forties. It was the first group, beyond the family group, where I was welcomed with open arms. warmth, and accepted as one of their own. Another link, of a different kind: my own approach to mathematics was confirmed in that of the group, and in those of the members of my new environment. It wasn't identical to the "Bourbachic" approach, but it was clear that the two were brothers.

This environment, moreover, must have represented for me that ideal place (or very close to it!), that **conflict-free place** whose quest had undoubtedly led me to mathematics, the science above all others, where any hint of conflict seemed to me to be absent! And although I spoke earlier of my "exceptional good fortune", it was clear to me that this good fortune had its downside. While it enabled me to develop my skills, and to show my worth as a mathematician in the midst of my elders who became my peers, it was also the welcome means of escape from conflict in my own life, and of a long spiritual stagnation.

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first of all, when there was still that "mathematical instinct" that makes you sense a rich substance or a solid work, without having to refer to a reputation or a renown. From the echoes that reach me here and there, it seems to me that both modesty and instinct have become rare things today in what used to be my mathematical milieu.

<sup>5</sup>(16)

In fact, many of the Bourbaki members surely had their own microcosm "of their own", more or less extensive, apart from or beyond the Bourbakian microcosm. But perhaps it's no coincidence that, in my own case, such a microcosm only formed around me after I had ceased to be part of Bourbaki, and all my energy had been invested in tasks that were personal to me.

## 7.8. (23) De Profundis

This "bourbachic" environment has certainly exerted a strong influence on me as a person, and on my vision of the world and my place in it. This is not the place to try and pinpoint this influence, and how it has expressed itself in my life. I'll just say that it doesn't seem to me that my inclinations towards fatuity, and their meritocratizing rationalizations, were in any way stimulated by my contact with Bourbaki and my insertion into the "Bourbach milieu" - at least not in the late forties and fifties. The seeds had long been sown within me, and would have found room to develop in any other milieu. The incident of the "null pupil" that I have reported is in no way typical, quite the contrary, of an atmosphere that would have prevailed in this environment, I repeat, but only of an ambiguous attitude in my own person. The atmosphere in Bourbaki was one of respect for the individual, an atmosphere of freedom - at least that's how I felt; and it was such as to discourage and attenuate any inclination towards attitudes of domination or fatuity, whether individual or collective.

This medium of exceptional quality is no more. He died, I don't know when, without anyone knowing, I suppose there must have been an insensible degradation in people. I suppose an insensitive degradation must have taken place in people - we've all had to "bottle up", grow stale. We've become important people, listened to, powerful, feared, sought after. ☐ The spark perhaps was still there, but the innocence

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lost along the way. Some of us may find it again before we die, like a new birth - but the environment that welcomed me is no more, and it would be pointless for me to expect it to resurrect. Everything is back to normal.

And respect, too, may have been lost along the way. By the time we had pupils, it was perhaps too late for the best to be passed on - there was still a spark, but no longer the innocence, nor the respect, except for "his peers" and "his people".

The wind can pick up and blow and burn - we're sheltered behind thick walls, each of us with "our own".

Everything's back to normal. ...

## 7.9. (24) Mes adieux, or: les étrangers

This retrospective of my life as a mathematician takes a completely different path than I had anticipated. To tell the truth, I wasn't even thinking of a retrospective, but only of saying in a few lines, or even a page or two, what my relationship to the world I had left was today, and perhaps, conversely, what my former friends' relationship to me was, according to the echoes that reach me from far and wide. I had intended, on the other hand, to take a closer look at the sometimes strange vicissitudes of some of the ideas and notions I had introduced in those years of intense mathematical work - I should say, rather: the new types of objects and structures I had the privilege of glimpsing and drawing out of the night of the totally unknown into the penumbra, and sometimes even into the clearest light of day! This statement now seems to stand out in what has become a meditation on a past, in an effort to better understand and assume a certain, sometimes confusing, present. Decidedly, the planned reflection on a certain "school" of geometry, which was formed at my instigation, and which vanished without (almost) leaving a trace, will have to wait for a more propitious occasion<sup>6</sup>. For the time being, therefore, my concern will be to bring this retrospective on

<sup>6</sup>This "more propitious occasion" appeared sooner than expected, and the reflection in question is the subject of the second part, "The Burial", of Harvest and Sowing.

my life as a mathematician in the world of mathematicians, not to epilogize on a work and its fate.

During the past five days, occupied by tasks other than these reflective notes, one memory came back to me with a certain insistence. It will serve as an epilogue to the De Profundis on which I'm working.

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had stopped.

It happened towards the end of 1977. A few weeks earlier, I had been summoned to appear before the Montpellier Criminal Court for the offence of having "gratuitously housed and fed a foreigner in an irregular situation" (i.e. a foreigner whose residence papers in France were not in order). It was on the occasion of this quote that I learned of the existence of this incredible paragraph of the 1945 ordinance governing the status of foreigners in France, a paragraph which forbids any French person to provide assistance in any form whatsoever to a foreigner "in an irregular situation". This law, which had no analog even in Hitler's Germany with regard to Jews, had apparently never been applied in its literal sense. By a very strange "coincidence", I had the honor of being taken as the first guinea pig for the first enforcement of this unique paragraph.

For a few days I was stunned, paralyzed, deeply discouraged. Suddenly, I felt like I'd gone back thirty-five years, to a time when life didn't carry much weight, especially for foreigners... . Then I reacted, I shook myself. For a few months, I invested all my energy in trying to mobilize public opinion, first at my university and in Montpellier, and then at national level. It was during this period of intense activity, for a cause that later proved to be lost in advance, that the episode that I could now call **my farewell took place**.

With a view to taking action on a national level, I had written to five "personalities" from the scientific world, particularly well-known (including a mathematician), to inform them of this law, which even today still seems as incredible as the day I was quoted. In my letter, I proposed a joint action to demonstrate our opposition to this scurrilous law, which was tantamount to outlawing hundreds of thousands of foreigners living in France, and singling out millions of other foreigners for public suspicion, like lepers, who would then become suspects, likely to bring the worst trouble to any Frenchman who wasn't on his guard.

□ Astonishingly, completely unexpectedly for me, I received no response from any of these five "personalities. Decidedly, I had things to learn. ...

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It was then that I decided to go to Paris for the Séminaire Bourbaki, where I was sure to meet up with many old friends, in order first of all to mobilize opinion in the mathematical community, with which I was most familiar. This milieu, it seemed to me, would be particularly sensitive to the cause of foreigners, since all my fellow mathematicians, like myself, have to deal on a daily basis with foreign colleagues, pupils and students, most if not all of whom have had difficulties with their residency papers, and have had to face arbitrariness and often contempt in the corridors and offices of police prefectures. Laurent Schwartz, whom I had informed of my project, told me that I would be given the floor at the end of the presentations on the first day of the Seminar, to explain the situation to the colleagues present.

And so it was that I arrived that day with a bulky packet of leaflets in my suitcase for my colleagues. Alain Lascoux helped me distribute them in the corridor of the Institut Henri Poincaré, before the first session and during the "entr'acte" between the two lectures. If I remember correctly, he even made a little leaflet of his own - he was one of the two or three colleagues who heard about the affair,



were moved and contacted me before my trip to Paris, to offer their help<sup>7</sup> (17). Roger Godement was also one of them, and even produced a leaflet entitled "A Nobel Prize in Prison? It was chic of him, but we were definitely not on the same wavelength: as if the scandal was to attack a "Nobel Prize winner", rather than the first lampoonist who came along!

The first day of the Bourbaki Seminar was indeed crowded, with many people I had known more or less closely, including Bourbaki's old friends and companions; I think most of them were there. Many of my former students too. It must have been ten years since I'd seen all these people, and I was glad to have this opportunity to see them again, even if it meant seeing a lot of them at once! But we'd end up meeting up again in smaller numbers...

The reunion though "wasn't that", it was pretty clear from the début. Many outstretched hands p. 55 and tight-lipped, to be sure, and plenty of "Hey, you there, what wind brings you?" questions, yes - but there was an air of indefinable awkwardness behind the cheerful tones. Was it because they weren't interested in the cause that brought me, when they'd come for a certain tri-annual mathematical ceremony that demanded their full attention? Or, irrespective of what brought me here, was it my very person who inspired this discomfort, rather like the discomfort that a defrocked priest would inspire among a group of well-to-do seminarians? I couldn't say - perhaps it was both. For my part, I couldn't help noticing the transformation that had taken place in certain faces that had once been familiar, even friendly. They had frozen, as it were, or slumped. A mobility I had known there seemed gone, as if it had never been. I found myself standing before strangers, as if nothing had ever linked me to them. Obscurely, I sensed that we didn't live in the same world. I had thought I'd found brothers on this exceptional occasion, and here I was before strangers. Admittedly well-mannered, I don't recall any bitter-sweet comments, nor any leaflets lying around on the floor. In fact, all (or almost all) the leaflets handed out must have been read, curiosity permitting.

That's not to say that the scurrilous law has been jeopardized! I had my five minutes, maybe even ten, to talk about the situation of those who for me were brothers, called "foreigners". The amphitheatre was packed with colleagues, quieter than if I'd been giving a mathematical lecture. Perhaps the conviction to speak to them was already gone. There was no longer, as there once was, a current of sympathy and interest. There must have been some people in a hurry, so I thought I'd cut it short, proposing that we meet again immediately, with colleagues who felt concerned, to discuss in greater detail what could be done... .

When the session was declared adjourned, there was a general stampede for the exits - clearly, everyone was on their way out.

had a train or metro that was about to leave, and was not to be missed at any price! In the space of a minute or two, the Hermite amphitheatre was empty - it was like a miracle! Three of us in the large, deserted amphitheatre, under the cruise lights. Three of us, including Alain and me. I didn't know the p.

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third, one of those unmentionable foreigners again I bet, in dubious company and illegal to boot! We didn't take the time to dwell on the quite eloquent scene that had just unfolded before us. Perhaps I was the only one who couldn't believe my eyes, and my two friends were kind enough to refrain from commenting on the matter. Clearly, I had just arrived...

The evening ended at Alain's and his ex-wife Jacqueline's home, where we took stock of the situation and went on to discuss

<sup>7</sup>(17)

Above all, it was outside the scientific community that I encountered warm echoes of the action to which I had committed myself, and active help. In addition to the friendly support of Alain Lascoux and Roger Godement, I must mention here above all that of Jean Dieudonné, who came to Montpellier for the Correctionnelle hearing, to add his warm testimony to others in favor of a lost cause.

## 2. A walk through a work or the Child and Mother

to review what could be done; to get to know each other a little better, too. Neither on that day, nor later, did I take the time to situate the episode I had just lived through in relation to a past. It was on that day, however, that I came to understand without words that a certain milieu, a certain world I had known and loved, was no more, that a living warmth I had thought I'd rediscovered had long since dissipated.

That hasn't stopped the echoes that still reach me, year after year, from that world whose warmth has fled, from disconcerting me and touching me painfully. I doubt that this reflection will change anything for the future - except, perhaps, that I'll rebel less at being touched in this way... .

# 8. Master and students

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## 8.1. (25) The student and the program

I haven't finished reviewing my relationships with other mathematicians, at a time when I felt part of the same world, the same "mathematical community". Above all, I'd like to examine my relationships with my students, as I experienced them, and with others for whom I was the elder statesman.

Generally speaking, I think I can say without reservation that my relationship with my students has been one of respect. In this respect at least, I believe that what I received from my elders when I was a pupil myself did not deteriorate over the years. As I had a reputation for doing "difficult" maths (admittedly a very subjective notion!), and moreover for being more demanding than other bosses (something already less subjective), the students who came to me were from the outset quite strongly motivated: "they wanted it"! There was just one student who at first was a bit "ollé ollé", it wasn't really clear whether he was going to start - and then yes, it went off without me having to push. ...

□As far as I can remember, I accepted every student who asked to work with me. For

For two of them, it became clear after a few weeks or months that my style of working didn't suit them. To tell the truth, it seems to me now that both of these situations were blockages, which I hastily interpreted as signs of an inability to work mathematically. Today, I'd be much more cautious about making such predictions. I had no hesitation in sharing my impressions with the two concerned, advising them not to continue in a career which, it seemed to me, was not suited to their dispositions. In fact, I knew that for one of these two students at least, I had made a mistake - this young researcher went on to make a name for himself in difficult subjects at the frontiers of algebraic geometry and number theory. I don't know whether the other student, a young woman, continued or not after her disappointment with me. It's not impossible that my impression of her abilities, expressed too peremptorily, discouraged her, even though she was perhaps just as capable of doing a good job as anyone else. It seems to me that I had given credit and confidence to these students as to others. On the other hand, I missed

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discernment to sort out what were surely signs of blockage, rather than ineptitude<sup>1</sup> (18).

From the early sixties onwards, over a period of ten years, eleven students worked with me on their doctoral dissertations<sup>2</sup> (19). Having chosen a subject to suit them, they each did their work with gusto, and (as I felt) they identified strongly with their chosen subject. There was one exception, however, in the case of a student who had chosen, perhaps without any real conviction, a subject that "had to be done", but which also had its ungrateful aspects, as it involved a technical fine-tuning, sometimes arduous, even arid, of ideas that had already been acquired, when there were hardly any surprises or suspense left

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<sup>1</sup>(18)

I believe that this lack of discernment was not due to any negligence on my part on those two occasions, but rather to a lack of maturity, an ignorance. It was only some ten years later that I began to pay attention to blocking mechanisms, whether in my own person, in those close to me or in students, and to measure the immense role they play in everyone's life, and not just at school or university. Of course, I regret not having had the discernment of greater maturity on these two occasions, but not for having expressed my impressions clearly, whether well-founded or not. When, in one case, I saw work done without seriousness, naming these things for what they are seems to me to be a necessary and beneficial thing. If, in yet another case, the conclusion I drew was hasty and unfounded, I was not the only one whose responsibility was engaged. The student thus shaken had the choice of either learning from it (which is perhaps what happened the first time), or letting himself be discouraged, and perhaps then changing profession (which isn't necessarily a bad thing either!).

<sup>2</sup>(19) **Jesus and the twelve apostles**

From 1970 to the present day, another student, Yves Ladegaillerie, has prepared and passed a thesis with me. The students of the first period are P. Berthelot, M. Demazure, J. Giraud. Mme M. Hakim, Mme Hoang Xuan Sinh. L. Illusie, P. Jouanolou. M. Raynaud, Mme M. Raynaud, N. Saavedra, J.L. Verdier. (Six of them completed their thesis work after 1970, at a time when my mathematical availability was most limited). Among these students, Michel Raynaud takes a special place, having found for himself the essential questions and notions that are the subject of his thesis work, which he moreover developed entirely independently; my role as "thesis director" properly speaking was therefore limited to reading the finished thesis, constituting the jury and sitting on it.

When I proposed a subject, I was careful to limit myself to those to which I had a sufficiently strong relationship to feel in a position, if need be, to support the student's work. A notable exception was Mme Michèle Raynaud's work on local and global Lefschetz theorems for the fundamental group, formulated in terms of 1-fields on suitable scalar sites. This question seemed (and indeed proved) difficult to me, and I had no idea of how to prove the conjectures I was proposing (which, incidentally, could hardly be doubted). This work continued in the early '70s, and Mme Raynaud (as had previously been the case with her husband) developed a delicate and original method without any assistance from me or anyone else. This excellent work also opens up the question of extending Ms. Raynaud's results to the case of *n*-fields, which seems to me to represent the natural outcome, in the context of schemes, of theorems of the "weak Lefschetz theorem" type. The formulation of the relevant conjecture here (which can hardly be doubted either) does, however, make essential use of the notion of *n*-fields, the pursuit of which is supposed to be the main object of the present work [*This is actually volume 3 of Réflexions Mathématiques, not the present volume 1 Récoltes et Semailles - see Introduction, p.(v).*], as its name "A la Poursuite des Champs" indicates. We'll come back to this in due course,

Another rather special case is that of Mme Sinh, whom I had first met in Hanoi in December 1967, during a month-long lecture-seminar I gave at the evacuated Hanoi University. The following year, I offered her the subject of her thesis. She worked under the particularly difficult conditions of wartime, her contact with me being limited to episodic correspondence. She was able to come to France in 1974/75 (on the occasion of the International Congress of Mathematicians in Vancouver), and complete her thesis in Paris (before a jury chaired by Cartan, and including Schwartz, Deny, Zisman and myself).

I should also mention Pierre Deligne and Carlos Contou-Carrère, both of whom were somewhat of a pupil, the former around 1965-68, the latter around 1974-76. Both obviously had (and still have) uncommon means, which they used in very different ways and with very different fortunes too. Before coming to Bures, Deligne had been a pupil of Tits (in Belgium) - I doubt he was a pupil of anyone in mathematics, in the usual sense of the word. Contou-Carrère had been a pupil of Santalo (in Argentina), and for a while of Thom (more or less). Both had the stature of mathematicians by the time contact was established, except that Contou-Carrère lacked method and craft.

My mathematical role with Deligne was limited to informing him, on the spur of the moment, of the little I knew about algebraic geometry, which he learned as if he'd always known it, and, along the way, to raising questions that were usually answered on the spot or in the days that followed. These are the

in perspective<sup>3</sup> (20). Carried away by the necessities of a vast program for which I needed arms, I must have lacked psychological discernment in proposing this subject, which was surely not suited to this student's particular personality. He, for his part, probably didn't realize what he was getting into! In any case, neither he nor I could see in time that things had got off on the wrong foot, and that it was better to start afresh.

□ Visibly he worked without real conviction, and without parting with an air that was always a little sad, maus- p. 58  
sade. I think I'd already reached the point where I didn't pay too much attention to these things, which (I should have remembered) are the night and day of all research work, and not just research! My role then was limited to being annoyed when the work seemed to be dragging on, and breathing a sigh of relief when it picked up again, and when the planned program was finally "completed".

It was only years after my awakening in 1970, having corresponded with this former pupil (now a teacher, like everyone else in these clement times!), that the idea came to me that something had definitely gone wrong in this case, that perhaps it wasn't a total success. Today, it feels like a failure, despite the "completed program" (by no means botched!), the diploma and the job. And I bear a large part of the responsibility, for having put the needs of a program before those of a person - a person who had entrusted herself to me with confidence. The "respect" that I claimed to have shown my students ("without any reservations") remained superficial here, divorced from what is the true soul of respect: loving attention to the person's needs, at least insofar as their satisfaction depended on me. The need, here, for joy in work, without which it loses its meaning and becomes a constraint.

In the course of this reflection, I've had occasion to speak of a "world without love", and I've been looking for the seeds of that world in myself, which I reject. Here is a major one - and I can't say today how it has arisen in others. This superficial respect, devoid of attention and true love, is also the "respect" I gave my children. With them, I've had the privilege of seeing this seed grow and proliferate. And I've come to understand that there's no point in begrudging the harvest... ...

## 8.2. (26) Rigor and rigor

With the exception of this one pupil, who was certainly no less "gifted" than the others, I can say that relations between my pupils and myself were cordial, often even affectionate. By necessity, all

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Deligne's earliest works that I know of. Those after 1970 (for him as well as for my "official pupils") are known to me only through very scattered and distant echoes [This is in *fact* volume 3 of *Réflexions Mathématiques*, and not the present volume 1 *Récoltes et Semailles* - see *Introduction*, p.(v)].

My role with Contou-Carrère, as he himself says at the beginning of his thesis, was limited to introducing him to the language of schematics. In any case, I've only been remotely involved in the work he's been preparing as a doctoral thesis in recent years, on a highly topical subject that falls outside my remit. Following a number of misadventures in the wide world, Contou-Carrère was recently led, in extremis and (it now seems to me) unwillingly, to call on my services to act as thesis director and jury. (This exposed him to the risk of becoming one of Grothendieck's "post-1970" students, in a conjecture where this can have serious drawbacks... .). I carried out this task to the best of my ability, and this will probably be the last time I do so (at the level of a state doctorate thesis). I am all the happier, in this rather special circumstance, for the friendly assistance of Jean Giraud, who also took a month or two out of his time to do a thorough reading of the voluminous manuscript, of which he made a detailed and warm report.

<sup>3</sup>(20)

It reminds me of the subject Monique Hakim had taken up, which wasn't much more engaging to tell the truth - I wonder how she managed to keep her spirits up! If she was struggling at times, it was certainly not to the point of making her sad or sullen, and the work between us was done in a cordial and relaxed atmosphere.

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learned to be patient with my two main faults as a "boss": my impossible handwriting (although I think everyone eventually learned to decipher it) and, more seriously (and which I didn't realize until much later), my fundamental difficulty in following someone else's thinking, without first translating it into my own images, and rethinking it in my own style. I was much more inclined to communicate to my pupils a certain vision of things that I had imbibed strongly, rather than encouraging them to develop a personal vision, perhaps quite different from my own. This difficulty in relating to my pupils hasn't disappeared yet, but it seems to me that its effects have been attenuated, because I'm aware of this propensity within myself. Perhaps my temperament, innate or acquired, predisposes me more to solitary work, which was mine for the first fifteen years of my mathematical activity (from 1945 to around 1960), than to the role of "master" in contact with pupils whose mathematical vocation and personality are not fully formed<sup>4</sup> (21). It's also true, however, that I've loved teaching ever since I was a child, and that from the 1960s to the present day, the students I've had have played an important role in my life. In other words, my teaching activity, my role as a teacher, has played and continues to play a major role in my life<sup>5</sup> (22).

During this first period of my teaching activity, there was no apparent conflict between any of my students and myself, which would have been expressed even by a temporary "coldness" in our relations. Only once did I find myself obliged to tell a pupil that he wasn't serious about his work and that I wasn't interested in continuing with him if it went on like that. Of course, he knew just as well as I did what he was talking about, he pulled himself together and the incident was closed without a cloud being cast. On another occasion, back in the early seventies, when most of my energy was devoted to the activities of the "Survivre et Vivre" group, a student to whom I had shown (as is my custom) the thesis report I had just written on his work, became angry, judging that certain considerations in the report called into question the quality of his work (which was in no way my intention). This time I rectified the situation without any difficulty. It didn't seem to me, then, that this brief incident could cast a shadow over our relationship,

but I may have been wrong. The relationship between this student and me had been more impersonal than with the other students (apart from the "sad student" I mentioned), a good working relationship without more, without a

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that would have passed between us. I don't think, however, that it was an unconscious lack of benevolence on my part that would have made me include in my report the considerations he judged to be disadvantageous to him, adding "that he wasn't going to let the thing pass" as a fellow student of his had done, who had already done his thesis with me. With this other student, of a sensitive and affectionate nature, I was bound by a relationship

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<sup>4</sup>(21)

Perhaps it would be more accurate to say that, for my temperament, I still lack the necessary **maturity** to fully assume a teaching role. My acquired temperament has long been marked by an excessive predominance of "masculine" (or "yang") traits, and one of the aspects of maturity is precisely a "yin-yang" balance with a "feminine" (or "yin") predominance.

(Added later.) Even more than maturity, I see that it's a certain **generosity** that I've lacked in my teaching life to date - a generosity that expresses itself in a more delicate way than availability of time and energy, and which is more essential. This lack didn't manifest itself visibly (through an accumulation of failed situations, let's say) in my first period of teaching, no doubt mainly because it was compensated for by a strong motivation in the students who chose to come and work with me. In the second period, on the other hand, from 1970 to the present day, it seems to me that this lack of motivation is at least one of the reasons, and in any case the one that involves me most directly, for the overall failure that I observe in my teaching at research level (from the DEA level upwards). On this subject, see "Esquisse d'un programme", par.8, and par.9 "Bilan d'une activité enseignante", where the sense of frustration that this activity has left me with over the last seven or eight years is apparent [*Compare also note (23iv), added later*].

<sup>5</sup>(22)

Not for much longer, perhaps, as I have decided to apply for admission to the Centre National de la Recherche Scientifique, thus putting an end to a teaching career in academia that has become increasingly problematic in recent years.

particularly friendly; if I had included in my report on his thesis the same kind of consideration that had so displeased his fellow student, it was surely not for lack of benevolence! On the other hand, for both of them, as for all my students, I wouldn't have given the go-ahead for a defense if I hadn't been fully satisfied with the work they presented. In fact, none of my students from that period had any difficulty in finding a suitable job quickly after their thesis.

Until 1970: I had virtually unlimited availability to my students<sup>6</sup> (22'). When the time was ripe, and whenever it would be useful, I would spend whole days with one or other of them, if need be, working on questions that hadn't been finalized, or reviewing together the successive stages of their work. As I experienced these work sessions, it doesn't seem to me that I ever played the role of "director" making decisions, but that each time it was a joint research project, where discussions took place on an equal footing, until both were completely satisfied. The student's energy was considerable, though of course not on a par with that of myself, who had more experience and sometimes a more acute sense of smell.

The thing, however, that seems to me to be most essential to the quality of all research, whether intellectual or otherwise, is not at all a question of experience. It's **the requirement of oneself**. It's not a matter of scrupulous conformity to any standards, rigorous or otherwise. It consists of extreme **attention to something**

delicate thing inside ourselves, which escapes all norms and measures. This delicate thing is the absence or presence of an understanding of the thing examined. More precisely, the attention with which I ☐veuxp . 61  
To speak is to pay attention to the **quality of understanding** present at every moment, from the cacophony of a heterogeneous pile of notions and statements (hypothetical or known), to the total satisfaction, the completed harmony of a perfect understanding. The depth of a search, whether its outcome is fragmentary or total understanding, lies in the quality of this attention. Such attention does not appear as the result of a precept to be followed, of a deliberate intention to "goof off", to be attentive - it arises spontaneously, it seems to me, from the passion to know, it is one of the signs that distinguish the drive to know from its egotistical counterfeits. This attentiveness is also sometimes called "**rigor**". It is an inner rigor, independent of the canons of rigor that may prevail at a given moment in a (let's say) given discipline. If in this book I allow myself to take liberties with canons of rigor (which I have taught and which have their *raison d'être* and their usefulness), I don't believe that this more essential rigor is any less in it than in my past publications, in canonical style. And if, despite everything, I've been able to pass on to my students something of greater value than language and know-how, it's undoubtedly this demand, this attention, this rigor - if not in the relationship with others and with oneself (although at this level it was as lacking in me as in anyone else), at least in the ~~mathematical~~ work - that I've been able to pass on to them.<sup>7</sup>

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<sup>6</sup>(22')

Even after 1970, when my interest in maths became sporadic and marginal in my life, I don't think there was an occasion when I recused myself when a student called on me to work with him. I can even say that, apart from two or three cases, the interest of my post-1970 students in the work they were doing was far below my own interest in their subject, even in the periods when I was only interested in maths on the days when I went to university. So the kind of availability I had for my pre-1970 students, and the extreme demand for work that was a principal sign of it, would have made no sense to most of my later students, who did maths without conviction, as if by a continual effort they'd had to make on themselves. ...

<sup>7</sup>(23) **The Child and the Master**

The term "transmit" here doesn't really correspond to the reality of things, which reminds me of a more modest attitude. This rigor is not something that can be transmitted, but at the very most awakened or encouraged, while it is ignored or discouraged from an early age, by the family environment as well as by school and university. As far back as I can remember, this rigor has been present in my quests, those of an intellectual nature at least, and I don't think it was passed on to me by my parents, and even less by masters, at school or among my mathematician elders. It just

(23). It's a modest achievement, but perhaps better than nothing.

### 8.3. (27) la bavure - or twenty years later

Except perhaps in the case of the two students I've mentioned, with whom a working relationship was finally not established, I don't recall any of the other students who came to me asking to work with me, coming with "stage fright" or fear. No doubt they already knew something or other about me, having attended my seminar at the IHES for at least some time. If there was any awkwardness at the start of our relationship, it eventually dissipated, leaving no trace, in the course of the work. I should, however, make two exceptions. One concerns a student who never really got to grips with his work, and who remained monosyllabic even during our work together. Perhaps

so he came at a time when I was becoming less available, and there were no afternoon and day-long sessions of work on parts with him. No, indeed I don't remember

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I don't think we ever had such sessions; I think we used to meet up for an hour or two to see how he was getting on. He must have had the worst time with me!

The other student, on the other hand, worked with me back when I still had complete disponibility for my students. Our relationship was cordial from the start. He's even one of the few students with whom I've established a friendly relationship, the ones I'd sometimes see at their place just as they'd come to mine, a sort of family-to-family relationship. It's true that even in these cases, the relationship always remained on a relatively superficial level, at least as far as I was concerned. On a conscious level, while I was already unaware of much of what was going on at home, under my own roof, I knew almost nothing about the lives of my mathematician friends, students or not, apart from the names of the wife and children (and even then, I sometimes forgot them, without ever being blamed!). Perhaps I represented an extreme case of "polard", but I believe that in the mathematical environment I knew, most if not all relationships, even friendly and affectionate ones, remained at that superficial level where we know very little about each other, except what is perceived at the informal level. This is surely one of the reasons why conflict between people was so rare in this environment, whereas it's clear to me that division existed within most of my colleagues and friends, and within their families, just as much as it did in my own home and everywhere else.

I don't believe that my relationship with this student differed from my relationship with others, nor did I feel at the time that, conversely, his relationship with me differed in any noticeable way from that of other students, particularly those with whom friendly ties were forged. It's only recently that I've come to realize that this must have been a stronger relationship than for most of my other students. The visible manifestations of an unspoken conflict came as an unexpected revelation, almost twenty years after he had been my pupil. Only then did I make the connection with a long-forgotten "little" fact. For a long time, perhaps even for the entire period (from a few

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seems to be one of the attributes of **innocence**, and therefore one of the things we are born with. Very early on, this innocence "sees a lot of green and a lot of black", which means that it is obliged to plunge more or less deeply, and that often there is hardly a trace of it left in the rest of life. In my case, for reasons I haven't yet thought of investigating, a certain innocence has survived at the relatively innocuous level of intellectual curiosity, whereas everywhere else it has plunged deep, unseen and unheard of, just like everyone else. Perhaps the secret, or rather the mystery, of "teaching" in the full sense of the word, lies in reconnecting with this seemingly vanished innocence. But there's no question of rediscovering this contact in the pupil, if it isn't already present or rediscovered in the person of the teacher himself. And what is "transmitted" by the teacher to the pupil is by no means this rigor or innocence (innate in both of them), but a respect, a tacit revaluation of this thing commonly rejected.



years) when we worked together on a more or less regular basis', □ this student had retained a certain "stage fright". This would manifest itself at each meeting, in unmistakable signs. These signs disappeared fairly quickly afterwards, as we worked together. I was, of course, embarrassed by these signs of discomfort, and I sensed that he was more so. We both pretended to ignore the matter, as if it was nothing. Surely the idea of talking about it wouldn't have occurred to either of us, nor would it have occurred to either of us to pay any undue attention to a strange, obviously interesting situation! For him, as for me, this "stage fright" must have felt like a simple "burr", which had no reason to exist. The "burr" reminded us regularly, but each time, it had the good taste of disappearing, leaving us free to get on with the serious business of maths - and at the same time to forget "what didn't belong". I don't remember stopping once to wonder about the meaning of the blunder, and I'm sure the same was true of my pupil and friend. No doubt nothing in what we had both known around us since our early childhood could suggest in him or in me the idea of any other attitude towards a troublesome thing than that of **removing it as far as possible, so** that it ceased to be troublesome. In this case, it was perfectly possible and even easy, and we were perfectly in agreement that we'd seen nothing, felt nothing and heard nothing.

Through the many echoes and cross-references that have come back to me over the last two or three years, I've come to realize that what was dismissed as irrelevant has not ceased to be, and to manifest itself. What sometimes came back to me doesn't "belong" either - and yet "it is", and now can't be dismissed out of hand.

...

## 8.4. (28) The unfinished harvest

Until the first "awakening" in 1970, my relationship with my students, like my relationship with my own work, was a source of satisfaction and joy, one of the tangible, irrefutable foundations of a sense of harmony in my life, which continued to give it meaning, even as elusive destruction raged in my family life. At that time, I saw no apparent element of conflict in my family life.

these relationships, none of which, at any time, even fleetingly, was the cause of frustration or pain.

It is a thing that may seem paradoxical, that the conflict in the relationship □ to such and such of my students has only become

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apparent that after this famous awakening, after a turning point that gave my life an openness it hadn't known before, and my person a little beginning of flexibility perhaps - qualities that, one might think, should be of a nature to resolve or avoid conflict, and not to provoke or exacerbate it.

On closer inspection, however, I can see that the paradox is only apparent, and that it disappears, whichever way you look at it. First of all, for a conflict to have a chance of being resolved, it must first have manifested itself. The stage of manifested conflict represents a maturing compared to that of hidden or ignored conflict, whose manifestations do exist, and are all the more "effective" for the fact that the conflict expressed by them remains ignored. So: for a conflict to manifest itself in a recognizable way, a **distance** must first have been reduced or disappeared. The changes that have taken place in my life over the last fifteen years, notably in the course of successive "awakenings", have all been changes, it seems to me, of a nature to reduce a distance, to erase an isolation. A conflict that has difficulty expressing itself to a prestigious, admired boss, feels more at ease with someone who has been stripped of a position of power (voluntarily, in this case), who has been exiled from a certain milieu holding authority and prestige, who is perceived less and less as an incarnation or privileged representative of some entity (such as mathematics), and more and more as an individual.

like any other: a person who is not only susceptible to harm, but who is also less and less inclined to hide wounds or sorrows. And thirdly and most importantly: my evolution since the first awakening, especially at that time and in the years that followed, was such as to raise (or perhaps awaken) questions, concern and "questioning" in the well-ordered world of my former pupils. I had ample opportunity to realize that this was the case not only for them, but also among my friends and companions in the mathematical world of yesteryear, and sometimes even among scientific colleagues who knew me only by hearsay.

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It has to be said, too, that resolving a conflict of any depth is one of the rarest things in the world. The most often, notwithstanding all the surface truces and reconciliations, the growing procession of our conflicts follows us without  $\square$  guère leaving us for a whole lifetime, only to finally let go between the sullen hands of undertakers. Occasionally, I have seen a conflict unravel somewhat, and sometimes even resolved with knowledge - but so far, no such thing has happened in the course of my relationship with one of my students, or with one of my old friends in the mathematical world. And I also know that it's by no means certain that such a thing will ever happen, even if I were to live another hundred years.

It's remarkable that the very moment of my break with a certain past, by which I mean the episode of my departure from the IHES (from the institution that represented something like the "matrix" of the mathematical microcosm that had formed around me) - that this decisive episode was at the same time the first occasion on which one of my students expressed deep antagonism towards me. It was surely this circumstance that made this episode particularly distressing, particularly painful, like a birth or childbirth that had taken place under particularly difficult conditions. Of course, I couldn't then see this episode, the meaning of which escaped me, in the light in which I have since learned to see it. This painful surprise remained with me for a long time to come. And yet, in the summer of that same year, that bitter departure felt like a liberation - like a door that had suddenly opened wide (all I had to do was push it!) onto an unsuspected world, beckoning me to discover it. And each new awakening since then has also been a new liberation: the discovery of a subjection, an inner fetter, and the rediscovery of the presence of an immense unknown, hidden behind the familiar appearance of what was supposed to be "known". But throughout these fifteen years, and right up to the present day, this stubborn, discreet and unwavering antagonism has followed me, as the one great and lasting source of frustration I have known in my life as a mathematician<sup>8</sup> (23'). Perhaps I could say that it was the price I paid for that first liberation, and for those that followed. But I'm well aware that liberation and inner maturation have nothing to do with a "price to pay", that they're not a question of "profits" and "losses". Or to put it another way: when the harvest is completed, when it's finished, there's no "profit" to be made.

has no loss - the very thing that seemed "loss" has become "profit". And it is becoming clear that I have not yet been able to bring this harvest to its term  $\square$  me, which remains, even as I write these lines, unfinished.

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<sup>8</sup>(23')

For the past seven or eight years, however, there has been another chronic "source of frustration" in my life as a mathematician, but one that has expressed itself much more discreetly over the years. It eventually became apparent as a result of the repetition and obstinate accumulation of the same type of "frustrating" situation in my teaching activity, and finally exploded into a sort of "fed-up", causing me to practically put an end to all so-called "research direction" activities. I touch on this question once or twice in the course of my reflection, and finally examine it at least a little at the end. At the very least, I describe this frustration, and examine the role it played in my "return to maths" (cf. par. 50. "Weight of a past").

## 8.5. (29) The Enemy Father (1)

The kind of students who started working with me after the turn of 1970, in the completely different environment of a provincial university, was also very different from the students before. Only two of them went on to work with me on a state doctorate thesis. The rest worked on DEA or post-graduate doctoral theses. I should also include a good number of students who have taken to certain introductory research "courses", which have given them the opportunity to ask themselves often unexpected mathematical questions, and sometimes to come up with original ways of solving them. I found the most active participation in certain "option courses" for first-year students. For students who have already spent a few years in the university environment, however, a certain freshness, a capacity for interest and personal vision are already more or less extinguished. Many of the students in the elective courses clearly had the makings of an excellent mathematician. Given the current economic climate, I was reluctant to encourage any of them to go down this path, which could have attracted them and where they could have excelled.

With the students who took one of my "courses" to prepare for a master's degree, the relationship didn't usually last beyond the end of the year. On each occasion, I had the impression that they quickly became cordial and relaxed, on the whole. Except in the case of one student afflicted with invasive "stage fright".<sup>9</sup>

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### <sup>9</sup>(23") Fear of playing

This student had worked with me on a DEA "work placement" for a whole year, and remained "contracted" in his working relationship with me right up to the end. It was a frankly friendly relationship, shot through with a mutual sympathy that could not be doubted. Yet there was this "stage fright"; this fear, the real cause of which was surely not fear of me, even though it looked like it. I might not even have noticed it, had this student not told me about it himself, no doubt to "explain" more or less the reason for an almost complete block in his work during the year.

As was the case with other students who, like him, were initially hooked on a certain geometrical substance, the blockage manifested itself as soon as it was a question of doing "work on parts", i.e. putting statements in black and white, or just grasping the meaning and significance of those I was providing and proposing to admit as the foundations of a language, as "rules of the game". School reflexes almost always push the student, faced with a situation where he's supposed to be "doing research", to adopt the implicit "rules of the game", transmitted by the teacher, as a "given" that's both vague and imperative, and which it's certainly not a question of trying to make explicit, let alone understand. The concrete form these implicit rules take are "recipes" for semantics or arithmetic, along the lines of, say, a mole book (or any other common textbook). What's more, the pupil expects the teacher to perform a task of the form "demonstrate that...". (Incidentally, I don't believe that the attitudes of most professional mathematicians, and of other scientists too, are essentially different - except that the "master" is replaced by the "consensus", which fixes the rules of the game of the moment and regards it as an immutable given. This consensus also fixes the "problems" to be solved, between which everyone feels free to choose as they see fit, even allowing themselves to modify them in the course of their work, or even invent new ones. . . ). I've noticed that my entirely different attitude to the mathematical substance I'm trying to fathom, and therefore also to the student, almost certainly triggers disarray, one of the signs of which is anxiety. Like all anxiety, this will tend to take on a face, to project itself onto an external "reason", plausible or not. One of the most common faces of anguish is fear.

Such difficulties hardly arose in the first period of my teaching activity, except perhaps in the two cases where a "teacher-student" relationship didn't continue beyond a few weeks, and perhaps (I can't say) in the case of the "sad pupil", who perhaps felt "riveted" to a subject that didn't inspire him at all, even though he had every opportunity to change it. In the case of the student (whom I also mentioned) who remained afflicted by stage fright for a long time, it's clear that the reason lay elsewhere. He was by no means blocked in his work, but on the contrary perfectly at ease with the theme he had chosen, on which he did a great deal of groundwork. In fact, most of my students during this period were former students of the Ecole Normale, and their contact with Henri Cartan had already shown them the example of another "approach to mathematics". At the opposite end of the spectrum (so to speak), in my second period as a teacher, at the University of Montpellier, it was among first-year students that the anguish I've mentioned least interfered with reflective work. For many of these students, astonishment at a different approach didn't provoke anguish or closure, but rather openness and a willingness to do interesting things for once! From my observations, the effect of a few years in college on a student's creative disposition is radical and devastating. It's a strange thing that, in this respect, the effect of the long years of high school seems relatively innocuous. Perhaps this is because

(23"), it was the same with students who were officially expected to prepare a research paper under my guidance, at one level or another. One difference (among many!) with my previous students was that our relationship wasn't so much confined to joint mathematical work. Often the exchange between the student and me involved our persons in less superficial ways<sup>10</sup> (23v). It's not surprising, then, that in this second period of my teaching activity, the conflicting elements in the relationship with certain students appeared more clearly and directly, even vehemently. Among my former

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first period, there are two in whom attitudes of systematic and unequivocal antagonism subsequently appeared (which I have had occasion to mention in passing), yet remained at the level of the informal, and perhaps even the unconscious. In the second, longer period, there were three students in whom I was confronted with antagonism. In two of them, it manifested itself acutely.

In the case of one of these pupils, antagonism arose overnight in what had been a most friendly relationship, long years after this friend had ceased to be my pupil. I suspect that the cause of the conflict was not so much my unspeakable conduct and personality, as a long-suppressed dissatisfaction at not having found the reception for his work (which had been excellent) that he would have been entitled to expect. This was the downside of the dubious privilege of having had me as his boss "after 1970", and he must have resented it, without really recognizing it even in his own heart.

With the other student, an acute antagonism had already emerged after a year and a half's work, in an atmosphere that had seemed very cordial. This was the first and only time that a relational difficulty between a pupil and myself had arisen at a time when he was still a pupil. It made it impossible for us to continue working together, despite the auspicious start and the auspicious enthusiasm for a magnificent theme. I had the feeling that there was an insidious lack of confidence in this young researcher's ability to do a good job (an ability which, for me, was beyond doubt), and that the manifestation of antagonism was a sort of "headlong rush" to get ahead of a dreaded failure, and blame it on an odious boss<sup>11</sup> (23").

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the university years are at an age when the creativity innate in us **must**, at the end of the day, be expressed through personal work, otherwise we'll be shipwrecked forever, at least in terms of creative work of an intellectual nature. It must have been a healthy instinct that during my student years (also at the University of Montpellier) I practically never set foot in class, devoting almost all my energy to personal mathematical reflection.

<sup>10</sup>(23v)

A particularly striking sign of this difference was the "stranger episode", which I've already mentioned (section 24). While I received expressions of sympathy from many people who were complete strangers to me, I don't recall any of my pre-1970 students thinking of expressing such sympathy, let alone offering to help me in the action I had embarked upon. On the other hand, I can't think of a single one of my students or former students from the second period who didn't express their sympathy and solidarity with me, and several actively joined the campaign I was running at local level. Beyond this restricted circle, the 1945 ordinance affair also created a certain amount of emotion among many students at the Faculty who knew me by name at most, and a good number of them came to the Palais de Justice on the day of my summons, to show their solidarity. This last circumstance suggests, moreover, that the difference I observed between the attitudes of my students "before" and "after" 1970 may express less the difference in **relations** between them and me, than a difference in **mentalities**. Clearly, my "before" pupils had become important people, and it takes a lot for important people to consent to be moved... . But the episode of my departure from the IHES in 1970 and my involvement in militant action seems to show that there's more to it than that. It was a time when none of them was yet such an important figure, and yet I don't remember any of them showing the slightest interest in the activity I was getting involved in. Rather, I think it must have made them uncomfortable, all of them without exception. This again points to a difference in mentality, but one that can't be blamed solely on differences in social status.

<sup>11</sup>(23") **The two brothers**

This student's antagonism took the form, from the outset, of a "class antagonism": I was the "boss" who had "power of life and death" over his mathematical future, which I could decide at my whim... . Of course, events only served to confirm this vision, since it wasn't long before I had to put an end to my responsibilities (which had become painful) to

One aspect common to all these appearances of conflict between students and myself, in the nearly twenty-five years I've been teaching mathematics, is a strong **ambivalence**. In all these cases, without exception, the antagonism manifests itself after the fact, often insidiously, in a relationship of sympathy that can be left in no doubt. I can even say that in all these cases, as in many others where a frankly antagonistic component has not manifested itself, my person has exerted and still exerts a strong attraction. It is surely the very strength of this attraction that also feeds the strength of the antagonism.

and ensures its continuity. It is still so, surely, in cases where antagonism takes the form of a violent antipathy, an outraged rejection; as also in such another case, at the opposite extreme, where under the pavilion of friendly respect is expressed (when the occasion is right) with an affectation of casual, delicately measured disdain... ..

Such situations of ambivalence, to tell the truth, are not peculiar to my relationship with some of my students or ex-students. In fact, they have abounded throughout my adult life, since at least the age of thirty (i.e., since my mother died). This has been true both in my love life and in my relationships with men, and more specifically, with men who are much younger than I am. I've come to understand that something in me, whether innate or acquired, seems to predispose me to act as a father figure. I seem to have the ideal build and the right vibe to make the perfect adoptive father! It has to be said that the role of Father fits me like a glove - as if it had been

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this student. This put him in a tricky situation, in these times when it's not so easy to find a "boss", especially when the subject has already been chosen. For the other student, frustrated in his legitimate expectations, the antagonism took a similar form. I felt like the tyrannical "mandarin", who could not tolerate contradiction from those (students or lower-ranking colleagues) he considered his subordinates.

Such a "class attitude" never manifested itself, if at all, during the relationship with my students of the first period. The obvious reason was that, in the pre-1970 context, there was no doubt that the student, once he had passed his thesis, would have a position as a lecturer, and would therefore enjoy a social status identical to mine, that of "university professor". Loquacious figures: the eleven students who began working with me before 1970 were given lectureships as soon as their work was completed, whereas none of the twenty or so students who worked more or less under my direction had access to such a position. It's true that only two of them were motivated enough to do a state doctorate thesis (an excellent one for both of them, by the way).

It's hardly surprising, then, that in this second period, certain ambivalences (whose deeper origins remained hidden) took the form of class antagonism, of ~~mistrust~~ (presented and felt as "visceral") towards the "boss". For one of those who had more or less been a pupil, friendly relations continued for around ten years without any apparent antagonistic episode, and yet marked by this same ambiguity, expressed in an attitude of ~~mistrust~~, held "in reserve" behind overt sympathy. To tell the truth, I was never fooled by this commanding "distrust", which appeared to me above all as a reason that this friend saw fit to give himself for not venturing outside the well-defined domain he had chosen as his own, in his professional life as in his life in general - something he was free to do, however, without anyone (except, at most, himself!) calling him to account. ...

In fact, these three cases are the only ones in all my teaching experience where a certain ambivalence in the relationship between a student (or someone who is more or less a student) and myself has been expressed by a "class attitude". Such an attitude appears particularly ambiguous when it manifests itself between colleagues within an academic "corps" where they both enjoy exorbitant privileges compared to the situation of ordinary mortals, privileges which make differences in rank (and salary) appear relatively insignificant. In fact, I've noticed that these attitudes disappear as if by magic (and with good reason!), as soon as the person concerned sees himself promoted to the position of which only the day before he was complaining to others.

Indeed, I detect a similar ambiguity in most, if not all, of the conflict situations I've witnessed within the mathematical world (and often outside it too). Those who are "cast", whether or not their rank corresponds to their expectations (justified or not), enjoy quite unheard-of privileges, which no other profession or career can offer. Those who don't fit in aspire to the same security and privileges (which doesn't necessarily prevent them from taking an interest in maths itself, and sometimes from doing great things). These days, when the competition for a place is fierce, and the unhoused are often treated like stragglers, I've more than once felt the connivance between the one who enjoys humiliating and the one who is humiliated - and who swallows and crushes. The real object of his bitterness and animosity is **not** the one who has made use of power, but is none other than **himself**, who has crushed himself and invested the other with this power which he uses at pleasure. The one who takes pleasure in humiliating is also the one who takes his revenge and compensates (without ever erasing it. . . ) for a long-lasting humiliation that has long since been buried and forgotten. And he who acquiesces in his own humiliation is his brother and emulator, who secretly envies it and in bitterness buries both the humiliation, and the humble message about himself that it brings him.

mine by birth. I won't try to count the number of times I've taken on such a role vis-à-vis another person, in perfect tacit agreement on both sides. More often than not, this distribution of father-son or father-daughter roles remained unspoken, even unconscious, but it also happened to be formulated more or less clearly. In some cases, too, I acted as a father without even entering into the game, I think, in ignorance of what was going on, both consciously and unconsciously.

I first became aware of my role as adoptive father in 1972, at the time of "Survivre et Vivre", when I was suddenly confronted with an attitude of violent rejection on the part of a young friend. (Interestingly, he was a maths student on the verge of dropping out!) Something in my behaviour towards third parties had disappointed him. I think I'd have had no difficulty in recognizing that his disappointment was well-founded, that I'd been ungenerous in this instance - but the violence of the reaction literally blew me away. It was like a sudden outburst of vehement hatred, which subsided almost immediately, when it became clear that he hadn't really managed to throw me off. (It was close, but I kept that to myself... ). I don't know how I got the intuition when he was projecting onto my

person, duly idealized, of unresolved conflicts with his father. This sudden intuition, now forgotten, didn't prevent me from penning the role of father for years to come, always with the

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same conviction, without the slightest suspicion. Of course, I was always as painfully astonished, not believing my eyes or the rest of it, when I was later confronted with signs of conflict, insidious or violent.

It was only after six or seven months of intense, solitary work on my parents' lives, which enabled me to see them in an unsuspected light, that I understood the illusory nature of this role of adoptive parent, which would replace (for the better, that's a given!) a real parent who does exist, and who would be declared (if only by tacit agreement) "failing". It's helping someone else to avoid the conflict where it exists, in his relationship with his father, let's say, and projecting it onto a third person (myself, in this case) who is entirely foreign to it. Since this meditation, which took place from August 1979 to March 1980, I've been vigilant about myself, so as not to allow myself to indulge in my unfortunate paternal vocation with my eyes closed. This hasn't prevented the false situation from recurring (as in my relationship with that student with whom I had to stop working) - but now, I believe, without any connivance on my part.

If I set aside the case of the pupil frustrated in these legitimate expectations, there's no doubt in my mind that in all the other cases where I've been confronted with antagonism in a pupil or ex-pupil, it's been the reproduction of the same archetypal conflict with the father: the Father both admired and feared, loved and hated - the Man to be confronted, defeated, supplanted, perhaps humiliated . . . but also the One we secretly wish we were, stripping Him of a strength to make it our own - another Self, feared, hated and shunned. . .

## 8.6. (30) The Enemy Father (2)

It wasn't the great turning point of 1970 that created antagonisms between some ex-students and me, against the backdrop of an idyllic, cloudless past. It only made visible antagonisms that could hardly be expressed within the more conventional framework of a typical boss-student (or ex-boss-ex-student) relationship. I suspect that such conflicts are not uncommon in the scientific world, but that they are often expressed in a more roundabout and less recognizable way than in relationships in

□that I've been involved in.

In retrospect, I don't feel that in my relationships with my students, I've been so inclined to take on a paternal role - in fact, I can't think of a single memory that goes beyond that.

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in this sense more or less. As far as **I'm** concerned, it seems to me that almost all the energy I invested in a relationship with a student was the same energy I also invested in mathematics, and in the realization of a vast program. In the first period, I can think of only one case where I had an interest in a student, in the nature of an affinity or sympathy, which had a strength comparable (if not equal) to that of mathematical interest. But even then, I don't feel that I've taken on a paternal role towards him. As for any ascendancy I may have exercised over him or other students, at one level or another, that's the sort of thing I paid no attention to in my relationship with my pupils. (Even today, I tend not to pay attention to it, either with the students who have worked with me in recent years, or even with other people.) Of course, in all these cases, the relationship between the pupil and myself was by no means "symmetrical", in the sense that for at least the duration of the teacher-pupil relationship (and probably even beyond, more often than not), the importance a pupil had in my life was not comparable to that which I had to take on in his, nor the psychic forces that the relationship brought into play in my person and in his. Except in the five or six cases where these forces manifested themselves in clearly recognized signs of antagonism, I realize that the nature of the relationships to me of my various pupils and then ex-pupils, over more than twenty years of teaching activity, remain a total mystery to me! In fact, it's not really my job to probe these mysteries, but rather that of each of them in his or her own right. But as long as you're interested in your own person, there may be hotter things to look at than the ins and outs of your relationship with your ex-boss... . In any case, even though I showed no inclination towards my pupils to take on a paternal role, it can't have been uncommon for me to have acted as a sort of adopted father for them, given my particular psychic "profile", which I mentioned earlier, and given the dynamics inherent in a situation where I could not fail to act as an elder, to say the least.

□ In any case, in several of the cases I've mentioned, this particular coloring of the relationship between a  
 There's no doubt in my mind that I'm a student. Outside my professional life, there have been many other cases where, with or without my connivance, I have acted as an adopted father to younger men and women, attracted by me and linked to me by mutual sympathy, but by no means by kinship. As for my own children, the paternal fibre in me towards them has been strong, and from an early age they have had an important place in my life. In a strange irony, however, none of my five children accepted the fact that I was their father. In the lives of the four of them that I've come to know closely, especially in recent years, this division in their relationship to me reflects a deep division within themselves; a refusal in particular of everything in them that makes them like me, their father... . But this is not the place to explore the roots of this division, which go back to a childhood torn apart, to my own childhood and that of my parents, as well as to my mother's childhood and that of her parents. Nor is this the place to measure its effects, in their own lives, or in those of their children... .

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## 8.7. (31) The power to discourage

To conclude this summary tour of my relationships in the mathematical world between 1948 and 1970, it remains for me to talk about my relationships with younger mathematicians, more or less beginners and therefore without the status of "colleague" in the strict sense of the word, without my playing the role of "boss" vis-à-vis them. These are young researchers whom I met for a year or two in my seminar at IHES, or on the occasion of such courses or seminars at Harvard or elsewhere, or also sometimes, on the occasion of a "seminar" at the University of Paris.

correspondence, for example when I'd received a piece of work from a young author for which he or she was looking for feedback, and surely also encouragement.

Dealing with novice researchers is part of a role that is less apparent than that of "boss" of such students, but just as important, as I have since come to realize. At the time, I didn't realize, as I have for the last six or seven years, that this role, for a mathematician in the limelight, represents a major challenge.

p. 72 considerable **power**. It is first of all the power **to encourage**, to stimulate, which exists as much in the case of visibly brilliant <sup>tra</sup>□vail (but perhaps served by clumsiness of presentation or a It exists even in the case of work that represents only a very modest contribution, or even negligible or nil by the standards of a senior in full possession of powerful means, proven experience of the subject, and extensive information. The power to encourage is there, as long as the work submitted to us has been written with seriousness - something generally discernible from the very first pages.

And the power to **discourage** exists just as much, and can be exercised at discretion whatever the work. This is the power that Cauchy used against Galois, and Gauss against Jacobi - it's not just that it exists and that eminent and feared men use it! If history has recorded these two cases, it's because the men who had to pay the price had sufficient faith and self-confidence to go their own way, despite the unsympathetic authority of those who were then calling the shots in the mathematical world. Jacobi found a journal to publish his ideas, and Galois the sheets of his last letter, acting as a "diary".

Today, it's certainly more difficult for an unknown or little-known mathematician to make a name for himself than it was in the last century. And the power of the prominent mathematician is not only psychological, but practical too. He has the power to accept or reject a work, i.e. to give or refuse his support for a publication. Rightly or wrongly, it seems to me that "in my day", in the fifties and sixties, refusal was not without appeal - if the work presented "worthy" results, it had a chance of finding the support of another eminence. Today, this is certainly no longer the case, as it has become difficult to find even a single influential mathematician who will agree to review (with whatever willingness he or she may have) a work in his or her field, unless the author has already acquired a reputation, or is recommended to him or her by a well-known colleague.

Over the past few years, I've seen some brilliant and influential mathematicians make use of the of their power to discourage and refuse, both with regard to such solid work that obviously had to be done'□ and with regard to such large-scale works clearly denoting the power and originality of their authors. On several occasions, the person who used his discretionary power in this way happened to be one of my p. 73 former students. This was probably the most bitter experience of my life as a mathematician.

But I'm straying from my point, which was to examine the way in which, in the days when I lent myself with conviction to the role of "mathematician in the limelight", I wielded the power to encourage and discourage that I spoke of. I should add that, at the more modest level where my scientific activity continued after 1970, as a teacher among others at a provincial university, this power did not cease to exist, either vis-à-vis my students or pupils, or (rarely, it's true) vis-à-vis oc- casional correspondents. But for my present purposes, it's the first period of my life as a mathematician that's important.

As far as my relationship with my students is concerned, from the first one I had to this very day, I think I can say without restriction that I've done everything in my power to encourage them.



in the work they had chosen<sup>12</sup> (23iv). It must be rare, even today, for it to be otherwise in the relationship between "boss" and pupil, and especially so in the case of a boss who has the means to train brilliant pupils, and with their help clear vast tracts of land ready for ploughing. Hardly believable, yet true, is the extreme case of the prestigious boss taking pleasure in extinguishing in brilliantly gifted pupils the mathematical passion that had animated him at a younger age.

But I'm digressing again! It's my relationship with the young researchers who weren't my students that needs to be examined. In such relationships, the egotistical forces in the person of the man in the spotlight would be less likely to push him in the direction of encouragement, while the successes of the young stranger approaching him would add little or nothing to his own glory. On the contrary, I believe that the play of egotistical forces alone, in the absence of genuine benevolence, would almost invariably tend to make the young stranger's successes less attractive to his own glory.

to push in the opposite direction, to use the power to discourage, to refuse. This, it seems to me, is neither more nor less than that general law, which can be constructed in all sectors of society: that egotistical desire <sup>dep. 74</sup>

to prove one's own importance, and the secret pleasure that comes with satisfying it are generally stronger and more appreciated, when the power at one's disposal finds occasion to cause one's fellow man discomfort, or even humiliation, rather than the other way round. This law is particularly brutal in certain exceptional contexts, such as war, concentration camps, prisons or psychiatric asylums, or even in the all-purpose hospitals of a country like ours... . But even in the most everyday contexts, each of us has had occasion to be confronted with attitudes and behavior that attest to this law. The correctives to these attitudes are, firstly, **cultural** correctives, stemming from a consensus, in a given environment, on what is considered "normal" or "acceptable" behavior; secondly, they are forces of a non-egotistical nature, such as sympathy towards a particular person, or sometimes, a spontaneous attitude of benevolence independent even of the person to whom it is addressed. Such benevolence is undoubtedly rare in any environment. As for the cultural corrective in mathematics, it seems to me that it has eroded considerably over the past two decades. This is certainly the case, in any case, in the circles I've visited.

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<sup>12</sup>(23iv) Teaching failure (1)

Since these lines were written, I've had the opportunity to speak with two of my ex-students from after 1970, in an attempt to probe with them the reason for the failure of my teaching at research level, at the University of Montpellier. They told me that my tendency to underestimate the difficulty of assimilating techniques familiar to me, but not to them, had had a discouraging effect on them, as they constantly felt that I had fallen short of my expectations of them. What's more (and this seems even more far-reaching to me), they sometimes felt frustrated when I "sold the worm" by giving them a shaped statement I had up my sleeves, instead of letting them discover it for themselves, at a time when they were already very close to it. After that, all they had to do was the "exercise" (which they weren't otherwise keen on) of demonstrating the statement in question. Herein lies the "lack of generosity" in me that I had noted in an earlier note (note 21), without elaborating further. It is disappointments such as these, above all, that represent my personal contribution to the disappearance of interest in research in both of us, after what was nonetheless an excellent start.

I realize that I was no more generous before 1970 than after. If I didn't have the same difficulties then, it's probably because the kind of students who came to me in those days were motivated enough to find even a "long exercise" appealing, as an opportunity to learn the trade and a host of other things along the way; and also, for a starter statement I was "selling the fuse" on, to come up with a host of others of their own that went far beyond the first. When I moved to a new teaching location, I made the necessary adjustment in the choice of topics for reflection that I proposed to my new students, by choosing mathematical objects that could be grasped by immediate intuition, independently of any technical baggage. But this essential adjustment was in itself insufficient, due to differences in **disposition** (in my new pupils compared to those of yesteryear), even more important, than a single difference in **baggage**. This ties in with the observation made earlier (beginning of par.25) about a certain **inadequacy** in me for the role of "master", which came out much more strongly in my second period as a teacher than in the first.

known.

Decidedly, I persist in straying from my point, which was not a discourse on the century, but a meditation on myself and my relationship with the more or less novice researchers who were not my students. I don't believe that the "law" I alluded to found expression in these relationships. For reasons that needn't be examined here, it would seem that egotistic forces, just as strong in me as in anyone else, have not taken this path in my life to manifest themselves at the expense of others (apart from a few cases dating back to my childhood). I think I can even say, having had the opportunity to examine the matter, that the basic tone of my disposition towards others is one of benevolence, a desire to help when I can help, to relieve when I can relieve, to encourage when I am in a position to encourage. Even in a relationship as deeply divided as that with this "indefatigable friend" of mine

p. 75 I've had to talk about, never has the fatuity in me led me astray to the point where I would have thought (even if by unconscious intent) of harming him. (I would have had the possibility of <sup>fai</sup>re, and "with the best conscience in the world" alright. of course). And I believe that, in most cases, this general benevolence (even if only a little on the surface) also marked my relations in the mathematical world, including with beginner mathematicians who, although not among my students, might have needed my support or encouragement.

I believe this was the case without exception, at least during the fifties and into the early sixties. It seems to me that, in those days at least, this benevolence was not limited to visibly brilliant youngsters like Heisuke Hironaka or Mike Artin (even though they were not yet renowned for their abilities). But it may well have faded to a greater or lesser extent during the sixties, under the influence of egotistical forces. I would be particularly grateful for any testimony on this subject.

My memory only recalls a specific case, which I'm going to talk about, and beyond this case, this famous "fog" that doesn't condense into any other specific case or fact, but rather gives me a certain inner attitude. I used to feel a certain irritation when it happened that another mathematician "stepped on my toes" without pretending to ask me anything, as if he was at home the young white boy! It must have been mostly cases of young people, not too up to speed, who thought they'd come across something I'd known for years, and even longer, in some very special cases. I don't think it happened very often, but maybe two, three or four times, I can't say. As I've just said, I can only recall one case in point, perhaps because the situation occurred with the same young mathematician on several occasions, in one form or another. I can say that in every respect this young researcher, whose home university was abroad, was perfectly correct in sending the work he had just done to me, who was supposed to be the person most in the loop. Each time, I reacted very coolly, for the reason I said. I can't even tell for sure if I was telling him straight out that what he was doing had been known to me for ages, and that for that reason it bothered me that he was publishing it without at least giving me a little bow in the introduction. Of course, if he had

p. 76 was my pupil, this authorial fatuity would not have come into play so much, on the one hand because of a sympathetic relationship that had already been established with the pupil, but also because it went <sup>de</sup> soi anyway that the pupil's work also contained the boss's ideas, unless otherwise stated! I think the situation must have happened twice, maybe even three times, with this same researcher, and each time I had an equally cool, equally discouraging attitude. I never agreed, if I remember correctly, to recommend a work by this researcher for publication in such-and-such a journal, nor to sit on a thesis jury (I seem to recall the question having arisen). It's almost as if I'd decided to choose him as my pet. The most beautiful,

is that his work in each case was perfectly valid - I believe it was carefully written, and I have no reason to suppose that he didn't come up with the ideas he was developing himself, which at the time were not yet widely known, and were (more or less) "well known" only to a handful of people in the know, such as Serre, Cartier, myself and one or two others. What's incomprehensible to me is that this young colleague (he ended up, of course, with a well-deserved thesis and job) never tired of addressing me, who "beat him cold" with every blow, and that he apparently never resented me for it. Still, I remember the surprise he once expressed at my reticence - he obviously didn't understand what was going on. He'd have had a hard time if he'd waited for me to explain! He had a beautiful head, a bit like a classic Greek, very youthful - rather soft, peaceful features, evoking an inner calm... . Now that I'm trying for the first time to put my finger on the impression of his person and physiognomy, I suddenly realize that he really did look a lot like that "indefatigable friend" I'd spoken about; they could have been brothers, this friend of my age in smiling tones, and this researcher, twenty years younger, in rather serious, but by no means sad, tones. It's not impossible that this resemblance came into play, that I projected onto one a disdain that had found no occasion to express itself with the other, disarmed as he was by the signs of such a faithful friendship! And indeed, I had to have developed a really thick shell, not to be disarmed by the obvious good faith and desire to do the right thing in this young man, who was certainly endearing, and who never tired of coming back to me without my deigning to give him so much as a smile!

## 8.8. (32) The ethics of the mathematician

The case I reported yesterday, now that I've finally <sup>taken</sup> <sup>the</sup> trouble to write it down in black and white, <sup>p. 77</sup> seems to me of considerable significance, greater in some respects than the other three cases (no doubt typical too) reported earlier, where forces of fatuity deeply disturbed in me a natural attitude of benevolence and respect. This time, using a position of real power (then that I, like everyone else, pretended to ignore this power), I used it to discourage a willing researcher, and refuse a work that deserved to be published. That's what we call **abuse of power**. It's no less blatant for not falling under an article of the penal code. Fortunately, the situation at the time was less difficult than it is today, so that this researcher was able, without too much difficulty I believe, to get his work published with the support of some colleagues more benevolent than myself, and his career as a mathematician was not seriously disrupted, let alone broken, by my abusive behavior. I'm happy about this in retrospect, but I don't want to make it an "extenuating circumstance". It's possible that in tougher times, I would have been more careful - but that's just a guess, and has little to do here. All the same, I think I can say that there was no secret malice in me, no desire to do harm caused by the irritation I mentioned. I reacted to this irritation in a "visceral" way, without the slightest inclination to criticize myself, and even less without the slightest inclination to look at what was going on inside me, or even the impact my reaction could have on the other person's life. I didn't realize how much power I had, and the thought of any responsibility that went with that power (even if only the power to encourage or discourage) never occurred to me during this relationship. It was a typical case of **irresponsible behavior**, the kind you find on every street corner, in the scientific world as elsewhere.

It's possible that the only case of his kind that I can remember is an extreme one, among a few others like it. What triggers an attitude without benevolence is the irritation of a vanity, impatient of

to see "the first comer" arrogating to himself the right to walk through guarded hunting grounds and take some small game that belongs only to the masters of these places... . This irritation has its own rationalizations, which

p. 78 have a nobler allure, one suspects. It's not my modest self that's at stake but no, but the love of art and mathematics, this young man who doesn't even have the excuse to be great the clumsy kind rather. he's going to ruin everything, woe betide us, if only he could do things better than I can, but the beautiful prescriptions I'd planned all went out the window, you've got to be a little shameless, frankly... . ! The meritocratic leitmotiv is always present: only the very best (like me) have the right to work for me, or those who put themselves under the protection of one of those! (As for the less common case where it's actually another great chef who steps on my toes, that's a different kettle of fish - one day at a time!) In the case in point, there was (I'm in no doubt about this) another force moving in the same direction, entirely unconscious, which had already played a major role in my relationship with the indefatigable friend of my early days: an automatism of rejection towards a certain type of person, not corresponding to the canons of "virility" I'd taken over from my mother. But this circumstance, which has its own significance and interest for an understanding of myself, is relatively irrelevant for my present purpose: that of finding in myself, in attitudes and behaviours that were mine when I was still part of a certain milieu, the typical signs of a profound degradation that I see there today.

If this case, which I have just examined, seems to me of greater significance than the others in which I failed to show kindness and respect, it is because it is the one in which a certain **elementary ethic** is violated in the mathematician's profession<sup>13</sup> (24). In the milieu where I was welcomed in my early days, the Bourbaki milieu and those close to Bourbaki, this ethic I'm talking about was generally implicit, but it was nevertheless present, alive, the object (it seems to me) of an intangible consensus. The only person who expressed it to me in no uncertain terms, as far as I can remember, was Dieudonné, probably one of the first times I was his guest in Nancy. He may have returned on other occasions. He obviously felt it was important, and I must have sensed the importance he attached to it, to have remembered it even today, thirty-five years later. Simply because of the moral authority of the group of my elders, and Dieudonné, who obviously expressed a group consensus at the time, I had to tacitly adopt this ethic as my own, without ever having given it a moment's thought, or understood what was behind it.

p. 79 was important. To tell you the truth, it didn't even occur to me that it might be useful for me to give it some thought.

□une reflection, convinced as I had been for a long time that my parents and myself represented, a perfect embodiment (or very nearly so) of an ethical, responsible and foolproof attitude<sup>14</sup> (25).

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<sup>13</sup>(24)

The ethics I'm talking about apply just as much to any other milieu formed around a research activity, and where the possibility of making one's results known, and taking credit for them, is a matter of "life or death" for the social status of any member, or even of "survival" as a member of this milieu, with all the consequences this implies for him and his family.

<sup>14</sup>(25) **Ethical consensus - and control of information**

Apart from the conversation with Dieudonné, I can't recall a single conversation in my life as a mathematician in which the ethics of the profession or the "rules of the game" were discussed. (I exclude here the discussions about the collaboration of scientists with the military, which took place in the early 70s around the "Survivre et Vivre" movement. They didn't really concern the relationships between mathematicians. Many of my friends in Survivre et Vivre, including Chevalley and Guedj, felt that the emphasis I placed at that time, especially in the early days, on this issue to which I was particularly sensitive, distracted me from more essential everyday realities, of precisely the kind I am examining in the present reflection). These things were never discussed between a student and myself. The tacit consensus, I believe, was limited to this one rule, not to present as one's own any ideas of others of which one might have been aware. This consensus, it seems to me, has existed since antiquity, and has never been contested in any scientific milieu. But in the absence of this other complementary rule, which guarantees every researcher the possibility of making his ideas and results known, the

Dieudonné didn't give me a long speech - it wasn't his style any more than it was that of any of his friends in Bourbaki. He must have mentioned it in passing, and as something that was taken for granted. He was simply insisting on a simple rule, seemingly unimportant, which is this: **anyone who finds a result worthy of interest must have the right and the opportunity to publish it, on the sole condition that this result is not already the subject of a publication.** So even if this result was known to one or more people, as long as they didn't take the trouble to put it down in black and white and publish it, so as to make it available to (hm!) the "mathematical community", any other person (innuendo: including the famous "first-timer"! ) who finds the result by his or her own means (innuendo: whatever his or her means, points of view and insights, and whether or not they seem "narrow" to people supposedly more in the know than he or she is... . ) must have the possibility of publishing it, according to his own means and insights. I seem to remember Dieudonné adding that if this rule was not respected, it opened the door to the worst abuses - it's possible that it was on this occasion and through his mouth that I learned of the historic case of Gauss refusing Jacobi's work, on the pretext that Jacobi's ideas had been known to him for a long time.

This simple rule was the essential corrective to the "meritocratic" attitude that existed in Dieudonné (and other Bourbaki members) as well as in myself. Respect for this rule was a guarantee of **probity**. I'm happy to be able to say, from everything that has come down to me to date, that this essential probity has remained intact in every member of the original Bourbaki group<sup>15</sup> (26). I note that this was not the case for other mathematicians who were part of the Bourbaki group or milieu. It has not remained intact in my own person.

The ethics Dieudonné spoke to me about in down-to-earth terms are dead as ethics. of a certain milieu. Or rather, this milieu  $\square$  I lui-même died at the same time as that probity which made it the soul. This probity has been preserved in certain isolated individuals, and it has reappeared or will reappear in others where it had deteriorated. Its appearance or disappearance in each of us is part of the crucial episodes of our spiritual adventure. But the stage on which this adventure takes place is profoundly transformed. A milieu that had welcomed me, that I had made my own, of which I was secretly proud, is no more. What made it worthwhile died within me, or at least was invaded and supplanted by forces of a different nature, long before the tacit ethics that governed it were openly disavowed in customs and professions of faith. If I've ever been surprised or offended, it's been out of wilful ignorance. What came back to me from this milieu that was once mine had a message to bring me about myself, which I've been happy to avoid until now.

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first rule remains a dead letter. In today's scientific world, men in positions of prestige and power have discretionary control over scientific information. In the milieu I knew, this control is no longer tempered by a consensus like the one Dieudonné spoke of, which perhaps never existed outside the restricted group whose spokesman he was. The scientific que in a position of power receives practically all the information it deems useful to receive (and often even more), and has the power, for much of this information, to prevent its publication while retaining the benefit of the information and rejecting it as "uninteresting", "more or less well known", "trivial", etc. ... I return to this situation in note (27).

<sup>15</sup>(26)

Bourbaki's "founding members" are Henri Cartan, Claude Chevalley, Jean Delsarte, Jean Dieudonné. André Weil. They are all still alive, with the exception of Delsarte, who died before his time in the 1950s, at a time when the ethics of the profession were still generally respected.

On re-reading the text, I was tempted to delete this passage, in which I may give the impression of issuing certificates of "probity" (or non-probity) that the interested parties have no use for, and that it is not my responsibility to issue. The reservations this passage may arouse are surely justified. I've kept it, however, out of concern for the authenticity of the testimony, and because it does convey my feelings, however misplaced they may be.



# 9. Harvest

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### 9.1. (33) The note - or the new ethics

Of course, a rule of deontology only takes on its meaning through an inner attitude, which is its soul. It cannot create the attitude of respect and fairness it seeks to express; at most, it can contribute to the permanence of such an attitude, in an environment where the rule enjoys general consensus. In the absence of an inner attitude, even if the rule is professed by the lips, it loses all meaning and value. No amount of exegesis, no matter how scrupulous or meticulous, can change that.

One of my friends and companions of yesteryear kindly explained to me recently that these days, alas, with the inordinate influx of mathematical production, "we" are absolutely obliged, whether we like it or not, to make a severe selection from the papers that are written and submitted for publication, to publish just a small part of them. He said this with an air of sincere regret, as if he himself were a bit of a victim of this inescapable fatality - a bit like the air he also had to say that he himself was one of the "six or seven people in France" who decide which articles will be published, and which not. Having become less loquacious with age, I confined myself to listening in silence. There was a lot to say on the subject, but I knew it would be wasted effort. One or two months later

I have learned, moreover, that this colleague refused a few years ago to recommend the publication of a certain note to the CRs' <sup>whose</sup> author as well as the theme (which I had suggested to him seven or eightp . 81 years) are close to my heart. The author had spent two years of his life developing this theme, which is admittedly not fashionable (although it still seems so topical to me). I think he did an excellent job (presented as a post-graduate thesis). I wasn't the "boss" of this young researcher, who happens to be brilliantly gifted (I don't know whether he'll continue to apply his gifts in mathematics, given the reception. . . ), and he did his job. ), and he did his work without any contact with me. But it's also true that there could be no doubt as to the origin of the theme developed; the poor guy was in a bad way, and he certainly didn't suspect a thing! This colleague put his foot down, at least, and I wouldn't have expected anything less from him: "I'm really sorry, but you understand... .". Two

years of work by a highly motivated novice researcher, against a three-page CR note - how much would it have cost the public purse? The absurdity of this enormous disproportion between one and the other is obvious. Surely this absurdity disappears, if we take the trouble to examine the underlying motivations. Only this colleague and former friend is in a position to fathom his own motivations, just as I am in a position to fathom mine. But without having to go too far, I'm well aware that it's not the inordinate influx of mathematical production you know, nor the public purse (or the patience of an imaginary "unknown reader" of the CRs) that it would have been a question of sparing...

This same draft note to the CRs had already had the honor of being submitted to another of the "six or seven people in France... .", who sent it back to the author's "boss", because this mathematics "didn't amuse him" (textual!). (The boss, disgusted but cautious, himself in a rather precarious position, preferred both times to crush rather than displease... .) Having had the opportunity to discuss the matter with this colleague and ex-student, I learned that he had taken the trouble to read the submitted note carefully and reflect on it (it must have brought back many memories for him. . . ), and that he had found that some of the statements could have been presented in a more user-friendly way. Yet he didn't deign to waste his precious time submitting his comments to the person concerned: fifteen minutes from the illustrious man, against two years' work from an unknown young researcher! He was "amused" enough by the maths to seize the opportunity to reconnect with the world.

p. 82 situation studied in the note (which could not fail to arouse in him, as in myself, a rich web of diverse geometric associations), to assimilate the description given, then, without difficulty given his baggage and his means, to detect clumsiness or shortcomings. He hasn't wasted his time: his knowledge of a certain mathematical situation has been clarified and enriched, thanks to two years of conscientious work by a researcher taking his first steps; work that the Master would certainly have been able to do (in broad outline and without demonstrations) in a few days. Having acquired this knowledge, we remember who we are - the case is judged, two years of work by Monsieur Personne is fit for the dustbin... .

Some people don't feel a thing when that wind blows - but even today, it still takes my breath away. This was surely one of the desired effects in this case (given the exquisite form of the refusal), but certainly not the only one. In the same interview, this friend of yesteryear confided in me, with an air of modest pride, that he only agreed to submit a note to the CRs when "the results stated astonished him, or he didn't know how to demonstrate them"<sup>1</sup> (27). This is undoubtedly one of the reasons why he rarely publishes. If he applied to himself

<sup>1</sup>(27) "Youth snobbery", or the defenders of purity

Ronnie Brown shared with me a reflection by J.H.C. Whitehead (of whom he was a pupil), speaking of the "snobbery of the young, who believe: that a theorem is trivial because its proof is trivial". Many of my old friends would do well to ponder these words. Today, this "snobbery" is by no means limited to young people, and I know more than one prestigious mathematician who routinely practices it. I'm particularly sensitive to it, because the best work I've done in mathematics (and elsewhere too. . . ), the notions and structures I've introduced that seem to me to be the most fruitful, and the essential properties I've been able to extract from them through patient and persistent work, all fall under the label of "trivial". (None of these things would have stood much chance of being accepted for a CR grade these days, were the author not already a celebrity!) My lifelong ambition as a mathematician, or rather my passion and joy, has been constantly to **find the obvious things**, and this is my sole ambition also in the present work (including this introductory chapter. . . ). The decisive thing is often already to see the **question** that had not been seen (whatever the answer may be, and whether it has already been found or not) or to come up with a statement (even if it is conjectural) that sums up and contains a situation that had not been seen or understood; if it is demonstrated, it doesn't matter whether the demonstration is trivial or not, which is entirely incidental, or even whether a hasty and provisional demonstration proves to be false. The snobbery of which Whitehead speaks is that of the jaded wine-lover who deigns to appreciate a wine only after he has ascertained that it has cost a great deal of money. More than once in recent years, taken back by my old passion, I've offered the best I had, only to see it rejected by that sufficiency. I've felt a pain that's still alive, a joy that's been disappointed - but that doesn't mean I'm homeless, and fortunately for me, I wasn't trying to fit in an article of my own.

The snobbery of which Whitehead speaks is an abuse of power and a dishonesty, not only an insensitivity or closure to the beauty of things, when it is exercised by a man of power against a researcher at his mercy, whose ideas he has free rein to assimilate and use, while blocking their publication on the pretext that they are "obvious" or



He knows everything, and it must be as difficult to surprise him as it is to find something demonstrable that he can't demonstrate. (This has only happened to me two or three times in the space of twenty years, and not even in the last ten or fifteen!) He's clearly proud of his "quality" criteria, which make him the champion of the highest standards in mathematics. I've seen his unfailing self-indulgence, and more than once his unbridled contempt for others, behind the appearances of smiling, good-natured modesty. I've also seen that he finds great satisfaction in it.

This colleague's case is the most extreme I've come across among representatives of the "new ethics". It is no less typical. Here again, both in the incident I have reported and in the profession of faith that rationalizes it, there is an ubiquitous absurdity, in terms of simple common sense - of such enormous dimensions that this former friend with such an exceptional brain, and surely also many of his colleagues of less prestigious status (who will be content not to approach him to present a note to the CRs) no longer see it.

To see, you have to look, at the very least. When one takes the trouble to look at the motivations (and one's own first and foremost),<sup>□</sup> then the absurdities appear in full light, and they cease on p. 83 at the same time as being absurd, by delivering their humble and obvious meaning.

If in recent years I have often found it so distressing to be confronted with certain attitudes and, above all, certain behaviours, it's surely because I could discern in them, obscurely, a caricature taken to the extreme, to the point of grotesqueness or odiousness, of attitudes and behaviours that had once been mine and that were brought back to me by some of my former students or friends. More than once, I've been triggered by the old reflex to denounce, to fight "evil" that has been clearly pointed out - but if I've given in to it, here and there, it's been with divided conviction. Deep down, I know that to fight is to continue skating on the surface of things, to evade the issue. My role is not to denounce, or even to "improve" the world in which I find myself, or to "improve" myself. My vocation is to learn, to know this world through myself, and to know myself through this world. If my life can bring any benefit to myself or to others, it's only insofar as I know how to be faithful to this vocation, how to be in tune with myself. It's time to remind myself of this, to cut short those old mechanisms in me, which here would like to push me to plead a cause (of a certain dead ethic, let's say), or to convince (of the supposedly "absurd" character of this ethic which has replaced it, perhaps), rather than to **probe** to discover and know, or to **describe** as a means to probe. In writing the preceding two or three pages, with no more precise aim than to say a few words about the current attitudes that have replaced those of yesterday, I have felt continually on my guard towards myself, in the mood of one who would be prepared at any moment to cross out with a broad stroke everything he has just written and throw it in the garbage can! However, I'm going to keep what I've written, which isn't false but nevertheless creates a false situation, because I'm involving others more than I'm involving myself. I felt deep down that I wasn't learning anything by writing, and that's surely what created this unease in me. Decidedly, it's time to return to a more substantial form of reflection, one that instructs me rather than pretending to instruct or convince others<sup>2</sup> (28).

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"trivial", and therefore "uninteresting". I'm not even thinking here of the extreme situation of plagiarism in the common sense of the term, which must still be very rare in mathematical circles. Yet from a practical point of view, the situation is the same for the researcher who pays the price, and the inner attitude that makes it possible doesn't seem much different to me either. It's simply more comfortable, since it's accompanied by the feeling of an infinite superiority over others, and the good conscience and intimate satisfaction of the person who poses as the intransigent defender of the intangible purity of mathematics.

<sup>2</sup>(28)

In writing the preceding pages, I was initially divided between a desire to "get it off my chest", and a concern to keep to myself.

## 9.2. (34) Silt and source

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It seems to me that, for the most part, I've come full circle in my relationships with other mathematicians of all ages and ranks, back when □ I was part of their world, the world of mathematicians; and in At the same time, and above all, because of the part I have played, through my own attitudes and behavior, in a certain spirit that I see there today, and which is certainly not new. In the course of this reflection - or journey, to put it more accurately - I came across four situations that struck me as typical of certain attitudes and ambiguities in myself, where spontaneous dispositions of benevolence and respect towards others were disrupted, if not totally swept away, by egotistical forces, and above all (in three of these cases at least) by **fatuity**. This fatuity was based above all on the supposed superiority conferred on me by a certain cerebral power, and the inordinate investment I made in my mathematical activity. It found confirmation and support in a general consensus that valued, almost without reservation, this cerebral power and this disproportionate investment.

It's the last of the situations examined, that of the "young misfit who stepped on my toes", which seems to me the most important of the four for my present purpose. The first three are typical of me, or of certain aspects of me, at a certain time (in a certain context too, it's true) - but, as I've had occasion to say over and over again, I don't consider them in any way typical of the milieu to which I belonged. Nor do I think they are typical of the current mathematical milieu in France, let's say - it's likely that the kind of chronic bewilderment that characterized my relationship with "l'ami infatigable", for example, is as uncommon nowadays as it must have been then. My attitude and behavior in the case of the "young misfit", on the other hand, is typical of what happens every day in the mathematical world, wherever you look. It's the attitude of benevolence and respect shown by the influential mathematician towards the young stranger that becomes the rare exception, when the said stranger is not his pupil (and yet. . . ), or the pupil of a colleague of comparable status and recommended by him. This is undoubtedly what was coming back to me in the aftermath of my "awakening" in 1970, which had loosened mute tongues - but the first-hand accounts I heard at the time remained remote for me,

because they didn't directly concern me or my dearest friends. I was affected more than superficially from the moment (around 1976) when the echoes □ which I

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In some cases, the lack of benevolence, or even an ostentatious attitude of contempt, was reinforced, at the very least, if not aroused, by the mere fact that a young researcher was a friend of mine, or even an ex-student who had become important, and even more so when those who were the target of malice were people I knew well, students on more than one occasion (post-1970 students, needless to say!), whose fate therefore touched me. In some cases, moreover, there was no doubt that the lack of benevolence, or even an attitude of ostentatious contempt, was reinforced at the very least, if not aroused, by the mere fact that such a young researcher was my pupil, or that he was taking the risk (without necessarily being my pupil) of doing what my friends of yesteryear and other colleagues also readily called "Grothendieckeries". . .

The "young misfit" wrote to me again in the early '70s, asking me very courteously (although he was under no obligation to ask me anything at all!) if I didn't mind him publishing a demonstration he'd found for a theorem he'd been told I'd authored, and which had never been published. I remember that I replied in the same bad-tempered mood as before, without saying yes or no I think, and implying, without knowing his demonstration (that he was

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discretion. So I'd been keeping to the point, which was surely the main reason for my discomfort, my feeling that "I wasn't learning anything". Since the lines noting this malaise were written, I've twice rewritten those pages that had left me feeling so discontented inside, getting more clearly involved and getting to the bottom of things. Along the way, I have indeed "learned something", and I also believe that I have managed to put my finger on something important that goes beyond the case in point and beyond myself.

I was of course ready to communicate it to myself, but I didn't care about it, busy as I was with my militant duties!), that it certainly wouldn't add anything to mine (although it would have helped, at the very least, if it had been written down in black and white and made available to the mathematical public, along with the statement itself!) This just goes to show the extent to which this famous "awakening" was still superficial, without any impact on certain behaviours rooted in fatuity and "meritocratic" attitudes, which I was surely denouncing at the same time in heartfelt articles in *Survivre et Vivre*, in public debates, etc. ... .

This is a very concrete answer to a question I had left open earlier. I might as well admit this humble truth, that such attitudes of fatuity are by no means overcome "once and for all" in my person, and I doubt they ever will be if not at my death. If there has been a transformation, it is not through the disappearance of vanity, but through the appearance (or reappearance) of curiosity about myself and the true nature of certain attitudes, behaviours etc. ... in myself. It's a

through this curiosity that I have become somewhat sensitive to the manifestations of vanity within myself. This profoundly modifies a certain inner dynamic, and thereby modifies the effects of "vanity"; that is of this force that often pushes me to conceal or counterfeit my healthy and fine perception of reality, in order to aggrandize myself and put myself above others while pretending the opposite. . 86

Perhaps such a reader will feel baffled, as I once did, by the apparent contradiction between the insidious and tenacious presence of **vanity** in my life as a mathematician (which he may also have glimpsed at times in his own), and what I call my **love**, or **passion**, for mathematics (which perhaps also echoes in his own experience of mathematics, or of some other person or thing). If he is baffled indeed, he has within him everything he needs to get back in touch (as I once did) with the reality of things themselves, which he can know first-hand, rather than spinning around like a squirrel trapped in an endless cage of words and concepts.

Will he who sees muddy water say that water and mud are one and the same thing? To know which water is not mud, all you have to do is go up to the spring, look and drink. To know the mud that isn't water, all you have to do is go up to the bank, dried by the sun and the wind, and detach a ball of grainy clay in your hand. Ambition and vanity can more or less regulate the proportion of one's life devoted to a particular passion, like mathematical passion, and can make it all-consuming, if the returns satisfy them. But the most devouring ambition is powerless on its own to discover or know the least of things - quite the contrary! In the moment of work, when little by little an understanding begins, takes shape, deepens; when in a confusion little by little an order appears, or when what seemed familiar suddenly takes on unusual, then disturbing aspects, until finally a contradiction bursts forth and upsets a vision of things that seemed unchanging - in such work, there is no trace of ambition, or vanity. What then leads the dance is something that comes from much further away than the "I" and its hunger for constant expansion (be it of "knowledge" and "knowing") - from much further away, surely, than our person or even our species.

This is the source, which is within each one of us.

### 9.3. (35) Mes passions

□ Three great passions have dominated my adult life, alongside other forces of a different nature. I ended up p. 87 recognize in these passions three expressions of the same deep-seated drive; three paths taken by the drive for knowledge in me, among the infinite number of paths open to it in our infinite world.

The first to manifest itself in my life was my passion for mathematics. At the age of seventeen, just out of high school, I let go of a simple inclination and turned it into a passion, which directed the course of my life for the next twenty-five years. I "knew" mathematics long before I knew the first woman (apart from the one I knew from birth), and today in my middle age, I see that it is still not consumed. She doesn't rule my life any more than I pretend to. Sometimes it slumbers, sometimes to the point where I think it's extinguished, only to reappear unannounced, as fiery as ever. It no longer devours my life as it once did, when I gave it my life to devour. She continues to leave a deep imprint on my life, like the imprint in a lover of the woman he loves.

The second passion in my life was the quest for a woman. This passion often presented itself to me in the guise of the quest for a companion. I wasn't able to distinguish one from the other until the latter came to an end, when I knew that what I was pursuing was nowhere to be found, or that I was carrying it within myself. My passion for women didn't really unfold until after my mother's death (five years after my first love affair, from which a son was born). It was then, at the age of twenty-nine, that I started a family, from which three more children were born. My attachment to my children was originally an indissoluble part of my attachment to my mother, a part of the power emanating from the woman who drew me to her. It's one of the fruits of this passion for love.

I didn't experience the presence of these two passions within me as a conflict, either in the early days or later on. I must have obscurely sensed the profound identity of the two, which became clear to me much later, after the appearance in my life of the third. Yet the effects of both passions on my life were not the same.

p. 88 could only be very different. The love of mathematics drew me into a certain world, that of mathematical objects, which surely has its own "reality" of its own, but which is not the world in which life unfolds of men. The intimate knowledge of mathematical things taught me nothing about myself, let alone about others - the impulse of discovery towards mathematics could only distance me from myself and from others. There may sometimes be communion between two or more in this same impulse, but this is communion on a superficial level, which in fact distances each of us from ourselves and from others. This is why my passion for mathematics has not been a maturing force in my life, and I doubt that such a passion can foster maturation in anyone<sup>3</sup> (29). If I gave this passion such a disproportionate place in my life for a long time, it was surely also precisely because it enabled me to escape the knowledge of conflict and the knowledge of myself.

The sex drive, on the other hand, whether we like it or not, launches us straight into the encounter with the other, and straight into the knot of conflict in ourselves as well as in the other! The quest for "companionship" in my life was the quest for conflict-free bliss - not the drive for knowledge, the drive for sex, as I liked to believe, but an endless flight from the knowledge of conflict in the other and in myself. (This was one of the two things I had to learn, so that this illusory quest would come to an end, and the anxiety that accompanies it like its inseparable shadow... .) Fortunately, no matter how much we run away from conflict, sex quickly brings us back to it!

One day I gave up trying to deny the teaching that conflict had stubbornly brought me, through the women I loved or had loved, and through the children born of these loves. When I finally began to

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<sup>3</sup>(29) **Fear of playing & The two brothers**

I'm talking here about an intense, long-term investment in mathematics, or in some other wholly intellectual activity. On the other hand, the unfolding of such a passion - which can be a way of reacquainting ourselves with a forgotten force within us, and an opportunity to measure ourselves against a reluctant substance and, in the process, renew and enrich our sense of identity with something truly personal to us - such an unfolding may well be an important stage in an inner journey, in a maturing.

listening and learning, and for years afterwards, it turned out that everything I learned, I learned from the women I had loved or loved<sup>4</sup> (30). Until 1976, at the age of forty-eight, it was the quest for women that was the only great maturing force in my life. If this maturation only took place in the years that followed, i.e. over the last seven years, it's because I protected myself from it (as I had learned to do from my parents and the people around me) by every possible means.

available to me. The most effective of these means was my investment in my mathematical passion.

The day the third great passion appeared in my life - a certain night in October 1976 p. 89

- the great fear of learning has vanished. It's also the fear of simple reality, of humble truths about myself first and foremost, or about people I care about. Strangely enough, I had never perceived this fear in myself before that night, at the age of forty-eight. I discovered it the very night this new passion, this new manifestation of the passion to know, appeared. It took the place, so to speak, of fear, finally recognized. For years, I had seen this fear clearly in others, but by some strange blindness, I couldn't see it in myself. The fear of seeing prevented me from seeing this very fear of seeing! I was strongly attached, like everyone else, to a certain image of myself, which for the most part hadn't changed since my childhood. The night I'm talking about is also the one when, for the first time, that old image collapsed. Other images like it followed, holding on for a few days or months, or even a year or two, thanks to stubborn forces of inertia, only to collapse in their turn under a scrutinizing gaze. The laziness of looking often delayed such a new awakening - but the **fear of** looking never reappeared. Where there's curiosity, there's no room for fear. When I'm curious about myself, there's no more fear of what I'm going to find than when I want to know the final word of a mathematical situation: there's a joyful expectation, impatient at times and yet obstinate, ready to welcome whatever comes its way, foreseen or unforeseen - a passionate attention on the lookout for the unequivocal signs that make it possible to recognize the true in the initial confusion of the false, the half-true and the maybe.

In curiosity about oneself, there's love, untroubled by any fear that what we're looking at might not be what we'd like to see. And to tell the truth, love for myself had silently blossomed in the months leading up to this night, which is also the night when this love took on an active, enterprising form, ruthlessly shaking up costumes and sets! As I said, other costumes and sets soon reappeared as if by magic, to be jostled in their turn, without invective or gnashing of teeth... .

The way this new passion has manifested itself in my life over the last seven years has led me to appear  
seem like the moving up-and-down of waves following one another, like the breaths of a vase<sup>te</sup> and peaceful  
piration. res

is is not the place to try to trace its sinuous, shifting line, or perhaps  
that, in counterpoint, of the manifestations of mathematical passion. I've given up trying to regulate the  
course of one or the other - it's rather this double movement of one and the other that today regulates the  
course of my life - or better said, **is** its course. Th . 90

In the months that had already preceded the appearance of the new passion - months of gestation and plenitude  
- the woman's quest began to change its face. It began to separate itself from the anxiety with which it had  
been imbued, like a "breath" that had freed itself from the oppression that had weighed it down, and regained  
its amplitude and rhythm. Or like a fire that had been smouldering, half-stifled for lack of an escape, and  
which, under a breath of fresh air, would suddenly spread out in crackling, agile flames.

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<sup>4</sup>(30)

In recent years, my children have taken over the task of teaching a sometimes reluctant pupil about the mysteries of human existence. ...

and lively!

The fire burned to satiation. A hunger that seemed unquenchable was satisfied. For the past two or three years, it seems that this quest has been consumed without a trace of ashes, leaving the field open to the song and counter-song of two passions. One, the passion of my youth, served for thirty years to separate me from a disowned childhood. The other, the passion of my middle age, led me to rediscover both the child and my childhood.

#### 9.4. (36) Desire and meditation

The night I mentioned, when a new passion took the place of an old fear that had vanished forever, was also the night I discovered meditation. It was the night of my first "meditation", which came about under the pressure of a pressing, urgent need, after I had been overwhelmed by waves of anguish in the preceding days. Like all anguish, perhaps, this was a "take-off anguish", insistently signalling the take-off between a humble and obvious reality about myself, and an image of myself that was forty years old and never questioned by me. Surely there must have been a great thirst for knowledge, alongside considerable escapist forces, and a desire to escape anguish, to be at peace as before. So there was intense work, which continued for a few hours until it came to an end, without me yet knowing the meaning of what was happening, let alone where I was going. In the course of this work, the red herrings have been recognized one after the other; or to put it better, it's this work that has made

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□ appear one by one these red herrings, each under the guise of an intimate conviction that I was finally taking the

I took the trouble to write it down in black and white, as if to get a better grasp of it, whereas until then it had remained in a blur. I was quite happy to write it down, without the slightest suspicion, it must surely have seduced me - in the mood of someone who doubts nothing, and for whom the mere fact of having written down an informal conviction in black and white was the irrefutable sign of its authenticity, the proof that it was well-founded. If it hadn't been for my indiscreet, not to say indecent, desire to know, I would have stopped every time at this "happy ending", and it was with this happy ending that the stage ended. And then, woe is me! I'd get the whim, God knows how and why, to take a closer look at what I'd just written to my complete satisfaction: it was there in black and white, all I had to do was reread it! And as I reread it carefully, naively, I sensed that something was a little off, that it wasn't quite so clear! Then, taking the trouble to look a little more closely, it became clear that it wasn't that at all, that it was all bogus, in other words, that I'd just been led astray! Each time, this partial discovery came as a famous surprise: "Wow, that's a good one", a joyful surprise that rekindled my thinking with a surge of new energy. We're going to get to the bottom of this, and I'm sure it's going to come out no later than now, so let's keep the momentum going! We'll take stock, take stock... and here we go again, with all the trappings of "the end of the story", we just have to believe it must be it this time, we'll write it down anyway, out of good conscience, and it's a pleasure even to write down such judicious and well-felt things, you'd really have to be wrong-headed not to agree, such obvious good faith, you can't do better than that, it's perfect as it is!

This was the new end of the stage, the new happy ending, on which I would have paused contentedly, if it hadn't been for the naughty little boy who once again got into mischief and, incorrigible as ever, decided to stick his nose into this last "fin mot" and happy ending. There was no stopping him, and here we go again!

And so, for four hours, the stages followed one □to another, like an onion whose I would have peeled off layer after layer (that's the image that came to me at the end of that night), to get to the **heart of the matter** - to the simple, obvious truth, a truth that was staring me in the face, and yet which I had managed for days and weeks (and my whole life, in fact) to conceal under this accumulation of "onion layers" hiding one behind the other.

The appearance at last of the humble truth was an immense relief, an unexpected and complete deliverance. I knew in that instant that I had touched the crux of anguish. The anguish of the last five days was well and truly resolved, dissolved, transformed into the knowledge that had just formed within me. The anguish had not only disappeared from my sight, as it had throughout the meditation, and several times during the previous five days too; and the knowledge into which it had been transformed was in no way in the nature of an idea, of a concession that I would have made, let's say, to be even and quiet (as had happened to me here and there during the same night); it was not an external thing that I would then have adopted or acquired to add to my person. It was **knowledge** in the full sense of the word, first-hand, humble and obvious, which was now part of me, just as my flesh and blood are part of me. It was, moreover, formulated in clear and unequivocal terms - not in a long speech, but in a silly little sentence of three or four words. This formulation had been the final step in the work that had just been carried out, which remained ephemeral, reversible as long as this final step had not been taken. Throughout this work, the careful, even meticulous formulation of the thoughts that formed, the ideas that presented themselves, had been an essential part of this work, each new departure of which was a reflection on the stage I had just gone through, which was known to me through the written testimony I had just given (with no possibility of retracting it in the mists of a failing memory!).

In the minutes that followed the moment of discovery and deliverance, I also knew the full significance of what had just happened. I had just discovered something even more precious than the humble truth of the last few days. This thing was the power in me, for as little as I□s interested, to know.

the final word on what's going on inside me, on any situation of division or conflict - and thus the ability to resolve entirely, by my own means, any conflict inside me of which I may have become aware. The resolution is not the result of some **grace**, as I had tended to believe in previous years, but of intense, obstinate and meticulous **work**, making use of my ordinary faculties. If there is such a thing as "grace", it is not in the sudden and definitive disappearance of a conflict within us, or in the appearance of an understanding of the conflict that would come to us ready-made (like the chickens in the land of Cocagne!) - but it is in the presence or appearance of this desire to know<sup>5</sup> (31). It was this desire that had guided me to the heart of the conflict in the space of a few hours - just as the desire to love leads us unfailingly to find the path that leads to the innermost depths of the woman we love.

Whether we're talking about self-discovery or mathematics, in the absence of desire, all so-called "work" is nothing but a mockery, leading nowhere. At its best, it keeps the person who indulges in it "beating around the bush" endlessly - the contents of the bush are reserved for those who are hungry enough to eat! As with all

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<sup>5</sup>(31)

I'm thinking here of the "yang" form of the desire to know - that which probes, discovers, names what appears... . It is having been **named** that makes the knowledge that appears irreversible, ineffaceable (even if it is subsequently buried, forgotten, ceases to be active. . . ). The "yin", "feminine" form of the desire for knowledge is in an openness, a receptivity, in a silent welcoming of knowledge appearing in deeper layers of our being, where thought has no access. The appearance of such openness, and of a sudden knowledge that for a time erases all traces of conflict, comes as a grace once again, touching deeply even though its visible effect may be ephemeral. I suspect, however, that this wordless knowledge that comes to us in this way, at certain rare moments in our lives, is just as ineffaceable, and its action continues even beyond the memory we may have of it.

When it comes to the desire to know myself, my knowledge of myself and the situations I'm involved in remains inert. When it comes to the desire for self-knowledge, then my knowledge of myself and the situations in which I'm involved remains inert, and I act not with full knowledge of the facts, but at the whim of simple inveterate mechanisms, with all the consequences that implies - a bit like a car being driven by a computer, not by a person. But whether it's meditation or mathematics, I wouldn't dream of pretending to "work" when there's no desire, when there's no hunger. That's why I've never meditated for even a few hours, or done maths for even a few hours<sup>6</sup> (32), without learning something; and more often than not (not to say always) something **unforeseen** and unpredictable. This has nothing to do with faculties that I have that others don't, but only comes from the fact that I don't pretend to work without really wanting to. (It's the strength of this "desire" that alone also creates the **requirement** I've talked about elsewhere, which means that in work we're not satisfied with a little more, but with a lot more.

p. 94 is satisfied only after having gone to the end of an understanding, however humble). Where discovery is concerned, work without desire is nonsense **and simagre**, just as much as making love without desire. To say true, I haven't experienced the temptation to waste my energy pretending to do something I have no desire to do, when there are so many exciting things to do, if only to sleep (and dream. . . ) when it's time to sleep.

It was on this very night, I believe, that I understood that the **desire** to know and the **power** to know and discover are one and the same thing. If we trust it and follow it, it is desire that leads us to the heart of the things we wish to know. And it's desire that makes us find, without even having to look for it, the most efficient method of knowing these things, and the one that suits us best. In the case of mathematics, it would seem that writing has always been an indispensable means, regardless of who is "doing math": doing math is above all **writing**<sup>7</sup> (33). The same is undoubtedly true of any work of discovery in which the intellect plays a major role. But surely this is not necessarily the case with "meditation", by which I mean the work of self-discovery. In my case, however, and up to now, writing has been an effective and indispensable means of meditation. As in mathematical work, it is the material support that fixes the

<sup>6</sup>(32) **A hundred irons in the fire, or: nothing is worth drying!**

When I was still doing Functional Analysis, i.e. up until 1954, I would sometimes persist endlessly on a question that I couldn't solve, even though I had no more ideas and was content to go round in circles with old ideas that obviously didn't "bite" any more. This was the case, in any case, for a whole year, notably for the "approximation problem" in topological vector spaces, which would only be solved some twenty years later by methods of a totally different order, which could only have escaped me at this point. I was driven then, not by desire, but by stubbornness, and by an ignorance of what was going on inside me. It was a painful year - the only time in my life when doing math had become painful for me! It took that experience to make me realize that there's no point in "skipping" - that once a piece of work has reached a standstill, and as soon as you've realized it, you have to move on to something else - even if it means coming back to the question at hand at a more propitious moment. This moment almost always comes quickly - the question matures, without me even pretending to touch it, simply by virtue of having worked enthusiastically on questions that may seem to have nothing to do with this one. I'm convinced that if I persisted, I wouldn't get anywhere even in ten years! It was from 1954 onwards that I got into the habit in maths of always having many irons in the fire at the same time. I only work on one of them at a time, but by a kind of miracle that constantly renews itself, the work I do on one also benefits all the others, which are waiting for their time. The same has been true, without any deliberate intention on my part, from my first contact with meditation - the number of burning questions to be examined has increased day by day, as reflection has continued. ...

<sup>7</sup>(33) **"Youth snobbery", or the defenders of purity**

This does not mean that moments when paper (or the blackboard, which is a variant of it!) is absent are not important in mathematical work. This is especially true in the "sensitive moments" when a new intuition has just appeared, when it's a question of "getting to know" it in a more global, more intuitive way than by "working on parts", which this informal stage of reflection prepares. In my case, this kind of reflection takes place mostly in bed or out for a walk, and it seems to me that it accounts for a relatively modest proportion of total work time. The same observations apply to meditation work as I've practised it up to now.



It gives us a tangible record of the work we've just done, to which we can refer at any time. Writing also gives us a tangible trace of the work we've just done, to which we can refer at any time. In long-term meditation, it's often useful to be able to refer back to written traces that bear witness to a particular moment of meditation in the days before, or even years before.

Thought, and its meticulous formulation, therefore play an important role in meditation as I've practiced it up to now. It is not, however, limited to the work of thought alone. Thought alone is powerless to grasp life. It is effective above all in detecting contradictions, often enormous to the point of grotesqueness, in our vision of ourselves and our relationships with others; but it is often not enough to grasp the meaning of these contradictions. For those driven by the desire to know, thought is an often useful and effective, even indispensable, tool, as long as we remain aware of its limitations, which are quite obvious in meditation (and more hidden in mathematical work). It is important that p. 95 thought knows how to fade away and disappear on tiptoe at sensitive moments when something else appears - perhaps in the form of a sudden, profound emotion, while the hand perhaps continues to run over the paper to give it a clumsy, stammering expression at the same time. ...

## 9.5. (37) Wonder

This retrospective on the discovery of meditation came here entirely unexpectedly, almost out of the blue - it wasn't at all what I set out to examine when I began. I wanted to talk about **wonder**. This night, so rich in so many things, was also rich in wonder at these things. Already in the course of the work, there was a kind of incredulous wonder at each new red herring uncovered, like a crude costume sewn from coarse white thread that I had been willing - it was scarcely believable! to take for real the most seriously in the world! Many times since then, in the years that followed, I've rediscovered the same sense of wonder as on that first night of meditation, at the enormity of the facts I was discovering, and the crudeness of the subterfuges that had made me ignore them until then. It was through its burlesque sides that I began to discover the unsuspected world I carry within me, a world that over the days, months and years has revealed itself to be prodigiously rich. On that first night, however, I had more to marvel at than vaudeville episodes. It was the night when, for the first time, I reconnected with a forgotten power that lay dormant within me, the nature of which still escaped me, except that it is a power, and which is at my disposal at any moment.

And the preceding months had already been rich in a mute wonder of something I'd been carrying inside me, probably for as long as I could remember, with which I'd only just made contact again. I felt this thing not as a power, but rather as a secret sweetness, as a beauty that was both peaceful and unsettling. Later, in the exultation of discovering my long-ignored power, I forgot those months of silent gestation, to which only a few scattered poems bore witness - love poems, that is to say, poems of love. perhaps would have detonated most often in the midst of my meditation notes. ...

It was only years later that I remembered those times d'émervaillement en la beauté p. 96 of the world and the one I felt resting inside me. I knew then that this gentleness and beauty I had felt within me, and this power I discovered shortly afterwards that profoundly changed my life, were two inseparable aspects of one and the same thing.

And now I can also see that the gentle, collected, silent aspect of this multiple thing that is creati-

vity in us, spontaneously expresses itself in wonder. And it's also in the wonder of an indi- target beauty revealed by the beloved, that man knows the beloved woman and she knows him. When wonder in the thing explored or in the being loved is absent, our embrace with the world is mutilated of the best that is in it - it is mutilated of what makes it a blessing for oneself and for the world. The embrace that is not wonder is a powerless embrace, a mere reproduction of a gesture of possession. It is powerless to engender anything other than yet more reproductions, bigger or fatter or thicker perhaps, who cares, never a renewal<sup>8</sup> (34). It's when we're children and ready to marvel at the beauty of things in the world and in ourselves, that we're also ready to renew ourselves, and ready as supple, docile instruments in the hands of the Worker, so that by His hands and through us, beings and things may perhaps be renewed.

I well remember that, in the informal group of friends who, for me, represented the mathematical milieu in the late forties and beyond - a milieu that was sometimes noisy and self-assured, and where a somewhat peremptory tone was not uncommon (but without any hint of smugness) - there was always room for wonder. The one in whom wonder was most visible was God-given. Whether he was giving a talk, or simply listening, when the crucial moment came and a sudden breakaway opened up, Dieudonné would be seen beaming and ecstatic. It was pure, infectious, irresistible wonder - where all traces of "me" had disappeared. As I recall him now, I realize that this wonder itself was a power, that it exerted an immediate action all around him, like a radiance of which he was the source. If I've ever seen a mathematician use a powerful and elementary "power of encouragement", it's him! I've never thought about it before, but I remember now that it was also in this way that he had already welcomed

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<sup>8</sup>(34) **The powerless embrace**

The word "embrace" is by no means a mere metaphor for me, and the common language here reflects a profound identity. It could be said, not without reason, that it's not true then that embrace without wonder is powerless - that the earth would be depopulated if not deserted, if it were so in the literal sense. The extreme case is that of rape, from which wonder is certainly absent, while it happens that a being is procreated in the raped woman. Of course, the child born of such an embrace cannot fail to bear the mark of this embrace, which will be part of the "package" he or she receives and must assume; but this does not prevent a new being from being conceived and born. that there has been **creation**, a sign of **power**. And it's also true that it happens that a mathematician I've seen, filled with sufficiency, finds and proves beautiful theorems, signs of an embrace that has not lacked force! But it's also true that if a mathematician's life is suffocated by his sufficiency (as was to some extent the case in my own life, at one time), the fruits of his embraces with mathematics are a blessing to him and to no-one else. And the same can be said of the father and mother of a child born of rape. When I speak of an "embrace without force", I mean above all the powerlessness to engender **renewal** in the person who believes he is creating, when in fact he is only creating a **product**, something external to him, with no deep resonance within himself; a product which, far from liberating him, creating harmony within him, binds him more closely to the fatuity within him of which he is a prisoner, which ceaselessly pushes him to produce and reproduce. This is a form of impotence at a deep level, behind the appearance of "creativity" which is basically just unbridled **productivity**.

I've also had ample opportunity to realize that **complacency**, the inability to marvel, is in the nature of a true blindness, a blockage of natural sensitivity and flair; a blockage that, if not total and permanent, is at least manifest in certain situations. It's a state in which a prestigious mathematician sometimes reveals himself, in the very things in which he excels, to be as stupid as the most stubborn of schoolchildren! On other occasions, he will perform prodigious feats of technical virtuosity. I doubt, however, that he is yet in a position to discover the simple and obvious things that have the power to renew a discipline or a science. They are far too far below him for him to deign to see them! To see what no one deigns to see, he needs an innocence that he has lost, or banished. . . It's no coincidence, surely, with the prodigious increase in mathematical production over the last twenty years, and the bewildering profusion of new results that overwhelm the mathematician who simply wants to "keep up to date", that (as far as I can judge from the echoes that reach me here and there) there has hardly been any real **renewal**, any far-reaching transformation (and not just by accumulation) of any of the major themes of thought with which I have been even remotely familiar. Renewal is not a quantitative thing, it is foreign to a quantity of investment, measurable in a number of mathematician-days devoted to a given subject by such and such mathematicians of such and such a "level". A million mathematician-days is powerless to give birth to something as childlike as the zero, which has renewed our perception of number. Only innocence has this power, a visible sign of which is wonder. ...

my very first results in Nancy'—resolving questions he had posed with Schwartz (on spaces  $(F)$  and  $(LF)$ ). The results were modest, nothing great or extraordinary, and nothing to marvel at. Since then, I've seen things of a completely different scale dismissed by the unapologetic disdain of colleagues who think they're great mathematicians. Dieudonné was in no way encumbered by such pretensions, justified or not. There was nothing of the sort that prevented him from being delighted even by the little things.

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There is a **generosity** in this capacity for delight, which is a benefit for those who are willing to let it blossom within themselves, as well as for those around them. This benefit is exercised without the intention of pleasing anyone. It's as simple as the fragrance of a flower, or the warmth of the sun.

Of all the mathematicians I've known, it was in Dieudonné that this "gift" appeared to me in the most dazzling, communicative and perhaps even active way, I can't say<sup>9</sup> (35). But in none of the mathematician friends I've enjoyed making friends with was this gift absent. It found occasion to manifest itself, perhaps in a more restrained way, at any time. It manifested itself every time I came to one of them to share something I'd just found and which had enchanted me.

If I've experienced frustration and sorrow in my life as a mathematician, it's above all in not finding, in some of those I've loved, that generosity I'd known in them, that sensitivity to the beauty of things, "small" or "great"; as if what had made the life of their being quiver had died out without a trace, smothered by the smugness of someone for whom the world is no longer beautiful enough for him to deign to rejoice in it.

Of course, there has also been that other pain, of seeing one of my friends of yesteryear treat another of my friends of today with condescension or contempt. But this pain is inflicted by the same closure, deep down. He who is open to the beauty of a thing, however humble, when he has felt that beauty, cannot help but also feel a respect for the one who conceived or made it. In the beauty of a thing made by the hand of man, we feel the reflection of a beauty in the one who made it, of the love he put into making it. When we feel this beauty, this love, there can be no condescension or disdain in us, any more than there can be condescension or disdain in us.

cannot be condescension or disdain— for a woman, in a moment when we feel her beauty, and the power within her, the sign of which is this beauty.

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## 9.6. (38) Return impulse and renewal

The rapture that radiated from time to time in Dieudonné's person must have touched something deep and strong within me, so that the memory comes back to me now with such intensity, such freshness, as if I'd just witnessed it again just now. (Although it's been almost fifteen years since I've had the opportunity to meet Dieudonné, except once or twice on the spur of the moment). Of course, I didn't pay any particular attention to it on a conscious level - it was just a slightly touching, at times almost comical, peculiarity of my colleague's expansive personality. What was important to me, however, was to have found in him the perfect collaborator, a dream collaborator I might say, to lay down in black and white, with meticulous care, loving care, what was to serve as the foundation for the vast perspectives I saw opening up before me. It is only at this moment when I evoke both of them that

<sup>9</sup>(35)

This "gift" is nobody's privilege; we're all born with it. When it seems absent in me, it's because I've driven it out myself, and it's up to me to welcome it back. In me or in such-and-such a person, this "gift" expresses itself in a different way than in another, less communicative, less irresistible perhaps, but it's no less present, and I couldn't say if it's less active.

the link suddenly appears to me: what made Dieudonné the ideal servant for a great task, whether within Bourbaki or in our collaboration on another great foundation project, was his **generosity**, the absence of any trace of vanity, in his work and in the choice of his major investments. I have constantly seen him take a back seat to the tasks he has made himself the servant of, lavishing his inexhaustible energy on them without seeking any return. There's no doubt that, without looking for anything in return, he found in his work and in the very generosity he put into it a fulfillment and blossoming that all those who knew him must have felt.

The rapture of discovery that I have so often felt radiating from her person, is immediately associated in me with a similar rapture, which I happened to witness in a very young child. Two memories rush through my mind - both of which take me back to my very young daughter. In the first picture, she must be a few months old, just starting to crawl. She must have dragged herself from the patch of grass where she'd been sitting to a gravel path. She discovered the little gravels in silent ecstasy - and in action, grabbing them with her hands and putting them in her mouth!

p. 99 In the other picture she must have been a year or two old, someone had just thrown pellets into a goldfish bowl. □ The fish swam towards them, mouths wide and mouths wide. open, to swallow the tiny yellow crumbs suspended in the air as they slowly sank into the water. The little girl had never realized before that fish eat the way we do. It was like a sudden dazzle, expressed in a cry of pure delight: "Look, Mommy, **they're eating!** There was indeed much to marvel at - she had just discovered, in a sudden flash, a great mystery: that of our kinship with all other living beings. ...

There's a communicative force in the delight of a little child that eludes words, a force that radiates from him and acts on us, even though we do our best, more often than not, to evade it. In moments of inner silence, we feel this force present in the child at all times. Only at certain moments is its action stronger than at others. It's in newborn babies, in the first days and months of life, that this kind of "force field" around the child is most powerful. More often than not, it remains sensitive throughout childhood, unravelling over the years until adolescence, when there often seems to be no trace left. Yet it can be found radiating around people of all ages, at privileged moments for some, or for others as a kind of breath or halo that surrounds them at all hours. I had the great good fortune to know such a person in my childhood, a man who has now passed away...

I'm also thinking of that other force, or power, that we sometimes feel radiating from a woman, especially at times when she's fulfilled in her body, in communion with it. The word that often comes to mind is "beauty", which evokes one aspect of it. It's a beauty that has nothing to do with canons of beauty or so-called "perfection"; it's not the privilege of youth or maturity. Rather, it's the sign of a profound accord within the person. And yet, it manifests itself in this radiance, a sign of power. It's a force that draws us back to the center from which it emanates - or rather, it calls forth within us a profound impulse to **return** to the body of Mother Woman from which we emerged, at the dawn of our lives. Its action is sometimes irresistibly powerful, overwhelming when it emanates from the woman aimée. But for those who don't deliberately close themselves off to it, elle is perceptible in every woman who allows her inner self to blossom. that beauty, that deep harmony.

p. 100 The force that radiates from the child is closely related to the force that emanates from the woman who loves herself within her body. The one is constantly born of the other, just as the child is constantly born of the Mother. But the nature of the force of childhood is not one of attraction, nor is it one of repulsion. Humble action

that this force exerts on those who do not shrink from it, is an action of **renewal**.

## 9.7. (39) Belle de nuit, belle de jour (or: the stables of Augias)

The memory of wonder in one of my children dates back to the very late fifties and early sixties. If I don't have a similar recollection for other children who were born later, it may be that my own capacity for wonder had dulled, that I had become too distant to commune in the delight of one of my children, or to witness it at all.

I've never yet thought of tracking the vicissitudes of this ability in my life, from childhood to the present day. Surely there would be a common thread here, a "detector" of great sensitivity. If I've never thought of following this thread, it's surely because this ability is of such a humble nature, almost insignificant in appearance, that it would hardly have occurred to me to pay any particular attention to it, absorbed as I was in discovering and probing what I used to call "the great forces" in my life (which continue to manifest themselves even today). And yet, this ability to look so humble provides a sign, of all signs, of the presence or absence of the rarest and most precious "force" within us... .

Throughout my adult life, I was never entirely cut off from this force. However arid my life may have become, I found in love the wonder of the child, the rapture of discovery. Through many deserts, the passion of love remained the living, vigorous link with something I'd left behind, an umbilical cord that silently continued to nourish me with warm, generous blood. And for a long time, too, wonder in the woman I loved was inseparable from wonder in the new beings she gave birth to - those brand-new, infinitely delicate, intensely alive beings who and inherited its power.

□ But my purpose here is above all to follow the vicissitudes of this "force of innocence" through my life as a mathematician, when I was part of the "world of mathematicians", from 1948 to 1970. Surely, wonder has never permeated my mathematical passion to the extent that it does the passion of love. Strangely enough, if I try to recall a particular moment of rapture or wonder in my mathematical work, I can't find any! My approach to mathematics, ever since the age of seventeen when I first became deeply involved in it, has been to set myself big **tasks**. Right from the start, they were always tasks of "tidying up", of cleaning up. I saw an apparent chaos, a confusion of heterogeneous things and sometimes imponderable mists, which obviously had to have a common essence and conceal an order, a hidden harmony that had to be brought out through patient, meticulous and often lengthy work. It was often mop and broom work, for the big jobs that already absorbed a considerable amount of energy, before coming to the finishing touches with a feather duster, which I was less passionate about, but which also had their charm and, in any case, an obvious usefulness. There was an intense satisfaction in the day-to-day work, in seeing little by little the order we had guessed at emerge, which always turned out to be more delicate, with a richer texture than what had been glimpsed and guessed at. The work was constantly rich in unforeseen episodes, most often arising from the examination of what might have seemed an infinitesimal detail that had been neglected until then. Often, the fine-tuning of such a "detail" threw unexpected light on work done years before. Sometimes, too, it led to new intuitions, the deepening of which became the object of another "big task".

So, in my mathematical work (apart from the "difficult year" around 1954 that I've already mentioned), there was constant suspense, and my attention was constantly held in suspense. Fidelity to my "tasks", moreover, prevented me from escaping too far, and I gnawed at my brakes in an impatience to get on with my work.

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I'd come to the end of them all, and set off into the unknown, the real unknown - even though the scale of these tasks had already become such that even with the help of the good people who'd eventually come to the rescue, the rest of my days wouldn't have been enough to see them through!

p. 102 My main guide in my work was the constant search for a perfect coherence, a complete harmony that I guessed behind the □ turbulent surface of things, and that I patiently strove to draw out. never tired of it. It was surely an old-fashioned sense of "beauty" that was my flair and my only compass. My greatest joy was not so much to contemplate it when it appeared in full light, as to see it gradually emerge from the cloak of shadow and mist in which it was constantly hiding. Of course, I didn't stop until I'd succeeded in bringing it into the clearest light of day. Sometimes, then, I experienced the fullness of contemplation, when all audible sounds contribute to a single, vast harmony. But even more often, what was brought into the light of day immediately became the motivation and the means for a new plunge into the mists, in pursuit of a new incarnation of the One who remained forever mysterious, unknown - constantly calling out to me, to know Her again... .

Dieudonné's pleasure and delight, it seems to me, was above all in seeing the beauty of things manifested in full light, and my joy was above all in pursuing it in the obscure recesses of mists and night. This is perhaps the profound difference between Dieudonné's and my approach to mathematics. The sense of the beauty of things, for a long time at least, must have been no less strong in me than in Dieudonné, although it may have dulled during the sixties, under the action of a fatuity. But it would seem that my perception of beauty, which manifested itself in Dieudonné's sense of wonder, took on different forms in me: less contemplative, more enterprising, less manifest in the emotion I felt and expressed. If this is the case, then my aim is to follow the vicissitudes of my openness to the beauty of mathematical things, rather than the mysterious "gift of wonder".

## 9.8. (40) Mathematics sport

It's quite clear that openness to the beauty of mathematical things never entirely disappeared in me, even in the sixties until 1970, when fatuity gradually took a growing place in my relationship to mathematics and to other mathematicians. Without a modicum of openness to the beauty of things, I would have been incapable of "functioning" as a mathematician, even at my best.

p. 103 modest - and I doubt that anyone can do any useful work in mathematics, if this sense of beauty doesn't remain alive in them, even a little. It is not so much, it seems to me, a pretended "brain power" which makes the difference between one mathematician and another, or between one work and another by the same mathematician; but rather the quality of finesse, of greater or lesser delicacy of this openness or sensitivity, from one researcher to another or from one moment to another in the same researcher. The most profound, the most fruitful work is also that which attests to the most delicate sensitivity for apprehending the hidden beauty of things<sup>10</sup> (36).

If that's the case, then I must believe that this sensitivity must have stayed with me right to the end, at times all the way through to the end.

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<sup>10</sup>(36)

Such a delicate sensitivity to beauty seems to me intimately linked to something I've had occasion to refer to as "exigency" (with regard to oneself) or "rigor" (in the full sense of the word), which I described as "attention to something delicate in ourselves", attention to a quality of understanding of the thing probed. This quality of **understanding** of a mathematical thing cannot be separated from a more or less intimate, more or less perfect perception of the "beauty" particular to that thing.

at least, since it was at the end of the sixties<sup>11</sup> that I began to catch a glimpse of the most hidden and mysterious mathematical thing I had ever discovered - the thing I called "motif". It is also the thing that has held the greatest fascination for me in my life as a mathematician (apart from certain reflections in recent years, which are intimately linked to the reality of patterns). There's no doubt that if my life hadn't suddenly taken an entirely unexpected course, taking me far away from the serene world of mathematical things, I'd have ended up following the call of this powerful fascination, leaving behind the "tasks" that had until then held me captive!

Perhaps I can say that, in the solitude of my work room, my sense of beauty remained unchanged until my first "awakening" in 1970, unaffected by the fatuity that so often marked my relations with my fellow creatures? A certain "flair" must even have been refined over the years, through daily, intimate contact with mathematical things. The intimate knowledge we can have of things, which sometimes enables us to apprehend beyond what we know in the moment and penetrate further into knowledge - this knowledge or maturity, and this "flair" which is its most visible sign, is closely related to openness to the beauty and truth of things. It fosters and stimulates such openness, and is the sum and fruit of all the moments of openness, all the "moments of truth" that have gone before.

What remains for me to examine, then, is the extent to which a spontaneous sensitivity to beauty has been disturbed to a greater or lesser degree, at times when it had had the opportunity to manifest itself in my relationship with this or that colleague.

□ That which memory delivers to me on this subject is not condensed into a tangible, precise fact, which I could here

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The memory here again is limited to a kind of fog. Here again, memory is limited to a kind of fog, which nevertheless gives me an overall impression that I must try to pin down. It's the impression left on me by a certain **inner attitude**, which must have become like second nature, and which manifested itself every time I received mathematical information about something that was more or less "up my alley". To tell the truth, in some relatively innocuous way, this attitude must have always been mine, it's part of a certain temperament, and I've had occasion to touch on it in passing. It's about this reflex, of first agreeing to take cognizance only of a **statement**, never of its demonstration, to try first of all to situate it in what is known to me, and to see if in terms of what is known the statement becomes transparent, obvious. This often leads me to reformulate the statement in a more or less profound way, in the sense of greater generality or greater precision, often both at the same time. It's only when I can't "fit" the statement in terms of **my** experience and images, that I'm ready (almost unwillingly at times!) to listen to (or read. . . ) the ins and outs that sometimes give "the" reason for the thing, or at least a demonstration, understood or not.

This is a peculiarity of my approach to mathematics, which, it seems to me, set me apart from all the other Bourbaki members when I was part of the group, and made it virtually impossible for me to fit into a group effort like them. This peculiarity must also have been a handicap in my teaching activity, a handicap that must have been felt by all my students until today, when (with the help of age) it has softened somewhat.

This trait in me is surely already indicative of a lack of openness. It implies only a partial openness, ready to welcome only what "comes to the point", or at least very reticent in welcoming everything else. In the choice of my mathematical investments, and the time I'm willing to devote to

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<sup>11</sup>(August 8) Once I've checked, it's clear that I started thinking about motifs at the beginning, not at the end, of the sixties.

this or that unexpected information, this deliberate intention of "partial closure" is today stronger than ever. It is even a necessity, if I am to be able to follow the call of what fascinates me ☐le most, without yet giving "my life to devour" to lady mathematics!

However, the "fog" gives me more than just this particularity, which I came to realize a few years ago (better late than never!). At a certain point, this reflex became like a **point of honor**; it would be the devil if I didn't manage to "get" this statement (assuming it wasn't already quite familiar to me) in less time than it takes to say it! If it were an illustrious stranger who was the author of the statement, there'd be the added nuance that **I** (who'm supposed to be in the know, after all!) wouldn't already have all that up my sleeves! And very often I did have it, and more - my attitude then would have tended to go along the lines of: "Well, you can go and get dressed - you'll come back when you've done a bit better!"

That was precisely my attitude in the case of the "young white boy who was stepping on my toes". I couldn't even swear that in what he was doing, there weren't interesting details that weren't covered by what I'd done in my "secret notes" - which is incidental<sup>12</sup> by the way. Finally, this episode also sheds light on the question I'm examining here: that of a profound disturbance in this openness to the beauty of mathematical things. It was as if, from the moment I had "done" such and such a thing, its beauty had disappeared for me, and all that remained was a vanity that claimed credit and profit. (Though I didn't deign to take the time to publish it - admittedly, there would have been too much of that.) It was a typical attitude of possession, analogous to that of a man who, having known a woman, no longer feels her beauty and runs after a hundred others without suffering for all that that another knows her. It was an attitude I disapproved of in love life, believing myself to be far above such vanity, while being careful not to notice the obvious fact that this was indeed my attitude towards mathematics!

I have a feeling that these crude competitive dispositions, "sporting" dispositions if you like, which I've just put my finger on in my person, must have started to become common in "my" mathematical milieu, around the time they were common in me. I'd be hard pressed to situate in time the moment of their appearance, or the one when they became like an intimate part of the air we breathed in this <sup>mi</sup>☐lieu, or the one my students breathed in contact with my person. The only thing I can say is that it must have taken place in the sixties, perhaps as early as the early sixties, or the late fifties. (If so, all my students were entitled to it - take it or leave it!) To place it, I'd need other specific cases, which at the moment escape my memory entirely.

This humble reality was, of course, in complete contrast to the noble image I had of my relationship with mathematics, and with young researchers in general. The crude subterfuge I used to fool myself was meritocratic in inspiration: for this image, all I retained was the relationship with my students (who contributed to my prestige, of which they were the noblest jewels!), and to particularly brilliant young mathematicians, whose merits I had recognized and whom I treated on an equal footing, just like my students, without waiting for their heads to be crowned with laurels (which of course didn't take long - you either have "flair" or you don't!). As for the youngsters who didn't happen to be among my pupils, or among those of a friend of mine, nor to be young geniuses, I didn't care what my relationship with them was. **They didn't matter.**

I think that this reality was more often than not softened, tempered, when I found myself in a relationship with a person.

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<sup>12</sup>(August 8) It has since dawned on me that this thing is not so "incidental" as all that, that it constitutes the dividing line between "sporting attitude" and the beginnings of dishonesty, a line which I may have crossed... ..



I had a personal relationship with the young researcher, either because I met him at my seminar, or because he wrote to me. It may be that the case of the "young white boy" is, from this point of view, something of an exception. It seems to me that, in the case of the researchers I've just mentioned, I must have considered them as having put themselves "under my protection", and this must have awakened in me a more benevolent attitude. In this case too, my desire to put myself forward could find an outlet, by giving my comments to the person concerned and making suggestions for taking up his work in a broader perspective perhaps, or by getting to the bottom of things. In such a case, there's a good chance that the young researcher, who for a limited time took on the role of pupil, would also find something to enjoy, and that he would have fond memories of his relationship with me. (Any feedback on this subject would be most welcome).

I was thinking here mainly of the case of younger researchers, when the "sporting" attitude was by no means p. 107 limited to my relationship with them, needless to say. But it's certainly in my relationship with young researchers that the impact, both psychological and practical, of a mathematician in the limelight tends to be strongest, with the most far-reaching consequences for their future professional lives.

## 9.9. (41) Krishnamurti, or liberation as hindrance

I stopped last night with a feeling of relief, of great satisfaction, the contentment of someone who hasn't wasted his time! I suddenly felt light and joyful - a slightly mischievous joy at times, bursting into mischievous laughter - the laughter of a joking brat. And yet I hadn't actually done anything, I'd just watched an episode I'd already "known", that of the famous "white boy who . . ." from a slightly different angle. An angle showing **my relationship to mathematics itself**, in certain circumstances, not just my relationship to mathematicians. That's all it took for a cherished myth to go up in smoke.

To tell the truth, this isn't the first time I've looked at my relationship with mathematics. Two and a half years ago, I had already devoted a few weeks or months to it. At that time, I had realized (among other things) the importance of egotistical, self-aggrandizing forces in my past investment in maths. But last night I had just put my finger on an aspect that had escaped me at the time. Now that I'm coming back to it, I realize that this aspect, the aspect of **the jealous attitude** in my relationship to maths, is in line with the "simple" discovery that came at the end of the first night when I "meditated" (meditating without knowing it, like Monsieur Jourdain writing prose. . .). It's quite possible that this had something to do with the joyful exultation that followed. Even if it wasn't consciously perceived, it was a bit like the reconfirmation, in a new light, of something I'd discovered earlier - and the pleasure then is the same as in mathematics, when without having looked for it you come across, by an entirely different means, something you know, that you've found perhaps years before. Each time, it's accompanied by a feeling of intimate satisfaction, as the harmony of things is revealed once again, and our knowledge of them is more or less renewed.

De plus, I think I've really "done the trick" this time! I'd had a feeling for days that he p. 108 there was still something to bring to light, though I couldn't say exactly what. I didn't try to force it, I felt I just had to let it happen, letting the thread I was following unfold freely, through landscapes that were both familiar and unexpected. Unexpected, because until now I'd never bothered to look at them. At a walking pace, I approached the remaining "hot spot". And I think it's the last one, in the journey I've just made, which is coming to an end.

And as soon as I reached this point, I had the impression of someone arriving at a belvedere, from where he can see the whole world unfold.

landscape he has just traversed, of which at any given moment he could still only perceive a portion. And now there's this perception of expanse and space, which is a liberation.

If I try to put into words what the landscape in front of me delivers to me, it comes to this: everything that has come to me, and often unwelcome and unwelcome, in my life as a mathematician in recent years, is the harvest and message of what I sowed, back when I was part of the world of mathematicians.

Of course, I've said this to myself over and over again over the years, and even in the notes I've just written. I've said it to myself, somewhat by analogy with other harvests that have come to me insistently, that I've long rejected and that I've ended up welcoming and making my own. From the very first one I accepted, even before I knew anything about meditation, I understood that every harvest had to have a meaning, and that to balk at it was only to evade a meaning and put off the deadline for a denouement. This knowledge has been precious to me, for it has often kept me from self-pity, and from the righteous indignation that is often a disguised form of it. This knowledge is in me like a half-maturity, which by no means yet puts an end to the inveterate reflex of refusing harvests when they seem bitter. When I say to myself "there's no point in begrudging", the harvest is not welcomed for all that. I don't pity myself or perhaps feel indignant, and yet I "balk"! Until the dish is eaten, it is not welcomed - and not to eat is to begrudge.

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To host and eat is a **job**: a certain energy  $\square$  "works", a job is done in broad daylight or in the shade, something is transformed... . Whereas reluctance is a waste of energy that is dispersed - to "begrudge"! And you can't do without the work of eating, digesting and assimilating. The mere fact of going through events, of "doing" or "acquiring" an experience, has nothing in common with work. It's simply possible **material** for a job that you're free to do, or not to do. In the thirty-six years since I first encountered the world of mathematicians, I've made use of the freedom I have by **avoiding** work, while the material, the substance to be eaten and digested, has increased year by year. The feeling of joyful liberation I've been experiencing since yesterday is surely a sign that the work I've been putting off in favor of other work or tasks has finally been done. It's about time indeed!

It's still too early to be sure that this is indeed the case, that there isn't some stubbornly obscure corner that has escaped my attention, to which I'll have to return. But it's also true that this feeling of liberation doesn't deceive - every time I've felt it in my life, I've been able to see afterwards that it was indeed a sign of **liberation**; of something lasting, acquired, the fruit of an understanding, a knowledge that has become a part of myself. I am free, if I please, to ignore this knowledge, to bury it where and how I please. But it is not in my power or anyone else's to destroy it, any more than you can destroy the ripeness of a fruit, make it return to a state of greenness that is no longer its own.

It's a great relief to have it confirmed, once again, that I'm not "better" than the others. Of course, this too is something I repeat to myself quite often - but **repeating** and **seeing** are definitely not the same thing! Lacking the innocence and mobility of a child who sees as he breathes, it often takes work to see the obvious - and now I've finally **seen** it: I'm not "better" than certain colleagues or ex-students who, just a few days ago, were "taking my breath away"! Judge the weight I've been relieved of! Perhaps it's gratifying in a way to think oneself better than others.

But it's also very tiring. It's an extraordinary waste of energy even - as it is every time you have to maintain a fiction. It  $\square$  rarely occurs to us, but it already takes energy, just

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to maintain the fiction against all odds, while the evidence at every step proclaims in my carefully plugged ears that it's all fake, so look stupid! Maybe it's a job sometimes to see,

but when it's done, it's done. It saves me, once and for all, having to walk around plugging my eyes and ears all the time - that's got to be done too! and having to suffer an intolerable outrage every time something falls on me that I've inadvertently put there.

Fed up with the ride! Once you've seen the merry-go-round, you're already off it. You've paid, okay, I've got the right to go for life on it, and even the duty to do so, as everyone will tell me: right, duty - it's up to the customer. It's very tiring too, all these rights that are duties and all these duties that are rights, which stick to me when I think I'm better than the others. It's normal after all, when you're better, you cash in discreetly (that's "rights") and you "pay", you do your duty for the honor of the human spirit and mathematics - it's very beautiful, it's true, honor, spirit, mathematics, who could say it better, bravo! bis! It's all very beautiful, but it's also very tiring, and ends up giving you a stiff neck. I've had my torticollis and now that's enough - I leave room for others to stand stiffly.

It's also normal (since I was talking about students) for the student to surpass the teacher. I was offended by it, I had energy to waste! No more of that!

What a relief!



# 10. L'Enfant s'amuse

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### 10.1. (42) The child

In fact, there must be corners where the broom hasn't been. Never mind, they'll come to my attention, and I'll always have time to take care of them. But as for my famous "mathematician past", the big clean-up is done, no doubt about it.

Now that I've seen once again that I'm no better than the others, I shouldn't I don't want to fall back into the same old trap of thinking I'm **better than I am**! To think I'm better **now**, off the merry-go-round and all, than I was fifteen years ago, or fifteen years ago, or fifteen years ago. days. I learned something□chose during those fifteen years, that's for sure, and during the fortnight too and even p. 111 since yesterday. When I learn something I mature, I'm not quite the same. I'm not "better" when I've learned something, than when the thing to be learned was still in front of me. A riper fruit is not "better" than a less ripe or green one. A season is not "better" than the one before it. The taste of the ripest fruit may be more pleasant, or less pleasant, depending on taste. I feel better about myself from one year to the next, so I guess the changes in me are "to my liking" - but they're not to the liking of all my friends and family. Every time I go back to doing maths, I get compliments from all sides, like: "What an idea he had to do something else! Everything's back in order, it's about time! It's worrying to see someone change... .

I learn, I mature, I change - so much so that sometimes I find it hard to recognize myself in the person I was and am rediscovering, through a memory or the unexpected testimony of others. I change, and there's also something that remains "the same". It's always been there, probably since birth, and maybe even before. I seem to have come to recognize it quite well in recent years. I call it "the child". By this thing, I'm no better at this moment than at any other time in my life; he was there, even if it would often have been difficult to guess his presence. By this thing too, I'm no better than anyone else, and no one else is better than me. In certain moments or in certain people, the child is more present. And that's a very good thing. It doesn't mean that someone is "better" than someone else, or than themselves at another moment.

Often, when I'm doing math, or making love, or meditating, it's the child who's playing. He's not always the only one "playing". But when he's not there, there's no math, no love, no meditation. You don't have to pretend - and I've rarely pretended.

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It's not just the child, that's for sure. There's the "me", the "boss" or the "big boss", call him what you will. Surely the boss is indispensable to the running of the company. If there's a boss, it must be have something to do with it. He looks after the housekeeping, and like all bosses, he has an annoying tendency to become intrusive. He takes himself terribly seriously and wants at all costs to be better than the boss on the other side. face. Invasive or not, he's just the boss, not the worker. He organizes, he orders, and he cashes in, that's for sure! - He takes profits as his due, and suffers losses as an outrage. But he creates nothing. Only the worker has the power to create, and the worker is none other than the child.

It's rare to find a company where boss and worker get along. More often than not, the worker is nowhere to be seen, locked away in God-knows-where. It's the boss who has pretended to take his place in the workshop, with the results we can only guess at. And often, when the worker is actually there, the boss wages war on him, either violently or through skirmishes - not much comes out of the workshop! Sometimes, too, there's a wary tolerance in the boss towards the worker, and he lets him get on with it, grumbling, without taking his eyes off him. It's like a constantly renewed truce in a war that has never ceased. And the worker is able to get some work done, thanks to the truce.

It's not at all certain that, by virtue of the meditation I've just made, my possessive attitude towards mathematics has magically disappeared! At the very least, I'd have to take a much closer look at the manifestations of possessiveness, one of which I've just touched on by name. This is not the place for this "introduction", which has become an "introductory chapter", which in turn is already getting long! One thing did click last night, however, and I'd like to come back to it now, something I'd noted with some surprise two or three years ago.

I was working on a mathematical question, I don't know what it was, and at some point (through some circumstance) it occurred to me that the question I was looking at had perhaps already been looked at, that it might well be dealt with in black and white in some book that it was up to me to consult in the library. The mere mention of this possibility had an astonishing effect: from one moment to the next, the desire had disappeared. Suddenly, the question I'd perhaps spent weeks thinking about, and was prepared to spend weeks more, had lost all interest for me! It wasn't spite, it was a sudden and total lack of interest. If I'd had the book in my hands, I wouldn't have bothered to open it.

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In fact, the eventuality was not confirmed, and so the desire ☐reappeared and I continued on my way. as if nothing had happened. Still, I was taken aback. Of course, if I'd really **needed** what I was doing to do **something else**, there wouldn't have been such a dramatic drop in interest. I've often redone familiar things, knowing or suspecting that they were familiar, without giving them the slightest thought. At the time, I was on a path where it was more economical, and much more interesting, to do things my way, as they presented themselves to me, than to go digging through books or articles. I would do it "in the stride" towards something else, towards which the desire would carry me. And of course, I was "in the know" enough to know that what was at the end was not to be found in any book or article.

This reminds me that mathematical work, even if done in solitude for years on end, is **not** a purely personal, individual endeavor, as meditation is - at least not for me. The "unknown" that I pursue in mathematics, for it to attract me with such force, must not only be unknown to me, but unknown to everyone. What is written in mathematical books is not unknown, even if I myself have never heard of it. Reading a book or an article has never attracted me; I've avoided it whenever I could. What it can tell me is never the unknown, and I've never heard of it myself.

the interest I take in her doesn't have the quality of desire. It's an "interest" of circumstance, an interest in a **information** that can be useful to me, as an instrument of a desire of which it is in no way the object.

On reflection, it doesn't seem to me that the event I reported was the sign of a jealous, possessive disposition, the sign of a vanity that had been disappointed. There was no spite or disappointment in me, simply the sudden disappearance of a desire that, just the moment before, had been intense. This was at a time when I had absolutely no thought of publishing anything, nor of ever wanting to publish again. This desire was not an expression of vanity, of the craving to accumulate knowledge, titles and credits - it was a real desire, the desire of a child passionate about play. And suddenly - nothing! Understand who can, I don't. ... Sorry!

## 10.2. (43) The troublemaker boss - or the pressure cooker

□ I feel I have finally completed this retrospective of my life as a mathematician. Of course, I haven't not exhausted my subject - volumes would be needed, assuming such a subject could be "exhausted". That wasn't my point. My point was to get to the bottom of whether or not I'd been part and parcel of the appearance of a certain "air" that I now feel in puffs, and if so, in what way. Now I know for sure, and it feels good. It could be fascinating to go further, to go deeper into what has only been glimpsed or touched upon. There are so many exciting things to look at, to do, to discover! As far as my past as a mathematician is concerned, it seems to me that what I **needed** to look at, to take on that past, has been seen.

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Undoubtedly, as I went deeper into this meditation, I would learn many interesting things about my present. One thing that this work has made me feel almost every step of the way is the extent to which I have remained attached to this past, the importance it has had to this day in my self-image, and also in my relationship with others; especially in my relationship with those I have, in a certain sense, left behind. My relationship with this past has undoubtedly been transformed in the course of this work, in the sense of a detachment, or a greater lightness. Time will tell. But it's likely that an attachment will remain, as long as my mathematical passion is not burnt out and quenched - as long as I "do maths". And I don't care to guess or predict whether it will die out before I do... .

For more than ten years, I had believed this passion to be extinct. It would be truer to say that I had **decreed** it extinct. That was the day I stopped doing maths for a while, and rediscovered the world! For three or four years, I was absorbed in an activity so intense that my old passion couldn't have found the slightest gap to slip through to manifest itself. These were years of intense learning, at a certain level that remained rather superficial. In the years that followed, mathematical passion manifested itself in sudden, totally unexpected bursts. These would last a few weeks or months, and I would stubbornly ignore their clear meaning. I'd decided once and for all that my craving for maths, which was decidedly good for nothing, was now a thing of the past, period! But the "good-for-nothing" didn't hear it that way - and I remained deaf.

□ Which may seem paradoxical, it was after the discovery of meditation (in 1976), with the entry of a new passion in my life, that the reappearances of the old one were particularly strong, almost violent - as if each time a lid popped off under too much pressure. It was only five years later, under the pressure of events, to put it mildly, that I took the trouble to examine what was going on. It was the longest meditation I've ever done on a seemingly well-defined issue: it took me six months of intense, stubborn work to get around a kind of iceberg,

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the visible summit of which had finally become embarrassing enough to force me, almost unwillingly, to go and have a look. I was faced with a situation of **conflict**, which appeared to be the conflict of two forces or desires: the desire to meditate, and the desire to do maths.

In the course of this long meditation, I learned step by step that the desire to do math, which I treated with disdain, was, just like the desire to meditate, which I valued to the hilt, a child's desire. The child has nothing to do with the disdain or modest pride of the big boss! The child's desires follow one another, as the hours and days go by, like the movements of a dance arising from one another. Such is their nature. They are no more opposed than the stanzas of a song, or the successive movements of a cantata or fugue. It's the bad conductor who declares one movement "good" and another "bad", creating conflict where there is harmony.

After this meditation, the boss has calmed down and is less inclined to stick his nose where it doesn't belong. The work this time was long, whereas I thought it would be done in a few days. Once the work is done, the "result" seems obvious, and can be summed up in a few words<sup>1</sup> (37). But if someone had said these words to me before or during the work, it probably wouldn't have done me any good. If the work took so long, it's because the resistance was strong and deep-rooted. The boss got a kick out of it, and he never batted an eyelid, because it was all done in an atmosphere where there was no way he'd get angry. One thing's for sure, it was a busy six months, and I couldn't have done without it; any more than a woman can do without the nine months of pregnancy to finally give birth to something as "obvious" as a toddler.

### 10.3. (44) The steam is reversed.

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□ It's been a year and a half since I meditated, apart from a few hours in December, to clear up an urgent question. And it's been a year since I invested most of my energy doing maths. This "wave" came like the others, math waves or meditation waves: they come without announcing their arrival. Or if they do, I never hear them! The boss has a slight preference for meditation, it seems: each time the meditation-wave is already followed by a maths-wave, whereas I thought it would last forever; and the maths-wave, which (it seemed to me) was a matter of a few days or weeks at most, lingers on and extends over months and maybe even, who knows, years. But the boss has come to understand that it's not he who makes these rhythms, and that he has nothing to gain by trying to regulate them.

But perhaps there has finally been a shift in the boss's "little preference", since it's been almost a year since it was agreed and decided, that I'd be away for at least a few years "doing maths again", officially so to speak: I've even applied for a position at the CNRS! More importantly, and entirely unexpected a year ago, I'm back to publishing. Even after the 1981 meditation I mentioned earlier, when the urge to do maths ceased to be treated as a poor relation, the idea would never have occurred to me that I might start publishing maths again. At a pinch, a book about meditation, or dreams and the Dreamer - and even then, I was far too busy with what I was doing to publish anything about math.

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<sup>1</sup>(37)

I hardly need to add, I think, that this long-term work has brought out, day by day, much more than the "result" I've just delivered in lapidary form. It's no different for a work of meditation than it is for a mathematical work motivated by a particular question that we set out to examine. Very often, the twists and turns along the way (which may or may not lead to a more or less complete clarification of the initial question) are more interesting than the initial question or the "final result".



feel like writing a book about it! And for what?!

It was a rather important decision, one that would affect the course of my life for years to come, and one that was taken somewhat on the spur of the moment, I'm not even sure when and how. One day, when there began to be a good deal of typed notes (I'll say! until then

I had confined myself to handwriting my mathematical cogitations...<sup>2</sup> (38), on homotopic fields and models, etc. ... it turned out to be a done deal: we're publishing this! And while we're at it, we might as well pull out all the stops and start a little series of mathematical reflections, the name of which was a no-brainer - all we had to do was capitalize it: "Réflexions Mathématiques" (Mathematical Reflections)! That's more or less what I'm getting back in this :

the famous "fog" that so often takes the place of a memory. I'm sure it's a very abbreviated memory

□I occurrence. The remarkable thing, in any case, is that this thing was done without even a pause to **look at** where I was going, what was pushing me, what was carrying me... . That's what I'd like to do again, on the momentum of this unexpected meditation, so that I can feel it's really finished.

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The question that immediately springs to mind: is this "remarkable thing" I've just noticed a sign of the (so-called?) "discretion" of the boss, who wouldn't dream of interfering (even with an indiscreet glance . . . ) in such a beautiful spontaneous movement that has no need of him etc. . . ; or is it a sign, on the contrary, that he's taken sides outright, and that the so-called "little preference" is pushing him all the way in the maths direction?

It was enough to put the question in black and white for the answer to appear! It's not the kid, who's in for a longer game than most, perhaps, who's decided that he's going to continue for X number of years without a hitch, and wisely fill in as many pages as it takes to make a reasonable number of volumes in a beautiful series with capital letters! The boss has planned and organized everything, and all the kid has to do is get on with it. Maybe the kid won't ask for anything better - there's no way of knowing in advance - but that's an incidental question. The kid's desires depend, to a certain extent at least, on the **circumstances**, which depend above all on the boss.

The boss has clearly made up his mind. In fact, he's just shown a certain flexibility, since a meditation has been going on under his benevolent eye for over a month now. It's also true that his benevolence is by no means disinterested, since the tangible product of the meditation, the notes I'm currently writing, will be the most beautiful cornerstone of the tower he already sees himself building, with the stones gracefully cut by the apparently willing child-worker. Decidedly, it's a little early to be complimenting him on his "suppleness"! A few hours of meditation three months ago, and all in all a year from now! and a half, that's pretty thin!

Yet I don't have the impression that there was, during all this time □ a desire for meditation that would have been p. 118 repressed, frustrated. In the few hours in December, I took stock and saw what I had to see; that was enough to transform a situation, which hadn't been clear. I resumed the thread of the interrupted mathematical work, without having to cut short anything else. It doesn't seem to me that a conflict has reappeared, I mean, the one that had been resolved over two years ago and which would have reappeared this time in reverse form. It's in the boss's nature to have preferences, and that's his right - it would be silly for him to pretend not to (although sillier things happen than that. . . ). It's not a sign of conflict, even though it's often the cause of it. As things stand, there really doesn't seem to be any need to

<sup>2</sup>(38)

These notes were in fact a continuation of the long letter to... which became the first chapter. They were t y p e d so as to be legible for this old friend, and for two or three others (Ronnie Brown in particular) whom I thought might be interested. This letter, by the way, was never answered, nor was it read by the addressee, who almost a year later (when I asked him if he'd received it) expressed sincere astonishment that I'd even thought for a moment that he could read it, given the kind of mathematics that was to be expected of me. ...

blame for lack of flexibility!

With that out of the way, it remains for me to try and pinpoint the boss's "motivations" for this turnaround, which took place as discreetly as possible but which, on closer inspection, is quite spectacular.

## 10.4. (45) Le Guru-pas-Guru - or the three-legged horse

This brings me back to a meditation I'd been doing from July to December 1981, after a four-month period of mathematical frenzy. This somewhat insane period (very fruitful, by the way, from a mathematical point of view<sup>3</sup> (39)) had ended, overnight, following a dream. It was a dream that described, in a parable of irresistible wildness, what was happening in my life - a parable of this frenzy. The message was dazzlingly clear, yet it took me two days of intense work to accept its obvious meaning<sup>4</sup> (40). Once that was done, I knew what I had to do. I didn't return to this dream in my work for the next six months, but I was doing nothing more than penetrating its meaning and fully assimilating its message. The day after the dream, the message was understood on a level that remained superficial and crude. What I needed to deepen, above all, was "my" relationship; that of the boss, I mean, to both of the two desires involved, which appeared to me to be antagonistic.

So much has happened in my life since that meditation, that it seems to me to be in the very distant past. If I try to formul<sup>er</sup> what I took away from what it taught me about motiva-

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In the twelve years since the "first awakening" (in 1970), the boss had bet on what was obviously "the wrong horse": **between mathematics and meditation** (which he liked to pit one against the other), **he had opted for meditation.**

That's one way of putting it, since the thing and the name "meditation" had only entered my life in October 1976, five years earlier. But in the beloved image of myself that had been given a fresh coat of paint in 1970, meditation had come at just the right moment, six years later, to enhance a certain attitude or pose, long spotted but never examined until that 1981 meditation. I called it my "master's syndrome", and some have (rightly) called it my "Guru pose". If I adopted the former designation rather than the latter, it was undoubtedly because it fostered a confusion about the nature of the thing, in which I liked to maintain myself. From my earliest childhood, I'd always had a spontaneous pleasure in teaching, which was in no way opposed to the spontaneous pleasure in learning, and which had nothing to do with my own personal interests.

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<sup>3</sup>(39)

This was the period, among others, of the "Long Walk through Galois theory", discussed in "Esquisse d'un Programme" (par. 3: "Corps de nombres associés à un dessin d'enfant").

<sup>4</sup>(40) **The visit**

The work on this dream is the subject of a long letter in English, to a friend and colleague who had dropped in on me the day before. Some of the materials used by the Dreamer, to bring this strikingly realistic dream out of apparent nothingness, were obviously borrowed from that short episode of the visit of a dear friend I hadn't seen for nearly ten years. So, on the first day of work and against my previous experience, I thought I could conclude that the dream that had come to me concerned my friend more than it concerned me - that it was **he** who should have had the dream, not me! It was a way of evading the message of the dream, which (I should have known from my past experience) concerned no one but me. I finally realized this in the night following this first, superficial phase of the work, which I resumed the next day in the same letter. Since that memorable letter, I have received no further sign of life from this friend, one of the closest I have ever had.

This work was the only meditation that took the form of a letter (and in English to boot), so I no longer have a written record of it. I was particularly struck by this episode, one of many that show how any sign of work that goes beyond a certain façade, and brings to light simple facts that we generally make a point of ignoring - how any such work inspires unease and fear in others. I'll come back to this later (see par. 47, "The solitary adventure").

nothing of a pose. It was this strength above all that was at play in my relationship with my students; this relationship was superficial, but it was strong and good-natured, by which I mean: without pose. It was after what I called my "awakening" in 1970, when a world that had been familiar to me was receding to the point of almost disappearing, and with it the students and the opportunities I had "to teach", to share things I knew and which for me had meaning and value - that "the boss" took his revenge as best he could: instead of teaching maths, which was just a good way to earn a living, but otherwise unworthy of my new greatness, I saw myself teaching a certain "wisdom" by life and example. I was careful, of course, not to say anything of the sort to myself or others, and when I received echoes in this direction, I surely had to recuse myself, pained by so much incomprehension on the part of such friends or relatives. No matter how many times I explained it to them, they still didn't get it - sorry pupils if ever there was one!

I had read a book or two by Krishnamurti that had made a strong impression on me, and my head had assimilated in a jiffy a certain message and certain values<sup>5</sup> (41). That was all it took to believe that everything was

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#### <sup>5</sup>(41) Krishnamurti, or liberation turned hindrance

It would be inaccurate to say that the only thing I took away from this reading was a certain vocabulary, and a propensity to make it my own and finally substitute it, as it should be, for reality. The reason I was so struck by Krishnamurti's first book (even though I'd only had the chance to read a few chapters) was that what he was saying totally overturned a number of things I'd always taken for granted, and which I immediately realized were **commonplaces** that had always been part of the air I'd breathed. At the same time, this reading drew my attention, for the first time, to far-reaching facts, especially that of flight from reality, as one of the most powerful and universal conditioning of the mind. This gave me an essential key to understanding situations that until then had been incomprehensible and therefore (without my realizing it until I discovered meditation five or six years later) generating anguish. I could immediately see the reality of this escape all around me. This eased some of my anxieties, without changing anything essential, because I could only see this reality in others, while taking it for granted that it didn't exist in myself, that I was the exception that confirmed the rule (and without asking myself any further questions about this truly remarkable exception). In fact, I was in no way curious about myself or others. This "key" can only **open in the hands** of the person motivated by the desire to penetrate. In my hands, it had become an exorcism and a pose.

It was at the beginning of 1974 that, for the first time, I realized that the destruction in my life, which was following me step by step, couldn't have come from others **alone**, that there was something **within me** that was attracting it, feeding it, perpetuating it. It was a moment of humility and openness, conducive to renewal. But the renewal remained peripheral and ephemeral, for lack of in-depth **work**. This "something inside me" was still vague. I could see that it was a lack of love, but the very idea of working to identify more closely where and how there had been a lack of love in me, how it had manifested itself, what its concrete effects had been, etc. . - (On the contrary, K. likes to insist on the vanity of all work, which he automatically equates with the ego's "craving to become"). So, with borrowed "wisdom" as my compass, I saw nothing to do but wait patiently for "love" to descend upon me like a grace from the Holy Spirit.

And yet, the humble truth I had just learned at the end of a wave had triggered the rise of a powerful wave of new energy, comparable to the one that was to carry me through my first foray into meditation two and a half years later. This energy did not remain entirely unused. A few months later, when I was immobilized by a providential accident, it led to a (written) reflection in which, for the first time in my life, I examined the worldview that had been the unspoken basis of my relationship with others, and which came to me from my parents and especially my mother. I realized very clearly that this vision had gone bankrupt, that it was incapable of accounting for the reality of relationships between people, and of fostering personal fulfillment and relationships with others. This reflection remains marked by the "Krishnamurti style", and also by the Krishnamurtian taboo on any real **work** towards understanding. It did, however, make tangible and irreversible a knowledge born a few months earlier, which at first remained vague and elusive. No book or other person in the world could have given me this knowledge.

To have the quality of a meditation, what this reflection lacked above all was a look at myself and my **vision of myself**, and not just my vision of the world, a system of axioms where I wasn't really "in the flesh". It also lacked a look at myself in **the moment**, at the very moment of reflection (which fell short of a real work); a look that would have led me to detect not only a borrowed style, but also a certain complacency in the literary aspect of these notes, a lack of spontaneity and authenticity. Insufficient though it was, and relatively limited in its immediate effects on my relationships with others, this reflection nonetheless seemed to me to be a step - probably a necessary one, given the starting point - towards the more profound renewal that was to take place two years later. It was then that I discovered meditation - and discovered that first unsuspected fact: **that there were things to discover about myself** - things that almost completely determined the course of my life and the nature of my relationships with others. . .

(while pretending otherwise, of course). I didn't need to read any more, I was able to improvise the purest Krishnamurti in speech and writing, in a speech of flawless coherence. But no matter how beautiful and flawless the discourse, at no point did it seem to be of any use to me or anyone else. It went on for years without me even pretending to take any notice. With the discovery of meditation, the jargon fell away from me overnight, without a trace. I knew then the difference between talk and knowledge.

The big boss immediately rectified the situation: Krishnamurti out, meditation in! Discreetly, needless to say, he now had to play the game with a completely different touch. Times had changed, with this kid now running between his legs, and a bit sharp-eyed at times. I guess the kid was busy elsewhere. In any case, it was only five years later, when a certain pot had exploded and the kid had run to see what was going on, that the great chef's scheme was revealed.

It wasn't so long ago after all, just over two years ago, that the Guru-without-an-air was finally stale - one more disguise down the drain! The poor boss was about to be stripped naked. Or to put it another way: the "Meditation" horse, which had taken the place of the horse with no name (which was definitely not to be called "Krishnamurtian"! ) was making really derisory returns, especially when compared with the coquettish returns of the "Mathematics" horse in the days when the boss was still betting on him. If he maintained the wrong bet for so long, it was out of sheer inertia - he'd already changed his bet once, which isn't all that common, and it took the full impact of an impacting event<sup>6</sup> (42). Bosses don't really like to change bets - and in this case, it was a sort of going back to the previous bet.

It was in 1973, when I retired to the countryside, that the returns from the new horse began to be really meagre compared to the old one. The unexpected appearance of meditation three years later gave them a bit of a boost. There was even the episode of a vertiginous peak from March to July.

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### <sup>6</sup>(42) The salutary snatch

"The "percussive" event in question was the discovery, at the end of 1969, that the institution I felt part of was partially financed by funds from the Ministry of the Armed Forces, something which was incompatible with my basic axioms (and still is today). This event was the first in a whole chain of others (each more revealing than the last!) which had the effect of: my departure from the IHES (Institut des Hautes Etudes Scientifiques), and one thing leading to another, a radical change of environment and investments.

During the heroic years of the IHES, Dieudonné and I were the only members, and the only ones to give it credibility and an audience in the scientific world: Dieudonné through the publication of "Publications Mathématiques" (the first volume of which appeared in 1959, the year after Léon Motchane founded the IHES), and I through the "Séminaires de Géométrie Algébrique". In those early years, the IHES' existence was very precarious, with an uncertain financing (thanks to the generosity of a few companies acting as patrons) and with its only premises a room lent (with visible bad humor) by the Fondation Thiers in Paris for the days of my seminar [*A recent brochure published by the IHES on the occasion of the twenty-fifth anniversary of its foundation (of which Nico Kuiper was kind enough to send me a copy) says nothing about these difficult beginnings, perhaps unworthy of the solemnity of the occasion, celebrated with great pomp last year.*]. I felt a bit like a "scientific" co-founder, with Dieudonné, of my home institution, and I intended to live out my days there! I had ended up identifying strongly with IHES, and my departure (as a consequence of my colleagues' indifference) was experienced as a kind of uprooting from another "home", before proving to be a liberation.

Looking back, I realize that there must already have been a need for renewal within me, I just can't say how long ago. It's surely no mere coincidence that the year before I left IHES, there was a sudden shift in my investment of energy, leaving behind the tasks that had been burning in my hands the day before, and the questions that fascinated me most, to throw myself (under the influence of a biologist friend, Mircea Dumitrescu) into biology. I was embarking on this with a view to making a long-term investment in the IHES (which was in keeping with the multidisciplinary vocation of this institution). Surely this was no more than an outlet for the need for a much more profound renewal, which could not have been achieved in the "scientific incubator" atmosphere of the IHES, and which took place during that "cascade of awakenings" to which I have already alluded. There have been seven, the last of which took place in 1982. The "military funds" episode was providential in triggering the first of these "awakenings". The Ministry of the Armed Forces and my ex-colleagues at IHES were finally grateful to me!

1979, which I won't go into here, where once again I pre-<sup>121</sup>sent the figure of an apostle, this time an apostle of a sa-

gesse immémoriale et nouvelle à la fois, chantée dans un ouvrage poétique de ma composition et que je me abstenu finalement de confier aux mains d'un éditeur<sup>7</sup> (43). But two years later, with the Guru definitively out of action, it was as if the Meditation horse had broken a leg (as far as returns to the boss were concerned) - there was no way, fingering or no fingering, to play the Gurus!

After that, it wasn't long - the three-legged horse down the hatch, along with the Apostle-Poet, The Guru-not-Guru and Krishnamurti-who-dare-not-say-his-name. And long live Mathematics!

We look forward to what happens next. ...

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<sup>7</sup>"The poetic work of my composition" contains much that I know first-hand, and which today appears to me to be just as important in my life, and "in life" in general, as when it was written, with the intention of publishing it. If I refrained from doing so, it was mainly because I later realized that the form was afflicted by a deliberate intention to "make poetic", so that its overly constructed overall conception, and many passages, lack spontaneity, to the point at times of painful stiffness or swelling. This form, ampoule at times, was a reflection of my dispositions, where it was decidedly often the "boss" who led the dance - heavily, it goes without saying... .

## 2. A walk through a work or the Child and Mother

# 11. Lonely adventure

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### 11.1. (46) The forbidden fruit

I had to stop writing for two days. After careful rereading, it seems to me that the above scenario is indeed, roughly speaking, a description of reality, a description that I'd now have to delve into a little more. In particular, I'd have to take a closer look at the respective merits of the two "horses" meditation and mathematics; and also try to understand which events or conjunctures ended up triggering the "swing" in the boss's wager, against the forces of inertia that would rather push him to keep a losing wager indefinitely.

Perhaps we should also sound out the kid's preferences. It's an established fact that he wants to change games from time to time, and the boss apparently has a modicum of flexibility so as not to force him to always play this and never that. Over the last few years, he's learned to take the kid into account, to work with him, without waiting for pots to explode. It's not complete harmony, but it's no longer war, more a kind of entente cordiale, which occasional tensions tend to soften, not harden.

When he's not too hard up against it, the kid is by nature quite flexible in his preferences. (Unlike the boss, who eventually learned a modicum of flexibility only unwillingly and in his old age... .) But just because the kid's flexible doesn't mean he doesn't have a preference of his own, which he'll be happy to accept.

is attracted more strongly to one thing than to another.

It's often hard to see clearly, to distinguish between the kid's desires and preferences or even what the boss has decided once and for all. When I used to say to myself: meditation is better, more important, more serious and all that than mathematics, for such and such reasons (the most pertinent, no doubt), it was the boss who gave himself good reasons afterwards to convince himself that the bet he was making was indeed "the right one". The kid doesn't say that one thing is "better" or "more important" than another. He's not one for speeches. When he feels like doing something, he just goes ahead and does it if no one is in his way, without questioning whether it's "important" or "better". His desires vary from one thing to another and from one moment to the next. To detect his preferences, it's no use listening to the boss's explanatory speeches, when he claims to speak for the kid when he can only speak for himself. It's only by observing the kid at play that we can identify his preferences.

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can perhaps detect his predilections. And even then, it's not so obvious: when he plays this with gusto, it doesn't always mean that he wouldn't play something else with delight, if the boss didn't give him a helping hand.

Clearly, what attracts him above all else is **the unknown** - to pursue into the nebulous recesses of the night and bring into the open that which is unknown to him and to everyone else. And I have the impression that when I added "and everyone else", I was referring to the child's desire, not the boss's vanity, who wants to impress the gallery and himself. It's also a well-known fact that what the kid brings back every time from the shadows of inexhaustible attics and cellars are "obvious", childlike things. The more obvious they seem, the happier he is. If they're not, it's because he hasn't done his job to the end, that he's stopped halfway between darkness and daylight.

In maths, "obvious" things are also things that sooner or later someone **has to** stumble upon. They're not "inventions" that you can do or not do. They're things that have always been there, that everyone comes into contact with without paying any attention to them, even if it means taking a long detour around them, or going over them.

p. 123 stumbling every time. After a year or a thousand, infallibly, someone ends up paying attention to the thing, digging around it, digging it up, looking at it from all sides, cleaning it up, and finally giving it a name. This

kind of work, my work of choice, another could do it every time, and what's more, another could do it every time.

**was bound to do so** at some point<sup>1</sup> (44).

It's a completely different story when it comes to self-discovery, in the non-collective game of "meditation". What I discover, no other person in the world, today or at any other time, can discover for me. It's up to me alone to discover it, which also means: to **assume it**. This unknown is not destined to be known, almost by force of circumstance, whether or not I take the trouble to be interested in it. If it waits in silence for the moment when it will be known, and if sometimes, when the time is ripe, I hear it calling, it is only I, the child in me, who is called to know it. He's not a stranger on borrowed time. Of course, I'm free to follow his call, or to evade it, to say "tomorrow" or "someday". But the call is addressed to me and to no one else, and no one but me can hear it, no one else can follow it.

Every time I've followed this call, **something has changed in the "company"**, more or less. The effect has been immediate, and immediately felt as a blessing - sometimes, as a sudden release, an immense relief, from a weight I was carrying without even realizing it, and whose reality is manifested by this relief, this liberation. On a smaller scale, such experiences are common in any work of discovery, and I've had occasion to talk about them. What distinguishes the work of self-discovery (whether it takes place in the open or remains underground) from any other work of discovery, however, is precisely that it really changes something in the "business" itself. It's not a quantitative change, an increase in output, or a difference in the size or even quality of the products leaving the workshop. It's a change in the **relationship between boss and worker-child**. Perhaps there's even a change in the boss himself, if that means anything other than his relationship with the worker-child. For example, he may look less at production - but this is also an aspect of his relationship with the worker, through the emergence of a concern or respect that had previously been foreign to him. In every case in which I meditated, the change was in the

p. 124 □ The new regulations are intended to **clarify** and **calm** relations between management and labor. Except in certain cases

where meditation remained superficial, "circumstantial" meditations under the sole pressure of an immediate and limited need, clarification has lasted until today, and so has appeasement.

<sup>1</sup>Needless to say, I'm disregarding the hypothesis - by no means improbable, to say the least - of the unexpected eruption of an atomic war or some other such joyous event, likely to put an abrupt end, once and for all, to the collective game called "Mathematics", and to much else besides... .



This gives the work of self-discovery a different **meaning** from any other work of discovery, even though many essential aspects are the same. There is a dimension to self-knowledge, and to the work of self-discovery, that sets it apart from all other knowledge and work. Perhaps this is the "**forbidden fruit**" of the Tree of Knowledge. Perhaps the fascination that meditation has exerted on me, or rather that of the mysteries it has revealed to me, is the fascination of the forbidden fruit. I've crossed a threshold where fear has disappeared. The only obstacle to knowledge is inertia, sometimes considerable, but finite and by no means insurmountable. I felt this inertia at almost every step, insidious and omnipresent. It exasperated me at times, but never discouraged me. (No more so than in mathematical work, where it is also the main obstacle, but of incomparably lesser weight). This inertia becomes one of the essential ingredients of the game; one of the protagonists, to put it better, in this delicate and by no means symmetrical game which has two - or three, to put it better: on the one hand, the child who dashes forward, and the boss (made of inertia) who puts the brakes on everything he can (while pretending not to be there), and on the other, the glimpsed form of the beautiful unknown, rich in mystery, at once near and far, who both evades and calls... .

## 11.2. (47) L'aventure solitaire

This fascination with "meditation" has been of considerable power for me - as powerful as the attraction of "woman", whose place it seems to have taken. The fact that I've just written "has been" doesn't mean that this fascination has been extinguished. In the year that I've been involved in mathematics, it has faded into the background. Experience tells me that this situation can be reversed overnight, just as this situation is itself the effect of an entirely unforeseen reversal. In fact, during each of the four long periods of meditation I went through (one of which was

extended over almost a year and a half), it was a matter of course for me that I was going to keep going until I **drew**

my last breath, to probe as far as I could into the mysteries of life and those of human existence. When the notes piled up in impressive stacks to the point of threatening to overwhelm my workroom, I even ended up having a piece of furniture made to measure to accommodate them, with plenty of room (thanks to a quick calculation of arithmetic progression) to accommodate those that would soon be added over the years; I had allowed for a margin of around fifteen years if I remember correctly (which was already starting to happen!). In this case, the boss had done things right, and for stewardship it was a fine piece of stewardship! That, and a large-scale tidying-up of all personal papers closely or remotely linked to meditation work, was in fact his last task undertaken and (almost) successfully completed, just before the switch of preferences and bets. It makes you wonder whether he didn't have an ulterior motive, and whether he didn't already see tomes of "Mathematical Reflections" filling the empty shelves supposedly intended for the "Notes" to come.

It's true that the passion for meditation and self-discovery is vast enough to fill my life for the rest of my days. It's also true that the mathematical passion is not consumed, but perhaps that hunger will be sated in the years to come. Something in me wishes it would, and feels mathematics to be a hindrance to a solitary adventure that only I can pursue. And it seems to me that this "something" inside me is **not** the boss, nor one of the boss's desires (which, by nature, is divided). It seems to me that mathematical passion still bears the mark of the boss, and in any case, that following it makes my life move in a closed circle; in the circle of an **ease**, and in a movement that is that of **inertia**, certainly not of renewal.

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I've been wondering about the meaning of this stubborn persistence of mathematical passion in my life. When I follow it, it doesn't really fill my life. It gives me joy, and it gives me satisfaction, but it doesn't in itself give me true fulfillment. Like any purely intellectual activity, intense, long-term mathematical activity has a rather **mind-numbing** effect. I see this in others, and especially in myself, every time I do it again.

This activity is so fragmentary, it involves only such a tiny part of our faculties of intuition, of sensitivity, that these become dulled to the force of not being used. For a long time, I didn't realize this.

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It's only since I've been meditating, it seems to me, that I've become aware of this thing. It's only since I've been meditating, it seems, that I've become aware of this thing. If you pay attention, it's obvious - **maths in large doses thickens**. Even after the meditation of two and a half years ago, where mathematical passion was recognized as a passion indeed, as an important thing in my life - when I give myself to this passion now, there's still a reserve, a reluctance, it's not a total gift. I know that a so-called "total gift" would in fact be a kind of abdication, it would be following an inertia, it would be a flight, not a gift.

There is no such reserve in me for meditation. When I give myself to it, I give myself totally, there is no trace of division in this giving. I know that in giving myself, I am in complete accord with myself and with the world - I am true to my nature, "I am the Tao". This gift is beneficial to myself and to all. It opens me up to myself and to others, by lovingly untying what remains knotted within me.

Meditation opens me up to others; it has the power to untie my relationship with them, even though the other remains tied up. But it's very rare that I have the opportunity to communicate with others in any way whatsoever about the work of meditation, about this or that thing that this work has made me aware of. This is not because it's "too personal". To take an imperfect image, I can only communicate about maths that interests me at a given moment, with a mathematician who has the essential background, and who at the same time is willing to take an interest in it too. It happens that for years I'm fascinated by such and such mathematics, without meeting (or even trying to meet) another mathematician with whom to communicate about it. But I know that if I looked for them, I'd find them, and that even if I didn't, it would simply be a matter of luck or circumstance; that the things I'm interested in can't fail to interest someone, or even a few people, whether ten years or a hundred, it doesn't really matter. This is what gives meaning to my work, even if it's done in solitude. If it

were there no other mathematicians in the world, and that there should be no more, I don't think doing maths would still make sense to me - and I suspect it's no different for any other mathematician,

or any other "researcher" whatsoever. This ties in with my earlier observation that, for me, the "mathematical unknown" is what nobody knows yet - it's something that doesn't depend on me alone, but on a collective reality. **Mathematics is a collective adventure**, going on for millennia.

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In the case of meditation, in order to communicate about it, the question of "baggage" doesn't arise; not at the point I'm at, at least, and I doubt it ever will. The only question is that of an interest in the other, which responds to the interest in me. So it's a question of curiosity about what's really going on in oneself and in others, beyond the de rigueur facades, which don't hide much as long as you're really interested in seeing what they cover. But I've learned that the moments when such interest appears in a person, the "moments of truth", are rare and fleeting. It's not uncommon, of course, to meet people who are "interested in psychology", as they say, who have read Freud and Jung and many others, and who would like nothing better than to have "interesting discussions". They have this

The baggage they carry with them, more or less heavy or light, what we call a "culture". It's part of the image they have of themselves, and reinforces that image, which they are careful never to examine, just like someone else who's interested in maths, flying saucers or angling. It's not this kind of "baggage", nor this kind of "interest", that I meant earlier - although the same words here refer to things of a different nature.

To put it another way: **meditation is a solitary adventure**. Its nature is to be solitary. Not only is the **work** of meditation solitary - I think this is true of any work of discovery, even when it is part of a collective effort. But the **knowledge** that arises from the work of meditation is "solitary" knowledge, knowledge that cannot be **shared**, let alone "communicated"; or if it can be shared, it's only in rare moments. It's a work, a knowledge that goes against the grain of the most inveterate consensus, and worries each and every one of us. This knowledge

is expressed simply, in simple, clear words. When I express it to myself, I learn by expressing it, because expression *même* is part of a job, driven by intense interest. But those same simple words and are powerless to communicate meaning to others, when they come up against the closed doors of indifference or fear. Even the language of dreams, with its infinite resources and strength, constantly renewed by a tireless and benevolent Dreamer, cannot penetrate these doors. ...

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There is no meditation that is not solitary. If there is a shadow of concern for anyone's approval, confirmation or encouragement, there is no work of meditation and no self-discovery. The same is true, it may be said, of any genuine work of discovery, at the very moment of the work itself. Of course. But outside of the work itself, the approval of others - be it someone close to you, or a colleague, or a whole milieu to which you belong - is important for the meaning of this work in the life of the person who gives it his or her all. This approval and encouragement are among the most powerful incentives that make the "boss" (to use this image) give an unconditional green light for the kid to give it his all. Above all, they determine the boss's investment. It was no different in my own investment in mathematics, encouraged by the kindness, warmth and confidence of people like Cartan, Schwartz, Dieudonné, Godement, and others after them. For meditation work, on the other hand, there's no such incentive. It's a passion of the young worker that the boss is basically kind enough to tolerate, because **it doesn't "earn" anything**. It bears fruit, of course, but it's not the kind of fruit a boss aspires to. When he's not fooling himself about it, it's clear that he's not going to invest in meditation - the boss is gregarious by nature!

Only children, by nature, are solitary.

### 11.3. (48) Donation and welcome

Speaking yesterday of the solitary essence of meditation, I was touched by the thought that the notes I've been writing for nearly six weeks, which have ended up becoming a kind of meditation, are nonetheless intended for publication. Inevitably, this has influenced the form of the meditation in many ways, notably in terms of brevity and discretion. One of the essential aspects of meditation, namely

constant attention to what was going on inside me at the very moment of the work, manifested itself only very occasionally, and superficially. Surely all this must have *inflated* on the course of the work and on

quality. I feel, however, that it has the quality of meditation, above all by the nature of its fruits, by the appearance of a knowledge of myself (in this case, that of a certain **past**) that I had evaded until now. Another aspect is spontaneity, which has meant that for none of the nearly fifty "sections" or

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sap.

"I couldn't have said at the outset what the substance would be; each time it was revealed only along the way, and each time the work brought new facts to light, or shed new light on hitherto neglected facts.

The most immediate sense of this work was that of a dialogue with myself, a meditation. However, the fact that this meditation is destined to be published, and moreover, to serve as an "overture" to the "Mathematical Reflections" that are to follow, is by no means an incidental circumstance, which would have been a dead letter in the course of the work. For me, it is an essential part of the meaning of this work. If I hinted yesterday that the boss is surely getting something out of it (he's a master at "getting something out of everything", or very nearly so!), that in no way means that its meaning can be reduced to this - to a belated, almost posthumous "return" of the famous three-legged horse! More than once, too, I've felt that the deeper meaning of an act sometimes goes beyond the motivations (apparent or hidden) that inspire it. And in this "return to mathematics", I've guessed a meaning other than that of being the result-sum of certain psychic forces that were present in my person at such and such a time and for such and such a reason.

This "meditation" that I'm pursuing in order to offer it to those I've known and loved in the mathematical world - if I feel it's an important part of this glimpsed meaning, it's not in the expectation that the gift will be welcomed. Whether or not it is accepted does not depend on me, but only on the person to whom it is addressed. I'm certainly not indifferent to whether it's accepted or not. But that's not **my** responsibility. My only responsibility is to be true to the gift I give, that is to say, to be myself.

What I learn from meditation are the humble and obvious things, things that don't pay for themselves. mine. They're also the ones I won't find in any book or treatise, however learned, profound, brilliant - the ones no one else can find for me. I have questioned a "fog", I have ☐ taken the trouble to listen to it, I have learned

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a humble truth about a "sporting attitude" and its obvious meaning, in my relationship to mathematics as in my relationship to others. I would have read "in the text" the Holy Scriptures, the Koran, the Upanishads, Plato, Nietzsche, Freud and Jung on top of it all - I would have been a prodigy of vast and profound erudition - but all that would have done was to **distance me** from this truth, a childish, self-evident truth. And I would have repeated Christ's words a hundred times, "Happy are those who are like little children, for theirs is the Kingdom of Heaven", and commented on them in fine detail, and it would only have served to keep me away from the child in me, and from the humble truths that bother me and that only the child can see. **These things** are the best I have to offer.

And I'm well aware that when such things are said and offered, in simple, clear words, they're not always welcomed. To welcome is not simply to receive information, with embarrassment or even interest: "Gee, who'd have guessed... . .", or "It's not so surprising after all... . .". Welcoming often means recognizing oneself in the giver. It means getting to know yourself through the person of another.

## 11.4. (49) Acknowledgement of a division

This short reflection on the meaning of the present work, and on giving and receiving, comes as a digression in the thread of reflection; or rather, as an illustration of certain aspects that distinguish "meditation" from any other work of discovery, and in particular from mathematical work. Yesterday, I realized that these aspects have a double effect, namely two effects **in opposite directions**: a unique fascination with "the kid", and a total lack of interest in "the boss". It seems that this double effect is in the nature of things, and cannot be mitigated by any compromise or arrangement. Whatever we do, when the kid

is his real predilection, the boss doesn't like it, not at all!

There's no doubt that this is the meaning of the shift that's taken place, which could well wipe the slate clean of meditation in my life in the years to come (with the exception of "occasional meditations", like three months ago). I don't think these have to be entirely barren years for that, any more than the year past has been fruitless. But it is also true that what I learned there (apart from maths) is minimal, if I p . 131  
The strange thing is that each of the four long periods of meditation I experienced were times of great fulfillment. The strange thing is that each of the four long periods of meditation I experienced were times of great fulfillment, with nothing to suggest that something inside me remained frustrated. Yet, if pots exploded, it was because somewhere there was pressure, and that pressure must not have been there that day; it must have been there, somewhere out of my sight, for weeks or months, while I was intensely and totally absorbed in meditation.

But now I'm getting carried away by the momentum of the pen (or rather, the typewriter). The reality is that (except in the last period of meditation, which was cut off midstream by a combination of events and circumstances), the intensity of meditation gradually diminished from one moment onwards, like a wave about to be followed by another about to take its place... . The feeling of fullness, in fact, followed this same movement, with the difference that it was only present at the time of the meditation-waves, not the "mathematical" waves.

The situation I'm trying to define is no longer, it seems to me, a situation of conflict, but it's becoming apparent that it still contains the germ, the potentiality of conflict. For me, it is now perhaps the most visible sign, through its impact on the course of my life, of a **division** within me. This division is none other than the boss-child division.

I can't put an end to it. All I can do, now that it's well and truly detected in this manifestation, is pay attention to it, and follow its signs and evolution over the months and years ahead. Perhaps this passion for maths, a little misguided it must be said, will burn itself out (as another passion in me has already burned itself out. . . ), to make way for the sole passion of discovering myself and my destiny.

This passion is vast enough, as I said, to fill my life - and surely my whole life won't be enough to exhaust it.

## 11.5. (50) The weight of a past

It's been a few days since I finished putting the finishing touches to "Récoltes et Semailles" - after having believed, for over a month, that I was surviving to finish in the next few days. Even this time- p . 132  
here, after I'd put the finishing touches to it, I wasn't entirely sure whether I'd actually finished it or not.  
- there was one question I'd left unanswered. It was to "understand what events or circumstances finally triggered the "tipping" in favor of mathematics instead of meditation, against considerable forces of inertia. Without any deliberate intention, my thoughts returned with some insistence to this question, in these last few days when I had already begun to branch off into other matters of a completely different order, including mathematical questions (of conformal geometry). I might as well make the most of this meditative "fin de lancée", to dig a little deeper and clear the decks.

Several associations come to mind, when I try to answer "on the spur of the moment" why "I'm getting back into maths" (in the sense of a major investment intended to be long-term, of the order of at least

a few years ago). Perhaps the strongest of all relates to the feeling of chronic frustration I've come to feel in my teaching activity over the last six or seven years. There's this increasingly strong feeling of being "**underemployed**", and even, quite often, of investing myself and giving the best of myself for morose students who don't care what I have to give.

I see wonderful things to do everywhere, just waiting to be done. Often, all that's needed to tackle them is a derisory baggage, and it's these things themselves that tell us what language to develop to understand them, and what tools to acquire to dig into them. I can't help seeing them, simply because of regular contact with maths (at however modest a level) as a result of teaching, even in those periods of my life when my interest in maths is most marginal. Behind everything we look at, no matter how little we look, there are other beautiful things, covering and revealing others in their turn... . Whether in maths or anywhere else, wherever you look with genuine interest, you'll see a richness revealed, a depth opened up that you'd guess is inexhaustible. The frustration I'm talking about is that of not being able to communicate this feeling of richness - of depth - to my students, even if it's only a spark of desire to explore at least what's right at their fingertips, to have fun with it.

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during the few months or years that they are in any case determined to invest in a so-called "research" activity, for the purposes of preparing this or that degree. Except for two or three of the students I've had since

At ten years of age, it seems that the very idea of "giving it their all" frightens them, and that for months and years they'd rather sit back and do nothing, or laboriously do some mole's work for which they know neither the ins and outs, as long as there's a diploma at the end. There's a lot to be said for this kind of paralysis of creativity, which has nothing to do with the existence or non-existence of "gifts" or "faculties" - and this goes back to the very beginning of my reflection, when I touched in passing on the root cause of such blockages. But that's not my point here, which is rather to note the state of chronic frustration that these situations, constantly repeated throughout the last seven years of my teaching career, have come to create in me.

The obvious way to "resolve" such frustration, at least insofar as it's that of the "mathematician" in me and not that of the teacher, is to do for myself at least some of those things that I despaired of seeing any of my students grasp at the end of the day. And that's what I've done here and there, whether it be occasional reflection lasting a few hours, or even a few days, on the bangs of my teaching activity, or during periods of mathematical frenzy (which sometimes occurred like veritable explosions. . . ), sometimes lasting weeks or months. Such occasional and intermittent work could usually only give rise to a very first rough sketch of a question, and to a most fragmentary vision - rather, a clearer vision of the work in perspective, whereas this work itself always remains to be done and, to be better seen, only appears all the more burning. Two months ago, I gave an overall sketch of the main themes I've begun to take the measure of. This is the "Esquisse d'un Programme", to which I have already alluded, and which will finally be attached to the present reflection, to constitute together volume 1 of "Réflexions Mathématiques".

It is quite clear that this ("private", so to speak) prospecting work alone was not enough to solve the problem. my frustration. This feeling of "being underemployed" surely reflected a **desire** (of egotic origin, I believe, i.e. "the boss's" desire) **to exert an action**. Here, it's not so much a question of acting on others (on my students

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let's say, set them in motion, "communicate something" to them, or help them get that degree that could enable them to apply for such and such positions, etc. . . ) than "mathematician" action: contributing to the discovery of such and such unsuspected facts, to the emergence of such and such a theory, etc. ... . This is immediately associated with

I've already made the observation that mathematics is a "collective adventure". If I reflect on my attitude when I was doing maths over the last ten years, at a time in my life when it would never have occurred to me that I might one day go back to publishing, and when it was also more or less clear that none of my present or future students would have anything to do with my prospecting work - it immediately occurred to me that these were by no means the dispositions of someone doing something for personal pleasure alone, or driven by an inner need that concerned only himself, with no relationship to others. When I do maths, I believe that somewhere within me it is clearly understood that this maths is meant to be communicated to others, to be part of a larger thing to which I am contributing, a thing that is by no means individual in nature. I could call this "thing" "mathematics", or better still, "our knowledge of mathematical things". The term "our" here undoubtedly refers, first and foremost, in concrete terms, to the group of mathematicians whom I know and with whom I have interests in common; but it is also beyond doubt that it goes beyond this restricted group just as much as it goes beyond myself. This "our" refers to **our species**, insofar as it, through some of its members down the ages, has been and is interested in the realities of the world of mathematical objects. I have never, before this very moment of writing, considered the existence of this "thing" in my life, let alone wondered about its nature and its role in my life as a mathematician and teacher.

The desire to take action to which I have alluded seems to take the following form in my life as a mathematician: to bring out of the shadows that which is **unknown to all**, not only to me (as I saw earlier), and this, moreover, for the purpose of being made **available to all**, thus enriching a common "patrimony". In other words, it's the desire to contribute to the enlargement and enrichment of this "thing", or "heritage", which goes beyond my person.

In this desire, certainly, the desire to enlarge my person through my works is not absent. By this aspect, I find again the craving for "growth" <sup>□d'</sup>agrandissement, which is one of the characteristics of the ego, of the "boss" ; p.

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This is its invasive and ultimately destructive aspect (cf. note 44' § 13.1.1 p. 260). However, I also realize that the desire to increase the number of things that (for a short or long time) will more or less bear my name, is far from exhausting, from covering up this desire or this more vast force, which drives me to want to contribute to enlarging a common heritage. It seems to me that such a desire could find satisfaction (if not "in my company", where the boss remains rather invasive, at least in a mathematician of greater maturity) while the role of one's own person would remain anonymous. Perhaps this is a "sublimated" form of the ego's tendency to enlarge, through identification with something beyond itself. Unless this kind of force is not egotistical in nature, but more delicate and profound, expressing a deep need, independent of any conditioning, that attests to the profound link between the life of a person and that of the whole species, a link that is part of the meaning of our individual existence. I don't know, and it's not my purpose here to probe such far-reaching questions.

Instead, I'd like to examine (from a more modest angle) a concrete situation concerning myself: a situation of frustration, with a partial and provisional outlet in the form of sporadic mathematical activity. The logic of the situation, therefore, was bound to lead me sooner or later to **communicate** what I found. Since, until last year, I was by no means prepared to make the large-scale, long-term investment in my mathematical passion that would have been necessary to "exploit" the mines I was uncovering for publication purposes, by means of detailed "piecework", I was left with the alternative of communicating to certain mathematician friends who were sufficiently "in the know" at least those things that were closest to my heart.

I think that if I had found a mathematician friend in the last ten years who plays opposite

of me as an **interlocutor** and source of information (as had been the case with Serre to a very large extent, for many years in the 50s and 60s), as well as a **relay** for any "information" I could pass on to him (a role Serre had not had to play in the past, as I

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myself!), my desire "to exercise action in maths" would have found sufficient satisfaction to resolve my frustration, while contenting me with an episodic and moderate *investissement d'énergie* in mathematics, leaving the lion's share to my new passion. The first time I approached a mathematician friend with such an expectation (at least implicit in me) was in 1975, and the last time in 1982, a year and a half ago. Funny coincidence, both times it was to try to "place" (for the purpose of being echoed and, who knows, developed at the end of the ends!) the same "program" of homo-logical and homotopic algebra, the first seeds of which date back to the fifties, and which was perfectly "mature" (according to the intimate conviction I had of it) even before the end of the sixties; a program of which a preliminary and broad outline development is the very theme of this *Poursuite des Champs* whose Introduction I'm supposed to be writing at the moment! The fact remains that, for reasons that undoubtedly differ from one case to another, my attempts to rediscover a "privileged interlocutor" relationship, such as I had (before 1970) with Serre, and then with Deligne, came to nothing. A common circumstance, however, was the relatively limited availability I was willing to give to maths. On the two occasions I've already mentioned (in 1975 and 1982), this surely contributed to the lack of communication. In fact, I was mainly looking to "place" something, without worrying too much about making the necessary effort to "(re)bring myself up to speed" to be on my side a satisfactory interlocutor for my correspondent, who was much more "in the know" than I was (to say the least!) when it came to current techniques in homotopy.

I could consider the "Letter to . . ." which serves as the first chapter of the *Poursuite des Champs* (letter from February last year, just over a year ago) as my last attempt to find an echo, from one of my friends of yesteryear, for some of my ideas and concerns of now. The continuation of the reflection begun (or rather, taken up) in that letter was to become (without my even suspecting it for weeks) the first mathematical text since 1970 promised to be published. It was only almost a year later that I received an indirect reaction to this substantial letter (compare note<sup>2</sup> (38)). It was more eloquent than any other letter received to date from a fellow mathematician, in making me feel certain attitudes towards my modest self that have become commonplace among my mathematician friends since I left the milieu of which I was a part with them. There is in this letter, from someone at

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to whom I had addressed myself as a friend, in a mood of warm sympathy, a deliberate intention to *dérision*, which reminded me in a particularly violent way of something I had come to realize

more and more clearly in recent years. Previously, I had had occasion to notice a distancing from myself in the mathematical "big world", and above all among those who had been my close friends (45). Here, it's no longer a question of distancing myself as a person, but rather of a consensus, in the nature of a fashion and as it presents itself as something to be taken for granted, between people who are "in the know" to some extent: that the thousand-page maths genre, and the notions I've been harping on about for a decade or two (46,47), aren't very serious at all; that there's a lot of bombast there for not much worthwhile, and that apart from the "general nonsense" toast around the notion of pattern and

<sup>2</sup>These notes were in fact a continuation of the long letter to... which became the first chapter. They were typed so as to be legible for this old friend, and for two or three others (Ronnie Brown in particular) whom I thought might be interested. This letter, by the way, was never answered, nor was it read by the addressee, who almost a year later (when I asked him if he'd received it) expressed sincere astonishment that I'd even thought for a moment that he could read it, given the kind of mathematics that was to be expected of me. ...



cohomologie étale (which sometimes have their uses; alas, we're willing to admit it), it's more charitable to at least forget the rest; that those who would nevertheless pretend to still be singing this kind of Grothendieckian trumpet, despite good taste and the obvious canons of seriousness, are to be lumped in with their Master, avowed or not, and have only themselves to blame if they are treated as they deserve... ..

Surely, the many echoes in this direction (which I've just transcribed "in plain English") that have reached me since 1976 (50), and especially over the last two or three years, have finally awakened in me a fighting spirit that had become somewhat dormant over the last ten years. Like a reflex, they've made me want to throw myself into the fray, to shut the mouths of these whitewashers who haven't understood a thing - a completely idiotic reflex, in fact, like that of the bull to whom all you have to do is show a piece of red cloth and wave it in front of his nose, and he'll immediately get into a frenzy and start moving, forgetting the path he was quietly following and which was his own! Still, I think this reflex is pretty epidermal, and wouldn't have been enough on its own to shake me off. Fortunately, doing maths is much more appealing than running into a piece of cloth and getting larded from all sides. But doing maths, while pursuing my own style of work} and approach to things, is also a bit like "throwing myself into the fray"; it means asserting myself in the face of signs of disdain and rejection - which I'm sure will come my way,

in response to the disdain my former friends have felt or thought they felt in me, if not towards them, at least towards a milieu with which they continue to identify unreservedly. It is therefore also, to some extent, following

instead of following **my** path.

This idea has occurred to me on several occasions over the past few weeks, and it's perhaps this aspect in particular that has prompted today's reflection. Along the way, another aspect has come to light, one in which the forces of the ego surely play a large part too, but which is not a simple combative reflex. Rather, it's a desire within me, the nature of which I can't yet clearly discern, to give meaning to the mathematical work I've been doing for the last ten or twelve years, or to see it take on its full meaning; a meaning which (I'm firmly convinced) cannot be reduced to that of private pleasure or personal adventure. But even if the nature of this desire remains misunderstood, since I haven't taken the time to examine it more closely, this reflection is enough to show me that it is indeed here, in this desire, that the force that weighs on me and forces my hand, so to speak, in favor of a mathematical investment - the "tipping" force - really lies. It would be just as effective. red fabric or not. If it's a sign of attachment to a past, it's the past of the last ten years, the "post-1970" past, and not the past of things already written in black and white, things done, things before 1970.

Basically, I'm not worried about these things, about what "posterity" will do to them in the future (although it's doubtful there will even be a posterity. . . ). What interests me in this past is by no means what I did (and the fortune that is or will be its), but rather what was not done, in the vast program that I had before my eyes at the time, and of which only a very small part was realized by my efforts and those of the friends and students who sometimes kindly joined me. Without having planned or sought it, this program itself was renewed, along with my vision and approach to mathematics. Over the years, the emphasis has shifted both in terms of themes and my own approach.

my first aim now is to probe the **mysteries** that have fascinated me most, such as "motifs", or "patterns".

that of the "geometric" description of the Galois group of  $\mathbb{Q}$  over  $\mathbb{Q}$ . Along the way, of course'  $\square$  <sup>1</sup> won't be able to p.

at least prevent me from sketching out the foundations here and there, as I've begun to do (among other things)

in "La longue Marche à travers la théorie de Galois", or as I'm doing now in "La Poursuite des Champs". But the subject has changed, and so has the style that expresses it.

To put it another way: in the last ten years, I've glimpsed mysterious and beautiful things in the world of mathematics. These things are not personal to me, they are meant to be communicated - the very meaning of having glimpsed them, as I see it, is to communicate them, to be taken up, understood, assimilated... . But communicating them, if only to oneself, also means deepening them, developing them a little - that's a **job**. I'm well aware, of course, that there's no way I could complete this work, even if I had a hundred years left to devote to it. But I don't have to worry about that today, about how many years or months I'm going to devote to this work out of the time I have left to live and discover the world, when there's **another** job waiting for me that only I can do. It is not in my power, nor is it my role, to regulate the seasons of my life.

# 12. NOTES for the first part of "RECOLTES ET SEMAILLES" (harvests and weeks)

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## 12.1. Ø

**Note 1** □ (Added March 1984) It is probably an overstatement to say that my "style" and "method" of workingp  
. 141 have not changed, whereas my style of expressing myself in mathematics has been profoundly  
transformed. Most of the time devoted over the last year to "La Poursuite des Champs" has been spent on my  
machine at  
typing reflections that are intended to be published virtually as they are (with the addition of relatively short  
notes added later to facilitate reading by cross-referencing, error correction, etc. . ). No scissors or glue to  
laboriously prepare a "definitive" manuscript (which, above all, must reveal nothing of the process that led  
to it) - that's a lot of changes in "style" and "method"! Unless you dissociate the mathematical work itself  
from the writing and presentation of results, which is artificial, because it doesn't correspond to the reality of  
things, since mathematical work is indissolubly linked to writing.

## 12.2. $\emptyset$

**Note 2** ( Added in March 1984) On rereading these last two paragraphs, I had a certain feeling of unease, due to the fact that in writing them, I implicate others and not myself. Obviously, the thought that my own person might be involved hadn't occurred to me while writing. I surely didn't learn anything when I confined myself to putting down in black and white (no doubt with a certain satisfaction) things that for years I have perceived in others, and seen confirmed in many ways. As I continue my reflection, I'm led to remember that there has been no shortage of contemptuous attitudes towards others in my life. It would be strange if the link I've grasped between contempt for others and contempt for oneself were absent in the case of myself; sound reason (and also the experience of similar situations of blindness towards myself, which I've come to realize) tells me that this must surely not be the case! For the time being, however, this is no more than a simple deduction, the only possible use of which would be to encourage me to see with my own eyes what's going on, and to see and examine (if it does indeed exist, or has existed) this as yet hypothetical contempt for myself, so deeply rooted in my own life.

p. 142 buried that it has completely escaped my gaze until now. It's true that there's been no shortage of things to look at! <sup>one</sup> This suddenly seems to me to be one of the most crucial, precisely because it's at such a crucial point in my life. hidden...<sup>1</sup>.

## 12.3. $\emptyset$

**Note 3** I'm thinking in particular of the famous conjectures by Mordell, Tate and Chafarevitch, all three of which were demonstrated last year in a forty-page manuscript by Faltings, at a time when the well-established consensus of those "in the know" was that these conjectures were "out of reach"! As it happens, "the" fundamental conjecture that serves as the keystone of the "Anabelian algebraic geometry" program I'm so fond of, is close to Mordell's conjecture. (It would even seem that the latter is a consequence of the former, which just goes to show that this program is not a story for serious people. ...). )

## 12.4. $\emptyset$

**Note 4** Even today, we come across "demonstrations" of uncertain status. So it was for years with Grauert's demonstration of the finiteness theorem that bears his name, which nobody (and there was no shortage of good-willers!) could read. This perplexity was resolved by other, more transparent demonstrations, some of which went even further, taking over from the initial one. A similar, more extreme situation is the "solution" to the so-called "four-color problem", the computational part of which was solved with a computer (and a few million dollars). This is a "demonstration" that is no longer based on the intimate conviction that comes from understanding a mathematical situation, but on credit given to a machine devoid of the faculty of understanding, and whose structure and operation the mathematician user is unaware of. Even supposing that the calculation is confirmed by other computers, following other calculation programs, I don't consider the four-color problem to be closed. It will just have changed its face, in the sense that it's no longer a question of looking for a counter-example, but only a demonstration (readable, of course!).

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<sup>1</sup>(August 1984) On this subject, however, see the last two paragraphs of the note "The massacre", n° 87.

## 12.5. Ø

**Note 5** This fact is all the more remarkable given that, until around 1957, I was regarded with a certain reserve by more than one member of the Bourbaki group, who ended up co-opting me, I believe, with some reluctance. There was a good-natured quip that I was a "dangerous specialist" (in Analyse Fonctionnelle). I sometimes sensed in Cartan a more serious unexpressed reserve - for some years, p. 143

I must have given him the impression of someone inclined towards gratuitous and superficial generalization. I saw him quite surprised to find in the first (and only) rather long essay I wrote for Bourbaki (on differential formalism on varieties) a reflection of any substance - he hadn't been too keen when I'd offered to take it on. (This reflection came in handy again years later, when I developed the residue formalism from the point of view of coherent duality). I was more often than not left behind during the Bourbaki congresses, especially during the joint readings of the essays, being unable to keep up with the readings and discussions at the rate they were going. Maybe I'm just not cut out for group work. In any case, this difficulty I had in fitting into the common work, or the reservations I may have aroused for other reasons in Cartan and others, never drew sarcasm or rebuff, or even a shadow of condescension, except once or twice from Weil (definitely a case apart!). At no time did Cartan deviate from an equal kindness towards me, imbued with cordiality and also with that distinctive touch of humor that for me remains inseparable from his person.

## 12.6. My friends from Survivre et Vivre

**Note 6** Among these friends, I should probably also count Pierre Samuel, whom I had previously known mainly through Bourbaki, as had Chevalley, and who (like Chevalley) played an important role in the Survivre et Vivre group. It doesn't seem to me that Samuel was much given to this illusion of the superiority of the scientist. Above all, I feel he contributed a great deal through the common sense and smiling good humor he brought to joint work, discussions and relations with others, as well as gracefully taking on the role of "ugly reformer" in a group inclined towards radical analyses and options. He remained with Survivre et Vivre for some time after I withdrew, acting as editor of the newsletter of the same name, and left with good grace (to join Friends of the Earth) when he felt that his presence in that group had ceased to be useful.

Samuel belonged to the same restricted milieu as me, but that didn't stop him from being one of my friends.

those bubbling years from which I think I learned something (however bad a student I was. . . ). These ways of being, like Chevalley's even though they hardly resemble each other, were a better antidote p. 144 for my "meritocratic" leanings, only the most incisive analysis!

It now seems to me that for all the friends I learned something from during this period, it was more through their way of being and their sensibility, which differed from mine, and from which "something" ended up being communicated, than through explanations, discussions, etc. ... . In addition to Chevalley and Samuel, I especially remember Denis Guedj (who had a great influence on the Survivre et Vivre group), Daniel Sibony (who kept his distance from this group, while pursuing his evolution with a half-disdainful, half-narcotic eye), Gordon Edwards (who was a co-actor in the birth of the "movement" in June 1970 in Montreal, and who for years did prodigious feats of energy to maintain an "American edition" of the Survivre et Vivre newsletter, in English), Jean Delord (a physicist about my age, who was also a member of the "Survivre et Vivre" group), and the other members of the group.

age, a fine, warm-hearted man who had taken a liking to me and the Survrien microcosm), Fred Snell (another US-based physicist from Buffalo, whose country house I stayed in for a few months in 1972).

Of all these friends, five are mathematicians, two are physicists, and all are scientists - which seems to show that the environment closest to me in those years remained an environment of scientists, and especially mathematicians.

## 12.7. Ø

**Note 7** The preceding paragraph is the first of the entire introduction to be heavily crossed out on my initial manuscript, and provided with numerous over-writings. The description of the incident and the choice of words initially went against the grain, against the current - a force was clearly pushing to get over the incident quickly, as if by conscience, to "get down to business". These are the familiar signs of **resistance**, here against the elucidation of this episode, and its significance as a revelation of an inner attitude. The situation is very similar to that described at the beginning of this introduction (par. 2), that of the "crucial" moment of the discovery of a contradiction and its meaning, in a mathematical work: it is then **the inertia** of the es-

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prit, its reluctance to part with an erroneous or inadequate vision (but one in which our person is in no way involved), which plays the role of the "resistance". This resistance is of an active nature, inventive if necessary to ~~die~~

drown a fish even without water, whereas the inertia I've been talking about is simply a passive force. In this case, even more than in the case of mathematical work, the discovery that has just appeared in all its simplicity, in all its obviousness, is followed in the instant by a feeling of relief from a weight, a feeling of **liberation**. It's not just a feeling - it's rather an acute, grateful perception of what has just happened, which **is** a liberation.

## 12.8. Ø

**Note 8** As will become clear later, this ambiguity did not "dissipate in the aftermath of the 1970 awakening". This is a typical strategic retreat of the "I", who writes off the period "before the awakening", which immediately becomes the demarcation line for an irreproachable "after"!

## 12.9. Ø

**Note 9** This is not entirely accurate; there is at least one exception among my closest colleagues, as will become apparent later. There was a typical "laziness" of memory, which often tends to "pass over" facts that don't "fit" with a familiar, long-established view of things.

## 12.10. Ø

**Note 10** For example, I've lost count of the number of letters, on mathematical as well as practical or personal matters, sent to colleagues or ex-students whom I considered friends, and who have never received a reply. It doesn't just seem to be a case of special treatment for



but a sign of a change in morals, according to echoes in the same vein. (Admittedly, these concern cases where the person sending a mathematical letter was not known to the recipient, a prominent mathematician... . )

## 12.11. Aldo Andreotti, Ionel Bucur

**Note 11** Of course, it's not impossible that I've forgotten - not to mention that my particularly "polar" disposition at the time would hardly encourage anyone to talk to me about this sort of thing, nor would it lead me to remember any conversation along these lines that might well have taken place. What is certain is that it must have been very exceptional, to say the least, for the question of fear to be broached (without even

to call it by that name... ), and it must be just as true today, especially in the "beau monde".

□ Among my many friends in that world, apart from Chevalley, who must have become aware of this am-  
The only other person I can think of who must have perceived it clearly was Aldo Andreotti. I had met him, his wife Barbara and their twin children (still very small) in 1955 (at a party at Weil's in Chicago, I believe). We remained close friends until the "great turning point" of 1970, when I left the milieu that had been ours and lost sight of them. Aldo had a very keen sensitivity, which hadn't been dulled by his dealings with mathematics and detective stories like mine. He had a gift for spontaneous sympathy for those he came into contact with. This set him apart from all the other friends I knew in the mathematical world, or even outside it. With him, friendship always took precedence over shared mathematical interests (of which there were plenty), and he was one of the few mathematicians with whom I spoke at all about my life, and he about his. His father, like mine, was Jewish, and had suffered in Mussolini's Italy, as I had in Hitler's Germany. I saw him always available to encourage and support young researchers, in a climate where it was becoming difficult to be accepted by the establishment. His spontaneous interest was always in people, not in mathematical "potential" or fame. He was one of the most engaging people I've ever had the good fortune to meet.

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This evocation of Aldo brings back memories of Ionel Bucur, who was also taken from us unexpectedly and before his time, and like Aldo, missed even more (I think) as a friend whom we like to meet again, than as a partner in mathematical discussions. We sensed in him a kindness, alongside an uncommon modesty, a propensity for constantly stepping aside. It's a mystery how a man so little inclined to take himself for granted or to impress anyone ended up as Dean of the Faculty of Sciences in Bucharest; no doubt because he didn't feel like refusing to accept the responsibilities that he was far from coveting, but which his colleagues or the political authorities were placing on his shoulders, which were, it must be said, robust. He was the son of peasants (something that must have played a role in a country where "class" is an important criterion), and had the common sense and simplicity of one. Surely he must have been aware of the fear that surrounds the man of notoriety, but certainly

so it must have seemed self-evident to him, a natural attribute of a position of power. I

don't think, however, that he himself □ had ever inspired fear in anyone, certainly not his wife Florica or to their daughter Alexandra, nor to their colleagues or students - and the feedback I've had is very much along these lines.

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## 12.12. Ø

**Note 12** The word "tomorrow" is to be taken literally, not as a metaphor.

## 12.13. Ø

**Note 13** Clearly, the foregoing description has no pretension other than to try and render as best I can, in concrete words, what this "fog" of memory delivers to me, which has not been condensed into any kind of case that is even remotely precise, of which I could have given a description here that was even remotely "realistic" or "objective". It would be a misrepresentation to suggest that colleagues who are reluctant to sit in the front rows, or who lack star or eminence status, are necessarily tied up in anguish when talking to one of the latter. This was clearly **not the** case for most of the friends I knew in this milieu, even among those who sometimes haunted conferences and seminars. What is unreservedly true is that the status of "eminence" creates a barrier, a gulf vis-à-vis those without such status, and that this gulf rarely disappears, even if only for the space of a discussion. I would add that the subjective distinction (which nevertheless seems very real to me) between the "front ranks" and the "marshes" cannot be reduced to sociological criteria (of social position, posts, titles, etc. . . ) or even of "status" or renown, but that it also reflects psychological particularities of temperament or dispositions that are more delicate to pin down. When I arrived in Paris at the age of twenty, I knew that I was a mathematician, that I had **done** maths, and despite the disorientation I've already mentioned, I basically felt "one of them", even though I was the only one to know it, and I wasn't even sure that I would continue to do math. Today, I'd be more inclined to sit in the back rows (on the rare occasions when the question arises).

## 12.14. Ø

**Note 14** One might think that this contradicts the assertion that there is no leader, but this is not the case.

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For Bourbaki alumni, it seems to me that Weil was perceived as the soul of the group, but never as a "leader". When he was there and when he liked it, he became a "ringleader" as I said, but he didn't do anything.

not the law. When he was in a bad mood, he could block discussion on a subject he disliked, even if it meant taking up the subject again at another congress when Weil wasn't there, or even the next day when he was no longer obstructing. Decisions were taken unanimously by the members present, given that it was by no means out of the question (nor even rare) for one person to be in the right against the unanimity of all the others. This may seem an aberrant principle for group work. The extraordinary thing is that it actually worked!

## 12.15. Ø

**Note 15** I didn't get the impression that this "allergy" to the Bourbaki style gave rise to any communication difficulties between these mathematicians and myself or other Bourbaki members or sympathizers, as would have been the case if the spirit of the group had been that of a chapel, of an elite within the elite. Beyond styles and fashions, there was in all members of the group a lively sense of mathematical substance, wherever it came from. It was only in the sixties that I remember one of my friends referring to mathematicians whose work he wasn't interested in as "pain in the ass". When it came to things I knew virtually nothing about, I tended to take such assessments at face value, impressed by such casual assurance - until one day I discovered that such a "pain in the ass" was an original and profound mind, which had not pleased my brilliant friend. It seems to me that with some

Bourbaki members, an attitude of modesty (or at least reserve) towards the work of others, when one is unaware of that work or understands it imperfectly, eroded at first, while that "mathematical instinct" which makes one feel a rich substance or a solid work, without having to refer to a reputation or a renown, still subsisted. From the echoes that reach me here and there, it seems to me that both modesty and instinct have become rare things today in what used to be my mathematical milieu.

## 12.16. ∅

**Note 16** To tell the truth, many of the Bourbaki members surely had their own microcosm "of their own", more or less extensive, apart from or beyond the Bourbakian microcosm. But perhaps it's no coincidence that in my own case, such a microcosm only formed around me after I had ceased to be part of Bourbaki, and all my energy was invested in tasks that were personal to me.

## 12.17. ∅

**Note 17** ☐ It was mainly outside the scientific community that I encountered warm echoes of the action . 149 to which I had committed myself, and active help. Apart from the friendly support of Alain Lascoux and Roger Godement, I must mention here above all that of Jean Dieudonné, who came to Montpellier for the Correctionnelle hearing, to add his warm testimony to others in favor of of a lost cause.

## 12.18. ∅

**Note 18** I believe that this lack of discernment was not due to any negligence on my part on those two occasions, but rather to a lack of maturity, an ignorance. It was only some ten years later that I began to pay attention to blocking mechanisms, whether in my own person, in those close to me or in students, and to measure the immense role they play in everyone's life, and not just at school or university. Of course, I regret not having had the discernment of greater maturity on these two occasions, but not for having expressed my impressions clearly, whether well-founded or not. When, in one case, I saw work done without seriousness, naming these things for what they are seems to me to be a necessary and beneficial thing. If, in yet another case, the conclusion I drew was hasty and unfounded, I was not the only one whose responsibility was engaged. The student thus shaken had the choice of either learning from it (which is perhaps what happened the first time), or letting himself be discouraged, and perhaps then changing profession (which isn't necessarily a bad thing either!).

## 12.19. Jesus and the twelve apostles

**Note 19** From 1970 to the present day, another student, Yves Ladegaillerie, has prepared and passed a thesis with me. The students of the first period are P. Berthelot, M. Demazure, J. Giraud. Mme M. Hakim, Mme Hoang Xuan Sinh. L. Illusie, P. Jouanolou. M. Raynaud, Mme M. Raynaud, N. Saavedra, J.L. Verdier. (Six of them actually completed their thesis work after 1970, at a time when my availability was still limited.

mathematics). Among these students, Michel Raynaud takes a special place, having found for himself the essential questions and notions that are the subject of his thesis work, which he moreover developed entirely independently; my role as "thesis director" proper was therefore limited to reading the finished thesis, constituting the jury and serving on it.

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□ When it was me who suggested a subject, I was careful to limit myself to those for which I had an interest. relationship strong enough for me to feel in a position, if need be, to support the student's work. A notable exception was Ms. Michèle Raynaud's work on local and global Lefschetz theorems for the fundamental group, formulated in terms of 1-fields on suitable scalar sites. The question seemed (and indeed proved) difficult, and I had no idea of how to prove the conjectures I was proposing (which, incidentally, could hardly be doubted). This work continued into the early '70s, and Mme Raynaud (as had previously been the case with her husband) developed a delicate and original method without any assistance from me or anyone else. This excellent work also opens up the question of extending Ms. Raynaud's results to the case of  $n$ -fields, which seems to me to represent the natural outcome, in the context of schemes, of theorems of the "weak Lefschetz theorem" type. However, the formulation of the relevant conjecture here (which can hardly be doubted either) makes essential use of the notion of  $n$ -field, the pursuit of which is supposed to be the main object of the present work<sup>2</sup>, as its name "A la Poursuite des Champs" indicates. We'll come back to this in due course,

Another rather special case is that of Mme Sinh, whom I had first met in Hanoi in December 1967, during a month-long lecture-seminar I gave at the evacuated Hanoi University. The following year, I offered her the subject of her thesis. She worked under particularly difficult wartime conditions, her contact with me being limited to occasional correspondence. She was able to come to France in 1974/75 (on the occasion of the International Congress of Mathematicians in Vancouver), and complete her thesis in Paris (before a jury chaired by Cartan, and including Schwartz, Deny, Zisman and myself).

Finally, I must mention Pierre Deligne and Carlos Contou-Carrère, both of whom were somewhat of a pupil, the former around 1965-68, the latter around 1974-76. Both obviously had (and still have) uncommon means, which they used in very different ways and with very different fortunes too. Before coming to Bures, Deligne had been a pupil of Tits (in

Belgium) - I doubt he was a student of anyone in mathematics, in the common sense of the term. Contou-Carrère had been a pupil of Santalo (in Argentina), and for a while of Thom! little □ou prou). One of them and the other already had the stature of a mathematician when the contact was established, except that

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Contou-Carrère lacked method and craft.

My mathematical role with Deligne was limited to informing him, on a weekly basis, of the little I knew about algebraic geometry, which he learned as if he'd always known it, and to raising questions along the way, which he usually answered on the spot or in the days that followed. These were the first works of Deligne's that I knew. His work after 1970 (for him as well as for my "official students") is known to me only through very scattered and distant echoes<sup>3</sup>.

My role with Contou-Carrère, as he himself says at the beginning of his thesis, was limited to introducing him to the language of schematics. In any case, I've only been remotely involved in the work he's been preparing as a doctoral thesis in recent years, on a highly topical subject that falls outside my remit.

<sup>2</sup>This is actually volume 3 of *Réflexions Mathématiques*, not the present volume 1 *Récoltes et Semailles* - see Introduction, p.(v).

<sup>3</sup>In particular, I had the opportunity to browse through some Berthelot and Deligne separate prints, which they were kind enough to send me.

Following a few misadventures in the wide world, Contou-Carrère was recently led, in extremis and (it now seems to me) unwillingly, to call on my services to act as thesis director and jury. (This exposed him to the risk of appearing as Grothendieck's pupil "after 1970", in a conjecture where this can present serious disadvantages... .). I carried out this task to the best of my ability, and this will probably be the last time I do so (at the level of a state doctorate thesis). I am all the happier, in this rather special circumstance, for the friendly assistance of Jean Giraud, who also took a month or two out of his time to do a thorough reading of the voluminous manuscript, of which he made a detailed and warm report.

## 12.20. ∅

**Note 20** This reminds me of the subject Monique Hakim had taken up, which wasn't much more engaging to tell the truth - I wonder how she managed to keep her spirits up! If she did suffer at times, it was certainly not to the point of making her sad or sullen, and the work between us was done in a cordial and relaxed atmosphere.

## 12.21. ∅

**Note 21** □ It would perhaps be more accurate to say that for the temperament that is mine, it is **maturity** which I still lack to fully assume a teaching role. My acquired temperament has long been marked by an excessive predominance of "masculine" (or "yang") traits, and one of the aspects of maturity is precisely a "yin-yang" balance with a "feminine" (or "yin") predominance.

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(Added later.) Even more than maturity, I see that it's a certain **generosity** that I've lacked in my teaching life to date - a generosity that expresses itself in a more delicate way than availability of time and energy, and which is more essential. This lack didn't manifest itself visibly (through an accumulation of failed situations, let's say) in my first period of teaching, no doubt mainly because it was compensated for by a strong motivation in the students who chose to come and work with me. In the second period, on the other hand, from 1970 to the present day, it seems to me that this lack of motivation is at least one of the reasons, and in any case the one that involves me most directly, for the overall failure that I observe in my teaching at research level (from the DEA level upwards). On this subject, see "Esquisse d'un programme", par.8, and par.9 "Bilan d'une activité enseignante", where the sense of frustration that this activity has left me with for the last seven or eight years is apparent<sup>4</sup>.

## 12.22. ∅

**Note 22** Not for much longer, perhaps, since I have decided to apply for admission to the Centre National de la Recherche Scientifique, thus putting an end to a teaching activity in a university environment, which in recent years has become increasingly problematic.

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<sup>4</sup>Compare also note (23iv), added later.

## 12.23. Ø

**Note 22** Even after 1970, when my interest in maths became sporadic and marginal in my life, I don't think there was an occasion when I recused myself when a student called on me to work with him. I can even say that, apart from two or three cases, my post-1970 students' interest in the work they were doing was far below my own interest in their subject, even in periods when I had to work with them.

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where I didn't worry much about maths except on the days I set foot in college. Also the kind of availability I had to mes pre-1970 students, and the extreme demand for work that was a sign of it

In the main, they would have made no sense to most of my later students, who did maths without conviction, as if by a continual effort they had to make on themselves... ..

## 12.24. The Child and the Master

**Note 23** The term "transmit" here doesn't really correspond to the reality of things, which reminds me of a more modest attitude. This rigor is not something that can be transmitted, but at most awakened or encouraged, while it is ignored or discouraged from a very young age, by the family environment as well as by school and university. As far back as I can remember, this rigor has been present in my quests, those of an intellectual nature at least, and I don't think it was passed on to me by my parents, and even less by masters, at school or among my mathematician elders. It seems to me to be one of the attributes of **innocence**, and therefore one of the things that everyone is born with. Very early on, this innocence "sees a lot of green and a lot of black", which means that it is obliged to plunge more or less deeply, and that often there is hardly a trace of it left in the rest of one's life. In my case, for reasons I haven't yet thought of investigating, a certain innocence has survived at the relatively innocuous level of intellectual curiosity, whereas everywhere else it has plunged deep, unseen and unheard of, just like everyone else. Perhaps the secret, or rather the mystery, of "teaching" in the full sense of the word, lies in reconnecting with this seemingly vanished innocence. But there's no question of rediscovering this contact in the pupil, if it isn't already present or rediscovered in the person of the teacher himself. And what is "transmitted" by the teacher to the pupil is by no means this rigor or innocence (innate in both of them), but a respect, a tacit revaluation of this commonly rejected thing.

## 12.25. Ø

**Note 23** For the past seven or eight years, however, there has been another chronic "source of frustration" in my life as a mathematician, but one that has expressed itself much more discreetly over the years. It eventually became apparent through an effect of repetition, of obstinate accumulation of the same type of "frustrating" situation.

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in my teaching activity, and by finally bursting into a sort of "fed up!", causing me to practically put an end to all so-called de " research direction" activity. I touch on this issue once or twice at the

In the course of my reflection, I finally examine it at least a little at the very end. At the very least, I describe this frustration, and examine the role it played in my "return to maths" (cf. par.50. "Weight of a past").

## 12.26. Fear of playing

**Note 23** This student had worked with me on a DEA "work placement" for a whole year, and remained "contracted" in his working relationship with me right up to the end. It was a frankly friendly relationship, shot through with a mutual sympathy that could not be doubted. Yet there was this "stage fright"; this fear, the real cause of which was surely not fear of me, even though it looked like it. I might not even have noticed it, had this student not told me about it himself, no doubt to "explain" more or less the reason for an almost complete block in his work during the year.

As was the case with other students who, like him, were initially hooked on a certain geometrical substance, the blockage manifested itself from the moment it was a question of doing "work on parts", i.e. putting statements in black and white, or just grasping the meaning and significance of those I was providing and proposing to admit as the foundations of a language, as the "rules of the game". School reflexes almost always push the student, faced with a situation where he's supposed to be "doing research", to adopt as a "given", both vague and imperative, the implicit "rules of the game" handed down by the teacher, and which it's certainly not a question of trying to make explicit, let alone understand. The concrete form these implicit rules take are "recipes" for semantics or arithmetic, along the lines of, say, a mole book (or any other common textbook). What's more, the pupil expects the teacher to perform a task of the form "demonstrate that...". (Incidentally, I don't believe that the attitudes of most professional mathematicians, and other scientists too, are essentially different - except that the "master" is replaced by the "consensus", which sets the rules of the game at the time and regards it as an immutable given. This consensus also defines the "problems" to be solved, between which everyone feels free to choose as they wish, even allowing themselves to modify them as they go along. of its work, or even to invent new ones. ... ). I noticed that the entirely different attitude that

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is mine vis-à-vis a mathematical substance that needs to be probed, and therefore also vis-à-vis the student, almost certainly triggers disarray, one of the signs of which is anguish. Like all anxiety, this will tend to take on a face, projecting itself onto an external "reason", plausible or not. One of the most common faces of anguish is fear.

Such difficulties hardly arose in the first period of my teaching activity, except perhaps in the two cases where a "teacher-student" relationship didn't continue beyond a few weeks, and perhaps (I couldn't say) in the case of the "sad student", who perhaps felt "riveted" to a subject that didn't inspire him at all, even though he had every opportunity to change it. In the case of the student (whom I also mentioned) who remained afflicted by stage fright for a long time, it's clear that the reason lay elsewhere. He was by no means blocked in his work, but on the contrary perfectly at ease with the theme he had chosen, on which he did a great deal of groundwork. In fact, most of my students during this period were former students of the Ecole Normale, and their contact with Henri Cartan had already shown them the example of a "different" approach to mathematics. At the opposite end of the spectrum (so to speak), in my second period as a teacher at the University of Montpellier, it was among first-year students that the anguish I've mentioned least interfered with reflective work. For many of these students, astonishment at a different approach didn't provoke anguish or closure, but rather openness and a willingness to do interesting things for once! From my observations, the effect of a few years in college on a student's creative disposition is radical and devastating. It's a strange thing that in this respect the effect of the long years of high school seems relatively

trivial. Perhaps the reason is that the college years come at an age when the creativity innate in us **must** ultimately be expressed through personal work, otherwise we'll be shipwrecked forever, at least in terms of creative work of an intellectual nature. It must have been a healthy instinct that during my years as a student (also at the Fac de Montpellier) I practically refrained from setting foot in class, devoting almost all my energy to <sup>personnel</sup> mathematical reflection.

## 12.27. The two brothers

**Note 23** This student's antagonism took the form, from the outset, of a "class antagonism": I was the "boss" who had "power of life and death" over his mathematical future, which I could decide at my own pleasure. ... Of course, events only confirmed this vision, as I soon put an end to my (now painful) responsibilities towards this student. This put him in a tricky situation, in these times when it's not so easy to find a "boss", especially when the subject has already been chosen. For the other student, frustrated in his legitimate expectations, the antagonism took a similar form. I felt like the tyrannical "mandarin", who could not tolerate contradiction from those (students or lower-ranking colleagues) he considered his subordinates.

Such a "class attitude" never manifested itself, if at all, during the relationship with my students of the first period. The obvious reason was that, in the pre-1970 context, there was no doubt that the student, once he had passed his thesis, would have a position as a lecturer, and would therefore enjoy a social status identical to mine, that of "university professor". Loquacious figures: the eleven students who began working with me before 1970 were given lectureships as soon as their work was completed, whereas none of the twenty or so students who worked more or less under my direction had access to such a position. It's true that only two of them were motivated enough to do a state doctorate thesis (an excellent one in both cases).

It's hardly surprising, then, that in this second period, certain ambivalences (whose deeper origins remained hidden) took the form of class antagonism, of distrust (presented and felt as "visceral") towards the "boss". For one of those who had been more or less a pupil, friendly relations continued for ten years without any apparent antagonistic episode, and yet marked by this same ambiguity, expressed by an attitude of mistrust, held "in reserve" behind manifest sympathy. To tell the truth, I've never been fooled by this "mistrust" of command, which

appeared to me above all as a reason that this friend sees fit to give himself for not venturing outside the well-defined domain he has chosen as his own, in his professional life as in his life all short - something he is free to do, however, without anyone (except, at most, himself!) calling him to account. ...

In fact, these three cases are the only ones in all my teaching experience where a certain ambivalence in the relationship between a student (or someone who is more or less a student) and myself has been expressed in a "class attitude". Such an attitude appears particularly ambiguous when it manifests itself between colleagues within a university "body" where they both enjoy exorbitant privileges compared to the situation of ordinary mortals, privileges which make differences in rank (and salary) appear relatively insignificant. In fact, I've noticed that these attitudes disappear as if by magic (and with good reason!), as soon as the person concerned sees himself promoted to the position of which only the day before he was complaining to others.

In fact, I detect a similar ambiguity in most, if not all, of the conflict situations I've encountered.



within the mathematical world (and often outside it too). Those who have been "castrated", whether or not their rank corresponds to their expectations (justified or not), enjoy quite unheard-of privileges, which no other profession or career can offer. Those who don't fit in aspire to the same security and privileges (which doesn't necessarily prevent them from taking an interest in maths itself, and sometimes from doing great things). These days, when the competition for a place is fierce, and the unhoused are often treated like stragglers, I've more than once felt the connivance between the one who enjoys humiliating and the one who is humiliated - and who swallows and crushes. The real object of his bitterness and animosity is **not** the one who has made use of power, but is none other than **himself**, who has crushed himself and invested the other with this power which he uses at pleasure. The one who takes pleasure in humiliating is also the one who takes his revenge and compensates (without ever erasing it. . . ) for a long-lasting humiliation that has long since been buried and forgotten. And he who acquiesces in his own humiliation is his brother and emulator, who secretly envies it and in bitterness buries both the humiliation, and the humble message about himself that it brings him.

## 12.28. Teaching failure (1)

### Note 23ivSince

these lines were written, I have had the opportunity to speak ☐with two of my ex-studentsp . 158  
 from after 1970, to try to probe with them the reason for the failure of my teaching at the level of re- research, at the University of Montpellier. They told me that the propensity I had to underestimate the difficulty that the assimilation of such techniques, familiar to me but not to them, could represent for them, had a discouraging effect on them, as they constantly felt they were falling short of the expectations I had of them. What's more (and this seems even more far-reaching to me), they sometimes felt frustrated when I "sold the worm" by giving them a shaped statement I had up my sleeves, instead of letting them discover it for themselves, at a time when they were already very close to it. After that, all they had to do was the "exercise" (which they weren't otherwise keen on) of proving the statement in question. Herein lies the "lack of generosity" in me that I had noted in an earlier note (note 21), without elaborating further. It is disappointments such as these, above all, that represent my personal contribution to the disappearance of interest in research in both of us, after what was nonetheless an excellent start.

I realize that I was no more generous before 1970 than after. If I didn't have the same difficulties then, it's undoubtedly because the kind of students who came to me in those days were motivated enough to find even a "long exercise" appealing, which was an opportunity to learn the trade and a host of other things along the way; and also, for a starter statement I was "selling the fuse" on, to come up with a host of others on their own that went far beyond the first. When I changed teaching location, I made the necessary adjustment in the choice of topics for reflection that I proposed to my new students, by choosing mathematical objects that could be grasped by immediate intuition, independently of any technical baggage. But this essential adjustment was in itself insufficient, due to differences in **disposition** (in my new students compared to those of yesteryear), even more important, than a single difference in **baggage**. This ties in with the observation made earlier (beginning of par.25) about a certain inadequacy in me for the role of "master", which came out much more strongly in my second period as a teacher than in the first.

## 12.29. Ø

**Note 23v** □ A particularly striking sign of this difference was seen in the "episode

strangers", which I have already mentioned (section 24). While I did receive expressions of sympathy from many people who were complete strangers to me, I don't recall any of my pre-1970 students thinking of expressing this, let alone offering me any help in the action I had embarked upon. On the other hand, I can't think of a single one of my students or former students from the second period who didn't express their sympathy and solidarity with me, and several actively joined the campaign I was running at local level. Beyond this restricted circle, the 1945 ordinance affair also created a certain emotion among many students at the Faculty who knew me by name at most, and a good number of them came to the Palais de Justice on the day of my summons, to show their solidarity. This last circumstance suggests, moreover, that the difference I observed between the attitudes of my students "before" and "after" 1970 may express less the difference in **relations** between them and me, than a difference in **mentalities**. Clearly, my "before" pupils had become important people, and it takes a lot for important people to consent to be moved... . But the episode of my departure from the IHES in 1970 and my involvement in militant action seems to show that there's more to it than that. It was a time when none of them was yet such an important figure, and yet I don't remember any of them showing the slightest interest in the activity I was getting involved in. Rather, I think it must have made them uncomfortable, all of them without exception. This again points to a difference in mentality, but one that can't be blamed solely on differences in social status.

## 12.30. ∅

**Note 24** The ethics I'm talking about apply just as much to any other milieu formed around a research activity, and where the possibility of making one's results known, and taking credit for them, is a matter of "life or death" for the social status of any member, or even of "survival" as a member of this milieu, with all the consequences this implies for him and his family.

## 12.31. Deontological consensus - and information control

**Note 25** Apart from the conversation with Dieudonné, I can't recall a conversation I've participated in or witnessed, during my life as a **mathématicien**, where the ethics of the profession were discussed, rules of the game" in relations between members of the profession. (I exclude here the discussions about the collaboration of scientists with the military, which took place in the early 70s around the "Survivre et Vivre" movement. They didn't really concern the relationships between mathematicians. Many of my friends in Survivre et Vivre, including Chevalley and Guedj, felt that the emphasis I placed at that time, especially in the early days, on this question to which I was particularly sensitive, distracted me from more essential everyday realities, of precisely the type I am examining in the present reflection). These things were never discussed between a student and myself. The tacit consensus was limited, I believe, to this one rule: not to present as one's own any ideas of others of which one may have been aware. This consensus, it seems to me, has existed since antiquity and has not been challenged in any scientific milieu to this day. But in the absence of this other complementary rule, which guarantees every researcher the possibility of making his ideas and results known, the first rule remains a dead letter. In today's scientific world, men in positions of prestige and power hold the key to success.

discretionary control of scientific information. This control is no longer tempered, in the milieu I had known, by a consensus such as Dieudonné spoke of, which perhaps never existed outside the restricted group whose spokesman he was. The scientist in a position of power practically receives all the information he deems useful to receive (and often even more), and has the power, for much of this information, to prevent its publication while keeping the benefit of the information and rejecting it as "uninteresting", "more or less well known", "trivial", etc. ... I return to this situation in note (27).

## 12.32. ∅

**Note 26** The "founding members" of Bourbaki are Henri Cartan, Claude Chevalley, Jean Delsarte, Jean Dieudonné. André Weil. They are all still alive, with the exception of Delsarte, who died before his time in the 1950s, at a time when the ethics of the profession were still generally respected.

Rereading the text, I was tempted to delete this passage, in which I can give the impression of to award certificates of "probity" (or non-probity) which the interested parties have no use for, and which it is not my responsibility ☐ de faire. The reservation this passage may arouse is surely justified. Nevertheless, I retain it, p. 161  
for the sake of authenticity, and because this passage does convey my feelings, however misplaced they may be.

## 12.33. Youth snobbery", or the defenders of purity

**Note 27** Ronnie Brown shared with me a reflection by J.H.C. Whitehead (of whom he was a pupil), speaking of the "snobbery of the young, who believe: that a theorem is trivial because its proof is trivial". Many of my old friends would do well to ponder these words. Today, this "snobbery" is by no means limited to young people, and I know more than one prestigious mathematician who routinely practices it. I'm particularly sensitive to it, because the best I've done in mathematics (and elsewhere too. . . ), the notions and structures I've introduced that seem to me to be the most fruitful, and the essential properties I've been able to extract from them through patient and persistent work, all fall under the label of "trivial". (None of these things would have stood much chance of being accepted for a CR grade these days, were the author not already a celebrity!) My lifelong ambition as a mathematician, or rather my passion and joy, has been constantly to **find the obvious things**, and this is my sole ambition also in the present work (including the present introductory chapter. . . ). The decisive thing is often already to see the **question** that had not been seen (whatever the answer may be, and whether it has already been found or not) or to come up with a statement (even if it is conjectural) that sums up and contains a situation that had not been seen or understood; if it is demonstrated, it doesn't matter whether the demonstration is trivial or not, which is entirely incidental, or even whether a hasty and provisional demonstration proves to be false. The snobbery of which Whitehead speaks is that of the jaded wine-lover who deigns to appreciate a wine only after he has ascertained that it has cost a great deal of money. More than once in recent years, caught up in my old passion, I've offered the best I had, only to see it rejected by that kind of smugness. I've felt a pain that's still alive, a joy that's been disappointed - but that doesn't mean I'm homeless, and fortunately for me, I wasn't trying to fit in an article of my own.

p. 162 The snobbery of which Whitehead speaks is an abuse of power and a dishonesty, not only an insensibility or a closure to the beauty of things, when exercised by a man of power against  
of a researcher to his <sup>merci</sup>, whose ideas he has free rein to assimilate and use, while blocking their publication on the grounds that they are "obvious" or "trivial", and therefore "uninteresting". I'm not even thinking here of the extreme situation of plagiarism in the common sense of the term, which must still be very rare in the mathematical world. However, from a practical point of view, the situation is the same for the researcher who pays the price, and the inner attitude that makes it possible doesn't seem much different to me either. It's simply more comfortable, since it's accompanied by a feeling of infinite superiority over others, and the good conscience and intimate satisfaction of the intransigent defender of the intangible purity of mathematics.

## 12.34. Ø

**Note 28** In writing the preceding pages, I was initially divided between a desire to "get it off my chest", and a concern for reserve or discretion. As a result, I had remained in a state of "à-peu-près", which was surely the main reason for my unease, my feeling that "I wasn't learning anything". Since the lines noting this malaise were written, I've twice rewritten those pages that had left me feeling internally discontented, getting more clearly involved and getting to the bottom of things. Along the way, I did indeed "learn something", and I also believe that I managed to put my finger on something important that goes beyond the case in point and beyond myself.

## 12.35. Ø

**Note 29** I'm referring here to an intense, long-term investment in mathematics, or in some other entirely intellectual activity. On the other hand, the unfolding of such a passion - which can be a way of reacquainting ourselves with a forgotten force within us, and an opportunity to measure ourselves against a reluctant substance and, in the process, renew and enrich our sense of identity with something truly personal to us - such an unfolding may well be an important stage in an inner journey, in a maturing.

## 12.36. Ø

**Note 30** In recent years, my children have taken over the task of teaching a sometimes reluctant pupil about the mysteries of human existence. ...

## 12.37. Ø

p. 163 **Note 31** I'm thinking here of the "yang" form of the desire to know - the one who probes, <sup>discovers</sup>, names what appears. ... It is having been **named** that makes the knowledge that has appeared irreversible, ineffaceable (even though it would later come to be buried, forgotten, cease to be active. ... ). The "yin", "feminine" form of the desire for knowledge is in an openness, a receptivity, in a silent welcoming of a

knowledge appearing in deeper layers of our being, where thought has no access. The appearance of such openness, and of a sudden knowledge that for a time erases all traces of conflict, comes as a grace once again, touching deeply even though its visible effect may be ephemeral. I suspect, however, that this wordless knowledge that comes to us in this way, at certain rare moments in our lives, is just as ineffaceable, and its action continues even beyond the memory we may have of it.

## 12.38. A hundred irons in the fire, or: there's no point in drying out!

**Note 32** **W h e n** I was still doing Functional Analysis, that is, until 1954, I would sometimes persist endlessly on a question I couldn't solve, even though I had no more ideas and was content to go round in circles with old ideas that obviously didn't "bite" any more. This was the case, in any case, for a whole year, notably for the "approximation problem" in topological vector spaces, which would only be solved some twenty years later by methods of a totally different order, which could only have escaped me at this point. I was driven then, not by desire, but by stubbornness, and by an ignorance of what was going on inside me. It was a painful year - the only time in my life when doing math had become painful for me! It took that experience for me to realize that there's no point in "skipping" - that once a piece of work has reached a standstill, and as soon as you've realized it has, you have to move on to something else - even if it means coming back to the question at hand at a more propitious moment. This moment almost always comes quickly - the question matures, without me even pretending to touch it, simply by virtue of working with gusto on questions that may seem to have nothing to do with this one. I'm convinced that if I persisted, I wouldn't get anywhere even in ten years! It was from 1954 onwards that I got into the habit in maths to always have many irons in the fire at the same time. I only work on one at a time, but by a kind of miracle that constantly renews itself, the work I do on one  $\square$  profits the other as well. all the others, biding their time. It was the same, without any deliberate intention on my part, from my first contact with meditation - the number of burning questions to be examined increased day by day, as the reflection continued. ...

p. 164

## 12.39. $\emptyset$

**Note 33** This does not mean that moments when paper (or the blackboard, which is a substitute!) is absent are not important in mathematical work. This is especially true in the "sensitive moments" when a new intuition has just appeared, when it's a question of "getting to know" it in a more global, more intuitive way than by "working on parts", which this informal stage of reflection prepares. In my case, this kind of reflection takes place mostly in bed or out for a walk, and it seems to me that it accounts for a relatively modest proportion of total work time. The same observations apply to meditation work as I've practised it up to now.

## 12.40. The powerless embrace

**Note 34** The word "embrace" is by no means a mere metaphor for me, and the common language here reflects a profound identity. It could be said, not without reason, that it is not true that "embrace" is a metaphor.

without wonder is powerless - that the earth would be depopulated, if not deserted, if it were so in the literal sense. The extreme case is that of rape, in which wonder is certainly absent, even though it happens that a being is procreated in the raped woman. Of course, the child born of such an embrace cannot fail to bear the mark of this embrace, which will be part of the "package" he or she receives and must assume; but this does not prevent a new being from being conceived and born. that there has been **creation**, a sign of **power**. And it's also true that some mathematicians I've seen, full of self-importance, find and prove beautiful theorems, signs of an embrace that didn't lack force! But it's also true that if the life of such a mathematician is suffocated by his smugness (as was to some extent the case in my own life, at one time), the fruits of these embraces with mathematics are a blessing to him and to no-one else. And the same can be said of the father and mother of a child born of rape. When I speak of a "powerless embrace", I mean above all the powerlessness to engender **renewal** in those who believe:

p. 165 create, whereas all he creates is a **product**, something external to him, with no deep resonance within himself; a □product which, far from freeing him, creating harmony within him, binds him more closely to the fatuity within him of which he is

a prisoner of the constant pressure to produce and reproduce. This is a form of powerlessness at a deep level, behind the appearance of "creativity" which is basically just unbridled **productivity**.

I've also had ample opportunity to realize that complacency, the inability to marvel, is in the nature of true blindness, a blockage of natural sensitivity and flair; if not total and permanent, at least manifest in certain situations. It's a state of affairs in which a prodigious mathematician sometimes reveals himself, in the very things in which he excels, to be as stupid as the most stubborn of schoolchildren! On other occasions, he will perform prodigious feats of technical virtuosity. I doubt, however, that he is yet in a position to discover the simple and obvious things that have the power to renew a discipline or a science. They are far too far below him for him to deign to see them! To see what no one deigns to see, he needs an innocence that he has lost, or banished. . . It's no coincidence, surely, with the prodigious increase in mathematical production over the last twenty years, and the unrolling profusion of new results with which the mathematician who simply wants to "keep up to date" is inundated, that (as far as I can judge from the echoes that reach me here and there) there has hardly been any real **renewal**, any far-reaching transformation (and not just by accumulation) of any of the major themes of thought with which I have been even remotely familiar. Renewal is not a quantitative thing, it is foreign to a quantity of investment, measurable in a number of mathematician-days devoted to a given subject by such and such a "level" of mathematicians. A million mathematician-days is powerless to give birth to something as childlike as the zero, which has renewed our perception of number. Only innocence has this power, a visible sign of which is wonder. ...

## 12.41. ∅

**Note 35** This "gift" is nobody's privilege; we're all born with it. When it seems absent in me, it's because I've chased it away myself, and it's up to me to welcome it back. In me or in such-and-such a person, this "gift" expresses itself in a different way than in another, less communicative, less irresistible perhaps, but it's no less present, and I couldn't say if it's less active.

## 12.42. ∅

p. 166 □ **Note 36** Such a delicate sensitivity to beauty seems to me intimately linked to something I've had

I've had occasion to refer to it as "exigency" (with regard to oneself) or "rigor" (in the full sense of the word), which I described as "attention to something delicate in ourselves", attention to a quality of understanding of the thing being probed. This quality of **understanding** of a mathematical thing cannot be separated from a more or less intimate, more or less perfect perception of the "beauty" particular to that thing.

## 12.43. ∅

**Note 37** I think I hardly need to add that this long-term work has brought out, day by day, much more than the "result" I have just delivered in lapidary form. It's no different for a work of meditation than it is for a mathematical work motivated by a particular question that we set out to examine. Quite often, the twists and turns of the road followed (which may or may not lead to a more or less complete clarification of the initial question) are more interesting than the initial question or the "final result".

## 12.44. ∅

**Note 38** These notes were in fact a continuation of the long letter to. . . which became the first chapter. They were typed so as to be legible for this old friend, and for two or three others (Ronnie Brown in particular) whom I thought might be interested. This letter, by the way, was never answered, nor was it read by the addressee, who almost a year later (when I asked him if he'd received it) expressed sincere astonishment that I'd even thought for a moment that he could read it, given the kind of mathematics that was to be expected of me. ...

## 12.45. ∅

**Note 39** This is the period, among others, of the "Long Walk through Galois theory", discussed in "Esquisse d'un Programme" (par.3: "Corps de nombres associés à un dessin d'enfant").

## 12.46. The visit

**Note 40** The work on this dream is the subject of a long letter in English, to a friend and colleague who had dropped in on me the day before. Some of the materials used by the Dreamer to bring this strikingly realistic dream out of apparent nothingness were obviously borrowed from this short episode of the visit.

of a dear friend I hadn't seen in nearly ten years. <sup>Ausi</sup>si, on the first day of work and against from my past experience, I thought I could conclude that the dream that had come to me concerned my friend more than it concerned me - that it was **he** who should have had the dream, not me! It was a way of evading the message of the dream, which (I should have known from my past experience) concerned no one but me. I finally realized this in the night that followed this first, superficial phase of the work, which I resumed the next day in the same letter. Since that memorable letter, I have received no further sign of life from this friend, one of the closest I have ever had.

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This work was the only meditation that took the form of a letter (and in English to boot), so I no longer have a written record of it. I was particularly struck by this episode, one of many that show how any sign of work that goes beyond a certain façade, and brings to light simple facts that we generally make a point of ignoring - how any such work inspires unease and fear in others. I'll come back to this later (see par. 47, "The solitary adventure").

## 12.47. Krishnamurti, or liberation turned hindrance

**Note 41** It would be inaccurate to say that the only thing I took away from this reading was a certain vocabulary, and a propensity to make it my own and ultimately substitute it, appropriately enough, for reality. The reason I was so struck by Krishnamurti's first book (even though I'd only had the chance to read a few chapters) was that what he was saying totally overturned a number of things I'd always taken for granted, and which I immediately realized were **commonplaces** that had always been part of the air I'd breathed. At the same time, this reading drew my attention, for the first time, to far-reaching facts, especially that of flight from reality, as one of the most powerful and universal conditioning of the mind. This gave me an essential key to understanding situations that until then had been incomprehensible and therefore (without my realizing it until I discovered meditation five or six years later) generating anguish. I've noticed

p. 168 immediately the reality of this escape all around me. This unraveled certain anxieties, without however changing anything essential, because I only saw this reality in others, while <sup>□</sup>me figuring (as going from I was, in fact, the exception that confirmed the rule (and without asking myself any further questions about this truly remarkable exception). In fact, I was in no way curious about myself or others. This "key" can only **open in the** hands of the person motivated by the desire to penetrate. In my hands, it had become an exorcism and a pose.

It was at the beginning of 1974 that, for the first time, I realized that the destruction in my life, which had been following me step by step, could not have come from others **alone**, that there was something **within me** that attracted it, fed it, perpetuated it. It was a moment of humility and openness, conducive to renewal. But the renewal remained peripheral and ephemeral, for lack of in-depth **work**. This "something inside me" was still vague. I could see that it was a lack of love, but the very idea of working to identify more closely where and how there had been a lack of love in me, how it had manifested itself, what its concrete effects had been, etc. . - (On the contrary, K. likes to insist on the vanity of all work, which he automatically equates with the ego's "craving to become"). So, with borrowed "wisdom" as my compass, I saw nothing to do but wait patiently for "love" to descend upon me like a grace from the Holy Spirit.

Yet the humble truth I'd just learned in the depths of a wave had triggered a powerful surge of new energy, comparable to that which would carry me through my first foray into meditation two and a half years later. This energy did not remain entirely unused. A few months later, when I was immobilized by a providential accident, it led to a (written) reflection in which, for the first time in my life, I examined the worldview that had been the unspoken basis of my relationship with others, and which had come to me from my parents and especially my mother. I then realized very clearly that this vision had failed, that it was incapable of accounting for the reality of relationships between people, and of fostering personal fulfillment and relationships with others. This reflection remains



by the "Krishnamurti style", and also by the Krishnamurtian taboo on any real **work** towards understanding. However, she made tangible and irreversible a knowledge born a few months before, restéep at first vague and elusive. No book and no other person in the world could have given me this knowledge. . 169

To have the quality of a meditation, what this reflection lacked above all was a look at myself and my **vision of myself**, and not just my vision of the world, a system of axioms in which I wasn't really "in the flesh". It also lacked a look at myself in **the moment**, at the very moment of reflection (which fell short of a real work); a look that would have allowed me to detect not only a borrowed style, but also a certain complacency in the literary aspect of these notes, a lack of spontaneity and authenticity. Inadequate though it was, and relatively limited in its immediate effects on my relationships with others, this reflection nonetheless seemed to me to be a step, probably necessary given the starting point, towards the more profound renewal that was to take place two years later. It was then, at last, that I discovered meditation - and discovered that first unsuspected fact: **that there were things to discover about myself** - things that almost completely determined the course of my life and the nature of my relationships with others. . .

## 12.48. The salutary wrench

**Note 42** "The "percussive" event in question was the discovery, at the end of 1969, that the institution to which I felt I belonged was partly financed by funds from the Ministry of the Armed Forces, something which was incompatible with my basic axioms (and still is, in fact). This event was the first in a whole chain of others (each more revealing than the last!) which resulted in: my departure from the IHES (Institut des Hautes Etudes Scientifiques), and one thing leading to another, a radical change of environment and investments.

During the heroic years of the IHES, Dieudonné and I were the only members, and also the only ones to give it credibility and an audience in the scientific world, Dieudonné through the publication of "Publications Mathématiques": the first volume of which appeared as early as 1959, the year after the IHES was founded.

IHES by Léon Motchane), and I by the "Séminaires de Géométrie Algébrique". In those early years, the IHES' existence remained <sup>most</sup>precarious, with uncertain funding (through the generosity of a fewp companies acting as patrons) and with the only premises a room lent (with visible bad humor) by the Fondation Thiers in Paris for the days of my seminar<sup>5</sup>. I felt a bit like a "scientific" co-founder, with Dieudonné, of my home institution, and I intended to end my days there! I had come to identify strongly with the IHES, and my departure (as a consequence of my colleagues' indifference) was experienced as a kind of uprooting from another "home", before proving to be a liberation. . 170

Looking back, I realize that there must already have been a need for renewal within me, although I can't say how long ago it was. It's surely no mere coincidence that the year before I left IHES, there was a sudden shift in my investment of energy, leaving the tasks that had been burning in my hands the day before, and the questions that fascinated me most, to throw myself (under the influence of a biologist friend, Mircea Dumitrescu) into biology. I went into it with the attitude of a

<sup>5</sup>A recent brochure published by the IHES to mark the twenty-fifth anniversary of its foundation (of which Nico Kniper was kind enough to send me a copy) says nothing about these difficult beginnings, perhaps unworthy of the solemnity of the occasion, which was celebrated with great pomp last year.

long-term investment in IHES (in keeping with the institution's multi-disciplinary vocation). Surely this was no more than an outlet for the need for a much more profound renewal, which could not have been achieved in the "scientific incubator" atmosphere of the IHES, and which took place during that "cascade of awakenings" to which I have already alluded. There have been seven, the last of which took place in 1982. The "military funds" episode was providential in triggering the first of these "awakenings". The Ministry of the Armed Forces, like my ex-colleagues at IHES, have finally earned my gratitude!

## 12.49. Ø

**Note 43** "The poetic work of my composition" contains many things that I know first-hand, and which today appear to me to be just as important in my life, and "in life" in general, as when it was written, with the intention of publishing it. If I have refrained from doing so, it is above all because I

I later realized that the form was afflicted by a deliberate intention to "make poetic", so that its overly-constructed ensemble conception, and many passages, lack spontaneity, au

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The form was at times painfully stiff and swollen. This form, bulky at times, was a reflection of my disposition, where it was decidedly often the "boss" who called the shots - heavily, it goes without saying... .

## 12.50. Ø

**Note 44** Needless to say, I'm disregarding the hypothesis - by no means improbable, to say the least - of the unexpected eruption of an atomic war or some other such joyous event, likely to put an abrupt end, once and for all, to the collective game called "Mathematics", and to much else besides... .

**Part two.**

**BURIAL (I)**

**or the Emperor of China's robe**



To those who have been my  
friends and to the few who  
have remained so  
and to those who came in great numbers to sing at my  
funeral

To the memory of a memorable

Colloquium... and to the entire

Congregation...



## 9. A) HERITAGE AND INHERITANCE

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## 13.1. I The student posthumous

### 13.1.1. Teaching failure (2) - or creation and fatuity

**Note 44'** [This note is called by section 50 of Chapter **VIII The Lonely Adventure** of Part (I) **Fatuity and Renewal** p. 227]

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This passage struck a chord with the friend I asked to read this last section, "The weight of a past" <sup>1</sup>(\*). He wrote to me: "For many of your former students, the aspect, as you say, of the invasive and almost destructive 'boss' has remained strong. Hence the impression you have." (Knowing, I presume, "the impression" that is expressed in certain passages of this section and notes n°s 46,47,50 that complete it.) Earlier he writes: "First of all, I think you did well to leave mathematics for a moment [!], because there was a kind of incomprehension between you and your students (apart from Deligne, of course). They were left a little dumbfounded...".

It's the first time I've heard such high praise for my role as "boss" before 1970, going beyond the usual compliments! Further up in the same letter: ". . I understand that your former students [read: those "before 1970"] don't really know what it's like to **create** mathematics, and that you may have had something to do with it. . . It's true that in their time, the problems were all posed...". <sup>2</sup>(\*\*).

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My correspondent probably means that it was **I** who was posing the "problems", and with them the notions of that I had to develop, rather than leaving it up to my students to find them; and that it was in this way that I may have obscured in them the knowledge of what is the essential part of the work of mathematical creation. This also ties in with a □ impression that emerged from the conversation with two of my post-1970 ex-students, mentioned in a previous note (note (23iv)). It's true that, in the students who came to me, I was above all looking for **collaborators** to develop intuitions and ideas that had already formed within me, to "push at the wheels", in short, of a carriage that was already there, which they therefore didn't have to pull out of a kind of nothingness (as my correspondent had to do). And yet, this is what has always been the most fascinating aspect of mathematical work for me, and the part of the work in which I felt that a "creation" was taking place, the "birth" of something more delicate and more essential than a simple "result".

If I sometimes see some of my students treating this prized thing with disdain, and thus displaying the "snobbery" of which J.H.C. Whitehead spoke (which consists in despising what one "would know how to dis- show") <sup>3</sup>(\*), I'm no doubt no stranger to it, in one way or another. The failure of my teaching, glaringly obvious in the period after 1970, is now also apparent to me, in a different and more hidden form, in my teaching in the first period, even though in the conventional sense it appears to have been a complete success! It's something I've already glimpsed from time to time over the last few years, and which I've mentioned in letters to several of my ex-students, without so far having received any real response from any of them.

I don't think it would be accurate, however, to say that the work I proposed to my students, and what they did with me, was purely technical work, of pure routine, unsuited to bringing their personalities into play.

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<sup>1</sup>(\*) (May 10) The friend in question is none other than Zoghman Mebkhout, who has kindly authorized me to lift the anonymity I felt I had to maintain regarding the origin of the letter (April 2, 1984) I quote in this note.

<sup>2</sup>(\*\*) (May 10) The above quotation is heavily truncated, out of respect for my correspondent's anonymity. See the following note for a full quotation of the passage from which this quotation is taken, and for comments on its true meaning, which had initially escaped me for lack of more detailed information.

<sup>3</sup>(\*) See note "Youth snobbery - or the defenders of purity", n° 27 p. 247.



creative faculties. I put at their disposal tangible and sure starting points, between which they were free to choose, and from which they could launch out, as I myself had done before them. I don't think I've ever proposed a subject to a pupil that I wouldn't have enjoyed tackling myself; nor do I think there has been such an arid path in the journey that any of them have taken with me. that I haven't myself gone through others just as arid in the course of my mathematical life, without getting discouraged or rushing through them.

when it was clear that the job had to be done and there was no other way.

□ Also it seems to me that the failure I see today has more subtle causes than the type of themes I proposed, and to what extent these remained nebulous or were, on the contrary, clear-cut. My share in this failure seems to me to be due rather to attitudes of fatuity in my relationship to mathematics; attitudes which I have had occasion to examine in this reflection. If not in the actual work with a particular student, then at least in the atmosphere that surrounded me. Fatuity, even when it is expressed in the most "discreet" way in the world, always goes in the direction of a closure, an insensitivity to the delicate essence of things and their beauty - whether these are "mathematical things", or living people whom we have the power to welcome, encourage, or also to look upon from the height of our grandeur, insensitive to the breath that accompanies us and to its destructive effects on others as on ourselves.

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### 13.1.2. A feeling of injustice and powerlessness

**Note 44** [The appearance of this note does not respect the chronological order of writing]

(May 10) Taking advantage of my friend's permission to quote freely from his letters as I see fit, I give here a more complete quotation<sup>4</sup> (\*), which places the truncated quote in its true context:

"It's true that I was very isolated between 75-80, apart from a few rare questions to Verdier. But I don't blame your former students for that period, because nobody really understood the importance of this link [read: between discrete coefficients and continuous coefficients]. That all changed in October 1980, when we discovered the first very important application of this link for semisimple groups, namely the proof of the Kazhdan-Lusztig multiplicity formula, where we made essential use of the category equivalence in question. This equivalence took the name of "Riemann-Hilbert correspondence" without further comment - after all, it's so natural! That's when I realized that your former students don't really know what a mathematical **creation** is, and that perhaps you were partly to blame. I still feel a sense of injustice and powerlessness. It's true that by their time, the problems had all been solved. The number of applications of this theorem is impressive, both in the context of the transcendental framework, but always under the name of correspondence.

of Riemann-Hilbert! I feel my name is unworthy of this result for □ many people and in particular for your former students. But as you can clearly see from the introductions to my work, it's your "duality" formalism that naturally leads to this result. But like you, I'm not worried about the future of this link between "constructible discrete coefficients" and crystalline coefficients (or holonomic D-Modules). It's clear that it applies in many fields both in the cohomology of spaces and in analysis."

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It was this passage from my friend's letter that inspired (in addition to the present note) the later "L'inconnu" (The unknown).

<sup>4</sup>(\*) See second b. de p. of the previous note. "The failure of teaching (2) - or creation and fatuity", n° 44'.

and the good Lord's theorem". From the terms of this letter, I had no idea (as I explain in its place) that this "feeling of injustice and powerlessness" in my friend was the reaction, not simply to an attitude of blind disdain systematically **minimizing** his contributions (an attitude which has become quite familiar to me, among some of my former students), but to a veritable operation of swindling, consisting in purely and simply **swindling** the paternity of a theorem - key. This situation became clear to me only eight days ago - see the note "L' Iniquité - ou le sens d'un retour" and the following notes (n° s 75 to 80), grouped together under the title "Le Colloque - ou faisceaux de Mebkhout et Perversité".

**Note 45** My change of environment and lifestyle has meant that opportunities to meet or make further contact with my old friends have become rare. This has not prevented signs of "distancing" from manifesting themselves in many ways, to a greater or lesser degree from one to another. With others, however, such as Dieudonné, Cartan or Schwartz, and indeed with all the "elders" who had given me such a warm welcome in my early days, I felt nothing of the kind. Apart from these, however, I have the impression that there are very few of my former friends or students in the mathematical world whose relationship with me (whether or not it finds occasion to express itself) has not become divided, "ambivalent", after I withdrew from what was once a common milieu, a common world.

## 13.2. II orphans

### 13.2.1. My orphans

**Note 46** [This note is called by section 50 of Chapter **VIII The Lonely Adventure** of Part **(I) Fatuity and Renewal** p. ]

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□ I'd like to take this opportunity to say a few words here about mathematical notions and ideas, of all those I've uncovered, which seem to me (by far) to have the greatest impact  $(46)_1^5$  (\*). First and foremost, there are five closely related key concepts, which I'll briefly review, in order of increasing specificity and richness (and depth).

The first is the idea of a **derived category** in homological algebra (see note 48 on p. 274), and its use in a "catch-all" formalism known as **the "six operations formalism"** (i.e. the operations of the "six operations").

$\otimes^L, Lf^* Rf_!, RHom, Rf^*, Lf^!$  )  $(46_2)$  for the cohomology of the most important types of "spaces" which have so far been introduced in geometry: "algebraic" spaces (such as schemas, schema- tic multiplicities, etc. . . ), analytic spaces (both complex analytic, and rigid-analytic and similar), topological spaces (pending, of course, the context of "moderated spaces" of all kinds, and surely many more besides, such as that of the category (Cat) of small categories, serving as homotopic models. . . ). This formalism covers both discrete and continuous coefficients.

The gradual discovery of this duality formalism and its ubiquity came about through a solitary, obstinate and demanding process of reflection, which continued between 1956 and 1963. It was in the course of this reflection that the notion of derived category gradually emerged, along with an understanding of its proper role in homological algebra.

<sup>5</sup>(\*) Readers will find in notes n° 46<sub>1</sub> to 46<sub>9</sub> some more technical comments on the concepts reviewed in the present note. On the other hand, independently of the specific **notions** I have introduced, the reader will find some reflections

on what I consider to be "the main part" of my work (within the part of my work "fully completed"), in note no.° 88 "La dépouille".

What was still lacking in my vision of the cohomological formalism of "spaces" was an understanding of the link between discrete coefficients and continuous coefficients, beyond the familiar case of "spaces". local systems and their interpretation in terms of integrable connection modules, or modules. This deep connection, first formulated in the framework of complex analytic spaces, was discovered and established (nearly twenty years later) by Zoghman Mebkhout, in terms of derived categories formed on the one hand using "constructible" discrete coefficients, on the other using the notion of "*D-Module*" or "complex of differential operators" (cf. note 46<sub>3</sub> p. ).

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For almost ten years, Zoghman Mebkhout pursued his remarkable work in almost total isolation, in the absence of encouragement from those of my former students who were best placed to give it to him, and to back him up with their interest and the experience they had acquired through my contact. This did not prevent him from discovering and proving two key theorems<sup>6</sup> (\*) of a new crystalline theory that was being born in the midst of general indifference, both of which (and this was a decidedly bad sign!) expressed in terms of derived categories: one giving the category equivalence between "constructible discrete" coefficients and crystalline coefficients (satisfying certain conditions of "holonomy" and "regularity") (48'), the other being "**the**" crystalline global duality theorem, for the constant application of a smooth complex analytic space (not necessarily compact, which entails difficulties techniques) to a point. These are deep theorems<sup>7</sup> (\*\*), which throw

a nouveau day on the cohomology of both analytic and schematic spaces (in characteristic zero for the moment), and hold out the promise of a far-reaching renewal of the cohomological theory of these spaces. After two unsuccessful applications to the CNRS, they finally earned their author a position as a research fellow (equivalent to an assistant or senior assistant at the University).

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No one in these ten years has thought of telling Mebkhout, who is grappling with the considerable technical difficulties of the transcendent context, about the "six-variance formalism", well known to my students<sup>8</sup> (\*), but nowhere to be found "on the net". He finally learned of its existence from me last year (in the form of a form that, apparently, is known only to me. . . ), when he was kind enough and patient enough to explain to me what he had done; to me, who wasn't so much into cohomology any more . . . Nor did anyone think to suggest to him that it might be more "profitable" to start with the context of zero-characteristic schemes, where the difficulties inherent in the transcendental context disappear, and where, on the other hand, the conceptual questions fundamental to the theory appear all the more clearly. No one thought of pointing out to him (or even noticed what was known to me from the time I introduced crystals<sup>9</sup> (\*\*)) that the "*D-Modules*" on spaces (analytic or schematic)

<sup>6</sup>(\*) (June 7) Mebkhout points out that, in addition to these two theorems, a third must be added, also expressed in terms of derived categories, namely what he has called (somewhat improperly perhaps) the "**biduality theorem**" for *D-Modules*, and which is the most difficult of the three. For an overview of Mebkhout's ideas and results and their uses, see Le Dung Trang and Zoghman Mebkhout, Introduction to linear differential Systems, Proc. of Symposia in Pure Mathematics, vol.40 (1983) part.2, pp. 31-63.

<sup>7</sup>(\*\*) (May 30) The demonstration of the second theorem runs up against the technical difficulties usual to the transcendental context, requiring the use of "eavesque" techniques, so I guess it can be classed as a "difficult" demonstration. That of the first theorem is "obvious" - and profound, using the full force of Hironaka's resolution of singularities.

As I point out in the penultimate paragraph of the note "Solidarity" (n° 85), once the theorem has been worked out, "anyone" who is well-informed is capable of proving it. Compare also with J.H.C. Whitehead's observation quoted in the note "Youth snobbery - or the defenders of purity" (n° 27). When I wrote this last note, as if under the silent dictation of a secret prescience, I had no idea how far reality would go beyond my timid, groping suggestions!

<sup>8</sup>(\*) They learned this first-hand in the SGA 4 and SGA 5 seminars, and by text, in R. Hartshorne's "Residues and Duality".

<sup>9</sup>(\*\*) (May 30) But I had time to forget it - only to remember it again by virtue of the second encounter with Mebkhout, last year. (See note "Rencontre d'outre-tombe", n° 78.

smooth spaces are no more and no less than "**module crystals**" (when we disregard any question of "coherence" for either of them), and that the latter was a catch-all notion that worked just as well for "spaces" with any singularities as it did for smooth spaces (46 ).<sup>4</sup>

Given Mebkhout's means (and uncommon courage), it is quite clear to me that, placed in an atmosphere of sympathy, he would have had no difficulty but great pleasure in establishing the complete formalism of the "six variances" in the context of the crystalline cohomology of zero characteristic schemes, while all the ideas essential for such a large-scale program (including his own in addition to those of the others) would have been available to him.

p. 180 of the Sato school and my own) were already, it seemed to me, combined. For someone of his caliber, this was a matter of a few years' work, as was the development of an all-purpose formalism from cohomologie étale was a matter of a few years (1962-1965), as long as the common thread of the six operations was already known (in addition to the two key theorems of base change). It's true that these were years carried by a current of enthusiasm and sympathy from those who were co-actors or witnesses, and not a work against the haughty smugness of those who have everything in hand. ...

This brings me to the second pair of notions I wanted to talk about, that of **schema**, and the closely related one of **topos**. The latter is the more intrinsic version of the notion of **site**, which I first introduced to formalize the topological intuition of "localization". (The term "site" was later introduced by Jean Giraud, who also did much to give the notions of site and topos the necessary flexibility). It was the obvious needs of algebraic geometry that led me to introduce diagrams and topos one after the other. This pair of notions potentially contains a vast renewal of algebraic geometry, arithmetic and topology, through a **synthesis of** these "worlds", too long separated, in a common geometric intuition.

The renewal of algebraic geometry and arithmetic through the point of view of diagrams and the language of sites (or of "descent"), and through twelve years of work on the foundations (not counting the work of my students and other willing participants), has been accomplished over the last twenty years: the notion of diagram, and that of spread cohomology of diagrams (if not that of spread topos and that of spread multiplicity) have finally entered the mainstream, and the common heritage.

On the other hand, this vast synthesis, which would also encompass topology, while for the past twenty years ideas have been

p. 181 essential and the main technical tools required seem to me to be gathered and ready<sup>10</sup> (\*), is still biding its time. For five years (since my departure from the mathematical scene), the fertile unifying idea and the The notion of topos, a powerful tool for discovery, has been relegated by a certain fashion<sup>11</sup> (\*) to the sidelines of serious notions. Even today, few topologists have the slightest inkling of this considerable potential expansion of their science, and of the new resources it offers.

In this renewed vision, the topological spaces, differentiable spaces, etc. ... that the topologist handles daily are, along with schemas (which he has heard of) and topological multiplicities, differentiable spaces, etc. ... .

<sup>10</sup>(\*) (May 15) These "essential ideas and principal technical means" had been brought together in the vast fresco of the SGA 4 and SGA 5 seminars, between 1963 and 1965. The strange vicissitudes that befell the writing and publication of the SGA 5 part of this fresco, which appeared (in unrecognizable, devastated form) eleven years later (in 1977), give a striking picture of the fate of this vast vision in the hands of "a certain fashion" - or rather, in the hands of some of my students who were the first to introduce it (see note b. on next p.). These vicissitudes and their meaning are gradually revealed in the course of reflection over the last four weeks, continuing in the notes "Le compère", "La table rase", "L'être à part". "The signal", "The renversement", "Le silence", "La solidarité", "La mystification", "Le défunt", "Le massacre", "La dépouille", notes n° s 63", 67, 67', 68, 68' and 84-88.

<sup>11</sup>(May 13) Further reflection in the six weeks since these lines were written (end of March), has revealed that this "fashion" was first and foremost established by some of my students - the very ones who were best placed to make a certain vision, ideas and technical means their own, and who chose to appropriate working tools, while disowning both the vision that had given rise to them, and the person in whom this vision had originated.

or schematics (which nobody talks about) are all embodiments of the same type of remarkable geometric objects, the **ringed topos** (46<sub>5</sub>), which play the role of "spaces" in which intuitions from topology, algebraic geometry and arithmetic converge into a common geometric vision. The "modular" multiplicities of all kinds that we encounter at every step (provided we have eyes open to see) provide so many striking examples (46<sub>6</sub>). Their in-depth study is a first-rate guide to the essential properties of geometric objects (or other objects, if there are objects that are not geometric. . .), whose variation, degeneration and generalization are described by these modular multiplicities. Yet this richness remains ignored, since the notion that allows us to describe it in detail doesn't fit into commonly accepted categories.

Another unexpected aspect brought by this recused synthesis<sup>12</sup> (\*\*), is that  $\square$  the homotopic invariants familiar from some of the most common spaces (46<sub>7</sub>) (or more precisely, their profinite compactifications) are equipped with unsuspected arithmetic structures, including operations of certain profinite Galois groups.

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...

And yet, for almost fifteen years now, it has been part of the "high society" to look down one's nose at anyone who utters the word "topos", unless it's in jest, or they have the excuse of being a logician. ) Nor has the yoga of derived categories, to express the homology and cohomology of topological spaces, penetrated among topologists, for whom K nneth's formula (for a ring of coefficients that is not a body) still continues to be a system of two spectral sequences (or, at the very least, a kyrielle of short exact sequences), and not a unique canonical isomorphism in a suitable category ; and who still ignore the base change theorems (for a proper morphism or by a smooth morphism, for example), which (in the neighboring framework of stale cohomology) were the crucial turning point for the strong "start" of this cohomology (cf. note 46<sub>8</sub> p. 270). I shouldn't be surprised when the very people who helped develop this yoga have long since forgotten about it, and beat the crap out of anyone who even pretends to want to use it!<sup>13</sup> (\*).

The fifth notion that is closest to my heart, perhaps more than any other, is that of "**motif**". It differs from the previous four in that "**the**" right notion of pattern (even above a basic body, let alone a basic schema of any kind) has not so far been the subject of a

satisfactory definition, even admitting for this purpose all the "reasonable" conjectures one would need. Or rather, visibly,  $\square$  **the** "reasonable conjecture" to be made, as a first step, would be that of p. 183

the **existence of** a theory, satisfying such data and such properties, that it would not be at all difficult

<sup>12</sup>(\*\*) (May 13) This synthesis was "rejected" in the first place, both in its spirit and in the key notion that makes it possible, by none other than the very person who was the main user and beneficiary, throughout his work, of the technical means it had enabled me to develop (with the language of diagrams and the construction of a theory of staggered cohomology). It's Pierre Deligne. Because of his exceptional influence (due to his exceptional means), and because of the very special position he occupied in relation to my work, of which he was like an implicit legatee, the discreet and systematic barrage he put up against the main ideas I had introduced (with the exception of the notion of schema and staggered cohomology) was highly effective, surely playing a leading role in the establishment of the "fashion" that **buried** these ideas, reduced for nearly fifteen years to a vegetative life. His work has been deeply marked by this ambiguity, which I first glimpsed in the reflection that continues that of the present note. (See "Refus d'un h ritage - ou le prix d'une contradiction", note

n  47 p. 271) This initial perception, vivid but still confused, of this permanent hindrance in Deligne's work after my departure, became clearer and confirmed in a striking way during all the reflection on this Burial, in which my friend plays the role of principal official.

<sup>13</sup>(\*) (May 13) In the course of further reflection, it became clear that the situation began to change with the Colloque de Luminy in June 1981: those who had "forgotten" (or rather, buried. . .) these notions began to strut about with them, without however ceasing to

(See notes  s 75 and 81 on this memorable Colloquium).

(and quite fascinating!), for someone in the know<sup>14</sup> (\*), to fully explain. In fact, I came very close to doing so, shortly before I "quit maths".

In some respects, the situation resembles that of the "infinitely small" in the heroic days of differential and integral calculus, but with two differences. Firstly, we now have the experience in building sophisticated mathematical theories, and an effective conceptual baggage, that our predecessors lacked. And then, in spite of the resources at our disposal and in the more than twenty years since this visibly essential notion first appeared, no one has deigned (or dared, in spite of those who don't deign. . . ) to put their hand to the grindstone and outline a theory of patterns, as our predecessors did for infinitesimal calculus without beating about the bush. However, it's as clear now for patterns as it was for "infinitesimals", that these beasts exist, and that they manifest themselves at every step in algebraic geometry, as long as we're interested in the cohomology of algebraic varieties and families of such varieties, and more particularly in their "arithmetic" properties. Perhaps even more so than for the other four notions I've mentioned, that of motif, which is the

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and richest of all, is associated with a multitude of intuitions of all kinds, by no means vague but for  $\square$ mululable often with perfect precision (even sometimes, if need be, admitting a few motivic premises). For me, the most fascinating of these "motivic" intuitions was that of the "motivic Galois group", which, in a sense, makes it possible to "put a motivic structure" on the Galois groups profi- ned by bodies and schemes of finite type (in the absolute sense). (The technical work required to give precise meaning to this notion, in terms of the "premises" providing a provisional foundation for the notion of motive, was accomplished in Neantro Saavedra's thesis on "Tannakian categories").

The current consensus is a little more nuanced for the notion of pattern than for its three brothers (or sisters) in misfortune (derived categories, duality formalism known as the "six operations", topos), in the sense that it is not treated outright as "bombast"<sup>15</sup> (\*). In practice, however, it all boils down to the same thing: as long as there's no way of "defining" a motif and "proving" something, serious people can only refrain from talking about it (with the greatest regret, it's a matter of course, but you're either serious or you're not... .). Of course, we may never be able to construct a theory of motives and "prove" anything about them, as long as we declare that it's not serious even to talk about them!

But the few people in the know (and who make the fashion) know very well that in terms of the premises, which remain secret, many things can be proved. In other words, ever since the notion first appeared in the wake of Weil's conjectures (though proven by Deligne, which makes it

even a good point!), **pattern yoga does** exist. But it has the status of a **secret science**, with admittedly very few initiates<sup>16</sup> (\*\*). It may be "not serious", but it nevertheless allows  $\square$ those rare initiates to say

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in a host of cohomology situations, "what you'd expect". It thus gives rise to

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<sup>14</sup>(\*) (May 13) I've come to understand that the only person (apart from myself) who to this day responds to the rather peculiar meaning of this "somewhat in the loop" is Pierre Deligne, who for four years has had the advantage of being the day-to-day confidant of my motivic reflections, at the same time as listening to "the little I knew about algebraic geometry". It's true that I've talked about these things to many other colleagues here and there, but none of them has apparently been "plugged in" enough to assimilate an overall vision that had been developing in me over several years, or to take my indications as a starting point for developing a vision and program on their own (as I myself had done from two or three "strong impressions" produced by some of Serre's ideas). Perhaps I'm mistaken, but it seems to me that people interested in the cohomology of algebraic varieties were not psychologically disposed to "take the motifs seriously" for as long as Deligne, who was an authority on cohomology and at the same time the only one supposed to know in depth what these motifs were all about, himself passed them over in silence.

(June 8) Checking this out, it appears that my first motivational reflections date back to the early sixties - so they've been going on for almost a decade.

<sup>15</sup>(\*) As I mentioned in a previous footnote, the derived categories were exhumed three years ago to great fanfare (without my name being mentioned). The topos and the six operations are still waiting for their time, and the motives

a multitude of intuitions and partial conjectures, which are sometimes accessible after the fact by the means at hand, in the light of the understanding provided by "yoga". Several of Deligne's works are inspired by this yoga<sup>17</sup> (\*), notably the one that (if I'm not mistaken) was his first published work, establishing the degeneracy of the Leray spectral sequence for a projective and smooth morphism of algebraic varieties (in null char., for the purposes of demonstration). This result was suggested by considerations of "weight", of an arithmetical nature. These are typically "motivic" considerations, by which I mean they can be formulated in terms of the "geometry" of motives. Deligne proved this statement with Lefschetz-Hodge theory and (if I remember correctly) said nothing about motivation (49), without which no one would have suspected anything so implausible!

In fact, the yoga of motifs was born, first and foremost, from this "yoga of weights" that I inherited from Serre<sup>18</sup> (\*\*). It was he who made me understand the charm of Weil's conjectures (now Deligne's theorem). He explained to me how (modulo a hypothesis of resolution of singularities in the envisaged characteristic) one could, thanks to the yoga of weights, associate "virtual Betti numbers" with every algebraic variety (not necessarily smooth or proper) over any body - something that struck me a lot at the time (46<sub>9</sub>). It was this idea, I believe, that was the starting point for my thinking on weights, which continued (in parallel with my tasks of writing foundations) throughout the years of

following years. (This is also the one I took up in the 70s, with the notion of "virtual pattern" on any base diagram, with a view to establishing a formalism of the "six operations" at least for p-adic patterns. virtual). Over the years, I've talked to Deligne (as a privileged interlocutor) about this yoga of motives, and to anyone who would listen<sup>19</sup> (\*). I certainly didn't want him and others to keep it a secret science, reserved for them alone. (⇒note 47 p. 271)

**Note** 46<sub>1</sub> At most, I would make an exception for the ideas and points of view introduced with the formulation I had given to the Riemann-Roch theorem (and with the two demonstrations I found of it), as well as for various variants of it. If I remember correctly, such variants appeared in the last lecture of the 1965/66 SGA 5 seminar, which was lost along with various other lectures from the same seminar. The most interesting seems to me to be a variant for constructible discrete coefficients,

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also, except for the little piece unearthed two years ago, with an alternate authorship (see notes n°s 51, 52, 59). (May 13)  
<sup>16</sup>(\*\*) (May 13) I understand now that the "very few insiders" were reduced to the one and only Deligne until 1982.

It's true that he revealed this "secret science" through certain important results included in this yoga, revealed as and when he was able to prove them, in order to take credit for them while concealing his source of inspiration, which remained secret. And yet, for fifteen years, no one has come up with a far-reaching theory of patterns, because our times are decidedly far from the bold dynamism of the heroic age of infinitesimal calculus!

<sup>17</sup>(\*) (May 13) Having familiarized myself with the bibliography to some extent. I now see that Deligne's entire work is rooted in this yoga. And my bibliographical sampling (as well as other cross-checking) leads me to assume that in Deligne's entire work, the only reference to this source is found in a lapidary line (quoting me in a headline with Serre) in "Hodge Theory I" in 1970. (See notes n°s 78 and 78.)<sub>1 2</sub>

<sup>18</sup>(\*\*) What I've got from Serre (early 60s?) is an initial idea or intuition, making me realize that there was something important to understand! This acted as an initial impulse, triggering a process of reflection that continued over the following years, first on a "yoga" of weights and soon on a broader yoga of patterns.

<sup>19</sup>(\*) (April 10) It seems to me that Deligne was the only one to "hear" - and he was careful to reserve the exclusive privilege of what he heard. It's also true that in writing these final lines, I was "delaying" events: two years ago, there was a partial exhumation of the Yoga des motifs, without any hint of a role I'd played in it! See notes n°s 50, 51, 59, prompted by an unforeseen discovery that shed unexpected light (for me at least) on the meaning of the burial that had been going on for twelve years. Until then, I had been vaguely aware of a sort of burial, without taking the time to look more closely. ...

which I don't know whether it has since been made explicit in the literature<sup>20</sup> (\*\*). Note that this also admits a "motivic" variant, which essentially amounts to asserting that the "characteristic classes" (in the Chow ring of a regular scheme  $Y$ ) associated with constructible  $\chi$ -adic bundles for different prime numbers  $\chi$  (prime to residual characteristics), when these bundles come from the same "word" "pattern" (e.g. are  $Rf_! (\underline{Z}_\chi)$  for a given  $f : X \rightarrow Y$ ) are all equal.

p. 187 **Note 46<sub>2</sub>** This formalism can be seen as a kind of quintessential "**global duality**" formalism in cohomology; in its most "efficient" form, freed of all superfluous assumptions (of smoothness in particular for the "spaces" and applications considered, or of cleanliness for the morphisms' There is a need to to complement it with a **local duality** formalism, in which we distinguish among the "coefficients" admitted the objects or "complexes"  $\square$  said to be "**dualizing**" (a notion made stable by the operation  $Lf^!$ ), i.e. those giving rise to a "**biduality theorem**" (in terms of the  $RHom$  operation) for coefficients satisfying suitable finiteness conditions (on degrees, and consistency or "constructability" on local cohomology objects). When I speak of the "six-variance formalism"; I hereafter imply this complete duality formalism, both in its "local" and "global" aspects.

A first step towards a deeper understanding of duality in cohomology was the progressive discovery of the six-variance formalism in a first important case, that of Noetherian schemes and cohomology-coherent complexes of modules. A second was the discovery (in the context of spread cohomology of schemes) that this formalism also applied for discrete coefficients. These two extreme cases were sufficient to establish the conviction that this formalism was **ubiquitous** in all geometric situations giving rise to a Poincaré-type "duality" - a conviction that was confirmed by the work (among others) of Verdier, Ramis and Ruget. This conviction will undoubtedly be confirmed for other types of coefficients, once the fifteen-year **blockade** against the development and widespread use of this formalism has been broken down.

This ubiquity seems to me a **fact of** considerable significance. It made it imperative to feel a profound unity between Poincaré's duality and Serre's duality, which was finally established with the required generality by Mebkhout. This ubiquity makes the "six-variance formalism" one of the fundamental structures in homological algebra for an understanding of cohomological duality phenomena "tous azimuts"<sup>21</sup> (\*). The fact that this rather sophisticated structure has not been made explicit in the past (any more than the "right" notion of "triangulated category", of which the Verdier version is still a very provisional and inadequate form) doesn't change a thing; nor does the fact that topologists, and even algebraic geometers who pretend to be interested in cohomology, continue to ignore the very existence of the duality formalism, as well as the language of derived categories on which it is based.

p. 188 **Note 46<sub>3</sub>** The point of view of D-Modules and complexes of differential operators was introduced by Sato and developed first by him and his school, in a rather different optic (it seemed to me to understand) Mebkhout's approach, which is closer to my own.

The various notions of "**constructibility**" for "discrete" coefficients (in the analytic-complex, analytic-real, piecewise linear contexts) were first teased out by me, it seems, in the late 1950s (and I took them up again a few years later in the context of staggered cohomology). At the time, I raised the question of the stability of this notion of higher direct images for

<sup>20</sup>(\*\*) (June 6) I found it again (in a similar form, and under the flattering name of "Deligne-Grothendieck conjecture") in an article by Mac-Pherson published in 1974. For details, see note n° 87<sub>1</sub>.

<sup>21</sup>(\*) The interested reader will find an outline of this formalism in the Appendix to this volume.



a proper morphism of real or complex analytic spaces, and does not know whether this stability has been established in the complex analytic case<sup>22</sup> (\*). In the real analytic case, the notion I had envisaged was in fact the wrong one, for want of Hironaka's notion of a real subanalytic set, which possesses the essential liminal property of stability by direct images. As for operations of a local nature such as  $\underline{R}\mathrm{Hom}$ , it was clear that the argument establishing the stability of constructible coefficients in the framework of excellent schemes of zero characteristic (using the resolution of Hironaka singularities) worked as is in the complex analytic case, and likewise for the biduality theorem (see SGA 5 I). In the piecewise linear framework, natural stabilities and the biduality theorem are "easy exercises", which I had enjoyed doing as a check on the "ubiquity" of the duality formalism, when starting up staggered cohomology (one of whose main surprises had been precisely the discovery of this ubiquity).

Returning to the semi-analytical case, the "right" framework in this direction for stability theorems (coefficients constructible by the six operations) is obviously that of "moderated spaces" (see Esquisse d'un Programme, par. 5, 6).

**Note 46<sub>4</sub>** Of course, the D-Modules point of view, combined with the fact that  $D$  is a coherent ring bundle, brings to light a more hidden notion of "coherence" for moduli crystals than the one I used to work with, and which still makes sense on spaces (analytic or schematic) that are not necessarily smooth. It would only be fair to call it "**M-coherence**" (M as in Mebkhout). It should be

It is therefore obvious to anyone with a clue (and in full possession of their healthy mathematical instincts) that the "right category of coefficients" which generalizes the complexes  $\square$  "of differential operators" p. 189

in the smooth case, must be none other than the "M-coherent" derived category of that of moduli crystals (a crystal complex being called **M-coherent** if its cohomology objects are). This makes reasonable sense without smoothness assumptions, and should encompass both the theory of ordinary "continuous" (coherent) coefficients, and that of "constructible" discrete coefficients (introducing suitable holonomy and regularity assumptions for the latter). If my vision of things is correct, the two new conceptual ingredients of Sato-Mebkhout's theory, compared with the previously known crystalline context, are this notion of M-coherence for moduli crystals, and the holonomy and regularity conditions (of a deeper nature) concerning M-coherent complexes of crystals. With these notions acquired, a first essential task would be to develop the six-variance formalism in the crystalline context, so as to encompass the two special cases (ordinary coherent, discrete) I had developed over twenty years ago (and which some of my ex-cohomology students have long since forgotten in favor of arguably more important tasks... ).

Mebkhout had eventually learned of the existence of the notion of "crystal" by frequenting my writings, and he felt that his point of view should give a good approach to this notion (at least in zero characteristic) - but this suggestion fell on deaf ears. Psychologically, it was hardly conceivable that he would embark on the vast task of laying the foundations, placed as he was in a climate of haughty indifference on the part of the very people who were cohomological authorities, and best placed to encourage - or discourage. ...

**Note 46<sub>5</sub>** (May 13) The focus here is on topos ringed by a **local commutative** ring. The idea of describing a "variety" structure in terms of the datum of such a ring bundle on a topological space, a

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<sup>22</sup>(\*) (May 25) Established by J.L. Verdier, see "Les bonnes références" note n° 82.

was first introduced by H. Cartan, and was taken up by Serre in his classic work FAC (Faisceaux algébriques cohérents). It was this work that provided the initial impetus for a reflection that led me to the notion of "schema". What was still missing in Cartan's approach, taken up by Serre, to encompass all the types of "spaces" or "varieties" that have arisen to date, was the notion of topos (i.e. precisely "something" on which the notion of "bundle of sets" makes sense, and possesses the familiar properties).

p. 190 **Note 46<sub>6</sub>** □ As other remarkable examples of topos that are not ordinary spaces, and for the-

As there doesn't seem to be a satisfactory substitute in terms of "accepted" notions either, I'd like to point out the following: quotient topos of a topological space by a local equivalence relation (e.g. manifolds of varieties, in which case the quotient topos is even a "multiplicity", i.e. is locally a variety); "classifying" topos for just about any kind of mathematical structure (at least those "ex- priming" in terms of finite projective limits). e. is locally a variety); "classifying" topos for just about any kind of mathematical structure (at least those "ex- priming in terms of finite projective limits and inductive limits of any kind"). When we take a "variety" structure (topological, differentiable, real or complex analytic, Nash, etc. . . or even smooth schematic on a given basis) we find in each case a particularly attractive topos, which deserves the name of "universal variety" (of the species under consideration). Its homotopic invariants (and in particular its cohomology, which deserves the name of "classifying cohomology" for the species of variety under consideration) should have been studied and known long ago, but for the moment it's taking no such course. ...

**Note 46<sub>7</sub>** These are spaces  $X$  whose homotopy type is "naturally" described as that of a complex algebraic variety. The latter can then be defined on a subbody  $K$  of the complex field, such that  $K$  is a finite-type extension of the prime field  $\mathbb{Q}$ . The profinite Galois group  $Gal(\bar{K}/K)$  operates then naturally on the homotopic invariants profinis of  $X$ . Often (e.g. when  $X$  is an odd-dimensional homotopic sphere) we can take the prime field  $\mathbb{Q}$  for  $K$ .

**Note 46<sub>8</sub>** (May 13) When I learned my first rudiments of algebraic geometry from Serre's FAC article (which was to "trigger" me in the direction of schemas), the very notion of base change was virtually unknown in algebraic geometry, except in the special case of base body change. With the introduction of the language of schemes, this operation has become probably the most commonly used in algebraic geometry, where it can be introduced at any time. The fact that this operation is still virtually unknown in topology, except in very special cases, strikes me as a typical sign (among many others) of topology's isolation from the ideas and techniques of algebraic geometry, and a tenacious legacy of the inadequate foundations of "geometric" topology.

p. 191 **Note 46<sub>9</sub>** □ (June 5) Serre's idea was that it should be possible to associate any scheme  $X$  of finite type on a body  $K$ , integers

$$h^i(X) \quad (i \in \mathbb{N})$$

which he calls his "virtual Betti numbers", so that we have :

**a)** for  $Y$  a closed subschema and  $U$  the complementary open

$$h^i(X) = h^i(Y) + h^i(U)$$

b) for smooth projective  $X$ , we have

$$h(X) = i.\text{th Betti number of } X$$

(defined, for example, via  $\chi$ -adic cohomology, for  $\chi$  prime to the characteristic of  $k$ ). If we admit singularity resolution for algebraic schemes over  $k$ , then it's immediate that the  $h^i(X)$  are uniquely determined by these properties. The **existence** of such a function  $X \rightarrow (h^i(X))_{i \in \mathbb{N}}$  for a fixed  $k$ , using the formalism of cohomology with proper support, can essentially be reduced to the case of

where the base field is finite. Working in the "Grothendieck group" of finite-dimensional vectors on  $Q_\chi$  on which  $\text{Gal}(\bar{k}/k)$  operates continuously, and taking the  $\chi$ -adic Euler-Poincaré characteristic (with proper support) of  $X$  in this group,  $h^i(X)$  then designates the virtual rank of the "weight component  $i$ " of  $\text{EP}(X, Q_\chi)$ , where the notion of weight is that deduced from Weil's conjectures, plus a weak form of singularity resolution. Even without resolution, Serre's idea is realized

thanks to the strong form of Weil's conjectures (established by Deligne in "Weil's Conjectures II").

I pursued heuristic reflections along these lines, leading me to a six-operation formalism for "virtual relative schemes", with the base body  $k$  replaced by a more or less arbitrary base scheme  $S$  - and to various notions of "characteristic classes" for such virtual schemes (of finite presentation) on  $S$ . Thus, I was led (to simplify matters by returning to the case of a basic body) to consider integer numerical invariants finer than those of Serre, denoted  $h^{p,q}(X)$ , satisfying properties analogous to a), b) above, and giving back the virtual Betti numbers of Serre by the usual formula

$$h^i(X) = \sum_{p+q=i} h^{p,q}(X)$$

### 13.2.2. Refusing an inheritance - or the price of a contradiction

**Note 47** [ This note is a direct continuation of note 46 p.13.2.1]

□ It should be noted that four of the five notions I have just reviewed (the ones that pass p. 192 for "not serious" things) concern cohomology, and above all, **the cohomology of algebraic schemes and varieties**. In any case, all four were suggested to me by the needs of a cohomological theory of algebraic varieties, first for continuous coefficients, then for discrete ones. In other words, the cohomology of algebraic varieties was my main motivation and a constant leitmotif in my work over the fifteen years from 1955 to 1970.

Remarkably, this is also the theme that Deligne still considers to be his main source of inspiration, if I am to believe what is said on the subject in last year's IHES brochure<sup>23</sup> (\*). I read this with some astonishment. Admittedly, I was still "on the scene" and all that, when Deligne (after his fine work on the Ramanuyam conjecture) developed his remarkable extension of Hodge's theory. Above all, for him as for me, it was a first step towards a formal construction of the notion of pattern on the field of complexes - for a start! In the early years after my "turning point" in 1970, I of course also heard of Deligne's proof of Weil's conjectures (which also proved Ramanuyam's conjecture), and in the wake of this, of the "Lefschetz cow theorem" in positive characteristic. I expected nothing less from him! I was even sure that he would

<sup>23</sup>(\*) (May 12) On the other hand, I've just noticed that nothing in the aforementioned brochure could lead the reader to suspect that my work has anything to do with the cohomology of algebraic varieties, or that of anything else! On this subject, see the note "L'Eloge Funèbre (1) - ou les compliments" (n° 98) written today. The brochure referred to is the one mentioned in the footnote to the note "L'arrachement salutaire", n° 42, and examined a little more closely in the note "L'Eloge Funèbre" mentioned above.

to have proved at the same time the "**standard conjectures**", which I had proposed towards the end of the sixties as a first step towards founding (at least) the notion of a "semisimple" pattern over a body, and translating some of the expected properties of such patterns in terms of  $\chi$ -adic cohomology properties and algebraic cycle groups. Deligne told me afterwards that his proof of Weil's conjectures would certainly not make it possible to prove the standard (stronger) conjectures, and that he had in fact no

how to approach them. That must be about ten years ago now. Since then, I'm not aware of any other really decisive progress that has taken place in understanding the aspects

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"motivic" (or "arithmetic") cohomology of algebraic varieties. Knowing Deligne's means, I had tacitly concluded that his main interest must have turned to other subjects - hence my astonishment to read that this was not the case.

What seems to me beyond doubt is that, for the last twenty years, it has scarcely been possible to make a major breakthrough in our understanding of the cohomology of algebraic varieties, without also appearing, more or less, as a "Grothendieck follower". Zoghman Mebkhout learned this the hard way, and (to a certain extent) so did Carlos Contou-Carrère, who soon realized that it was in his interest to change his subject (47<sub>1</sub>). One of the very first things that cannot be avoided is the development of the famous "six-variance formalism" in contexts of various coefficients, as close as possible to that of patterns (which, for the moment, play the role of a kind of ideal "horizon line"): crystalline coefficients in zero characteristic (in the tradition of the Sato school and Mebkhout, Grothendieck sauce) or  $p$  (studied above all by Berthelot, Katz, Messing and a whole group of visibly motivated younger researchers), "stratified promodules" à la Deligne, (which appear as a dualized variant, or "pro", of the "ind"-notion of coherent D-module, or D-coherent crystal), and finally "Hodge-Deligne" coefficients (which seem just as good as motifs, except that their definition is transcendental and restricted to basic schemes of finite type over the field of complexes). . . At the other end of the spectrum is the task of clearing the very notion of pattern from the mists that surround it (and for good reason. . . ), and also, if at all possible, tackling such precise questions as "standard conjectures". (For the latter, I had been thinking, among other things, of developing a theory of "intermediate Jacobians" for projective and smooth varieties over a body, as a means perhaps of obtaining the trace positivity formula, which was one of the essential ingredients of the standard conjectures. )

These were tasks and questions that burned in my hands right up to the moment when I "left maths" - burning, juicy things, none of which at any time appeared to me to form a "wall", a stopping point<sup>24</sup> (\*). They were an inexhaustible source of inspiration and substance.

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sables something where all you had to do was pull where it stuck out (and it "stuck out" everywhere!) and something would come along, the expected as well as the unexpected. With my limited means, but without being divided in my work, I know just how much can be achieved if you put your mind to it, in a single day, or in a year, or in ten. And I also know, having seen him at work at a time when he was not divided in his work, what Deligne's means are, and what he can do in a day, a week, or a month, when he puts his mind to it. But no one, not even Deligne, can, in the long run, do fruitful work, work of profound renewal, while looking down on the very objects that it is basically a question of probing, as well as the language and a whole arsenal of tools that have been developed for this purpose by such and such a predecessor (and with his assistance what's more, among many others who have put their hand to the dough. . . ). ) (59).

I'm also thinking of the "Deligne-Mumford" compactification of the modular multiplicity  $M_{g,v}$  (on Spec  $Z$ ),

<sup>24</sup>(May 25) Yet this is what was kindly suggested in that famous jubilee brochure, by an anonymous pen I think I recognize. On this subject, see the note "L'Eloge Funèbre (2)". which follows "L'Eloge Funèbre (1)" quoted in the previous b. de p. note.

for connected smooth algebraic curves of genus  $g$  with  $v$  marked points. They were introduced<sup>25</sup> (\*) on the occasion of the problem of proving the connectedness of modular spaces  $M_{g,v}$  in any characteristic, by a specialization argument from characteristic zero. These objects  $M_{g,v}$  seem to me (along with the group  $Sl(2)$ ) the most beautiful, the most fascinating I've encountered in mathematics (47<sub>2</sub>). Their very existence, with such perfect properties, seems to me a kind of miracle (perfectly well understood, moreover), of incomparably greater scope than the fact of connectedness that they were intended to demonstrate. For me, they contain the quintessence of what is most essential in algebraic geometry, namely the totality (more or less) of all algebraic curves (over all conceivable basic bodies), which are precisely the ultimate building blocks of all other varieties.

algebraic. But the kind of objects we're talking about, "smooth proper multiplicities on  $\text{Spec}(\mathbb{Z})$ ", eludes still to the "accepted" categories, i.e. those we are **willing** (for reasons we don't care to examine) to "admit". The average person speaks of them by allusion at most, and with an air of apology for appearing to be still making "general nonsense", while we are certainly careful to say "stack" or "field", so as not to utter the taboo word of "topos" or "multiplicity". This is the reason without no doubt why these unique gems have not been studied or used (as far as I know) since their introduction over ten years ago, except by myself in seminar notes  $\square$ rest unpublished. At

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Instead, we continue to work either with "coarse" varieties of modules, or with finite coverings of modular multiplicities that have the appearance of being real schemes - both of which, however, are only relatively fallible and lame shadows of those perfect gems from which they originate, and which remain practically banished. ...

Deligne's four works on the Ramanuyam conjecture, on mixed Hodge structures, on the compactification of modular multiplicities (in collaboration with Mumford), and on Weil's conjectures, each constitute a renewal of our knowledge of algebraic varieties, and thus a new starting point. These fundamental works follow each other within a space of a few years (1968-73). For nearly ten years, however, these major milestones have not been the springboards for a new launch into the glimpsed and the unknown, or the means for a more far-reaching renewal. Instead, they have resulted in a morose stagnation (47<sub>3</sub>). It's certainly not that the "means" that were there ten years ago, on the part of some and others, have magically disappeared; nor that the beauty of things at our fingertips has suddenly vanished. But it's not enough for the world to be beautiful - we must also deign to rejoice in it. ...

**Note** 47<sub>1</sub> I'm thinking here of Contou-Carrère's promising start, five or six years ago, on a theory of relative local Jacobians, their links with global Jacobians (known as "generalized Jacobians") for smooth curve schemes and not necessarily proper on any scheme, and with Cartier's theory of commutative formal groups and typical curves. Apart from an encouraging reaction from Cartier, the reception of Contou-Carrère's first note, by those best placed to appreciate it, was so cool that the author refrained from ever publishing the second, which he kept in reserve, and hastened to change subject (without, however, avoiding further mishaps)<sup>26</sup> (\*).

I had suggested to him the theme of local and global jacobienes, as a first step towards a program of local and global jacobienes.

which dates back to the late 1950s, and which is notably oriented towards a theory of a dualistic "adequate" complex.

lique" in any dimension, formed with local Jacobians (for local rings of dimension arbitrary), in analogy with the residual complex of a noetherian scheme (formed with the dualizing modules

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<sup>25</sup>(\*) In Pub. Math. 36, 1969, pp. 75-110. See comments in note n° 63<sub>1</sub>

<sup>26</sup>(\*) (June 8) See the sub-note (95<sub>1</sub>) to the note "Cercueil 3 - ou les jacobienes un peu trop relatives", n° 95.

of all its local rings). This part of my cohomological duality program found itself (along with others) somewhat relegated to oblivion, during the sixties, due to the influx of other tasks which then appeared more urgent.

**Note 47<sub>2</sub>** In truth, it is the "Teichmüller tower" into which the family of all these multiplicities fits, and the discrete or profinite paradigm of this tower in terms of fundamental groupoids, that constitutes the richest, most fascinating single object I have encountered in mathematics. The group  $S\chi(2, \mathbb{Z})$ , with the "arithmetic" structure of the profinite compactivity of  $S\chi(2, \mathbb{Z})$  (consisting in the operation of the Galois group  $\text{Gal}(\mathbb{Q})$  on it), can be considered as the main building stone for the "profinite version" of this tower. On this subject, see the indications in "Esquisse d'un Programme" (pending the volume(s) of *Réflexions Mathématiques* that will be devoted to this theme).

**Note 47<sub>3</sub>** This observation of "morose stagnation" is not the considered opinion of someone who is well acquainted with the main episodes in the last ten years concerning the cohomology of algebraic schemes and varieties. This is simply the overall **impression** of an "outsider", which I got from conversations and correspondence with Illusie, Verdier and Mebkhout, among others, in 1982 and 1983. This impression could surely be qualified in many ways. For example, Deligne's work "Conjectures de Weil II", published in 1980, represents substantial new progress, if not a surprise in terms of the main result. It seems that there has also been progress in crystalline cohomology of  $\text{car. } p > 0$ , not to mention the "rush" around intersection cohomology, which has ended up making some people (unwillingly) return to the language of derived categories, even making them remember long-repudiated paternities. ...

### 13.3. III La Mode - ou la Vie des Hommes illustres

#### 13.3.1. Instinct and fashion - or the law of the strongest

**Note 48** [ This note is called by note 46 p. 265]

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□ As is well known, the theory of derived categories is due to J.L. Verdier. Before he undertook the foundation work I had proposed, I had confined myself to working with derived categories in a heuristic way, with a provisional definition of these categories (which later turned out to be the right one), and with an equally provisional intuition of their essential internal structure (an intuition that turned out to be technically wrong in the intended context, as the "cone mapping" does **not** depend functorially on the arrow in a derived category that is supposed to define it, and which defines it only to non-unique isomorphism). The theory of the duality of coherent beams (i.e. the "six variances" formalism in the coherent framework), which I had developed towards the end of the fifties<sup>27</sup> (\*), only made sense as a module in the foundations of the notion of derived category, as Verdier subsequently did.

The text of Verdier's thesis (passed only in 1967), some twenty pages long, seems to me the best introduction to the language of derived categories written to date, placing this language in the context of its essential uses (many of which are due to Verdier himself). It was only the introduction to a work in progress, which ended up being written later. I can pride myself on being, if not the only one, at least one of the very few people who can testify to having held this work in their hands,

<sup>27</sup>(\*) It still lacked an  $Rf$  operation, (cohomology with proper support) for a non proper morphism, which was introduced six or seven years later by Deligne, thanks to his introduction of the context of coherent promodules, which seems to me to be an important new idea (successfully taken up in his theory of stratified promodules).

which is supposed to establish the validity of the title of Doctor of Science awarded to its author on the basis of the introduction alone! This work is (or was - I don't know if a copy still exists somewhere... ) the only text, to date, that presents systematic foundations of homological algebra from the point of view of derived categories.

□ Perhaps I'm the only one to regret that neither the introductory text nor the foundations themselves have been

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published<sup>28</sup> (\*), so that the technical baggage essential for using the language of derived categories is scattered in three different places in the literature<sup>29</sup> (\*\*). This absence of a systematic reference text of comparable weight to the classic Cartan-Eilenberg book seems to me both a **cause** and a typical **sign** of the disaffection that struck the formalism of derived categories after my departure from the mathematical scene in 1970.

It's true that as early as 1968, it had become clear (in connection with the needs of a cohomological theory of traces, developed in SGA 5) that the notion of derived category in its primitive form, and the corresponding notion of triangulated category, were insufficient for certain needs, and that further groundwork remained to be done. A useful but still modest step in this direction was taken (mainly for the purposes of the trace cause) by Illusie, with the introduction in his thesis of "filtered derived categories". It would seem that my departure in 1970 was the signal for a sudden and definitive halt to any re-reflexion on the foundations of homological algebra, as well as on those, intimately linked, of a theory of patterns (48<sub>1</sub>). However, as far as the former were concerned, all the essential ideas for *en- vergure* foundations seemed to have been acquired in the years before my departure (48<sub>2</sub>). (Including the key idea of the "derivator", or "machine for making derived categories", which seems to be the common richer object underlying the triangulated categories we've encountered so far, an idea that would eventually be developed to some extent in a non-additive framework, almost twenty years later, in a chapter in volume 2 of *The Pursuit of Fields*). What's more, much of the groundwork to be done had already been laid by Verdier, Hartshorne, Deligne and Illusie, work that could be used as it stood for a synthesis taking up the ideas acquired in the broader perspective of derivation.

□ It's true that this disaffection in the past fifteen years<sup>30</sup> (\*) for the very notion of category

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This derivative approach, which for some is akin to a disavowal of the past, is in line with a certain fashion, which tends to look with disdain on any fundamental reflection, however urgent<sup>31</sup> (\*\*). On the other hand, it's quite clear to me that the development of staggered cohomology, which "everyone" uses today without looking twice (if only implicitly via Weil's conjectures. . . ), could not have taken place without the conceptual baggage represented by the derived categories, the six operations, and the language of sites and topos (first developed precisely for this purpose), not to mention SGA 1 and SGA 2. And it's just as clear that the stagnation we can see today in the cohomological theory of algebraic varieties could not have arisen, let alone taken root, if some of my students had known how to follow their healthy mathematical instincts during those years, rather than a fashion they were among the first to introduce, and which has long since become law with their support.

<sup>28</sup>(\*) (May 25) After these lines were written, I discovered that the first embryo of Verdier's thesis, dating from 1963 (four years before the defense), was published in 1967. See notes "Le compère" and "Thèse à crédit et assurance tous risques", n° 63"and 81.

<sup>29</sup>(\*\*) These places are: Hartshorne's well-known seminar on coherent duality, containing the only part published to date of the duality theory I had developed in the second half of the '50s; one or two Deligne papers in SGA 4; one or two chapters of Illusie's voluminous thesis.

<sup>30</sup>(\*) (May 24) these "fifteen years" should be qualified - see note no.° 47<sub>3</sub>, as well as the more detailed note "Thèse à crédit et assurance tous risques", no.° 81.

<sup>31</sup>(\*\*) (May 25) For a reflection on the forces at work in the emergence and persistence of this fashion, see the note "Le Fossoyeur - ou la Congrégation toute entière", n° 97.

**Note** 48<sub>1</sub> The same can be said (with certain reservations) of my entire program on the foundations of algebraic geometry, of which only a small part was completed: it came to a screeching halt with my departure. The stoppage hit me particularly hard in the duality program, which I considered particularly juicy. Zoghman Mebkhout's work, pursued against all odds, is nevertheless in line with this program (renewed by the contribution of unforeseen ideas). The same is true of Carlos Contou-Carrère's 1976 work (mentioned in note (47<sub>1</sub>) p. 273) - which he prudently suspended sine die. There was also work on duality in the fppf cohomology of surfaces (Milne). That's all I know about it.

It's true that I never thought of writing an outline of the long-term work program that I had set myself for me in the years between 1955 and 1970, as I did for the last twelve years, with *Esquisse d'un Programme*. The reason for this, I believe, is simply that there was never an opportunity to do so. sion (like now my application to join the CNRS) to motivate such exposure work.

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The letters to Larry Breen (from 1975), reproduced in the appendix to Chap I of the *History of Models* (Mathematical Reflections 2), give some indication of certain theories (notably duality) on my pre-1970 agenda, theories that are still waiting for arms to enter the common heritage.

**Note** 48<sub>2</sub> The same is true for the theory of motives, except that it is likely to remain conjectural for some time to come.

### 13.3.2. The service stranger and the good God theorem

**Note** 48 [ This note is called by note 46 p. ]

While it is customary to call the key theorems of a theory by the names of those who have done the work of identifying and establishing them, it would seem that Zoghman Mebkhout's name was deemed unworthy of this fundamental theorem, the culmination of four years of obstinate and solitary work (1975-79), against the fashion of the day and the disdain of his elders. The latter, on the day when the theorem's significance could no longer be ignored, took pleasure in calling it the "Riemann-Hilbert theorem", and I trust (although neither Riemann nor Hilbert would surely have asked for so much. . . ) that they had excellent reasons for doing so. After all (once the feeling of a need - that of an understanding of the precise relationships between general discrete coefficients and continuous coefficients, appeared against the general indifference, that it was refined and specified by delicate and patient work, that, after successive stages, the right statement was finally found, that it was written down in black and white and proved, and when finally this theorem, the fruit of solitude, proved itself where it was least expected - after all that) this theorem appears so obvious (not to say "trivial", for those who "would have known how to prove it" . . . ) that there's really no point in burdening our memory with the name of a vague stranger on duty!

Encouraged by this precedent, I propose to call any theorem "Adam and Eve's theorem". fundamental to a theory, or even to go back even further and give honor where honor is due, by simply calling it "**God's theorem**"<sup>32</sup> (\*).

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□ As far as I know, apart from myself, Deligne was the only one before Mebkhout to feel the interest he There was a need to understand the relationships between discrete coefficients and continuous coefficients in a framework broader than that of stratified modules, so as to be able to interpret any "constructible" coefficients in "continuous" terms. The first attempt in this direction was the subject of a seminar (still unpublished) by De-

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<sup>32</sup>(\*) In my life as a mathematician, I've never had the pleasure of inspiring, or even encouraging, a student to write a thesis containing a "God's theorem" - at least not one of comparable depth and scope.



line at the IHES in 1968 or '69, where he introduces the "stratified promodule" point of view and gives a comparison theorem (over the field of complexes) for transcendental discrete cohomology and the associated De Rham-type cohomology, which still makes sense for schemes of finite type, over any base field of zero square. (Apparently, he was still unaware at that time of the remarkable result of his distant predecessors Riemann and Hilbert... ) Even more than Verdier<sup>33</sup> (\*) or Berthelot<sup>34</sup> (\*\*), Deligne was in a particularly good position to appreciate the interest of the direction in which Mebkhout's research was heading in 1975, and subsequently the interest of Mebkhout's results, in particular the "theorem of the good God", which gives a more delicate and deeper apprehension of discrete coefficients in terms of continuous coefficients, than the one he himself had worked out. However, this did not prevent Mebkhout from continuing his work in painful moral isolation, and the credit he deserves (all the more so, I would say) for his pioneering work remains unacknowledged even today, five years after<sup>35</sup> (\*\*\*)).

### 13.3.3. Canned weight and twelve years of secret

**Note 49** [ This note is called by note 46 p. ]

□ Verification done (in Publications Mathématiques 35, 1968), I note that towards the end of the article "Théorème de Lefschetz et critères de dégénérescence de suites spectrales", three lines allude to "weighty considerations" which had led me to conjecture (in a slightly less general form) the main result of the work. I doubt that this sybilline allusion could have been useful to anyone, nor understood at the time by anyone other than Serre or myself, who were already aware of it anyway<sup>36</sup> (\*).

In this connection, I'd like to point out that a very precise "yoga of weights", including the behavior of weights for operations such as  $R\Gamma_{\pm}^i$  and  $R\Gamma_{\dagger}^i$ , was well known to me (and therefore to Deligne) as early as the late sixties, in the wake of Weil's conjectures. Some of this yoga is finally established (in the context of *l-adic* coefficient bundles, until it is in the more natural setting of motives) in Deligne's work "Conjectures de Weil II" (Publications Mathématiques 1980). Unless I'm mistaken, in the twelve years or so between the two moments<sup>37</sup> (\*\*), there has been no trace in the literature of an exposé, however succinct and partial, of the yoga of weights (still entirely conjectural), which for all that time remained the exclusive privilege of a few (two or three?) initiates<sup>38</sup> (\*\*\*)). However, this yoga constitutes an essential first key to understanding the properties of weight.

<sup>33</sup>(\*) It would seem that Verdier, as official thesis supervisor for Zoghman Mebkhout's thesis (and who in this capacity even "granted him some discussions"), was the main person involved (apart from Mebkhout himself) in the concealment that took place around the authorship of this fundamental theorem, and of the credit due to his "pupil" in the renewal of the cohomological theory of algebraic varieties by the point of view of *D*-Modules developed by Mebkhout. However, I'm not aware that he was more moved by this than Deligne.

<sup>34</sup>(\*\*) (May 25) In writing these lines, I refrained (with some hesitation) from including the name of my friend Luc Illusie in this list of my students who would have been "best placed" to give Zoghman Mebkhout the encouragement that should have been self-evident. I didn't notice a certain uneasiness within me, which might have taught me that I was giving a helping hand to someone I care about, in order to relieve him of a responsibility that falls to him and to my other "cohomology students".

<sup>35</sup>(\*\*\*) (May 25) In fact, Deligne and Verdier themselves were the first to do this. On this subject, see the note "L'Iniquité - ou le sens d'un retour", n° 75.

<sup>36</sup>(\*) (April 29) For a closer look at this article, which is instructive in more ways than one, see the note "Eviction" (n° 63).

<sup>37</sup>(\*\*) (April 19) I note on a list of Deligne's publications that I have just received and read with interest, that it mentions of "weights" as early as 1974, in a paper presented by Deligne at the Vancouver Congress - that's six years of "secrecy around weights" instead of twelve. Yet this secret seems to me to be inseparable from the similar secret surrounding the motifs (during the twelve years 1970-1982). The meaning of this secrecy has just become clearer in the course of today's reflection, in the long

double-note that follows n° 51-52).

<sup>38</sup>(\*\*\*) (May 25) It would seem, from all the information that has come to light in the course of our discussions, that these "two or more three insiders" are limited to the one and only Deligne, who seems to have taken great care to reserve for himself the exclusive benefit of the

"In other words, it was a **way of** recognizing oneself in a given situation and of making predictions with a reliability that had never been seen before,

At the same time, it represented one of the most urgent and fascinating **tasks** facing the cohomological theory of algebraic varieties. The fact that this yoga remained virtually ignored until it was finally established (in certain important aspects at least), seems to me a particularly striking example of **the information-blocking** role often played by the very people whose privileged position and functions are supposed to ensure its wide dissemination<sup>39</sup> (\*).

### 13.3.4. You can't stop progress !

**Note 50** [This note is called by section 50 of chapter **VIII The solitary adventure** of part **(I) Fatuity and Renewal** p. ]

My first experiences in this direction were the unexpected fruits of my unsuccessful efforts to get Yves Ladegaillerie's thesis on isotropy theorems on surfaces published - a work as good as any of the eleven state doctorate works ("pre-1970", it's true!) for which I had been the "boss". As I recall, these efforts continued for a good year or more, and involved many of my former friends (not to mention one of my former students, as it happens)<sup>40</sup> (\*\*). The main episodes still seem like vaudeville to me today!

It was also my first encounter with a certain new spirit and mores (now commonplace in the circle of my old friends), which I've already alluded to here and there in the course of my reflection. It was during that year (1976) that I learned for the first time, but not for the last, that it is nowadays considered unserious (at least on the part of the first-timer. . . ) to actually demonstrate delicate things that everyone uses and that I'm sure you'll agree with me.

predecessors were always content to admit (in this case, the non-existence of wild phenomena in surface topology)<sup>41</sup> (\*\*\*). Or to demonstrate a result that encompasses □ as special cases

or corollaries of several well-known deep theorems (which obviously shows that the so-called

---

possession of this yoga, which he had inherited from me, until 1974 (see previous p. b. note)) when the time was ripe to be able to present it as his own ideas, without any reference to me or Serre (see notes n° s 78<sup>1</sup>, 78<sup>2</sup> .

(April 18, 1985) Since these lines were written, I have also had occasion to become acquainted with Deligne's paper "Théorie de Hodge I" at the Congrès Int. Math, Nice (1970) (Actes, t.1, p. 425-430). Contrary to what I had reason to believe from the fragmentary information in my possession, this article sets out a substantial part of weight yoga as early as 1970. As to the origin of these ideas, it confines itself to a sibylline and perfunctory mention of an article by Serre (a foreigner, by the way).

to the question), and "Grothendieck's conjectural theory of motives". (Compare notes n° s 78<sup>1</sup>, 78<sup>2</sup> .) The question<sub>1 2</sub>

of the behavior of the notion of weight by operations such as  $Rf_!$  and  $Rf^*$  is not even mentioned, nor is it important.

will not be until the quoted article "Weil's Conjecture II" from 1980, where my name is not mentioned in connection with the theorem.

of this work, any more than is Serre's or mine in the paper "Weights in the cohomology of algebraic varieties" mentioned in the previous b. de p. note (from a year ago to the day).

<sup>39</sup>(\*) On this subject, see also sections 32 and 33, "The mathematician's ethic" and "The note - or the new ethic (1)", as well as the two related notes, "Deontological consensus and information control" and "Youth snobbery, or the defenders of purity", n° s 25,27.

<sup>40</sup>(\*\*) On this subject, see the note "Coffin 2 - or cut to length", n° 94.

<sup>41</sup>(\*\*\*) See also the episode "The note - or the new ethic" (section 33). This famous "note" had precisely the wrong idea.

to make explicit notions and statements that had hitherto been left in the dark, yet which I have implicitly used to establish results that bear my name and that everyone has been shamelessly using for almost twenty-five years (something, incidentally, that the two illustrious colleagues knew perfectly well). (June 8) For further details, see note "Coffin 4

- or topos without flowers or wreaths" (n° 96). The "results that bear my name" are results on the generation and finished presentation of certain fundamental global and local profi nis, "demonstrated" in SGA 1, among others.

by descent techniques that remain heuristic in the absence of careful theoretical justification, accomplished in Olivier Leroy's (apparently "unpublishable") work on Van Kampen-type theorems for fundamental topos groups.

new can only be a special case or an easy consequence of known results). Or to take the trouble only, when stating a result or describing a situation in terms of another, to carefully formulate the natural hypotheses (a sign of regrettable bombast), rather than limiting oneself to some case in point to the liking of the high-flying person issuing the opinion. (Just last year, I saw Contou-Carrère reproached for not having confined himself in his thesis to a basic body instead of a general scheme - while conceding the mitigating circumstance that it was surely at the insistence of his boss of circumstance that he had had to do so. The person who expressed himself in this way was, however, sufficiently familiar with the subject to know that, even if we limit ourselves to the body of complexes, the necessities of demonstration force us to introduce general basic diagrams. ...). )

The excesses of a certain fashion today go so far as to disgrace not only careful demonstrations (or even demonstrations at all), but often even formal statements and definitions. Given the price of paper and the longevity of the gorged reader, it will soon be out of the question to bother with such costly luxuries! Extrapolating from current trends, we can predict the moment when a publication will no longer need to spell out definitions or statements, but will simply name them with code words, leaving it to the indefatigable and genial reader to fill in the blanks according to his or her own insights. The referee's task will be made all the easier, as all he'll have to do is look in the "Who is Who" directory to see if the author is known to be credible (in any case, no one could contradict the blanks and dotted lines that make up the brilliant article), or if he's an unavowable unknown who will be (as is already the case today and has been for a long time) automatically ejected. ...



# 10. B) STONE AND MOTIFS

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## 14.1. IV Motives (burial of a birth)

### 14.1.1. Souvenir d'un rêve - or the birth of motifs. . .

**Note 51** [This note is called note 46 p. ]

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□(April 19) Since these lines (which end the note "My orphans", n° 46) were written, it is less than a month, I've noticed that they're a bit behind the times! I've just received "Hodge Cycles, Motives and Shimura Varieties" (LN 900), by Pierre Deligne, James S. Milne, Arthur Ogus and Kuang-Yen Shih, which

Deligne was kind enough to send me, along with a list of his publications. This collection of six texts, published in 1982, represents an interesting development since 1970, with the mention of motives in the title and a presence of this notion in the text, albeit still modest, especially via the notion of "motivic Galois group". Of course, we're still a long way from the overall picture of a theory of motives, which for the past fifteen or twenty years has been awaiting the bold mathematician who will be willing to "paint it, vast enough...".





to serve as inspiration, Ariadne's thread and horizon line for one or more generations of arithmetician geometers, who will have the privilege of establishing their validity (or at any rate of discovering the final word on the reality of the motifs. . . ) (53).

It's also since 1982<sup>1</sup> (\*), it would seem, that the tide of fashion has begun to turn, more or less, towards derived categories; Zoghman Mebkhout (in a perhaps somewhat euphoric flight of fancy) already sees them on the verge of "invading all areas of mathematics". If their usefulness, which simple mathematical instinct (for a well-informed person) made quite obvious in the early sixties, is only just beginning to be recognized now, it is (it seems to me) mainly thanks to the solitary efforts of Mebkhout, who for seven years stuck to the thankless task of wiping the slate clean, with the courage of one who trusts his instinct alone, against a tyrannical fashion... ..

□ It's remarkable to read this first publication, which dedicates (twelve years after my departure from the mathematical scene) a modest re-entry of the notion of motif into the areopagus of accepted mathematical notions, nothing could lead the uninformed reader to suspect that my modest person was in any way associated with the birth of this long-taboo notion, and with the unfolding of a rich and precise "yoga", which (in a very fragmentary form) appears there as if it had emerged from nothing, without allusion to any paternity (51 ).<sub>1</sub>

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When, just three weeks ago, I wrote in a page or two about the yoga of patterns - even one of my "orphans", and one that was closer to my heart than any other - I must have been way off the mark! No doubt I was dreaming, as I seemed to recall years of gestation of a vision, tenuous and elusive at first, and growing richer and more precise as the months and years went by, in an obstinate effort to try and grasp the common "motif", the common quintessence, of which the many cohomological theories then known (54) were so many different incarnations, each speaking to us in its own language about the nature of the "motif" of which it was one of the directly tangible manifestations. No doubt I'm still dreaming, remembering the strong impression made on me by such an intuition of Serre's, who had been led to see a Galois profinite group, an object that seemed to be essentially discrete in nature (or, at least, reducible to the Galois profinite group).

tautologically to simple systems of **finite** groups), as giving rise to an immense projective system of **analytic**  $l$ -adic groups, or even **algebraic** groups on  $\mathbb{Q}_l$  (by moving to suitable algebraic envelopes), which even had a tendency to be reductive - with the introduction of any

the arsenal of intuitions and methods (à la Lie) of analytic and algebraic groups. This construction made sense for any prime number  $l$ , and I felt (or I dream I felt. . . ) that there was a mystery to be probed, about the relationship of these algebraic groups for different primes; that they all had to originate from the same projective system of algebraic groups on the only natural common subbody of all its bodies

the  $\mathbb{Q}$  body, the "absolute" body of zero characteristic. And since I like to dream, I continue to dream that I remember entering this glimpsed mystery, through a work that was surely only a dream since I wasn't "demonstrating" anything; that I ended up understanding how the notion of motif provided the key to understanding this mystery - how, by the very fact of the presence of a category (here that of "smooth" patterns on a given basic scheme □ for example, patterns on a given basic body),

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having internal structures similar to those found on the category of linear representations of an algebraic pro-group over a  $k$ -field (the charm of the notion of algebraic pro-group having been revealed to me previously by Serre as well), we can indeed reconstitute such a pro-group (as soon as we have a suitable "fiber functor"), and interpret the "abstract" category as the category of its

<sup>1</sup>(\*) (May 25) I delay again, this time by a year - the turning point comes in June 1981 with the Colloque de Luminy, see the note "L'Iniquité - ou le sens d'un retour", n° 75.

linear representations.

This approach to a "motivic Galois theory" was inspired by the approach I had found, years before, to describe the fundamental group of a topological space or scheme (or even of any topos - but now I feel I'm going to offend delicate ears that "topos don't amuse". . . ), in terms of the category of spreadable coverings on the "space" under consideration, and the fiber functors on it. And the very language of "**motivic Galois groups**" (which I might just as well have called motivic "fundamental groups", the two kinds of intuition being for me the same thing, since the late fifties... . ), and that of "fiber functors" (which correspond exactly to the "manifest embodiments" referred to above, i.e. to the various "cohomological theories" that apply to a given category of patterns) - this language was designed to express the profound nature of these groups, and to suggest their immediate links with Galois groups and with ordinary fundamental groups.

I still remember the pleasure and wonder, in this game with fiber functors, and with the torsors under Galois groups that make it possible to pass from one to the other by "twisting", of finding in a particularly concrete and fascinating situation the whole arsenal of notions of noncommutative cohomology developed in Giraud's book, with the sheaf of fiber functors (here above the étale topos, or

better still,  $Q$ 's fpqc topos - non-trivial and interesting topos if ever there was one!), with the "link" (in groups or algebraic pro-groups) that link this sheaf, and the avatars of this link, realized by various algebraic groups or pro-groups, corresponding to the various "sections" of the sheaf, i.e. the various cohomological functors. The various complex points (for example) of a scheme of zero characteristic gave rise (via the corresponding Hodge functors) to as many sections of the sheaf, and to torsors of passage from one to the other, these torsors and the pro-groups operating on them being provided with struc-  
 remarkable algebraic-geometric patterns, expressing the specific structures of Hodge cohomology

- but here  $\square$  m anticipating another part of the motives dream. ... Those were the days when those who today make the

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mode hadn't yet declared that topos, sheaves and the like didn't amuse them and that it was therefore bullshit to talk about them (I wouldn't have minded recognizing topos and sheaves where they were. . . ). And now, twelve years on, the same people are pretending to discover and teach that sheaves (or even topos) do indeed have something to do with the cohomology of algebraic varieties, or even with the periods of abelian integrals... .

I could evoke here the dream of another memory (or the memory of another dream. . . ) around the dream of motifs, also born of a "strong impression" (I'm definitely in full subjectivity!) made on me by some of Serre's comments on a certain "philosophy" behind Weil's conjectures. Their translation into cohomological terms, for  $l$ -adic coefficients with variable  $l$ , made me suspect remarkable structures on the corresponding cohomologies - the "filtration by weights" structure<sup>2</sup> (\*). Surely the "motif" common to the various  $l$ -adic cohomologies was to be the ultimate support of this essential arithmetic structure, which then took on a geometric aspect, that of a remarkable structure on the geometric object "motif". To speak of "work" (when, of course, it was still a matter of guessing games, no more and no less) when it was a matter of "guessing" (with only the inner coherence of a vision being formed as a guide, using scattered elements known or conjectured here and there... . ), on the specific structure of the various cohomological "avatars" of a motif, how the filtration of weights was translated<sup>3</sup> (\*\*), starting with Hodge's avatar (at a time when the

<sup>2</sup>(\*) (January 24 1985) For a rectification of this distorted memory, see note no. ° 164 (I4), and sub-note no. ° 164<sub>1</sub>, giving details of the "yoga of the weights" filtration.

<sup>3</sup>(\*\*) (February 28, 1985) There's a slight confusion in my mind here. It is, in fact, the filtration closely linked by the

Hodge-Deligne theory had not yet seen the light of day, and for good reason...<sup>4</sup>(\*\*\*)). This allowed me (in my dream) to see Tate's conjecture on algebraic cycles (yet another "strong impression" that inspired the Dreamer in his dream of motives!) and Hodge's (55) competing in the same vast picture, and to come up with two or three conjectures of the same kind, which I've mentioned to some people who must have forgotten them, because I've never used them.

I never heard of them again, nor of  $\square$  "standard conjectures". Anyway, they were just p. 209

conjectures (and unpublished ones at that. . . ). One of these did not concern a particular cohomological theory, but gave a direct interpretation of the filtration of weights on the motivic cohomology of a nonsingular projective variety over a body, in terms of the geometric filtration of this variety itself by closed subsets of given codimension (the codimension playing the role of the "weight")<sup>5</sup> (\*).

And then there was the work (I should put quotation marks on "work", but I can't get used to it!) of "guessing" the behavior of the weights through the six operations (since lost. . . ). Here again, I never had the impression of inventing, but always of discovering - or rather of listening to what things were saying to me, when I took the trouble to listen to them with pen in hand. What they said had a peremptory precision that could not be mistaken.

Then there was a third "pattern-dream", which was like the marriage of the two previous dreams - when it came to interpreting, in terms of structures on motivic Galois groups and on the torsors under these groups that serve to "twist" a fiber functor to obtain (canonically) any other fiber functor<sup>6</sup> (\*\*), the various additional structures with which the category of patterns is equipped, one of the very first of which is precisely that of filtration by weights. I seem to remember that there was never any question of riddles, but rather of mathematical translations in due form. These were all brand-new "exercises" on linear representations of algebraic groups, which I enjoyed doing for days and weeks on end, feeling that I was getting closer and closer to a mystery that had fascinated me for years! Perhaps the most subtle notion I had to grasp and formulate in terms of representations was that of the "polarization" of a motif, taking my inspiration from Hodge's theory and trying to decant what still made sense in the motivic context. This was a reflection

which must have been around the time of my reflection on a formulation of the "standard conjectures", inspired one

$\square$  and the other by Serre's idea (still him!) of a "Kählerian" analogue of Weil's conjectures. In such a p. 210

situation, when things themselves tell us what their hidden nature is, and by what means we can most delicately and faithfully express it, and yet many essential facts seem beyond the immediate reach of demonstration, simple instinct tells us to simply write down in black and white what things insistently tell us, and all the more clearly as we take the trouble to write under their dictation! There's no need to worry about demonstrations or complete constructions - to bother with such requirements at this stage of the work would be tantamount to denying ourselves access to the most delicate, most essential stage of a vast work of discovery - that of the birth of a vision, taking shape and substance out of an apparent nothingness. The simple act of **writing, naming, describing**

- if only to describe elusive intuitions or simple "suspicions" reluctant to take shape

- has a **creative power**. This is the instrument of the passion to know, when it is invested in things that the intellect can apprehend. In the process of discovering these things, this work is the creative stage above all others, which always precedes the demonstration and gives us the means to do so - or, to put it another way, to discover them.

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"levels".

<sup>4</sup>(\*\*\*) This was at a time when the young Deligne had probably never heard the word "schema" in a mathematical context, nor the word "cohomology". (He became acquainted with these notions through my contact, from 1965 onwards).

<sup>5</sup>(\*) (February 28, 1985) This is in fact filtration by "levels" (see previous footnote).

<sup>6</sup>(\*\*) Just as the fundamental groups  $\pi_1(s)$ ,  $\pi_1(y)$  of some "space"  $X$  at two "points"  $x$  and  $y$  reduce from each other by "twisting" by the torsor  $\pi_1(x, y)$  classes of paths from  $x$  to  $y$ . . .

to put it better, without which the question of "demonstrating" something doesn't even arise, before anything that touches on the essential has been formulated and seen. By the sheer virtue of an effort to formulate, what was shapeless takes shape, lends itself to examination, decanting that which is visibly false from that which is possible, and that above all which accords so perfectly with the totality of things known, or guessed at, that it in turn becomes a tangible and reliable element of the vision in the making. This vision becomes richer and more precise as the work of formulation progresses. Only ten suspected things, none of which (let's say Hodge's conjecture) leads to conviction, but which mutually clarify and complete each other and seem to contribute to the same still mysterious harmony, acquire in this harmony the force of vision. Even though all ten would eventually prove false, the work that has led to this provisional vision has not been done in vain, and the harmony it has given us a glimpse of and enabled us to penetrate is not an illusion, but a reality, calling us to know it. Through this work alone, we have been able to enter in intimate contact with this reality, this hidden and perfect harmony. When we know that things have reason to be what they are'  $\square$  <sup>that</sup> our vocation is to know them, not to dominate them, then the day when an error erupts is a day of exultation (56) - just as much as the day when a demonstration teaches us beyond any doubt that something we imagined was indeed the faithful and true expression of reality itself.

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In either case, such a discovery comes as a reward for work, and could not have happened without it. But whereas it would only come at the end of years of effort, or even if we never learned the final word, reserved for others after us, work is its own reward, rich in every moment of what this very moment reveals to us.

**Note** 51<sub>1</sub> (June 5) Zoghman Mebkhout has just drawn my attention, however, to a mention of "Grothendieck's motives" made on page 261 of the volume quoted, in an article by Deligne that "takes up and completes a letter to Langlands". It reads: "I will not be referring to Grothendieck's motives, as he defined them in terms of algebraic cycles, but to **absolute Hodge motives**, similarly defined in terms of absolute Hodge cycles". Grothendieck's motives" (not underlined) are named here, not as a source of inspiration, but to distance ourselves from them and insist that we're talking about **something else** (which we take care to underline). This distancing is all the more remarkable, given that the validity of Hodge's conjecture (a conjecture known to Deligne, I suppose, as it is to every reader of his article-letter, starting with his original addressee Langlands) would imply that the two notions are **identical** ! !

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Of course, as early as 1964, when I developed the notion of motivic Galois group, it was well known to me that a notion of "Hodge pattern" could be developed on the same model, with a corresponding notion of "motivic Galois-Hodge group", which was introduced independently by Tate (I can't say whether it was before or after) and then given the name of Hodge-Tate group (associated with a Hodge structure). The crude swindle (which doesn't seem to bother anyone, coming from such a prestigious figure) consists in purely and simply swallowing the paternity of a new and profound notion, that of motif, and of the whole rich fabric of intuitions I had developed around this notion, under the derisory pretext that the technical approach taken towards this notion (via absolute Hodge cycles, instead of of algebraic cycles) is (perhaps, if Hodge's conjecture is wrong) different from the one I had (very tentatively) adopted. This yoga, which I had been developing  $\square$  over a period of nearly ten years, has been the main source of inspiration in Deligne's work since its inception in 1968. Its fruitfulness and power as a tool for discovery were clear long before I left in 1970, and its identity is independent of any technical approach taken to establish the validity of this or that limited part of Deligne's work.

this yoga. Deligne had the merit of identifying two such approaches, independently of any conjecture. On the other hand, he was not honest enough to name his source of inspiration, endeavoring as early as 1968 to hide it from public view so as to reserve the exclusive benefit for himself, until he (tacitly) claimed credit for it in 1982.

### 14.1.2. Burial - or the New Father

**Note** 52 Returning to the dream of the motifs, I seem to remember that I dreamt it aloud. Admittedly, dreaming is by its very nature solitary work - but the twists and turns of this tenacious work, which went on for years on the sidelines of a vast task of writing foundations that absorbed most of my time - these twists and turns had a day-to-day witness, much closer than Serre, who confined himself to following things from afar. ... <sup>7</sup>(\*). I could have added that I even told him what I didn't "know" in the common sense of the term - those mathematical "dreams" (on the theme of patterns as on others) which always found in him an attentive ear and an alert mind, like mine, eager to understand.

It's true that when I wrote that Pierre Deligne may have been "a bit of a pupil", this is still a subjective impression (57), not corroborated (as far as I know) by any written or at least printed record, which could lead anyone to suspect that Deligne might have learned something from me - whereas it's a pleasure for me to remember that I never spoke mathematics with him without learning something. (And even when I stopped talking mathematics with him, I continued to learn more difficult and perhaps more important things from him, including on the very day I'm writing this. these lines... ).

□ Having been informed lately by a third party, who had guessed (one wonders how!) that I might be interested in the matter, of the existence of a text by Deligne and others in which motives or at least "Tanakian categories" were discussed, and having mentioned this to Deligne, he expressed his sincere surprise that I should be interested in such things. Looking through the copy he was kind enough to send me, however, I can see that his surprise was perfectly justified. Clearly, I'm a complete stranger to the subject at hand. At most, the introduction hints in passing that certain "standard conjectures" (which I had made at the time, one wonders why) would have a consequence for the structure of the category of patterns over a body. ... The reader curious to know more would be at a loss, for throughout this book he will find no details or references to these conjectures, which are no longer mentioned; nor any mention of the one and only published text in which I explain the construction of a category of patterns over a body in terms of the standard conjectures; nor of the only other published text from before 1970 in which patterns are discussed, by Demazure (in a Bourbaki Seminar, if I remember correctly), which followed my principle of ad hoc construction, in a slightly different perspective. ... <sup>8</sup>(\*).

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<sup>7</sup>(\*) (May 25) The beginnings of my thinking on motives, however, predate Deligne's appearance. My manuscript notes on Galois motivic theory date from 1964.

<sup>8</sup>(\*) After verification, I note that apart from a few pages on standard conjectures (Algebraic Geometry, Bombay, 1968, Oxford Univ. Press (1969) pp. 193-199), there is no published mathematical text by me in which motives are mentioned. In Demazure's talk (Séminaire Bourbaki n° 365, 1969/70), following Manin's talk in Russian, mention is made of talks on I had done at the IHES in 1967, and which (I suppose) constituted a first overall sketch of a vision of the motives. An account of the standard conjectures and their relation to the Weil conjectures, more detailed than the announcement at the Bombay congress, is given by Kleiman (Algebraic Cycles and the Weil conjectures, in Dix exposés sur la cohomologie des schémas, Masson-North Holland, 1968, pp. 359-386). I was not aware of any reflection on the standard conjectures, in particular towards a demonstration of them, outside my own before 1970. The deliberate intention to ignore these key conjectures (which I

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□ Even Neantro Saavedra, who had the good fortune to be one of my "pre-1970 students", was duly cited. He had done a thesis with me on what I called, I believe, "rigid tensorial categories", and which he called "Tannakian categories". One still wonders by what miraculous chance Saavedra had been able to foresee the needs of Deligne's theory of patterns, which was to blossom ten years later! In fact, in his thesis he does exactly **the** work that technically constitutes the key to a motivic Galois theory, just as J.L. Verdier's thesis was in principle **the** work that technically constitutes the key to a formalism of the six operations in cohomology. One difference (among others) in Saavedra's honor is that he took the trouble to publish his work; admittedly, he hadn't had the pen of Hartshorne, Deligne and Illusie combined to dispense with such a formality. Yet, ten years later, Saavedra's thesis is reproduced ab ovo and practically in toto in the remarkable collection, this time by Deligne and Milne. This may not have been essential, if it was merely a matter of correcting two particular points in Saavedra's work (58). But there is a reason for everything, and I think I can see why Deligne himself took the trouble<sup>9</sup> (\*), even though it runs counter to his own extremely exacting standards for publication, which he is known to apply with exemplary rigor when it comes to others...<sup>10</sup>(\*\*).

As for the authorship of the notions and motivic yoga themselves, for an uninformed reader (and informed readers are becoming rare and will eventually die their own death. . .) this authorship cannot be in the slightest doubt - without any need here to go bothering distant Hilbert and Riemann, let alone the good Lord. ) there can be no doubt whatsoever as to their authorship - and there's no need to go bothering distant Hilbert and Riemann, let alone the good Lord. If the prestigious author, whose beautiful result on absolute Hodge cycles

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on abelian varieties appears as the starting point, and birth to say the least, of the theory of motives, does not breathe a word of its paternity, it is there a modesty □ which honors him and in perfect agreement with the customs

and the ethics of the profession, which dictate that we leave it to others (if need be) to give honor where honor is obviously due: to the legitimate Father... .

**Note 53** Touched by the vicissitudes of this orphan, and doubting that another will do the work whose need and scope I am apparently the only one, even today, to feel, I presume that the "bold mathematician" in question will be none other than myself, once I have completed the Poursuite des Champs (which I anticipate will occupy me for another year or so).

**Note 54** Since then, two new cohomological theories have appeared for algebraic varieties (apart from the Hodge-Deligne theory, a natural extension, in the "motivic" spirit, of the Hodge cohomology): Deligne's theory of "stratified promodules" and, above all, that of crystals, a "*D-Modules*" version à la Sato-Mebkhout, with the new light provided by the theorem of the good God (aka Mebkhout) mentioned earlier. This approach to constructible discrete coefficients is likely to replace Deligne's earlier version, as it is probably better suited to expressing relations with De Rham's cohomology. Moreover, these new theories do not provide new functor-fibers on the category of smooth patterns on a given scheme, but rather (modulo a more thorough groundwork than has been done so far) a way of precisely apprehending the "Hodge" embodiment of a (not necessarily smooth) pattern on a finite-type scheme over the body

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said, in my Bombay sketch, that I considered them, along with the resolution of singularities of excellent schemes, to be the most important open problem in algebraic geometry), seems to me to have a lot to do with the impression of stagnation that the cohomological theory of algebraic varieties gives me, from the echoes that have come back to me.

<sup>9</sup>(\*) On this subject, see the reflections in the note "La table rase", n° 67.

<sup>10</sup>(\*\*) (June 8) And even more so, when it comes to works that bear the mark of my influence - on this subject, see the episode entitled "The note - I'm not the only one! or the new ethics". Section 33.

of complexes, or the "De Rham" incarnation on a finite-type scheme over a body of zero characteristic. It is likely, moreover, that the (apparently still unwritten) theory of Hodge-Deligne coefficients on a finite-type scheme on  $\mathbb{C}$ , will eventually appear as contained in the (equally unwritten) theory of crystalline coefficients à la Sato-Mebkhout (with an additional filtration datum), or more precisely as a kind of intersection of the latter with the theory of constructible discrete coefficients

Q-vectorial ... . As for elucidating the relationship between Mebkhout's crystalline theory and that of the developed in positive characteristic by Berthelot and others, this is a task Mebkhout has been feeling since before 1978, in a climate of general indifference, and which seems to me to be one of the most fascinating that immediately arises for our understanding of "the" cohomology (unique and indivisible, motivic knowledge!) of algebraic varieties.

**Note 55** <sup>□</sup> I may have been dreaming, but my dream about the relationship between Hodge's patterns and structures made me put my finger, without even doing so on purpose, on an inconsistency in the "generalized" Hodge conjecture as originally formulated by Hodge, and to replace it with a rectified version which for the (I'd wager) must be no more or less false than the "usual" Hodge conjecture on algebraic cycles.

### 14.1.3. Prelude to a massacre

**Note 56** I'm thinking in particular, in the context of the cohomology of algebraic varieties, of Griffiths' discovery of the falsity of a seductive idea we'd long had about algebraic cycles, namely that a cycle homologically equivalent to zero had a multiple that was algebraically equivalent to zero. This discovery of a brand-new phenomenon struck me enough for me to spend a week trying to grasp Griffiths' example, transposing his construction into a new one.

(which was transcendental, on the  $\mathbb{C}$  body) into a construction "as general as possible", and valid in particular on bodies of any characteristic. The extension wasn't entirely obvious, with (if I remember correctly) Leray spectral sequences and Lefschetz's theorem.

(June 16) This reflection had been the occasion for me to develop, in the étale context, the cohomological theory of "Lefschetz brushes". My notes on this subject are developed in the SGA 7 II seminar (by P. Deligne and N. Katz) in lectures XVII, XVIII, XX by N. Katz (who takes care to refer to these notes, which he followed closely). In the introduction to the volume by P. Deligne, on the other hand, where it is stated that the key results of the volume are talks XV (Picard-Lefschetz formulas in étale cohomology) and XVIII (Lefschetz brush theory), the author is careful not to point out that I had anything to do with this "key theory" of Lefschetz brushes. Reading the introduction gives the impression that I have nothing to do with the themes developed in the volume.

The long SGA 7 seminar, which in 1967-69 followed on from the SGA 1 to SGA 6 seminars developed at my instigation between 1960 and 1967, was conducted jointly by Deligne and myself, who had kicked things off with a systematic theory of evanescent cycle groups. The presentations by volunteers, the two seminar volumes (SGA7 I and SGA 7 II) have not yet been published.

were not published until 1973, in the care of Deligne. While it had been understood at the time of the seminar that

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After I left, Deligne informed me of his desire (which seemed strange to me) for the seminar to be **split in two**, with Part I presented as directed by me, and Part II by him and Katz. I now perceive in it an "operation" that prefigures the "SGA 4 operation<sup>1</sup>" aimed (among other things) at bringing to light the whole series of foundations SGA 1 to SGA 7, which in his mind

and its conception was inseparable from my person, as was the EGA series of *Eléments de Géométrie Algébrique*, as a collection of all-purpose texts in which my person would play only an episodic, even superfluous role. This tendency is very clear, even brutal, in the SGA 4 volume<sup>1</sup> and above all in the massacre of the SGA 5 seminar, to which this volume is indissolubly linked. On this subject, see, among others, the notes "La table rase" and "Le massacre", n°s 67 and 87, and especially "La dépouille. . ." (n° 88).

(June 17) The overall conception of the SGA 7 seminar (in which I made no distinction between parts "I" and "II", and still don't) was due to me, and Deligne had made important contributions (reported in my report on Deligne's work, written in 1969, see n°s 13, 14 of this report), the most crucial for the purposes of the seminar being the Picard-Lefschetz formula, proved by a specialization argument from the transcendental case already known. The split of the seminar into two parts was unjustified both mathematically and in terms of the respective contributions - there are substantial contributions from both Deligne and myself in each of the two "pieces" of SGA 7.

Of course, I would have been delighted if Deligne had continued the series of APG foundations I had inaugurated.

- which was nowhere near the end of its run! This "SGA 7 operation" is by no means a continuation, but I feel it is a sort of brutal "saw" (or chainsaw. . .), **bringing** the SGA series to an **end** with a volume that ostentatiously distances itself from my person, even though it is linked to my work and bears its mark just as much as the others. While my person is concealed as far as possible, the tone towards my work is not yet that of the barely disguised contempt of the "SGA 4 operation<sup>1</sup>", which represents an even more brutal saw cut in the unity of the SGA 4 and 5 seminar, and the means and pretext for the ruthless ransacking of the unpublished SGA 5 part of it, the torn-off pieces of which are shared equally between Deligne and Verdier.

...

p. 218 **Note 57** □ I hasten to add that the same remark applies to the other mathematician of great means of whom I ventured to say (in note no.° 19) that he was "a bit of a pupil", ten years after Deligne.

**Note 58** This reminds me that Notes readings (which had published six or seven "pre-1970" doctorates done with me) never wanted to publish Yves Ladegaillier's "post-1970" one (reason: they don't publish theses!). On the other hand, they did publish Saavedra's thesis a second time. ... Incidentally, I had told Deligne about Ladegaillier's beautiful isotopy result, which was being rejected everywhere (with the further secret hope that he would lend his help to publish it) - but didn't seem to interest him (reason: his incompetence in surface topology. . .).

Curtain. . .

#### 14.1.4. The new ethics (2) - or the fair d'empoigne

**Note 59** (April 20) In the few weeks since these lines were written, which note a contradiction and its price, I was surprised to discover that the person concerned had already, two years ago, found a very simple way of "resolving" the said contradiction - all it took was a little thought! We could call it "the early burial method" (which the reader can read about in the double note (50) (51), written yesterday in the fresh emotion of the discovery). I'm sorry to say that the unexpected reappearance of the anticipated deceased on the famous "mathematical stage" (which sometimes looks rather like a jostling match. . .) risks introducing technical complications for the smooth application of this brilliant method! In a previous note ("Deontological consensus - and control of information", n° 6) I felt (a little)



still confused) that the most universally accepted rule of ethics in the scientific profession "remained a dead letter" in the absence of respect, by those in control of scientific information, for every scientist's right to make his or her ideas and results known. At about this time, I also took the trouble to describe in some detail a case in point where, for me, the disregard for this right was flagrant, and where I also felt that this disregard was bordering on disregard for the primary rule, on which there is general consensus. (See "The note - or the new ethic", section 30).

□ It's not the only time I've felt this very particular uneasiness, when I saw the **spirit** of this The first rule was scorned, while the person who made it was "thumbed" both by his position (above suspicion!) and his means, and by the casualness of his form. I try to pinpoint this malaise in the note ("the snobbery of youth - or the defenders of purity") that relates to the quoted section. When one allows oneself to despise the "obvious" things I'm talking about there, and in the same spirit also (might I add now) the (perhaps profound) things that are neither demonstrated nor patented as published "conjectures" known to all, one might as well (given the little I) consider them common property (trivial, it goes without saying)<sup>11</sup> (\*), and therefore also, when the time comes, as "one's own" with the greatest casualness and the best conscience in the world - it being understood, of course, that one wouldn't dream of appropriating a ten-page or one-hundred-page (or just ten-line) muscular demonstration that establishes a result "that one hasn't been able to demonstrate" (59 ). I didn't think I had such a good feeling or such a good word to say (on the subject of "dead letter"), since I've just seen the undecided "limit" of the case cited above blithely crossed - and surely crossed with the best conscience in the world, **given how little: a dream**, and what's more, not even demonstrated (nor, above all, **published** . . . ).<sup>12</sup> (\*\*)

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Fortunately, I've got a bit of a backbone - when I need to, I can express what I feel as best I can, and that's not all. that I want to say, I've acquired (rightly or wrongly) credibility, and thus a chance to be listened to when I have something to say, or to publish it if I feel the need. On the other hand, I realize more vividly what "feeling of injustice and powerlessness" of one who □ is wronged without recourse, when he feels his hands and feet tied p.

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to the arbitrariness of "those who have everything in their hands" - and use it as they see fit.

It's true that in my life as a mathematician, I've sometimes behaved badly with an equally good conscience, and I've had the opportunity in my reflection to talk about cases that this has brought to the surface from the mists of oblivion and ambiguity never examined. By probing them, I finally understood that I shouldn't be surprised if today (and for a long time now) the pupil has blithely surpassed the master, nor should I disown anyone to whom I have sympathy or affection. But it's healthy, for me as for everyone else, to call a spade a spade, whether that spade belongs to my house or to someone else's.

### 14.1.5. Appropriation and contempt

**Note!** 59' (June 8) I'm no longer convinced, as far as my friend Pierre Deligne is concerned, having had the opportunity to note that he has finally slipped into the game of "tacit paternity" with regard to the l-adic cohomology tool, i.e. what I call "mastery" of spread cohomology. There has been a remarkable evolution between the "SGA 4 operation<sup>1</sup>" (where my name is still pronounced, but with an affectation of disinterested disdain) and the "SGA 4 operation" (where my name is still pronounced, but with an affectation of disinterested disdain).

<sup>11</sup>(\*) Such was the fate of "Le théorème du bon Dieu" (alias Mebkhout).

(June 8) And, as in pattern yoga, we take care to cleverly create the appearance of authorship, without ever saying so outright! See on this subject (in the case in point) the note "Le Prestidigitateur" n° 75", and for the brilliant general method or style, the note "Pouce!" n° 77, as well as the note that follows "Appropriation and contempt", n° 59'.

<sup>12</sup>(\*\*) It would be wrong to be embarrassed, as the event seems to show that the general consensus these days is that the something quite normal - at least from someone of such high standing! What we call "good conscience" is no more, no less,

2. A walk through a work or the Child and Mother  
than a feeling of agreement with the prevailing consensus in the milieu to which one belongs.

(See the notes "La table rase" and "L'être à part" for the initial phase, and the notes "L' Eloge Funèbre (1), (2)" for the final phase).

Intermediary phases in this escalation include the "memorable article" on so-called "perverse beams" in 1981 (see the notes "L'Iniquité - ou le sens d'un retour" and "Pouce!", n° 75 and 77), and the exhumation of the motifs in LN 900 the following year (the Eloge Funèbre taking place the following year, in 1983). In all these cases and others of lesser scope that I've observed, the inner attitude and "method" that enables Deligne to appropriate the credit of others' ideas with a clear conscience is that of **contempt** (which remains partially tacit, so "little" in fact that it's not even worth talking about, when you're going to use it to do really powerful things - Weil's conjectures, the theory of so-called "per- vers" beams. . . Once the operation has been completed and appropriation accepted by all, it's always

time to put things right and strut modestly with what has been appropriated. The same contribution is the object of casual scorn, as long as it still seems tainted with the name of one of <sup>those</sup> ☐ it is intended to bury, and

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has been taken up when it has been appropriated by himself (*l-adic* cohomology, motifs, while waiting for Mebkhout's yoga) or by some good buddy (yoga of derived categories, yoga of duality, appropriated by Verdier with Deligne's active encouragement).

## 14.2. V My friend Pierre

### 14.2.1. The child

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☐ **Note** 60 (April 21) To take up this dream of a memory, which is not just the memory of birth a vision... I remember well (even though I've forgotten so much!) the pleasure I took in talking with him, who soon became a confidant of everything that intrigued me, or that enlightened and delighted me day by day in my love affair with mathematics, than he had ever been a "pupil". His ever-awakening interest, the ease with which he took in everything ("as if he'd always known. . . ") were a constant source of enchantment for me. He was a perfect listener, driven by the same thirst for understanding that animated him as me - a highly awakened listener, a sign of communion. His comments always met my own intuitions or reservations, when they didn't throw some unexpected light on the reality I was trying to pin down through the mists that still surrounded it. As I've said elsewhere, he often had the answers to the questions I raised, often on the spot, or would elaborate on them in the days or weeks that followed. In other words, the listening was shared, when he in turn explained to me the answers he had found, i.e. quite simply the reason for things, which always appeared with that perfect naturalness, with that same ease that had often enchanted me with some of my elders like Schwartz and Serre (and also, with Cartier). It was this same simplicity, this same "obviousness" that I had always pursued in my understanding of mathematical things. Without having to say it, it was clear that with this approach and these high standards, he and I were "from the same family".

I sensed from the moment we met that his "means", as they say, were of a very rare quality, far beyond the modest means at my disposal, even though we were on the same wavelength in terms of our passion for understanding and our demand for understanding mathematical things. I also had the vague feeling, although I couldn't put it into words at the time, that this "strength" I saw in him (and which I also sensed in him) was the same as the "strength" I saw in myself.



The ability to "see" obvious things that nobody else could, was the strength of childhood, the innocence of a child's eyes. There was something of the child in him, far more apparent than in any other mathematician I've known, and surely not by chance. He told me

that one day, when he was still in high school I think, he amused himself by checking the multiplication table (and along the way and by force of circumstance, the addition table too),  $\square$  for numbers from 1 to 9, in terms p. 224

definitions. He certainly wasn't expecting any surprises - if there was any surprise (a pleasant one, as always. . . ), it was that the demonstration could be done nicely and completely in just a few pages, the story of perhaps half an hour. When he laughed and told me the story, I could feel that it had been half an hour well spent - and that's something I understand even better today than I did then. This little story struck me, even impressed me (though I don't think I let it show) - I sensed in it the sign of an **inner autonomy**, a freedom from received knowledge, which had also been present in my relationship with mathematics in my childhood, from the very first contacts (69)<sup>13</sup> (\*).

This relationship of privileged interlocutor for each other, when we saw each other practically every day I believe<sup>14</sup> (\*\*), continued over a period of five years, from 1965 (if I remember correctly) to 1969 inclusive. I still remember the pleasure I had, in that year, in writing a detailed report on his work, when I was proposing to co-opt him as a professor in the institution where I had worked since its foundation (in 1958), and where most of my mathematical work was accomplished. I no longer have a copy of this report (64), in which I reviewed, I believe, a good dozen of my friend's works, almost all unpublished at the time (many have remained so), and most if not all of which, in my opinion, carried the weight of the main substance of a good state doctorate thesis. I was prouder and happier to present this eloquent report than I would have been had I been presenting a report on my own work (something I've only done twice in my life, and each time under compulsion. . . ). Many of these works were answers to questions I had raised (the only one published among them being the work already mentioned

on the degeneracy of the spectral Leray sequence for a clean and smooth morphism of schemes (63)). The two most important par $\square$ contre were answers to questions Deligne himself had asked, and he was p. 225

It was clear that their scope was of a completely different order than a "good state doctorate thesis". These were his work on the Ramanuyam conjecture (published in the Bourbaki seminar), and the work on mixed Hodge structures, also known as "Hodge-Deligne theory".

It's a strange thing, and one I was far from suspecting when I wrote this sparkling report, that I was to leave less than a year later this institution where I was about to have my young and impressive friend co-opted, and where I intended to end my days. And (now that I've put these two double episodes together) it's another strange thing, and no more surely the effect of mere "chance", that this same (now less young!) friend announced to me a month or two ago his own departure from this same institution, when it had also been a year since I had resumed regular mathematical activity, in the sense of a kind of unexpected "re-entry" onto the mathematical scene (if not into the "great world". . . ).

On more than one occasion in Récoltes et semailles, I've spoken of my departure - of this "salutary uprooting" - and even more of the "awakening" that closely followed it, and which made this episode a crucial turning point in my life.

<sup>13</sup>(\*) Incidentally, it seems to me that this freedom has never entirely disappeared during my life as a mathematician, and that it is once again present as it was in my childhood. Two or three years ago, I revisited the little episode of the multiplication table for my friend. I felt he was embarrassed by this evocation of a childhood memory, which no longer visibly corresponded to his self-image. I wasn't really surprised by his embarrassment, but saddened to see something that I knew well, but still found hard to admit, confirmed once again... ..

<sup>14</sup>(\*\*) This was the case at least as long as I lived in Bures, where he was housed in a studio at the IHES. From 1967 onwards (when I moved to Massy), I think we still saw each other once or twice a week, at least as long as I remained involved in mathematics.

my life. In the intense years that followed, the world of mathematicians, with those I had loved in it, and that very thing that had fascinated me most in mathematics itself, became very distant - as if drowned in the mists of memory of another "myself", who would have died ages ago... .

But both before this episode, and in the years that followed this first major turning point, I knew that the man who had been (a little<sup>15</sup> (\*)) my pupil and (a lot of) my confidant and friend, had only to follow the spontaneous impulse within him of a child who plays and wants to know, to discover and bring to light new and unsuspected worlds, and to fathom them and know their intimate nature - and thereby reveal them to his fellow creatures as well as to himself. So, if after my departure (with no spirit of return!) I saw "a bold and inspired mathematician" sketch out (for a start. . . ) the vast picture I had glimpsed, and of which I had still only drawn a series of partial and provisional sketches, it was indeed he - who had everything in his power to make the world a better place.

p. 226 hands to do it! Brushing up this first large-scale picture, a "master builder" bringing together in a common vision the essentials of what was known and guessed about the  $\square$  cohomology of varieties. For someone in whom such an overall vision was already ready to emerge from the mists of the as yet unwritten, this algebraic work was the work of a few months, not even years. (It would have to be taken up again and deepened over the years - or generations, if generations were needed - until the final word on the reality of the motifs was fully understood and established). And I had no doubt that this work, which used to "burn in my hands", would be done any moment now, and at least over the next two or three years, while it was still hot. After my departure, there was certainly only one person left who was called upon, by his very impulse of knowledge, to do this burning and fascinating work. Once the "maître d'oeuvre" had been written and tested, and the construction of the work more or less completed, I would leave it to others to continue this work, however fascinating it may be, and embark on other adventures, in this world of mathematical things, where every bend in the road reveals the promise of a new, limitless world, provided we have open, new eyes to see... .

At a time when my life was still taking place in the warm scientific incubator that isolated it from the noises of the world, and when Deligne was developing his extension of Hodge's theory (this must have been in 1968 or '69), it was a matter of course between us that this work was a very first step towards realizing, testing and refining a certain **part** of this "tableau des motifs", which had never been put down in black and white in its entirety<sup>16</sup> (\*). In the years following my departure from the "étuve", at a time when mathematics was a long way off for me, it came as no surprise to learn that Weil's conjectures had finally been demonstrated. (If there was any surprise, it was that the "standard conjectures" had not been demonstrated in the same breath, even though they had been developed precisely with a view to an approach to Weil's conjectures.

p. 227 Weil, at the same time as a means of establishing at least a theory of semisimple patterns on a body<sup>17</sup> (\*\*).) I was well aware that neither by  $\square$  this first draft towards a general theory of coefficients to the Hodge, nor by this demonstration of certain key conjectures (among a number of others that are more or less well known) he had yet reached his full potential - indeed, he was far from it. And I waited without impatience, while most of my attention was absorbed elsewhere. (-> 61)

<sup>15</sup>(\*) For the meaning of this scruple in me to consider the (too!) brilliant Deligne as one of my students, see the note "L'être à part" (n° 67).

<sup>16</sup>(\*) That this Hodge-Deligne theory never (as far as I know) went beyond this first draft, that it never expanded into a theory of "Hodge-Deligne coefficients" (and the "six operations" on them) above the type schemes finished on the field of complexes, is inseparable from this other strange fact: that this vast "tableau des motifs" has never been painted, and that its very existence has been carefully hushed up to this very day. ...

<sup>17</sup>(\*\*) It's only in recent years that I've become vaguely aware (but more precisely lately!) that the "standard conjectures", as much as the very notion of pattern for which they provided a first "constructive" approach, had been **buried**, for reasons that are now particularly clear to me. (Compare also the previous footnote).

## 14.2.2. The funeral

**Note** 61 I had been privileged to see the first flowering of a child's impulse, bearing the promise of a vast deployment. Over the next fifteen years, I came to realize that this promise was constantly being deferred. There was this delicate thing in him that I had been able to sense and recognize (at a time when I was insensitive to so many things!), a thing that is of an entirely different nature from cerebral power (which crushes as well as penetrates. . . ) - a thing that is essential above all for any truly creative work. I'd sensed it in others at times, but in no mathematician I'd ever known had it manifested itself with comparable force. And I expected (as a matter of course) that this thing would continue to blossom in him and transform itself, and express itself effortlessly in a unique work, of which I would have been a modest precursor. But strangely enough (and surely there's a deep and simple connection between so many "strange things") - I've seen this "delicate thing", this "strength" that's neither muscle nor brain, gradually fade away over the years, as if **buried** under successive layers, and thicker and thicker - layers of **something else** I know only too well - the most common thing in the world! It's not necessarily a bad match for brain power, consummate experience or a trained flair in a particular discipline, which can force the admiration of some and the fear of others, or both, through the accumulation of works, brilliant perhaps and surely having their strength and beauty. But **that's** not what I had in mind when I spoke of "unfolding" or "blossoming". The blossoming I had in mind is the fruit of an innocence, eager to know and always ready to rejoice in the beauty of the small and great things of this inexhaustible world, or of such and such a part of this world (such as the vast world of mathematical things. . . ). It is he who alone has the power of profound renewal, whether of self, or of knowledge of the things of this world. It is this power that has been fully realized, it seems to me, in the modest person of a Riemann<sup>18</sup> (\*). This true fulfillment is foreign to  $\square$ contempt: to the disregard of others (those we feel far below us... ), or that of things too "small" or too obvious for us to deign to take an interest in, or of those we feel fall short of our legitimate expectations; or the contempt of such a **dream** perhaps, speaking to us insistently about the things we profess to love... . It is foreign to contempt, just as it is foreign to the fatuity that feeds it.

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Certainly, with his impressive "means", but even more so with that delicate thing that impresses no one and **creates**, the "pupil" was destined to far surpass the "master". I had no doubt that, in the years following my departure from this place where I had witnessed such a beautiful flight, Deligne would give his full measure in the deployment of a vast and profound work, of which I would have been one of the precursors. The echoes of such a work would not fail to reach me over the years, while I myself, in the pursuit of other quests far from mathematics, could only imperfectly appreciate the full scope and beauty of the new worlds he was about to discover.

But the pupil cannot surpass the teacher by **disowning** him in his innermost self, by secretly striving, before himself and before others, to erase all trace of what he has contributed (whether the contribution was for the better, or for the worse. . . ) - any more than the son can truly surpass the father by disowning him. This is something I've learned above all through my relationship with my children, but also (later on) through my relationship with some of my former pupils; and above all with the one, of all people, whom I've always been scrupulous about calling "pupil", having sensed from the moment I met him that I had to learn from him.

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<sup>18</sup>(\*) The work of Riemann (1826-1866) is contained in a modest volume of around ten works (it's true that he died in his forties), most of which contain simple, essential ideas that profoundly renewed the mathematics of his time.

of him, as much as he of me<sup>19</sup> (\*). But it was only almost ten years after that meeting, after 1975 and especially since I've been meditating on the meaning of what I experience and witness, that I began to sense this **hindrance** in the man who continued to be dear to me. And I also felt, obscurely, that this secret disavowal of my person and of a role I had played in crucial years of his life, was too,

more profoundly, a disavowal **of itself**, (It is so, no doubt, whenever we disavow and want to erase something that has well and truly taken place, and whose fruit is ours to gather. ... ).

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However, lacking any kind of "connection" to "what was being done in maths", and to what he was doing himself<sup>20</sup> (\*), I never realized, until I thought about it a few weeks ago, how much this hindrance also weighed on the very thing in which he had invested his all: his mathematical work. Certainly, more than once in the last eight or nine years, I've seen simple common sense or a mathematician's healthy instincts wiped out by a deliberate gesture of disdain (towards me) or contempt (towards others whom it was in his power to discourage) (66). Indeed, he was not the only one of my former students, with or without quotation marks, in whom I witnessed such attitudes towards people I cared about (or towards others). But in no other case have I been so painfully affected. More than once in the course of my reflections over the past two months, I've alluded to this experience, "the most bitter I've ever had in my life as a mathematician" - and I've also said what it ultimately taught me, at the end of this Harvest and Sow reflection. This sorrow was so vivid, it taught me something so far-reaching about a person who was still dear to me (while I continued to evade what it also taught me about myself and my past. . . ), that the question of its impact on his mathematical "creativity", or even on that of the person who had been discouraged or humiliated, became entirely secondary, not to say derisory.

The note "Refus d'un héritage - ou le prix d'une contradiction" (Refusal of an inheritance - or the price of a contradiction) is the first written reflection in which I took stock of what had come back to me in bits and pieces, here and there, over the years, both on the "state of art", and on the work of the man I had known so well and so little. It was also the first time I had seen, in a single glance, the full "**price**", or the full weight, in his work as a mathematician, of the refusal he had carried within him for more than fifteen years. In writing this note, however, I was "delaying", since for two years already (and without "anyone" seeing fit to inform me), the reasons had been brought out of the secrecy in which they had been kept.

for twelve years. ... And today, as I write this final stage (I believe) of my reflection on my mathematical past, two days after having read in large lines this memorable volume which

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the perception of this crushing weight has become striking. It's the weight that, day after day and through a hundred detours, those who are made to fly enjoy dragging along - a flight that's supple and light, joyful and intrepid in its pursuit of the unknown, for its own joy and that of the wind that carries it. ... <sup>21</sup>(\*)

If he doesn't steal, and if he's content to be a man admired and feared, accumulating proof of his superiority over others, I have nothing to worry about, If he drags the weights he likes to drag, surely he finds some

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<sup>19</sup>(\*) (June 14) On the subject of my deliberate and persistent intention to minimize what I had to contribute, and to deny the reality of a master-pupil relationship, see the note "L'être à part", n° 67 . Clearly, there's no comparison between what my friend learned from me ("as if he'd always known", of course!), and what I learned from him. It would undoubtedly have been otherwise, had I continued my intense mathematical investment to this day, and maintained regular mathematical contact between us.

<sup>20</sup>(\*) Since 1970, I've received four offprints by Deligne, which (like most of the offprints I still receive) I skimmed through on the spot. It wasn't enough to give me an idea of a mathematical work, even in outline or through its main themes.

<sup>21</sup>(\*) I don't mean to suggest that it's the privilege of a few exceptional beings to be called upon to "fly" and discover the world - surely we're all called upon by birth! However, this capacity rarely finds the opportunity to blossom, even if only in a very limited direction (such as mathematical work). But in one person I've seen such an ability (in the "mathematical" direction) preserved as if by miracle, only to regress over the years.



satisfactions - just as I myself have taken pleasure in dragging along weights, and continue today to drag along those I haven't yet been able to part with along the way. Of what I had to offer, the best and the worst, he took what he liked. I don't have to worry about his choices, which are his alone; nor do I have to decide here whether they are the best or the worst (62). What's "best" for one person is "worst" for another, or sometimes for the same person (as long as he changes, which is admittedly not very common. . . ).

But the choices we make, and the actions that express them (even though our words often deny them), we make at our peril. While they often bring us the expected gratifications (which we receive as "the best"), these very gratifications sometimes end up having setbacks (which we reject as "the worst", and often as an outrage). When we finally understand that setbacks are not an outrage, we often regard them as a price to be paid, which we pay with reluctance. Sometimes, however, we come to understand that such setbacks are something other than ruthless cashiers, to whom we have to pay for the good time we've had, whether we like it or not. That they are patient and obstinate messengers, who never tire of coming back to bring us the same message over and over again - an unwelcome message, to be sure, and one that is constantly rejected. - because even more than the setback itself, it's its humble message, always rejected, that appears to us as "the worst": worse than a thousand setbacks, often worse than a thousand deaths and the destruction of the entire universe, for which we no longer care. ...

□ On the day at last when it pleases us to welcome the message, eyes suddenly open and see: what was and the crushing weight from which we are suddenly relieved is the very thing we were clinging to only yesterday, as "the best".

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### 14.2.3. The event

**Note** 62 (April 21) People will tell me that if I have nothing to worry about, then why am I going on for pages and pages about a personal relationship that concerns only me and the person concerned!

If I felt the need to reflect retrospectively on certain important aspects of a relationship, it was under the impact of a specific event that affected me closely (even though I learned of it two years late). This event, on the other hand, is in the public domain, in an even more obvious way than the behaviours and routine acts of prominent mathematicians (such as Deligne, or myself) towards others of lesser renown or beginners (although their effect on the lives of others is often of a quite different scope than in the present case). The event in question (i.e. the publication of the "memorable volume" of Lecture Notes LN 900, a.k.a. the "funeral volume"), like everything surrounding it, struck me as **unhealthy**, rightly or wrongly. I felt it was healthy for everyone, starting with the "interested party" himself, to give a detailed account of some of the ins and outs, getting to the bottom of things as I see them today.

With this account and these thoughts, I'm not trying to convince anyone of anything (far too tiresome, and moreover hopeless!)<sup>22</sup> (\*), but simply to understand the events and situations in which I found myself involved. If they inspire others to think beyond the usual clichés, this testimony will not be published in vain.

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<sup>22</sup>(\*) (May 25) If I felt the need here to repeat to myself that it was "far too tiring" and "hopeless" to try to convince, it's undoubtedly because somewhere inside me, the intention to convince was nonetheless well and truly present, and also perceived. The entire period of reflection between April 19 (when I learned of the "memorable volume" LN 900) and April 30 was marked by a state of inner tension, and also division, in the face of the impact of an entirely unexpected "event", which I was trying as best I could to reconcile.

to assimilate the message. This tension was finally resolved with the note "Le retour des choses" (n° 73) of April 30, when in fact reflection had just returned to my own person, to immediately provide me with the obvious key to this message.

#### 14.2.4. Eviction

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□  
**Note 63** (April 22) This article (\*) appeared in Publications Mathématiques in 1968, i.e. two years later. before I left the world of mathematicians. Its starting point had been a conjecture I'd told Deligne about, of a property of degeneracy of spectral sequences which at the time might have seemed quite incredible, but which nonetheless became plausible by "arithmetic" means, as a consequence of Weil's conjectures. This motivation was of great interest in its own right, as it showed how much could be gained from a "yoga of weights" implicit in Weil's conjectures (a yoga first glimpsed by Serre, in certain important aspects). Since then, I've routinely applied it to all sorts of analogous situations, to draw conclusions of a "geometric" nature (for the cohomology of algebraic varieties) from "arithmetic" arguments. These remained heuristic as long as Weil's conjectures were not established, but were nonetheless highly probative, and represented a first-rate **means of discovery**. Deligne's "geometrical" demonstration for the particular conjecture in question, using Lefschetz's theorem (established then in null char. only), had interest in an entirely different direction, in addition to the first merit of not depending on any conjecture. The link indicated by the two approaches between two seemingly unrelated things - Weil's conjectures (and the yoga of weights that represented the most fascinating aspect of them for me at the time), on the one hand, and Lefschetz's theorem, on the other - was in itself highly instructive.

The interesting thing here, for my own present purposes, and which only became clear to me today, is that the reader of this article will have very little chance of suspecting that I had anything to do with the initial motivation of the main result, and no chance at all of learning from this article what that motivation had been. (See also the beginning of note (49). The **spontaneous** approach (including, I'm sure, on the part of the author himself) to the exposition of a result like this would have been to start with the (admittedly striking) conjecture, to indicate the first, equally striking, reason found for it, which was a good opportunity to finally "sell" this famous yoga of the weights, of much greater scope in itself

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that the main result of the work<sup>24</sup> (\*); then to follow with the "Lefschetz theorem" point of view<sup>25</sup> (\*\*) which proved the initial conjecture under slightly more general conditions (any basic scheme, not necessarily clean and smooth over a body), but in characteristic zero only. On the other hand, the exposition that follows begins with generalities of homological algebra (as pretty as can be, and presented with the author's customary elegance), generalities that he, like everyone else, must have since forgotten, axiomatization-style, of Lefschetz's theorem. The main result (the only one everyone remembers, of course) appears as cor. X towards the middle of the article, while in "remark 2.9" some

<sup>23</sup>(\*) This is Deligne's article on the degeneracy of spectral sequences and Lefschetz's theorem (Publications Mathématiques 35, 1968) cited in the note "Poids en conserve et douze ans de secret", n° 49).

<sup>24</sup>(\*) It was yoga itself that remained a secret (I think) for the next six years!

(June 7) And (as it has since appeared) which was then presented by Deligne "on his own account", without any allusion either to Serre or to me. (See notes n° 78<sub>1</sub>, 78<sub>2</sub>).

<sup>25</sup>(\*\*) (June 17) The idea of using Lefschetz's theorem ("Vache") to demonstrate a degeneracy of spectral sequences is a good one. due to Blanchard, who only obtained the degeneracy theorem under the draconian assumption (rarely verified) that the local system formed by the rational cohomology of fibres is trivial. I was familiar with Blanchard's work, and mentioned it to Deligne, who thus drew on Blanchard's idea for his demonstration, even though he hadn't read the article. Serre, who remembered Blanchard's demonstration better than I did, pointed out to Deligne that his demonstration was in fact an easy adaptation of Blanchard's. This is what Deligne points out in his remark 2.10. This remark, in which he quotes Serre, is written in such a way as to give the impression that he only learned of Blanchard's idea after the fact, which is in no way the case. The two main **sources** for his article have therefore been overlooked: on the one hand, the arithmetical **motivation**, which made it possible to foresee a considerable strengthening of Blanchard's result, and on the other, Blanchard's **demonstration idea**, which he elegantly adapts to obtain a result that Blanchard had probably not dared to hope for, and for that reason had not even tried to "get" by his method.

Somewhere near the end (the reader doesn't quite know why) the word "weight" and my name are pronounced. ...

I can't remember what impression the article made on me when it first appeared - as I was in the know, I must have just glanced at it. I must surely have sensed an intention to "distance myself", but also felt that it was only natural that my friend should be concerned about not risk appearing as a disciple (or "foal") of a "master"<sup>26</sup> (\*\*\*), It is true that if there had been in him the quiet assurance□in his own strength, he would have had no hesitation in writing a work of greater scope greater and more useful for everyone (including surely for himself), without fear of not being seen for what he is... . (65). p. 234

The situation was somewhat analogous with the publication of his first large-scale work the following year, on mixed Hodge theory. (At the time, I regarded this work as comparable in scope to Hodge theory itself, seeing it as the starting point for a theory of "Hodge- Deligne coefficients", which unfortunately never saw the light of day. ... ) As I said, it was obvious to both him and me that this work had its "motivation" in the yoga of patterns I had arrived at over the preceding years - it was a first approach towards a tangible realization of this yoga. To emphasize such a link in his work, it seemed to me (and it must have seemed to me then too), would at once have given his work an even wider scope than it already had on its own merits. At the same time, it was yet another opportunity to draw the reader's attention to the reality of patterns, perceptible at every step behind that of Hodge's structures (63 ).<sup>1</sup>

It's only with hindsight that these omissions take on their full meaning, against the backdrop of six years of silence on weight yoga<sup>27</sup>, twelve years of silence (not to say, banning) on<sup>28</sup> patterns, the unusual re-entry of these in the LN 900 volume-burial, stagnation in Hodge- Deligne theory after a dazzling start... . But no one can do great things in a bogeyman's mood!

In any case, had I been more mature when I left IHES in 1970, it would have been clear to me from that moment on that there was a profound ambiguity with regard to me in the man who, over the past five years, had been my closest friend. Moreover, behind the amiable facade of good company within the same hushed institution, my departure was ultimately to my advantage.

everyone, for reasons that I think I can discern with hindsight, and which were not the same for everyone. Visi□blement this departure suited my young friend, recently settled in the place, and to whom he would have all it took was a show of solidarity with me (in the face of the hesitant indifference of the other three permanent colleagues) to turn around an indecisive situation. If I didn't understand what was going on at the time, it was because I really didn't want to understand things that were quite clear and even eloquent! It was as if, often in the course of my life, there was an anguish in me (never called by that name!) that signalled a "take-off" between a reality that was tangible and simple, and an image of reality that I didn't want to understand. p. 235

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<sup>26</sup>(\*\*\*) (May 26) About this attitude of mine, see the note that follows this one, "Ascension" (n° 63').

(June 8) Drawing a parallel with his own style of appropriating other people's ideas, which I see here the first typical example, I realize that my friend's motivation was in no way to preserve an "autonomy" in relation to a prestigious "master", but rather to conceal the role of other people's ideas in the genesis of his own.

We're also looking forward to appropriating these ideas from others (at a later date). (On this subject, see the two notes "Le Prestidigitateur" and "Appropriation et mépris", n° 75" and 59'.) Regarding my share of responsibility for the unfettered development of this propensity in my friend, see the two notes "The Ascent" and "Ambiguity", as well as "The Being Apart" (n° 63',

63", 67'), in which the role of my complacency towards the brilliant young man Deligne becomes apparent.

<sup>27</sup>(\*) (April 19, 1985) For corrections to "six years" and "twelve years", see b. de p. note (\*\*\*) p. 302 (part dated April 18, 1985), for weights.

<sup>28</sup>(\*) (April 19, 1985) For corrections to "six years" and "twelve years", see sub-note "Pre-exhumation" (n° 168<sub>1</sub>), for reasons.

the image of my role in the institution I was leaving, and perhaps even more so, the image of my relationship with my friend. It was this refusal to acknowledge an irrefutable reality, and the anguish of this contradiction to which I was clinging, that made the episode of this "salutary uprooting" so painful at the time<sup>29</sup> (\*).

To tell the truth, since I'd never written anything about this relationship (apart from the beginnings of a few episodic letters to my friend, none of which got any response. . . ), I hadn't realized before that the first signs (discreet, of course, but which can't be mistaken) of ambivalence in my friend's relationship with me go back at least to 1968. ), I hadn't yet realized that the first signs (discreet, admittedly, but unmistakable) of ambivalence in my friend's relationship with me date back at least to 1968, two years before "Le grand tournant". It was a time when the relationship seemed perfect, a mathematically unclouded communion, in the context of a simple, affectionate friendship. It's all very well to mock the beautiful "tartines" about innocence, the creative child and so on!

Yet I know that this communion was a **reality**, by no means an illusion; just as this "delicate thing" was a reality - this creative force, of which the work that followed gives only a pale reflection. "Innocence" and "conflict" are two tangible realities, recognizable to the slightest awakened perception, by no means concepts; and they seem to me by nature foreign to each other, one excluding the other. Yet there's no doubt that these two realities coexisted in my friend's relationship with me, at different times of the year.

p. 236 different levels<sup>30</sup> (\*\*). It doesn't seem that at the time I'm talking about here, "conflict" interfered with mathematical creativity - at least not in the work done in solitude, or the work done in interviews face-to-face. It's also true that in the two articles I've just mentioned, which after all are among the most tangible fruits of this work, the imprint of "conflict" is already clearly visible. And with the benefit of fifteen years' hindsight and the reflection of days and weeks gone by, I can see that this imprint (however discreet it may be) strikingly prefigures the particular form that this gradual grip of conflict on the initial impetus was to take, stripping it over the years of its rarest essence - that which makes for great destinies(\*).

**Note** 63<sub>1</sub> (May 26) Compare also with the remark in footnote<sup>31</sup> (\*) at the end of note 60, noting the "blockage" of the natural development of Hodge-Deligne theory, as a result of attitudes of rejection towards certain key ideas introduced by me (here, the six operations - to which the motives are indissolubly linked), of the same nature as that examined here, apparent therefore from the publication of Hodge Theory I and II.

The same attitude, striving as far as possible (if not beyond!) to erase all traces of my influence, can be found in the work (already mentioned in note n° 47) written in collaboration with Mumford, on Mumford-Deligne compactifications of modular multiplicities. (This work also predates my departure.) The work uses a principle of passing topological results on

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<sup>29</sup>(\*) See note no.° 42.

<sup>30</sup>(\*\*) On two or three other occasions, I have witnessed such coexistence in the same person at a given time, y including myself at times.

<sup>31</sup>(\*) Such lofty lyricism has made me lose touch with down-to-earth realities. If I describe this "imprint" as "discreet", it's because I'm wrapped up in a layer myself, and I find it hard to separate myself from the blinders I still hold dear! Having finally got rid of them, I realize that the "imprint" in question is a crude concealment, which I didn't want to see because of a certain complacency in myself, which I clearly realize in the note of June 1 "Ambiguity", n° 63". As for the "grip of conflict on the initial impetus" of my brilliant young friend, I speak of it almost as a regrettable fatality of which the poor man would be the unwitting victim, losing in the process, alas, the benefit of "great destiny". Yet he is responsible for his own destiny, just as I am for mine. If, even before my departure, he chose the role of his master's gravedigger (for a start), and if circumstances (including the spirit of the times) were conducive to this choice, granting him the role of Big Boss to whom all blows are permitted, he also chose to taste to the dregs the privileges that prestige and power can give, including that of crushing (discreetly) and despoiling. You can't have it all, and it's in the nature of things that by this choice (in which he's in good company) he loses the benefit of more delicate and less sought-after things. ... (Undated footnote, early June).

the body  $C$  (known by transcendental means) to results in  $\text{car. } p > 0$ , which I had introduced in the late fifties' <sup>□</sup> for fundamental group theory. By the early sixties, I had suggested <sup>p</sup> to use this method to prove the connectedness of modular varieties in any characteristic<sup>32</sup> (\*). However, this idea ran up against technical difficulties that had stopped Mumford, and which were elegantly overcome in their work by the introduction of modular **multiplicities**, and a "compactification" of these that has perfect properties. The very idea of modular multiplicities can be found, "between the lines" at least, in my "Teichmüller" talks at the Cartan seminar, given at a time when the language of sites and topos did not yet exist. The very language used by Deligne ("algebraic stack") where there was a whole language of sites, topos, multiplicities tailor-made to express this kind of situation, shows quite clearly (with hindsight and in the light of much larger later "operations") the intention to erase the provenance of some of the main ideas implemented in this brilliant work. It was surely this attitude (as I first sensed in the note "Refus d'un héritage - ou le prix d'une contradiction", n° 47) that had a "chainsaw effect", cutting short further reflection on modular multiplicities, which nonetheless appear to me to be among the most beautiful and fundamental of all "concrete" mathematical objects identified to date.

In passing, I'd like to point out that the arguments I introduced in the late 1950s make it possible (thanks to Mumford-Deligne compactification) not only to prove the connectedness of modular multiplicities in any characteristic, but also to determine their " $p$ -first fundamental group", as the " $p$ -first profinite compactification" of the ordinary Teichmüller group.

#### 14.2.5. The ascent

**Note** 63 (May 10) With the additional hindsight of less than three weeks, I now realize that this attitude, which was intended to be "understanding" in relation to this "quite natural" intention to distance oneself, was in reality a lack of clear-sightedness and complacency towards my brilliant young friend. If I had relied on my healthy faculties of perception, instead of letting myself be dazzled and given the lie to vague clichés posing as "understanding" or even "generosity" ("I'm not going to give him a hard time just because he doesn't put my name up. . . ."), I would have realized that my friend's attitude was one of "understanding" and "generosity". ), I would have realized then what I realize now, sixteen years later. I could call it a lack of probity.

vis-à-vis the reader, vis-à-vis myself and vis-à-vis himself. Seeing things simply and unafraid <sup>to □ call</sup> them by name, I would have been able to talk about them simply, as I am now, and my <sup>p</sup> friend had the opportunity to learn from them - or at least he would have understood that even with the means at his disposal, his elders (or at least one of them) expected him to show the same probity in his work as they did themselves. So I can see that on that occasion, before my departure from the mathematical scene, at a time when I was by no means "out of the game" and undoubtedly exercised a certain moral ascendancy over my young friend, I failed to live up to my responsibility towards him, through the **laxity** I displayed at the time<sup>33</sup> (\*). This was confirmed by the publication of "Hodge Theory II", Deligne's thesis work, in which he makes no reference to my motives or to me. It's true that, even then, the mathematics and the very person of my friend were very far away and appeared to me as if through a fog!

<sup>32</sup>(\*) (September 1984) verified, this circumstance is indeed mentioned in the introduction to the work cited (p. 75).

<sup>33</sup>(\*) (May 28) The word "complacency" here better expresses the nature of my attitude, than the somewhat elusive word "laxity". This complacency in my relationship with my young and ; brilliant friend became clearer to me in yesterday's reflection, see the note "L'être à part", n° 67'.

In the light of what I've seen of my friend's development, both spiritual and mathematical (and the two aspects are closely intertwined), I can see that when I first met him and was impressed by his intellectual means, his acuity of vision and his liveliness of understanding in mathematics, I could discern no lack of maturity in him ; nor (subsequently) the effects of his meteoric social rise, in the space of just four years, from unknown student to mathematical star and tenured professor, vested with considerable privilege and power, at an already prestigious institution. I have no regrets about facilitating his ascent and speeding it up - but I can see that it was due to a lack of discernment and maturity on my part. The "favor" I did him was not a favor. It won't have been a "service", at least until my friend himself has completed this harvest, which he prepared with my carefree assistance.

### 14.2.6. Ambiguity

**Note 63** (June 1) In the three weeks since this observation of "laxity" (or "complacency", to use the more appropriate expression that has appeared in the meantime) in my relationship with my friend Pierre, I have had the opportunity in my reflection to realize more clearly a certain lack of rigor, a complacency in myself. They manifested themselves in my relationship, first of all, with the one I more than

p. 239 but also to other mathematicians for whom I was the eldest. What I have detected□justifiably so far in this sense has been expressed by a certain ambiguity in me, and without

I'm sure I was also a pupil, in situations where my pupil took on ideas and methods he'd learned from me, or even a detailed master builder of a whole body of work he was doing, without clearly indicating its source or even alluding to it. Such situations were quite common in the sixties, after my departure and right up to the last few years. It seems to me that in all these situations, at some level I sensed the ambiguity, which was expressed by a shadow of unease, never examined until these very last days. The motivation that made me play along with a certain connivance, and that made me pass over this malaise without ever paying attention to it, was to **conform to** a certain image I had of myself, and of what so-called "generosity" should be. True generosity is not born of conformism, of a concern to be (and appear, to oneself and others) "generous". The repressed discomfort was always a clear sign that this "generosity" was fake, that it was an **attitude**, not the spontaneous, unreserved gift of true generosity.

In this malaise, I see two components of different origins. One comes from the "boss", the "me" who remains frustrated, because he hasn't been able to have it both ways: taking credit for a job he knows he's done (to a greater or lesser extent), and at the same time living up to a certain brand image, which includes (among many other things) the eponymous label of "generosity". The other component comes from "the child", of the one in me who is not fooled by attitudes and facades, and who has the simplicity to feel what this situation has of false<sup>34</sup> (\*). Not only false towards myself, but also□vis-à-vis the other.

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<sup>34</sup>(\*) (June 5) When I say here that the discomfort comes (in part) from "the child", it's a way of speaking that gives a false image of reality. It's not the candid perception of a false situation that creates discomfort. The discomfort is the sign of **resistance** against this perception, of a take-off between the reality actually perceived at a certain level (in this case, that of a false situation), and an **image** of reality to which I cling (in this case, that I'm being "generous" and that I couldn't do less!), to the benefit of which **I dismiss**, I repress the unwelcome perception. In this case, as soon as I abandon resistance and allow the perception to appear in the field of conscious gaze, the "discomfort" has ceased, along with the false situation. I was going to add "assuming it's a false situation involving my present, and not a situation in the past". But on reflection, I realize that these false situations "from the past", of which I have just spoken, have remained present as such until today, or at least until the reflection of three days ago,

In short, my "generosity" consisted in entering into a game where the other presents as his own ideas that come from others, and thus gives an image of himself and of a certain reality, which he and I both know is false. So we're in this together, in what we might call a "cheating" game, in which both he and I have had our share. It's a "cheat" at least according to the consensus that prevailed "in my day", and which, it seems to me, is still being paid lip service to today. Surely I would not have entered into such a game if it had been a question of someone else's ideas being used as if they had been found by my "protégé"<sup>35</sup> (\*). However, the fact that I tacitly agree to my own ideas being presented as someone else's does not, it seems to me, change the essential nature of the thing - the only difference is that in this case there are two of us cheating, instead of just one. And even apart from this aspect concerning myself (that I myself am taking part in cheating, in behavior that goes against the very consensus I claim to adhere to), it's quite clear that there's no generosity in encouraging others to cheat (even if it looks like we're doing it at our own expense - which is in no way the case), or at the very least to adopt an attitude of ambiguity towards a consensus to which they too pretend to adhere, while at the same time violating it. True generosity is by nature beneficial to all, starting with the person in whom it manifests itself and the person to whom it is addressed. My ambiguous attitude, arousing or encouraging ambiguity in others, and allowing myself to pose as "generous" when, logically, the other must appear to be a bit of a cheat (and in fact we both are) - this attitude is a benefit neither to me nor to the other.

All I had to do was examine the matter and the obvious would become apparent, without even having to refer to an experience, a "lesson from events". Yet it was events that brought me to this point. examination, finally making me discover something obvious that I was just as capable of discovering thirty□years ago,

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before another student appeared on the horizon to learn a trade with me, and to imbibe a certain spirit in the exercise of that trade. I've had occasion to talk about the "rigor" in the work itself, which I believe I demonstrated (see the section "Rigor and thoroughness", n° 26). But today I've also noticed, outside the "work" itself, an absence of rigor, expressed in the ambiguity and complacency I mentioned earlier. It seems to me that this ambiguity in me was not communicated to me by any of my elders, all of whom (I believe) were as demanding of me as they were of themselves. Beyond the ambiguity of the particular attitude, I detect an ambiguity in my own person, which I had occasion to mention more than once in the first part of *Récoltes et Semailles*. This ambiguity began to be resolved with the discovery of meditation in 1976, although some of the signs of this ambiguity, expressed in attitudes and behaviour that have become habitual (notably in my relationship with my students) must have persisted to the present day.

Clearly, this ambiguity within me has found fertile ground in some of my students. What was done by tacit agreement has even become, it seems, a fundamental note in the mores of the mathematical "big world" today, where fishing in troubled waters (with or without the agreement of the "interested party"), or even plundering (when the one who allows himself to do so is part of the intangible elite), seems to have become such a common practice that nobody seems to be surprised by it anymore, even though everyone is careful not to talk about it. The "boss" in me

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by the very fact of never having been examined and thus resolved. I remained a prisoner to the point of mechanically reproducing the same situations as soon as the opportunity arose. The knowledge of my meditation "power" (which I mentioned in the section "Desire and meditation", n° 36) was of no use to me, as I was unable to pay day-to-day attention to the situations in which I found myself.

I'm involved, and in the incessant game of perception and "sorting" of perceptions, this game of the child and the boss silencing him. ...  
<sup>35</sup>(\*) This expression "my protégé", used by one of my former pupils to refer to one of my current pupils who had just done some great things in mathematics, made me cringe. And yet, the ambiguous situation I'm now examining, on balance, establishes a false relationship in which one of the two protagonists does indeed act as the other's "protégé".

would like to stand out, to denounce, to take offense - and yet in doing so, I'm only perpetuating the same ambiguity in myself that I can now see has proliferated.

## 14.2.7.

**Note 63**<sup>36</sup> (April 24)<sup>36</sup> (\*) Flipping through an offprint of Mebkhout that I had just received two days ago, I came across a reference to a work by J.L. Verdier entitled "Catégories Dérivées, Etat 0" published in SGA 4<sup>1</sup><sub>2</sub> (Lecture Notes n° 569, pp. 262-311). I apologize for not realizing this earlier. publication, having never before had the honor of holding this volume in my hands, of which neither p. 242 Verdier nor Deligne (who is the author)<sup>37</sup> have seen fit to send me a copy, on its publication or later. I don't know whether C. Chevalley and R. Godement, who with me formed the jury that awarded J.L. Verdier the title of "docteur es sciences" on the strength of a 17-page introduction (still unpublished), were themselves entitled, ten years later, to receive "L'état 0" (50 pages this time) of this "thesis" like no other! I seem to remember once holding in my hands a serious work of foundations of some hundred pages, which could reasonably pass for a good doctoral thesis, and which corresponded roughly to the work of foundations I had proposed to Verdier around 1960 - except that it had already become clear by then that the framework of "triangulated categories" developed by him (to express the internal structure of derived categories) was insufficient.

Needless to say, my name is nowhere to be found in this "State 0" of a thesis. Indeed, one wonders what it would have to do with it. It's well known that the derived categories were introduced by Verdier, to enable him to develop the so-called "Poincaré-Verdier" duality of topological spaces, and the so-called "Serre-Verdier" duality of analytic spaces, while waiting for a vague unknown in the service<sup>37</sup> (\*) to develop a synthesis of the two on his behalf, appropriately called (the Elève Inconnu could do no less!) the "Poincaré-Serre-Verdier duality". After all that, all I had to do was follow suit and make the necessary adaptations to develop the Poincaré-Verdier duality and the Serre-Verdier duality within the very specific framework, my faith, of the coherent cohomology of schemes... .

I've only just become aware (libraries are useful!) of SGA 4<sup>138</sup> (\*\*), in which I've again been honored to be listed as Deligne's co-author, or rather "collaborator" (sic) (without seeing fit to inform me, let alone consult me). This was obviously a precursor to the memorable "volume enterrement" published five years later, which I had the pleasure of reading a few days ago (see notes n° 50, 51 and following, inspired by the event). But I didn't get to hold the pre-burial volume in my hands, with this piece of evidence of a phantom thesis that doesn't say its name

name, to understand as early as last year that the next state of this "thesis" would never be written by anyone but myself. And so I set to work on La **Poursuite des** ☐ **Champs**, where p. 243 seventeen years ago.

<sup>36</sup>(\*) This note comes from a footnote to "L'instinct et la mode - ou la loi du plus fort" (n° 48) - in which I stated that Verdier's work on derived categories had never been published, without realizing that a "Etat 0" of his thesis had appeared in 1977. For an overview of Verdier's strange twists and turns in relation to the theory that was supposed to constitute his thesis work, see the note "Thèse à crédit et assurance tous risques", n° 81.

<sup>37</sup>(\*) See the note "L'inconnu de service et le théorème du bon Dieu" for some information on this dubious character. (note n° 48').

<sup>38</sup>(\*\*) see note on this volume, "La table rase", n° 67.



## 14.2.8. The investiture

**Note** 64 (April 25) Yesterday, however, I found a copy in my office at the University. In fact, it's two consecutive reports, written a year apart in April (?) 1968 and April 1969. In seventeen pages, I review fifteen pieces of work carried out over three years of scientific activity at IHES. These included work on Ramanuyam's conjecture, the compactification of modular sites, and the extension of Hodge's theory. The body of work reviewed in this report (if only the works I have just named) bears witness to a prodigious creativity, unfolding with perfect ease, as if at play. Leaving aside the demonstration of Weil's conjectures, still in the wake of this first plunge into the unknown, it seems to me that the subsequent work gives only a pale image of this unique flight of a young mind with exceptional means, and also benefiting from exceptional conditions for its blossoming. Yet something about these "exceptional conditions" must have nourished this other force, foreign to the drive for knowledge, which ended up taking over and supplanting it, diverting and absorbing the initial impulse. And obviously, this "something" was also linked to me... .<sup>39</sup>(\*)

This short report with commentary (which I intend to include as an appendix to the present volume) seems to me to be interesting in more ways than one, including from a mathematical point of view (although some of the work reviewed remains unpublished to this day). In several places in the report, I anticipate that such work, which Deligne had confined himself to outlining and dealing with the crucial points, will be developed further by future students. These students never appeared, given the changes that subsequently took place in its relationship to the common man<sup>40</sup> (\*\*). Of the ideas I review, the only one to my □connaissance that was developed by someone else (who would thus appear to be a pupil of Deligne) was the theory of cohomological descent, developed by Saint Donat in SGA 4 (so still in the period of the initial impetus), a theory that has since become one of the most commonly used tools in the cohomological arsenal.

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Amusingly and characteristically, for three of the four works that have since been the subject of articles by Deligne<sup>41</sup> (\*), I take touching care to make clear, in passing, the relationship of these works to ideas I had introduced and questions I had raised - as if to pre-empt, one would say, the silence the author was going to make about them in his articles (none of which had appeared, or even, I believe, been written, at the time I reported).

## 14.2.9. The node

**Note** 65 (April 26) It's clear, too, that keeping a large-scale "yoga" (that of weights, and beyond that, that of motives), about which I had spoken here and there to others than him, but which he was the only one to know about, is not a good idea.

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<sup>39</sup>(\*) (May 26) On the subject of a certain complacency within me that gave rise to this "something", see the note (two weeks later than the present one) "L'ascension" (n° 63').

<sup>40</sup>(\*\*) In the days when I worked with him regularly at IHES (in my seminar in particular), Deligne's relations with the other The kindness of the mathematicians, especially the young researchers (often beginners) who came to the seminar, was unmistakable. I noticed the same openness to other people's thoughts, even if they were awkward or confused, as in our mathematical tête-à-tête. He had that ability to follow the thoughts of others in their images and language, which I've always lacked, and which (it seems to me) predisposed him much more than me to the role of "master", able to stimulate the blossoming of a vocation, a creativity in others.

<sup>41</sup>(\*) The only one of the four works in question not directly influenced by me is the one on Ramanuyam's conjecture, deducing it from Weil's conjectures. It takes place in a research direction (that of modular forms) that constituted one of the most serious "holes" in my mathematical culture. The other three works are those on the degeneracy of the cence of Leray's spectral suite, on Hodge-Deligne theory, and on modular multiplicities (in collaboration with Mumford), discussed in the note "Eviction" (n° 63) and in sub-note n° 63<sub>1</sub>.

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to have assimilated it intimately and to grasp its full scope, conferred on him an additional "superiority", as the exclusive possessor of an incomparable instrument of discovery for an understanding of the cohomology of algebraic varieties. However, I don't think that this temptation played a decisive role, at a time when I was still very much present and active in the mathematical world, and when there was nothing to foreshadow my departure sine die. She must have appeared with or after my departure, which was an unhopd-for "opportunity" to seize an inheritance (which was rightfully hers!), hiding both the inheritance and its provenance. It is here that I see once again, in an extreme and particularly striking case, the crux of a profound contradiction, which goes far beyond any specific case. I want to bet on the ignorance, the disdain, the deep-seated doubt that surrounds the creative force that lies within our own person - that heritage. unique and of greater value than anything a person could ever pass on. It is this ignorance, this insidious alienation □ from what is most precious, most rare within us, that makes it possible for us to envy the strength perceived in others, and covet for ourselves the fruits and outward signs of this strength in others that we have forgotten in ourselves. As soon as this envy, this desire to **supplant**, takes root and finds an opportunity to proliferate, as soon as it channels the energy available for creative fulfillment, this alienation within us deepens, settles in permanently. The closer we come to the coveted "goal" of supplanting, crowding out, dazzling, the more we distance ourselves from and cut ourselves off from this delicate force within us, and clip the wings of our own creative impulse. In our tenacious effort to rise, we have long forgotten that we are meant to fly.

In his relationship with me, from the day we met, I felt my friend was perfectly at ease, without any sign that he was in the least impressed or dazzled by my reputation or person, or that there was any unspoken doubt in him, whether about his gifts or faculties in the mathematical field, or about anything else. It's also true, it seems to me, that he had received a friendly and affectionate welcome from me and my environment, including my family, which was likely to put him at ease. But the simple, seemingly unproblematic naturalness that drew me to him as it drew others to me, had surely not waited for this encounter to appear and blossom. The impression he gave off, which made him so endearing, was one of harmonious balance, where his penchant for mathematics was in no way a devouring goddess. Next to him, I was a bit of an unrepentant "polard", not to say a "thick brute" - and I remember his discreet astonishment at my lack of deep contact with nature around me and the rhythm of the seasons, which I passed through without seeing anything, as much as saying... .

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Yet this profound "doubt", which I would have been incapable of perceiving then (or perhaps even today, in similar circumstances), must have been present in my friend long before we met. Looking back, I can see the first unambiguous sign of this as early as 1968, and even clearer signs in the years that followed<sup>42</sup> (\*). These are "indirect" signs, however - none of the ones I've been able to First-hand observation doesn't come in the form of doubt, a lack of assurance - rather, and increasingly over the years by what may seem the opposite: a smugness, a □ propos deliberate disdain, even contempt. But such an "opposite" reveals its opposite, with which it forms a pair and of which it is the shadow.

I also heard through an intermediary that, for a prestigious (and notoriously awkward) mathematician whom he had never had the opportunity to meet on a personal level, he would have been in great tension at the prospect of a meeting, in a sort of irrational fear of not being considered by the great man as worthy of his own greatness. This testimony was so contrary to what I myself had seen in my young friend, that I found it hard to believe (this was in 1973). In retrospect, it

<sup>42</sup>(\*) (May 10) In fact, another "very clear" sign dates back to 1966, see footnote (\*) to note no. ° 82 (p. 329).

However, the signs of division that I'm aware of elsewhere all point in the same direction.

This division, and the role I played as a sort of fixer of a conflict that was undoubtedly diffuse before our meeting, would probably have remained hidden in the usual circumstances of the evolution of a relationship with someone who was (in one sense or another) a "master", or at least someone who transmits or confides. In this way, my departure will have **revealed** a conflict unknown to all, and which perhaps only I know about.

And my "return" today is a second, more untimely revelation. I can't imagine what it will reveal to me, beyond what it has already taught me about my own past and present, and about the people I have loved and to whom I am still linked today. Nor what it will reveal to the person who, for the past week, has been at the center of this final stage of my reflection, which I called last month (and I didn't think I was saying it so well. . . ) **"the weight of a past"**.

## 14.2.10. Two revolving

**Note 66** (April 25) This deliberate disdain and antagonism in my friend Pierre's relationship with me has been confined exclusively to the mathematical and professional level. The personal relationship has remained to this day one of affection and friendly respect, manifested more than once by delicate attentions that have touched me, surely signs of genuine feelings without ulterior motive.

In the intense years that followed my departure from the IHES, this episode faded into oblivion, as did the long misunderstood teaching it had given me. So, for more than ten years, my friend remained for me (as a matter of course) my privileged interlocutor in mathematics.

tics; or to be more precise, between 1970 and 1981 he was the only interlocutor (apart from one episode) with whom I consider addressing me during periods of my sporadic mathematical activity, when the need for a p. 247 the interlocutor was felt.

It was also to him, as the mathematician closest to me, that I turned just as spontaneously on the first occasions (between 1975 and 1978) when I had to ask for assistance, surety or support for students working with me. The first of these occasions was the defense of Mrs. Sinh's thesis in 1975, which she had prepared in Vietnam under exceptionally difficult conditions. He was the first person I contacted to sit on the thesis jury. He declined, suggesting that it could only be a bogus thesis, and that he had no intention of endorsing it. (I did, however, have the skill to circumvent the good faith of Cartan, Schwartz, Deny and Zisman to lend me a hand in this deception - and the defense took place in an atmosphere of interest and warm sympathy). It took three or four similar experiences over the next three years before I finally understood that my prestigious and influential friend was deliberately antagonistic towards my "post-1970" students, as well as towards work that bore only the stamp of my influence (at least that undertaken "after 1970"). I don't know whether the attitudes of overt contempt that I witnessed on several of these occasions are also to be found to a greater or lesser extent in his relationship with other mathematicians whom he considers to be far below him. The very spirit of a certain elitism he prides himself on professing would lead me to suppose so. The fact remains that since 1978 I have refrained from addressing him on any matter whatsoever. This has not prevented his power to discourage from manifesting itself effectively.

It was also around the same year that the first signs, discreet at first, of an attitude of disdain towards my own mathematical activity appeared. The first occasion had been my reflection on cellular maps, after a discovery about them that had flabbergasted me (see *Esquisse d'un Programme*,

par. 3: "Bodies of numbers associated with a child's drawing"). This discovery (admittedly "trivial", and which had nothing to move or even interest my prestigious friend) was the starting point and first material for that other mathematical **dream**, of comparable dimensions to that of the motifs, which began to take shape only three years later (January-June 1981), with "La Longue Marche à travers la théorie de Galois". These notes and others from the same period (some two thousand handwritten pages) constitute a very early version of his work.

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tour through this "new continent" that a trivial remark on a child's drawing had given me a glimpse of.

□ In the course of this intense work, I wrote two or three times to my friend, to tell him about some of my ideas, and occasionally ask him questions of a technical nature. When it pleased him to speak to my questions, his comments were always as clear and as pertinent, and bore witness to the same "means" that had impressed me even at his young age. But a smugness had dulled the eagerness to understand that had enchanted me then, and the ability to apprehend great things through "small" things, as well as to apprehend or conceive great designs, by listening to each other. This ability is not a matter of intellect, of simple "efficiency", or of "mastery" of an already established discipline or known techniques. It's a reflection, at the level of the intellect, of something of an entirely different essence - of the child's **gift of wonder**. This gift in him seemed extinguished, as if it had never been. It was so at least in his relationship with me, after it had been so first in his relationship with my "later" students. He had become an important man, and his approach to mathematics had become neither more nor less than that "sporting" attitude which I first examined only a month or two ago, and to which I myself was by no means a stranger. ...

Perhaps I would have been able to come to terms with the obvious absence of this communion in a shared passion, this deep bond that had once bound us together. I would no doubt have been content to submit (when the opportunity arose) more or less technical questions or simple requests for information to my friend's astuteness and vast knowledge of the world of mathematical things. But in that year (1981) the signs of this disdainful affection suddenly became so brutal<sup>43</sup> (\*), that I lost all interest in communicating with him again on mathematical matters, even occasionally. (⇒ 67 )

### 14.2.11. The table shaved

**Note 67** (April 26) It was while I was writing the preceding lines yesterday that I made the connection between this new turning point in our relations and the publication in 1982 (practically at the same time as this dramatic turnaround) of the "remarkable volume" of Lecture Notes, which consecrated my mathematical funeral without flowers or wreaths! At a time when I had been declared mathematically "dead", it was a kind of grace for my friend to continue answering mathematical questions here and there, which, in the end, were still valid, were no longer relevant...

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□ Essaying to tune into the meaning of events, I get the feeling that this isn't a ha- either. sard si la première apparition d'un dédain, d'un désintérêt mathématique (vis à vis de choses, de plus, dont son "sain instinct" mathématique devait lui dire qu'elles étaient brûnantes et ju juuuses), dans sa relation à ma propre personne tout au moins, se place à peu près le moment de la parution du volume de pré-enterrement  $SGA_2 4^1$ , cinq ans avant<sup>44</sup> (\*). The circumstances surrounding the publication of this volume already bear witness

<sup>43</sup>(\*) (May 28) For new insights into this second turning point, see also the note "La Perversité", n° 76.

<sup>44</sup>(\*) On this subject, see the note "Le compère" (n° 63" ) of the day before this one.

(June 5) The reflections of this note are taken up in this note and the three that follow ("La table rase", "L'être à part", "Le feu vert", "Le renversement"), which hint at the meaning of "operation  $SGA_2 4^1$ " and its link to the "dismantling" of the mother seminary SGA 5. This reflection is taken up again in the "My students" procession, and in particular in the continuation "My students (1)-(7)", where little by little the picture emerges of a veritable massacre of the seminary where my cohomology

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The fact that I was introduced as Deligne's "collaborator", without consulting me or even informing me, and without sending me a copy, is in itself more eloquent. The mere fact of introducing me as Deligne's "collaborator", without deigning to consult me or even inform me, and without even sending me a copy, seems to me in itself more eloquent than a speech. Not to mention the fact that Deligne's book was essentially intended to make more accessible to a wider public the work I had developed over fifteen years earlier, at a time when I had not yet heard the name of my brilliant friend! This disdain, and later arrogance, must have been fuelled, on the one hand, by my absenteeism, which meant that I wasn't aware of anything and was, in fact, "cashing in" without knowing it; but also by a certain climate, which meant that this kind of misunderstanding could "pass", without apparently eliciting the slightest comment. The fact remains that I have not received a single echo from anyone (particularly among the many friends I had thought I still had in the world of mathematicians) about this volume, nor about the burial volume it has prepared.

□ In the introduction, the author doesn't beat about the bush to set the scene. The aim of the volume is to spare the non-expert "recourse to the lengthy presentations of SGA 4 and SGA 5", "to prune out unnecessary details", "to enable the user to forget SGA 5, which can be considered as a series of digressions, some of them very interesting" (how nice of these "digressions"! ). The existence of SGA 4<sup>1</sup> "will soon make it possible to publish SGA 5 as it stands" - a mysterious assertion, since one wonders how this publication (of something one is advised to forget), which had already dragged on for a dozen years, and which presented a perfectly coherent set of results (and which had not waited for Deligne to be identified and proven) could be subordinated to the existence of SGA 4<sup>1</sup> (\*).

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In asking this question, I also see a simple answer, and a possible explanation for the vicissitudes of this poor seminar SGA 5 (68), (which I had developed at length in 1965/66, eleven years before the publication of Deligne's volume SGA 4<sup>1</sup><sub>2</sub><sup>45</sup> (\*). The first hint of this can already be seen when it is stated (page 2) that in the original version of SGA 5 "the Lefschetz-Verdier formula was established only conjecturally" (which is harsh for Verdier, who is supposed to have been able to prove his theorem, which predates SGA 5<sup>46</sup> (\*\*)) and that "moreover, the local terms were not calculated". This may seem an unfortunate omission for the non-expert reader (for whom this volume is primarily intended). Readers with a bit of experience know that the said local terms are still not "calculated" today, and that the brilliant and peremptory author himself would be at a loss if asked what he meant in this case (in the general case) by "calculating"<sup>47</sup> (\*\*\*) (but apparently nobody thought of asking him this indiscreet question).

An ambiguous sentence "this seminar (?) contains another demonstration, it completes, in the case particular Frobenius morphism", seems to suggest that SGA 5 does not give (as one might have expected, for

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In all this, there's a casual contempt, of which the "discreet disdain" (which I saw appear around the same time), in my friend's relationship with me, was only a very pale reflection.

Another association came to me a week or two ago, for the moment of that "first turning point" in my friend's relationship with me, at the end of 1977 or during 1978. It was in 1978 that my friend got his well-deserved "medal" (for proving Weil's conjecture). The way in which this new title (linked to the demonstration of a conjecture "of proverbial difficulty") was internalized by my friend, is strikingly apparent in the Funeral Eulogy (concerning my late self) and its counter- Eulogy (concerning my late self).

part (concerning his own), published admittedly only five years later on a "grand occasion". See note "L'Eloge Funèbre (1) - ou les compliments", n° 104.

<sup>45</sup>(\*) See a footnote (April 28) to the note "Le feu vert" (n° 68) for an elucidation of this "mystery".

<sup>46</sup>(\*\*) (June 10) For further details, see sub-note n° (87) to "The massacre" note n° 87.

<sup>47</sup>(\*\*\*) (June 10) In the general Lefschetz-Verdier formula, for a cohomological correspondence between a bundle of coefficients and itself, the "local terms" (corresponding to the related components of the set of fixed points) are unambiguously defined by the very fact of writing the formula. The question of "calculating" these local terms only takes on a precise meaning in special cases, one of the simplest of which is that of the Frobenius morphism, where they are given simply by the ordinary traces of the endomorphisms induced on the fibres at these points. This formula was fully demonstrated in the oral seminar as a special case of a much more general one.

a volume of digressions!), at the end of the ends, a complete demonstration of the main "result" it announces, a trace formula implying the rationality of  $L$  functions à la Weil; fortunately, "this seminar" comes to save, better late than never, a very compromised situation. ...

On page 4, we learn that the aim of the "Arcata" lectures was "to give demonstrations of the fundamental theorems in stale cohomology, rid of the gangue of nonsense"<sup>48</sup> (\*) that surrounds them in SGA 4". He has the charity not to dwell on this regrettable nonsense that is rife in SGA 4 (such as topos and other similar horrors - the reader can flatter himself that he has escaped it by the providential appearance of this brilliant volume, finally making a clean sweep of the regrettable "gangue" that had preceded it. ... ) (67) (67<sub>1</sub>).

As I've just gone through the introduction to the volume and the introductions to its various chapters, I've re-produced the assessments and declarations of intent that seem to me to most clearly announce the color, among two or three others (style: digressions, admittedly, but "very interesting") that seem to me intended above all to "pass the pill" (which has indeed passed without a problem). For example, the author is honest enough to state at the outset that "for complete results and detailed demonstrations, SGA 4 remains indispensable". This volume, however ambiguous in spirit and motivation, is not a scam<sup>49</sup> (\*\*). Its role seems to me to be more that of a sounding board, obviously conclusive: there was really no need to bother!

p. 252 □ There's a kind of **escalation in absurdity** (seemingly unnoticed by all!) from one volume to the next. prepares (APG 4<sub>2</sub><sup>1</sup>, and LN 900). In both cases, we see a man of impressive means, made for discovering and exploring and probing vast worlds, set about "redoing" the work of a predecessor, first myself, then a former pupil of mine (Saavedra), when in so doing he had nothing essential to contribute to the work of these predecessors, which had been done with care and by getting to the bottom of things. (What he contributed in total could be set out in some twenty or thirty pages, it seems to me.). In the first case, the reason given was plausible: to give the non-expert user tear-free access to<sup>50</sup> (\*), without having to rely on the voluminous SGA 4 and SGA 5 seminars. (It's the first time, however, that we've seen the author show such concern for the common man, taking precedence here over the pleasure of doing math. ... ) The second time, the work consisted almost entirely of **copying** the thesis Saavedra had done with me! This thesis was a perfect reference, and the fact that the demonstration of one statement in it was false, and that another statement contained an unnecessary hypothesis, was surely no reason to rewrite the whole article. Of course, no "reason" was given for such a strange thing.

<sup>48</sup>(\*) In my day, the term "general non-sense" did not have a pejorative connotation, but rather a slightly jokey, good-natured one. It's no coincidence that the adjective "general" has been "forgotten" here, to mean "non-sensé", which in good French means neither more nor less than non-sens, and suggests the idea of bombast, of "bullshit".

<sup>49</sup>(\*\*) (May 26) See, however, the following day's note, "Le renversement" (n° 68'), where I go back over this impression, which turns out to have been hasty= In further reflection, a large-scale "SGA 4<sup>1</sup> - SGA 5" operation was gradually revealed, for the "benefi ce" mainly of Deligne, with the help or tacit agreement of all my "cohomologist" students, "The honesty" that I believe I can observe (on the strength of the statement, in line 7 of the introduction, which has just been quoted), plays here the role of

the "punch line" designed to give the impression of a "thumb". My friend used this style as early as 1968 (see "Poids en conserve et douze ans de secret", and "L'éviction", notes n° 49 and 63). See also the notes "Pouce!" and "La robe de l'Empereur de Chine", n° 77 and 77'.

<sup>50</sup>(\*) (June 10) When I wrote this note, I had only just "landed" and hadn't yet grasped the true meaning of "operation APG 4<sup>1</sup>" (and its link with the vicissitudes of SGA 5, of which I had only just had a sudden foreknowledge). I've since realized that the motley collection of texts published under the misleading name of SGA 4<sup>1</sup> (see the note "Le renversement", n° 68') is in no way intended as a popularization ("without tears") of the SGA 4 and SGA 5 seminar (which forms the core of my "SGA"). published mathematical work), but that it represents a manoeuvre to replace it (acting as a precursor that's a bit muddy around the edges), and to appear as the true masterwork on stale cohomology, which would be due to Deligne. For a striking formulation (by an anonymous writer) of such an imposture, six years after the "coup de sonde" named SGA 4<sup>1</sup>, see "L'Eloge Funèbre (1) - ou les compliments" (note n° 104).

Yet I didn't have to hold SGA 4<sup>1</sup> in my hands to feel the meaning of this seemingly absurd thing: Deligne "redoing" Saavedra's thesis, ten years later! It's surely the same as the meaning of that scarcely less absurd thing that had prepared it: Deligne doing (twelve years later) a "digest" (a little condescending around the edges), of a certain part of Grothendieck's published work. This is precisely the part of Grothendieck's work he can't pretend to do without, if he continues to be interested in the cohomology of algebraic varieties (from which he can't seem to detach himself). And Saavedra's thesis is the work of all, published and bearing the mark of my influence, which he can in no way do without, if he wants to take up "on his own account" the notion of motivic Galois group that I had developed, and finally exploit (fifteen years later!) this obviously crucial notion. First with SGA 4<sup>1</sup>, and five years later with the landmark Milne-Deligne (alias Saavedra) article in LN 900, my friend indulged in an illusory sense of liberation from something he surely felt a painful obligation to do: to have to constantly refer to the very person he was trying to supplant and deny, or even to such and such another who referred to him.

To arrive at this intimate conviction about the common meaning of these two "absurd" acts, I didn't need to go through all my prolific friend's (fifty-one) publications, a list of which I received (for the first time) about ten days ago. To tell the truth, I haven't even thought of going back through the four offprints in my possession<sup>51</sup> (\*), to seek confirmation of what I think I know. If, in the future, I consult any of my friend's works, it will be to find something other than what I already know. I'm sure I'll then have the pleasure of learning beautiful mathematical things, which I used to have the even greater pleasure of learning in person from him!

**Note** 67<sub>1</sub> (1) (June 14) I found two other micro-crooks (of detail) in SGA 4<sup>1</sup>. One is in the "Breadcrumb trail for SGA 4, SGA 4<sup>1</sup>, SGA 5" (admire the suggestive sequence!), where the author writes (p. 2) that to establish in étale cohomology a "duality formalism analogous to that of coherent duality. ... Grothendieck used the resolution of singularities and the purity conjecture", giving the impression that this formalism was ultimately established only by him, Deligne, in the case (sufficient for many applications) of finite-type schemes on a regular scheme of dimension 0 or 1 (see same paragraph). He knows full well that the formalism of the six variances (i.e. the theory of global duality) was established by me without any "conjecture", and that his restriction is only founded for the biduality (or "local duality") theorem - which, by the way, becomes in SGA 5 (under Illusie's pen) "Deligne's theorem"!

In addition, on page 100 there is a section entitled "The Nielsen-Wecken method", which is the method of which I introduced into algebraic geometry to prove a Nielsen-Wecken-type formula, proven by these authors (in the transcendental context) by a technique of triangulations unusable in the context algebraic. Deligne learned about this method (as well as the names of Messrs Nielsen and Wecken, whose fine German article he didn't need to read!) from me, in the SGA 5 seminar of "technical digressions", which SGA 4<sup>1</sup> is designed to make us forget! In this section, neither SGA 5 nor I are alluded to, and the reader has the choice, for the authorship of this method, between Nielsen-Wecken (if he's very misinformed) and the brilliant, modest author of the volume.

Interestingly, in this entire volume, Verdier's "Woodshoie" proof for a trace formula including the case I needed (for Frobenius morphisms) is not mentioned. This demonstration (apparently forgotten, in favor of the more general method developed in SGA 5) was the missing link to fully justify my cohomological interpretation of the functions

<sup>51</sup>(\*) Not counting the works in the IHES Mathematical Publications, which director Nico Kuiper has been kind enough to send me for nearly fifteen years.



L. Clearly, there was an agreement (tacit, no doubt) between Deligne and Verdier - Verdier giving Deligne credit for the trace formula for Weil's conjectures, in return for the part of SGA 5 he had taken over on his own account the previous year (1976). (See note "Les bonnes références" n° 82.) Other compensation: the appearance in SGA 4<sup>1</sup> of the "Etat 0" of derived and triangulated categories, from which my name is equally absent. Four years later, under Deligne's pen, the duality of algebraic geometry took on the name of "Verdier's duality" - Verdier had not done a bad job! (See the end of note n° 75 "L' Iniquité - ou Le sens d'un retour").

### 14.2.12. The being at part

**Note 67'** (May 27)<sup>52</sup> (\*) The passages quoted, like all the circumstances surrounding the publication of this remarkable volume, SGA 4<sup>1</sup>, testify to my friend's deliberate intention to divert and scorn the central part of my work, represented by the two interrelated seminars SGA 4 and SGA 5. Not the least of these "circumstances", which came to light in the course of reflection from April 24 (see the note "Le compère", n° 63) to May 18 (see the notes "La dépouille. . .", ". . . et le corps", n° 88, 89), was the ransacking of the original SGA 5 seminar, which took the form of the 1977 edition-massacre (see in particular the note "Le massacre", n° 87).

p. 255 This deliberate attempt at derision on my friend's part takes on its full meaning if we remember that the SGA 5 oral seminar represented the young  $\square$ homme Deligne's first contact with schemas, cohomological techniques

and in particular the duality formalism, and with  $\chi$ -adic cohomology, when he arrived at the IHES in 1965 at the age of 21, with the specific aim of learning "algebraic geometry" with me. It was in this oral seminar, and in the notes of the SGA 4 seminar that had taken place two years before, that he had the privilege of learning first-hand the ideas and techniques that have dominated his work to this day<sup>53</sup> (\*).

This essential aspect of the context of the "SGA 4<sup>1</sup> - SGA 5 operation", and beyond it, of the relationship between the company and its customers.

even from my friend Pierre to myself, was clearly not present when I wrote the previous note ("La table rase (1)", n° 67), nor in the part of the reflection on the Burial that precedes it. The memory of this "young man Deligne", arriving at the SGA 5 seminar where he still had everything to learn and where he did indeed (and very quickly) learn a lot, only came back in the last stages of the reflection, as if against my will. My deliberate intention, from the very year of young Deligne's appearance in my mathematical "microcosm", not to count him among my students (as if by doing so I would have failed in my obligation of modesty towards such a brilliantly gifted person), made me downplay, or to put it more accurately, totally ignore until these very last weeks, a reality that is nonetheless obvious and tangible, and which is commonly expressed by the double appellation (which I objected to) of "teacher-student"<sup>54</sup> (\*\*). I was happy to forget, to ignore, that there had indeed been a "transmission" of something from me to him, something that for me as for him had great **value**, in a sense that was surely quite different for him and for me. What I was passing on, in those four years of close mathematical contact between him and me, was something in which I had put the best of myself, something nourished by my own experience.

<sup>52</sup>(\*) This note is a footnote to the previous note "La table rase", of which it is a complement, written one month later to the day.

<sup>53</sup>(\*) A similar comment can be made for each of my other cohomologist students Verdier, Illusie, Berthelot, Jouanolou - see the note "Solidarity", and the four notes that follow it (notes n° 85 to 89).

<sup>54</sup>(\*\*) (June 14) This deliberateness is quite apparent in the way I finally resolve to talk about him (as if in so doing I was violating an obligation of reserve or modesty towards someone who liked to distance himself from me... ) four months ago, in the note "Jesus and the twelve apostles" n° 19.

of my strength and my love - something that (I think) I gave without reserve and without really measuring or even, perhaps, feeling the price.

Surely, what I was giving was fodder for a passion to know in him ☐ in tune with the one that ani- me. mait - and to **something else** too, which I didn't feel until much later, and without yet linking it to this "transmission" that had taken place and which I was happy to ignore. To put it another way, what I gave was **also** received, at another level that remained hidden from me, not as tools to fathom a fascinating and inexhaustible Unknown, but as **instruments** to supplant (at first), and later to establish a domination, a ruthless "superiority" over others.

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Without even taking into account what came back to the "child" in my friend, eager to discover, and what came back to the "boss" in him, eager to supplant, dominate (or even crush), but from the more superficial point of view of the part certain ideas play in a work, techniques, tools - it's been an unexpected discovery over the past six weeks to what extent my friend's work, which took off the year we met, has been nourished to this day by what I'd passed on to him. I had imagined, when I left the mathematical scene fifteen years ago, that "the little" I had given my friend-non-student (a "little" whose role in his impressive initial impetus I could see clearly) would be a springboard for a flight that would take him far beyond his starting point, **away from** my work and my person. What happened, however, was that my friend remained attached to this point of departure to this day, **attached** to the very work that was simultaneously to be disowned, derided or forgotten, and "used". It's a typical case of a conflicted link to one's father or mother, which indefinitely holds one in the orbit of those he or she is destined to leave and surpass, the one who takes pleasure in cultivating this conflict within him or herself, instead of launching out to meet the world. ...

I can see today that by my deliberate intention to treat my young friend as a "being apart", and not simply as one of my students who seemed to have more means than the others - and by my deliberate intention to minimize or forget in my relationship with him the price of what I was transmitting (and the **power** that I was thereby placing in his young hands . ) - through these attitudes within myself, I was unwittingly feeding a fatuity and a conflict within him, both of which remained hidden from me. At the same time, I was entering into a certain game - or rather, there was a game between the two of us in perfect harmony, and I'd be hard pressed to say who "started it" (assuming the question makes any sense): myself out of "modesty", claiming that my young friend was far too bright to be anyone's pupil, and that the little I'd been able to bring him wasn't really worth the trouble.

to talk about it - and himself distancing himself (even before I left) from my person and my work, disowning (under my <sup>com</sup>☐plaisant eye) the soil that had well and truly nurtured it.

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It's only by writing this note that I'm finally seeing clearly this game, of which a diffuse perception had only been present for a week or two. And I also see that this "modesty" or "humility" in me was a false modesty, a false humility: a lack of simplicity, to see things simply for what they are. In this game, there was complacency towards my young friend - seed that proliferated a hundredfold! - and, more subtly, an indulgence in myself, by making a kind of pedestal of a "privileged relationship", extraordinary and all<sup>55</sup> (\*). (Just as any lack of simplicity, perhaps, is basically a self-indulgence... .)

<sup>55</sup>(\*) Compare with the note of May 10 "L'ascension" (n° 63') where for the first time I perceive this ingredient of complacency in what was my relationship with my friend Pierre. This perception had remained isolated and fragmentary until now, when it has been which is the subject of this note "L'être à part".

## 14.2.13. The green light

**Note** 68 (April 27) To tell the truth, I've never given much thought to the meaning behind the strange vicissitudes of the SGA 5 seminar. The oral proceedings in 1965/66 had not given rise to any particular difficulties, whereas the drafting by successive and often failing volunteers dragged on for **eleven years**<sup>56</sup> (\*\*)! It was in 1976 that Illusie finally took matters into its own hands, writing up what was left over and publishing the whole thing. Today is the first time (after almost twenty years since that seminar) that I realize "there's something to understand". Maybe I'm the only one...

p. 258 The first idea that comes to mind is that among the seminar's more or less active listeners, who were also more or less familiar with the previous seminars SGA 1 to SGA 4, there must have been a phenomenon of **satur- ration in** relation to the tide of "grothendieckeries", breaking over them like a sort of tidal wave without reply<sup>57</sup> (\*\*\*). Clearly, some of the editors lacked faith, and must not have sensed this very well. where it was all going, and why on earth I'd been so stubborn, for a whole year, to want to turn it around and around until I'd completely mastered the <sup>es- sential</sup> formal properties of the cohomology, and the whole arsenal of new notions associated with it. The fact that no trace remains of either the seminar's final lecture, setting out open problems and conjectures (never published to my knowledge), or the introductory lecture reviewing Euler-Poincaré and Lefschetz-type formulas in various contexts, is a particularly eloquent sign of a general disaffection. I don't recall perceiving this disaffection at the time (or even afterwards, until today<sup>58</sup> (\*)), as I was so engrossed in my tasks at the time.

The fate of SGA 5, which originally had as strong a **unity** as any of my other seminars, and which was gradually **dismantled** (68 ) in the eleven years that followed without being written up, could have shown me that the great projects I had so doggedly pursued, and for which I had for some years found arms to assist me, had by no means become a joint undertaking, but remained personal to me. My program gave rise here and there to occasional collaborations, but failed to become a driving force in any of my students at the time - a force that would have inspired him to work on a longer-term, more far-reaching project than the one he had pursued with me in his thesis, whose main role in his life would have been to help him learn the mathematical profession he had chosen.

p. 259 The only one, it seems to me, to have grasped as a whole (if not made his own) a certain overall vision, going beyond the framework of a particular "collaboration" on such and such a question or for the development of such and such a particular tool, was Deligne. This is why I must surely have seen in him (without ever having to formulate it) much more a designated "heir" than a "pupil". The term "heir" here better captures what I'm trying to express than the term "continuator", which came to mind at first, but which might suggest the idea of a "successor". of a work that would be limited by a received inheritance. On the contrary, I felt this "inheritance" to be a simple **contribution** I was in a position to make □ for the deployment of a personal vision, which would be nourished

<sup>56</sup>(\*\*) Writing the whole seminar, based on my detailed notes for the oral presentations, would have taken me just a few months.

<sup>57</sup>(\*\*\*) This goes hand in hand with the impression of students who remained "a little dumbfounded", expressed in the letter quoted in the note "Teaching failure (2) - or creation and fatuity" (n° 44').

<sup>58</sup>(\*) (May 26) After getting back into the swing of things at the SGA 5 seminar, I remembered an impression I'd had for some time. of unease I'd had, when I leafed through (it must have been 1977, the year of its publication) the copy of the published seminar I'd just received. This impression of "mutilation" (which then remained in a diffuse, informal form) was due above all, perhaps even entirely (I must not have spent much time looking more closely, although it would have been well worth it. . . ), to the absence of the introductory and final presentations, and above all (I think) to the casualness with which this absence was announced, as something almost taken for granted - why on earth would anyone have bothered to include them! At some level, I must have "sensed something", which I only took the trouble to let rise and examine this month (almost seven years later!), in the note "Le massacre" and in the two notes "La dépouille. . . ", ". . . and the body" that follow it.

of many other contributions (as was indeed the case even before my departure), and which was destined to effortlessly surpass all that had preceded and nourished it.

Returning to the sad fate of SGA 5, the thought that occurred to me yesterday was that this fate was perhaps not unconnected with the ambiguity of Deligne's relationship with me and my work, particularly given the as- cendant that his strong mathematical personality could not fail to exert on all my students<sup>59</sup> (\*). I'm sure he must have found some inner satisfaction in the vicissitudes that affected the notes of this seminar, stripped of what made up the unity and impetus of the oral seminar. On reflection, however, it is clear that the primary and es- sential cause of these vicissitudes does **not lie** in the dispositions of a single participant. Without yet clearly discerning this cause, there is no doubt that it concerns above all myself **and** the people who pretended in 65/66 to take charge of editing the seminar. Surely it lies in their relationship to my person, or perhaps also in their relationship to a certain way of doing mathematics (or a certain program, or a certain vision of things) that I embodied for them. The fate of SGA 5 now seems to me to be an eloquent and tenacious **revelation** of something I've never yet taken the trouble to examine, for want of even realizing it, and which even now I'm only glimpsing<sup>60</sup> (\*\*). Perhaps these lines will encourage some of the protagonists of this collective misadventure to share their own impressions with me.

□ Perhaps there is a lesson, however (at least a provisional one) that I can draw from the episode right now SGA 5, which first prefigured, and then illustrated, this spectacular **halt** after my departure, on almost the entire line, from the famous "program" in which I was embarked. Contrary to what I must have more or less believed in the euphoric sixties (happy as I was to have finally found some goodwill to back me up!), it seems to me today that the concretization of a vast per- sonal vision through tenacious and meticulous work cannot be in the nature of an adventure or a **collective** undertaking. Or rather, if there is such a thing as a "collective undertaking", it's not one that would be achieved through ten or twenty (or even thirty) years' work around a single person. If the vision is to become a common heritage for all, it will be embodied here and there under the pressure of needs alone, through the day-to-day work of this or that other person who may only know the predecessor by name (and even then!), whose vision had been too vast for his arms alone to be enough to bring it to fruition<sup>61</sup> (\*)

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<sup>59</sup>(\*) (April 28) An eloquent sign of this ascendancy is that SGA 5 was only published when Deligne saw fit to signal to Illusie to take an active interest in it - in other words, at the **precise moment** when he himself needed it as the basic text for his "digest" SGA 4<sup>1</sup>, destined to replace it. (See the end of the introduction to SGA 5, written by Illusie.) This sheds light on and gives full meaning to this statement (which I still described as "mysterious" only yesterday) in the "Table rase" note (note n° 67)), that "the existence of SGA 4<sup>1</sup> will soon enable us to publish SGA 5 as is". The "tel: quel" here is a touch of humor that I was probably the only one to sense (as early as the day before yesterday), and to appreciate at its true value! (Seen on "dismantling" that the published version represents in relation to the original seminar).

<sup>60</sup>(\*\*) (May 26) This is the very "something" referred to in the penultimate footnote, which has come to the surface in the course of reflection over the past few weeks, and especially since the moment (May 12) when I took the trouble, for the first time since its publication, to take a closer look at what had become of "a splendid seminar" in the hands of my cohomology students, in the massacre-edition that had been published, for the first time since its publication in 1977, to take a closer look at what had become of "a splendid seminar" in the hands of my cohomology students, in the massacre edition that was made eleven years later.

<sup>61</sup>(\*) (April 28) Perhaps "my arms alone" would have been enough to carry out the vast program of work I had in mind towards the end of the sixties, but only if I had made myself the exclusive servant of that program for the next twenty or thirty years. Today, I'm glad I didn't follow that path, which could have been mine, but whose pitfalls and dangers I now clearly see.

## 14.2.14. The reversal

**Note!** 68' (April 28) As an example (among many others<sup>62</sup> (\*\*)) of this dismantling, I thought back to the fate of one of the key presentations in SGA 5, which ended up being written by none other than Deligne (who I believe had taken charge of it as early as 1965, to "keep" his commitment eleven years later. . . ) according to my oral presentation, only to be incorporated without further ado into SGA 4<sup>1</sup> ! This is the formalism of the cohomology class associated with an algebraic cycle on a regular scheme, which develops with ease by passing to "sup-ports" cohomology in the support of the cycle under consideration. Like almost all constructions in stale cohomology (useful also in many other contexts, where they have become common practice), I had developed this one at the end of the fifties in the framework of coherent cohomology (here, Hodge and

p. 261 De Rham, which, in the context of "abstract" algebraic geometry, are first studied in one of my early Bourbaki lectures). It is so <sup>natural</sup> that it self-evidently implies the compatibility usual with cup-products<sup>63</sup> (\*).

As I write these lines, I realize that the sleight of hand used to include this crucial talk in SGA 4<sup>1</sup> has led to the brilliant result that Deligne, who did take part in the SGA 5 seminar in 65/66<sup>64</sup> (\*\*), **does not appear** on the cover as one of my "collaborators" (something that had already struck me yesterday, while leafing through the published volume Lecture Notes n° 589) and that I am the one entitled (eleven years after the seminar) to be listed as Deligne's "collaborator". It's quite a **reversal of fortune**, I must say! At the time of publication of SGA 4<sup>1</sup>, to which I had unknowingly contributed, I had stopped all public mathematical activity for seven years - so much so, in fact, that I never bothered with the publication of poor SGA 5, which for me was part of a past I'd left behind... .

(April 30) As for SGA 5, it now appears to be a rather motley collection of texts with no tail or head (they got lost along the way!), and which only "stand up" with reference to the text APG 4<sup>1</sup><sub>2</sub>. Remarkably, and something I've only just noticed, the very name SGA 4<sup>1</sup> actually suggests that this text **precedes SGA 5, which exists only by reference to it**<sup>65</sup> (\*\*\*). If the author of this text had been less ambiguous<sup>66</sup> (\*), and for sentimental reasons wished to insert his "digest" ("plus a few new results") into the SGA series in which he had played his part, the obvious name would have been SGA 5 1/5.

I see this as a second sleight of hand, which makes me realize that Deligne's share of SGA 5's fate is heavier than I thought even three days ago. It also brings me back to the feeling

<sup>62</sup>(\*\*) (May 28) I didn't get round to this "dismantling" until the May 12 reflection, in the note (more appropriately named) "The Massacre" (n° 87).

<sup>63</sup>(\*) (May 28) In the coherent framework, see my Bourbaki lecture no. ° 49 (May 1957), § 40 In the note "Les bonnes références" (no. ° 82) of May 8, I discovered that these ideas, as well as those I had developed in the same SSA 5 seminar for the of homology associated with cycles (and many others) were taken up by J.L. Verdier, without a word about the existence of an SGA 5 seminar or about myself. This operation took place in 1976, a year before the "SGA 4 operation<sup>1</sup>" (with which it seems to me to be closely associated), and in full view of all the ex-auditors and participants of the 1965/66 SGA 5 mother seminar.

<sup>64</sup>(\*\*) (May 28) And it was even there that he first heard of the things he so brilliantly exposes in the SGA 4 pirate-volume<sup>1</sup> ! On this subject, see yesterday's note "L'être à part" (n° 67' ). Compared to the methods used by his friend Verdier the year before, and to those he himself has used on other occasions, my friend here nevertheless stays below the line patent plundering, since he presents me as the author of the paper on cycles (with, admittedly, the brilliant result of being able to present me as his collaborator), and doesn't even pretend to be unaware that I've had something to do with the theory of stellar cohomology, the formula of traces, and so on. For a decisive step in this direction, see however, the note "L'Eloge Funèbre (1) - ou les compliments" (n° 104).

<sup>65</sup>(\*\*\*) (May 28) For a deeper meaning of this "violent insertion" of SGA 4<sup>1</sup> between the two indissoluble parts SGA 4 and SGA 5 at one end, forming the heart of my written work, see the note "La dépouille... ." (n° 88).

<sup>66</sup>(\*) (May 28) The expression "ambiguous provisions" is definitely an understatement here!

expressed the day before, that SGA 4<sup>1</sup> was not a scam operation. If apparently nobody (starting with Illusie, whose good faith is certainly not in question<sup>67</sup> (\*\*)) noticed the "operation", this is undoubtedly due to this "ascendancy" that I've already noticed, and also I think to the charm of my friend's person, both of which place him above suspicion!

## 14.2.15. Squaring the circle

**Note 69** (April 27) Around the age of eleven or twelve, when I was interned in the Rieucros concentration camp (near Mende), I discovered compass drawing games, enchanted in particular by the six-pointed rosettes obtained by dividing the circumference into six equal parts using the opening of the compass.

compass transferred to the circumference six times, causing it to fall right back on the starting point. This experimental finding had convinced me that the length of the circumference was exactly equal to **sixp** times that of the shelf. When, later on (at the lycée in Mende, I think, where I ended up going), I saw in a textbook that the relationship was supposed to be much more complicated, that we had  $l = 2\pi R$  with  $\pi = 3.14 \dots$ . I was convinced that the book was wrong, that the authors of the book (and no doubt those who had preceded them since antiquity!) must never have drawn this very simple line, which clearly showed that we simply had  $\pi = 3$ . Typically, I realized my error (which consisted in confusing the length of an arc with the length of the rope that joins the ends) when I expressed my astonishment at my predecessors' ignorance to someone else (an inmate, Maria, who had given me some voluntary private lessons in maths and French), just as I was about to show her why we should have  $l = 6R$ .

The confidence a child can have in his or her own abilities, rather than taking things learned at school or read in books at face value, is a precious thing. Yet it is constantly discouraged by those around us. Many will see in the experience I relate here an example of childish presumption, which had to bow to received knowledge - with the facts finally revealing a certain ridiculousness. As I experienced this episode, however, there was no sense of disappointment or ridicule, but rather that of a new discovery (after the one I had hastily interpreted by the false formula  $\pi = 3$ ): that of an error, and at the same time that we should have  $\pi > 3$ , because obviously the length of an arc is **greater** than that of the string joining the two ends. This inequality was well in line with the rejected formula  $\pi = 3.14 \dots$  which, as it turned out, looked reasonable, while at the same time I had to admit that there might be some not-so-idiots out there who had looked into the matter. At that point, my curiosity was satisfied, and I don't remember wanting to find out more about the ins and outs of this all-important number,

<sup>67</sup>(\*\*) It's high time we took this opportunity to thank Luc Illusie for the care and self-sacrifice with which he saw to the successful completion of a number of distressed presentations and the publication of the "package"; and this under conditions that were certainly not the least encouraging, not least of which was my total absenteeism!

(May 26) In the light of subsequent reflections, pursued in notes n° 84 to 89 and especially in the note "The Massacre", these thanks lavished on Illusie take on an enormous and unforeseen comic dimension, which I was far from expecting. I had no idea when I wrote these lines! It's true that I wrote them against a certain reluctance on my part, which expressed itself in particular by "forgetting" the (already planned) acknowledgements in the "main" text of the note, so that I had to "make up for it" with a footnote. This reluctance was undoubtedly due to the unease I had already felt from the first time I held in my hands this volume called SGA 5 (and which I didn't have the opportunity to hold in my hands again, I believe, until the last few weeks), an unease I mentioned in the footnote (dated today, May 26) to the previous note "The Signal". This inattention illustrates the importance, in meditation, of vigilant attention to what's happening within oneself at the very moment. In the absence of such vigilance, the reflection here remained below the level of meditation, on a superficial level - whereas attention to this reticence would have led me to probe its origins, and thus also to take a closer look at what had become of this fine seminar (something I didn't do until two weeks later).

you had to believe that they were sending him a letter<sup>68</sup> (\*)

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□ This experience was undoubtedly one of the very first that taught me a certain caution, when my own insights seem to contradict generally accepted knowledge: that such a situation may merit careful consideration. Prudence, which is a fruit of experience, marries and complements (without altering) the spontaneous confidence in one's own ability to know and discover, and the assurance given by the original knowledge of this power within us.

## 14.2.16. funeral

**Note 70** (April 28) Thinking back last night to this story about the cover of SGA 4<sup>1</sup>, where I unknowingly appear as a "collaborator" of my illustrious ex-student, the thing seemed so incredible that a doubt came to me if I wasn't being betrayed by my memory, and hadn't indeed been consulted and would have given my agreement without thinking too much about it. But this assumption goes so far against the grain of the attitude that was mine until last year, namely that there was no question of my publishing maths again (and even less so, not as a "collaborator" with someone, and of someone whose relationship with me already seemed to me to be fraught with profound ambiguity) - that it is even more "incredible" than what it was supposed to "explain", and which in the end has nothing mysterious or inexplicable for me! As a matter of conscience, I checked my friend's letters between 1976 and the present day (there aren't many of them, and it was a quick job), without finding, of course, any allusion to the publication of SGA 4<sup>1</sup>. I did write a few lines to the person concerned himself, to ask him if he could give me some explanations about this "hoax", which I didn't really appreciate...<sup>69</sup> (\*)

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When, in my reflection three days ago, I mentioned the turning point that took place three years ago in my relationship with my friend Pierre, when I lost interest □ in continuing to communicate with him on mathematically (see "Two turning points", note (66)), I remembered a certain impression that had been strongly present at the time. To put it in context, I should first point out that, during the ten years that had elapsed, while my friend had played for me the role of practically the one and only mathematical interlocutor, I had expected (as much a matter of course as the role I was making him play) that he would **relay** the mathematical thoughts and ideas I shared with him, and in turn communicate them to mathematicians who might be interested in them. As I've explained elsewhere (see section 50, "The weight of a past"), it was the feeling of having such a relay interlocutor that gave my sporadic periods of mathematical activity a deeper meaning than that of satisfying a craving, by linking them to a collective adventure that went beyond my own person. It was this feeling, no doubt, that

<sup>68</sup>(\*) (April 28) The above evocation brought back other memories, which show that this famous number  $\pi$  intrigued me more than I first thought I remembered. The approximate value 344/133, found in a book (perhaps the same one), had struck me - it was so pretty that I could hardly believe it was only approximate! Knowing only fractional numbers at the time, I was intrigued by what the numerator and denominator of the irreducible fraction expressing  $\pi$  might look like - they must have been quite remarkable numbers! Needless to say, I didn't get very far with these childish musings on squaring the circle.

<sup>69</sup>(\*) (May 26) My friend kindly honored me with a reply, which dispelled the last trace of doubt. He had indeed listed me as a "collaborator" because of the presentation of SGA 5 that he had written and included in SGA 4<sup>1</sup> - and he hadn't thought it necessary to ask for my agreement to this transfer, or to be listed as a "collaborator", nor had he thought it necessary to send me a copy of this volume to which I had collaborated so well, given that "I hadn't done maths for seven years".

(June 5) I have just received (better late than never!) a letter (dated May 30) from Contou-Carrère, replying to a letter of April 14 in which I asked him (as a matter of conscience) if he had ever seen a copy of SGA 4<sup>1</sup> among my books. It would seem that there was such a copy, which Contou-Carrère had kept with him (unless he bought it and no longer remembers?). On the other hand, Deligne's reply seems to confirm that he hadn't thought it worth sending a copy: "It might indeed have been a good idea to send you a copy of 4 1/2; I doubtless thought that you wouldn't have seen the point" (letter of May 15).

The fact that, for so long, I felt no desire to publish what I found, and even less regret at having withdrawn from the mathematical scene (such regret, incidentally, never appeared, and I "reappeared" on the said "scene" without any deliberate intention, and before I even realized it! )

I can't say to what extent my friend lived up to this expectation - it's possible that he played the expected role as long as he maintained that mathematical availability, driven by curiosity and affectionate sympathy at the same time, which had made possible and quite natural this exceptional role he played in my relationship to the world of mathematicians (and also, to some extent, in my relationship to mathematics itself). When I asked myself the previous question, a day or two ago, I received (as if in immediate partial response!) a letter from Larry Breen, sending me copies of various correspondence from 1974 and 1975, including two lines from Deligne from 1974, accompanying a copy of a letter (which I had just written to him about Picard's field formalism), which asked his opinion about my letter. In it, he refers to me as "the master", in which I think I sense a half-pleasant, half-affectionate intonation. I can't recall any other occasion when I heard from others about things I'd told my friend since I left in 1970. It's quite possible that there have been and that I've forgotten, not to mention the fact that even during the episodes of my mathematical activity, he was relatively uninvolved.

I rarely felt the need to consult my friend, and until 1977 or 1978 the reflections I made to him on occasion were limited in scope. So there wasn't much to "relay", to *proprementally* speaking, p. 266 until this moment<sup>70</sup> (\*).

Things changed in 1977, when for the first time since the sixties, I was strongly "hooked" on an exceptionally rich substance. This was the beginning of my reflections on maps, and one thing leading to another (around the same time), on a new approach to regular polyhedra (see *Esquisse d'un Programme*, par. 3 and 4). By this time, too, it was clear to me that the facts I had just put my finger on opened up unsuspected vistas, comparable in breadth and depth to those I had glimpsed (and more than glimpsed, subsequently) with the birth of the notion of pattern.

It's strange that, on this occasion, I was still addressing my friend with the expectation that he would echo these things that had amazed me and what they were making me glimpse - whereas the total silence that for seven or eight years already had surrounded the very name "motif" was eloquent enough to teach me that my expectation was illusory! This astonishing lack of discernment illustrates the deliberate intention I had (even after discovering meditation a year or two earlier) to pay no attention to my relationship with mathematics or mathematicians, supposedly part of the distant past.

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<sup>70</sup>(\*) I could make an exception for my first thoughts on a theory of unscrewing stratified structures, which I must have mentioned to Deligne in the early '70s. He had greeted my expectations on this subject with indulgent sympathy, rather like that accorded to a grown-up child who doubts nothing. (These were dispositions he often had in his relationship with me, and which were surely often well-founded!) My friend's scepticism, motivated by his knowledge of certain phenomena of savagery of which I was unaware, didn't convince me - rather, the facts he was pointing out made me suspect from that moment on that the context of "topological spaces", commonly adopted for "doing topology", was inadequate for flexibly expressing certain topological intuitions I felt essential, such as that of "tubular neighbourhood". Over the next ten years, I had little occasion to return to these reflections, and I had to forget my "suspicions" for a while, which were brought up to date again (and then became an intimate conviction) by my reflections of December 81.

- January '82, stimulated by the need for a theory of "unscrewing" the "Teichmüller tower". (Compare *Esquisse d'un Programme*, par. 5, 6).

(June 5) As another exception, I could count my reflections on virtual relative patterns and virtual patterns (above a general basic pattern), which I seem to remember sharing with Deligne. As these were things closely related to a yoga he had decided to bury (until the time of exhumation in 1982), it's not surprising that he didn't make

For some hints, see note no. ° 46, .



and well out of date! My first thought was along these lines (\*) takes place precisely in 1981, the year the second "turning point" in my relationship with my friend, which I've had occasion to mention. But even in this meditation, which lasted for several months, the relationship with the other mathematicians was barely touched upon, and the relationship with the one among them who had been undoubtedly the closest of all (at least in terms of our shared passion) was not even touched upon, as far as I can remember. It would have been very useful!

Looking back and reflecting on it now, it's clear that what happened at that moment, which surprised and frustrated me so much (the sudden appearance of a discreet disdain, where I had expected to share the still fresh joy of a discovery that had made a deep impression on me), was indeed what had to happen. It was precisely the **scope** of what I had to communicate, which had motivated my expectation of an interest in tune with my own, that was to arouse in my friend, for the first time in his relationship with me, the reflex of **discouragement**. This reflex must have been all the stronger, given that I was already "pre-buried" by the publication of SGA 4<sup>1</sup>. When I returned to the fray three years later, as my friend (armed with his beautiful theorem on absolute Hodge cycles) was about to take care of the burial in due form, with the "memorable volume" published the following year<sup>72</sup> (\*\*), this same reflex came into play, but with a completely different brutality. (This episode put an end to communication on a mathematical level, but without "discouraging" me... .)

In both cases, the disinterest was obviously sincere, as it had been in other cases, when it had been expressed towards others than myself. It wasn't the first time I'd seen in him (or in others) forces alien to the thirst for knowledge neutralize it, and take the place of the mathematician's flair.

It was on these two occasions, in 1978 and again in 1981, that I first glimpsed, as if in a flash, the **"price"** of this contradiction in my friend that had been known to me for many years, but whose significance, as a hindrance and limitation in his work and in his understanding of mathematical things, had never been clear to me until then. But it was only in the course of the meditation that I pursued for the past month, on the sense of a certain **burial** that had been taking place insidiously since I left, that this □ ported ended up gradually appearing in full view.

On an obvious level, the funeral I've discovered over the last few days and weeks, which I'd been anticipating for several years but never thought of attributing a particular role to anyone, has been first and foremost the funeral of **my mathematical work**, and through it and above all, of **myself**. The best placed of all, of course, to put a hand to this burial (which many others, in their heart of hearts, were calling for), and to preside over the anonymous obsequies, was the friend who, in the eyes of all, had once been the legitimate heir. If he presided over the funeral, he was certainly not alone! But at a deeper level, the one my friend was so discreetly burying, throughout those twelve long years, was none other than **himself**; that thing in him, rather that impresses no one, a delicate and elusive thing like the fragrance of a flower or a fruit, and that has no price. (⇒ 71)

## 14.2.17. The tomb

**Note** 71 But following the thread of associations, I've strayed from my purpose, which was to evoke a certain "strong impression", the memory of which has come back to me insistently over the last three days. This impression occurred at the time of the "turning point" in my relationship with my friend, when I was confronted with signs of

<sup>71</sup>(\*) On this subject, see "The troublemaker boss - or the pressure cooker" (s. 43).

<sup>72</sup>(\*\*) This is the Lecture Notes 900 volume, see note "Souvenirs d'un rêve - ou naissance des motifs" (n° 51).

(at once muted and brutally obvious) of a kind of deliberate contempt - signs that made me put an end to our relationship on a mathematical level. I then understood that the moment had arrived when I had nothing more to expect from the continuation of such a relationship, and the "decision" was made on its own, without division or regret, as the first fruit of this late (and very partial) understanding.

There was no anger in me, and even less bitterness (I don't remember during the course of our relationship feeling any movement of anger towards my friend, nor any bitterness, except at the time of my departure from the IHES, when he was not the only one to be included in it). But there was a sadness, as I turned that page in my relationship with someone who continued to be dear to me, when the strongest bond that had attached me to him had dried up and perished. And like a sting that stayed with me in the years that followed, there also remained this unresolved frustration, of the joy I had brought to share with him, to the one who seemed closest and best placed to share it, and which had come up against the closed doors of complacency. This frustration has finally been resolved, it seems to me, through the meditation I'm currently pursuing. Just today, this one has come back to show me that what was happening to me was what was supposed to happen' ☐ and that the person primarily responsible for this frustration is no one else

that I myself, who had seen fit to indulge in an illusory image of a certain reality, rather than use my healthy faculties and look at this reality with awakened eyes

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It was against the backdrop of this sadness, and also of this frustrated expectation, that this strange impression appeared, which came not as the fruit or outcome of reflection (which didn't happen then), but as an immediate and irrefutable intuition. It was that everything I could say to my friend on a mathematical level, and everything I'd been saying to him for years, I was entrusting or had entrusted to a **tomb**. While I never mentioned this impression to anyone, nor did I write it down in black and white in the course of any subsequent reflection, I do remember that it was this image of a **tomb** that was then present, and the very word that expresses it (in French), and which I have just written down. This "impression" or image must have arisen, at that moment, as the visual expression (so to speak) of some understanding that, at some level, must have been forming and present for a long time, as the fruit of a whole set of perceptions that must have taken place over months and years, without attention retaining them or memory registering them; perceptions that were simple and obvious, no doubt, but which I hadn't "retained" because they seemed undesirable to someone inside me who often has the power to sort them out as he pleases. . . Neither at the time, nor since, has this peremptory image been associated with any precise, tangible recollection of an "event" in line with this image, and which could have given rise to it in me - the memory of this sudden image must have crossed my mind only rarely afterwards, and today is the first time I've dwelt on it in the slightest.

If no memory or association arose at the time, it's surely because I didn't have the minimum availability to welcome it. Strangely enough, at the time I was engaged (if I place the<sup>73</sup> (\*) moment correctly) in a meditation on my relationship with mathematics, without this episode, which spoke to me strongly enough, after all, of a certain past through a present, making me think of interrupting the "thread" of my reflection, to include a reflection on the ins and outs of what had just happened and which was not without consequence in my life.

☐ The first (and, to be honest, the only) association that arose even now (having just evoked and to say that on the spot it had appeared disjointed from any memory or association. . . ) was the fate that had befallen my "dream" of patterns - the mathematical vision of all that had been dear to me,

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<sup>73</sup>(\*) (June 11) Cross-checking confirms this to be the case. This "second turning point" occurred in the second half of 1981.

in my mathematical past. If that past perhaps still had some secret hold over me, it was through that dream - and that secret hold (which I think I glimpse as I write these lines) itself had the force, beyond words, of the dream. If, as the legacy of a past investment, a passionate investment in mathematics, an unspoken, deep-seated frustration had arisen over the past ten years, it was indeed that of seeing a deathly silence surround those things which for me were alive, and which I had entrusted to my friend as living, vigorous things, ready to leap into the light of day! With me gone, it was he and no one else who had the power and vocation to watch over this blossoming, to make available to everyone what he alone (with me) could intimately feel. And without ever saying it to myself in these or any other terms - without ever stopping (as far as I can remember) even for a thought about what I had left behind - somewhere inside me I must have realized, over the years, that this dream that was always dear to me, I had entrusted to a "tomb".

And then, with this evocation and the first association it arouses in me, I see a flood of other associations appearing in its wake, revealing to me that I have indeed touched a nerve center - the point of all, perhaps, through which the (long-ignored) weight of my mathematical past is exerted.

But this is not the place, it seems to me, to follow these associations, as this "final" stage of my reflection is already getting long in the tooth. It seems to me that I've said enough in this reflection about my friend Pierre as well as about motives - and surely too much for many people's taste! And I think it's time, as far as these notes are concerned, to bring them to a close, with a sort of **assessment** of what this reflection on a double funeral is teaching me, for the time being.

## 14.3. VI The return of things - or the Unanimous Agreement

### 14.3.1. One foot in the merry-go-round

**Note 72** (April 29) .....

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□ It seems to me that most of the descriptive and decanting work that had to be done, on the subject that occupies, is complete, as far as the "partial images" about a certain situation are concerned. (It's obvious that these notes, intended for publication, only give an abridgment of the actual work, while it's out of the question here to spell out in detail all the elements that contribute to the formation of this or that partial "image". . . .) Surely, through this same work, a certain overall image could not fail to take shape, still vague, and waiting to be formulated to take shape and life and tell me what it has to say. From yesterday's reflection, I feel it's ready to blossom, urging me to give it a voice.

To tell the truth, what yesterday's reflection (which I've just reread) taught me most of all **concerns none other than myself**. It's with a certain relief that I see the reflection returning to the firm ground of a reflection on myself, whereas for the past week it has often given me the feeling of involving someone else's person more than my own. Yesterday's reflection finally revealed to me something that is surely quite obvious: the strength of my attachment to a certain past, to my "mathematician past", and the particular role played in it by this famous "dream" of motives.

Once the point is finally made, its obviousness is obvious - the most recent and clearest sign perhaps being the emotion triggered by the discovery (two years later) of a certain "event", of this

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<sup>74</sup>(\*) I thought it wise to spare the reader a good page of considerations on meditation in general, which were a way of beating around the bush - a sign of the resistance to getting to the heart of the matter.



the "furtive" (and belated) re-entry of motifs into the mathematical menagerie, under the guidance of my former "pupil" and friend! This emotion was immediately translated into the resumption of a reflection that seemed to be over, - a resumption that materialized as dry as a fifty-page stream of retrospective reflections! As a result (and I've already realized this several times during this untimely resumption), it would seem that that I'm not yet as "off the merry-go-round" as I thought I was a month or two ago, in my exultation.

of the end of a stage and the feeling of liberation (by no means illusory) that this stage had brought me - □avec p. 272 the teaching that "I wasn't better than the others", and that "I shouldn't be surprised if the pupil surpassed the teacher"<sup>75</sup> (\*). Yet this teaching didn't stop me from being surprised - it was enough for the "pupil" to overtake me in a direction I hadn't anticipated at all! But if the teaching didn't prevent me from being "astonished", it was nonetheless invaluable to me on more than one occasion in the course of the past reflection, to save me from the usual pitfalls (or at least **some of them**).

To come back to the strength of this "hold", to the strength of my attachment to this dream of motifs, it has already appeared in many other places in the present volume, whether in Récoltes et Semailles (where motifs are mentioned several times and in quite eloquent terms), or in the Esquisse d'un Programme (where, "objectively", motifs had nothing to do with it), or in the Esquisse Thématique (where motifs are a bit like unhatched eggs in a flock of vigorous chicks). In the latter text, which dates back twelve years and is obviously written in distant dispositions, this last paragraph on motifs is the only one, it seems to me, where we suddenly feel a warmth passing through... ..

The remarkable thing is that this attachment never occurred to me in the fourteen years since I left, until yesterday, when I finally glimpsed the obvious, and finally formulated it for myself today. During the meditation of almost three years ago (July to December 1981), I came to realize the first obvious fact: that I still had a passion for mathematics, which had expressed itself eloquently over the years. But my attachment to a past, as far as I can remember, went unnoticed at the time, and has remained so to this day.

I must have begun to glimpse it, however, with the reflection "The weight of a past", which came to me as a matter of conscience at a time when the meditation on my past as a mathematician seemed already complete (except that I hadn't yet been able to perceive the **weight** of that past!). In fact, as I wrote it, I sensed that I was still on the surface of things, without really penetrating them. The notes I had to add later (first

(46) (47)) then led me in a direction that for a good while distanced me from myself, focusing my attention on a mathematical work (and on those aspects of it that seemed most "important" to me), then on the vicissitudes of that work and the role of others in them, rather than on myself.

□I just reread this reflection "The Weight of a Past" (s. 50). Towards the end of it, I begin to Indeed, I glimpse that the "tipping force" (towards a mathematical investment other than episodic) could be due to an "attachment to the past" (as a mathematician), but rather to "the past of these last ten years, the past "after 1970", and not the past of things already written in black and white, of things done, those before 1970". A few lines later, however, I recall, but only "in passing", that in the "vast program I had before my eyes at the time ... only a small part of it has been realized". In writing these lines, I must have been thinking above all of those parts of the "vast program" that were immediately realizable, whose motivating force (!) was nevertheless nowhere near that represented by the "dream of motives". (Its justification (but by no means its formulation) appeared then as one of the great tasks "on the horizon". . . )

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<sup>75</sup>(\*) See "No more merry-go-round!", n° 41.

It's clear that my attachment to the "motive dream" is (as I'm sure all attachments are) primarily (if not exclusively) egotistical. It's the desire not only to **contribute** to a collective work, but also to have that contribution **recognized**. Assuming that the "vast picture of motives" had indeed been painted to the full extent I saw it since the late sixties, but that my part in the blossoming of this vision had been silenced, my displeasure would no doubt have been no less (and perhaps greater?) than the displeasure I felt when I came across the "memorable volume" (in which I see certain notions and ideas that I had identified and brought to light, but (so I felt, at least) deprived of the breath and intense life that had so fascinated me in them)<sup>76</sup> (\*).

Until this egotistical desire to see things from my distant or more recent mathematical past "recognized" is consumed, it's probably premature to claim that I'm "off the merry-go-round". The mathematical "merry-go-round" no longer **contains** me, as it once did some of my friends. But I've certainly still got a foot in it, and I suspect it'll stay there as long as I keep on doing maths!

### 14.3.2. The return of things (or a foot in the flat )

**Note**<sup>73</sup> (April 30) I've been thinking about the fate of the SGA 5 seminar, and how this fate was linked to the publication of SGA 4<sup>1</sup>. A situation that had been confusing, and which I've only examined in the last few months.

p. 274 days and glimpses in passing,<sup>2</sup>  $\square$  is now very clear to me. I've just added a note footnote<sup>77</sup> (\*) on this subject to my thoughts of three days ago (see "The signal", note (68)), and it seems to me that with the comments I had already made there before yesterday (also in footnotes) and with the thoughts of the day before ("Table rase", note (67)), I have expressed myself clearly enough for there to be no point in making yet another overall summary of a situation which now appears eloquently enough<sup>78</sup> (\*\*).

Having reached this point, it's important to note that the first and foremost person responsible for the "sad fate" that befell SGA 5, and for the use that was made of a situation of abandonment, is none other than myself. If the various "volunteers" (who took on editorial work they didn't really want to do) were clearly not in tune with themselves, neither was I, who stubbornly refused to heed the lesson of a situation that spoke for itself. After all, three whole years elapsed between the end of the oral seminar and my departure from the world of mathematics (which immediately translated into a virtually total lack of interest in my published work over the following fourteen years). It's true that during those three years I was fully occupied with my other tasks, including continuing the SGA seminar (with SGA 6 and SGA 7), writing the EGA, reflecting on the often juicy questions arising from day to day, and among these, the gradual maturation of an overall vision of patterns. . . Taken up with these tasks, I chose to turn a blind eye to the fate of a past seminar, which (together with SGA 4 from the previous year) constituted the most profound mathematical contribution I've been able to make, in terms of fully accomplished work I mean, and also the one with undoubtedly the widest scope.

<sup>76</sup>(\*) (June 14) This "displeasure" is due above all, it seems to me, to this impression of impudence, of deliberate disregard for a link that one affects to ignore, to hold as negligible. The situation is quite different when ideas or results you've discovered are rediscovered by others, which happens quite often.

<sup>77</sup>(\*) This prohibitively long footnote has been turned into a separate footnote, "Le renversement" (n° 68').

<sup>78</sup>(\*\*) I return to this subject on May 9 and the following days, see notes n° s 84-89.

The situation could only deteriorate further after my unavoidable departure, allowing the most prestigious of my ex-students to carry out the ingenious operation of inserting his famous SGA 4<sup>1</sup> between the gangue of nonsense and superfluous details of SGA 4 and SGA 5, and doing me the honor of promoting me to collaborator on what is presented as the central key-text, destined (as he says with that candor that makes up his charm) to do the following charitably "forget" the heavy gangue that surrounds it. ...

□ In short, the choices I made, from before my departure and through my departure, implied consequences for the fate of my published work, or (in the case of SGA 5) work awaiting publication, as well as for the part of my "work" that remained in the state of a dream - an **unpublished** dream, that is. I don't regret my choices, and it's not my place to complain, when I see today certain consequences of those choices that are not to my liking! It is my responsibility, however, to examine these consequences (and all the more so when I don't like them!), to get an overall picture of the facts<sup>79</sup> (\*) (which I've done), and to learn from them what I can. That's what I've still got to do, and today's reflection will perhaps be, at the very least, a first step in that direction. A number of things have come together for me over the last few days, and I'd like to start by putting them in black and white.

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The main force, the "drive" behind my investment in my pupils in general, in the first period of the sixties, was the desire to **find "arms"** to carry out "**tasks**" that my instinct told me were urgent and important (at least from my own mathematical point of view). This "importance" was certainly not purely subjective, it was not simply a matter of "taste and color", and often (I think) the student who took on such a task that I proposed to him felt that it "fit the bill", and also, perhaps, what its place might be within a larger scheme of things.

Yet, as far as this "drive" was concerned, this motivating force within me that pushed me towards completing the tasks, it wasn't some "objective" importance that was at stake - whereas the "importance" of Fermat's conjecture, Riemann's hypothesis or Poincaré's hypothesis left me perfectly cold, that I didn't really "feel" them. What distinguished these tasks from all others, in my relationship to them, was that they were **my** tasks; those I had felt, and made my own. I knew that having felt them had been the culmination of a delicate and profound work, a creative work, which had made it possible to identify the crucial notions and problems that were the subject of this or that task. They were, and no doubt still are (to a large extent), a part of who I am. The link that bound me (or still binds me today) to them, was by no means clear-cut, when I entrusted such and such a task to a pupil - well on the contrary, this link was acquiring new life, new vigor! This link didn't have to be said □ (and I'm "saying" it here, don't

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if only to myself, for the first time). This link was as obvious to the student who had chosen to work with me, and on the task of his choice, as it was to me, and also (I'm convinced) to anyone else. It's the deep bond between the person who conceived a thing, and that thing - and which is not altered, but (it seems to me) strengthened by those who, after him, also make that thing "theirs" and bring the best of themselves to it. It's a bond I've never examined closely. It seems to me deeply rooted in the nature of the "I", and universal in nature, It's a link that we sometimes affect to ignore, as if we were above such pettiness - it's even possible that I've sometimes entered into such an affectation<sup>80</sup> (\*). But the few times in recent years (or in recent days and weeks) when I've been confronted with an attitude in others that affects to ignore this link (of which they are aware) that links me to such and such a task that has

<sup>79</sup>(\*) (May 28) Read here "facts known to me". On the following day, entirely unexpected new facts rekindled my thoughts on the Burial and led me to triple the volume of my notes on the subject.

<sup>80</sup>(\*) What's certain is that I was following the "bon ton", which consists in ignoring this kind of thing, contrary to the de rigueur images! (May 30) On this link, see the note ". . and the body", n° 89.

has been accomplished (by another, or by myself) or merely designated, I am touched in a sensitive place. We can call this place "vanity" or "fatuity" and put other names to it - and I don't claim that these terms are out of place here, but whatever we call it, I'm not ashamed to talk about it or to be as I am, and I know that the thing I'm talking about is the most universal thing in the world! No doubt this attachment to "one's work" is not as strong from one person to another. In my life, where "Doing" has been the constant focal point of my great investments of energy since childhood, this link has been strong and remains so today.

I can therefore say that the main force driving my relationship with my students was that I saw in them welcome "arms" for the accomplishment of "my" tasks. The formulation may sound cynical, but it simply expresses an obvious reality, surely felt by my students as well as myself. The fact that they were "my" tasks in no way prevented them from also doing "their" tasks - and it was this 'identification in them with their task that mobilized in them the energy necessary for their accomplishment; just as identification with this same task had mobilized in me the energy that had brought it into being and taken shape, and continued to mobilize the energy I continued to invest in the subject. This energy was essential if I was even to "function" as the "master", i.e. as the elder who teaches a craft (which is also an art), and which cannot be done without mobilizing considerable energy. Never in my teaching past have I felt a contradiction in the fact that the same task was profoundly "his" for the student who was working with me, while remaining just as deeply "mine". I don't believe that this situation is in the least conflictual, nor that it has ever given rise to the desire for conflict<sup>81</sup> (\*). In this situation of simultaneous investment in, and identification with, the same task, both the student and myself found (it seems to me) our account, in a working relationship that was perfectly clear, and which in itself (it still seems to me) contained no conflictual elements. On the personal level, on the other hand, the relationship remained superficial - which in no way prevented it from being cordial, even friendly and sometimes even affectionate.

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The investment in my tasks, and **through them** in my students-collaborators for these tasks, was (as I said) of an egotistical nature (like any investment, no doubt). Surely, carrying out these tasks was above all, for the "I", a means of enlarging itself, through the realization of an overall work of vast proportions that "my arms alone" would not have been able to bring to fruition. From a certain point in my life as a mathematician, there was this constant ambiguity of a cohabitation, a close interpenetration between **the "child"** and his thirst for knowledge and discovery, his wonder at things seen and those examined closely, and, on the other hand, the **ego**, the **"boss"**, rejoicing in his works, eager to expand and increase his glory by multiplying his works, or by the dogged, relentless pursuit of an overall construction of grandiose proportions! In this ambiguity, I see a division that continues to weigh on my life and leave a deep mark on it - a division that perhaps will remain as long as I live. Such a division is certainly not unique to me, but perhaps in my life, filled with the "best" as well as the "worst", this division has taken more extreme forms than in others.

So I can say that for this invasive, eager-to-expand "I" (who wasn't alone in this respect) my pupils were first and foremost welcome "collaborators", not to say "instruments" - welcome "arms" for the construction of an imposing work that would say "my" glory!<sup>82</sup> (\*\*)

<sup>81</sup>(\*) If, encouraged by a certain context, one of my students has wanted to take over a role that had been mine, in a work done with me, this was done at a time when he had long since ceased to be a student.

<sup>82</sup>(\*\*) I wrote this sentence with some hesitation, weighing up my words in the full knowledge that it could be seized upon as a sort of cynical admission of the horrible Mandarin finally throwing off the mask! But I'm well aware that I won't prevent anyone who wants to drown an embarrassing fish from doing so at their leisure. It won't stop me from pursuing my aim of discovering and stating the obvious, including the humble truth written above, which will only surprise those who have never bothered to look.



This is something, it seems to me, that became quite clear□already in the course of my meditation three years on my relationship to mathematics (and beyond that, to "doing" in general), even if I sometimes forgot about it afterwards. It's the thing that's been on my mind these last few days, to make the connection with this other remarkable fact: that it was precisely by one of my students (with quotation marks, mind you!) from that time, and the one who was closest to me of all, and the only one to "feel" effortlessly and in their entirety these great designs within me that seemed to push me relentlessly to realize them - that it was he of all people who, after I left (and in his own heart, no doubt even before.

«.) a mis en oeuvre au cours des ans cet **Enterrement** aux dimensions de l' Oeuvre (les majuscules ici ne sont pas de trop !), et qui a finalement "présidé aux Obsèques" (avec une majuscule de plus, pour faire bon poids !). What's striking about this situation is the enormous, irresistible, **Ubuesque comedy of the whole thing!** I must have sensed this comedy in a vague way over the last few days, but it only revealed itself to me in its true nature at this very moment, when I placed the last capital letter over my solemn funeral - in a sudden, irresistible burst of laughter! It was precisely **laughter** that had been lacking until now in this so-called "final" stage of reflection, where the dominant note was rather the pained air of the "Monsieur bien" disappointed in his legitimate expectations (or even abominably deceived), when the pained air gave way to sarcastic, well-sent comments (one is used to expressing oneself, or one isn't!). I definitely feel I'm on the right track again, after this long digression (that word reminds me of something. . . ) in the sad tones.

And just now I've come up with a name for this "note" (I'm not sure what it's a note to, but whatever.) that it's time to close. It will be "**Le retour des choses**". (⇒ 74)

### 14.3.3. The agreement

**Note 74** I finally feel - phew! - that I'm nearing the end of this "final stage", which has stretched over twelve days, each of which (as in the past) was presented as "the last". Perhaps the final word was spoken just a few minutes ago. My (symbolic) funeral was a **return of things**, a harvest of seeds made by my own hands. (And my burial in□chair and bone, if I have this happiness of dying leaving behind me living men and women who can bury me, will also be a return to something I left at birth... .<sup>83</sup>(\*)). It seems to me that whatever remains to be added will be little more than an **epilogue**.

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The famous "dearest pupil of them all" was not the only one of my dear pupils to bury me with gusto, and those who did indeed put their hand to the dough may not be the only ones among them, present at the funeral without displeasure! But I don't really care who it was! (Knowing more about it, if nothing else, won't tell me anything more). I've finally understood this "return of things", and having understood it, I'm reaping the benefits.

Yet I haven't yet extracted all the substance this benefit has in store for me. It's not yet clear to me exactly **what it was about** me that made certain ex-students take advantage of the funeral and burial. Is it only this "greed" of which I spoke, which (it seems to me) does not

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itself.

<sup>83</sup>(\*) (May 28) This sudden association with my own death presented itself forcefully. I was tempted to dismiss it, and then to suppress this unexpected parenthesis, which seemed to come like hair on the soup. I refrained from doing so, out of a kind of respect. Strangely enough, the next day I learned that on the evening of April 30, when I was continuing my reflection, in the commune where I live, the (seriously ill) sister of a friend had died. I saw Denise for the first time, and on her deathbed, the very same day. The following day, May 2, I joined my friend and many other living men and women to lay her to rest on a beautiful spring day...

which they had accommodated without difficulty (and probably without even noticing it, at least not on a conscious level) when they first started working with me? Was it then the "occasion" (my departure, etc.) that "made the thief", and **revealed a general propensity**, in them as in the "pupil of all", to bury his "master" or his "father", when the circumstances were propitious? Perhaps I was more "master" (or more "father". . . ) than nature, and this circumstance played a part in triggering this "burial syndrome" with a vengeance! For the moment, I don't know! Perhaps the echoes I gather (I hope) will enable me to see more clearly, and to better assimilate the unexpected food before which I am now sitting.

There weren't any students to discreetly take part in the funeral and burial, even though no non-ex-students were in a position (as far as I know) to play a prominent role. Clearly, many of my old friends found this to their liking. The whole thing doesn't seem too mysterious to me.

p. 280 □As I've had occasion to say in passing, more than once I've witnessed the deep malaise created in my friends of yesteryear by my untimely departure from the mathematical scene. It's the malaise that arises from anything that obscurely feels like a **provocation** to profound questioning, to renewal. In this particular case, it was natural that this unease among mathematicians should be strongest among my friends, those who had known me, and who could feel the full force of the investment I had made in the values that are still theirs; not to mention the fact that each of these friends has made, and continues to make, an investment of comparable strength in these values, and in the substantial "returns" they offer. I had already had ample opportunity to observe such unease among other scientists, right from the start of the Survivor period. But that didn't stop me from being surprised every time I saw unequivocal signs of distancing, and sometimes even enmity, among one of my old friends, to whom I continued to feel the same sympathy. What must have made my "abandonment" particularly intolerable for some of them was precisely the fact that I was supposed to be one of the "best" of them, surely the last one they would have suspected of playing such a trick on them! (And I did indeed sometimes sense a tone of **resentment** in such of my old friends in the mathematical world.) It's only natural, then, that they should find it in their hearts to see that all this "grothendieckery" was, after all, a lot of paper for very little money, etc. etc. A single person, no matter how prestigious, is not enough to make a fashion - it's even more important that the fashion you want to launch responds to an expectation, a secret desire, in many others, before it becomes a consensus and the law<sup>84</sup> (\*).

In the fourteen years since my departure, I have perhaps tended to underestimate the unease it has created in the "wider world" - even though, for me, my departure in June 1970 came so naturally that there was not even a "decision" to be made: new tasks had taken over from the old ones overnight, and the latter had suddenly receded into the background as if from the distant past! (It's also true that I didn't experience such unease among my colleagues at the University of Montpellier, who form a completely different milieu from the one I'd left). Perhaps I also underestimate the role that such unease may have played among my ex-students "before 1970", including

p. 281 many of them belong to this same milieu, and "go all out" in their mathematical investment. It's possible that□this uneasiness played a role no less strongly in them than in the other friends I thought I had in the same environment. In any case, each situation (between me and one of my former friends or students) is unique and different from all the others, and any general assumptions I may make are of very limited and provisional scope.

Returning once again to the more solid ground of the case in point, I am struck by the fact that the two ex-students

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<sup>84</sup>(\*) (May 28) See in the same vein the note of May 14, "Le Fossoyeur - ou la Congrégation toute entière", n° 97.

whose active participation in my beloved master's funeral I was able to witness, were also the very ones who had first drawn my attention to themselves by their contemptuous, discouraging attitudes: towards younger mathematicians who were "post-1970 students", where the influence of my ideas and approach to mathematics was clearly visible. This coincidence certainly came as no surprise (although, of course, events surprised me at every turn!). Another interesting coincidence is that both were among those with whom the personal relationship was the most friendly and even affectionate (and for one, this relationship has continued, and in this tone, to this day). This is in line with the general observation that it's the closest relationships that have the greatest virtue of attracting and fixing the forces of conflict.

Yet another coincidence struck me. Among all the students I've had over the past twenty-five years, there are two who stand out for me, both for their exceptional "means", and for their investment in mathematics commensurate with these means. (An investment of a strength comparable to that which I myself made during twenty-five years of my life). For both of them, moreover, I made a point of counting them among my pupils, even though it's true that they both learned things from me that were useful to them<sup>85</sup> (\*). It was in the nature of things that both of them would discover their own tasks, without my having to suggest any of those I had (or have) in mind.

reserve - and the thesis work of both was carried out independently of me<sup>86</sup> (\*\*).

So many □ points in common! As a point of dissimilarity, I would say that the youngest (unless I'm mistaken) of the p.

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two is today "at the pinnacle of honors" (whose detailed enumeration I'll spare the reader, and to the known modesty of the person concerned), and that he is one of the most influential mathematicians, that is to say, also one of the most powerful; the other is for the moment a delegated assistant, in a position that the incumbent will take over next year. There are other points of dissimilarity, which explain to some extent this difference in fortunes - just as there are other points of similarity on which it is pointless to dwell here. Except for the fact that, of all the students I've had, it was with both of them that my personal relationship was also the closest and friendliest, while a common passion had from the outset created a strong bond between each of them and me. The **coincidence** I'd like to mention now is that, as far as I know, they were the only students too (in quotes, that's a given!), who vis-à-vis the "big world" did their utmost to minimize or erase, as far as possible, this very simple and obvious link to me.

It's a truly striking coincidence, the meaning of which still eludes me at the time of writing. For both, I could invoke reasons of conjuncture, different from one to the other. And it is quite possible and even probable that for both of them, at a certain level which is probably no longer that of fully conscious intentions, such a reason (of fatuity for one, of prudence for the other) came into play. I doubt, however, that this ready-made explanation will provide an understanding of the matter, in either case.

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<sup>85</sup>(\*) (May 28) That's an understatement, as I later found out to my chagrin! See yesterday's note "L'être à part", n° 67'.

<sup>86</sup>(\*\*) (May 28) That's not quite true. Both of them made essential use in their work of tools that I had and which they learned from me. Beyond this role, Hodge-Deligne's theory in the work that constitutes his thesis (Hodge Theory II, Publications Mathématiques n° 40, 1972, p. 5-57) stems directly from the yoga of motives that he got from me - "mixed Hodge structures" being the "obvious" answer to the (also "obvious") question: "What is the difference between the two?

in the perspective of motives) to "translate" in terms of Hodge "structures" ("in a suitable sense") the notion of motive not necessarily semi-simple on the field of complexes. Beyond a brilliantly conducted "translation exercise", there are of course original and profound ideas in this work that are "independent of my person". But it is also clear that the Hodge-Deligne theory would not exist today (nor, doubtless, almost all of the work of Deligne or any of my other students) if they had not had access to the ideas and tools that I introduced into mathematics, and which they learned from me.

the other. Surely, deeper still, other forces must have been at work, the real ones, behind the familiar appearances of fatuity or pusillanimity. Surely, these acts that express them have something important to say to each other. But surely, too, the appearance of the same acts in two such different people, as if they'd given each other the word (something certainly unthinkable, given the difference in their personalities), is a sign that they're on the same wavelength.

p. 283 fortunes!), also has something important to say to me, and about none other than myself. Could this be nothing more or less than a reproduction of the eternal **rejection of the father**? The latter, however, has  
□'embarras du choix

among the avenues open to him to express himself! Or is it because that sure instinct of the unconscious, which makes it touch "just right" in the most sensitive or vulnerable places (when it comes to "touching"), has meant that both have fallen on the **same** spot? I'd actually be inclined to think so. But that's an inferred thing, not a seen thing, whereas lacking eyes with the gift of seeing clearly and deeply, I feel a bit like a blind man groping around in the dark, trying as best he can to "see" with his hands or his ears or his epidermis, which aren't really made for seeing.

So as not to close on this note of **perplexity** (prejudicial to my reputation), but on a note that would be pleasing to a benevolent and hypothetical reader, I will only say the concluding name, which appeared earlier, and which seems to me to express well the content common to the various considerations of this **epilogue** (to a reflection on a funeral), namely:

**The Unanimous Agreement!**

# 11. C) THE BEAUTIFUL WORLD

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## 11.1. VII The Colloquium - or Mebkhout and Perversity bundles

### 11.1.1. Iniquity or the meaning of a return

□

p. 285 **Note 75** (May 2) I'm definitely not done learning! I've just read two

texts, which shed unforeseen light (for me at least) on the "escamotage" (of Meb- khout's work) already mentioned ("L'inconnu de service et le théorème du bon Dieu", note (48)). It concerns the role played by the two illustrious colleagues and former students whose disdainful indifference to Zoghman Mebkhout I noted, without however questioning their professional bona fides. Both texts are part of the Proceedings of the **Luminy Colloquium** (July 6-11, 1981) entitled **Analyse et topologie sur les espaces singuliers**, published in Astérisque n° 100 (1982).

The first of these texts is the introduction to the Colloquium, signed by **B. Teissier** and **J.L. Verdier** (the same man who acted as Z. Mebkhout's official thesis director). This one-and-a-half-page text begins with an explanation of a certain "Riemann-Hilbert correspondence", which is clearly destined to play a leading role in the Colloquium (and which is none other than the "theorem of the good Lord" alias Mebkhout). In this correspondence (and this is what gives it its charm and depth, and necessitates the introduction of derived categories), a regular holonomic **module** (i.e., a regular holonomic complex reduced to degree zero) is associated with a constructible complex of C-vector bundles, which can be characterized (it is said) by purely topological properties that make sense for constructible complexes of sheaves over a not necessarily smooth variety defined over any body.

This, it is explained, is the starting point for the Colloquium's "main theme", "**perversity, intersection complex, purity**" - the (complex  $\square$  of) so-called "**perverse**" beams<sup>1</sup> (\*) being none other than those which, "morally", correspond ("à la Mebkhout") to the simplest complexes of regular holonomic differential operators, expressed using a single D-Module.

The second text is part<sup>2</sup> (\*\*) of the long article by **A.A. Beilinson, J. Bernstein and P. Deligne** on perverse bundles, referred to in the introduction as the central work of the Colloquium. As can be seen from the table of contents and the other pages at my disposal, this paper marks the sudden re-entry of derived and triangulated categories into the public arena, in the wake of Mebkhout's obscure work and the famous "Riemann-Hilbert" theorem.

Incredibly, in both texts, Z. Mebkhout's name is absent. Mebkhout is absent, just as he is absent from the bibliography. I should point out that not only was J.L. Verdier perfectly aware of Mebkhout's work (and with good reason!), but so was Deligne (and it would be difficult even to imagine that it could be otherwise, for someone so well-informed about current mathematical events, and when it's about the subject that touches him most closely<sup>3</sup> (\*\*\*)).

I don't know what happened to B. Teissier<sup>4</sup> (\*\*\*\*) and the other participants in the Colloque de Luminy, notably the two co-authors with Deligne of the article cited<sup>5</sup> (\*\*\*\*). It seems that none of the participants was so curious to know the authorship of the ideas and the key theorem that had had the virtue of mobilizing them.

<sup>1</sup>(\*) (May 4) See note no. ° 76, "Perversity", on this strange application.

<sup>2</sup>(\*\*) (May 4) I've since received the full article, which confirms what the part I had already shown me.

<sup>3</sup>(\*\*\*) In particular, Mebkhout's work and his "theorem of the good God" represent a decisive advance on Deligne's earlier work (from 1969), which he refrained from publishing. On this subject, see note n° 48' already quoted.

<sup>4</sup>(\*\*\*\*) (June 12) B. Teissier had long been interested in Mebkhout's work, and had thus been one of the very few to have an encouraging attitude towards him. He was therefore perfectly aware of the scam, to which he knowingly lent his support. He justified himself to Mebkhout by assuring him that, in any case, he "couldn't have done anything about it".

<sup>5</sup>(\*\*\*\*\*) (May 28) I have since learned that A.A. Beilinson and J. Bernstein were informed of Mebkhout's results by P. Deligne (in October 1980) and by Mebkhout (in detail in November 1980, at a conference in Moscow). These two authors made essential use of the God's theorem in their demonstration of a famous conjecture known as the Kazhdan-Lusztig conjecture, even before the Colloque de Luminy in June 1981 - Compare the quotation from Zoghman Mebkhout's letter in the note "A feeling of injustice and powerlessness" (note n° 44").

(June 3) For further details on the solidarity of all Colloquium participants, see the following note "The Colloquium", n° 75'.

I assume that it was taken for granted, a little (a lot) like in the volume of the lecture Notes LN 900 which, the following year, was to consecrate the re-entry of motifs on this same "public square"<sup>6</sup> (\*\*\*\*\*); that paternity belonged to the most brilliant among the brilliant mathematicians who had taken the initiative of the Colloquium and

p. 287 had animated it. What everyone knew for sure was that it was neither Riemann nor Hilbert, otherwise the brilliant Colloquium ☐ would have taken place in 1900 and not in 1981, two years after the pupil's thesis defense.

Unknown by Jean-Louis Verdier.

The kind of operation I've witnessed here is perhaps now commonplace<sup>7</sup> (\*) and perfectly acceptable, as long as it's carried out by mathematicians who are at the top of their game, and the one who pays the price is a vague unknown (even though he's been kindly invited to join in the fun). The fact that one of these men is a great mathematician, both in terms of his means and his work (which puts him above suspicion from the outset), doesn't change the nature of the matter. Surely I'm old-fashioned - in my day, this kind of operation was called a **swindle**, and this one strikes me as a **disgrace** to the generation of mathematicians who tolerate it.

The brilliance of genius takes nothing away from such a disgrace. It adds an unprecedented dimension, perhaps unique in

p. 288 the history of our science<sup>8</sup> (\*\*). ☐ Behind the apparent absurdity and gratuitousness of the act (carried out by someone whom fate has blessed beyond measure, yet who delights in plundering. . . ), we can glimpse the action of forces other than the mere desire to shine, or the gratuitous desire to humiliate or despair those who feel defenceless and voiceless. ), the action of forces other than the mere desire to shine, or the gratuitous desire to humiliate or despair those who feel defenseless and voiceless.

Since I'm definitely in the middle of a "tableau de mœurs", I'd like to point out (almost as a matter of course) that my name is equally absent from the quoted texts. Yet I was pleased to note that there is not a single page of the quoted article (among those in my possession<sup>9</sup> (\*)) that is not deeply rooted in my work and bears its mark, right down to the notations I introduced, and the names used for the notions that come into play at every step - which are the names I gave them when I first became acquainted with them before they were named. There are, of course, some minor adjustments - for example, the biduality theorem that I had worked out in the fifties<sup>10</sup> (\*\*) has been renamed "Verdier duality" for the occasion, still the same Verdier, there's no mistake. ... <sup>11</sup>(\*\*\*). However, it has not been possible for my name not to appear at least implicitly, through occasional references to texts that are still irreplaceable (despite SGA 4<sup>1</sup>, which is not quite sufficient<sub>2</sub> for its purpose), namely EGA and SGA. (In the explanation of the acronym SGA = Séminaire de Géométrie Algébrique du Bois Marie, my name of course does not appear, but in EGA, honest or not, the full designation is given, with the names of the authors including mine... . ) Another detail that struck me, and which testifies to the obsessive strength of the burial syndrome (in someone who, however, has no obsessive "profile" whatsoever): the two references I saw to SGA make a point of explaining each time especially "Mr. Artin's theorem in SGA 4. ... .", lest the misguided reader get the idea that said theorem might be due to the carefully non

<sup>6</sup>(\*\*\*\*\*) See notes n° s 51,52,59.

<sup>7</sup>(\*) I'm thinking of two other "operations" along the same lines, which took shape with the publication of LN 900 (see previous b. de p. note) and APG 4<sup>1</sup> five years earlier (see notes n° s 67, 67', 68, 68').

(May 9) For a third such operation, closely related to the previous ones, see the "Good references" note (n° 82). on another "memorable article", this time by J.L. Verdier.

<sup>8</sup>(\*\*) Nor have I ever heard of such a thing in the history of any other science or art than mathematics.

<sup>9</sup>(\*) (May 4) And the others too, of which I have since become aware.

<sup>10</sup>(\*\*) The same goes for the theory of dualité étale, which becomes "dualité de Verdier" under the pen of his generous friend Deligne!

<sup>11</sup>(\*\*\*) (May 5) Compare notes n° s 48', 63". Throughout this long Burial, which has been going on for nearly fifteen years, and throughout the discovery that the principal "anticipated deceased" has just made over the past month, J.L. Verdier seems decidedly inseparable from his prestigious friend, who lavishes him with the wreaths of flowers that are de



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named, when it is quite clear that the presentation was indeed made, thank God, by a named author! (77)

□ It's <sup>all</sup> fair game in today's "beau monde", it seems. Without indulging myself (and it's not meant for that. ... ) this guéguère is not really detrimental to the anticipated deceased, whose symbolic remains are thus left to the vagaries of this fairground, which I have been discovering with wonder for barely two weeks. It doesn't gnaw at my life with the feeling of **iniquity** suffered in impotence. It hasn't broken the joy and impetus that carry me to the encounter with mathematical things and those of the world around me, nor has it burned the delicate beauty of these things in me. I can consider myself happy, and I **am...**

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And I'm happy too about my unexpected "return", the meaning of which had escaped me. If it were to teach me only what I have learned in these past days, this return will not have been in vain, as it has already fulfilled me ( $\Rightarrow$  76).

### 11.1.2. The symposium

**Note 75'** (June 3) I have received details of the other participants in the colloquium, which dispel all doubts. Although no talk by Mebkhout had been scheduled in the Colloquium's official program, Verdier was obliged to ask him on the spot and in extremis to give a talk, to make up for the shortcomings of one of the official talks (which had been entrusted to Brylinsk'i, who knew little about D-Module theory). Mebkhout was thus able to set out his ideas and results, and in particular the Good God Theorem, in such a way as to leave no doubt as to the authorship of this theorem, and of the philosophy that goes with it, which had led to the spectacular revival of the cohomology of algebraic varieties, culminating in this Colloque. So, **all the participants in the colloquium were made aware of this paternity**, through this presentation. I also assume that all of them, without exception, have since been acquainted with the Colloquium Proceedings, and in particular with the Introduction and the cited article by Beilinson, Bernstein and Deligne. Not a single one, apparently, found anything wrong with it - or if they did, they didn't let on. Zoghman Mebkhout received no such feedback. So, all the Colloquium's participants can justifiably be considered to be in solidarity with the mystification that took place during the Colloquium.

This collective mystification was already clear at the Colloquium, since no one found anything wrong with the fact that, in Deligne's oral presentation on so-called "perverse" beams, the name of Mebkhout is not pronounced. The speaker confined himself to stating the good Lord's theorem, saying □ **that** he wasn't going to demonstrate this in his talk. He made it clear, moreover (with the modesty with which he

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is accustomed to) that "there was no merit" in guessing the extraordinary and a priori unpredictable properties of the beams he calls "perverse", obviously suggested by the "Riemann-Hilbert correspondence" he had just mentioned<sup>12</sup> (\*). Everyone found it normal that he should refrain from naming the person who had had the "merit" of discovering this providential correspondence, and that he should give the appearance that the author was none other than himself, even though they had just learned, or would learn in the following days, that this was not the case. It must have been some sort of inadmissible misunderstanding that a vague participant in the Colloquium should be the author of such a remarkable theorem, and everyone did their utmost to rectify the situation and establish a consensus which attributed authorship to the one who was clearly the right person for the job - the one who **should have** been the author<sup>13</sup> (\*\*).

<sup>12</sup>(\*) Compare with pages 10 and 11 of the article quoted.

(June 7) For details on the art of escamotage, see the following note "Le Prestidigitateur", n° 75".

<sup>13</sup>(\*\*) (June 5) everything fits together! The reflection that continued in the "l'Elève" procession (following on from the "Le Colloque"), and a certain tone as well (notably again in a recent and brief exchange of letters with Deligne, see first footnote to the note "Les obsèques", n° 70), show me that for Deligne and my other cohomology students, it is clear that

Characteristically, **Mebkhout's paper does not appear in the Colloquium proceedings**. Verdier had asked Mebkhout not to write his paper, saying that the Colloquium was intended to present new results, whereas Mebkhout's had already been published for over two years.

When you don't get bogged down in a technical discourse, and look at what's actually been done, you'll be able to see what's really going on.

p. 291 the forces and appetites that animated uni 'and the others, you'd think you were watching a film about mafia rule in the underworld of some distant ☐Megapolis, It's

The actors are among the noblest jewels of French and international science. The Grand Chef, who runs the operation with his finger on the pulse, is none other than the man who once looked to me like a modest, smiling spiritual son, or at least a (no less modest and smiling) legitimate heir. As for the one who can be drilled and cut, the "soft" one in a world of "hard" ones who don't give quarter, by a strange "coincidence" whose meaning I still don't fully grasp, he too is closely linked to me. He's my "pupil" like the Great Chief (and like him, "pupil" with quotation marks... ) - the one who took me on when I'd already been declared dead and buried for years... .

### 11.1.3. The conjurer

**Note!** 75" (June 7) The "memorable article" (referred to in the previous two notes) displays a consummate art of casual evasion. The equivalence of categories that has been the essential motivation of the whole work is introduced for the first time in a sentence in the fourth rage of the Introduction (page 10, lines 9 to 15), without giving it a name, only to be followed immediately by the kyrielle of consequences for the notion of the so-called "perverse" bundle (pages 10 and 11). No further mention is made until the end of page 16, when we read<sup>14</sup> (\*):

"We would like to point out that on the following points, **which would have found their place in these notes**, we have failed in our task.

- The relationship between perverse beams and holonomic modules. As mentioned in this introduction, it has played an important heuristic role. The essential statement is 4.1.9 (**not proved here**). . . "  
(To continue with other "points that would have found their place. . . ")

I hasten to find out what this "essential statement" is that the authors haven't found the leisure to include in their work, or at least not to demonstrate. Let's look for it:° 4.1.9. ... I'm looking for an "essential statement", a theorem in the form of a scholia, with a reference **where** the authors have demonstrated it or are going to demonstrate it, since they don't prove it **here**. . .

p. 292 But no matter how hard I look, there's no trace of a "theorem 4.1.9" - there's only one passage that answers the number 4.1.9- So I start reading the "remark" at random (without conviction☐- there must be a mistake of numbering. ... ), I read that "the analogue of 4.1.1 in complex cohomology is true... . "Unfortunately, I'll have to go back to 4.1.1 to find out what it's all about. I skipped over it and skimmed through the text that followed - and lo and behold, I couldn't believe it, eleven lines later, a sentence that starts with "We know that... . " and ends with "induces an equivalence of the category . . with that of perverse beams".

Phew - so that was it after all! But no matter how hard I looked, I couldn't find the slightest hint to clarify that cryptic "We know that... . ". Readers who didn't already "know" it must be feeling pretty silly, not to the

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it's been a long time since Deligne should have been the one to discover and master staggered cohomology; and at a certain level (that which commands behavior and attitudes), they're convinced that it's really him, next to whom I'd be a sort of clumsy, clumsy auxiliary who would be more detrimental than anything else to the harmonious unfolding of a theory (leading to Deligne's theorem-ex-Weil's conjectures) and to a distribution of roles satisfactory to all concerned. . .

<sup>14</sup>(\*)Emphasis added.

the situation. What's clear to him in any case (apart from the fact that he's not up to it), is that this result "which would have found its place in his notes", which is "recalled" here in the course of a technical remark - something the reader should know anyway - is obviously due to the authors of the "notes" in question, or to one of them; the most prestigious perhaps and who wrote the article (there's an unmistakable "house style" . ), or the one who gave the oral presentation, whose well-known modesty prevents him from saying "it's me! - but everyone understood without having to say it...

It immediately brings back memories of my reflections over the last few weeks. The very first is Deligne's first work in 1968, which I finally (sixteen years later) took the trouble to look at a little more closely in the note "L'éviction" (n° 63) of April 22 (three days after the discovery of the pot-aux-roses LN 900). Here I find the same style, with variations no doubt due to the intervening thirteen years of "breaking-in". In the 1968 article, whose main inspiration came from me, he names me in passing and in a sybilline way towards the end of the article, just to be "in order". Here, he no longer takes such care - experience has long since shown him that there's absolutely no point! On the other hand, in the article of his young age, since he felt obliged to name me, he compensated by entirely retracting the initial motivation for his work (and the yoga of the weights with it, only to release it under an alternate paternity six years later, while awaiting the exhumation of the motives eight years later still... .). In any case, even hiding (and keeping for his own benefit. ... ) the article's essential arithmetical motivation, it "stood up," this article was perfectly understandable, living up to the author's reputation for doing things ☐ perfectly .

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Here, the theory he develops would be incomprehensible without the heuristic motivation. So he points to the latter, referring to it as "the essential statement", while treating it from under the leg - without honouring it with a name, or a formal statement baptized theorem or proposition, there isn't even a "correspondence" (known as Riemann-Hilbert) - he left that to his friends Verdier and Teissier. He doesn't have to give it a name (given the few<sup>15</sup> (\*) - surely he'd demonstrate it in five minutes!) or name anyone - others will take care of that for him and to his complete satisfaction. There is clearly a yoga, a philosophy, that the author handles with perfect mastery and authority, without having to name anything - this "little" that he pretends to disdain ("which would have found its place in these notes"), he knows full well he'll get more of, as long as he knows how to keep quiet and wait. The first time he played this game successfully, the "few" were "weight considerations" alluded to in a sibylline remark (waiting to bring out the philosophy of weights with great fanfare, six years later). The second time, as far as I know, was when I left in 1970 - the "little" was the "dream of motives", which for twelve years didn't deserve to be honored with a word (just think - a dream, and a dead man's dream at that, not to mention unpublished!), while we wait to discover the real motifs this time (and what we can do with them) and to claim, as modestly as ever, undisputed authorship<sup>16</sup> (\*\*\*).

#### 11.1.4. Perversity

**Note 76** (May 4) I well remember the first time I heard the name "faisceaux pervers", must be two or three years ago, that it struck me unpleasantly, arousing in me a feeling of unease. This feeling reappeared the two or three times I heard this unusual name again. There was a sort of inner "recoil", which remained at the surface of my consciousness and would have been expressed without a doubt (if I had stopped to examine it), but which was not.

<sup>15</sup>(\*) (June 14) To put this "little" in context, I'd like to remind you that Deligne devoted a seminar at the IHES to trying to develop a translation of constructible discrete coefficients in terms of continuous coefficients, without arriving at a satisfactory result.

On this subject, see the note "L'inconnu de service et le théorème du bon Dieu", n° 48'.

<sup>16</sup>(\*\*)For further comments on this technique of "appropriation through contempt", see the following day's note, n° 59'.

then) by something like: what an idea to give such a name to a mathematical thing! Or even

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□ any other living thing or being, except at a pinch a person - for it is obvious that of all "things" of the universe, we humans are the only ones to whom this term can sometimes be applied. ...

It seems to me (although I'm not entirely sure) that it was none other than Deligne himself who first spoke to me about so-called "perverse" beams, when he dropped by my place after the Colloque de Luminy<sup>17</sup> (\*). It must even have been one of the last mathematical conversations between us - there were no others after his visit. It was precisely during this visit that this "sign" appeared, which led me a few weeks or months later (while this sign was reconfirming itself in the exchange of mathematical letters that followed this encounter) to put an end to a communication on the mathematical level<sup>18</sup> (\*\*). (For this episode, see the note "Two turning points", n° 66. )

Coming back to the so-called (wrongly i) "perverse" beams, it's obvious that "normally", these beams should have been called "Mebkhout beams", which would only have been fair. (On more than one occasion, I've named mathematical notions I've worked out and studied after predecessors or colleagues who were much less closely associated with them than Mebkhout was with this beautiful notion - which, incidentally, would seem to me to be more "sublime" than perverse!) The circumstances in which Deligne found himself at the time he was discovering and naming this notion derived from Mebkhout's work, preparing to rob him when he himself was already "fulfilled beyond measure" - these circumstances can rightly be called "perverse". Surely my friend himself must have felt it in his innermost being, at a certain level where one is not fooled by the facades one likes to flaunt. I sense in the attribution of this name (which seems aberrant at first sight) an act of **bravado**, a kind of drunkenness in a power so total, that it can even allow itself to display (symbolically, by the display of a provocative name whose true meaning no **one** will allow themselves to read!) its true nature of "perverse" spoliation of others.

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□ It seems by no means impossible that at some deep level, I perceived the tone of these dispositions in my friend, and that this contributed to the unease I mentioned<sup>19</sup> (\*). This uneasiness was expressed in particular by my inattention to the explanations he had to give me, although I don't think there had been an occasion before this meeting when I hadn't followed what he was telling me with sustained attention, and especially when it concerned mathematics. There was a kind of blockage in me with regard to this notion called (God knows why) "perverse" - I didn't really want to hear about it, even though it was very closely linked to issues I was (and still am to some extent) very close to.

In fact, the whole article by Deligne et al. was typical "**grothendieckery**" and all

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<sup>17</sup>(\*) If this is indeed the case (as I'm now convinced it is) I must give credit to my friend's modesty, for I had no idea (on a conscious level at least) that it was none other than he who had introduced and named them. I had to read the "memorable article" to realize this.

(May 28) To tell the truth, the article doesn't say this any more than it says that Deligne is the father of the Riemann-Hilbert correspondence. However, I had no doubts about his authorship of the term "faisceaux pervers", which was subsequently confirmed to me.

<sup>18</sup>(\*\*) On a purely personal level, this relationship continued in the same tone of affectionate friendship as before, with no apparent change. My friend used to come every other year or so to visit me, usually on some kind of hike. I did have a visit again last summer, which was a welcome opportunity to get to know his wife Lena and their infant daughter Natacha. I think it was on the way back from yet another Colloque de Luminy, about which I've heard very little (apart from a few vague, morose allusions from Mebkhout, who had been given the honor of being invited again, and who could think of nothing better to do than to get back into the game... .). They stayed at my place for two or three days, and the contact was excellent all round.

<sup>19</sup>(\*) I would even be inclined to think that this is indeed the case. On more than one occasion, I've been able to see for myself the extent to which the deepest perception of things is of a finesse and acuity that have no comparison with what skims the surface at the conscious level. The fully "awakened" man is undoubtedly the one in whom these perceptions are constantly integrated into conscious vision and conscious experience - the one who lives fully according to his true means, and not just on a paltry portion of those means.

that could just as easily have come from my pen (with the sole exception of the name of the main concept)! It's something I've already expressed in the second part of the previous note (n° (75)), and something I've also sensed from the moment I read the article quoted - but without this diffuse feeling yet being embodied in the striking observation I've just made. It makes me aware once again, in a striking way, of the profound contradiction of the person who cannot help (in a certain sense

sense) to reproduce and assimilate the very one it is a question of denying, of handing over to disdain - the one it is a question of burying, and who is also at the same time  $\square$  the one one **wants to be** and that (in a certain sense) one **is**.

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The day before yesterday, as I was writing the previous note ("l'Iniquité - ou le sens d'un retour"), I had already been struck by the coincidence that this turning point in the relationship between my friend and me, suddenly impoverished of a communion in a common passion, which had been its *raison d'être* and most powerful mainspring, took place on my friend's return from that memorable Colloque, the meaning of which had just revealed itself to me. What had puzzled me at our meeting in July '81, which on one level was as friendly and affectionate as on the other occasions we met, was this "sign", discreet in tone and air, yet brutally obvious, of a deliberate gesture of disdain. It was like a sort of **down payment** that my friend was making, this time at the level of a personal relationship, on the implicit and equally "discreet" (and just as "brutally obvious") disdain that he had just publicly expressed towards me, as a public figure, at the Colloque de Luminy, in the context of a brilliant display of technical virtuosity between the stars of the day. It was the same "disdain" that had just been expressed (but this time with an altogether different "perverse" brutality) towards the man who had dared (even a little) to claim to be me, and who had thereby condemned himself to be, for my friend Pierre (at a certain level at least), nothing more than "another Grothendieck"<sup>20</sup> (\*) who had to be crushed at all costs. . .

#### 11.1.5. **Pouce!**

**Note 77** (May 5) Another detail struck me as I perused this memorable article<sup>21</sup> (\*\*) which dominated (at this that no less memorable Colloque de Luminy in June 1981. The last chapter, under the suggestive title "From F to C", describes at length a remarkable principle that I had introduced into algebraic geometry twenty years ago - that must have been before the birth of the notion of pattern (which in gives the **most  $\square$  deep** illustrations, via Weil's ex-conjectures). This principle ensures that for some p . 297

In the case of statements concerning schemes of finite type over a body, it suffices to prove them over a finite base body (i.e., in a situation "of an arithmetical nature") to deduce their validity over any body, and in particular over the body of complexes - in which case sometimes the algebraic-geometric result envisaged can be reformulated by transcendental means (e.g., in terms of integer or rational cohomology, or in terms of Hodge structures etc.)<sup>22</sup> (\*). My friend learned this from none other than me and from me, on numerous examples over the years<sup>23</sup> (\*\*). The authorship of this principle (which in an elementary form is even spelled out in EGA IV - don't ask me which paragraph and which number... .) is well known<sup>24</sup> (\*\*\*) . So much so that

<sup>20</sup>(\*) In our personal relationship, my friend calls me by the affectionate diminutive (of Russian origin) of my first name, Alexander, which is also what my family and closest friends have called me since childhood.

<sup>21</sup>(\*\*) See note n° 75 about the "memorable article".

<sup>22</sup>(\*) (May 6) It seems to me that the first example of the use of such a principle can be found in Lazard's theorem on the nilpotence of algebraic group laws on the affine space  $\underline{E}$  (over any body). I was struck by his demonstration, and drew inspiration from it for a number of other statements, as well as for a "philosophy" that has dominated my thinking on pattern theory.

<sup>23</sup>(\*\*) See the note "Eviction" (n° 63) for one such example.

<sup>24</sup>(\*\*\*) (June 5) It is perhaps abusive for me to claim to be the "father" of a principle whose first known application is by Lazard (see previous note (\*)). My role, as on other occasions, was to sense the generality of someone else's idea,

When my brilliant friend was awarded the Fields Medal at the Helsinki Congress in 1978, N. Katz couldn't resist mentioning it in passing in his speech in honor of P. Deligne, thus rectifying a somewhat embarrassing systematic "oversight" on the part of his illustrious laureate. I read this speech just a few days ago, along with the "memorable article" itself.

p. 298 In any case, in this article, the philosophy behind the transition from "arithmetic" to "geometry" is presented in such terms that there can be no doubt in the mind of an uninformed reader that the brilliant lead author (excuse me, I impair. . . ) has only just discovered this wonderful principle of such far-reaching significance.

It's true that I haven't patented the method, and nowhere does my brilliant friend say that he's the brilliant inventor; nor does he claim in plain English that he's the father of that famous "correspondence" (admire the term, which smacks of his nineteenth century!) modestly attributed to Riemann and Hilbert (men worthy of sponsoring the children of such a prestigious successor) - nor does he specify in the "memorable volume" (LH 900) that it was indeed he who invented motives, motivic Galois groups and the whole philosophy that goes with them (and of which he has still only released a fragment). There's nothing to be said either for this famous SGA 4<sup>1</sup>, where I've even been honored to be listed as a "contributor" to this volume, which so brilliantly develops ab ovo étale cohomology, deigning to call on (despite their regrettable gangue of superfluous details etc.) the two satellite volumes SGA 4 and SGA 3.) to the two satellite volumes, SGA 4 and SGA 5, which have been consigned to oblivion, but to which I am generously credited with providing a few technical additions and digressions (some of them even "very interesting")<sup>25</sup> (\*).

In all these cases - and in many other micro-cases I've witnessed over the last five or six years, without it ever occurring to me to **pinpoint my discomfort** and give a name to what I was witnessing or co-acting in<sup>26</sup> (\*\*) - in all these cases, I recognize the same **style**. My friend is always and totally "**thumbed**" - he can help himself at ease, with the complete good conscience that comes from admiring his peers and his blunders (with all due respect), guaranteeing total impunity.

### 11.1.6. The Chinese emperor's robe

p. 299 **Note 77** (May 7) Of course, those who see what my friend Deligne is doing and are in the know at all for the ins and outs, I mean those who haven't just learned about the maths "being done" from the publications of the person concerned himself, or other brilliant (though not always golden) stars of his generation - these colleagues (and they're not that rare after all!) are well aware, at **some level**, of what's going on. They must have sensed, in the "big" cases, that particular uneasiness that I myself have felt on more than one occasion in the face of these "micro-cases" a hundred times less serious than the "big" ones.

and systematize it to the point of making it a "reflex" or "second nature". In the context of the yoga of weights and patterns, it's likely that the first to use this principle was Serre (not me), with his idea of virtual Betti numbers, which set me on the path to just such a general yoga of weights and patterns. (See note n° 46, for Serre's idea in question.) It is

It's also true that it's common practice to attribute the authorship of a "principle" of reasoning that has become commonplace, not to the author, but to the author's "principle".

It's not the first time we've seen a trace of it, but the one who first perceived its general scope, systematized and popularized it. In this sense, we can say that N. Katz's correction (mentioned in the following sentence), attributing the paternity of this principle to me, is justified.

<sup>25</sup>(\*) For details of the "SGA AT operation", see the four notes "La table rase", "L'être part see the four notes "La table rase", "L'être à part", "Le Feu vert", "Le renvers- ment" (notes n° s 67, 67', 68, 68').

<sup>26</sup>(\*\*) The first step towards "pinpointing my discomfort" in a specific case was taken in Récoltes et Semailles less than a year ago, three months ago, in the reflection (which turned out to be quite laborious - and with good reason) "The note, or the new ethic" (section 33). This reflection was taken up again in a note to that reflection, "Le snobisme des jeunes, ou les défenseurs de la pureté" (note n° 27), and then again less than two weeks ago (under the impact of the discovery (the day before) of the "memorable volume" (LN 900)) with note n° 59: "La nouvelle éthique (2) - ou la foire d'empoigne". As I wrote this, a nuance remained in my mind I had no hesitation in using the rather crude term "foire d'empoigne". The discoveries that have followed since have shown me that no hesitation was required.

bigger than these. But what they sensed was so **enormous**, so **incredible** that it must never have surfaced - as it finally began to surface with me, in the course of a **work**, which expressed itself in these two texts around a micro-case referred to in the previous b. de p. note. Indeed, I've never heard of anything like it in the history of our science or any other. Instead of "surfacing", for some people "it" must have **become the norm**, or at least been considered **normal** - as long as an obviously brilliant man, admired by all, practised it with the greatest naturalness in the world, in full view of everyone and without the thing ever (as far as I know) eliciting the slightest comment.

Over the past few days, I've been reminded many times of the tale "The Dress of the Emperor of China", in which the aforementioned emperor, deceived by unscrupulous swindlers and his own vanity, announces that he will appear in a solemn procession wearing the most sumptuous garments the world has ever seen, prepared for him at great expense by so-called tailor artists. And when he appears in the procession, surrounded by the pomp and circumstance of his Court in full regalia, the "artists" bowing and scraping, and the entire imperial family, no one in the procession or among the people gathered to contemplate the seventh wonder dares to believe the testimony of his eyes, and everyone makes a point of admiring and raving about the unsurpassable splendor of the garments with which he is now adorned. Until a small child who had strayed into the crowd exclaimed, "But the emperor is naked!" - and then all of a sudden the whole crowd, as if with one voice, cried out with the little child: "But the emperor is naked!"

And I feel like the little child who believes the testimony of his eyes, even though what he sees is quite unheard of, never seen before and ignored and denied by all.

□ Whether the child's voice will be enough to bring some back to the humble testimony of their healthy faculties, that's another story. A tale is a tale, it tells us something about reality - but it's not reality<sup>27</sup> (\*).

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### 11.1.7. Encounters from beyond the grave

**Note 78** (May 6) It's only been five days since I received this generous package of documents from my friend Zoghman Mebkhout, including above all the two texts already examined from the "memorable Colloquium" - that Colloquium built around a monumental **mystification**! The note "l'Iniquité - ou le sens d'un retour", in which I try to assimilate the quite incredible meaning of this new "event", was written on the very day (the day after May 1st) that I received these documents, still in the emotion of discovery<sup>28</sup> (\*\*).

Since April 19, when I finally became aware of the "memorable volume" of the Notes readings (LN 900 - see notes (51) (52)), this has been the third great discovery on the subject of the solemnities of the Great Burial, and the one that seems to me to be of the greatest significance, both in terms of the light it sheds on the actions of the "Great Burial".

<sup>27</sup>(\*) (June 14) After writing this note, the name "The robe of the Chinese Emperor" struck me as a natural sub-title for the Burial, expressing a particularly striking aspect of it. Later, as the focus shifted to my students as a whole, and even to "the entire congregation" of the Mathematical Establishment, this subtitle seemed less appropriate. However, I've come to realize that the parable that first came to mind when thinking of my friend Deligne, applies equally to all aspects and adventures of the Burial, which at every step reach the Ubuesque in the unbelievable (which everyone makes a point of modestly ignoring) that is nonetheless true. For reflections along these lines, see in particular the notes "On n'arrête pas le progrès!", "Le Colloque", "La Victime - ou les deux silences", "La plaisanterie - ou les deux silences", "La plaisanterie - ou les deux silences", "La plaisanterie - ou les silences", "La plaisanterie - ou les silences".  
ou les complexes poids", "La mystification", "Le Fossoyeur - ou la Congrégation toute entière" (n° s 50, 75', 83, 85', 97), none of which particularly concern my friend Pierre.

<sup>28</sup>(\*\*) Along with the section "The note - or the new ethic (1)", this note is the only note or section I've had to rewrite several times, because what "came out" in the first version (and even in the next one) was weighted down by the inertia of my customary vision of things, which fell far short of the reality I was examining.



of people to whom I have been closely linked, than by its implications as a "tableau de mœurs" of an era, apparently unique (but it is true that I am ignorant of history. . . ).

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The second discovery had closely followed the first - that of the exhumation of the "motifs", for twelve years buried. After the "memorable volume", I was treated to the "memorable seminar" - that "seminar" that never took place, given a bogus name (both SGA and number 4 1/2), and enriched with the "State 0" of a phantom thesis, not to mention a central presentation from the (real) SGA 5 seminar (which appears later, even though it predates it by twelve years); a presentation "borrowed" for the purposes of the operation without further ado. This brilliant operation, and the role it played in the strange vicissitudes that befell this poor SGA 5 seminar (dismantled from the head, the tail and the middle!) were gradually revealed in the course of a reflection that continued between April 24 and 30. (See the five notes "Le compère", "La table rase", "L' Etre à part", "Le signal", "Le renversement", n°s 63", 67, 67', 68, 68'.)

As soon as I had digested this discovery, and as my retrospective reflection on "Mon ami Pierre" drew to a close, and on April 30 I had proudly put the final and definitive mark (that was a sure thing!) on my life, I decided to take the plunge.

- this time I was finally there i) under this interminable Burial, with the "final note" with the doubly euphoric name "Epilogue - ou l'Accord Unanime" - that I receive this package of misfortune, which calls into question final point, epilogue, page layouts and numbering... . A quick glance at the documentation and the accompanying annotations and letters made it clear that my period was gone, as were the beautiful arrangements for a first-class Funeral, the final details of which I was about to polish - I was ready to take up the master of ceremonies' harness. . .

God knows my friend Zoghman had plenty of time to inform me of the situation! It must have been going on for ten years in latent form, and three years at least in "acute form" (and that's putting it mildly) - ever since the Colloquium in question, where he must have sensed the wind without having to wait for the publication the following year of the highly official "Proceedings" under the patronage of his illustrious ex-patron and protector.

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A few months after he had defended his thesis (in February 1979), he had come to bring me a copy to the village where I had lived for six years. Unluckily, I had just left (never to return).  
return, except in passing... ) a few days before, to retire in solitude. He only met my daughter, who later handed me the thesis. It was the following year, I think that we finally got to know each other, in college. in Montpellier, where we chatted for an hour or two. I wasn't really into maths at the time, and couldn't really remember either a thesis I'd flipped through in a few minutes, or the name of its author. That didn't stop the contact from being warm. I remember an immediate current of mutual sympathy. We didn't talk so much about maths (not that I can remember), but mostly about more or less personal things. Zoghman told me afterwards (something I'd forgotten) that he'd been able to explain the D-Modules "philosophy" to me a little, and that he'd been pleased with the meeting, to have felt me "vibrate" if at all by learning new things from him, and yet also (in a way) "expected". What I remember most of all was the impression he made on me - an impression of stubborn, calm strength, that of a "go-getter". At the time, much more than when we met last year or during the correspondence that followed, I had the impression of a strong affinity of temperaments - this "go-getter" side in particular. But the two or three years that have elapsed between the two encounters seem to have dented it quite a bit. ...

I don't remember Zoghman telling me at our first, brief meeting about the isolation in which he had worked, the lack of any encouragement from the "luminaries" who had been my students. If he hinted at it, he must not have insisted. Even then, the whole thing didn't appeal to me.

surprise<sup>29</sup> (\*). I couldn't say whether this was before or after the Colloque de Luminy in June 1981<sup>30</sup> (\*\*). If it was afterwards, he would still have had some hot stuff on his stomach - and he really didn't give the impression of it. Rather that of a man who knows what he wants to do and what he wants, and who follows his instincts.

quietly, without seeking trouble and without being sought out.

□ We didn't continue then to write to each other. But I remembered him well, and early last year I wrote to him I wrote him a note, at random, to ask if he might be in a position to tackle a magnificent work on the foundations of a "moderate topology" which (it seemed to me) was just waiting for someone of his calibre to take it up. Although Zoghman didn't make it clear to me at first, it turned out that he wasn't really interested in this prospect - on the other hand, he seemed happy to seize the opportunity of a new encounter. At the time, I was too out of the loop to fully appreciate the situation, and imagined that D-Module theory was now a done deal, as is, say, coherent duality theory (78<sub>1</sub>), and that Mebkhout had perhaps run out of "big tasks". It was only when we met last summer that I realized that in the very theory he had started, there was no shortage of "big tasks" - and some of them had not even been started, because they had not even been seen!

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In any case, it was a perfect opportunity for a second meeting, and this time not as casual as the first. Zoghman must have stayed at my place for maybe a week last summer, in June I think. Mathematically speaking, our meeting served mainly to bring me up to speed as best we could on D-Module yoga. I've been slow to "thaw out", having lost touch with my old cohomological loves, and being mostly embroiled in the writing of "Poursuite des Champs", which is set in rather different registers. Zoghman wasn't discouraged to see me listening with a slightly distracted ear, and returned to the charge without tiring, with a touching patience. I was finally triggered, I think, when I realized that these famous D-Modules were nothing other than what I had long ago called **module crystals**, and that as such they still made sense in singular spaces. All of a sudden, I saw a whole network of intuitions from my crystalline-differential past rising up from forgotten depths, and slightly rusty reflexes from my "six operations" past being reactivated...

Perhaps it was Zoghman who was a bit of a loose cannon, or maybe it was more that he decided afterwards that he wasn't going to risk his fingers in that particular gear (any more than my friend Pierre wanted to put his - although he'd been all fire and brimstone while I'd been around... ). (⇒ 78)

**Note 78** □ There are, however, a number of "fine" results of consistent duality, notably on the structure of "dualizing differential modules", their relation to "naive" differential modules, and trace and residue applications in the non-smooth flat case, which I had developed in the late fifties which, to my knowledge, have never been published. Nevertheless, for the most part, the theory of coherent duality (in the schematic framework at least), as well as that of stellar duality (and its variant for the discrete cohomology of locally compact spaces, developed by Verdier on the stellar model), or linear algebra or general topology, appear to be theories that have essentially **been completed**<sup>31</sup> (\*), in the nature, therefore, **of tools that are** perfectly perfected and ready for use, and not so much of a **substance**.

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<sup>29</sup>(\*) (May 30) That's not quite true - I'm reprojecting more recent disillusioned dispositions onto the past. When I met Zoghman just last summer, I remember being surprised that none of my cohomology students (Deligne, Verdier, Berthelot, Illusie in particular) had supported Zoghman in his work. This surprise was repeated when Deligne came to see me ten days later (I must have mentioned something to him about Zoghman, but I got no response) and by the

(See "Mystification", n° 85').

<sup>30</sup>(\*\*) (June 3) That was back in February 1980, a year after he defended his thesis.

<sup>31</sup>(\*) (June 12) This is not quite true for stellar duality, until the purity conjectures and the "biduality theorem" are proved in all generality.

that would have to be penetrated and assimilated.

### 11.1.8. The victim - or the two silences

**Note** 78 We met in an atmosphere of friendly trust and affection. This atmosphere, however, did not live up to its promise. I realize now that from that moment on, my friend's trust was far from complete. It was two years after the famous Colloque, and a year after the publication of the "Actes" in *Astérisque*<sup>32</sup> (\*\*)-at a time when he was the victim of a scandalous spoliation.

p. 305 But he didn't bother to inform me until just four days ago! When he came last year, he was returning from another Colloque de Luminy<sup>33</sup> ('\*\*\*') (this time squarely on the theme of D-Modules)'□où<sup>ou</sup> on which he had again generously invited and rushed to attend. He spoke of it in terms both bitter and vague, suggesting that now that he'd pulled the chestnuts out of the fire, it was "the others who had done it all". I could imagine the picture indeed - especially Verdier suddenly remembering the paternity of the triangulated categories (and derived ones too, while we're at it!) he had left to one side for ten or fifteen years, barely tolerating his "pupil" Mebkhout's use of them in his work... (81).

Although he didn't want to explain himself clearly at the time, Zoghman seemed to have his heart set on Verdier, which was understandable given his ex-boss's less-than-encouraging behavior. And yet, my other cohomology students - Deligne, Berthelot, Illusie - hadn't bothered to take an interest in what he was doing or to support him in any way. But it almost seemed as if Zoghman took this for granted, having never (or so it seemed) experienced anything other than this attitude among his elders. If he held a grudge against any of my former students, it was solely and exclusively against Verdier.

From Zoghman's hints (which he obviously didn't want to spell out), I understood that "they" were systematically putting the scope of what he'd done - period. This is, after all, the most common thing in the world. Since judging the importance of a thing is largely subjective, it's commonplace and almost universal to attribute more merit and importance to one's own work, to that of one's buddies and allies, than to that of others, and especially to those one feels like minimizing for one reason or another. (And the "reason" in this case wasn't exactly a mystery to me!) Nothing could have made me suspect that, far beyond such common attitudes, there was here an operation of pure and simple swindling, where there was no question whatsoever of "minimizing", but rather of **swindling** Mebkhout's authorship of the ideas and results that were breathing life back into where there had been stagnation... .

p. 306 And yet, if there was one person in the world to whom it was natural for my friend to open up, it was me. whose work had inspired him during those years of obstinate work, sometimes bitterly, against the fashion of the day - I who received him affectionately in my home, making myself a bit□his pupil at my learning as best I could what he took pleasure in teaching me<sup>34</sup> (\*).

<sup>32</sup>(\*\*) (October 9) Zoghman tells me that these "Actes" were not published until early 1984.

<sup>33</sup>(\*\*\*) (May 7) There's a slight memory lapse here - I think he was getting ready to go to the Colloquium. At the time, of course, there was no shortage of reasons for those "bitter terms" (and vague ones) I remembered. But this bitterness was further heightened by his visit to Luminy after his stay with me. I had echoes of it in a phone call he gave me on his return from Luminy. From that moment on, I had the distinct feeling that he had come to Luminy for the pleasure of being mistreated by "the people" (without really asking me which ones) who had generously invited him, for the pleasure of being able to treat him as a negligible quantity. I must have told him so, or let him know, which must not have improved my friend's attitude towards me.

<sup>34</sup>(\*) Zoghman didn't tell me about mine, and he didn't tell me about his own funeral either, even though he'd had a front-row seat to the proceedings for nearly ten years! To tell the truth, his "protectors" (a little reluctant on the edges) had even agreed to let him carry with his hands a small corner of the coffin carrying my remains - but they couldn't forgive him for being the only one among the guests who sometimes took the liberty of uttering the name that all the others kept quiet!

So my friend must have felt at odds in his relationship with me, and he couldn't find it in himself to take on the responsibility.

After my friend had passed through an atmosphere of warm affection, there was an immediate "backlash". I had the impression that he had decided to transfer to me the mistrust and bitterness that had built up in him over the past eight or ten years, under the sting of the indifference and disdain he had encountered in some of my former students. In the months that followed, the correspondence between us never left the *aigredoux* register - it finally stopped with a New Year's greetings card, which never received a reply.

It was only at the end of March that I contacted Zoghman again, to send him "Le poids d'un passé" and the notes I had then added to this section (n° s 45, 46, 47, 50). It was to ask him if he would agree to my including him, as I had done, in the short reflection on my work (in the note "My orphans", n° 46), when it would be clear to all that I was using information he had given me, and which he might consider confidential. I was by no means sure that my friend would not prefer (like others before him) to "crush rather than displease". It would have hurt my feelings if he had.

It took me a long time to get her answer, which I received only ten days later. I was somewhat expecting her to would still be half-flesh, half-fish - but □ this time she was downright warm. He'd give me his p . 307 I agreed wholeheartedly, even emotionally, with the terms in which I spoke of him.

It's on page 6 of his long (eight-page) letter that he points out, as if in passing and with reference to the "impressive number" of applications of his theorem ("both in the framework of stale topology and in the transcendental framework") that it still appears in the literature under the name of the "Riemann-Hilbert correspondence"<sup>35</sup> (\*). He says it in such an almost incidental way, and with such a delightfully illegible handwriting, that it almost went completely unnoticed! But then I remembered, it really was a strange thing. So strange, in fact, that it hardly seemed believable, and then perhaps my friend was exaggerating, obviously he was angry with everyone, including me, even though I only wanted good things for him. So I added a note (holy Zoghman, I thought I'd finished!) called "L'inconnu de service et le théorème du bon Dieu", in addition to two others "L'instinct et la mode - ou la loi du plus fort" (I'd also thought a lot about him, among others, when writing it) and "Poids en conserve et douze ans de secret". This note on "L'inconnu de service", I wrote at first without total conviction; Zoghman seemed to me so knotted up and full of contradictions that I wondered what I was getting myself into by simply echoing him, without knowing the facts for myself. The thought hadn't occurred to me that there might be a scam, let alone that Verdier or Deligne themselves were involved. There was nothing in what Zoghman had told me to suggest this... .

Yet both of them were so closely linked to this theorem of the good Lord, that its authorship could hardly be concealed without at least their tacit agreement. It must have worked on me in the days that followed. I remembered that Deligne had given it a lot of thought, this problem solved (ten years later) by Zoghman - and then Verdier, after all, acted as research director; even if he didn't go out of his way for his pupil and would rather have beaten him cold and discouraged him than anything else, he

must at least have known what the two main theorems in this work were - Zoghman surely explained them to him, during those famous "interviews" that Verdier □ was kind enough to grant him! I have therefore added to the note on p . 308

a commentary on the relationship of Mebkhout's work to an earlier attempt by Deligne, and a b. de p. note on the role of Verdier. At the same time, it was also a sounding board for my friend

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a past fraught (as mine was) with ambiguity, and speak to me plainly and clearly. Talking about his funeral also meant talking about mine and the role he himself had played in it ... . In any case, if I ended up discovering this famous funeral in all its splendor, it was against a kind of "conspiracy of silence" that encompassed both my friend Zoghman and my friend Pierre - and no doubt most of the friends I had in the "great mathematical world".

(June 3) For further details, see note n° !78" below.

<sup>35</sup>(\*) See the quotation from his letter in the note "A feeling of injustice and powerlessness", n° 44.

Zoghman...

You'd think that Zoghman would jump at the chance to finally, finally reveal his batteries, hidden for three years, which will finally bring out the clear truth and triumph the cause of the oppressed! But not at all! Fifteen days of silence, followed by a letter about everything (in maths) except God's theorem - or rather, he confined himself to giving me the precise reference in his thesis, which I had asked for. (I still wanted to know where this famous theorem, to which I was so firmly committed, had been proved!)

In my reply to this letter, I had to say a few words to him about the "vast swindle with regard to my work" I had just discovered (with the "memorable volume" LN 900, and moreover "promising me much pleasure" in the days to come in making the acquaintance of SGA 4<sup>1</sup> in the college library) - so<sub>2</sub> that after another ten days' silence, my friend finally got in touch!

This time, at last, he "pulled out all the stops" - a **great deal**, in fact, of judiciously-chosen documents, enabling me (who hardly ever haunts libraries, or even the piles of separate prints piling up in my office at university... ) to give me a well-balanced idea of an "atmosphere", in which many of those who didn't take part in my long and solemn Obsèques still remain<sup>36</sup> (\*). Alongside the main "piece of evidence" (the two articles from the famous Colloquium, exposing the incredible mystification), and another "memorable article" (this time from the pen of Verdier<sup>37</sup> (\*\*)). there was the speech by

p. 309 N. Katz on the "Fields Laureate" Deligne, plus a presentation by Langlands and another by Manin at the same Helsinki Congress 1978; then Deligne's "Théorie de Hodge I" at the Nice Congress 1970 (where he is made another allusion in line 3 to a "conjectural theory of Grothendieck's motives" (78<sub>1</sub> ), and "Weight in the Cohomology of Algebraic Varieties" by the same Deligne, Vancouver Congress 1974 (where my name is not mentioned (78<sub>2</sub> )); plus finally a correspondence with A. Borel (yet another old friend, whom I learn at the same time is back in Zurich. . . ), and two notes to Mebkhout's CRAS, one of which from 1980 is a summary of Chap. V of his thesis (passed the previous year), giving a little more emphasis to the theorem of the good Lord<sup>38</sup> (\*). Not to mention another document - shhh! communicated under the seal of secrecy, and of which I won't say another word here. ...

Two letters accompany this substantial dispatch (letters of April 27 and 29), one very long and both substantial. Now that he's finally let the cat out of the bag (the real one, this time!), Zoghman continues to urge me to exercise extreme caution, as he had been doing ever since I contacted him again. If I listened to him, I'd be careful not to make public my reflective notes, which would remain an absolute secret between him and me - at least not the part that implicates anyone, since "they" have "all the power" and "everyone is with them"<sup>39</sup> (\*\*)! And yet, I had warned Zoghman that these notes, from which I sent him the extracts concerning him, were destined to be made public, and as soon as possible.

All the elements seem at last to be in place for the just cause of the oppressed to triumph, but the "victim" seems to be doing everything in his power to continue muddying the waters as if by magic.

<sup>36</sup>(\*) (June 12) Katz, Manin, Langlands don't seem to be part of it. . .

(March 1985) For a different take on Katz, see "Dotting the I's", n° 164 (II5), and "Maneuvers" (n° 169), "Episode 2".

(April 1985) Similarly for Langlands, see note "Pre-exhumation (2)", n° 175 . 1

<sup>37</sup>(\*\*) For more on this article, see "Les bonnes références", n° 82.

<sup>38</sup>(\*) For a precise reference for this note, Mebkhout's thesis and the Good God Theorem, see the note "Le pavé et le beau monde - ou vessies et lanternes", n° 80.

<sup>39</sup>(\*\*) (May 30) Carried away by my impetus, I'm exaggerating a little here. At no time did Zoghman suggest that I refrain from publishing

this or that part of my notes. Lately, he's even been insisting that these notes should actually appear in book form, for the benefit of "posterity", whereas a limited edition like a preprint seems to him to be a bit "like a sword in the water".

secret regret (one would say) at having sold that famous "fuse" of which Zoghman must have been (until the fateful May 2) the one and only holder. This ambiguity is apparent in every line (I'm hardly exaggerating), right down to the latest letters I've just received - including the very last one, in which he sends me, with an air of sombre triumph, the "memorable article" in its entirety (whereas, with the "big package" he first sent me, he had only managed to part with the first twenty pages of this masterpiece<sup>40</sup> (\*\*\*)).

□ As for the friend Pierre I mean Deligne (who is neither Pierre nor "friend" to everyone. . . ), it's p. 310  
it's just that he doesn't sing its emotional praises - it seems that it's no longer he, Zoghman, who's the "victim", but no, it's Deligne, poor fellow, who's been so badly influenced by those around him - the only villain, and the one who surrounded him so badly, is Verdier (and yet. . . follow my gaze instead, . . .): it's clear that I "must have done something" to Verdier for him to be such a coward for the sheer pleasure of doing harm, not to mention the fact that I was also his boss and I was also the one who awarded him the title of doctor and the glory and all the rest - the means, in short, of "absolute power"!<sup>41</sup> (\*)

Clearly, if my friend has a grudge against anyone, it's not really against his illustrious ex-boss, whom he's only had the honor of meeting for an "interview" three times in ten years in all (if I've understood correctly what he wrote to me most recently) - a vertiginously distant man, entirely out of reach - but it's the one he can come and see whenever he pleases, and share both his bread and his lodgings...<sup>42</sup> (\*\*).

Each time Zoghman takes a new step to divulge some new element, making me a little more aware of a situation of despoilment in which he is the victim (and can help a little to unravel it), I feel that it's like a **wrench**, the culmination of an exhausting inner struggle. I

has a **role** with which he seems to have identified body and soul, clinging to it as if it were his most precious possession - a role with which he seems to have identified body and soul, clinging to it as if it were his most precious possession.

this role of <sup>victim</sup>□ which he can only maintain by keeping around this role and the situation that justifies it, p. 311  
absolute secrecy<sup>43</sup> (\*). And he may indeed be torn, and resent me more than ever, at this moment when, with his reluctant collaboration (snatched away, as it were, by the logic of a situation created by none other than me, with those unfortunate reflections on an uneventful Funeral. . . ), this secret will come to an end, and with it, perhaps, this role in which it has pleased him to maintain himself, for how long I cannot say.

This "burial" of my friend Zoghman was achieved by the combined care of **two silences**, each responding to the other and provoking it in turn, in a seamless round in which the role of one closely matches the role of the other - the despoilers and the despoiled. If on more than one occasion I was struck by the fact that the "burier" was at the same time, and more profoundly, his own "buried", I was equally struck by the fact that in the person of another friend, the "buried" was at the same time, and more profoundly, his own "burier" - in close connivance.

<sup>40</sup>(\*\*\*) (October 9) Zoghman told me that, in fact, he didn't have a Xerox of the complete article in his possession at first, which he pulled out only later.

<sup>41</sup>(\*) It's not the first time I've heard this "absolute power" claptrap, with which one would like to convince oneself of one's own powerlessness and justify it. If anyone has invested anyone with "absolute power" over himself, Zoghman, it's none other than Zoghman himself!

<sup>42</sup>(\*\*) (May 8) It's no coincidence, moreover, that the unequivocal signs of conflict in my friend's relationship with me appeared in the very aftermath of this stay, when he "shared my hand and my bed" in an atmosphere of unreserved affection, abolishing a feeling of "distance" that our first brief encounter no doubt couldn't entirely erase.  
Here I come across a situation with which I have long been familiar, and which I discuss (in relatively general terms) in the two notes "The Enemy Father (1), (2)" (sections n° s 29, 30). Little did I suspect, when I wrote them as a commentary on the preceding reflections, the extent to which the archetypal situation I describe there would become the constant focus of a long reflection yet to come, just when I thought I was nearing the end of my journey!

<sup>43</sup>(\*) (May 30) Since these lines were written (May 6), my friend's attitude has changed dramatically, and lately I've seen no signs of attachment to a victim role. It goes without saying that the lines which follow (like those which preceded) concern certain episodes in my friend's life, and in no way claim to define a temperament or describe a permanent bias.

with the very people whose willing victim he delights in being.

And I can see that the person primarily responsible for his own spoliation is none other than my friend Zoghman himself, who for three years has acquiesced by his silence to his humiliation by those who take their pleasure in him. He had everything in his hands to fight for - and for three years he chose to forget he even had hands, and to be defeated without having fought<sup>44</sup> (\*\*).

p. 312 **Note 78<sub>1</sub>** I had never held this short preliminary communication in my hands, but only the more circumstantial "Hodge Theory II, III" publications that appeared in Publications Mathématiques. This is why I had been under the impression that Deligne had not seen fit to ever allude to a role motive theory in the genesis of his ideas on Hodge theory. I thought that if he had wanted to mention any role I might have played with him<sup>45</sup> (\*), he would probably have done so with "Hodge Theory II", his thesis work, which was the perfect opportunity to mention such things<sup>46</sup> (\*\*). I've just seen that he's fulfilled the formality of mentioning me once and for all, with this lapidary line<sup>47</sup> (\*\*\*) alluding to "Grothendieck's conjectural theory of motives", with even a reference at the key (to Demazure's talk at the Bourbaki seminar).

Once again, nothing to say! The idea never occurred to him to specify that he had learned this theory (all conjectural, let's not forget!) from **another source** than this meagre text by Demazure, which can give no image of a theory of great richness (all conjectural!), which runs like a thread through all Deligne's subsequent work on weight yoga - pending the escalation of the "pirate volume" LN 900, where the motivic Galois groups are finally exhumed (fifteen years later) (this time without even a laconic reference line containing the name of the deceased. . . ).

p. 313 On reflection, in this laconic quote, I recognize the same "thumb!" style. - a quote from pure form, to be fair, with a reference that is in no way likely to enlighten the reader (in this case, on obvious and deep-rooted relationships with ideas that it is precisely to hide<sup>48</sup> (\*) - and which have remained hidden during the twelve years that followed), but **of a nature to deceive him**.

**Note 78<sub>2</sub>** I didn't have to hold this<sup>49</sup> (\*\*) text in my hands (which I learned about a few years ago). weeks) to know that my name wasn't on it. Nor was Serre's, who was the first to glimpse a "philosophy of weights", which I later worked out in great detail.

<sup>44</sup>(\*\*) (May 30) This is an admittedly subjective view of someone with the temperament of a fighter, someone in whom this fibre might have seemed absent. It would seem, however, that since these lines were written, my friend's fighting spirit has been reawakened, and he is determined to fight back against an iniquity of which he has been the victim.

(April 18, 1985) For a different, less "harsh" take on my friend's provisions, see also the "Roots" note (n° 171 ).<sub>3</sub>

<sup>45</sup>(\*) (May 30) Until a few weeks ago, I systematically downplayed this role. See note

"Being apart" n° 67' of May 27, where I first became aware of this attitude in myself and perceived its meaning.

<sup>46</sup>(\*\*) (May 30) Nor do I remember being asked to sit on the thesis jury. The funeral was already going well train. . .

<sup>47</sup>(\*\*\*) Serre is also implied in the same line by the cross-reference sign [3] - the curious reader will find his name in the bibliography at Hodge I. This expeditious reference line is the only one between 1968 and the present day where there is any allusion (however cryptic) to the "sources" it mentions in a single breath: Serre (alias [3]), motifs, Grothendieck. ...

(May 28) However, I have since come across another such allusion, very interesting in view of the very special occasion. On this subject, see the note "L'Eloge Funèbre (1) - ou les compliments" n° 104 , and the end of the note that precedes it ("Le Fossoyeur - ou la Congrégation toute entière" n° 97), situating this "particular occasion".

<sup>48</sup>(\*) As I write these lines, I am reminded of a revealing incident involving "weights" two years earlier, mentioned at the start of the note "Canned weights and twelve years of secrecy" (n° 49), and in more detail at the start of the note "The eviction" (n° 63). For the "pouce! style" in general, see the reflection in the note "Pouce!" (n° 76). It's a style with which I'm becoming quite familiar!

<sup>49</sup>(\*\*) "Weights in the Cohomology of Algebraic Varieties", by P. Deligne, Vancouver Congress 1974, Proceedings, pp. 78-85.

### 11.1.9. The Boss

**Note!** 78" (June 3) Zoghman explained to me that he only gradually became aware, and confused at first, of the "swindle" that was going on around my work. The manuscript Verdier had given him in 1975 (see "Les bonnes références" note n° 82) had been providential for him, notably in introducing him & to the notion of constructibility and its essential properties, as well as to the biduality theorem, from which he had drawn inspiration for the biduality theorem (or "local duality") in the context of D-Modules. It was only years later, while reading SGA 5 (a massacre edition, admittedly, but not so massacred as to give an attentive reader like him the slip) that he began to realize something. For a long time, he had been filled with admiration and gratitude for his distant elder, convinced that the ideas he drew on were his own. In fact, it would seem that for years he was convinced that the theory of duality known as "de Verdier" was indeed due to Verdier, or at least to "Serre-Verdier", and that the idea of duality he calls "de Poincaré-Verdier" was also due to Verdier. It was only around 1979 (the year he defended his thesis) that he began to realize that there was something wrong - but I presume he had to be careful not to let anything show about it to his prestigious "boss", nor to me, when we met in February 1980 and June 1983.

It was only with the Colloque Pervers, in June 1981'□ then that he began to sense the escamotage that was p . 314 that he also began to realize more clearly the world he had strayed into<sup>50</sup> (\*)! Surely, for him, I had to be part of that world, where my former students (or at least some of them) had the upper hand and plundered the posthumous pupil with the same casualness as the departed master. The only difference, as it turned out, was that I was dead and they were alive and kicking. ...

I can imagine that even after the Colloque Pervers, Zoghman still found it hard to believe the testimony of his healthy faculties, which told him quite clearly what had happened. He only received the famous Introduction to the Colloquium Proceedings, signed by B. Teissier and his "patron-sic" Verdier, in January 1984. After denying the evidence for almost three years, the shock was all the greater, I understand. It was two months later that I contacted him again, sending him the notes "Mes orphelins" and "Refus d'un héritage - ou le prix d'une contradiction" at the end of March, and it was another month later that he finally decided to "let me in on the joke" and tell me about the "Mystification du Colloque Pervers".

### 11.1.10. My friends

**Note** 79 And here I am about to finish and make public this reflection which will put an end to the secrecy that Zoghman himself has maintained around the spoliation of which he is the victim, and from which he also reaps the obscure benefits<sup>51</sup> (\*\*). Perhaps it will be unwelcome to him, just as it may be unwelcome to my friend...

Pierre, to whom I'll hand-deliver it as soon as it's finished and the text cleaned up and printed<sup>52</sup> (\*\*\*).

The □ best thing I have to offer my friend Zoghman as well as my friend Pierre, perhaps both the p . 315

<sup>50</sup>(\*) Zoghman ended up having such a low opinion of his ex-boss, that he was convinced that everything Verdier had done in the sixties (which I review in a b. de p. note to note no. ° 81 "Thesis on credit and comprehensive insurance") had been more or less dictated or at least blown by me.

<sup>51</sup>(\*\*) (May 30) I would remind you that this reflection was inspired by a disposition in my friend that now seems outdated. (Compare two b. de p. notes of May 30 with note n° 78'.)

<sup>52</sup>(\*\*\*) I didn't think I'd ever have the chance, in the years that lie ahead, to go back for a few more years. days in the capital. But my friend Pierre has travelled often enough, for more than ten years, to meet me deep in the remote countryside, for me to travel on this exceptional occasion, following up on an often-repeated invitation that has never yet been put to good use.



Will they receive it as the worst: as a calamity, or as an outrage. All the worse, since my testimony is public - just as the silences of both have been rites of public acts, and which commit one as they commit the other.

Whether they reject or accept my testimony is their choice, and the same goes for Jean-Louis, whom I counted among my friends, just as Zoghman and Pierre do today. These choices affect me closely, and they are not mine. I have no temptation to predict what they will be. It won't be long before I know, and I await what the weeks and months ahead will bring with intense interest, suspense - and without a shadow of anxiety. My only concern and responsibility is that what I offer is the best I have to offer - that is, to be true.

Some may be surprised that I speak so bluntly of people I call "friends", and see in this name a stylistic clause, or even an intonation of irony that is absent. When I refer to Zoghman Mebkhout or Pierre Deligne as "friends", it's a reminder of the feelings of sympathy, affection and respect that are within me as I write. Respect tells me that I don't have to "spare" a friend, any more than I have to "spare" myself - like me, he is worthy of encountering the humble truth, and no more than me, he needs sparing.

If I don't refer to Jean-Louis Verdier as a "friend", it's in no way because I consider him less "good", or less "deserving", than my friends Zoghman and Pierre, or myself, but because life has distanced us from each other. The feelings of sympathy and affection that bound me to him fifteen or more years ago have more or less faded with time, and have not had the opportunity to be revived by any kind of personal contact. The few attempts I have made to re-establish such contact have met with no response, and I don't know whether reading these reflections will revive a relationship that had frozen. But even though he's no longer a "friend" to me, I don't think I'd be disrespecting him by not treating him more kindly than myself or my friends, and I know that to do otherwise would be a disservice to him or anyone else. Not to mention that both he and my friend Pierre, if any

that they insist on "defending" (or attacking) themselves rather than taking the risk of looking at themselves, are not lacking in means or support. And not to mention that where they have had the opportunity to discourage

or to crush, more than once both have done so, ruthlessly and mercilessly.

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### 11.1.11. **The pavement and the beautiful world (or: bladders and lanterns. . . )**

**Note 80** (May 9) It's about time I finally gave a reference for this famous Riemann-Hilbert-(Deligne qui ne dit pas son nom) theorem - Adam and Eve - bon Dieu - (and especially not Mebkhout), which everyone quotes at length (including myself), and for which apparently nobody has yet thought to ask where it is demonstrated. Having been led to believe by my friend Zoghman that the "me- morable theorem" was to be found in his thesis, I did indeed find it in the table of contents, under the name (admittedly down-to-earth and worthy of a cad) "Une équivalence de catégories", Chap. III, par. 3, p..

75. To make matters worse, it's not even entitled to the name "theorem" but is called "Proposition 3.3" (and what's worse, my name appears, again underlined, on the same page). I'll even admit that, having failed to read the previous 75 pages to recognize myself, I wasn't entirely sure whether this was it - Zoghman confirmed that it was, and I trust him<sup>53</sup>. The demonstration (it would seem) is the subject of Chap V of the same thesis - which was passed at the University of Paris VII on February 15, 1979, before a Jury comprising D. Bertrand,

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<sup>53</sup>(\*) (April 17, 1985) It would appear that the generally used form of the "God's theorem" is not that of the theorem quoted here, but a related form demonstrable by the same methods. See the note "Ecllosion d'une vision - ou l'intrus" (n° 171<sub>1</sub>), and in particular today's b. de p. note therein.



R. Godement, G. Houzel, Le Dung Trang, J.L. Verdier. Interested parties who have not yet received a copy from the author (who sent his thesis to all those he could suspect, rightly or wrongly, might be interested) need only ask him, and he will be pleased to do so... ... Of course, he sent a copy to each of my former cohomology students, none of whom has been heard from since. They must have changed subject in the meantime, unluckily. ...

It has to be said that Zoghman definitely doesn't have the knack of selling his merchandise, of presenting it in a clear and appealing way - these are things that have to be learned, and he wasn't as lucky as my former students to learn the ropes from a virtuoso of the trade who didn't skimp on his time. But he can't complain, he's had his "three interviews", and perhaps one day one of the "luminaries" will have the idea of even acknowledging his indigestible pamphlet. He must have realized himself that the paving stone was not easy to read (even if it wasn't lost on Riemann or Hilbert. . . ): he wrote a note to the CRAS, which is still shorter, to draw attention to his famous theorem, the title of which I'll give you in a thousand:

"On the Hilbert-Riemann problem"! I knew my friend Pierre Deligne wasn't any better at history than I was, so all he had to do was restore the chronological order,  $\square$  and contribute the pretty folklop designation . 317  
 "Zoghman had it coming. ... This note is dated 3.3.1980, Series A, p. 415-417.

Verdier must have learned of the theorem in one of the "three interviews" he gave to his pupil - sic (or at the time of the defense), but he must not have realized it. As for Deligne, he finally realized something, I can't say when, but what's certain is that he knew about it in October 1980, and so did Bernstein and Beilinson, according to what he himself says. Mebkhout himself went to Moscow to explain his results (at length) to Beilinson and Bernstein (in case they had trouble reading him). I don't know if they or Deligne ever read the thesis or the subsequent note to the CRAS, but they must have figured out what was in it, since next year's "memorable Colloque" at Luminy was, coincidentally, all about it.

To sum up, and according to the latest information from my intelligence service, there were at least five people perfectly aware of the situation, who took part in the mystification known as the "Colloque Pervers", namely (in alphabetical order of the actors) A.A. Beilinson, J. Bernstein, p. Deligne, J.L. Verdier and Z. Mebkhout - plus a whole Colloque acultees, surely brilliant mathematicians to boot, who apparently wanted nothing better than to be mystified and take bladders. Mebkhout - plus a whole Colloquium of aculturous people, surely brilliant mathematicians to boot, who apparently wanted nothing better than to be mystified and to take bladders for lanterns<sup>54</sup> (\*). Which proves once again that we mathematicians, from the illustrious Medalist to the obscure unknown student, are not a hair smarter or wiser than the average person.

## 11.2. VIII L' Elève - alias le Patron

### 11.2.1. Credit thesis and comprehensive insurance

**Note 81**  $\square$  (May 8) It seems time to express myself in more detail on the "thesis-" affair. p. 319  
 phantom", which I had only mentioned "in the aftermath" in two previous notes (notes (48) and (63" )). An inattentive or ill-disposed reader might say that I am simultaneously reproaching my ex-student J.L. Verdier for two contradictory things - for having "buried" the derived categories, and for having "published" them

<sup>54</sup>(\*) (June 3) In fact, it appears that all Colloquium participants, without exception, had been briefed on the situation on the spot. On this subject, see the note "Le Colloque", n° 75', written today.

(in SGA 4<sup>1</sup>) and claim authorship; just as the same reader would say that I reproached P. Deligne for both "burying" the motifs, and exhuming them (in LN 900). So it may not be superfluous to give a retrospective of the situation, from 1960 to the present day.

Around 1960 or 1961, I proposed to Verdier, as a possible thesis project, the development of new foundations for homological algebra, based on the formalism of derived categories that I had developed and used in previous years for the purposes of a coherent duality formalism in the context of schemes. It was understood that in the program I was proposing to him, there were no serious technical difficulties in prospect, but above all a conceptual work whose starting point was acquired, and which would probably require considerable developments, of dimensions comparable to those of the Cartan-Eilenberg book of foundations. Verdier accepted the proposed subject. His foundations work continued satisfactorily, materializing in 1963 in a "Etat 0" on derived and triangulated categories, multigraphed by the IHES. This 50-page text is reproduced as an Appendix to SGA 4<sup>1</sup> in 1977.

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(as stated in note (63' "))<sup>55</sup> (\*).

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□ If the defense didn't take place in 1963, but in 1967, it's because it was unthinkable that this 50-page text, the embryo of a foundational work yet to come, could constitute a state doctorate thesis - and the question of course didn't even arise. For the same reason, when he defended his thesis on June 14, 1967 (before a Jury including C. Chevalley, R. Godement and myself, who presided), there was no question of presenting this work as a thesis. The text submitted to the jury, 17 pages long (+ bibliography), is presented as an **introduction** to a major work in progress. It outlines the main ideas behind this work, placing them in the context of their many uses. Pages 10, 11 give a detailed description of the chapters and paragraphs planned for this seminal work.

If the title of Doctor of Science was awarded to J.L. Verdier on the strength of this 17-page text, outlining ideas which he himself says are not his own<sup>56</sup> (\*), it was clearly a contract of good faith.

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□ between the jury and himself: that he was committed to completing and making available to the public this work for which he

<sup>55</sup>(\*) This text alone may seem a meagre result for two or three years' work by a gifted young researcher. But most of Verdier's energy was then devoted to acquiring the indispensable basics of homological algebra and algebraic geometry, by attending my seminars in particular, and by working one-on-one. His contributions to the duality formalism (see below) came later, once Artin and I had developed the stale duality formalism in detail in SGA (1963/64), when I suggested (in parallel with his work on the foundations of derived categories) that he develop the same formalism in the context of "ordinary" topological spaces and smooth morphisms of such spaces.

It was around the time I began my "Séminaires de Géométrie Algébrique" series with SGA 1 (in 1960) that I was contacted by Verdier, along with Jean Giraud and Michel Demazure, asking if I had any work for them - and they were knocking at the right door! A coincidence that struck me from the moment I wrote the note "Mes

Orphans" (n° 46) when the three of them contacted me, they had just formed a small seminar called the "Orphans' Seminar" (on the theme of automorphic functions, approaching calculations with a zinc strand), given that their boss (or godfather to the CNRS?) had just left for a year without warning, leaving them hungry and a little empty. This void was quickly filled... .

<sup>56</sup>(\*) The beginning of the thesis reads:

"This thesis was written under the supervision of A. Grothendieck. The essential ideas it contains are due to him. Without his initial inspiration, his constant help and his fruitful criticism, I could not have completed it. I would like to express my deep gratitude to him.

I would like to thank Claude Chevalley for chairing my thesis jury and for his patience in reading this text. My thanks to R. Godement and N. Bourbaki for introducing me to mathematics.

"The term "this thesis" can hardly refer to anything other than the body of foundational work undertaken, of which the text submitted constitutes the introduction - work which was therefore not, strictly speaking, "completed" at the time of the defense.

(May 30) This inconsistency reflects the ambiguity of a situation for which I was primarily responsible, as thesis director and (if the cover of the copy of the thesis in my possession is to be believed) as president of the Jury. At For me, the lack of "rigor" towards a brilliant pupil is a complacency in the same direction as the one I showed towards Deligne (see the note "L'être à part", n° 67'), and has contributed its share to bringing about the same results.

presented a brilliant introduction. This contract was not kept by the candidate<sup>57</sup> (\*): the text he announced, a text on the foundations of homological algebra from a new, proven point of view, was never published.

Clearly, if Verdier's work between 1961 and 1967 had been limited to writing the skeletal "Etat 0" of 1963, the jury would not have considered accepting this "thesis on credit". The writing of his work had to be sufficiently advanced to allow completion within a year or two, and for practical reasons it seemed appropriate that Verdier should have the title without waiting for the work on which it was based to be completed.

It should be added that between 1964 and 1967, Verdier had made some interesting contributions to duality formalism (81<sub>1</sub>), which, together with the foundational work he was supposed to be pursuing, could justify the credit given to him. His contributions to duality as a whole could, in a pinch, have constituted a reasonable doctoral thesis. Such a thesis, however, would by no means have been in the style of the work I am accustomed to proposing, all of which consists in the systematic development to completion of a theory whose need and urgency I sense (82<sub>2</sub>). I don't recall Verdier ever raising the question of presenting such a "thesis on titles", and I doubt I would have accepted, since such a thesis would have in no way corresponded to the "contract" that was signed between him and me, when I entrusted him with the beautiful subject of derived categories, with the task of developing foundations on a vast scale.

As J.L. Verdier's thesis supervisor and president of the jury, I accept full responsibility for my thoughtlessness in awarding him (jointly with C. Chevalley and R. Godement, trusting the title of doctor on work that had not yet been done<sup>58</sup> (\*\*)).

□ I am not justified in complaining if I now see some of the fruits of my levity. But this p. 322 does not prevent me from stating this publicly, and that the actions of my ex-student J.L. Verdier are his responsibility alone, and that of no one else.

Not to keep the contract he had made with me and with the Jury who had placed their trust in him, was a way of burying the point of view of derived categories that I had introduced and that he had taken on the task of founding through a major work. This work may have been done, but it was never made available to the user. It was a way of "writing off" a set of ideas that he himself had helped to develop.

Mebkhout's revival of the notion of derived category met with no encouragement from Verdier (nor, for that matter, from any of my other cohomological "luminaries"). The de facto boycott of derived categories seems to me to have been total until about 1981<sup>59</sup> (\*), when they made their comeback in force at the "memorable Colloque" at Luminy (see note (75)), under the sudden impetus of need.

However, State 0 of Verdier's "thesis" had already been published four years earlier, in 1977, as an appendix to the

<sup>57</sup>(\*) It is all the more remarkable that J.L. Verdier should have refused my proposal to sit on Contou-Carrère's thesis jury in December 1983, with J. Giraud, and myself acting as research director, believing that the thesis (though entirely written and carefully read by J. Giraud) and the jury would not offer sufficient guarantees of seriousness, without referring to the control of a Commission des Thèses des Universités **Parisiennes** (Sic).

<sup>58</sup>(\*\*) To this responsibility, I should add that of not having ensured, during the two years that followed (before my departure from the mathematical scene) that Verdier actually kept to the contract he had made. It has to be said that my energies were so focused on pursuing the foundational work I had taken on myself, not to mention motivational reflections and the like, that I didn't have to think too much about the unpleasant task of reminding others of their obligations. I had to learn of Verdier's decision to abandon the publication of the planned work in the early '70s, at a time when I was absolutely no longer into maths, and when the idea would not have occurred to me to "react".

<sup>59</sup>(\*) (May 30) These somewhat dubious forms of style are in fact out of place. As Zoghman Mebkhout (who paid to find out) confided to me, what I said about the status of homological algebra "Grothendieck style" corresponds to reality.

volume SGA 4<sup>1</sup> (see note n° 63'') - so ten years after the defense of his thesis, and at a time when (to my knowledge(\*))  $\frac{1}{2}$  Mebkhout was the only one to make use of derived categories in his work, against the fashion of the seven years that had preceded. Unless I'm mistaken(\*) , he remained the only one to do so, right up to the time of the great "rush" around the famous "Riemann-Hilbert correspondence" at the Colloquium already mentioned, where Deligne alias Riemann-Hilbert appeared as the father of this "correspondence" - sic, and Verdier (with his providential Etat 0 abundantly quoted by his generous friend) appeared as the father of derived categories and algebra.

p. 323 homological style 2000, with no mention of my humble self and even less of Mebkhout<sup>60</sup> (\*\*).

□ In light of these events, I believe I understand the reason for the unexpected publication of this State 0 which (as it says in the introduction to SGA<sub>2</sub> 4<sup>1</sup> by the same friend) "had become unobtainable" and that nobody cared about "finding", except (perhaps) Zoghman Mebkhout<sup>61</sup> (\*). So there was just this one unfortunate fellow who, in his own corner and against all odds, persisted in using these notions of a bygone age, without anyone really knowing what he was getting at - so stubborn, in fact, that we began to doubt whether he wouldn't one day come up with something that would do the trick, you never knew... . After all, the man to whom he sometimes imprudently referred as one of his sources of inspiration (alongside the Master's works) had, in the past, proved or found things with all that, things that couldn't be ignored even if the author - and the Master himself - were forgotten, Jean-Louis Verdier himself, had he not made his start to stardom with this "Lefschetz-Verdier" formula, which he would have been hard-pressed even to write down, let alone prove, without all these notions fit for the dustbin. . .

While my influential ex-student of almost ten years (since he had rid himself of a certain annoying formality. . . ) **was betting against** derived categories and would continue to bet against them until time X (of the famous Colloquium), he must have thought it prudent (you never knew. . . ) to pre-empt events that might occur. . . ) to anticipate events that might occur, in other words, to take out "all-risk insurance", by publishing (not the large-scale work that was one day supposed to constitute a thesis, but) a "text-witness", a sort of exhibit "in case... ."; a text that would "attest to his claim to paternity over an **orphan** whom he] had taken a liking to, and whom he continued, pending events, to disown<sup>62</sup> (\*\*).

p. 324 **Note 81<sub>1</sub>** □ The contributions in question are: 1) Foundations of a duality formalism in the context.

of locally compact spaces and 2) that of Galoisian modules (in collaboration with J. Tate); 3) the Leschetz-Verdier **fixed-point** formula; 4) duality in locally compact spaces.

Contributions 2) and 3) are "unexpected" compared with what was known. The most important contribution seems to me to be 3). Its demonstration follows easily from the duality formalism (both for "discrete" and "continuous" coefficients), which does not prevent it from being an important ingredient in the arsenal of "all-purpose" formulas available to us in cohomology. The existence of this formula was discovered by Verdier, and came as a (pleasant!) surprise to me<sup>63</sup> .

<sup>60</sup>(\*\*) Compare with the comments in the notes "Le compère" and "L'Iniquité - ou le sens d'un retour" (n° s 63''' and 75).

<sup>61</sup>(\*) In any case, it was while perusing the bibliography of a work by Z. Mebkhout that I had just received, towards the end of April, that I learned of the publication of this "State 0", when I had even forgotten the existence of this text from another age. ...

<sup>62</sup>(\*\*) If J.L. Verdier had really wanted to make known the yoga of derived categories, which has been buried for seven years, he would have chosen to publish the introductory text which constitutes his thesis, rather than a technical text which nobody cared about and which only acquires interest in the background of yoga and its many uses. But it's understandable that he had no desire to append to the 50-page text the 17 pages of his thesis, containing now embarrassing statements about the role of the one who must not be named. ...

<sup>63</sup>(\*) (April 19, 1985) I return to this beautiful formula, its role and its strange vicissitudes during the Burial, in the three notes "Les vraies maths. . .", ". . . and "nonsense", "Tricks and creation" (n° 169, 169, 169), in the fourth

part of Récoltes et Semailles.

The formalism of duality in the context of locally compact spaces is essentially the "necessary" adaptation of what I had done in the context of scalar cohomology of schemes (and without the difficulties inherent in this situation where everything was still to be done). He did, however, contribute an interesting new idea, that of the direct construction of the functor  $f^!$  (without prior lissification of  $f$ ) as a right-hand adjoint of  $Rf_!$ , with an existence theorem to boot. This procedure was taken up by Deligne in étale cohomology, enabling him to define  $f^!$  in this framework, without any lissification hypothesis.

These comments make it clear, I think, that by 1967 Verdier had demonstrated his capacity for original mathematical work, which of course; was the determining factor in the credit he received.

**Note 81<sub>2</sub>** As another example, I would point to the detailed development of the duality formalism in the context of locally compact spaces, in the spirit of the "all-purpose" formalism of the six operations and derived categories, of which Verdier's presentation at the Bourbaki Seminar would constitute an embryo. Even in the context of topological **varieties** alone, there is still, to my knowledge, no satisfactory reference text for Poincaré's duality formalism.

□(June 5) There are two other directions in which I note with regret that Verdier did not see fit to go all the way.

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the end of work that he had started off strongly enough to **take credit for** (I mean, by starting up a duality formalism in the context of discrete coefficients and locally compact topological spaces), whereas the essential ideas are not due to him and he doesn't care (any more than for the derived categories) to make himself the **servant of a task** and put at the user's disposal a complete formalism (as I tried to do in the three seminars SGA 4, SGA 5, SGA 7).

The duality program that I was planning and that I suggested he develop was part of the general topological spaces (not necessarily locally compact) and applications between such that are "separated" and locally "smoothifiable" (i.e. locally the source is immersed in a  $Y \times \mathbb{R}^n$ , where  $Y$  is the goal space). This was clearly suggested by the analogy with the stale cohomology framework

**any** schemes. Verdier was able to see, in the context of locally compact spaces, that the assumption of local smoothness of applications was unnecessary (which came as a surprise). Nevertheless, the context of locally compact spaces (which excludes "parameter spaces" that are not locally compact) is visibly short-sighted. A more satisfactory context would be the one that would cover both the one chosen by Verdier, and the one I was planning, i.e. the one where topological spaces (or even topos?) are (more or less?) arbitrary, and where applications  $f : X \rightarrow Y$  are subject to the restriction of being 1) separate and 2) "locally compactifiable", i.e.  $X$  plunges locally into a compact  $Y \times K$ ,  $K$ .

In this context, the fibers of an "admitted" application would be locally compact quel- conical spaces. Another step would be to admit that  $X$  and  $Y$ , instead of being topological spaces, are "topological multiplicities" (i.e. topos that are "locally like a topological space"), or even topos of any kind, by restricting the applications in a suitable way (to be made explicit), so as to find fibers that are **locally compact multiplicities**, subject if need be to additional conditions (close perhaps to the point of view of Satake's  $G$ -varieties), for example (and lastly, to the point of view of Satake's  $G$ -varieties).

rigorously!) to be locally of the form  $(X, G)$ , where  $X$  is a compact space with **finite** operator group  $G$ .

To my knowledge, even the "ordinary" de Poincaré duality has not been developed in the case of multi- smooth compact topological cities (smooth: which are locally like a topological variety). The case of a classifying space of a finite group seems to show that we can hardly hope to have a duality theorem (absolute global) other than module torsion, more precisely, by working with a ring of coefficients that is

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a  $\mathbb{Q}$ -algebra. With this restriction, I wouldn't be surprised if Poincaré's duality ("six operations" style) worked as is in this context. It's not surprising that nobody has ever looked at it (except some unrepentant differential geometers, pretending to look at the cohomology of the "leaf space" of a foliage), given the general boycott of the very notion of multiplicity, instituted by my cohomology students, led by Deligne and Verdier.

To put it bluntly, what's missing is a fundamental reflection of the following type: describe (if you can) in the context of any topos and bundles of "discrete" coefficients on them, notions of "cleanliness", "smoothness", "local cleanliness", "separation" for a morphism of topos, enabling us to derive a notion of "admissible morphism" of topos  $f: X \rightarrow Y$ , for which the two operations  $Rf_!$  and  $Lf^!$  make sense (one adjoint of the other) so as to obtain the usual properties of the six-operation formalism. Here, topos are considered as non-ring, or perhaps as provided with Rings (which are assumed if necessary to be constant or locally constant), assuming (initially at least) which ringed topos morphisms  $f: (X, \mathcal{A}) \rightarrow (Y, \mathcal{B})$  are such that  $f^*(\mathcal{B}) \rightarrow \mathcal{A}$  **is an isomorphism** (81<sub>3</sub>). The foregoing considerations suggest that when we restrict ourselves to Rings of coefficients of carac-

(i.e., which are  $\mathbb{Q}$ -algebras), we can be much broader in the notion of "morphism".  
admissible", so as to encompass "fibers" that are e.g. multiplicities (topological or schematic), rather than ordinary "spaces" (topological or schematic).

A first step in this direction (apart from the cases treated by me, then by Verdier on the same model) is due to Tate and Verdier, in the context of discrete or profinite groups. The memory of this first step encouraged me to pursue a reflection along these lines last year, in the context of small categories (generalizing discrete groups) serving as homotopic models. Without going very far, this reflection was nevertheless enough to convince me that there must be a complete formalism of the six operations in the context (Cat) of the category of small categories. (See on this subject the  $\square$  "Pursuit of Fields", Chap.VII, par.136, 137.) The development of such a theory in (Cat), or even in Pro(Cat), just like a theory of this type in the context of topological or schematic spaces and multiplicities, would for me have as its main interest to be a step towards a better understanding of "discrete duality" in the context of general topos.

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Illusie told me last year that he had struggled with duality perplexities in the case of semisimplicial spaces (or schemes). It seemed to me to be the same old tobacco - to be able to detect the existence of a six-operation formalism in a particular case, and to understand it. But it would seem that the mere prospect of a fundamental reflection has the gift of freezing each and every one of my former students - at least among my cohomology students. If I went to any trouble with them, it was with the conviction that they would not stop right where they had left off (in terms of conceptual work) with me, and remain wringing their hands every time a new situation showed that the work they and their buddies had done with me was insufficient. The conceptual work we do is **always** insufficient in the long run, and it's by taking it up again and going beyond it, and not otherwise, that mathematics progresses. Between 1955 and 1970, each year again I found that what I had done in previous years was not sufficient to the task, and I went back to the drawing board, at least when someone else (e.g. Mike Artin) with the point of view of "algebraic varieties" in his sense) hadn't already done so. But it would seem that my students have also buried the example I set them, along with myself and my work.



**Note 81<sub>3</sub>** I seem to recall that in the formalism of the six variances in (say) staggered cohomology, the assumption that the ring bundles serving as coefficients are locally constant is unnecessary - the essential assumption is that they are prime torsion bundles with residual characteristics, **and** that  $f^1(B) \rightarrow A$  is an isomorphism. When we abandon this last assumption, we have to enter a theory (never yet made explicit, to my knowledge) that "mixes" the "discrete space" duality, and the "coherent" duality (relative to the rings of coefficients and their homomorphisms). As a result, we envisage replace, on the diagrams (or more general topos)  $X, Y$ , the rings of coefficients  $A, B$  by relative (not necessarily affine) schemes  $X', Y'$  on  $X, Y$ , and ringed topos morphisms  $\square$  p. 328  $(X, A) \rightarrow (Y, B)$  by commutative diagrams of the type

$$\begin{array}{ccc} X' & \xrightarrow{\quad} & X \\ \downarrow & & \downarrow \\ Y' & \xrightarrow{\quad} & Y \end{array}$$

with a "six operations" formalism in such a context. When  $X, Y$ , etc. . . are the ponc- tual topos, we should find the usual coherent duality.

## 11.2.2. The right references

**Note 82** (May 8) This is J.L. Verdier's article "Classe d'homologie associée à un cycle", published in Astérisque n° 36 (SMF), p.101-151 in 1976. In a way, this rather unbelievable article (although nothing should surprise me any more. . . ) is a counterpart to the "perverse article" by Deligne et al. With one reservation: it practically consists in **copying** over fifty pages, in a slightly different context, notions, constructions and reasoning that I had developed at length ten or fifteen years earlier - terminology, notations, everything is there verbatim! I'd have thought I'd returned to a session of the APG 5 seminar held in 1965/66, where these things were spelled out (apparently to the satiation of the participants<sup>64</sup> (\*) for an entire year. After this seminar, at least, all these things became part of the "well-known" for people in the know<sup>65</sup> (\*\*). Verdier had attended, of course, as had Deligne (the only one who was never left behind, even though it was the first time he'd set foot in my  $\square$  séminaire<sup>66</sup> (\*) - it took p. 329 do it... ). It's true, well, well, that in 1976 the "writing-sic" of this famous seminar by "volunteers-sic" who were fed up with it had been dragging on for ten years - I see now that one of these "volunteers" took charge of the "writing" in his own way, even before the publication of SGA 5 in

<sup>64</sup>(\*)For comments along these lines, see notes n° s 68, 68' "Le signal" and "Le renversement", in which I examine the strange vicissitudes of the writing of this seminar, and the relationship between these and Deligne's "SGA 4 operation<sup>1</sup>", The following reflections are my own.

reveals another unforeseen aspect of these vicissitudes and of the dismemberment of the mother seminary by the combined efforts of Verdier and Deligne. Verdier's and Deligne's publications on this dismemberment date from 1976 and 1977 - they constitute the "green light" given to Illusie to prepare (eleven years later. . . ) the publication of SGA 5 (which, Deligne, dixit in SGA 4<sup>1</sup>, "can be seen as a series of digressions, some of them very interesting").

<sup>65</sup>(\*\*) For a reflection on this "hasty" impression, see the note on "Silence" (n° 84).

<sup>66</sup>(\*) The year of this seminar was (I think) the year I met Deligne, who must have been nineteen at the time. He was "got into the swing of things" very quickly, and even took on the task of writing up my lectures on staggered duality from the previous year (which he must have known from my explanations and notes), and also the lecture on the cohomology class associated with a cycle, which was discussed in the note quoted n° 68' ("Le renversement"), and which will be discussed a little more in this one. The fact that with the means at his disposal, and a complete mastery of the subject, he waited eleven years to write the essay, to include it then in his SGA 4<sup>1</sup> without informing me, now shows me, in retrospect, that as early as 1966 (and not only as early as 1968 as I may have assumed - see note n° 63, "L'éviction") - therefore as early as the first year of our meeting, there was a profound ambiguity in my friend's relationship with me, expressing itself from that moment on in a perfectly clear way, of which I I've refrained from reading it until now!

1977 ! It would appear that the vicissitudes of this unfortunate seminar were not only to Deligne's advantage, as he took advantage of the situation in his own way. But at the time, Deligne still took care to mention in his essay (on the cohomology class associated with a cycle) "d'après un exposé de Grothendieck" (after a paper by Grothendieck), while dismantling SGA 5 from one of his key lectures and attaching it to his SGA 4<sup>1</sup> as a matter of course. (It's true that this compensated him for being able to present me as his "collaborator"! - see note "Le renversement", n° 68).

Coming back to the **homology** class (not to be confused!) associated with a cycle (which, according to the title, is the subject of Verdier's article), I had developed this formalism in great detail, over several presentations, during the oral seminar, before an audience that, incidentally, begged for mercy (except always the only Deligne, always dashing and fresh... ). It was one of the innumerable "long exercises" I developed that year on the formalism of duality in the étale frame, feeling the need to arrive at a complete mastery of all the points I felt needed to be thoroughly understood. The interest here was to have a valid formalism on an ambient scheme that was not necessarily regular - the passage to the **cohomology** class in the case of

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regular, and the link with my old construction using cohomology with supports and immediately giving compatibility with cups-products, being immediate. I also found that □ this part of the seminar does part of the lot that didn't make it into the published version - no doubt Illusie (on whom all the hard work of preparing a releasable (hmm) edition eventually fell) must have been quite happy that Verdier took care of it, mutatis mutandis (i.e. here: without changing a thing!).

As the saying goes, "it hardly needs saying" that my name does not appear in either the text or the bibliography (except implicitly by the ever-present reference SGA 4, which we'd still have to find a replacement for... ). There's no allusion to a "Seminar on Algebraic Geometry" under the acronym SGA 5, which the author might have heard of - although I seem to remember seeing him busy taking notes (like everyone else, except Deligne of course. . . ).

Incidentally, I've exaggerated just a little by saying that my name is absent from the text - it makes a single, mysterious and lapidary appearance on page 38, section 3.5, "Fundamental cohomology class, intersection" (here we come, the crux of the matter!). The reference consists of a cryptic sentence whose meaning escapes me, I confess: "The idea of systematically using weight complexes (??? those damn weights again!) is due to Grothendieck and was put into shape by Deligne" - without any further explanation of these mysterious "weight complexes" whose idea I had and of which I hear mention here for the first time. There will be no further mention of them in the rest of this article (nor was there any mention of them in the 37 pages before). Understand who can! As for the content of the said section, it is copied without further ado from the SGA 5 seminar which had taken place ten years earlier (and by which time this construction was already five or six years old, see note n° 68'), a seminar which he is careful not to quote. The reference to Deligne (who is said to have "perfected" an idea that had already been perfected when my friend was still in high school!) is a "flower", the idea of which no doubt came to the author because the young and newcomer Deligne had indeed taken on the task of writing my paper on this subject (and refrained from doing so for eleven years, for the benefits we know, see note cited). This "flower" is part of the exchange of courtesies between inseparable friends.

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There is, however, one (undoubtedly) new and very interesting result in the article (th.3.3.1., page 9) on stability of discrete bundles analytically constructible by direct higher images through an analytic and proper morphism. Verdier had learned about all-round constructibility from me one day, fifteen years earlier, as well as □ the stability conjecture, which I had asked myself (and told who would listen) in the late fifties, before I had the pleasure of making his acquaintance. Reading the article, the idea wouldn't occur to an uninformed reader (but those are becoming rare. . . ). I

I'm still repeating myself, I'm afraid) that the author isn't serving up hot-off-the-press notions and statements he's only just discovered. He doesn't have to say it's him - because it's self-evident. It's the famous "thumb" style that's so obviously catching on.

Apart from this detail (which, I feel, is in line with the new canons of the trade), there must still be around ten pages (out of fifty), around this interesting result, that present the author's personal work. All things considered, what strikes me most about Verdier, as with Deligne, is that he's perfectly capable of doing beautiful mathematics. Even in this sad article, there's a hint of it in the theorem quoted. But by keeping himself (like his friend) in a gravedigger's mood, he operates, like his prestigious friend, on a paltry fraction of his means. A sign (which astounded me) of apparent mediocrity, in a mathematician who nevertheless gave proof of astute and flair, was the total lack of instinct to feel the scope of the work of his "pupil-sic" Mebkhout, whom he took pleasure in treating from the height of his greatness, without ever having been able to do himself a work of comparable depth and originality<sup>67</sup> (\*). Not that he isn't just as capable as Mebkhout or me. But he has never given himself a chance to do great things, that is, to let go of a passion - rather than **using** mathematics and its gifts **to** dazzle, dominate or crush. Up until now, he's been content to take up as is the fruitful notions and points of view that have already been baked in. Indeed, he seems to have totally lost touch with the meaning of **mathematical creation**.

Yet I seem to remember that when he worked with me, that sense was still there. Nothing external about him prevents that sense from resurfacing. Just as in his friend, in whom I often felt this same eclipse of something delicate and lively, obturated by the same fatuity. .332

This incredible 50-page article, which appeared in a standing magazine, sheds new light on the "The note - or the new ethic" incident (s.33). where a note to the CRAS of **a few pages**, summarizing a solid and **original** work, on an important subject (in my humble opinion), the fruit of **two years' work** by a highly gifted young mathematician, was rejected by two eminences as "devoid of interest"<sup>68</sup> (\*). One of these eminences was none other than Pierre Deligne - the same Deligne who did not disdain to copy in toto and in person the humble doctoral thesis of one of my students (whom he made a point of quoting). (This duplicate, enhanced by a prestigious signature, makes the largest article in the "memorable volume" LN 900 of a no less prestigious collection! See end of notes (52), (67)).

The "tableau de mœurs" is growing by the day, without my having to come out of retirement and hit the streets to mingle with the "big world". A few hours spent here and there leafing through a few well-chosen "great texts" were enough to edify me...

### 11.2.3. The joke or the weight complex

**Note 83** ( May 8-9) I've been thinking about this "weight complex" referred to in the "reference - thumb" in Verdier's memorable article<sup>69</sup> (\*\*) - a reference that's sheer nonsense. As soon as I saw this ludicrous reference, an association came to mind that kept running through my head. This isn't the first time, far from it, that I've found myself faced with something

<sup>67</sup>(\*) The same astonishing lack of flair was evident on this same occasion in Deligne, who didn't "feel the wind" (the importance of Mebkhout's ideas) until 1980 it seems, even though Mebkhout had been working in this direction since 1974. On more than one occasion, I've had occasion to observe my friend's natural flair blocked by sufficiency, especially since 1977 (or 78), which seems to have been a first "turning point" (see notes "Two turning points" and "Funerals", n° s 66,70).

<sup>68</sup>(\*) For details, see the note "Casket 4 - or topos without flowers or wreaths", n° 96.

<sup>69</sup>(\*\*) See previous note "Good references".

that seem to defy rational explanation - even though the meaning is clear and unambiguous and clearly perceived, but at a different level from that of conventional logic. This was the only one on which I had consciously operated for most of my life - with the result that I was constantly overwhelmed by "bizarre", incomprehensible events - distressing in their irreducible saugrenousness! My life changed a lot from the moment (less than ten years ago) when I started

p. 333 to live on a wider register of my faculties. I've come to understand that every absurdity, every so-called "nonsense" has a **meaning** - and the mere fact of <sup>□</sup>knowing this, and from then on being curious about the meaning behind the nonsense, often opens me up to its obvious meaning.

In this nonsense about "weight complexes", I think I sense an act of **bravado** of the same nature as in the appellation "faisceaux pervers"<sup>70</sup> (\*) - the pleasure, in this case, of proving to oneself that one **can afford**, in a journal of standing and in a text that claims to be a standard reference text<sup>71</sup> (\*\*), to say a related nonsense, and that **nobody** will dare to even ask a question! And I'm convinced that the wager contained in this bravado, in the eight years since the article appeared, has **been won to this** very day: that I was the first today to put the naive question to the author.

Of course, the time (or place) at which a saugrenuité appears, in this case at the precise moment of the one and only reference to my person, is by no means coincidental; nor is the form it takes, here by allusion to a type of notion, "weight", entirely foreign to the theme of the entire article, and by the improvisation of a composite notion "weight complex" that never existed! The association that immediately presented itself to me could well provide the key to the more precise meaning of the saugrenuité, beyond the bravado, the demonstration of power. It's the association with an allusion just as sibylline and just as purely formal (but without yet having the added dimension of saugrenuité!) in Deligne's article quoted at the beginning of note (49)<sup>72</sup> (\*\*\*). It was an obscure allusion, in an article where the word "weight" was rigorously absent and where nobody but Serre or I would have been able to see them, to "weight considerations" which had led me to conjecture (in a less general form, it is clearly stated) the main result of the work. As I explain in the more detailed note "The eviction" (n° 63), behind this rhetorical allusion lies the intention to **conceal** both my role, and the ideas (concerning "weights" and "weights") that I'd been working on.

p. 334 their relationship to cohomology in general, and Hodge's in particular) which he intended to reserve for himself alone. This intention must have been all the more clearly perceived by Verdier as he himself <sup>□</sup>me "operates" on the same diapason (in his relationship with me, at least, which seems to me to be the main cement between the two inseparable friends). In either case, an honest presentation would have consisted in starting the article by clearly indicating the source(s) for the main ideas, or for the question(s) that motivated the article.

Having said this, here's the meaning I see behind the symbolic language of apparent nonsense: I can allow myself, without the slightest embarrassment, to display patent **nonsense** in front of everyone, and at the same time express through this nonsense my true intention, with this absurd allusion-reference to the "weight complex": that is, I have no more intention of revealing anything about the role of Gr. in this work, any more than Deligne had such an intention with his empty allusion to "**weight** considerations"-which allusion made no more sense to the reader then than it does now with the imaginary "**weight-complexes**" I've just mentioned.

<sup>70</sup>(\*) See "Perversity", n° 76.

<sup>71</sup>(\*\*) And it would seem that this text is indeed a standard reference today - or at least it has been for years.

was one of Zoghman's bedside texts (he recently sent it to me). It was there that he learned about the notion of constructibility (which plays an essential role in his theorem), and for a long time he was convinced that Verdier was the brilliant inventor of this crucial notion for him.

<sup>72</sup>(\*\*\*) This is the note "Poids en conserve - et douze ans de secret". For a more detailed examination of this Deligne article from the point of view that interests us here, see "L'éviction", note n° 63, quoted below.

to invent right now, for the sake of the cause and my own pleasure!

I've just posted this note, written yesterday - I was interrupted earlier by a phone call from Verdier, whom I'd tried to reach during the day, to ask him just that question. I explained to him that I was trying to learn a little about cohomology, something he knew I'd never understood, and that Mebkhout had passed on to me for my instruction an old article by him, Verdier, a work that had long served as his bedside text. I was now trying my best to read it, but there was this cryptic reference - it was nice of him to quote me, of course - but I had no idea what he was talking about.

He was quite happy, even a little flattered, but yes, with a broad smile that protruded behind an air of paternal joviality, that I'd end up like this in my old age, learning cohomology on this old paper of his. I didn't expect him to contradict me when I said that he knew I'd never understood anything about cohomology - obviously that had been agreed long ago. ... As for those famous "weight complexes", I could feel his broad smile again at the end of the line (I'm making it up!), delighted that someone (and the addressee himself, no less) had finally picked up on his point.

something that had gone by the wayside for so long. At the same time, there was also a hint of embarrassment - more the embarrassment (I think) of not having been able to hide from a  $\square$  pleasure (like the pleasure one would take in ap

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slightly salacious story... ), than not knowing what to answer. Dumped as I was, he really didn't have to worry about that! Without a moment's hesitation, he turned to Deligne (whose name I hadn't mentioned), who had given a demonstration in one of his articles, in which he also quoted me (he couldn't quite remember where) - in any case, it was a question of weights, but yes, he'd forgotten a little, of course - but not arithmetical weights, because I was quite right, it wasn't the same....

His tone was jovial and unapologetic, and he made it clear that he'd already given me quite a bit of his time - with a slightly hurried air, but without losing his debonair, slightly protective tone. I apologized for bothering him like that, with a rather stupid question, and thanked him for his explanation. My apologies were sincere and so were my thanks - he had indeed taught me everything I wanted to know.

<sup>73</sup>(\*).

## 11.3. IX My students

### 11.3.1. The silence

**Note 84**  $\square$  (May 9) I was perhaps a little brisk yesterday, writing that in "the correct reference" (see note (82)) p. 337 what the author and ex-student shamelessly recopied as "part of the realm of the 'well known' for those in the know". I tried to explain who these "people in the know" were - with the conclusion that **they were no more, no less, than the dear listeners of this SGA 5 seminar** in 1965/66 - listeners, as I've had occasion to say, and judging by the vicissitudes of the writing of this seminar in the hands of volunteers whose lack of conviction I hadn't wanted to sense, it was often more "more" than "less" (always with the exception of the same Deligne ; of course). Indeed, there was no risk of other people "getting involved" as long as SGA 5 had not been written and published, precisely to enable people to "get involved" by reading it! This seminar was in fact published (as fate would have it) after the two "memorable

<sup>73</sup>(\*) Even with my droopy airs, I didn't really feel like I was putting on an act (I don't have the gifts for it), it was perfectly natural - in truth, I'm a bit droopy in all this stuff I haven't handled for nearly fifteen years! But I think that even when I'm old and ripe for the hearse, I'll still be able to feel the difference between an empty walnut and a full one. .

publications" by two of my dearest students and comrades-in-arms, namely the article in question by Verdier in 1976 (in which he says nothing about the origin of the ideas he develops, published there under his pen and for the first time), and Deligne with SGA 4<sup>1</sup>, which has already been discussed at length<sup>74</sup> (\*). After that, we cordially invite Illusie to take care of publishing the rest!

I can't remember in detail who took part in this seminar - whether Artin was there or not, for example. I think that more or less all my students from the first period must have been there in any case.

- with the exception of Mrs. Sinh and Saavedra (whom I hadn't met at the time) and perhaps Mrs. Hakim. There was also Bucur (since deceased). Houzel, Ferrand - I'm not counting Serre - who never had a taste for big cohomological fuss, and who came to put his feet up cautiously from time to time. While no one except Deligne perhaps had a good sense of where all this was leading,

it seems to me that there must have been ten or twelve listeners (not very involved) who were at least

p. 338 following enough to be considered "in the loop" ☐ <sup>the</sup> thought that ran through my head since yesterday, it's that among all these people "in the know", thus representing cohomological expertise (if not all "luminaries" like Illusie and Berthelot, with their "cohomological" theses that were decidedly weighty), and even apart from Verdier and Deligne - there must be quite a few who've had Verdier's article in their hands! A certain air in Verdier gives me the conviction that nobody ever suggested to him that something might be wrong. And I also know that nobody ever drew my attention to it - I learned of the existence of this article on May 2. exactly a week ago today, thanks to Mebkhout, who of course had known about the scam for years.

This gives concrete meaning to the euphoric observation of the "Unanimous Agreement" (to bury my modest self) made ten days ago (note (74))! This agreement encompasses many (if not all) of my "pre-1970" students, i.e. many of those who today set the tone in the mathematical world; and it includes (or has included) my friend Zoghman himself, treated as a Cinderella by the beau monde and clinging against all odds to a kind of "fidelity to my work" (to use his own expression<sup>75</sup> (\*)), which he has had the temerity and obstinacy to claim for himself at times, with the consequences that we know. Go figure!

In short, I was wrong to suggest that such and such a standing journal published a sort of "boilerplate" article that merely copied what was "well known". What the author was copying in full view (if not of everyone, but) of many witnesses was neither published nor "well known" (except for the cohomology class of a cycle in the coherent framework, where I had published it ages ago); and these were additional ideas that I would be remiss to play down, given that I didn't consider it a waste of my time to spend a year developing these and other ideas in a seminar, in front of a large audience. Verdier's article is probably a useful and well-done "digest" of a small part of the ideas and techniques I had developed: precisely so that they can be passed on to a wider audience.

the realm of the "well-known", the daily bread of those who use cohomology (or homology) for objects that more

p. 339 or less deserve the name "varieties" ☐ From this point of view, then, Verdier has done what was useful to do<sup>76</sup> (\*), and in the end I have no reason to be unhappy. However, from what I sensed from my ex-student

<sup>74</sup>(\*) See notes n° s 67, 67', 58, 68'.

<sup>75</sup>(\*) (June 7) Reading all the notes on L'Enterrement during a recent visit. Zoghman points out that this expression that he had used of "fi délité à mon oeuvre" didn't really capture his thoughts. Rather, he had in mind a ~~confidence~~ in his own judgment and mathematical instincts, which told him that my work provided him with some of the ideas he needed. It's all about **self-confidence, which is** essential if you want to do something truly innovative.

<sup>76</sup>(\*) He did so, it's true, at the expense of the "dismantling" of the original SGA 5 seminar, of which he and Deligne were the main players and "beneficiaries".

(June 7) The reflection of May 12, three days later (see note "The massacre" n° 87) made it clear that Illusie was even more directly associated than Verdier with what appears to be more a "massacre" indeed than a dismantling - even if he

and friend even today, on the phone, and through many other things I've sensed from him (the "biggest" of which, or at least the most "spectacular", is the mystification of the Pervers Colloquium) - I can feel that **something is amiss**. That memorable Colloquium was certainly brilliant, mathematically speaking, in many respects. What's "wrong" is at a completely different level. I could try to define it in words, but I'm afraid that wouldn't make much sense. Anyone who doesn't feel what's wrong with this Colloquium - and I'm sure with many other Colloquia too, without mystification or anything - won't feel it a hair more, once I've made this attempt to "pin it down" and even succeeded in doing so to my complete satisfaction... ..

The question that remains open for me is whether this "sign" represented by what is undoubtedly a relatively common occurrence today (of an author presenting as his own the unpublished ideas of others) - whether this sign is that of a general degradation of morals, So, is it just a typical sign of a "spirit of the times" in today's mathematical world, or does it have more to teach me about myself - about the person I once was, and who is now coming back to me, through the attitudes towards me of those who were my students?

The two possible meanings are by no means mutually exclusive. My ex-students' relationship with me could not have found this way of expressing itself, if a certain state of morality didn't encourage them to do so. In fact, even before this "sign", I saw many others that seem to me to be even more eloquent in terms of a "picture of morals". What struck me about this sign is the particularity that sets it apart from all the others: it seems to **involve most of my former pupils at the same time**.

Such a circumstance cannot be fortuitous. To simply put it down to a "deterioration of the mores" (all that's real) would be a way of evading its more personal meaning, which implicates me as a "person".

it involves each and every one of my ex-students. If I say "each", which seems to go beyond the actual amplitude of this

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sign, I'm weighing my words carefully. For this sign is a timely reminder that it is scarcely conceivable that one of my former students has not at least been confronted with situations of this kind. For years now, I've felt a certain "wind" about me, blowing through the world of mathematicians I've left (a wind whose origin and reasons I now clearly see, it seems). There's no way that any of them could have failed to feel the breath of this wind, be it on the occasion of an "incident" such as the publication of this gravedigger-article, or on any other occasion. Whether he wanted to or not, such an encounter inevitably raised (or raised again) the question of his relationship with me, who had taught him his trade. And the sign I've noticed, beyond the one that just brought me to this point, is that **I haven't heard a single echo on this subject from any of my students**<sup>77</sup> (\*). This is a "coincidence" the meaning of which still escapes me - but which cannot fail to make sense (84 ).<sub>1</sub>

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The day is dawning - I feel it's time to stop. I'm not sure that this is the time and place, in Harvest and Sowing, to pursue further the meaning of this striking coincidence. It's a harvest perhaps reserved for other tomorrows, if my reflections of this night meet with an echo in one or other of those who were my students.  
⇒ 85

**Note 84<sub>1</sub>** (May 16) This perfect agreement between my former students, in this complete silence towards me, goes in the same direction as other signs. One is the complete silence that also greeted the episode "Les étrangers" (see section 24) - a silence I have already pondered somewhat in note n° 23v.

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was not the "beneficiary" and that he acted on behalf of others.

<sup>77</sup>(\*) (May 31) Interestingly, the one and only person who ever hinted at the existence of a funeral was an African friend of mine who had done a 3° cycle thesis with me about ten years ago (so "post-1970 student", and of modest status), with whom I have remained on friendly terms. The letter in which he implied this must be from two or three years ago, at a time when I wasn't at all surprised. I didn't then ask for details about his impressions, which he has only recently returned to.

## 2. A walk through a work or the Child and Mother

On the other hand, with the exception of Berthelot, who sent me numerous separate prints, and Deligne, who sent me four (out of some fifty publications) and one from Illusie, I haven't received separate prints from any of my former students. That says a lot about the ambivalence in their relationship with me. Send prints to

p. 341 part, even though it was doubtful whether I would ever make use of it in my work<sup>78</sup> (\*), would have been the way to

p. 341 to let the person who had taught them their trade know that this trade in their hands did not remain inert, that it was alive and active. But it is also true that for at least some of them, their publications also testify to their participation in a tacit burial of which it was better not to inform the anticipated deceased, trade or no trade... . On the other hand, I have received numerous offprints from several authors working in crystalline cohomology<sup>79</sup> (\*\*), and even a good number of offprints from fellow analysts whom I hardly know by name, when their work takes up (and sometimes solves) questions I had asked thirty years ago or more, when it was obvious that I would not return to the subject I had left and that, from a "utilitarian" point of view, they were wasted offprints. But these colleagues must have sensed something that my students didn't want to sense. - Of course, in the sixties, my students were the first to be served for all my publications, both my articles and the great EGA and SGA series, and every one of them (except Mrs. Sinh and maybe Saavedra) must be in possession of my complete work published between 1955 and 1970 (in the ten thousand pages I presume).

It's true that my ex-students are in good company: none of my former close friends in the mathematical "big world", including those whose work is closely related to mine or who played a role in the development of my program of work in the sixties, have seen fit to conti-...

continue to send me separate prints after my departure from<sup>80</sup> (\*\*\*). Only recently, among

p. 342 the fifteen or twenty friends of yesteryear (including some students) to whom I sent the Esquisse d'un Programme

p. 342 (which, among other things, announced the resumption of intense research activity, after a hiatus of fourteen years and on research themes closely related to those we used to pursue together), only two (Malgrange and Demazure) took the trouble to send me a few lines of thanks. The few more detailed (and, what's more, warm) feedbacks I've received have come from young mathematicians I've only recently met, and from my old friend Nico Kuiper, who is in no way connected to the kind of things I do. He found out about the text via an intermediary, and was delighted with my unexpected "homecoming"<sup>81</sup> (\*).

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<sup>78</sup>(\*) (May 31) This may even have seemed out of the question until 1976, when I made it quite clear in the early '70s that I had no intention of ever returning to mathematics. The lecture I gave at the IHES in 1976, on De Rham complexes with divided powers, showed quite clearly that I was still interested in mathematics.

<sup>79</sup>(\*\*) (May 31) These are young authors whom I don't know personally, and I presume they've followed the example of Berthelot, who for them must be a figurehead. The strange thing here is that for at least the last two years (since the Colloque de Luminy, September 6-10, 1982), Berthelot is actively trying to bury me (see on this subject the b. de p. note of May 22 to the note "les cohéritiers. ...", n° 91) - could this be a recent turning point in his relationship with me? I don't recall receiving the offprint of the article-survey on crystalline cohomology et al. in which he passes my name under silence - he had to be careful not to send it to me!

<sup>80</sup>(\*\*\*) (May 31) Of course, the psychological reasons that might have prompted them to send me some were far less strong than in the case of my students - but, one might naively think, far stronger than among my fellow analysts, or even among the many algebraic geometers whose prints I received separately, and whom I know little or nothing about personally. Clearly, after my departure from the common milieu, the fact of having been friends has created or reinforced, in my former friends in the mathematical world, the automatisms of rejection that I have had occasion to observe. (On the subject of these attitudes, alluded to in passing here and there in Récoltes et Semailles, see the note "Le Fossoyeur ou la Congrégation toute entière" of the 24th.

May, n° 97.)

<sup>81</sup>(\*) (May 31) This is almost the only echo from an old friend of mine (or one of my former students) in the of acquiescence to my "homecoming". This is hardly surprising, given that the appearance of the deceased unseemly interrupts the normal course of a funeral ceremony... ..

(June 17) However, I recently had the pleasure of receiving a warm letter from Mumford, who says he is "thrilled" and



"very excited" by the ideas sketched out in the Esquisse, and who confirms that the key technical result I needed was

### 11.3.2. **solidarity**

**Note 85** (May 11) This story of the ill-fated SGA 5 seminar keeps running through my head. The "good reference"<sup>82</sup> (\*\*) definitely sheds new light on this story, and at the same time gives new meaning to the brilliant "SGA 4 operation"<sup>1</sup>.<sup>2</sup>

The more I think about it, the **bigger** the SGA 5 story seems. My first impression, when I "disembarked" just a few weeks ago (see notes n°s 68, 68'), was that a situation of debandade among the poor ex-auditors of this seminar in 65/66 had been put to good use in his own way by my friend Pierre, for his famous operation, and that no one else had anything to do with it. And as for the misfortunes of SGA 5, this was neither he nor anyone else, but rather "ut other than myself, who had not, alas, known how to enthuse my volunteer-editor listeners, nor do for them the □ work they stubbornly refused to do while saying they p. 343 were about to get down to business. Then it turned out over the last few days that there was one, after all, whose enthusiasm was reawakened ten years later, to publish (without reference to the seminar) what he liked to take from it, thus creating a good reference for his own account, at a time when the other "volunteers" still hadn't decided to get going.

What's become increasingly clear to me since yesterday is that it's not just two "villains", but **every single one of my "cohomologist" students** who are directly involved in the cover-up that took place at this seminar. Unless I'm mistaken, every single one of them attended this seminar - namely (in chronological order of appearance of my "cohomologist" students): Verdier, Berthelot, Illusie, Deligne, Jouanolou. (I'm not counting Jean Giraud, who operated on quite different registers from those mostly discussed in SGA 5 or its predecessor SGA 4).

This seminar, which I did **for the benefit of my students** in the first place, and even though they sometimes asked, grace - **I consider that it wasn't crap**. Every one of them, during that year, learned a good deal about his job as a "cohomology user mathematician"! The things I was doing to them, taking up in the spread-out framework and in a much more circumstantial way ideas I'd first developed in the coherent framework - these things they couldn't find anywhere else but in that one seminar made for their benefit, given that nobody before me had ever bothered to do them - and that nobody but me even felt what there was to do, and why. (Except always Deligne, who learned it over the months in this very seminar, being quicker on the uptake than the others). It was having taken this seminar (and the previous one) and having worked on it at home as best they could, and nothing else, that meant they were now "in the know" about duality formalism, and they were **the only ones** to be. This **privilege**, it seems to me, created an **obligation** for them: to ensure that this privilege did not remain in their hands alone, and that what they had learned from me, and which has been indispensable baggage in all their subsequent work up to the present day, was made available to all, and this within a reasonable and customary timeframe - of the order of a year at most, or even two at a pinch.

□ We'll say, not without some reason, that it was up to me above all others to see to that. But if I accepted in good faith when students and other listeners offered to help with the writing (which, for those who took it seriously, could only be good for them) - not for the benefit of being able to twiddle my thumbs while they did a job that fell to me. I've

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for my combinatorial description of Teichmüller's tower is well and truly proven. This is the first time since 1978 that an old friend of mine has latched on to my "Anabelian" ideas, whose exceptional scope (comparable to that of pattern yoga) has been obvious to me since the very beginning. ...

(March 28, 1985) Since these lines were written, I have also received a very warm letter from I.M. Gelfand (dated Sept. 3, 1984), in response to the Esquisse.

<sup>82</sup>(\*\*) See note n° 82.

continued, with the help of Dieudonné and others (including, incidentally, Berthelot and Illusie in 1966/67) to develop' basic texts that seemed equally urgent to me, and that no one else would have done in my place or without my assistance<sup>83</sup> (\*). These texts have themselves become indispensable references, including for my "cohomology students", who are just as happy as everyone else to find them ready when they need them.

With the mastery of cohomological ideas and techniques they had acquired through their work with me and the seminars they had attended or participated in, writing this seminar through their joint efforts represented a task of derisory dimensions, if we compare it with the service that was being rendered to the famous "mathematical community", or perhaps also, later on, with an obligation of loyalty they might feel towards me. I've already said that for me (the one with the helping hand), it must have been a job of the order of a few months to write the entire seminar. By dividing the work between the five of them, with the writing experience they had each acquired in those years, and with my detailed manuscript notes at their disposal, the investment for each of them was of the order of a month or two at the very least. They were much better equipped to do this than other editors, such as Bucur, who would have liked nothing better than to entrust a task, which was clearly beyond him, to younger, more directly motivated hands.

As long as I was around (so in the three years that followed), I can see how a "leave it to me" reflex might have come into play - I was supposed to coordinate everything and deal with the "volunteers". It's likely that if I'd asked each of them to give two or three presentations

within a short space of time, it was up to me to do the same, and finally get it over with, they wouldn't have recused themselves. It was from the moment I withdrew from the mathematical world that the situation changed completely. They

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found themselves **the sole trustees of a certain inheritance**, both implicit (in the absence of a will) and very concrete. It's true that, from a practical point of view, my departure was tantamount to a **disappearance** - I was indeed "deceased", in the sense that there was no one outside them to know about the inheritance, to be able to use it and to be concerned (for better or for worse. . . ) about its fate.

If, for the seven years following my departure, this heritage remained hidden (apart from "the good reference" in 1976!), it's because **my students didn't want it to become public during that time**. All things considered, the situation doesn't seem very different to me from that of the "yoga of motives", which was thoroughly known only by Deligne (apart from myself), and which he saw fit to keep to himself for his own benefit. If there is a difference at first sight, it is that in this case there is only one "beneficiary" instead of five, and that there is no common measure between the depth of what was, concealed by one, and what was jointly concealed by the five.

I certainly don't know everyone's deeper motivations - even in Deligne's case I have an apprehension that remains hazy and no doubt will remain so. But on a "practical" level, Deligne's game (with the SGA 4 "s operation - and all the rest) is quite clear. And what's also clear is that these operations couldn't have been carried out **without everyone's solidarity**. It seems to me that Jouanolou isn't too much in the picture - he doesn't strike me as a "luminary", I have the impression that he has long since left the cohomological quagmires (85<sub>1</sub>). But I can't imagine Illusie and Berthelot not having had their hands on both SGA<sub>2</sub> 4<sup>1</sup> and "la

good reference", and they can read as well as me and are no stupider than I am.

If Illusie suddenly became involved in the publication of SGA 5, at the precise moment when Verdier used, where

where Deligne needs a logistical base for his famous SGA 4<sup>1</sup> (by unpacking it) the two seminars from which this text and all his work derive), whereas Illusie had

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<sup>83</sup>(\*) Between the 1960s and 1970s, I had to work at an average rate of a thousand pages a year of texts (EGA, SGA, articles), almost all of which were to become standard references (something that was quite clear to me when I wrote them, or when I encouraged a collaborator to do so with my assistance).

had ten years to do it, it's surely no coincidence. If the closing presentation on open problems and conjectures that I  $\square$ vais made in 1966 "has unfortunately not been written, any moreover [sic] than p. 346 his very fine introductory talk, which reviewed the formulas of Euler-Poincaré and Lefschetz in various contexts (topological, complex analytic, algebraic)", it's surely no coincidence either - but that's a funeral I don't know anything about. And it's no coincidence either that it seemed as natural to Illusie as it did to Deligne (and just worthy of a mention in passing among the "changes of detail") to amputate the seminar from one of its key presentations, which passes into SGA 4<sup>1</sup> without further ado<sup>84</sup> (\*).

I don't know what were the intentions (conscious or unconscious) of Luc Illusie, whom I like like Pierre Deligne, and who (like him) has always shown me great kindness<sup>85</sup> (\*\*). But I've noticed that, alongside Deligne, he's become the co-actor of a **shameless mystification**: that which passes off the SGA 5 mother seminar of 1965/66 (the very one in which Deligne first heard of schemas, stale cohomology, duality and other "digressions") as a kind of shapeless, vaguely ridiculous appendix to a collection of texts with the misleading name SGA 4<sup>1</sup> written eight years later, which pretends to present itself as anterior (both by the number in its title, and by the number of publication in the Notes readings, and finally by the author's unusual comment "Its existence (of SGA 4<sup>1</sup>) will soon allow us to publish SGA 5 **as it stands**"<sub>2</sub> - emphasis added) - and which, moreover, affects to treat with undisguised disdain the works from which this meagre collection is entirely derived.

Without these works, treated with this beautiful casualness, **none** of Deligne's great works, which are the foundation of his career, would have been possible.

its well-deserved prestige, would not be written now, nor in a hundred years (and the same without doubt for Illusie and my other cohomology students). There is in the esprit of this "SGA 4 V operation a p. 347 **impudence**, of which Illusie is the guarantor (without even realizing it, no doubt), and which could only have been displayed with the tacit approval of a **consensus**. The first people involved in this consensus, apart from Deligne himself, are the very people who were my students and the main beneficiaries of a certain heritage, handed over before their very eyes to the vagaries of the jockeying for position and disdain.

And those airs of peremptory smugness, those paternal, protective airs that I was able to appreciate in my ex-student as recently as the day before yesterday in our telephone conversation<sup>86</sup> (\*), and also those more discreet airs of condescension that I was able to appreciate in my friend Pierre in the aftermath of the brilliant double operation "SGA 4<sup>1</sup> - SGA 5" (of which I was far from having the slightest suspicion at the time and for another seven years) - these airs are **not the** products of solitude, but the signs of a consensus that has **never been called into question**. These tunes tell me something not only about Verdier and Deligne, but also about all those who were my students, and before all others, about those who were (by virtue of their work themes and the tools they wielded every day) the first to be concerned.

The term "mystification", which came to me without having sought it out, opportunely reminds me of that other mystification, in which the same cynicism is on display - that of the so-called "Pervers" Colloquium. The two now seem **intimately, indissolubly linked - it's the same spirit that made both possible**. With the possible exception of Jouanolou, who is no longer so much a part of the "big world", I consider these same ex-students to be the same.

<sup>84</sup>(\*) (May 16) In fact, as I discovered the very next day (see note n° 87), there had been a veritable "massacre" of the mother seminary (or father!) SGA 5, at the hands of Verdier, Deligne and Illusie.

<sup>85</sup>(\*\*) Even after I left in 1970, Illusie showed me a lot of kindness - for a long time he sent me beautiful Christmas cards at the end of the year. I'm afraid I didn't have to write back very often to thank him and give him some sign of life - these signs of a faithful friendship came to me like messengers from a past that seemed so infinitely distant, and with which I'd lost touch.

(May 16) On the other hand, there has been no inclination on Illusie's part to continue or resume contact on a mathematical level, and even last year (when I contacted him about mathematical questions) I sensed his reluctance. In the fourteen years since I left, I have received one and only one offprint from him, dated 1979.

<sup>86</sup>(\*) For this conversation, see the note "Jokes - or 'weight complexes'" (n° 83).

cohomologists jointly and severally responsible for this disgrace. As far as Berthelot and Illusie are concerned, nothing allows me to prejudge malice or bad faith (which cannot be doubted in the case of Verdier as in that of Deligne). But at the very least, I note a blindness, a blockage in the use of healthy faculties, the underlying reason for which, of course, escapes me. If it weren't for a deliberate intention of indifference and disdain, surely Zoghman Mebkhout, as the only person in the '70s to openly claim to be an admirer of my work, and on subjects that were close to both of them (without that they deign to notice it), would have had the benefit of the minimum "favorable prejudice" so that they would at least be aware so  $\square$  so it little of what he does, and hence realize the interest of the direction in which he had been engaged since 1974, an interest that was **obvious**! Neither of them deigned to notice anything, coming from a vague stranger who still pretends to be a Grothendieck. I don't know if they've opened it, or if they've gone through the shorter, more digestible texts that explain what it's all about - in any case, they haven't deigned to acknowledge receipt of it (nor has Deligne, who obviously sets the tone).

That didn't stop them and the other participants in the memorable Colloque<sup>87</sup> (\*) from learning about the remarkable "Riemann-Hilbert correspondence", without the slightest thought of questioning its origin or authorship, or at least (as solid mathematicians) where it was demonstrated (85<sub>1</sub>). But I trust that Deligne was happy to explain this demonstration, which is surely quite obvious to people like them - precisely the kind of demonstration, using Hironaka-style resolution of singularities, that they learned a long time ago from none other than me (85<sub>2</sub>). Riemann-Hilbert, Hironaka abracadabra - that was it!

Clearly, like Verdier and Deligne, they've completely forgotten what **mathematical creation** is all about: a vision that gradually unravels over months and years, bringing to light the "obvious" thing that no-one had seen, taking shape in an "obvious" statement that no-one had thought of (even though, in this case, Deligne had been trying in vain for a whole year. . . ). ) - and which anyone can then demonstrate in five minutes, using the ready-made techniques he had the advantage of learning sitting on the benches of a distant seminar he doesn't deign (or hasn't kept) to remember. ...

If I've spoken bluntly of Berthelot and Illusie, it's not because I particularly want to smear them (after an initial settling of scores with their two friends). I know that they're no "worse" or dumber than most of their dear colleagues or me, and that the lack of flair and sound judgment that

I see in them in this instance (and sometimes, too, that of the necessary respect for others. . . ) is by no means inveterate, but the effect of a **choice**. This choice undoubtedly offered **them**  $\square$  **returns** that pleased them - and may have done so.

Perhaps this other "return" that comes with my reflection will be unwelcome to one or the other. If it were, it would simply be that he's still reproducing the same choice, which is also that of operating on a tiny part of his faculties, even if it means mistaking bladders for lanterns and vice versa, and hopelessly confusing empty nuts (from the boyfriend) and full nuts (from a vague stranger). To each his own! ( $\Rightarrow$  86, 87)

**Note** 85<sub>1</sub> Jouanolou is the only one of my students, along with Verdier, who did not publish his thesis. This seems to me to be a sign of disaffection with the foundational work he had developed, namely that of  $\chi$ -*adique* cohomology from the point of view of derived categories. Since most of his work on this theme took place **after** my departure, i.e. at a time when my students, Deligne and Verdier in

<sup>87</sup>(\*) (June 12) I have since learned that neither of them took part in this Colloque (Luminy, June 1981). See, however, the note "La mystification", n° 85'.

head, had given the signal for a general disaffection with the ideas I had introduced into homological algebra, and in particular that of the derived category, the context hardly encouraged Jouanolou to identify with his work and to do him the (well-deserved) honor of publishing it. As these same Deligne and Verdier, in the wake of the work of Zoghman Mebkhout (aka Elève Inconnu (de Verdier) aka élève posthume (de Grothendieck)), have come to discover (with great fanfare and mutual publicity) the importance of derived categories (see notes n°s 75,77,81), Jouanolou's scorned thesis has, since the Colloque Pervers, regained all its topicality ; a relevance it would never have ceased to have, had the development of the cohomological theory of schemas continued normally after my departure in 1970. A striking detail that illustrates a certain drastic "turn" in Deligne's options after my departure: it was Deligne himself (who had clearly understood the importance of developing the formalism of  $\chi$ -adic cohomology in the context of triangulated categories) who provided Jouanolou with a key technical idea for a formal definition of the triangulated  $\chi$ -adic categories he was studying, an idea that is developed in the thesis. (See my 1969 "Report" on Deligne's work, par.8.)

(May 30) See also, on the subject of Jouanolou's work, the note "Les cohéritiers. . . ", n° 91.

**Note** 85<sub>2</sub> Significantly, it was in this same SGA 5 seminar that everyone learned this demonstration principle, which is used to prove the biduality theorem in cohomology as well.

(in cases where singularity resolution is available), that  $\square$  finitude theorems for p. 350

$Rf_*$  without any cleanliness assumption on  $f$ , and similarly for  $RHom$ ,  $Lf^!$ . (These finiteness theorems were also omitted from the published version of SGA 5, to be appended to SGA 4<sub>2</sub><sup>1</sup>, without Illusie even seeing fit to point this out in his introduction - I only realize this as I write these lines!) Zoghman, who didn't have the advantage of attending the seminar (he got "the right reference" instead), learned the procedure in another place where I had used it (for De Rham's theorem for smooth schemes on  $\underline{C}$ ).

He could also learn it from "the good reference", where my demonstrations are copied into the analytical framework, to establish what my students and listeners at SGA 5 have since liked to call the "Verdier duality" (which was known to me before I had the pleasure of making his acquaintance). It all adds up! **The same demonstration** (copied from me at the same time as the statement) is used by Verdier as a title of authorship for a duality he learned nowhere else than in that dislocated and scorned SGA 5 seminar - and it is used **against** Mebkhout, becoming (by its very "obviousness") a (tacit) pretext and means for shamelessly robbing him of the credit for an important discovery.

(May 30) It seems to me that the first time I used singularity resolution à la Hironaka, and understood the extraordinary power of resolution as a demonstration tool, was for a "three spoonfuls" demonstration of the Grauert-Remmert theorem, describing a complex analytic structure on certain finite coverings of a complex analytic space, and the analogous statement

in the case of schemes of finite type on  $C$ . (It's not impossible that the principle has been blown to me, in this on the same occasion, by Serre). This last result is the main ingredient in the proof of the comparison theorem between étale cohomology and ordinary cohomology (the rest being reduced to unscrewing, thanks to the formalism of  $Rf_!$ , plus a bit of solving to go from  $Rf_!$  to  $Rf_*$  . . . )

### 11.3.3. The mystification

**Note!** 85<sup>1</sup> (June 3) In fact, I'm learning that they didn't have to wonder about paternity, since both Berthelot and Illusie learned the theorem from the mouth of Mebkhout, the first...

p. 351 in February 1982, the second as early as 1979 (the year Mebkhout defended his thesis). Although neither of them took part in the Colloquium in question, they are nonetheless in solidarity with the mystification that took place at the Colloquium.

place at this Colloquium, because it is impossible that they were unaware of the cover-up that took place of Mebkhout's authorship of the *theoreme du bon Dieu*. I can imagine that, along with all the other Colloque participants, they were the first to be fooled by the collective mystification organized by their friends Verdier and Deligne (a mystification in which four of my five cohomology students appear to be involved). As far as Illusie is concerned, at least, I was struck, during a telephone conversation with him after Mebkhout's visit to my home last summer, by how little he obviously thought of him - he was quite astonished (almost pained on the part of his old master, in whom he would surely have expected better judgment... ) to see me give Mebkhout a leading role in restarting the cohomological theory of algebraic varieties. Consensus of considerable force had decided to rank Mebkhout among the vague unknowns, and my friend Illusie blithely lived with this triple contradiction, without asking himself any questions: the leading role played by the theorem of the good God and the philosophy that goes with it; the evasion surrounding the authorship of these things (an evasion in which he himself participates in the company of many); and the low esteem he has for the format and role of Mebkhout (who he knows perfectly well is the unnamed author of these things, which have renewed a field of mathematics in which he, Illusie, is a figure of eminence).

I find here again the complete blockage of common sense and sound judgment, even in something as seemingly impersonal as judgment on scientific matters, a blockage to which I have had occasion to allude more than once already, and which each time again baffles me. And this contradiction I see here in Illusie's (and surely many others') relationship with Mebkhout, my "posthumous pupil", is surely no more than one of the many effects of a more crucial contradiction to be found in his relationship with me. It is this contradiction, in him more particularly and in my other pupils too, that becomes increasingly clear in the reflection pursued in the notes of the present procession to the Funeral, formed by my pupils of yesteryear...

#### 11.3.4. The deceased

**Note 86** ( May 11) As is so often the case, it was with some reluctance that I set about this task.

new reflection, on the theme "SGA 5 - SGA 4<sup>1</sup> - Perversity", which might have seemed to have been examined and

re-examined over and over again: "It's going to make a deplorable impression on a reader who's probably had enough of it ever since he heard about it; it doesn't look elegant at all to go into details again, SGA 5 çî SGA 4<sup>1</sup> that, it's and doesn't deserve any more toast... .".

p. 352 □ I'm glad I didn't let myself be intimidated by that well-known refrain, which would like to prevent myself from getting to the bottom of something (at least as far as I'm able to go at the time), on the pretext that it's decidedly "not worth it", that there's nothing to do but let it run its course... . If there have been times when I've discovered things that I consider useful and important, it's always been in moments when I've known not to listen to what presents itself as the voice of "reason", or even "decency", and follow this indecent urge inside me to go and see even what is supposed to be "uninteresting" or of poor appearance, or even messed up or indecent. I can't remember a single time in my life when I've regretted having looked at something a little more closely, against inveterate reflexes that would have prevented me from doing so. These inhibition reflexes were even stronger in *Récoltes et Semailles* than on other occasions, because this reflection is destined to be made public, which immediately imposes certain

child parties), and conciseness (for the sake of the reader). But I don't think so,  
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finally, that these constraints never prevented me from either tackling something I wanted to tackle, or going as far into it as I felt I wanted to. In the cases that may at one time have seemed like borderline cases, I forged ahead with the assurance that, should the need arise, I would always have the resource of not including in Harvest and Sowing anything that would "escape" my indiscreet reflection. These "borderline cases" arose exclusively when I hesitated to involve others, and never when it came to involving myself. But even in the first case, it turns out (and this came as a surprise) that I never had to make use of this "resource": the text of Récoltes et Semailles represents the complete version of my reflection - at least of the part of this reflection that found its way into writing to express itself.

I feel that with the brief reflection of the previous note<sup>88</sup> (\*), the situation has become considerably clearer. I mean that a certain essential aspect of a situation which had been confusing to pleasure, and which I have just evoked by the triple name of a "theme" (SGA 5 - SGA 4<sup>1</sup> - Perversity), has appeared to me in full light: that of a "solidarity", a "connivance" which had only been confusedly perceived until then. This doesn't mean that I believe I've fathomed and understood all the ins and outs of a "solidarity", a "connivance" that had only been dimly perceived until then.

complex situation, directly and particularly obviously involving at least seven people: Zoghman Mebkhout (acting in a sense as a "revealer" of a certain situation), my five ex-students cohomologists, and myself. I don't even flatter myself to have perceived all the springs and motivations that have been at play in my own person, in relation to the "SGA 5 etc. ... .", in the nearly twenty years since that "unfortunate seminar" took place! But I feel in a much better position than I was yesterday (or even just this morning), to understand and situate the echoes that I hope will reach me on this subject from at least one or other of the main parties involved.

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The main question that arises for me (it seems to me that it was already present at another stage of reflection, and now reappears with new vigour) is (it seems to me) this: is what happened with this Burial by my students, (more or less) in their entirety, something completely **atypical**, linked to certain particularities of my person and my singular destiny (such as my departure from the mathematical scene nearly fifteen years ago, the circumstances surrounding it, etc. ) ? Or is it, on the contrary, something "quite natural", due to a simple combination of circumstances - following the principle that "opportunity makes the thief"? I hesitate to think so, though I can't at the moment discern, or even glimpse, what particular aspect of myself has had the virtue of creating such perfect and unanimous **agreement** among my former students, to bury both the "master", and those who claim to be his followers or whose work clearly bears his mark (without, however, being "theirs"). Is it this sort of "aura" of Father that surrounds me, and which I've had occasion to mention? Or is it the challenge posed to each of them by the mere fact of my departure? At the moment, I wouldn't be able to say, for lack of eyes that can see... . Perhaps the coming months will teach me something on this subject<sup>89</sup> (\*).

More than once in the last three weeks, I've thought of this other strange "coincidence": it's that the discovery of the Burial "in all its splendour" (with the four-stroke LN 900 - SGA 4<sup>1</sup> - SGA 5 - Colloque Pervers, then back to SGA 5 and SGA 4<sup>1</sup>)<sup>2</sup> - that this discovery came at a time when I had just completed an in-depth reflection on my past as a mathematician and my relationship with my students. It was a time, therefore, when I had just come "to terms with myself" on the subject of my students.

of this past, to the best of my ability, and as far as the facts then known to me allowed, as they were often hazy memories. Or to put it another way: it was leprendered by

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<sup>88</sup>(\*) This refers to the note "La solidarité" n° 85, dated the same day.

<sup>89</sup>(\*) (May 30) For a reflection along these lines, see the note "Le Fossoyeur - ou la Congrégation toute entière", n° 97.

the exact moment when I was finally **ready** to learn and profit from it.

Chance" did things so well that there wasn't even a break in the meditation. The reflection that had begun with this short retrospective on the fate of the most important notions (in my opinion) that I had introduced<sup>90</sup> (\*) (a reflection that remained in a certain vagueness, where only a certain basic tone emerged insistently... ) - this reflection continued quite naturally on Thursday April 19. It was, it's true, still in the throes of the emotion aroused by that impression of "impudence" (to use the term from earlier, which also aptly describes something I felt at the time), on reading the "memorable volume" LN 900.

In this new departure from "the same" thinking, the main driving force was "the boss" - my self-esteem and sense of decency were affected, and by writing down my emotions I freed myself from them to a certain extent. It was indeed "me", "the boss", who visibly led the dance in the ten days that followed - days marked by the absence of smiles and laughter, by unfailing seriousness. I had to go through this ten-day detour before the reflection returned to the center it had left - to myself. I still remember the relief of that return - like coming out of a tunnel when daylight appears again! It was then that I found myself laughing and smiling again, as if we'd never left each other. It was April 29th. The next day, the 30th, the last day of the month, I was happy to put the finishing touches to this ultimate stage of reflection.

It was also the moment, surely, when I was ready to receive the next "package", this time sent by my friend Zoghman - the "Colloque" package received the day after tomorrow. Today is the tenth day I've been working on assimilating the substance of this package. But at this stage, while I've been gnawing at the bit to get to the bottom of this rebound that just kept on rebounding, the smile hasn't faded for a single day. And today, I truly believe (for the thirtieth time, it's true!), is finally the day of closure.

Five days ago I'd already had that same feeling that I'd reached the end of my rope, that there was nothing left but work to be done.

of stewardship: adding a few footnotes here and there, retyping pages too overloaded with ratures (each time a sign of a thought that had remained somewhat confused, and which needs to be put into

This seemingly mechanical work, from which the text always emerges with a new face... ). . . This was when I had just written what is now the note "Mes amis" (n° 79), which spontaneously flowed into "final chords". However, I ended up separating these chords from the beginning of the note. In fact, it turned out that this famous stewardship work had broken down: the "footnotes", typed without line spacing, became real notes (**not** footnotes) of nice dimensions, which had to be retyped with line spacing, and then tried as best I could to fit in here and there. It took days before I realized that another procession, after the one called "The Colloquium", was forming to join the procession - and that the last of the processions would not (as I had decided in my head) be the said Colloquium, but would be led by **the Student**. And just today, as the first procession, reduced to a single note, was enriched by a second ("A feeling of injustice and powerlessness"), I also knew who would lead it: it was "**L'élève posthume**". So the procession, opened by a Pupil (posthumous and lower-case, as befits his humble state) and closed by yet another Pupil (by no means humble this time), seems to me to be complete at last!

It's also time, it seems to me, after a first "false arrival", to return to the chords of a final De Profundis, which are more appropriate today than they were five days ago. Here they are, as I wrote them down then, and which also express my feelings at the moment.

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<sup>90</sup>(\*) See notes "My orphans" and "Refusal of an inheritance - or the price of a contradiction" of March 31 (n° s 46,47).

(May 31) Finally, it was another "false arrival" - the "final agreements" were premature this time too! Twenty days went by, during which the "housekeeping work" continually broke down into rethinking this and that aspect that had been neglected. Six more notes joined the "L' Elève" procession, which was supposed to close the parade. The Fourgon Funèbre appeared in the wake of the Elève, carrying four coffins accompanied by the Fossoyeur. He was definitely lacking to give body and meaning to a funeral convoy that didn't seem to be conveying anyone.

Having become cautious through experience, I'm waiting for events to unfold and won't take any chances for the time being.

to predict whether the procession is finally complete, or whether a forgotten procession will sneak in again at the last minute, so as not to miss the ultimate Ceremony<sup>91</sup> (\*).

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### 11.3.5. The massacre

**Note 87** (May 12)<sup>92</sup> (\*\*) For the edification of the somewhat cohomologist reader, and above all for my own, I would like to review the details of this plundering of a splendid seminary, in the hands of two of my former cohomologist students and under the benevolent eye of the others<sup>93</sup> (\*\*\*) - of the same seminary where they learned, twelve years before anyone else and from the hand of the workman himself, the basics and finesses of the trade that made their reputation.

Two of my oral presentations have never been made available to the public in any form. One is the closing lecture on open problems and conjectures, which "unfortunately was not written up", given how little - and indeed, the author of the introduction to the murder-edition deemed it unnecessary even to mention **what** open problems and conjectures it was. And why should he have taken the trouble, when these were merely problems (which everyone is free to pose as they please!) and conjectures (not even demonstrated!) (87<sub>1</sub>). The other was the lecture that opened the seminar, placing it right from the start in a broader context (topological, complex analytic, algebraic) and reviewing formulas of the Euler-Poincaré, Lefschetz, Nielsen-Wecken type, some of which constituted one of the seminar's main applications. The ". . . any more than. . . ." with which the author of the introduction goes on to point out, in the course of a sentence, the disappearance of this presentation, speaks volumes about a **casual attitude** which at the time was clearly self-evident, even though the author of the seminar had been out of circulation for seven years.

There's a whole series of talks I gave on the formalism of homology and cohomology classes. logie associated with a cycle (regular ambient pattern in the cohomological case)<sup>94</sup> (\*\*\*\*). They have been equally divided: cohomology for Deligne, homology for Verdier - who nevertheless overflows a little on cohomology, even if it means making the small reference to Deligne with the famous "complexes". p. 357 weight"<sup>95</sup> (\*). (Not to mention the finitude theorem for RHom and the biduality theorem, copied verbatim from the seminar - in any case, the lion's share will go to Deligne, which was to be expected. ... ) The author of the introduction does not see fit to mention the homology lectures alone. Indeed, there was no need, since the previous year his friend Verdier had taken on the task of providing the missing "good reference" (without alluding to a seminar, or to me).

<sup>91</sup>(\*) (June 12) Caution was the order of the day, as a new "Mes élèves" procession separated from the one originally called "L'Elève", which became "L'Elève - alias le Patron".

<sup>92</sup>(\*\*) This note follows on from the previous day's reflection on "Solidarity" (n° 85).

<sup>93</sup>(\*\*\*) Further reflection reveals that one of these "others" lent a hand efficiently for this operation. on behalf of others.

<sup>94</sup>(\*\*\*\*) For details, see note no. ° 82 "Good references".

<sup>95</sup>(\*) See note (83) "The joke - or weight-complexes".

There were oral presentations on finiteness theorems for the operations  $R f^! \wedge$  ( $f$  not proper), and as a corollary, for the operations  $RHom_*$  ; and  $Lf^!$  . The key theorem was proved using a Hironaka-style singularity resolution technique (valid only in cases where the resolution is available).

These arguments, which I used, have come into common use since the seminar (see note (85<sub>2</sub>)). Deligne has managed to prove these finitude theorems, as well as the biduality theorem, under other, more helpful hypotheses, which have already been verified in most applications. It might have been expected that he would ask to include these refinements in the seminar where he had the privilege of learning étale cohomology, and the ideas and techniques at the basis of all his subsequent work. But this circumstance is used as a "reason" for amputating this part of the seminar. As for the biduality theorem, under Illusie's pen (and within the framework of the diagrams) it became "Deligne's biduality theorem" (introduction to Lecture I). This was only fair, since in the analytical case, Verdier had already claimed it as his own the previous year (without even having to go to the trouble of finding another demonstration).

Then there's Illusie's "generic Kûnneth formula". No one before had thought of developing this kind of statement, inspired by the intuition that "generically" i.e. in the vicinity of the generic point of the basis, a relative scheme behaves like a "locally trivial fiber" in the topological context. In an elegant demonstration similar to the one mentioned above, Deligne manages to eliminate the singularity-resolving hypothesis I had made. It's awarded - presentation deleted and "replaced" by a reference to a presentation by the same Illusie in the so-called "earlier" seminar.

SGA 4<sup>1</sup><sub>2</sub>.

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There is a series of lectures on the formalism of non-commutative traces, developed as a means of explicating the local terms of the Lefschetz-Verdier formula in cases that had never been treated. These lectures were eventually written, it seems, by Bucur, whose manuscript "got lost in a providential move" - it's turning into a vaudeville!<sup>96</sup> (\*) In the introduction to SGA 5, written by Illusie, these lectures become "Grothendieck's theory of **commutative** traces, generalizing [brilliantly] Stallings" (which were non-commutative!). The slip of the tongue<sup>97</sup> (\*\*) can only be due to a badly (or too well. . . ) inspired secretary, who must have been involved with my friend Ionel Bucur's movers. (The word "brilliantly" is an interpolation of my pen, to better render the thought infallibly suggested by this slip of the tongue, also providential).

I've got nothing to complain about, since Illusie has taken on the job of redoing the work (and even, he tells us, a "more sophisticated" version, since it's put in the Beamtique sauce - I seem to remember, though, Illusie, that you made more "sophisticated" innovations than this in my day... .). It must have taken a long time even, if I remember that I spent weeks putting the machine together; my manuscript also got lost in the same providential move, and God knows if one of the dear listeners, overwhelmed by my oral faconde, was at least able to take comprehensible notes... .

Remarkably, and this is something I hadn't noticed before, he doesn't insert this talk in the place in Lecture XI where it was intended (which no doubt also corresponds to the place it had in the oral seminar), preferring to leave a gaping hole there and make his talk an apocryphal one, called "Calculs de termes locaux" ("Calculations of local terms"). The title does, however, seem to correspond to what I seem to remember him doing in the oral seminar.

<sup>96</sup>(\*) It was no doubt this circumstance that inspired Deligne's unexpectedly brilliant criticism of SGA 5, in which the local terms of Lefschetz Verdier's formula (which "remained conjectural", remember!!!) were not even calculated! (See note "la table rase", n° 67, about the absurdity of this criticism, which for an informed reader is similar to that of Verdier's famous "weight complex" the previous year (see note n° 83). So it was Verdier who became the schoolmaster!)

<sup>97</sup>(\*\*) This is the slip of the tongue attributing to me the authorship of a theory of "commutative" traces (for which I was not expected), instead of "non-commutative". That it has survived into the published edition is all the more remarkable given that Illusie was perhaps the most meticulous of my students, down to the last detail.

strange. But from line 1 of his introduction to this talk, the author is quick to □detrompt us: "Cetp  
 exposé, written in January 1977, **does not correspond to any oral presentation in the seminar**". And he  
 continues with Lefschetz-Verdier formulas (that name rings a bell, though, and I thought I'd actually developed  
 a theory of non-commutative traces, in order to calculate "local terms" in certain cases. . . ), then on a formula  
 by Langlands and a demonstration by Artin-Verdier in 1967 (this was a year after the final agreements of  
 the oral seminar, which must have influenced these authors, at least one if not both of whom followed him).  
 Towards the end of the page, we learn as if in passing, contrary to what had been announced at the  
 beginning, that there is also a "second part of this talk, of a much more technical nature" (I've read this  
 language somewhere. . . ) which is (admire the nuance) "**inspired by the method used by Grothendieck**  
 to establish the Lefschetz formula for certain cohomological correspondences on curves", with a reference to  
 Lecture XII of the same seminar and above all to the indispensable SGA 4<sup>1</sup> ; Obviously, there was no  
 reason, for so little, to include this lecture in<sub>2</sub> place of the gaping hole - the "more sophisticated version" of  
 earlier will have done things right. It was even nice of Illusie and Deligne to cite me as a source of  
 "inspiration", when the example of their friend Verdier the previous year had clearly shown that there was  
 absolutely no need for such scruples.

I return to Illusie's introduction to the volume that goes by the name of SGA 5. In it, we learn once again -  
 as Deligne had already announced in his introduction to SGA 4<sup>1</sup> - that it was indeed **thanks to**  
**his friend** that the seminar has finally been published:

"I would like to thank P. Deligne for having convinced me to write, in a new version of Lecture III, a  
 demonstration of the Lefschetz-Verdier formula, **thus removing one of the obstacles to the publication of**  
**this seminar**".

Once again, we're in the middle of a farce - repeated as is by the docile Illusie in the introduction to SGA  
 4<sup>1</sup> ! If the seminar remained unpublished for more than ten years, it's because no one (until Deligne saved  
 the day in 1977) had yet considered the possibility of publishing it.

good idea to write a demonstration of the so-called (and rightly so) "Lefschetz-Verdier formula", of which none  
 other than □ his inseparable friend and my ex-student Verdier himself has proudly borne the paternity **since aup**  
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**minus** 1964 (87<sub>2</sub>), i.e. for at least two years by the time my seminar ended, and was just waiting to be made  
 available to everyone!

Finally, as another and last (?) mutilation of the seminar, there was the disappearance of the fine talk  
 Serre had given on the "(Serre-) Swan module" - a talk entitled "Introduction à la théorie de Brauer". It's  
 fortunate that Serre, seeing the turn events were taking, had the good sense to include his talk in his book  
 "Représentations linéaires des groupes finis" (Hermann, 1971), and make it available to the mathematical  
 public.(87)<sub>3</sub>

This time, I think, I've come full circle. The picture of the fate of a seminar in which I had put the best of  
 myself (88)<sup>98</sup> (\*), and which I find twenty years later unrecognizable, butchered by the very people who had  
 been its exclusive beneficiaries - or at least by three of them, and with the assent of all the other participants.

I don't regret having taken the trouble, once again, to follow through on what had gradually come to my  
 attention. This "return of things"<sup>99</sup> (\*\*) that I noticed, at the end of a long retros-

<sup>98</sup>(\*) For the meaning of this expression "of the best of myself", see the following notes "La dépouille. . . ", ". . . and the body", n°  
 88, 89. The first of these situates the SGA 5 seminar, with SGA 4 inseparable from it, as the masterpiece of the part of my  
 work "entirely completed".

<sup>99</sup>(\*\*) See note of this name (n° 73) dated April 30.

pective about my relationship with one of my former students, sensing even then that he wasn't the only one to "bury me with gusto" - I've only now become aware of his breath, his "smell" (to use an expression that then appeared in one of my dreams) - the breath of **violence**. This breath is concealed and revealed at the same time by the speech<sup>100</sup> (\*\*\*) (seemingly detached and impassive) presenting a highly technical substance. What this violence is aimed at, through a "corpse" delivered at mercy,

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is the very person of the one who was the "master", the "Father" - at a time when the "pupils" have long since taken his envied place, without encountering any resistance; and that □ also long ago they have elected from among themselves the new "Father", called to replace the old and reign over them.

I feel this breath, and yet it remains for me a foreign thing, misunderstood. To "understand" it, this breath would have to live in me, or have lived in me. But four years ago, for the first time, I felt and measured the significance of something in my life that I had never thought about, that had always seemed self-evident: that my identification with my father as a child **was not** marked by conflict - that at no time in my childhood **did I fear or envy my father**, while at the same time devoting unreserved love to him. This relationship, perhaps the most profound that has marked my life (without my even realizing it before this meditation four years ago), which in my childhood was like a relationship with an other self both strong and benevolent - this relationship was not marked by division and conflict. If, through all my often-torn life, the knowledge of the strength that lies within me has remained alive; and if, in my life by no means free of fear, I have not known fear either of a person or of an event - it is to this humble circumstance that I owe it, ignored until well into my fifties. This circumstance has been a priceless privilege, for it is the intimate knowledge of the creative force within one's own person that **is** also that force, enabling it to express itself freely according to its nature, through creation - through a creative life.

And this privilege, which has exempted me from one of the most profound marks of conflict, is at the moment also like a hindrance, like a "void" in my experience of life. A void that's hard to fill, where many others have a rich tapestry of emotions, images and associations, offering them the path (provided they're curious enough to take it) to a profound understanding of others as well as themselves, in situations that I manage (by dint of repetition and cross-checking) to apprehend as best I can, but in the face of which I remain like a stranger - with the desire for knowledge within me still hungry.

**Note** 87<sub>1</sub> (87<sub>1</sub>) (May 31) This closing presentation, surely one of the most interesting and substantial with the opening talk, was obviously not lost on everyone, as I see from reading Mac Pherson's paper "Chern classes for singular algebraic varieties" (Chern classes □ for singular algebraic varieties, *Annals of Math.* (2) 100, 1974, pp. 423-432) (received April 1973)-In this paper, under the name of "Deligne-Grothendieck conjecture", I repeat one of the main conjectures I had introduced in this paper in the schematic framework. It is taken up by Mac Pherson in the transcendental framework of algebraic varieties over the field of complexes, the Chow ring being replaced by the homology group. Deligne had learned this conjecture<sup>101</sup> (\*) in my talk in 1966, the same year he had appeared in the seminar where he began to familiarize himself with the language of diagrams and cohomological techniques (see the note "L'être à part" n° 67)-It's nice of you to have done me the honor of including me.

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<sup>100</sup>(\*\*\*) These are mainly the introductory texts accompanying SGA 5 (written by Illusie) and SGA 4. <sup>1</sup>  
(written by Deligne).

<sup>101</sup>(\*) (June 6) In a slightly different form, see the rest of today's note.

(March 1985) For further details, given by Deligne himself, see the note "Les points sur les i", n° 164 (II 1).

in the name of conjecture - a few years later it would no longer have been appropriate... .

(June 6) I'd like to take this opportunity to explain the conjecture I'd put forward in the seminar in the schematic framework, while also pointing out the obvious variant in the complex analytic (or even rigid-analytic) framework. I conceived it as a "Riemann-Roch" theorem, but with discrete coefficients instead of coherent coefficients. (Zoghman Mebkhout told me, incidentally, that his view of D-Modules should make it possible to consider the two Riemann-Roch theorems as contained in a single crystalline Riemann-Roch theorem, which would thus represent in zero characteristic the natural synthesis of the two Riemann-Roch theorems I introduced into mathematics, one in 1957, the other in 1966). We fix a ring of coefficients  $\Lambda$  (not necessarily commutative, but noetherian to simplify and moreover of prime torsion to the characteristics of the schemes under consideration, for the purposes of stable cohomology... ). For a scheme  $X$  we denote by

$$K(X, \Lambda)$$

the Grothendieck group formed by constructible étale bundles of  $\Lambda$ -modules. Using the functors  $Rf_!$ , this group depends functorially on  $X$ , for  $X$  noetherian and scheme morphisms which are separated and of finite type. For regular  $X$ , I postulated the existence of a homomorphism of groups canonical, playing the role of the "Chern character" in the consistent RR theorem,

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$$ch_X : K(X, \Lambda) \rightarrow A(X) \otimes_{\mathbb{Z}} K(\Lambda), \quad (15.1)$$

where  $A(X)$  is the Chow ring of  $X$  and  $K(\Lambda)$  the Grothendieck group formed with  $\Lambda$ -modules of finite type. This homomorphism was to be determined solely by the validity of the "discrete Riemann-Roch formula", for a **proper** morphism  $f : X \rightarrow Y$  of regular schemes, which formula is written as the consistent Riemann-Roch formula, with Todd's "multiplier" replaced by the total relative Chern class :

$$ch_Y(f_!(x)) = f_* (ch_X(x) c(f)), \quad (15.2)$$

where  $c(f) \in A(X)$  is the total Chern class of  $f$ . It's not hard to see that in a context where we have the resolution of singularities in Hironaka's strong form, RR's formula does indeed uniquely determine the  $ch$ .

Of course, we assume that we're in a context where the Chow ring is defined (I'm not aware of anyone having even attempted to write a theory of Chow rings for regular schemes of finite type over a body). Alternatively, we can also work in the graduated ring associated with the usual "Grothendieck" ring  $K^\circ(X)$  in the coherent context, filtered in the usual way (see SGA 6). Alternatively, we can replace  $A(X)$  by the even  $l$ -adic cohomology ring, the direct sum of  $H^{2i}(X, \mathbb{Z}_l(i))$ . This has the disadvantage of introducing an artificial parameter  $l$ , and giving formulas that are less "purely numerical" fines, while the Chow ring has the charm of having a continuous structure, destroyed by switching to cohomology.

Already in the case where  $X$  is a smooth algebraic curve over an algebraically closed field, the calculation of  $ch_X$  involves delicate local invariants of the Artin-Serre-Swan type. In other words, the general conjecture is a profound one, the pursuit of which is linked to an understanding of the higher-dimensional analogues of these invariants.

Remark. Designating in the same way by  $K(X, \Lambda)$  "the Grothendieck ring" formed with the construc-

of finite tor-dimensional  $\Lambda$ -spreads (which ring operates on  $K(X, \Lambda)$  when  $\Lambda$  is commutative. . . ), we must likewise have a homomorphism

$$ch_X : K(X, \Lambda) \rightarrow A(X) \otimes_{\Lambda} K(\Lambda)$$

again giving rise (mutatis mutandis) to the same Riemann-Roch (RR) formula.

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□ Let  $cons(X)$  now be the ring of constructible integer functions on  $X$ . We define less tautological canonical homomorphisms

$$K(X, \Lambda) \rightarrow Cons(X) \otimes_{\Lambda} K(\Lambda),$$

$$K^*(X, \Lambda) \rightarrow Cons(X) \otimes_{\Lambda} K^*(\Lambda),$$

If we now restrict ourselves to schemes **of zero characteristic**, then (by using Euler-Poincaré characteristics with proper supports) we see that the group  $Cons(X)$  is a covariant functor with respect to finite-type morphisms of noetherian schemes (in addition to being contravariant as a ring functor, which is independent of characteristics), and the preceding tautological morphisms are functorial. (This corresponds to the "well-known" fact, which I believe was not proved in the SGA 5 oral seminar, that in **zero characteristic**, for a locally constant bundle of  $\Lambda$ -modules  $F$  on an algebraic scheme  $X$ , its image by

$$f_! : K(X, \Lambda) \rightarrow K(e, \Lambda) \rightarrow K(\Lambda)$$

is equal to  $d\chi(X)$ , where  $d$  is the rank of  $F$ ,  $e = Spec(k)$ ,  $k$  the base field assumed to be algebraically closed. . . ). This immediately suggests that the Chern homomorphisms (1. ) and (1' ) must be derivable from the tautological homomorphisms (2. ), (2' ) by composing with a "universal" Chern homomorphism (independent of any ring of coefficients  $\Lambda$ )

$$ch_X : Cons(X) \rightarrow A(X),$$

so that the two " $\Lambda$ -coefficient" versions of the RR formula appear to be formally contained in an RR formula at the level of constructible functions, which is always written in the same form.

When working with schemes on a fixed basic body (again, of any characteristic), or more generally on a fixed **regular** basic scheme  $S$  (for example  $S = Spec(\underline{\mathbb{Z}})$ ), the form of the Riemann-Roch formula most in line with the usual writing (in the coherent framework familiar since 1957) is obtained by introducing the products

$$ch_X(x)c(X/S) = c_{X/S}(x)$$

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(where  $X$  is in a  $K(X, \Lambda)$  or  $K^*(X, \Lambda)$  indifferently), which we might call **the Chern class of  $X$  relative to the basis  $S$** . When  $x$  is the unit element of  $K(X, \Lambda)$  i.e. the class of the constant bundle of value  $\Lambda$ , we find the image of the total relative Chern class of  $X$  with respect to  $S$ , by 1 "canonical homomorphism of  $A(X)$  into  $A(X) \otimes K(\Lambda)$ . This being the case, RR 's formula is equivalent to the fact that the formation of these relative Chern classes

$$c_{X/S} : K(X, \Lambda) \rightarrow A(X) \otimes K(\Lambda),$$

for a regular variable scheme  $X$  over  $S$  (of finite type over  $S$ ), with  $S$  fixed, is functorial by



with respect to eigenmorphisms, and similarly for the variant (5'). In null characteristic, this reduces to the functoriality (for eigenmorphisms) of the corresponding application

$$c_{X/S} : \text{Cons}(X) \rightarrow A(X) .$$

It is in this form of the existence and uniqueness of an absolute "Chern class" application (6), in the case where  $S = \text{Spec}(C)$ , that the conjecture in Mac Pherson's work is presented, the relevant conditions (here as in the general case of zero characteristic) being a) the functoriality of (6) for proper morphisms and b) we have  $c_{X/S}(1) = c(X/S)$  (in this case, the "absolute" total Chern class). Compared with my initial conjecture, however, the form presented and proved by Mac Pherson differs in two ways. One is a "minus", in that it is placed, not in the Chow ring, but in the whole cohomology ring, or more precisely the whole homology group, defined by transcendental means. The other is a "plus" - and it is here perhaps that Deligne has made a contribution to my initial conjecture (unless this contribution is due to Mac Pherson himself<sup>102</sup> (\*)). It's that for the existence and uniqueness of an application (6), we don't need to restrict ourselves to regular schemes  $X$ , provided we replace  $A(X)$  by the entire homology group. The same is likely to be true in the general case, where we denote by  $A(X)$  (or better, by  $A_*(X)$ ) the **Chow group** (which is no longer a ring in general) of the noetherian scheme  $X$ . Or to put it another way: while the heuristic definition of *ch-invariants* $_X(x)$  (for  $x$  in  $K_*(X, A)$  or  $K^*(X, A)$ ) makes essential use of the assumption that the ambient scheme is regular, as soon as we multiply it by the "multiplier"  $c(X/S)$  (when the scheme  $X$  is of finite type over a fixed regular scheme  $S$ ), the product obtained (4) seems to retain a meaning without any assumption of regularity<sup>about</sup>□, as an element of a tensor product.

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$$A_*(X) \otimes K_*(\Lambda) \text{ or } A_*(X) \otimes K^*(\Lambda) ,$$

where  $A_*(X)$  denotes the Chow group of  $X$ . The spirit of Mac Pherson's demonstration (which does not use singularity resolution) would suggest the possibility of an explicit "computational" construction of the homomorphism (5.), by "making do" with the singularities of  $X$  as they are, as well as with the singularities of the coefficient bundle  $F$  (whose class is  $x$ ), to "collect" a cycle on  $X$  with coefficients in  $K_*(\Lambda)$ . This would also be in the spirit of the ideas I had introduced in 1957 with the coherent Riemann-Roch theorem, where I did self-intersection calculations in particular, taking care not to "move" the cycle under consideration. A first obvious reduction (obtained by immersing  $X$  in an  $S$ -scheme) would be to the case where  $X$  is a firm subscheme of the regular  $S$ -scheme. . .

The idea that it should be possible to develop a **singular** (coherent) Riemann-Roch theorem was familiar to me, I can't say how long ago, but I never tried to test it seriously. It was more or less this idea (apart from the analogy with the "cohomology, homology, cap-product" formalism) that led me in SGA 6 (in 1966/67) to systematically introduce the  $K_*(X)$  and  $K^*(X)$  and the  $A_*(X)$ ,  $A^*(X)$ , instead of just working with the  $K^*(X)$ . I don't remember whether I also thought of something like this in the SGA 5 seminar in 1966, and whether I hinted at it in the oral presentation. As my handwritten notes have disappeared (in a removal perhaps?) I'll probably never know... .

(June 7) Looking through Mac Pherson's article, I was struck by the fact that the word "Riemann-Roch" is not mentioned - which, incidentally, is why I didn't immediately recognize the conjecture I made in the SGA 5 seminar in 1966, which was for me (and still is) a "Riemann-Roch" type theorem. It seems that when Mac Pherson wrote his article, he didn't even realize that it was a "Riemann-Roch" theorem.

<sup>102</sup>(\*) (March 1985) This is indeed the case, cf. note no. ° 164 cited in the previous footnote.

of this obvious kinship. I presume that the reason for this is that Deligne, who after my departure put this conjecture into circulation in the form he liked, took care as far as possible to "erase" it.

p. 367 the obvious kinship with the Riemann-Roch-Grothendieck theorem. I think I sense his motivation for doing so. On the one hand, it weakens the link between this conjecture and myself, and makes it more plausible to call-

the "Deligne-Grothendieck conjecture" under which it is currently circulating. (NB I don't know if it's in circulation in the schematic case, and if so, I'd be very curious to know under what name). But the deeper reason seems to me to lie in his obsessive desire to deny and destroy, as far as possible, the fundamental unity of my work and my mathematical vision<sup>103</sup> (\*). This is a striking example of how, in a mathematician of exceptional means, a fixed idea entirely unrelated to any mathematical motivation, can obscure (or even completely block out) what I have called the "healthy mathematical instinct". This instinct cannot fail to perceive the analogy between the two "continuous" and "discrete" statements of the "same" Riemann-Roch theorem, which I had of course emphasized in the oral presentation. As I indicated yesterday, this kinship will no doubt soon be confirmed by a statement in form (conjectured by Zoghman Mebkhout), at least in the complex analytic case, allowing both to be deduced from a common statement. Clearly, given Deligne's "gravedigging" attitude towards the Riemann-Roch theorem<sup>104</sup> (\*\*), he was not likely to discover the unique statement that links them in the analytic framework, and even less likely to raise the question of an analogous statement in the general schematic framework. Nor was he able, in such circumstances, to find the fruitful point of view of D-Modules in the cohomological theory of algebraic varieties, arising all too naturally from ideas that had to be buried - or even to recognize, for years, the fruitful work of Mebkhout, succeeding where he himself had failed.

p. 368 **Note** 87<sub>2</sub> (May 31) This is the year of my Bourbaki paper on the rationality of *L-functions*, in which I heuristically use Verdier's result (???) (and especially the expected form of local terms in the case of d'espèce), without waiting for Illusie to demonstrate it thirteen years later, at Deligne's invitation. It seemed to me, moreover, when Verdier showed me his ultra-general formula that came as a surprise, that he demonstrated it with a few lines of "six operations" formalism - this is the kind of formula where (almost) to write it is to demonstrate it! If there was any "difficulty", it could only be at the level of verifying one or two compatibilities<sup>105</sup> (\*). What's more, both Illusie and Deligne knew perfectly well that the demonstrations I had given in the seminar for various explicit trace formulas **were complete**, and did not depend in any way on Verdier's general formula, which had simply acted as a "trigger" to encourage us to make explicit and prove trace formulas in as general a range of cases as possible. The bad faith of both is obvious here. As far as Deligne is concerned, it was already clear to me when I wrote the note "La table rase" (n° 67) - but probably not to an uninformed reader, nor of course to an informed reader who renounces the use of his healthy faculties.

<sup>103</sup>(\*) Compare with the commentary in the note "La dépouille" (n° 88) on the deeper meaning of the SGA 4 operation<sup>1</sup>, similarly aimed at shattering into an amorphous set of "technical digressions" the deeper unity of my work around staggered cohomology, through the "violent insertion" of the foreign text SGA 4<sup>1</sup> between the two indissoluble parts SGA 4 and SGA 5 which develop this work.

<sup>104</sup>(\*\*) These dispositions, with regard precisely to the Riemann-Roch-Grothendieck theorem, are particularly clear in the "Funeral Eulogy"; see the note "Funeral Eulogy (1) - or compliments", n° 104.

<sup>105</sup>(\*) (June 6) It would also seem that, via the biduality theorem (now known as Deligne's theorem), the demonstration of the Lefschetz-Verdier formula depended on a singularity resolution hypothesis, which Deligne manages to dispense with in the case of *fi ni* type schemes on a body. This is a good opportunity to fish in troubled waters and give the impression that SGA 5 would be subordinate to the "seminar-sic" SGA 4<sup>1</sup> which "précède" it (and which was actually published before it). him!).

(June 6) As for Illusie, he played right into his friend's hands, trying to muddy the waters to give the appearance of an ultra-technical oral seminar that didn't even give complete demonstrations of all the results, especially the trace formulas. These were, however, demonstrated there (and for the first time) in 65/66, and it was there that both he and Deligne had the privilege of learning them, and the delicate technique that goes with it<sup>106</sup> (\*\*).

□ This reminds me that, of course, I had taken the trouble to demonstrate Lefschetz-'s formula.

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Verdier in the seminar - it was the least I could do, and a particularly striking application of the formalism of local and global duality that I set out to develop. The question came to me these days: why on earth, when there were a dozen or so papers still being written by my dear students, so that Deligne and Illusie were spoilt for choice when it came to naming their technical "obstacle" to the publication of SGA 5, they chose the theorem of their good friend Verdier, who at the time was taking the credit for it as his own, just as he had never bothered to write (or at least make available to the public) the theorem of derived and triangulated categories. There's a kind of **defiance** in the absurdity (or in a kind of collective cynicism in the group of my ex-cohomology students, whom I consider to be in solidarity in this operation-massacre), which reminds me of the "weight-complexes" brilliantly invented by Verdier the previous year (see the note of that name, n° 83), or (in the iniquitous register) with the "perverse" name given by Deligne to bundles that should be called "Mebkhout bundles" (see the note "La Perversité", n° 76). I sense such inventions as acts of domination and contempt towards the entire mathematical community.

- and at the same time a **gamble**, which was clearly won until the unexpected appearance of the deceased, who appears almost as the only one awake before a community of sleepyheads...

**note 87<sub>3</sub>** (June 5) After this assessment of a massacre, we will appreciate the value of Illusie's statement in line 2 of his introduction to the volume entitled SGA 5:

"Compared with the original version, the only significant changes concern Lecture II [generic K nneth for- mules], which has not been reproduced, and Lecture III [Lefschetz- formula], which has been reproduced.

Verdier], which has been completely rewritten and expanded with an appendix numbered III B<sup>107</sup> (\*). A part some changes of □ detail and additions of footnotes, the other presentations have been left **as they are**" (emphasis added).

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Here again, Illusie complacently echoes another well-sent joke from his inenarrable friend, namely that the existence of SGA 4<sup>1</sup> "will soon<sub>2</sub> allow SGA 5 to be published **as is**" (see "Clean slate" note n° 67) - and Illusie does his utmost in his talks and introductions to lend credence to this.

<sup>106</sup>(\*\*) In the second paragraph of the Introduction to the volume published as SGA 5, Illusie presents as "the heart of the seminar" the three lectures III, III B, XII on Lefschetz's formula in stale cohomology, whereas we have seen that in the introduction to lecture III B., he takes care to specify (contrary to reality) that "this lecture corresponds to no oral lecture of the seminar" and that in the introductions to lectures III and III B., he does his utmost to give the impression that these are "the heart of the seminar", he is careful to specify (contrary to reality) that "this lecture does not correspond to any oral lecture of the seminar" and that in the introductions to lectures III and III B, he does his utmost to give the impression that these are subordinate to SGA 4<sup>1</sup>, and that lecture III is presented as "conjectural"! ! In fact, the entire SGA 5 seminar was technically independent of Lecture III (Lefschetz-Verdier formula), which played the role of a heuristic motivator, and Lecture III B is no more than the "hole" (Lecture XI) created by the move to Bucur, which was the welcome pretext for this further dismemberment.

To lend credence to the version of a seminar of "technical digressions" (blown up by his friend Deligne), Illusie was c a r e f u l t o skip the introductory presentation, in which I had painted a preliminary picture of the main themes that were to be developed in this seminar, a picture in which the trace formulas form only a small part (taking on particular importance because of their arithmetical implications, in the direction of Weil's conjectures). For an overview of these "big themes", see sub-note no.° 87<sub>5</sub> below.

<sup>107</sup>(\*) Which is presented as part of the "heart of the seminar"! (See previous b. de p. note.)

imposture (that SGA 5, where he and his friend learned their trade, would depend on the volume-pirate SGA 4<sup>1</sup>, made of bries and bracs gleaned or plundered over the following twelve years), by a luxury of cross-references to SGA 4<sup>1</sup> every page turn...

The final word goes (as it should) to Deligne, who wrote to me a month ago (May 3), in response to a laconic request for information (see the beginning of the note "Les Obsèques", n° 70):

"In short, if it had been seven years since you were doing maths [?!] when this SGA 4 text<sup>1</sup><sub>2</sub> appeared, this simply corresponds [?] to the long delay in editing SGA 5, **which was too incomplete to be usefully published as it stood.**

I hope these explanations please you."

If they haven't "approved" me, at least they've edified me. . .

**Note 87<sub>4</sub>** (June 6) Perhaps it's time to indicate what the main themes were that were developed in the oral seminar, and of which the published text gives an idea only by cross-checking.

I) Local aspects of duality theory, whose essential technical ingredient is (as in the coherent case) the biduality theorem (supplemented by a "cohomological purity" theorem). I have the impression that the geometrical meaning of the latter theorem, as a local Poincaré duality theorem, which I had explained so well in the oral seminar, has since been entirely forgotten by my former students<sup>108</sup> (\*).

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II) Trace formulas, including "non-commutative" trace formulas more subtle than the formula of the usual traces (where both members are integers, or more generally elements of the ring of coefficients, such as  $\mathbb{Z}/n\mathbb{Z}$  or an  $\ell$ -adic ring  $Z$ , or even  $Q$ ), placing ourselves in the algebra of a  $\square$ -finite operating group on the considered scheme, with coefficients in a suitable ring (such as those considered in the previous parenthesis). This generalization came very naturally, since even in the case of Lefschetz formulas of the usual type, but for "twisted" bundles of coefficients, we were led to replace the initial scheme by a Galois coating (usually branched) serving to "untwist" the coefficients, with the Galois group operating on it. Nielsen-Wecken" type formulas are thus naturally introduced into the schematic context.

III) Euler-Poincaré formulas. On the one hand, there was a detailed study of an "absolute" formula for algebraic curves, using Serre-Swan modules (generalizing the case of moderately branched coefficients, giving rise to the more naive Ogg-Chafarévitch-Grothendieck formula). On the other hand, there were novel and profound conjectures of the "discrete" Riemann-Roch type, one of which reappeared seven years later, in a hybrid version, under the name of the "Deligne-Grothendieck conjecture", proved by Mac Pherson by transcendental means (see note n° 87 ).<sub>1</sub>

The comments I couldn't fail to make on the profound relationships between these two themes (Lefschetz formulas, Euler-Poincaré formulas) were also lost without trace. (As was my habit, I left all my handwritten notes to the volunteer-writers, and no written trace remains of the oral seminar, of which I did, of course, have a complete set of handwritten notes, even if some of them were succinct).

IV) Detailed formalism for homology and cohomology classes associated with a cycle, derived naturally from the general duality formalism and the key idea of working with the cohomology "with supports" in the cycle under consideration, using cohomological purity theorems.

<sup>108</sup>(\*) This geometrical interpretation has at least been preserved in Illusie's writing.

V) Finiteness theorems (including generic finiteness theorems) and generic Künneth theorems for cohomology with any support.

The seminar also developed a technique for passing from torsion coefficients to  $\ell$ -adic coefficients (lectures V and VI). This was the most technical part of the seminar, which as a rule worked with torsion coefficients, and then "go to the limit" to deduce the corresponding  $\ell$ -adic results. This point of view was a  $\text{pro-}\ell$ visory  $\text{pis-aller}$ , pending Jouanolou's thesis (still unpublished at (at present) providing the formalism needed directly in the  $\ell$ -adic framework.

I don't include among the main "themes" the calculations of some classical schemes and the cohomological theory of Chern classes, which Illusie highlights in his introduction as "one of the most interesting" of the seminar. Since the program was so full, I didn't feel it necessary in the oral seminar to dwell on these calculations and this construction, since it was sufficient to repeat, practically verbatim, the reasoning I had given ten years earlier in the context of Chow rings, on the occasion of the Riemann-Roch theorem. On the other hand, it was obvious that it had to be included in the written seminar, to provide a useful reference for the user of étale cohomology. Jouanolou took on this task (Lecture VIII), which he had to regard not as a service to the mathematical community, while learning basic techniques essential for his own use, but as a chore, since its writing dragged on for years<sup>109</sup> (\*). It was no different, it seems, for his thesis, which remains a ghostly reference just like Verdier's. ... The "passage à la limite" section shouldn't be counted as one of the seminar's "main themes" either, in the sense that it isn't associated with any particular geometrical idea. Rather, it reflects a technical complication peculiar to the context of étale cohomology (distinguishing it from transcendental contexts), namely that the main theorems on étale cohomology concern in the first place **torsion** coefficients (prime to residual characteristics), and that to have a theory that corresponds to rings of coefficients of zero characteristic (as is necessary for Weil's conjectures), one must pass to the limit on rings

coefficients  $Z/\ell^n Z$  to obtain "l-adic" results.

All this said, the only one of the five main themes of the oral seminar that appears in complete form in the published text is theme I. Themes IV and V have simply disappeared, absorbed by SGA 4. Themes IV and V have disappeared altogether, absorbed by SGA 4<sup>1</sup>, with the advantage of being able to refer to them extensively and give the impression that SGA 5 depends on a text by Deligne that appears to predate it. Themes II and III appear in the published volume in mutilated form, still maintaining the same imposture of dependence on the SGA 4 text<sup>1</sup> (which in reality emerged entirely from the mother seminar SGA 4, SGA 5).

### 11.3.6. The skinning

**Note 88** □(May 16) The set of two consecutive seminars SGA 4 and SGA 5 (which for me are like a single "seminar") develops from nothingness both the powerful instrument of synthesis and discovery represented by the **language of** topos, and the perfectly perfected, perfectly effective **tool of** stable cohomology - better understood in its essential formal properties, from that moment on, than even the cohomological theory of ordinary spaces<sup>110</sup> (\*). This whole represents the most profound and innovative contribution I have made to mathematics, at the level of a fully completed work. At the same time, and without wishing to be, while at every moment everything unfolds with the naturalness of

<sup>109</sup>(\*) (June 12) Going through the presentation in question, I was convinced of Jouanolou's perfect connivance with my other cohomology students.

<sup>110</sup>(\*) Even if we restrict ourselves to the spaces closest to "varieties", such as triangulable spaces.

obvious things, this work represents the most extensive technical "tour de force" I've accomplished in my work as a mathematician<sup>111</sup> (\*\*). For me, these two seminars are indissolubly linked. They represent, in their unity, both the **vision** and the **tool** - the topos, and a complete formalism of scalar cohomology.

While the vision is still rejected today, the tool has, over the last twenty years or so, profoundly re-imagined algebraic geometry in its most fascinating aspect for me - the "arithmetic" aspect, apprehended by an intuition, and by a conceptual and technical baggage, of a "geometric" nature.

It was certainly not only the intention of suggesting that his cohomological "digest" **had anteriority** over the SGA 5 part that motivated Deligne to give it the misleading name SGA 4<sup>1</sup> - nothing prevented him from doing so.

after all, while we're at it, to call it SGA 3<sup>1</sup> ! In "Operation SGA 4<sup>1</sup>" I sense the intention to present the work from which all his own work stems (the work from which he cannot detach himself!) - a work of obvious and profound unity, clearly apparent in the two seminars, SGA 4 and (the real) SGA 5, as thing **divided** (as itself is divided. . .), **cut in two** by this violent insertion of a foreign and disdainful text; of a text that would like to present itself as the living heart, the quintessence of a thought, of a vision in which he had no part<sup>112</sup> (\*), and the two "quarters" that surround it as a sort of vaguely grotesque appendix, like a collection of "digressions" and "technical complements" to the work that Deligne claims to be central and essential, and in which my humble self is graciously admitted (before total burial) to the number of "collaborators"<sup>113</sup> (\*\*).

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Chance" had done it right. This "corpse left to mercy - this "unfortunate seminar" always left behind by the "editors", and at the time of my departure in the hands and at the discretion of my cohomology students - this was not **just any** part of the master's work! It wasn't SGA 1 and SGA 2 (where I was developing, in my corner and without even realizing it, the tools that were to be the two essential technical aids for the "take-off" of the main work to come), nor SGA 3 (where my contribution consisted mainly of incessant - and sometimes arduous - scales and arpeggios to hone the "all-out" technique of schematics), nor SGA 6 (systematically developing my ten-year-old ideas on the Riemann-Roch theorem and the intersection formalism), or even SGA 7 (which, through the inner logic of a reflection, stems from the possession of the central tool, mastery of cohomology). It is indeed the **main part of** my work, the writing of which had remained unfinished (and by their care. . .), that I have left, at least in part, in the hands of my cohomology students. It was this main part of the work that they chose to massacre and appropriate as their own, forgetting the unity that is their meaning and beauty, and their creative virtue (90).

And it's no coincidence either that, equipped with heterogeneous tools and denying the spirit and vision that had brought them into being from nothing, none were able to discern the innovative work where it was being reborn, against their indifference and disdain. Nor that after six years, when at the end of the line the new tool was finally

apprehended by Deligne, they unanimously buried the one who had created it in solitude - Zoghman Mebkhout, ☐the disowned master's posthumous pupil! And it's no more a coincidence that, after the momentum had died down

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Deligne's initial work (which in just a few years had led him to launch a new theory of

<sup>111</sup>(\*\*) Some difficult or unforeseen results were obtained by others (Artin, Verdier, Giraud, Deligne), and some parts of the work were done in collaboration with others. This in no way detracts (in my mind at least) from the strength of my appreciation of the place of this work in my body of work. I intend to come back to this point in more detail, in an appendix to the Thematic Outline, and to dot the i's and cross the t's where it has obviously become necessary.

<sup>112</sup>(\*) This line of thought had reached full maturity, in terms of both key ideas and essential results, even before the young man Deligne appeared on the scene to learn algebraic geometry and cohomological techniques from me, between 1965 and 1969. (May 30) On this subject, see the note "L'être à part", n° 67'.

<sup>113</sup>(\*\*) See notes "Le feu vert", "Le renversement", n° s 68, 68'.

Hodge, and towards the demonstration of Weil's conjectures), and despite its prodigious means and the brilliant means of my cohomology students, I note today this "morose stagnation" in a field of prodigious richness where everything still seems to be to be done. This should come as no surprise, when for nearly fifteen years the main source of inspiration and some of the "big problems"<sup>114</sup> (\*), even though they are present and confronting us at every step, remain carefully bypassed and concealed, like the messengers of the one whom for fifteen years it has been our constant aim to bury.

### 11.3.7. . . and the body

**Note** 89 (May 17) The thought, the vision of things that lived within me and that I had thought I was communicating, I see as a living body, healthy and harmonious, animated by the power of renewal of living things, the power to conceive and engender. And now this living body has become a **corpse**, shared between some and others - this limb or quarter, duly stuffed, serving as a trophy for one, another, butchered, as a puzzle or boomerang for another, and yet another, who knows, as it is, for the family kitchen (we're not that far from it!) - and all the rest is good for rotting in the dump... .

That's the picture I've come to see, in terms that may be colorful but seem to express a certain reality of things. The puzzle may well fracture a skull here and there<sup>115</sup> (\*\*)

- but never will these scattered pieces, neither trophy nor puzzle nor family soup, have the power so simple and obvious in the living body: that of the loving embrace that creates a new being. ...

(May 18) This image of the living body, and of the "remains" with its pieces scattered to the four winds, must have been forming in me throughout the past week. □ The comical form in which it presented itself under my pen-p

typewriter in no way implies that this image is in the least an **invention**, a tad macabre, a burlesque improvisation on the spur of the moment. The image expresses a **reality**, felt pro- fondly at the moment it took material form through a written formulation. I must have been aware of this reality in bits and pieces, here and there, over the fourteen years since my "departure", and perhaps even before. Bits and pieces of information recorded at first on a superficial level by a distracted attention, absorbed elsewhere - but which all pointed in the same direction, and which must have assembled, o n a deeper level, into a certain image - an unformulated image that I didn't bother to take note of, when I had other things to worry about. This image has been considerably enriched and clarified in the course of reflection since the end of March, six or seven weeks ago. More precisely, scattered pieces of information, finally examined by the care of a fully present conscious attention, have been gradually assembled into **another** image, at the more superficial level of the thought that examines and probes, through work that might seem independent of the presence, in deeper layers, of the first. This conscious work culminated six days ago in the sudden vision of the "massacre" that took place - when I felt the "breath", the "smell" of a **violence**, for the first time I believe in the whole reflection<sup>116</sup> (\*). It was also the moment when, in the layers close to the surface, that

<sup>114</sup>(\*) This "main source of inspiration" is, of course, the "yoga of motives". It has been active in Deligne alone, who has kept it to himself for his own "benefit", and in a narrow form deprived of much of its force, denying some of the essential aspects of this yoga. Among the "great problems" inspired by this one, which have been ignored or discreetly discredited, I see right now (outsider that I am) the standard conjectures, and the development of the formalism of the "six operations" for all the usual types of coefficients, more or less close to the "motifs" themselves (which play in their respect the role of "universal" coefficients - those which give rise to all the others). Compare with the comments to this

See "My orphans", n° 46.

<sup>115</sup>(\*\*) (May 31) And it will even be used to prove a theorem "of proverbial difficulty"!

<sup>116</sup>(\*) (June 12) In recent years, I've sensed violent intent on the part of some of my ex-students towards some of my "co-students", but never a violence that could be felt as coming from a collective will (grouping five of them here).

the feeling of a living, harmonious body, which is well and truly "massacred" - and also the one in which the deeper, diffuse image must have begun to surface, perhaps bringing to the image-in-form a carnal dimension, a "smell" that thought alone is powerless to give.

This "carnal" aspect came to the fore again in a dream last night - it's under the impulse of this dream that I now return to the lines I wrote yesterday. In this dream, I was cut quite deeply in several places on my body. First of all, there were cuts on my lips and in my mouth itself, bleeding profusely as I rinsed out my mouth with copious amounts of water (heavily reddened by blood) in front of my face. an ice pack. Then wounds in the belly, also bleeding profusely, especially one from which blood was coming out.

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□ by jerks, as if it were an artery (the Dreamer didn't care about anatomical realism). The thought I pressed my hand in front of the wound and curled up to stop the blood - it did indeed stop flowing, eventually forming a clot and a very large scab. Later, when I carefully lifted the scab, delicate healing had already begun. I was also cut on one finger, and it was surrounded by an impressive dressing doll... ...

I have no intention of embarking on a more delicate and detailed description of this dream, nor of probing it in depth here (or elsewhere). What this dream "as is" already reveals to me with startling force is that this "body" of which I spoke yesterday, and which as I was writing I saw as detached from me, like a child perhaps that I had conceived and procreated and which had gone out into the world to follow its own path - this body remains today an intimate part of my person: that it is **my** body, made of flesh and blood and a life force that enables it to survive deep wounds and regenerate itself. And my body is also, without doubt, the thing in the world to which I am most deeply, most indissolubly linked. ...

The Dreamer did not follow me in the image of the "massacre" and the sharing of the remains. This image was meant to convey the reality of intentions, of dispositions in **others** that I had strongly perceived, and not the way in which I myself experienced this aggression, this mutilation of which I was the object through something to which I remain closely linked. The Dreamer has just given me a glimpse of the extent to which I remain linked to it. This ties in with what I perceived (albeit less forcefully) in the reflection in the note "Le retour des choses - ou un pied dans le plat" (n° 73), where I try to pinpoint the feeling of this "deep link between the one who conceived a thing, and this thing", which appeared in the course of the reflection that day. Before that reflection on April 30 (barely three weeks ago), and for the rest of my life, I have pretended to ignore this link, or at least to minimize it, following the well-trodden path of current clichés. Worrying about the fate of a work that has left our hands, and of course worrying about whether our name remains attached to it in any way, is felt to be petty and mean-spirited - whereas it seems natural to everyone that we should be deeply touched when a child of the flesh we have raised (and believe we have loved) chooses to repudiate the name he or she was given at birth.

### 11.3.8. The heir

□

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**Note 90** (May 18) I don't know if any student in the sixties (apart from Deligne) was able to sense this essential unity, beyond the limited work he was doing with me. Some may have sensed this in a confused way, and that this perception was lost without return in the years following my departure. What is certain, however, is that from our first contact in 1965, Deligne sensed this living unity. It was this fine perception of a unity of purpose in a vast design that was surely the main stimulus for his intense interest in everything I had to communicate and pass on. This interest

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people) and directed against me, through my work.



demonstrated, without ever wavering, throughout the four years of constant mathematical contact between 1965 and 1969<sup>117</sup> (\*). He gave mathematical communication between us that exceptional quality I've already mentioned, and which I've experienced with other mathematician friends only in rare moments. It was this perception of the essential, and the passionate interest it stimulated in him, that enabled him to learn as if by playing everything I could teach him: both the technical means (zinc strand diagram technique, Riemann-Roch yoga and intersections, cohomological formalism, étale cohomology, topos language) and the overall **vision** that unites them, and finally the **yoga of patterns** that was then the main fruit of this vision, and the most powerful source of inspiration I had yet discovered.

What's clear is that Deligne was the only one of my students, right up to the present day, who at a certain point (as early as 1968, I believe) had fully assimilated and made his own the totality of what I had to transmit, in its essential unity as well as in the diversity of its means<sup>118</sup> (\*\*). It was, of course, this circumstance, felt by all, that made him the designated "legitimate heir" to my work. work. Clearly, this heritage neither encumbered nor limited him - it was not a burden, but gave him a sense of identity. wings; I mean: he nourished with his vigor these "wings" <sup>□</sup>which he had from birth, like other visions and p. 379 other heritages (less personal of course...) would nourish it...

The heritage he had nurtured in those crucial years of growth and development, and the unity that makes up its beauty and creative virtue, which he had sensed so well and which had become like a part of himself - my friend subsequently<sup>119</sup> (\*) disowned them, striving relentlessly to hide the heritage, and to deny and destroy the creative unity that was its soul. He was the first among my students to set an example by appropriating tools, "pieces", while striving to dislocate the unity, the living body from which they came. His own creative impulse was slowed down, absorbed and finally dislocated by this deep division within him, driving him to deny and destroy the very thing that made him strong, that nourished his impulse.

I see this division expressed in three interdependent, indissolubly linked effects. One is the dissipation of energy, scattered in the effort to deny, dislocate, supplant, hide. The other lies in the rejection of certain ideas and means, essential to the subject's "natural" development. which he has chosen as his central theme<sup>120</sup> (\*\*). The third is the attachment to this theme, of all themes, which is about to supplant, to oust a master who is present every step of the way and who must be constantly erased - the very theme p. 380

who is most intensely invested with the fundamental contradiction that dominated his life as a mathematician.

What I know at first hand, and a basic instinct or flair that has never deceived me, make

<sup>117</sup>(\*) This period comprises five years, of which my friend spent one (1966) in Belgium doing his military service.

<sup>118</sup>(\*\*) When I say "totality", I mean everything that was essential, both in vision and in means. This doesn't mean, of course, that there weren't unpublished ideas and results that I never thought of telling him about. On the other hand, I don't think there was any mathematical reflection from the years 1965-69 that I didn't talk about "on the spot" to my friend, always with pleasure and profit.

<sup>119</sup>(\*) Strangely enough, this division must have been present as early as the first year of our meeting (already expressed in an ambiguous attitude towards the SGA 5 seminar, which was his first contact with schemes, Grothendieck-style cohomological techniques, and staggered cohomology), and at the latest and in unequivocal form as early as 1968 (see note "L'éviction", n° 63). - at a time, therefore, when mathematical communication was perfect, and when the growth of his mathematical thought seems to me not yet marked by the conflict. At the time, he made a number of interesting contributions (which I take great pleasure in highlighting in the Introduction to SGA 4) on topics that he did his utmost, immediately after my departure, to bury.

<sup>120</sup>(\*\*) This refusal has manifested itself in the burial of derived and triangulated categories (until 1981), of the formalism of the six variances (until today), of the language of topos (also), and by a sort of "blocking by disdain" of the vast program of foundations for homological and homotopic algebra, which I'm now trying (twenty years later) to sketch out with La Poursuite des Champs, and which he had of course also felt the need for. However, even though it was inspired by the yoga of motifs (buried until 1982), this yoga remained mutilated of part of its force, being detached from the formalism of the six variances which constitutes an essential formal aspect. It seems to me that this aspect has also been rigorously banished from Hodge-Deligne's theory.

it's quite clear to me that if Deligne hadn't been torn by this profound contradiction in his own work, mathematics today wouldn't be what it is<sup>121</sup> (\*) - that it would have undergone, in several of its essential parts, far-reaching renewals like the one I myself had been the main instrument of - the very one that this same Deligne was bent on countering and diverting!<sup>122</sup> (\*\*)

There was also no doubt that he was ideally suited to be the driving force behind a powerful school of geometry, a continuation of the one that had formed around me - a school nourished by the vigor of the one from which it had emerged, and the creative power of the one who was taking over from me. But this school that had formed around me, this nurturing matrix that had surrounded intense years of training - it broke up the day after my death.

p. 381 of my departure. If this was the case, it was precisely because I couldn't find, in the person who was obviously taking over from me<sup>123</sup> (\*\*\*), the person who would also be the soul of a group united by a common adventure, for a common task.

whose dimensions are beyond anyone's means.

I have the impression that, after my departure, each of my students found themselves in their own corner, with a wealth of work to do - there's no shortage of that anywhere in maths - but without this "corner" fitting into a whole, and without this "work" being carried along by a current, by a wider purpose. Surely, as soon as I left, if not even before, the eyes of most of my students or ex-students were focused on the designated "successor", the most brilliant among them and also the closest to me. At this sensitive moment, my friend must have felt, perhaps for the first time in his life, the power over others that was suddenly in his hands, the power of life or death he had over the fate of a certain school from which he had come, and whose friends he had rubbed shoulders with for four years were no doubt expecting him to ensure its continuity. The situation was entirely in his hands, and it was he who would set the tone... He did indeed set the tone, by destroying the legacy, and first and foremost that confidence and expectation<sup>124</sup> (\*) which those who, with him, had been pupils of the same master, could not fail to bring him. ...

I'm sure many people are impressed by Deligne's work, and not without reason. But I'm also well aware that this work, beyond the impressive initial impetus (ending with the demonstration of Weil's conjectures), is far from "living up to its potential". It certainly testifies to an uncommon technical mastery and ease, placing him among the "best". But it lacks the humble virtue that

<sup>121</sup>(\*) When I wrote these lines about "mathematics today", I wasn't just thinking about the more or less profound knowledge we have of mathematical things today. I was also thinking, in the background, of a certain **spirit** in the world of mathematicians, and more particularly in what might be called (without sarcastic or mocking intonation) "the big mathematical world": the one that "sets the tone" for deciding what is "important", even "licit", and what is not, and the one that also controls the means of information and, to a large extent, careers. Perhaps I'm exaggerating the importance that a single person, in the position of figure de proue, can have on the "spirit of the times" in a given milieu at a given time. Deligne's career seems to me to be comparable (for better or for worse) to Weil's in the milieu that had welcomed me twenty years earlier, and with which I had identified myself for twenty years.

(May 31) Compare with the (complementary) reflections in the note "Le Fossoyeur - ou la Congrégation toute entière", n° 97.

<sup>122</sup>(\*\*) (June 16) I'm convinced that the very fact that the key ideas I've introduced into mathematics are already developing normally, on the momentum gained in the sixties (cut short by the "chainsaw effect" discussed in the next two notes. . . ), mathematics today, fifteen years after my departure, would have been different from what it is, in some of its essential parts.

...  
<sup>123</sup>(\*\*\*) This **de facto succession** was expressed by unequivocal concrete signs: he took over from me at the IHES (from which I left the year after he joined - see note "L'éviction", n° 63), and he took over, with the means I had developed at this final for some fifteen years (from 1955 to 1970), the central theme of the cohomology of algebraic varieties.

<sup>124</sup>(\*) (May 26) In the course of further reflection, I detected yet another "expectation" regarding my tacit heir, this time coming not from my students alone, but from "the entire Congregation" - see the end of the note "Le Fossoyeur - ou la Congrégation toute entière" (n° 97). I have little doubt that these two opposing expectations, one linked to a moment in time and the other continuing throughout the fourteen years of Burial, are both real. Much more so, I'm inclined to think that for many of my former students, the two expectations must have been present simultaneously: that of finding in the most brilliant among them the one who would ensure the continuity of a School and a work in which they had their place and their part - and that of seeing erased (if that were possible) all trace of the one whose departure suddenly called out to them with such force, in the quietude of the well-trodden paths. ...

the virtue of renewal. This virtue he carried within him, this freshness or innocence of the little child, has long since been deeply buried, denied. I was about to write that by this "virtue" and by his not very <sup>common</sup> gifts, as well as by the exceptional circumstances of which he p. 382 benefited from the deployment of his gifts, Deligne was called upon to "dominate" the mathematics of our time, just as a Riemann, or a Hilbert, had each "dominated" the mathematics of their time. Inveterate habits of thought, rooted in common parlance, have suggested to me this image of "domination", which nevertheless gives a distorted apprehension of reality. These great men undoubtedly fully "grasped", "assimilated", "made their own" the mathematics known in their time, which undoubtedly also gave them an exceptional mastery of technical means. But if they rightly seem "great" to us, it's not through their technical prowess, "wresting" difficult demonstrations from surly substance. It's by the renewal each of them brought to several important parts of mathematics, by simple and fruitful "ideas", that is to say: for having turned their gaze on simple and essential things, to which no one before them had deigned to pay attention. This childlike ability to **see** simple, essential things, however humble and disdained by all - **this is the** power of renewal, the creative power in everyone. This power was present to a rare degree in the young man I knew, unknown to all, a modest and passionate lover of mathematics. Over the years, this humble "power" has seemed to disappear from the person of the admired and feared mathematician, enjoying unfettered his prestige, and the (sometimes discretionary) power it gives him over others.

This **stifling** in my friend of something very delicate and very vivid, neglected by everyone and which has creative power, I've felt it many times since I left, and more and more in recent years. But it took the discoveries of the last few weeks, and the reflection I've been pursuing since the end of March (following on from Récoltes et Semailles), to begin to feel the full extent of the devastating effect of this suffocation in the life of my friend, and among many others I've known closely. Not only on some of my "later" (and assimilated) students, who were subjected to his malevolence (perhaps unconscious in some cases), which was exercised against each and every one of them and weighed heavily on three of them; but also, it seems to me

glimpse it now, among my students "before", by the destruction of a **continuity** in the subject, and that of a sense of a whole, a unity, giving a deeper and broader meaning to their work than ce <sup>lui</sup> p. 383 an accumulation of separate prints bearing their names (91)<sup>125</sup> (\*).

More than once in the last seven years, and more than once again in the last few weeks and days, I've felt a sadness, at what feels, on some level, like an immense **waste** - when what is most precious in oneself and in others is squandered or smothered as if for pleasure. Yet I've also come to learn that such "waste" is a staple of the human condition, to be found in one form or another everywhere, in the lives of individuals, from the humblest to the most illustrious, as well as in the lives of peoples and nations. This very "mess", which is nothing other than the action of conflict and division in everyone's life, is a substance of a richness and depth that I have barely begun to fathom, a nourishment that it is up to me to "eat" and assimilate. So this mess, and every other mess I encounter at every step, and every other thing that happens to me at the turn of the road and is so often unwelcome - this mess and other unwelcome things carry within them a  **blessing**. If meditation has meaning, if it has the power of renewal, it's insofar as it enables me to receive

<sup>125</sup>(\*) (June 16) This second aspect only came to my attention in the course of my reflection on L'Enterrement. If ever I saw a prestigious mathematician make use of the "power to discourage", it was the very man who once seemed to me to be my heir apparent. When I wrote the section "The power to discourage", I had thought a lot about him (before the thought came back to me), but without yet having the slightest inkling (at least not on a conscious level) of the extent to which this power had found occasion to be exercised among the very people for whom he must have been (as he was for me) the model of the perfect mathematician. ...

the benefit of that which (through my inveterate reflexes) presents itself as "evil", where it allows me to **nourish** what seems designed to destroy.

Nourishing yourself with your experience, letting it renew you instead of constantly evading it - that's what it means to take full responsibility for your life. I have this power within me, and it's up to me at any given moment to make use of it, or to let it go to waste. It's the same for my friend Pierre, and for each of my students - free, like me, to feed on the "mess" I'm finishing reviewing in these last days of a long meditation. And the same goes for the reader who reads these lines, intended for him or her.

### 11.3.9. The co-heirs

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p. 384 **Note** 91 (May 19) The echoes that have reached me here and there about my former pupils have been more than sparse.

Hardly any of them gave me any sign of life after my departure, if only by sending me prints<sup>126</sup> (\*). However, by collecting the few that have reached me, I can form an idea, admittedly very approximate. Perhaps it will become clearer in the months to come, if this reflection prompts some of them to come forward.

I've already had occasion to note the profound rupture in Deligne's work after my departure, even though in some respects he appears, unwillingly, as a successor, and therefore as part of a certain continuity. And I had the feeling that this rupture must have had a profound effect on the work of all my other students. It's this impression that I'd like to explore a little more closely.

The only one of these students whose work seems to be an obvious (at least at first sight) extension of the work he had done with me, seems to be Berthelot<sup>127</sup> (\*\*). He's also the only one who, for a long time, sent me numerous separate prints - perhaps even all his separate prints. They are all on the difficult subject of crystalline cohomology, the systematic start-up of which is the subject of his thesis. Yet it seems to me that, as with my other (commutative) "cohomology" students, his work is marked by the disaffection of some of the main ideas I had introduced: derived categories (and triangulated categories, cleared by Verdier), six-operation formalism, topos (91<sub>1</sub>). As Zoghman Mebkhout himself says, his own work, so close in theme to Berthelot's (91<sub>2</sub>), is in line with these ideas, combined with those of the Sato school. If they hadn't been repudiated by my cohomology students, led by Deligne and Verdier, there's a good chance that from the very beginning of the seventies,

p. 385 Mebkhout's crystalline theory (which he began to develop only from 1975, against the disinterest of these same students) would already have reached the full maturity of a formalism of <sup>six</sup>□operations, which it still hasn't reached today<sup>128</sup> (\*).

Incidentally, I remember talking to Verdier about the question, which intrigued me, of the link between constructible discrete coefficients and continuous coefficients, without it seeming to catch his eye. It must have caught on later

<sup>126</sup>(\*) (May 31) On this subject, see note no. ° 84, following the note on "Silence" (no. ° 84).

<sup>127</sup>(\*\*) Based on the theme of duality that Verdier pursued for several years after I left, in the context of the spaces

I'm sure that, in the case of Berthelot, there's an impression of continuity. But it seems to me that this has been something of a "routine continuity", whereas the one whose signs (or absence of signs) I'm mainly looking for is a creative continuity, continuing an initial impulse into the unknown. ...

<sup>128</sup>(\*) (June 7) I hesitated to hazard this assessment, which could be interpreted as minimizing the originality of Mebkhout's theory. This would not be at all in line with my thinking, and all the more so as I have an excellent opinion of the abilities of each of my cohomology students (when they are not blocked by prejudices alien to mathematical common sense). My friend Zoghman himself dispelled any scruples I might have had, saying that he was convinced that "normally", it was my students who should have been developing his theory from the very beginning in the 70s. At a certain level, they're surely the first to be convinced: it's they, or Deligne, who **should have** developed it - and with the general degradation of morals, that's all it takes to behave as if they did (or as if Deligne did).

was) well and truly! On this subject, see the notes "Le Colloque" and "La mystification", n° s 75' and 85'.

(See note "L'inconnu de service et le théorème du bon Dieu", n° '48.) In fact, he was so "blocked" by his burial syndrome that, until October 1980, he failed to perceive the importance of Mebkhout's work - and when he finally did, it was in the grave-digging mood we all know (see notes n° s 75 to 76).

As far as I'm aware, Verdier's work since his thesis defense has essentially been limited to redoing in the analytical context (which sometimes presents additional technical difficulties) what I had done in the coherent schematic framework, without introducing any new ideas. It's even rather extraordinary, with the reflexes he was supposed to have developed, and well-informed as he was, that he didn't come across Mebkhout's theory himself, by dint of turning his crank - and that he didn't at least recognize that his "pupil" was doing some interesting things, which had escaped him (as they had escaped Deligne).

To tell the truth, while intrigued by the question of the relationship between discrete coefficients and coefficients continuous, I hadn't really had any inkling of Mebkhout's crystalline theory, which would blossom in the decade following my departure. On the other hand, there was a vast theme, arising from my <sup>reflexions</sup> of cohomology both commutative and non-commutative of the fifties (1955-1960), and which was just beginning (in the "commutative" context i.e. in terms of additive categories) in Verdier's work, started in the early sixties and left behind after his defense (see note n° 81). The non-commutative aspect was initiated later in Giraud's thesis, which developed a geometric language, in terms of 1-fields over a topos, for non-commutative cohomology in dimension  $\leq 2$ . By the second half of the sixties, the inadequacy of these two primers was quite obvious: both in terms of the inadequacy of the notion of "triangulated category" (teased out by Verdier) to account for the richness of structure associated with a derived category (a notion destined to be replaced by the considerably richer notion of **derivator**), and in terms of the need to develop a geometric language for non-commutative cohomology in any dimension, in terms of n-fields and  $\infty$ -**fields** over a topos. One sensed (or I sensed) the need for a synthesis of these two approaches, which would serve as a common conceptual foundation for homological algebra and homotopic algebra. Such a work was also in direct continuity with Illusie's thesis work, in which both aspects are represented.

Bousfield-Kan's seminal work on homotopic limits (Lecture Notes n° 304), published in 1972, was also in line with this diffuse program, which since at least 1967 had been just begging to be developed. In January of last year, without yet suspecting that I would be embarking on the Pursuit of Fields a month later, I submitted to Illusie some thoughts on the "integration" of homotopy types (familiar to homotopists as "homotopic (inductive) limits"), at a time when I was still completely unaware of the existence of Bousfield and Kan's work, and that this type of operation had already been examined by others than myself. It turned out that Illusie was equally unaware of it, despite the fact that he was supposed to have remained in homologous-homotopic waters for all the time since my "death" in 1970! This just goes to show how far he seems to have lost touch with certain realities that are part and parcel of the fundamental thinking he himself had been pursuing in the sixties<sup>129</sup> (\*). He must have made his own little hole, from which he hardly ever emerges. ...

□ With the disdain that has befallen the very notion of topos and all the "categorical nonsense", it is not surprising

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<sup>129</sup>(\*) This notion of "integrating" homotopy types had come to me again, in the context of unscrewing stratified structures, which I took up at the end of 1981.

that Giraud now has a total disaffection for what had been his first major theme of work. It's true that Deligne, with the exhumation of motives two years ago, pretended to have suddenly discovered the interest of the arsenal of non-commutative cohomology, sheaves, links and consorts, as if he had just introduced them himself, along with motives and motivic Galois groups<sup>130</sup> (\*). It's doubtful that this kind of circus will rekindle a flame that he himself has worked so hard to extinguish. ... In February last year, I sent Giraud a copy of the twenty-page letter that became Chapter 1 of the opening chapter of *La Poursuite des Champs*. It's a non-technical reflection, in the course of which I managed to "jump with both feet" over the "purgatory" that had once stopped Giraud (and many others) from handling the notion of "non-strict"  $n$ -category (which I now call " $n$ -field"), which remained heuristic and yet was visibly fundamental. This was the start of the *Poursuite des Champs*. When we met (on friendly terms) last December for Contou-Carrère's thesis defense, I learned from Giraud that he hadn't even had the curiosity to read the letter! I got the impression that he'd drawn a long line under such things. The idea that there might be some rich substance, in a direction he had long since abandoned, didn't even seem to occur to him. I tried, unsuccessfully I'm afraid, to get him to understand that there was a juicy and vast work here that had been waiting for nearly twenty years to be done, and which I finally got round to in my old age, to at least give a broad outline, under the dictation of things themselves, of a rich substance that the "deceased" me continues to feel strongly about, while my students have long since forgotten about it.

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Jouanolou also abandoned a research direction he had just begun with his thesis. This direction had become the object of the disdain of a fashion established by the very person who had provided him with a master technical idea for the theme he had chosen. With the "rush" on triangulated categories with the Colloque Pervers three years ago, this same Deligne suddenly pretended (without laughing) to discover the big job of foundations in perspective, the lack of which is suddenly being felt at all ends, and which he had been the first to discourage for ten years - The need for such a job was quite obvious to me as soon as 1963/64 with the beginnings of étale cohomology; and for Deligne just as much, from the moment he started hearing about  $l$ -adic cohomology and triangulated categories, i.e. when he arrived at my seminar the following year. Beyond the construction of "constructible triangulated categories" on the ring  $\mathbb{Z}_l$  (above a basic scheme, let's say), and the development of the formalism of the "six operations" within this framework (something accomplished, it seems to me, in Jouanolou's thesis), to make analogous work by replacing the base ring  $\mathbb{Z}_l$  by an arbitrary (more or less?) Noetherian  $\mathbb{Z}_l$ -algebra, e.g.  $Q_l$  or an (algebraic?) extension of  $Q_l$ . This is one of the things for which time has been ripe for some twenty years, and which are still waiting to be done, when the wind of contempt that has blown over them has died down... .

The natural continuation of Ms. Raynaud's work (weak Lefschetz theorems in staggered cohomology, in terms of 1-fields) would have been placed in a context of strictly taboo  $\infty$ -fields, let's not talk about it! The same goes for Ms. Sinh's work, begun in 1968 and completed only in 1975 - a natural continuation would have been the notion of an enveloping  $\infty$ -category of Picard of a so-called "monomial" category, or of triangulated variants of such a category<sup>131</sup> (\*) - let's not! Another was to transpose her work in terms of fields onto a topos - what a horror! As for Monique Hakim, she too had the misfortune to write her thesis on a subject which, these days since my untimely departure, looks a little ridiculous on

<sup>130</sup>(\*) See "Souvenir d'un rêve. . . - or the birth of motifs", note n° 51.

<sup>131</sup>(\*) See "Souvenir d'un rêve. . . - or the birth of motifs", note n° 51.

edges - relative diagrams on a locally ringed topos, I ask you! His little book on the subject, published in Grundlehren (Springer), must sell three or four copies a year - no wonder I've got bad press there, and they're not too keen to accept any text I might recommend. For me, it was a first test-step towards a "relativization" of all "absolute" notions of "varieties" (algebraic, analytic, etc. . . ) on general "bases", the need for which is obvious to me (91<sub>3</sub> ). It's true that we've done just fine without them until now. But it's also true that we've done without maths for the two million years we've been around.

The fact remains that Monique Hakim, who was not motivated to write her thesis in the same way as I was to offered it to him, surely had no desire to keep any contact with a theme which (detached from p . 389

In the context of a favorable consensus, or stubborn thought pursuing a tenacious and sure vision against all odds, it can no longer make the slightest sense.

As for Neantro Saavedra Rivano, he seems to have disappeared entirely from circulation - I can find no trace of his name even in the official world directory of mathematicians. What is certain is that his somewhat categorical thesis subject could hardly have been in good press with the gentlemen who decide what is serious and what is not. The most natural continuation of this thesis, in my opinion, would have been neither more nor less than this "vaste tableau des motifs", a theme decidedly a little broad for this student's more modest aims. Yet he ended up having the unexpected honor of having his thesis redone ab ovo et in toto by one of these great gentlemen himself, barely two years ago (see notes on "L' Enterrement - ou le Nouveau Père" and "La table rase", n° s 52 and 67.)

Finally, the only ones among my twelve "pre-1970" students for whom it's not too clear to me whether or not there was a more or less drastic or profound **break in** their work, compared to the one they had to follow in my contact, are Michel Demazure and Michel Raynaud (91<sub>4</sub> ). All I know is that they've continued to do maths, and that they're part (as you'd expect, given their brilliant means) of what I called earlier "the great mathematical world".

The foregoing brief reflection, based on what is sometimes very little data, is of course largely hypothetical and very approximate. I hope that those mentioned here will forgive me for any perhaps gross errors of assessment, which I'll be happy to rectify if they'd be so kind as to let me know. Here again, I realize that everyone's case is surely different from everyone else's, and represents a much more complex reality than someone as distant as me can reasonably apprehend, let alone express in a few lines. All these reservations aside, I have the impression that this reflection has not been in vain, for me at least, to identify by a few concrete facts, a still diffuse impression that had emerged yesterday (and which was undoubtedly present at an informal level for many years): that of a **break** that was made p . 390

in many of my students in the aftermath of my departure, reflecting on a personal level the sudden disappearance, overnight, of a "school" to which they must have felt a part during crucial formative years in their mathematical profession.

**Note** 91<sub>1</sub> (May 22) I've just come across an article-survey from the Colloque "Analyse *p*-adique et ses applications" at CIRM, Luminy (September 6-10, 1982), by P. Berthelot, entitled "Géométrie rigide et cohomologie des variétés algébriques de car. *p*" (24 pages), which outlines the main ideas for a synthesis of Dwork-Monsky-Washnitzer cohomology and crystalline cohomology. The initial ideas (and the very name) of crystalline cohomology (inspired by Monsky-Washnitzer cohomology), and the idea of complementing these with the introduction of sites formed by rigid-analytic spaces, ideas that I had introduced in the

In the sixties, they became the daily bread for all those working in the field, starting with Berthelot, whose thesis developed and fleshed out some of these initial ideas. Nevertheless, my name is conspicuously absent from both the text itself and the bibliography. Here we have a fourth clearly identified pupil-croquemort. Who's next?

(June 7) It's a remarkable fact that more than fifteen years after I introduced the starting ideas for crystalline cohomology, and more than ten years after Berthelot's thesis established that the theory was indeed "the right one" for clean, smooth schemes, we still haven't reached what I call a situation of "mastery" of crystalline cohomology, comparable to that developed for stellar cohomology in the SGA 4 and 5 seminar. By "mastery" (in the first degree) of a cohomological formalism including duality phenomena, I mean no more and no less than full possession of a six-operation formalism. While I'm not "in the know" enough to be able to appreciate the difficulties specific to the crystalline context, I wouldn't be surprised if the main reason for this relative stagnation lies in the disaffection of Berthelot and others for the very idea of this formalism, which makes them neglect (just like

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Deligne's Hodge theory, which remained in its infancy) the first essential "level" to be reached in order to have a fully "adult" cohomological formalism. These are  $\square$  the same kind of dispositions

which have surely led him to overlook the relevance of Mebkhout's point of view to his own research. NB When I speak here of "crystalline cohomology" in a context where one abandons assumptions of cleanliness (as is necessary for a "fully grown-up" formalism), it is understood that one is working with a crystalline site whose objects are (power-divided) "thickenings" that are not purely infinitesimal, but are "proper" (power-divided) topological algebras. The need for such an extension of the primitive crystal site (which for me was only a first approximation for the "right" crystal theory) was clear to me from the start, and Berthelot learned it (along with the initial ideas) by null other than myself. A written allusion to this link can be found in *Esquisse Thématique*, 5 e.

**Note 91<sub>2</sub>** It's quite extraordinary that nobody but me seems to have realized that Mebkhout's unnamed theory was an essential new part of a crystalline theory. As someone who has been completely "out of cohomology" for nearly fifteen years, I realized this as soon as Mebkhout took the trouble to explain to me what he had done last year. In any case, when I mentioned the matter (as a matter of course) to Illusie, he seemed to see it as a rather "kooky" combination of things (*D-Modules* and crystals) that really had nothing to do with each other. Yet I know first-hand that he has a mathematician's flair, and so do my other students (cohomologists in this case, starting with Deligne) - but I can see that in certain situations, he's no longer any use to them... ... The more I think about it, the more I find it extraordinary that in such an atmosphere, Mebkhout still managed to do his job, without letting his own mathematical flair be defused by the total incomprehension of his elders, so far above him. . .

**Note 91<sub>3</sub>** It was especially since my lectures at the Séminaire Cartan on the foundations of the theory of complex analytic spaces, and on the precise geometrical interpretation of "level modular varieties" à la Teichmüller, towards the end of the fifties, that I understood the importance of a double generalization of the common notions of "variety" we've been working with so far (algebraic, real or complex analytic, differentiable - or  $\square$  subsequently, their "moderate topology" variants). One is to broaden-

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gir the definition so as to admit arbitrary "singularities", and nilpotent elements in the structural bundle of "scalar functions" - along the lines of my foundational work with the notion of schema.



The other extension is towards a "relativization" above suitable locally annelated topoi ("absolute" notions being obtained by taking a punctual topos as a base). This conceptual work, matured for over twenty-five years and initiated in Monique Hakim's thesis, is still waiting to be taken up. A particularly interesting case is that of the notion of relative rigid-analytic space, which allows us to consider ordinary complex analytic spaces and rigid-analytic spaces over local bodies with variable residual characteristics, as "fibers" of the same relative rigid-analytic space; just as the notion of relative scheme (which has finally become commonplace) allows us to link together algebraic varieties defined over bodies of different characteristics.

**Note 91<sub>4</sub>** While Demazure's thesis work, like Raynaud's, makes essential use of a consummate schematic technique they learned from me, the essential ideas in their respective works are not part of the "Grothendieckian" panoply, which distinguishes their work from that of my other students of the first period. It's possible that this circumstance resulted in a continuity in their work, free from a rupture due to the effect of the "master's burial syndrome". This doesn't necessarily mean that this syndrome didn't affect one or the other in another way. Three years ago, I was struck by Raynaud's attitude towards Contou-Carrère's work on local relative Jacobians. The results announced are profound, difficult and beautiful, and go far beyond a simple generalization of "well-known" things. There's an unexpected link with Cartier's theory of typical curves, some wonderful explicit formulas - all entirely within Raynaud's (and my) grasp. The freshness of his welcome must have weighed decisively in Contou-Carrère's strategic retreat, abandoning for profit and loss a subject in which he had invested himself wholeheartedly and which, it may have seemed, was only going to get him into trouble. ... <sup>132</sup>(\*). My letter to him, in which I expressed my (pained) surprise at his insensitivity to the beauty of these results, went unanswered.

### 11.3.10. . . and the chainsaw

**Note 92** □ When I moved to the area nearly four years ago, there were not far from p . 393 me a beautiful cherry orchard. Often, when I went for a walk, I'd go and have a look. It was a pleasure to see these thick cherry trees in the prime of their lives, with their powerful trunks, which seemed to have always been one with this piece of land, where wild grasses proliferate freely. They must not have known about fertilizers or pesticides, and in cherry season, that's where I'd go to pick tasty ones. There must have been twenty or thirty trees.

One day, when I went back there, I saw all the trunks cut down to man-height, the crowns slumped on the ground next to the trunk, stumps in the air - a vision of carnage. With a good chainsaw, it must have been done in an hour or so. I'd never seen anything like it - when you cut a tree, you usually take the trouble to bend down and cut it flush with the ground. There's a shortage of cherries, of course, and this cherry orchard wasn't going to produce tons, that's for sure - but these stumps of trunks said something other than shortage and yields... .

Yesterday I had that feeling again, of a vigorous trunk, with powerful roots and generous sap, with strong, multiple branches extending its momentum - cut clean off, at man's height, as if for pleasure. It was taking the trouble to look at the main branches one by one, and seeing each one cut off, that finally made me see what had happened. What was made to unfold, in the continuity of a momentum,

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<sup>132</sup>(\*) For further details, see sub-note no. ° 95<sub>1</sub> to note "Cercueil 3 - ou les jacobiniennes un peu trop relatives", no. ° 95.

2. A walk through a work or the Child and Mother

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of a deep-rooted inner necessity, has been sliced clean through, with a clean slice, to see itself designated for all to see as an object of derision.

This reminds me of the "misunderstanding" Zoghman referred to, which supposedly took place between me and my students (except Deligne). What's clear, in fact, is that neither impetus nor vision were communicated from me to any of my students (apart from Deligne, who is decidedly "apart" indeed!). Each of them assimilated a technical baggage, useful (and even indispensable) for doing a well-done job on the subject they had chosen, and which could even be of use to them later on. I can't say whether there was any hint of something else, going beyond that. If there was had, it didn't stand a chance against the chainsaw, which quickly trimmed it... ..

I'm well aware that if there are still people doing maths -□and unless they give up completely the kind of maths we've been doing for over two millennia - they can't help but one day or other breathe new life into each of these branches that I see lying inert. Some of them have already been taken over by my friend-with-the-saw, and it's quite possible, if God gives him life, that he'll do the same with some or all of them. Most of them, however, are no longer in his style. But perhaps he'll also eventually tire of constantly substituting himself for someone else, something that's surely very tiring and, what's more, not at all profitable, so that he'll be content to be himself (which isn't bad at all).

# 15. C) THE BEAUTIFUL WORLD

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## 15.1. VII The Colloquium - or Mebkhout and Perversity bundles

### 15.1.1. Iniquity or the meaning of a return

□

p. 285 **Note 75** (May 2) I'm definitely not done learning! I've just read two

texts, which shed unforeseen light (for me at least) on the "escamotage" (of Meb- khout's work) already mentioned ("L'inconnu de service et le théorème du bon Dieu", note (48)). It concerns the role played by the two illustrious colleagues and former students whose disdainful indifference to Zoghman Mebkhout I noted, without however questioning their professional bona fides. Both texts are part of the Proceedings of the **Luminy Colloquium** (July 6-11, 1981) entitled **Analyse et topologie sur les espaces singuliers**, published in Astérisque n° 100 (1982).

The first of these texts is the introduction to the Colloquium, signed by **B. Teissier** and **J.L. Verdier** (the same man who acted as Z. Mebkhout's official thesis supervisor). This one-and-a-half-page text begins with an explanation of a certain "Riemann-Hilbert correspondence", which is clearly destined to play a leading role in the Colloquium (and which is none other than the "theorem of the good Lord" alias Mebkhout). In this correspondence (and this is what gives it its charm and depth, and necessitates the introduction of derived categories), a regular holonomic **module** (i.e., a regular holonomic complex reduced to degree zero) is associated with a constructible complex of **C-vector** bundles, which can be characterized (it is said) by purely topological properties that make sense for constructible complexes of sheaves over a not necessarily smooth variety defined over any body.

This, it is explained, is the starting point for the Colloquium's "main theme", "**perversity, intersection complex, purity**"-the (complex  $\square$  of) so-called "**perverse**" beams<sup>1</sup> (\*) being none other than those which, "morally", correspond ("à la Mebkhout") to the simplest complexes of regular holonomic differential operators, expressed using a single D-Module. . 286

The second text is part<sup>2</sup> (\*\*) of the long article by **A.A. Beilinson, J. Bernstein and P. Deligne** on perverse bundles, referred to in the introduction as the central work of the Colloquium. As can be seen from the table of contents and the other pages at my disposal, this paper marks the sudden re-entry of derived and triangulated categories into the public arena, in the wake of Mebkhout's obscure work and the famous "Riemann-Hilbert" theorem.

Incredibly, in both texts, Z. Mebkhout's name is absent. Mebkhout is absent, just as he is absent from the bibliography. I should point out that not only was J.L. Verdier perfectly aware of Mebkhout's work (and with good reason!), but so was Deligne (and it would be difficult even to conceive that it could be otherwise, for someone so well informed about current mathematical events, and when it's about the subject that touches him most closely<sup>3</sup> (\*\*\*)).

I don't know what happened to B. Teissier<sup>4</sup> (\*\*\*\*) and the other participants in the Colloque de Luminy, notably the two co-authors with Deligne of the article cited<sup>5</sup> (\*\*\*\*). It seems that none of the participants was so curious to know the authorship of the ideas and the key theorem that had had the virtue of mobilizing them.

<sup>1</sup>(\*) (May 4) See note n° 76, "Perversity", on this strange application.

<sup>2</sup>(\*\*) (May 4) I've since received the full article, which confirms what the part I had already shown me.

<sup>3</sup>(\*\*\*) In particular, Mebkhout's work and his "theorem of the good God" represent a decisive advance on Deligne's earlier work (from 1969), which he refrained from publishing. On this subject, see note n° 48' already quoted.

<sup>4</sup>(\*\*\*\*) (June 12) B. Teissier had long taken an interest in Mebkhout's work, and had been one of the very few to have an encouraging attitude towards him. He was therefore perfectly aware of the fraud, to which he knowingly lent his support. He justified himself to Mebkhout by assuring him that, in any case, he "couldn't have done anything about it".

<sup>5</sup>(\*\*\*\*\*) (May 28) I have since learned that A.A. Beilinson and J. Bernstein were informed of Mebkhout's results by P. Deligne (in October 1980) and by Mebkhout (in detail in November 1980, at a conference in Moscow). These two authors made essential use of the God's theorem in their demonstration of a famous conjecture known as the Kazhdan-Lusztig conjecture even before the Colloque de Luminy in June 1981. Compare the quotation from Zoghman Mebkhout's letter in the note "Un sentiment d'injustice et d'impuissance" (note n° 44").

(June 3) For further details about the solidarity of all Colloquium participants, see the following note "The Colloquium", n° 75'.

I assume that it was taken for granted, a little (a lot) like in the volume of the lecture Notes LN 900 which, the following year, was to consecrate the re-entry of motifs on this same "public square"<sup>6</sup> (\*\*\*\*\*); that paternity belonged to the brightest among the brilliant mathematicians who had taken the initiative of the Colloquium and

p. 287 had animated it. What everyone knew for sure was that it was neither Riemann nor Hilbert, otherwise the brilliant Colloquium  $\square$  would have taken place in 1900 and not in 1981, two years after the pupil's thesis defense.

Unknown by Jean-Louis Verdier.

The kind of operation I've witnessed here is perhaps now commonplace<sup>7</sup> (\*) and perfectly acceptable, as long as it's carried out by mathematicians who are at the top of their game, and the one who pays the price is a vague unknown (even though he's been kindly invited to join in the fun). The fact that one of these men is a great mathematician, both in terms of his means and his work (which puts him above suspicion from the outset), doesn't change the nature of the matter. Surely I'm old-fashioned - in my day, this kind of operation was called a **swindle**, and this one strikes me as a **disgrace** to the generation of mathematicians who tolerate it.

p. 288 The brilliance of genius takes nothing away from such a disgrace. It adds an unprecedented dimension, perhaps unique in  $\square$  the history of our science<sup>8</sup> (\*\*). Behind the apparent absurdity and gratuitousness of the act (carried out by someone whose lot has been fulfilled beyond measure, yet who delights in plundering. . . ), we can glimpse the action of forces other than the mere desire to shine, or the gratuitous desire to humiliate or despair those who feel defenceless and voiceless. ), the action of forces other than the mere desire to shine, or the gratuitous desire to humiliate or despair those who feel defenseless and voiceless.

As I'm definitely in the middle of a "tableau de mœurs", I'd like to point out (almost as a matter of course) that my name is equally absent from the quoted texts. Yet I was pleased to note that there is not a single page of the quoted article (among those in my possession<sup>9</sup> (\*)) that is not deeply rooted in my work and bears its mark, right down to the notations I introduced, and the names used for the notions that come into play at every step - which are the names I gave them when I first became acquainted with them before they were named. There are, of course, some minor adjustments - for example, the biduality theorem that I had worked out in the fifties<sup>10</sup> (\*\*) has been renamed "Verdier duality" for the occasion, still the same Verdier, there's no mistake. ...<sup>11</sup> (\*\*\*). However, it has not been possible for my name not to appear at least implicitly, through occasional references to texts that are still irreplaceable (despite SGA 4<sup>1</sup>, which is not quite sufficient<sub>2</sub> for its purpose), namely EGA and SGA. (In the explanation of the acronym SGA = Séminaire de Géométrie Algébrique du Bois Marie, my name of course does not appear, but in EGA, honest or not, the full designation is given, with the names of the authors including mine... . ) Another detail that struck me, and which testifies to the obsessive strength of the burial syndrome (in someone who, however, has no obsessive "profile" whatsoever): the two references I saw to SGA make a point of explaining each time especially "Mr. Artin's theorem in SGA 4. ... .", lest the misguided reader get the idea that said theorem might be due to the carefully non

<sup>6</sup>(\*\*\*\*\*) See notes n°s 51,52,59.

<sup>7</sup>(\*) I'm thinking of two other "operations" along the same lines, which took shape with the publication of LN 900 (see previous b. de p.) and APG 4<sup>1</sup> five years earlier (see notes n°s 67, 67', 68, 68').

(May 9) For a third such operation, closely related to the previous ones, see the "Good references" note (n° 82). on another "memorable article", this time by J.L. Verdier.

<sup>8</sup>(\*\*) Nor have I ever heard of such a thing in the history of any other science or art than mathematics.

<sup>9</sup>(\*) (May 4) And the others too, of which I have since become aware.

<sup>10</sup>(\*\*) The same goes for the theory of dualité étale, which becomes "dualité de Verdier" under the pen of his generous friend Deligne!

<sup>11</sup>(\*\*\*) (May 5) Compare notes n°s 48', 63" . Throughout this long Burial, which has been going on for nearly fifteen years, and throughout the discovery that the principal "anticipated deceased" has just made over the past month,



J s friend, who lavishes him with the wreaths of flowers that are de rigueur on this mournful occasion.

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named, when it is quite clear that the presentation was indeed made, thank God, by a named author! (77)

□ All of this, it seems, is fair game in the "beau monde" today. Without indulging myself (and it's not meant for that. ... ) this guéguère is not really detrimental to the anticipated deceased, whose symbolic remains are thus left to the vagaries of this fairground, which I have been discovering with wonder for barely two weeks. It doesn't gnaw at my life with the feeling of **iniquity** suffered in impotence. It hasn't broken the joy and impetus that carry me to the encounter with mathematical things and those of the world around me, nor has it burned the delicate beauty of these things in me. I can consider myself happy, and I am... .

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And I'm happy too about my unexpected "return", the meaning of which had escaped me. If it were to teach me only what I have learned in these past days, this return will not have been in vain, as it has already fulfilled me ( $\Rightarrow$  76).

## 15.1.2. The symposium

**Note 75'** (June 3) I have received details of the other participants in the colloquium, which dispel all doubts. Although no talk by Mebkhout had been scheduled in the Colloquium's official program, Verdier was obliged to ask him on the spot and in extremis to give a talk, to make up for the shortcomings of one of the official talks (which had been entrusted to Brylinsk'i, who knew little about D-Module theory). Mebkhout was thus able to set out his ideas and results, and in particular the Good God Theorem, in such a way as to leave no doubt as to the authorship of this theorem, and of the philosophy that goes with it, which had led to the spectacular revival of the cohomology of algebraic varieties, culminating in this Colloque. So, **all the participants in the colloquium were made aware of this paternity**, through this presentation. I also assume that all of them, without exception, have since been acquainted with the Colloquium Proceedings, and in particular with the Introduction and the cited article by Beilinson, Bernstein and Deligne. Not a single one, apparently, found anything wrong with it - or if they did, they didn't let on. Zoghman Mebkhout received no such feedback. So, all the Colloquium's participants can justifiably be considered to be in solidarity with the mystification that took place during the Colloquium.

This collective mystification was already clear at the Colloquium, since no one found anything wrong with the fact that, in Deligne's oral presentation on "perverse" beams, the name of Mebkhout is not pronounced. The speaker confined himself to stating the good Lord's theorem, saying □ that he wasn't going to demonstrate this in his talk. He made it clear, moreover (with the modesty with which he

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is accustomed to) that "there was no merit" in guessing the extraordinary and a priori unpredictable properties of the beams he calls "perverse", obviously suggested by the "Riemann-Hilbert correspondence" he had just mentioned<sup>12</sup> (\*). Everyone found it normal that he should refrain from naming the person who had had the "merit" of discovering this providential correspondence, and that he should give the appearance that the author was none other than himself, even though they had just learned, or would learn in the following days, that this was not the case. It must have been some sort of inadmissible misunderstanding that a vague participant in the Colloquium should be the author of such a remarkable theorem, and everyone did their utmost to rectify the situation and establish a consensus which attributed authorship to the one who was clearly the right person for the job - the one who **should have** been the author<sup>13</sup> (\*\*).

<sup>12</sup>(\*) Compare with pages 10 and 11 of the article quoted.

(June 7) For details on the art of escamotage, see the following note "Le Prestidigitateur", n° 75".

<sup>13</sup>(\*\*) (June 5) everything fits together! The reflection that continued in the "l'Elève" procession (following on from the "Le Colloque" procession), and a certain tone too (notably again in a recent and brief exchange of letters with Deligne, see first footnote to the note "Les obsèques", n° 70), show me that for Deligne and my other cohomologist students, it's clear

Characteristically, **Mebkhout's paper does not appear in the Colloquium proceedings**. Verdier had asked Mebkhout not to write his paper, saying that the Colloquium was intended to present new results, whereas Mebkhout's had already been published for over two years.

When you don't get bogged down in a technical discourse, and look at what's actually been done, you'll see that it's not just a question of the technical.

p. 291 the forces and appetites that animated uni 'and the others, you'd think you were watching a film about mafia rule in the underworld of some far-off ☐Megapolis, It's

The actors are among the noblest jewels of French and international science. The Grand Chef, who runs the operation with his finger on the pulse, is none other than the man who once looked to me like a modest, smiling spiritual son, or at least a (no less modest and smiling) legitimate heir. As for the one who can be drilled and cut, the "soft" one in a world of "hard" ones who don't give quarter, by a strange "coincidence" whose meaning I still don't fully grasp, he too is closely linked to me. He's my "pupil" as is the Great Chief (and like him, "pupil" with quotation marks... ) - the one who took me on when I'd already been declared dead and buried years ago... .

### 15.1.3. The conjurer

**Note!** 75" (June 7) The "memorable article" (referred to in the previous two notes) displays a consummate art of casual evasion. The equivalence of categories that has been the essential motivation of the whole work is introduced for the first time in a sentence in the fourth rage of the Introduction (page 10, lines 9 to 15), without giving it a name, only to be followed immediately by the kyrielle of consequences for the notion of the so-called "perverse" bundle (pages 10 and 11). No further mention is made until the end of page 16, when we read<sup>14</sup> (\*):

"We would like to point out that on the following points, **which would have found their place in these notes**, we have failed in our task.

- The relationship between perverse beams and holonomic modules. As mentioned in this introduction, it has played an important heuristic role. The essential statement is 4.1.9 (**not proved here**). . . "  
(To continue with other "points that would have found their place. . . ")

I hasten to find out what this "essential statement" is that the authors haven't found the leisure to include in their work, or at least not to demonstrate. Let's look for it:° 4.1.9. ... I'm looking for an "essential statement", a theorem in the form of a scholia, with a reference **where** the authors have proved it or will prove it, since they don't prove it **here**. . .

p. 292 But no matter how hard I look, there's no trace of a "theorem 4.1.9" - there's only one passage that answers the number 4.1.9- So I start reading the "remark" at random (without conviction☐- there must be a mistake of numbering. ... ), I read that "the analogue of 4.1.1 in complex cohomology is true... . "Unfortunately, I'll have to go back to 4.1.1 to find out what it's all about. I skipped over it and skimmed through the text that followed - and lo and behold, I couldn't believe it, eleven lines later, a sentence that starts with "We know that... . ." and ends with "induces an equivalence of the category . . with that of perverse beams".

Phew - so that was it after all! But no matter how hard I looked, I couldn't find the slightest hint to clarify that cryptic "We know that... . ". Readers who didn't already "know" it must be feeling pretty silly, not to the

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it's been a long time since Deligne should have been the one to discover and master staggered cohomology; and at a certain level (that which commands behavior and attitudes), they're convinced that it's really him, next to whom I'd be a sort of clumsy, clumsy auxiliary who would be more detrimental than anything else to the harmonious unfolding of a theory (leading to Deligne's theorem-ex-Weil's conjectures) and to a distribution of roles satisfactory to all concerned. . .

<sup>14</sup>(\*)Emphasis added.

the situation. What's clear to him in any case (apart from the fact that he's not up to it), is that this result "which would have found its place in his notes", which is "recalled" here in the course of a technical remark - something the reader should know anyway - is obviously due to the authors of the "notes" in question, or to one of them; the most prestigious perhaps and who wrote the article (there's an unmistakable "house style" . ), or the one who gave the oral presentation, whose well-known modesty prevents him from saying "it's me! - but everyone understood without having to say it...

It immediately brings back memories of my reflections over the last few weeks. The very first is Deligne's first work in 1968, which I finally (sixteen years later) took the trouble to look at a little more closely in the note "L'éviction" (n° 63) of April 22 (three days after the discovery of the pot-aux-roses LN 900). Here I find the same style, with variations no doubt due to the intervening thirteen years of "breaking-in". In the 1968 article, whose main inspiration came from me, he names me in passing and in a sibylline way towards the end of the article, just to be "in order". Here, he no longer takes such care - experience has long since shown him that there's absolutely no point! On the other hand, in the article of his young age, since he felt obliged to name me, he compensated by entirely retracting the initial motivation for his work (and the yoga of the weights with it, only to release it under an alternate paternity six years later, while awaiting the exhumation of the motives eight years later still... .). In any case, even hiding (and keeping for his own benefit. ... ) the article's essential arithmetical motivation, it "stood up," this article was perfectly understandable, living up to the author's reputation for doing things ☐ perfectly .

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Here, the theory he develops would be incomprehensible without the heuristic motivation. So he points to the latter, referring to it as "the essential statement", while treating it from under the leg - without honouring it with a name, or a formal statement baptized theorem or proposition, there isn't even a "correspondence" (known as Riemann-Hilbert) - he left that to his friends Verdier and Teissier. He doesn't have to give it a name (given the few<sup>15</sup> (\*) - surely he'd demonstrate it in five minutes!) or name anyone - others will take care of that for him and to his complete satisfaction. There is clearly a yoga, a philosophy, that the author handles with perfect mastery and authority, without having to name anything - this "little" that he pretends to disdain ("which would have found its place in these notes"), he knows full well he'll get more of, as long as he knows how to keep quiet and wait. The first time he played this game successfully, the "few" were "weight considerations" alluded to in a sibylline remark (waiting to bring out the philosophy of weights with great fanfare, six years later). The second time, as far as I know, was when I left in 1970 - the "little" was the "dream of motives", which for twelve years didn't deserve to be honored with a word (just think - a dream, and a dead man's dream at that, not to mention unpublished!), while we wait to discover the real motifs this time (and what we can do with them) and to claim, as modestly as ever, undisputed authorship<sup>16</sup> (\*\*\*).

#### 15.1.4. Perversity

**Note 76** (May 4) I well remember the first time I heard the name "faisceaux pervers", must be two or three years ago, that it struck me unpleasantly, arousing in me a feeling of unease. This feeling reappeared the two or three times I heard this unusual name again. There was a sort of inner "recoil", which remained close to the surface of my consciousness, and which would undoubtedly have been expressed (if I had stopped to examine it) on the other occasions.

<sup>15</sup>(\*) (June 14) To put this "little" in context, I'd like to remind you that Deligne devoted a seminar at the IHES to trying to develop a translation of constructible discrete coefficients in terms of continuous coefficients, without arriving at a satisfactory result.

On this subject, see the note "L'inconnu de service et le théorème du bon Dieu", n° 48'.

<sup>16</sup>(\*\*)For further comments on this technique of "appropriation through contempt", see the following day's note, n° 59'.

then) by something like: what an idea to give such a name to a mathematical thing! Or even

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□ any other thing or living being, except in a pinch a person - for it is obvious that of all "things" of the universe, we humans are the only ones to whom this term can sometimes be applied. ...

It seems to me (although I'm not entirely sure) that it was none other than Deligne himself who first spoke to me about so-called "perverse" beams, when he dropped by my place after the Colloque de Luminy<sup>17</sup> (\*). It must even have been one of the last mathematical conversations between us - there were no others after his visit. It was during this very visit that this "sign" appeared, which led me a few weeks or months later (while this sign was being comforted in the exchange of mathematical letters that followed this encounter) to put an end to a communication on the mathematical level<sup>18</sup> (\*\*). (For this episode, see the note "Two turning points", n° 66. )

Coming back to the so-called (wrongly i) "perverse" beams, it's obvious that "normally", these beams should have been called "Mebkhout beams", which would only have been fair. (On more than one occasion, I've named mathematical notions I've worked out and studied after predecessors or colleagues who were much less closely associated with them than Mebkhout was with this beautiful notion - which, incidentally, would seem to me to be more "sublime" than perverse!) The circumstances in which Deligne found himself at the time he was discovering and naming this notion derived from Mebkhout's work, preparing to rob him when he himself was already "fulfilled beyond measure" - these circumstances can rightly be called "perverse". Surely my friend himself must have felt it in his innermost being, at a certain level where one is not fooled by the facades one likes to flaunt. I sense in the attribution of this name (which seems aberrant at first sight) an act of **bravado**, a kind of drunkenness in a power so total, that it can even allow itself to display (symbolically, by the display of a provocative name whose true meaning no **one** will allow themselves to read!) its true nature of "perverse" spoliation of others.

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□ It seems by no means impossible that at some deep level, I perceived the tone of these dispositions in my friend, and that this contributed to the unease I mentioned<sup>19</sup> (\*). This uneasiness was expressed in particular by my inattention to the explanations he had to give me, although I don't think there had been an occasion before this meeting when I hadn't followed what he was telling me with sustained attention, and especially when it concerned mathematics. There was a kind of blockage in me with regard to this notion called (God knows why) "perverse" - I didn't really want to hear about it, even though it was very closely linked to issues I was (and still am to some extent) very close to.

In fact, the whole article by Deligne et al. was typical "**grothendieckery**" and all

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<sup>17</sup>(\*) If this is indeed the case (as I'm now convinced it is) I must give credit to my friend's modesty, for I had no idea (on a conscious level at least) that it was none other than he who had introduced and named them. I had to read the "memorable article" to realize this.

(May 28) To tell the truth, the article doesn't say this any more than it says that Deligne is the father of the Riemann-Hilbert correspondence. However, I had no doubts about his authorship of the term "faisceaux pervers", which was subsequently confirmed to me.

<sup>18</sup>(\*\*) On a purely personal level, this relationship continued in the same tone of affectionate friendship as before, with no apparent change. My friend used to come every other year or so to visit me, usually on some kind of hike. I did have a visit again last summer, which was a welcome opportunity to get to know his wife Lena and their infant daughter Natacha. I think it was on the way back from yet another Colloque de Luminy, about which I've heard very little (apart from a few vague, morose allusions from Mebkhout, who had been given the honor of being invited again, and who could think of nothing better to do than to get back into the game... .). They stayed at my place for two or three days, and the contact was excellent all round.

<sup>19</sup>(\*) I would even be inclined to think that this is indeed the case. On more than one occasion, I've been able to see for myself the extent to which the deepest perception of things is of a finesse and acuity that have no comparison with what skims the surface at the conscious level. The fully "awakened" man is undoubtedly the one in whom these perceptions are constantly integrated into conscious vision and conscious experience - the one who lives fully according to his true means, and not just on a paltry portion of those means.

that could just as easily have come from my pen (with the sole exception of the name of the main concept)! It's something I've already expressed in the second part of the previous note (n° (75)), and something I've also sensed from the moment I read the article quoted - but without this diffuse feeling yet being embodied in the striking observation I've just made. It makes me aware once again, in a striking way, of this profound contradiction of the person who cannot help (in a certain

sense) to reproduce and assimilate the very one it is a question of denying, of handing over to disdain - the one it is a question of burying, and who is also at the same time  $\square$  the one one **wants to be** and that (in a certain sense) one **is**.

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The day before yesterday, as I was writing the previous note ("l'Iniquité - ou le sens d'un retour"), I had already been struck by the coincidence that this turning point in the relationship between my friend and me, suddenly impoverished of a communion in a common passion, which had been its raison d'être and most powerful mainspring, took place on my friend's return from that memorable Colloque, the meaning of which had just revealed itself to me. What had puzzled me at our meeting in July '81, which on one level was as friendly and affectionate as on the other occasions we met, was this "sign", discreet in tone and air, yet brutally obvious, of a deliberate gesture of disdain. It was like a sort of **down payment** that my friend was making, this time at the level of a personal relationship, on the implicit and equally "discreet" (and just as "brutally obvious") disdain that he had just publicly expressed towards me, as a public figure, at the Colloque de Luminy, in the context of a brilliant display of technical virtuosity between the stars of the day. It was the same "disdain" that had just been expressed (but this time with an altogether different "perverse" brutality) towards the man who had dared (even a little) to claim to be me, and who had thereby condemned himself to be, for my friend Pierre (at a certain level at least), nothing more than "another Grothendieck"<sup>20</sup> (\*) who had to be crushed at all costs. . .

### 15.1.5. Pouce!

**Note** 77 (May 5) I was struck by another detail while reading this memorable paper<sup>21</sup> (\*\*), which dominated (so they say) the no less memorable Colloque de Luminy in June 1981. The last chapter, under the suggestive title "From F to C", describes at length a remarkable principle I had introduced into geometry twenty years ago - it must have been even before the birth of the notion of motif (which in gives the <sup>most  $\square$  deep</sup> illustrations, via Weil's ex-conjectures). This principle ensures that for some p . 297

In the case of statements concerning schemes of finite type over a body, it suffices to prove them over a finite base body (i.e., in a situation "of an arithmetical nature") to deduce their validity over any body, and in particular over the body of complexes - in which case sometimes the algebraic-geometric result envisaged can be reformulated by transcendental means (e.g., in terms of integer or rational cohomology, or in terms of Hodge structures etc.)<sup>22</sup> (\*). My friend learned this from none other than me and from me, on numerous examples over the years<sup>23</sup> (\*\*). The authorship of this principle (which in an elementary form is even spelled out in EGA IV - don't ask me which paragraph and which number... .) is well known<sup>24</sup> (\*\*\*). So much so that

<sup>20</sup>(\*) In our personal relationship, my friend calls me by the affectionate diminutive (of Russian origin) of my first name, Alexander, which is also what my family and closest friends have called me since childhood.

<sup>21</sup>(\*\*) See note n° 75 about the "memorable article".

<sup>22</sup>(\*) (May 6) It seems to me that the first example of the use of such a principle is to be found in Lazard's theorem on the nilpotence of algebraic group laws on the affine space  $\underline{E}$  (over any body). I was struck by its demonstration, and drew inspiration from it for a number of other statements, as well as for a "philosophy" that has dominated my thinking on pattern theory.

<sup>23</sup>(\*\*) See the note "Eviction" (n° 63) for one such example.

<sup>24</sup>(\*\*\*) (June 5) It is perhaps abusive for me to claim to be the "father" of a principle whose first known application is due to Lazard (see previous note (\*)). My role, as on other occasions, has been to sense the generality of someone else's idea,

When my brilliant friend was awarded the Fields Medal at the Helsinki Congress in 1978, N. Katz couldn't resist mentioning it in passing in his speech in honor of P. Deligne, thus rectifying a somewhat embarrassing systematic "oversight" on the part of his illustrious laureate. I read this speech just a few days ago, along with the "memorable article" itself.

p. 298 In any case, in this article, the philosophy behind the transition from "arithmetic" to "geometry" is presented in such terms that there can be no doubt in the mind of an uninformed reader that the brilliant lead author (excuse me, I impair. . . ) has only just discovered this wonderful principle of such far-reaching significance.

It's true that I haven't patented the method, and nowhere does my brilliant friend say that he's the brilliant inventor; nor does he claim in plain English that he's the father of that famous "correspondence" (admire the term, which smacks of his nineteenth century!) modestly attributed to Riemann and Hilbert (men worthy of sponsoring the children of such a prestigious successor) - nor does he specify in the "memorable volume" (LH 900) that it was indeed he who invented motives, motivic Galois groups and the whole philosophy that goes with them (and of which he has still only released a fragment). There's nothing to say either for this famous SGA 4<sup>1</sup>, where I've even been honored to be listed as a "contributor" to this volume, which so brilliantly develops ab ovo étale cohomology, deigning to call on (despite their regrettable gangue of superfluous details etc.) the two satellite volumes SGA 4 and SGA 3.) to the two satellite volumes SGA 4 and SGA 5, which have been consigned to oblivion, but to which I am generously credited with providing a few technical additions and digressions (some of them even "very interesting")<sup>25</sup> (\*).

In all these cases, and in many other micro-cases that I've witnessed over the last five or six years, without the idea ever occurring to me to **pinpoint my discomfort** and give a name to what I witnessed or co-acted<sup>26</sup> (\*\*) - in all these cases, I recognize the same **style**. My friend is always and totally "**thumbed**" - he can help himself at ease, with the complete good conscience that comes from admiring his peers and his blunders (with all due respect), guaranteeing total impunity.

### 15.1.6. The Chinese emperor's robe

p. 299 **Note 77** (May 7) Of course, those who see what my friend Deligne is doing and are in the know at all for the ins and outs, I mean those who haven't just learned about the maths "being done" from the publications of the person concerned himself, or other brilliant (though not always golden) stars of his generation - these colleagues (and they're not that rare after all!) are well aware, at **some level**, of what's going on. They must have sensed, in the "big" cases, that particular uneasiness that I myself have felt on more than one occasion in the face of these "micro-cases" a hundred times less serious than the "big" ones.

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and systematize it to the point of making it a "reflex" or "second nature". In the context of the yoga of weights and patterns, it's likely that the first to use this principle was Serre (not me), with his idea of virtual Betti numbers, which set me on the path to just such a general yoga of weights and patterns. (See note no. 46<sub>9</sub> for Serre's idea in question.) It's also true that it's common practice to attribute the authorship of a "principle" of reasoning that has become commonplace, not to the author where we first find a trace of it, but to the person who first perceived its general scope, systematized and popularized it. In this sense, N. Katz's correction (referred to in the following sentence), attributing the paternity of this principle to me, is justified.

<sup>25</sup>(\*) For details of the "SGA AT operation", see the four notes "La table rase", "L'être part see the four notes "La table rase", "L'être à part", "Le Feu vert", "Le renvers- ment" (notes n°s 67, 67', 68, 68').

<sup>26</sup>(\*\*) The first step towards precisely "pinpointing my malaise" in a specific case was taken in Harvest and Sowing less than three months ago, in the reflection (which turned out to be quite laborious - and for good reason) "The note, or the new ethic" (section 33). This reflection was taken up again in a note to that reflection, "Le snobisme des jeunes, ou les défenseurs de la pureté" (note n° 27), then again less than two weeks ago (under the impact of the discovery (the day before) of the "memorable volume" (LN 900)) with note n° 59: "La nouvelle éthique (2) - ou la foire d'empoigne". As I was writing this, there remained in me a tinge of hesitation about using the rather blunt term "jumble sale". The discoveries I've made since then have shown me that there's no need for hesitation.

bigger than these. But what they sensed was so **enormous**, so **incredible** that it must never have surfaced - as it finally began to surface with me, in the course of a **work**, which expressed itself in these two texts around a micro-case referred to in the previous b. de p. note. Indeed, I've never heard of anything like it in the history of our science or any other. Instead of "surfacing", for some people "it" must have **become the norm**, or at least been considered **normal** - as long as an obviously brilliant man, admired by all, practised it with the greatest naturalness in the world, in full view of everyone and without the thing ever (as far as I know) eliciting the slightest comment.

Over the past few days, I've been reminded many times of the tale "The Dress of the Emperor of China", in which the aforementioned emperor, deceived by unscrupulous swindlers and his own vanity, announces that he will appear in a solemn procession wearing the most sumptuous garments the world has ever seen, prepared for him at great expense by so-called tailor artists. And when he appears in the procession, surrounded by the pomp and circumstance of his Court in full regalia, the "artists" bowing and scraping, and the entire imperial family, no one in the procession or among the people gathered to contemplate the seventh wonder dares to believe the testimony of his eyes, and everyone makes a point of admiring and raving about the unsurpassable splendor of the garments with which he is now adorned. Until a small child who had strayed into the crowd exclaimed, "But the emperor is naked!" - and then all of a sudden the whole crowd, as if with one voice, cried out with the little child: "But the emperor is naked!"

And I feel like the little child who believes the testimony of his eyes, even though what he sees is quite unheard of, never seen before and ignored and denied by all.

□ Whether the child's voice will be enough to bring some back to the humble testimony of their healthy faculties, that's another story. A tale is a tale, it tells us something about reality - but it's not reality<sup>27</sup> (\*).

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### 15.1.7. Encounters from beyond the grave

**Note 78** (May 6) It's only been five days since I received this generous package of documents from my friend Zoghman Mebkhout, including above all the two texts already examined from the "memorable Colloquium" - that Colloquium built around a monumental **mystification**! The note "l'Iniquité - ou le sens d'un retour", in which I try to assimilate the quite incredible meaning of this new "event", was written on the very day (the day after May 1st) that I received these documents, still in the emotion of discovery<sup>28</sup> (\*\*).

Since April 19, when I finally became aware of the "memorable volume" of the Notes readings (LN 900 - see notes (51) (52)), this has been the third great discovery on the subject of the solemnities of the Great Burial, and the one that seems to me to be of the greatest significance, both in terms of the light it sheds on the actions of the "Great Burial".

<sup>27</sup>(\*) (June 14) After writing this note, the name "The robe of the Chinese Emperor" struck me as a natural sub-title for the Burial, expressing a particularly striking aspect of it. Later, as the focus shifted to my students as a whole, and even to "the entire congregation" of the Mathematical Establishment, this subtitle seemed less appropriate. However, I've come to realize that the parable that first came to mind when thinking of my friend Deligne, applies equally to all aspects and adventures of the Burial, which at every step reach the Ubuesque in the unbelievable (which everyone makes a point of modestly ignoring) that is nonetheless true. For reflections along these lines, see in particular the notes "On n'arrête pas le progrès!", "Le Colloque", "La Victime - ou les deux silences", "La plaisanterie - ou les complexes poids", "La mystification", "Le Fossoyeur - ou la Congrégation toute entière" (n°s 50, 75', 83, 85', 97), none of which specifically concern my friend Pierre.

<sup>28</sup>(\*\*) Along with the section "The note - or the new ethic (1)", this note is the only note or section I've had to rewrite several times, because what "came out" in the first version (and even in the next one) was still burdened by the inertia of my usual vision of things, which fell far short of the reality I was examining.



of people to whom I have been closely linked, than by its implications as a "tableau de mœurs" of an era, apparently unique (but it is true that I am ignorant of history. . . ).

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The second discovery had closely followed the first - that of the exhumation of the "motifs", for twelve years buried. After the "memorable volume", I was treated to the "memorable seminar" - that "seminar" that never took place, given a bogus name (both SGA and number 4 1/2), and enriched with the "State 0" of a phantom thesis, not to mention a central presentation from the (real) SGA 5 seminar (which appears later, even though it predates it by twelve years); a presentation "borrowed" for the purposes of the operation without further ado. This brilliant operation, and the role it played in the strange vicissitudes that befell this poor SGA 5 seminar (dismantled from the head, the tail and the middle!) were gradually revealed in the course of a reflection that continued between April 24 and 30. (See the five notes "Le compère", "La table rase", "L' Etre à part", "Le signal", "Le renversement", n°s 63", 67, 67', 68, 68'.)

As soon as I had digested this discovery, and as my retrospective reflection on "Mon ami Pierre" drew to a close, and on April 30 I had proudly put the final and definitive mark (that was a sure thing!) on my life, I decided to take the plunge.

- this time I was finally there i) under this interminable Burial, with the "final note" with the doubly euphoric name "Epilogue - ou l'Accord Unanime" - that I receive this package of misfortune, which calls into question final point, epilogue, page layouts and numbering... . A quick glance at the documentation and the accompanying annotations and letters made it clear that my period was gone, as were the beautiful arrangements for a first-class Funeral, the final details of which I was about to polish - I was ready to take up the master of ceremonies' harness. . .

God knows my friend Zoghman had plenty of time to inform me of the situation! It must have been going on for ten years in latent form, and three years at least in "acute form" (and that's putting it mildly) - ever since the Colloquium in question, where he must have sensed the wind without having to wait for the publication the following year of the highly official "Proceedings" under the patronage of his illustrious ex-patron and protector.

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A few months after he had defended his thesis (in February 1979), he had come to bring me a copy to the village where I had lived for six years. Unluckily, I had just left (never to return).  
return, except in passing... ) a few days before, to retire in solitude. He only met my daughter, who later handed me the thesis. It was the following year, I think that we finally got to know each other, in college. in Montpellier, where we chatted for an hour or two. I wasn't really into math at the time, and couldn't remember much about the thesis I'd flipped through in a few minutes, or the name of its author. That didn't stop the contact from being warm. I remember an immediate current of mutual sympathy. We didn't talk so much about maths (not that I can remember), but mostly about more or less personal things. Zoghman told me afterwards (something I'd forgotten) that he'd been able to explain the D-Modules "philosophy" to me a little, and that he'd been pleased with the meeting, to have felt me "vibrate" if at all by learning new things from him, and yet also (in a way) "expected". What I remember most of all was the impression he made on me - an impression of stubborn, calm strength, that of a "go-getter". At the time, much more than when we met last year or during the correspondence that followed, I had the impression of a strong affinity of temperaments - this "go-getter" side in particular. But the two or three years that have elapsed between the two encounters seem to have dented it quite a bit. ...

I don't remember Zoghman telling me at our first, brief meeting about the isolation in which he had worked, the lack of any encouragement from the "luminaries" who had been my students. If he hinted at it, he must not have insisted. Even then, the whole thing didn't appeal to me.

surprise<sup>29</sup> (\*). I couldn't say whether this was before or after the Colloque de Luminy in June 1981<sup>30</sup> (\*\*). If it was afterwards, he would still have had some hot stuff on his stomach - and he really didn't give the impression of it. Rather that of a man who knows what he wants to do and what he wants, and who follows his instincts.

quietly, without seeking trouble and without being sought out.

□ We didn't continue then to write to each other. But I remembered him well, and early last year I wrote to him I wrote him a note, at random, to ask if he might be in a position to tackle a magnificent work on the foundations of a "moderate topology" which (it seemed to me) was just waiting for someone of his calibre to take it up. Although Zoghman didn't make it clear to me at first, it turned out that he wasn't really interested in this prospect - on the other hand, he seemed happy to seize the opportunity of a new encounter. At the time, I was too out of the loop to fully appreciate the situation, and imagined that D-Module theory was now a done deal, as is, say, coherent duality theory (78<sub>1</sub>), and that Mebkhout had perhaps run out of "big tasks". It was only when we met last summer that I realized that in the very theory he had started, there was no shortage of "big tasks" - and some of them had not even been started, because they had not even been seen!

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In any case, it was a perfect opportunity for a second meeting, and this time not as casual as the first. Zoghman must have stayed at my place for maybe a week last summer, in June I think. Mathematically speaking, our meeting served mainly to bring me up to speed as best we could on D-Module yoga. I've been slow to "thaw out", having lost touch with my old cohomological loves, and being mostly embroiled in the writing of "Poursuite des Champs", which is set in rather different registers. Zoghman wasn't discouraged to see me listening with a slightly distracted ear, and returned to the charge without tiring, with a touching patience. I was finally triggered, I think, when I realized that these famous D-Modules were nothing other than what I had long ago called **module crystals**, and that as such they still made sense in singular spaces. All of a sudden, I saw a whole network of intuitions from my crystalline-differential past rising up from forgotten depths, and slightly rusty reflexes from my "six operations" past being reactivated...

Perhaps it was Zoghman who was a bit of a loose cannon, or maybe it was more that he decided afterwards that he wasn't going to risk his fingers in that particular gear (any more than my friend Pierre wanted to put his - although he'd been all fire and brimstone while I'd been around... ). (⇒ 78)

**Note 78** □ There are, however, a number of "fine" results of consistent duality, notably on the structure of "dualizing differential modules", their relation to "naive" differential modules, and trace and residue applications in the non-smooth flat case, which I had developed in the late fifties which, to my knowledge, have never been published. Nevertheless, for the most part, the theory of coherent duality (in the schematic framework at least), just like that of stellar duality (and its variant for the discrete cohomology of locally compact spaces, developed by Verdier on the stellar model), or linear algebra or general topology, appear as theories that have essentially **been completed**<sup>31</sup> (\*), in the nature of **tools that are perfectly perfected and ready for use, and not so much of a substance**.

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<sup>29</sup>(\*) (May 30) That's not quite true - I'm reprojecting more recent disillusioned dispositions onto the past. When I met Zoghman just last summer, I remember being surprised that none of my cohomology students (Deligne, Verdier, Berthelot, Illusie in particular) had supported Zoghman in his work. This surprise was repeated when Deligne came to see me ten days later (I must have said something to him about Zoghman, but I got no response), and later in a telephone conversation with Illusie (see the note "La mystification", n° 85').

<sup>30</sup>(\*\*) (June 3) That was back in February 1980, a year after he defended his thesis.

<sup>31</sup>(\*) (June 12) This is not quite true for stable duality, until the purity conjectures and the "biduality theorem" are proved in all generality.

that would have to be penetrated and assimilated.

### 15.1.8. The victim - or the two silences

**Note** 78 We met in an atmosphere of friendly trust and affection. This atmosphere, however, did not live up to its promise. I realize now that from that moment on, my friend's trust was far from complete. It was two years after the famous Colloque, and a year after the publication of the "Actes" in *Astérisque*<sup>32</sup> (\*\*)-at a time when he was the victim of a scandalous spoliation.

p. 305 But he didn't bother to inform me until just four days ago! When he came last year, he was returning from another Colloque de Luminy<sup>33</sup> ('\*\*\*') (this time squarely on the theme of D-Modules)'□où<sup>ou</sup> on which he had again generously invited and rushed to attend. He spoke of it in terms both bitter and vague, suggesting that now that he'd pulled the chestnuts out of the fire, it was "the others who had done it all". I could imagine the picture indeed - especially Verdier suddenly remembering the paternity of the triangulated categories (and derived ones too, while we're at it!) he had left to one side for ten or fifteen years, barely tolerating his "pupil" Mebkhout's use of them in his work... (81).

Although he didn't want to explain himself clearly at the time, Zoghman seemed to have his heart set on Verdier, which was understandable given his ex-boss's less-than-encouraging behavior. And yet, my other cohomology students - Deligne, Berthelot, Illusie - hadn't bothered to take an interest in what he was doing or to support him in any way. But it almost seemed as if Zoghman took this for granted, having never (or so it seemed) experienced anything other than this attitude among his elders. If he held a grudge against any of my former students, it was solely and exclusively against Verdier.

From Zoghman's hints (which he obviously didn't want to spell out), I understood that "they" were systematically putting the scope of what he'd done - period. This is, after all, the most common thing in the world. Since judging the importance of a thing is largely subjective, it's commonplace and almost universal to attribute more merit and importance to one's own work, to that of one's buddies and allies, than to that of others, and especially to those one feels like minimizing for one reason or another. (And the "reason" in this case wasn't exactly a mystery to me!) Nothing could have made me suspect that, far beyond such common attitudes, there was here an operation of pure and simple swindling, where there was no question whatsoever of "minimizing", but rather of **swindling** Mebkhout's authorship of the ideas and results that were breathing life back into where there had been stagnation... .

p. 306 And yet, if there was one person in the world to whom it was natural for my friend to open up, it was me. whose work had inspired him during those years of obstinate work, sometimes bitterly, against the fashion of the day - I who received him affectionately in my home, making myself a bit□his pupil at my learning as best I could what he took pleasure in teaching me<sup>34</sup> (\*).

<sup>32</sup>(\*\*) (October 9) Zoghman tells me that these "Actes" were not published until early 1984.

<sup>33</sup>(\*\*\*) (May 7) There's a slight memory lapse here - I think he was getting ready to go to the Colloquium. At the time, of course, there was no shortage of reasons for those "bitter terms" (and vague ones) I remembered. But this bitterness was further heightened by his visit to Luminy after his stay with me. I had echoes of it in a phone call he gave me on his return from Luminy. From that moment on, I had the distinct feeling that he had come to Luminy for the pleasure of being mistreated by "the people" (without really asking me which ones) who had generously invited him, for the pleasure of being able to treat him as a negligible quantity. I must have told him so, or let it be understood, which couldn't have improved my friend's attitude towards me.

<sup>34</sup>(\*) Zoghman didn't tell me about mine, and he didn't tell me about his own funeral either, even though he'd had a front-row seat to the proceedings for nearly ten years! To tell the truth, his "protectors" (a little reluctant on the edges) had even agreed to let him carry with his hands a small corner of the coffin carrying my remains - but they couldn't forgive him for being the only one among the guests who sometimes took the liberty of uttering the name that all the others kept quiet!

So my friend must have felt at odds in his relationship with me, and he couldn't find it in himself to take on the responsibility.

After my friend had passed through an atmosphere of warm affection, there was an immediate "backlash". I had the impression that he had decided to transfer onto me the mistrust and bitterness that had built up in him over the past eight or ten years, under the sting of the indifference and disdain he had encountered in some of my former students. In the months that followed, the correspondence between us never left the *aigredoux* register - it finally stopped with a New Year's greetings card, which never received a reply.

It was only at the end of March that I contacted Zoghman again, to send him "Le poids d'un passé" and the notes I had then added to this section (n° s 45, 46, 47, 50). It was to ask him if he would agree to my including him, as I had done, in the short reflection on my work (in the note "My orphans", n° 46), when it would be clear to all that I was using information he had given me, and which he might consider confidential. I was by no means sure that my friend would not prefer (like others before him) to "crush rather than displease". It would have hurt my feelings if he had.

It took me a long time to get her reply, which I received only ten days later. I was somewhat expecting her to would still be half-flesh, half-fish - but □ this time she was downright warm. He'd give me his p . 307 I agreed wholeheartedly, even emotionally, with the terms in which I spoke of him.

It's on page 6 of his long (eight-page) letter that he points out, as if in passing and with reference to the "impressive number" of applications of his theorem ("both in the framework of stale topology and in the transcendental framework") that it still appears in the literature under the name of the "Riemann-Hilbert correspondence"<sup>35</sup> (\*). He says it in such an almost incidental way, and with such a delightfully illegible handwriting, that it almost went completely unnoticed! But then I remembered, it really was a strange thing. So strange, in fact, that it hardly seemed believable, and then perhaps my friend was exaggerating, obviously he was angry with everyone, including me, even though I only wanted good things for him. So I added a note (holy Zoghman, I thought I'd finished!) called "L'inconnu de service et le théorème du bon Dieu", in addition to two others "L'instinct et la mode - ou la loi du plus fort" (I'd also thought a lot about him, among others, when writing it) and "Poids en conserve et douze ans de secret". This note on "L'inconnu de service", I wrote at first without total conviction; Zoghman seemed to me so knotted up and full of contradictions that I wondered what I was getting myself into by simply echoing him, without knowing the facts for myself. The thought hadn't occurred to me that there might be a scam, let alone that Verdier or Deligne themselves were involved. There was nothing in what Zoghman had told me to suggest this... .

Yet both of them were so closely linked to this theorem of the good Lord, that its authorship could hardly be concealed without at least their tacit agreement. It must have worked on me in the days that followed. I remembered that Deligne had given it a lot of thought, this problem solved (ten years later) by Zoghman - and then Verdier, after all, acted as research director; even if he didn't go out of his way for his pupil and would rather have beaten him cold and discouraged him than anything else, he must at least have known what the two main theorems in this work were - Zoghman surely explained them to him, during those famous "interviews" that Verdier □ was kind enough to grant him! I have therefore added to the note on p . 308 a commentary on the relationship of Mebkhout's work to an earlier attempt by Deligne, and a b. de p. note on the role of Verdier. At the same time, it was also a sounding board for my friend

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a past fraught (as mine was) with ambiguity, and speak to me plainly and clearly. Talking about his funeral also meant talking about mine and the role he himself had played in it ... . In any case, if I ended up discovering this famous funeral in all its splendor, it was against a kind of "conspiracy of silence" that encompassed both my friend Zoghman and my friend Pierre - and no doubt most of the friends I had in the "great mathematical world".

(June 3) For further details, see note n° !78" below.

<sup>35</sup>(\*) See the quotation from his letter in the note "A feeling of injustice and powerlessness", n° 44.

Zoghman...

You'd think that Zoghman would jump at the chance to finally, finally reveal his batteries, hidden for three years, which will finally bring out the clear truth and triumph the cause of the oppressed! But not at all! Fifteen days of silence, followed by a letter about everything (in maths) except God's theorem - or rather, he confined himself to giving me the precise reference in his thesis, which I had asked for. (I still wanted to know where this famous theorem, to which I was so firmly committed, had been proved!)

In my reply to this letter, I had to say a few words to him about the "vast swindle with regard to my work" I had just discovered (with the "memorable volume" LN 900, and moreover "promising me much pleasure" in the days to come in making the acquaintance of SGA 4<sup>1</sup> in the college library) - so<sub>2</sub> that after another ten days' silence, my friend finally got in touch!

This time, at last, he "pulled out all the stops" - a **great deal**, in fact, of judiciously-chosen documents, enabling me (who hardly ever haunts libraries, or even the piles of separate prints piling up in my office at university... ) to give me a well-balanced idea of an "atmosphere", in which many of those who didn't take part in my long and solemn funeral still remain<sup>36</sup> (\*). Alongside the main "piece of evidence" (the two articles from the famous Colloquium, exposing the incredible mystification), and another "memorable article" (this time from the pen of Verdier<sup>37</sup> (\*\*)). there was the speech by

p. 309 N. Katz on the "Fields Laureate" Deligne, plus a presentation by Langlands and another by Manin at the same Helsinki Congress 1978; then Deligne's "Théorie de Hodge I" at the Nice Congress 1970 (where he is made another allusion in line 3 to a "conjectural theory of Grothendieck's motives" (78<sub>1</sub> ), and "Weight in the Cohomology of Algebraic Varieties" by the same Deligne, Vancouver Congress 1974 (where my name is not mentioned (78<sub>2</sub> )); plus finally a correspondence with A. Borel (yet another old friend, whom I learn at the same time is back in Zurich. . . ), and two notes to Mebkhout's CRAS, one of which from 1980 is a summary of Chap. V of his thesis (passed the previous year), giving a little more emphasis to the theorem of the good Lord<sup>38</sup> (\*). Not to mention another document - shhh! communicated under the seal of secrecy, and of which I won't say another word here. ...

Two letters accompany this substantial dispatch (letters of April 27 and 29), one very long and both substantial. Now that he's finally let the cat out of the bag (the real one, this time!), Zoghman continues to urge me to exercise extreme caution, as he had been doing ever since I contacted him again. If I listened to him, I'd be careful not to make public my reflective notes, which would remain a matter of absolute secrecy between him and me - at least not the part that implicates anyone, given that "they" have "all the power" and "everyone is with them"<sup>39</sup> (\*\*)! And yet, I had warned Zoghman that these notes, from which I sent him the extracts concerning him, were destined to be made public, and as soon as possible.

All the elements seem at last to be in place for the just cause of the oppressed to triumph, but the "victim" seems to be doing everything in his power to continue muddying the waters as if by magic.

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<sup>36</sup>(\*) (June 12) Katz, Manin, Langlands don't seem to be among them. . .

(March 1985) For a different take on Katz, see "Dotting the I's", n° 164 (II5), and "Maneuvers" (n° 169), "Episode 2".

(April 1985) Similarly for Langlands, see note "Pre-exhumation (2)", n° 175 . . . 1

<sup>37</sup>(\*\*) For more on this article, see "Les bonnes références", n° 82.

<sup>38</sup>(\*) For a precise reference for this note, Mebkhout's thesis and the theorem of the good Lord, see the note "Le pavé et le beau monde - ou vessies et lanternes", n° 80.

<sup>39</sup>(\*\*) (May 30) Carried away by my impetus, I'm exaggerating a little here. At no time has Zoghman suggested that I refrain from publishing any part of my notes. Lately, he's even been insisting that these notes should indeed appear in book form, for the benefit of "posterity", whereas a limited edition like a preprint seems to him a bit "like a sword in the water".

secret regret (one would say) at having sold that famous "fuse" of which Zoghman must have been (until the fateful May 2) the one and only holder. This ambiguity is apparent in every line (I'm hardly exaggerating), right down to the latest letters I've just received - including the very last one, in which he sends me, with an air of sombre triumph, the "memorable article" in its entirety (whereas, with the "big package" he first sent me, he had only managed to part with the first twenty pages of this masterpiece<sup>40</sup> (\*\*\*)).

□ As for the friend Pierre I mean Deligne (who is neither Pierre nor "friend" to everyone. . . ), it's p. 310  
it's just that he doesn't sing its emotional praises - it seems that it's no longer he, Zoghman, who's the "victim", but no, it's Deligne, poor fellow, who's been so badly influenced by those around him - the only villain, and the one who surrounded him so badly, is Verdier (and yet. . . follow my gaze instead, . . .): it's clear that I "must have done something" to Verdier for him to be such a coward for the sheer pleasure of doing harm, not to mention the fact that I was also his boss and I was also the one who awarded him the title of doctor and the glory and all the rest - the means, in short, of "absolute power"!<sup>41</sup> (\*)

Clearly, if my friend has a grudge against anyone, it's not really against his illustrious ex-boss, whom he's only had the honor of meeting for an "interview" three times in ten years in all (if I've understood correctly what he wrote to me most recently) - a vertiginously distant man, entirely out of reach - but it's the one he can come and see whenever he pleases, and share both his bread and his lodgings...<sup>42</sup> (\*\*).

Each time Zoghman takes a new step to divulge some new element, making me a little more aware of a situation of despoilment in which he is the victim (and can help a little to unravel it), I feel that it's like a **wrench**, the culmination of an exhausting inner struggle. I

has a **role** with which he seems to have identified body and soul, clinging to it as if it were his most precious possession - a role with which he seems to have identified body and soul, clinging to it as if it were his most precious possession.

this role of <sup>victim</sup>□ which he can only maintain by keeping around this role and the situation that justifies it, p. 311  
absolute secrecy<sup>43</sup> (\*). And he may indeed be torn, and resent me more than ever, at this moment when, with his reluctant collaboration (snatched away, as it were, by the logic of a situation created by none other than me, with those unfortunate reflections on an uneventful Funeral. . . ), this secret will come to an end, and with it, perhaps, this role in which it has pleased him to maintain himself, for how long I cannot say.

This "burial" of my friend Zoghman was achieved by the combined care of **two silences**, each responding to the other and provoking it in turn, in a seamless round in which the role of one closely matches the role of the other - the despoilers and the despoiled. If on more than one occasion I was struck by the fact that the "burier" was at the same time, and more profoundly, his own "buried", I was equally struck by the fact that in the person of another friend, the "buried" was at the same time, and more profoundly, his own "burier" - in close connivance with him.

<sup>40</sup>(\*\*\*) (October 9) Zoghman told me that, in fact, he didn't have a Xerox of the complete article in his possession at first, which he pulled out only later.

<sup>41</sup>(\*) It's not the first time I've heard this "absolute power" claptrap, with which one would like to convince oneself of one's own powerlessness and justify it. If anyone has invested anyone with "absolute power" over himself, Zoghman, it's none other than Zoghman himself!

<sup>42</sup>(\*\*) (May 8) It's surely no coincidence that the unequivocal signs of conflict in my friend's relationship with me appeared in the very aftermath of this stay, when he "shared my hand and my bed" in an atmosphere of unreserved affection, abolishing a feeling of "distance" that our first brief encounter probably couldn't entirely erase. Here I encounter a situation with which I have long been familiar, and which I discuss (in relatively general terms) in the two notes "Le Père ennemi (I), (2)" (sections n°s 29, 30). Little did I suspect, when I wrote them as a commentary on the reflections that had preceded them, the extent to which the archetypal situation I describe there would constantly find itself at the center of a long reflection yet to come, just as I thought I was nearing the end of this journey!

<sup>43</sup>(\*) (May 30) Since these lines were written (May 6), my friend's attitude has changed dramatically, and lately I've seen no signs of attachment to a victim role. It goes without saying that the lines which follow (like those which preceded) concern certain episodes in my friend's life, and in no way claim to define a temperament or describe a permanent bias.

with the very people whose willing victim he delights in being.

And I can see that the person primarily responsible for his own spoliation is none other than my friend Zoghman himself, who for three years has acquiesced by his silence to his humiliation by those who take their ease with him. He had everything in his hands to fight for - and for three years he chose to forget he even had hands, and to be defeated without having fought<sup>44</sup> (\*\*).

p. 312 **Note 78<sub>1</sub>** I had never held this short preliminary communication in my hands, but only the more circumstantial "Hodge Theory II, III" publications that appeared in Publications Mathématiques. This is why I had been under the impression that Deligne had not seen fit to ever allude to a role motive theory in the genesis of his ideas on Hodge theory. I thought that if he had wanted to mention any role I might have played with him<sup>45</sup> (\*), he would probably have done so with "Hodge Theory II", his thesis work, which was the perfect opportunity to mention such things<sup>46</sup> (\*\*). I've just seen that he's fulfilled the formality of mentioning me once and for all, with this lapidary line<sup>47</sup> (\*\*\*) alluding to "Grothendieck's conjectural theory of motives", with even a reference at the key (to Demazure's talk at the Bourbaki seminar).

Once again, nothing to say! The idea never occurred to him to specify that he had learned this theory (all conjectural, let's not forget!) from **another source** than this meagre text by Demazure, which can give no image of a theory of great richness (all conjectural!), which runs like a thread through all Deligne's subsequent work on weight yoga - pending the escalation of the "pirate volume" LN 900, where the motivic Galois groups are finally exhumed (fifteen years later) (this time without even a laconic reference line containing the name of the deceased. . . ).

p. 313 On reflection, in this laconic quote, I recognize the same "thumb!" style. - a quote from pure form, to be fair, with a reference that is in no way likely to enlighten the reader (in this case, on obvious and deep-rooted relationships with ideas that it is precisely to hide<sup>48</sup> (\*) - and which have remained hidden during the twelve years that followed), but **of a nature to deceive him**.

**Note 78<sub>2</sub>** I didn't have to hold this<sup>49</sup> (\*\*) text in my hands (which I learned about a few years ago). weeks) to know that my name wasn't on it. Nor was Serre's, who was the first to glimpse a "philosophy of weights", which I later worked out in great detail.

<sup>44</sup>(\*\*) (May 30; This is an admittedly subjective view of someone with the temperament of a fighter, someone in whom this fibre might have seemed absent. It would seem, however, that since these lines were written, my friend's fighting spirit has been reawakened, and he is determined to fight back against an iniquity of which he has been the victim.

(April 18, 1985) For a different, less "harsh" take on my friend's provisions, see also the "Roots" note (n° 171 ).<sub>3</sub>

<sup>45</sup>(\*) (May 30) Until a few weeks ago, I systematically downplayed this role. See note

"Being apart" n° 67' of May 27, where I first became aware of this attitude in myself and perceived its meaning.

<sup>46</sup>(\*\*) (May 30) Nor do I remember being asked to sit on the thesis jury. The funeral was already well under way...

<sup>47</sup>(\*\*\*) Serre is also implied in the same line by the cross-reference sign [3] - the curious reader will find his name in the bibliography at Hodge I. This expeditious reference line is the only one between 1968 and the present day where there is any allusion (however cryptic) to the "sources" it mentions in a single breath: Serre (alias [3]), motifs, Grothendieck. ...

(May 28) However, I have since come across another such allusion, very interesting in view of the very special occasion. On this subject, see the note "L'Eloge Funèbre (1) - ou les compliments" n° 104 , and the end of the note that precedes it ("Le Fossoyeur - ou la Congrégation toute entière" n° 97), situating this "particular occasion".

<sup>48</sup>(\*) As I write these lines, I am reminded of a revealing incident involving "weights" two years earlier, mentioned at the start of the note "Canned weights and twelve years of secrecy" (n° 49), and in more detail at the start of the note "The eviction" (n° 63). For the "pouce! style" in general, see the reflection in the note "Pouce!" (n° 76). It's a style with which I'm becoming quite familiar!

<sup>49</sup>(\*\*) "Weights in the Cohomology of Algebraic Varieties", by P. Deligne, Vancouver Congress 1974, Proceedings, pp. 78-85.

### 15.1.9. The Boss

**Note!** 78" (June 3) Zoghman explained to me that he only gradually became aware, and confused at first, of the "swindle" that was going on around my work. The manuscript Verdier had given him in 1975 (see "Les bonnes références" note n° 82) had been providential for him, notably in introducing him & to the notion of constructibility and its essential properties, as well as to the biduality theorem, from which he had drawn inspiration for the biduality theorem (or "local duality") in the context of D-Modules. It was only years later, while reading SGA 5 (a massacre edition, admittedly, but not so massacred as to give an attentive reader like him the slip) that he began to realize something. For a long time, he had been filled with admiration and gratitude for his distant elder, convinced that the ideas he drew on were his own. In fact, it would seem that for years he was convinced that the theory of duality known as "de Verdier" was indeed due to Verdier, or at least to "Serre-Verdier", and that the idea of duality he calls "de Poincaré-Verdier" was also due to Verdier. It was only around 1979 (the year he defended his thesis) that he began to realize that there was something wrong - but I presume he had to be careful not to let anything show about it to his prestigious "boss", nor to me, when we met in February 1980 and June 1983.

It was only with the Colloque Pervers, in June 1981'□then that he began to sense the escamotage that was p . 314 that he also began to realize more clearly the world he had strayed into<sup>50</sup> (\*)! Surely, for him, I had to be part of that world, where my former students (or at least some of them) had the upper hand and plundered the posthumous pupil with the same casualness as the departed master. The only difference, as it turned out, was that I was dead and they were alive and kicking. ...

I can imagine that even after the Colloque Pervers, Zoghman still found it hard to believe the testimony of his healthy faculties, which told him quite clearly what had happened. He only received the famous Introduction to the Colloquium Proceedings, signed by B. Teissier and his "patron-sic" Verdier, in January 1984. Having denied the evidence for nearly three years, the shock was all the greater, I understand. It was two months later that I contacted him again, sending him the notes "Mes orphelins" and "Refus d'un héritage - ou le prix d'une contradiction" at the end of March, and it was another month later that he finally decided to "let me in on the joke" and tell me about the "Mystification du Colloque Pervers".

### 15.1.10. My friends

**Note** 79 And here I am about to finish and make public this reflection which will put an end to the secrecy that Zoghman himself has maintained around the spoliation of which he is the victim, and from which he also reaps the obscure benefits<sup>51</sup> (\*\*). Perhaps it will be unwelcome to him, just as it may be unwelcome to my friend...

Pierre, to whom I'll hand-deliver it as soon as it's finished and the text cleaned up and printed<sup>52</sup> (\*\*\*).

The □best thing I have to offer my friend Zoghman as well as my friend Pierre, perhaps both the p . 315

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<sup>50</sup>(\*) Zoghman ended up having such a low opinion of his ex-boss, that he was convinced that everything Verdier had done in the sixties (which I review in a b. de p. note to note no.° 81 "Thesis on credit and comprehensive insurance") had been more or less dictated or at least blown by me.

<sup>51</sup>(\*\*) (May 30) I would remind you that this reflection was inspired by a disposition in my friend that now seems outdated. (Compare two b. de p. notes of May 30 with note n° 78'.)

<sup>52</sup>(\*\*\*) I didn't think I'd ever have the opportunity, in my remaining years, to return to the capital for a few days. But my friend Pierre has travelled often enough, over more than ten years, to meet me deep in the remote countryside, for me to travel on this exceptional occasion, following up on an often-repeated invitation that has never yet been put to good use.



Will they receive it as the worst: as a calamity, or as an outrage. All the worse, since my testimony is public - just as the silences of both have been rites of public acts, and which commit one as they commit the other.

Whether they reject or accept my testimony is their choice, and the same goes for Jean-Louis, whom I counted among my friends, just as Zoghman and Pierre do today. These choices affect me closely, and they are not mine. I have no temptation to predict what they will be. It won't be long before I know, and I await what the weeks and months ahead will bring with intense interest, suspense - and without a shadow of anxiety. My only concern and responsibility is that what I offer is the best I have to offer - that is, to be true.

Some may be surprised that I speak so bluntly of people I call "friends", and see in this name a stylistic clause, or even an intonation of irony that is absent. When I refer to Zoghman Mebkhout or Pierre Deligne as "friends", it's a reminder of the feelings of sympathy, affection and respect that are within me as I write. Respect tells me that I don't have to "spare" a friend, any more than I have to "spare" myself - like me, he is worthy of encountering the humble truth, and no more than me, he needs sparing.

If I don't refer to Jean-Louis Verdier as a "friend", it's in no way because I consider him less "good", or less "deserving", than my friends Zoghman and Pierre, or myself, but because life has distanced us from each other. The feelings of sympathy and affection that bound me to him fifteen or more years ago have more or less faded with time, and have not had the opportunity to be revived by any kind of personal contact. The few attempts I have made to re-establish such contact have met with no response, and I don't know whether reading these reflections will revive a relationship that had frozen. But even though he's no longer a "friend" to me, I don't think I'm showing him any disrespect by not treating him any more kindly than I treat myself or my friends, and I'm well aware that to do otherwise would be to do him or anyone else a disservice. Not to mention that both he and my friend Pierre, if any

that they insist on "defending" themselves (or attacking) rather than taking the risk of looking at themselves, are not lacking in means or support. And not to mention that where they have had the opportunity to □discourage

or to crush, more than once both have done so, ruthlessly and mercilessly.

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### **15.1.11. The pavement and the beautiful world (or: bladders and lanterns. . . )**

**Note 80** (May 9) It's about time I finally gave a reference for this famous Riemann-Hilbert-(Deligne qui ne dit pas son nom) theorem - Adam and Eve - bon Dieu - (and especially not Mebkhout), which everyone quotes at length (including myself), and for which apparently nobody has yet thought to ask where it is demonstrated. Having been led to believe by my friend Zoghman that the "me- morable theorem" was to be found in his thesis, I did indeed find it in the table of contents, under the name (admittedly down-to-earth and worthy of a cad) "Une équivalence de catégories", Chap. III, par. 3, p..

75. To make matters worse, it's not even entitled to the name "theorem" but is called "Proposition 3.3" (and what's worse, my name appears, underlined again, on the same page). I'll even admit that, having failed to read the previous 75 pages to recognize myself, I wasn't entirely sure whether this was it - Zoghman confirmed that it was, and I trust him<sup>53</sup>. The demonstration (it would seem) is the subject of Chap V of the same thesis - which was passed at the University of Paris VII on February 15, 1979, before a Jury comprising D. Bertrand,

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<sup>53</sup>(\*) (April 17, 1985) It would appear that the generally used form of the "God's theorem" is not that of the theorem quoted here, but a related form demonstrable by the same methods. See the note "Ecllosion d'une vision - ou l'intrus" (n° 171<sub>1</sub> , and in particular today's b. de p. note therein.



R. Godement, G. Houzel, Le Dung Trang, J.L. Verdier. Interested parties who have not yet received a copy from the author (who sent his thesis to all those he could suspect, rightly or wrongly, might be interested) need only ask him, and he will be pleased to do so... ... Of course, he sent a copy to each of my former cohomology students, none of whom has been heard from since. They must have changed subject in the meantime, unluckily. ...

It has to be said that Zoghman definitely doesn't have the knack of selling his merchandise, of presenting it in a clear and appealing way - these are things that have to be learned, and he wasn't as lucky as my former students to learn the ropes from a virtuoso of the trade who didn't skimp on his time. But he can't complain, he's had his "three interviews", and perhaps one of the "luminaries" will one day have the idea of even acknowledging his indigestible pamphlet. He must have realized himself that the paving stone was not easy to read (even if it wasn't lost on Riemann or Hilbert. . . ): he wrote a note to the CRAS, which is still shorter, to draw attention to his famous theorem, the title of which I'll give you in a thousand:

"On the Hilbert-Riemann problem"! I knew my friend Pierre Deligne wasn't any better at history than I was, so all he had to do was restore the chronological order, <sup>□</sup>and contribute the pretty folklop designation "correspondence" and that was it, Zoghman had it coming. ... This note is dated 3.3.1980, Series A, p. 415-417. . 317

Verdier must have learned of the theorem in one of the "three interviews" he gave to his pupil - sic (or at the time of the defense), but he must not have realized it. As for Deligne, he finally realized something, I can't say when, but what's certain is that he knew about it in October 1980, and so did Bernstein and Beilinson, according to what he himself says. Mebkhout himself went to Moscow to explain his results (at length) to Beilinson and Bernstein (in case they had trouble reading him). I don't know if they or Deligne ever read the thesis or the subsequent note to the CRAS, but they must have figured out what was in it, since next year's "memorable Colloque" at Luminy was, coincidentally, all about it.

To sum up, and according to the latest information from my intelligence service, there were at least five people perfectly aware of the situation, who took part in the mystification known as the "Colloque Pervers", namely (in alphabetical order of the actors) A.A. Beilinson, J.Bernstein, p. Deligne, J.L. Verdier and Z. Mebkhout - plus a whole Colloque acultees, surely brilliant mathematicians to boot, who apparently wanted nothing better than to be mystified and take bladders. Mebkhout - plus a whole Colloquium of aculturous people, surely brilliant mathematicians to boot, who apparently wanted nothing better than to be mystified and to take bladders for lanterns<sup>54</sup> (\*). Which proves once again that we mathematicians, from the illustrious Medalist to the obscure unknown student, are not a hair smarter or wiser than the average person.

## 15.2. VIII L' Elève - alias le Patron

### 15.2.1. Credit thesis and comprehensive insurance

**Note 81** <sup>□</sup>(May 8) It seems time to express myself more circumstantially on the "thesis-" affair. phantom", which I had only mentioned "in the aftermath" in two previous notes (notes (48) and (63<sup>'''</sup>)). An inattentive or ill-disposed reader might say that I'm simultaneously reproaching my ex-student J.L. Verdier for two contradictory things - for having "buried" the derived categories, and for having "published" them

<sup>54</sup>(\*) (June 3) In fact, it appears that all Colloquium participants, without exception, had been made aware of the situation on the spot. On this subject, see the note "Le Colloque", n° 75', written today.

(in SGA 4<sup>1</sup>) and claim authorship; just as the same reader would say that I reproached P. Deligne for both "burying" the motifs, and exhuming them (in LN 900). So it may not be superfluous to give a retrospective of the situation, from 1960 to the present day.

Around 1960 or 1961, I proposed to Verdier, as a possible thesis project, the development of new foundations for homological algebra, based on the formalism of derived categories that I had developed and used in previous years for the purposes of a coherent duality formalism in the context of schemes. It was understood that in the program I was proposing to him, there were no serious technical difficulties in prospect, but above all a conceptual work whose starting point was acquired, and which would probably require considerable developments, of dimensions comparable to those of the Cartan-Eilenberg book of foundations. Verdier accepted the proposed subject. His foundations work continued satisfactorily, materializing in 1963 in a "Etat 0" on derived and triangulated categories, multigraphied by the IHES. This 50-page text is reproduced as an Appendix to SGA 4<sup>1</sup> in 1977.

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(as stated in note (63' "))\*<sup>55</sup> (\*).

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□ If the defense didn't take place in 1963, but in 1967, it's because it was unthinkable that this 50-page text, the embryo of a foundational work yet to come, could constitute a state doctorate thesis - and the question of course didn't even arise. For the same reason, when he defended his thesis on June 14, 1967 (before a Jury including C. Chevalley, R. Godement and myself, who presided), there was no question of presenting this work as a thesis. The text submitted to the jury, 17 pages long (+ bibliography), is presented as an **introduction** to a major work in progress. It outlines the main ideas behind this work, placing them in the context of their many uses. Pages 10, 11 give a detailed description of the chapters and paragraphs planned for this seminal work.

If the title of Doctor of Science was awarded to J.L. Verdier on the strength of this 17-page text, outlining ideas which he himself says are not his own<sup>56</sup> (\*), it was clearly a contract of good faith.

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□ between the jury and himself: that he was committed to completing and making available to the public this work of which he

<sup>55</sup>(\*) This text alone may seem a meagre result for two or three years' work by a gifted young researcher. But most of Verdier's energy was then devoted to acquiring the indispensable basics of homological algebra and algebraic geometry, by attending my seminars in particular, and by working one-on-one. His contributions to the duality formalism (see below) came later, once Artin and I had developed the stale duality formalism in detail in SGA (1963/64), when I suggested (in parallel with his work on the foundations of derived categories) that he develop the same formalism in the context of "ordinary" topological spaces and smooth morphisms of such spaces.

It was around the time I began my "Séminaires de Géométrie Algébrique" series with SGA 1 (in 1960) that I was contacted by Verdier, along with Jean Giraud and Michel Demazure, asking if I had any work for them - and they were knocking at the right door! Coincidentally, from the moment I wrote the note "Mes Orphelins" (n° 46) when the three of them contacted me, they had just formed a small seminar called "Séminaire des orphelins" (on the theme of automorphic functions, approaching calculations with a zinc strand), as their boss (or sponsor at the CNRS?) had just left for a year without warning, leaving them hungry and a little empty. That emptiness was soon filled... .

<sup>56</sup>(\*) The beginning of the thesis reads:

"This thesis was written under the supervision of A. Grothendieck. The essential ideas it contains are due to him. Without his initial inspiration, his constant help, his fruitful criticism, I could not have completed it. I would like to express my deep gratitude to him.

I would like to thank Claude Chevalley for chairing my thesis jury and for his patience in reading this text. My thanks to R. Godement and N. Bourbaki for introducing me to mathematics.

"The term "this thesis" can hardly refer to anything other than the body of foundational work undertaken, of which the text submitted constitutes the introduction - work which was therefore not, strictly speaking, "completed" at the time of the defense.

(May 30) This inconsistency reflects the ambiguity of a situation for which I was primarily responsible, as thesis director and (if the cover of the copy of the thesis in my possession is to be believed) as president of the Jury. There was a lack of "rigor" on my part towards a brilliant student, a complacency that goes in the same direction as that which I had shown towards Deligne (see the note "L'être à part", n° 67'), and which contributed its share to make bear the same fruits.

presented a brilliant introduction. This contract was not kept by the candidate<sup>57</sup> (\*): the text he announced, a text on the foundations of homological algebra from a new, proven point of view, was never published.

Clearly, if Verdier's work between 1961 and 1967 had been limited to writing the skeletal "Etat 0" of 1963, the jury would not have considered accepting this "thesis on credit". The writing of his work had to be sufficiently advanced to allow completion within a year or two, and for practical reasons it seemed appropriate that Verdier should have the title without waiting for the work on which it was based to be completed.

It should be added that between 1964 and 1967, Verdier had made some interesting contributions to duality formalism (81<sub>1</sub>), which, together with the foundational work he was supposed to be pursuing, could justify the credit given to him. His contributions to duality as a whole could, in a pinch, have constituted a reasonable doctoral thesis. Such a thesis, however, would by no means have been in the style of the work I am accustomed to proposing, all of which consists in the systematic development to completion of a theory whose need and urgency I sense (82<sub>2</sub>). I don't recall Verdier ever raising the question of presenting such a "thesis on titles", and I doubt I would have accepted, since such a thesis would have in no way corresponded to the "contract" that was signed between him and me, when I entrusted him with the beautiful subject of derived categories, with the task of developing foundations on a vast scale.

As J.L. Verdier's thesis supervisor and president of the jury, I accept full responsibility for my carelessness in awarding him (jointly with C. Chevalley and R. Godement, trusting the title of doctor on work that had not yet been done<sup>58</sup> (\*\*)).

□ I am not justified in complaining if I now see some of the fruits of my levity. But this p. 322 does not prevent me from stating this publicly, and that the actions of my ex-student J.L. Verdier are his responsibility alone, and that of no one else.

Not to keep the contract he had made with me and with the Jury who had placed their trust in him, was a way of burying the point of view of derived categories that I had introduced and that he had taken on the task of founding through a major work. This work may have been done, but it was never made available to the user. It was a way of "writing off" a set of ideas that he himself had helped to develop.

The revival of the notion of derived category in Mebkhout's work met with no encouragement from Verdier (nor, for that matter, from any of my other cohomological "luminaries"). The de facto boycott of derived categories seems to me to have been total until about 1981<sup>59</sup> (\*), when they made their comeback in force at the "memorable Colloque" at Luminy (see note (75)), under a sudden surge of demand.

However, State 0 of Verdier's "thesis" had already appeared four years earlier, in 1977, as an appendix to the

<sup>57</sup>(\*) It is all the more remarkable that J.L. Verdier refused my proposal to sit on Contou-Carrère's thesis jury in December 1983, with J. Giraud, and myself acting as research director, believing that the thesis (though entirely written and carefully read by J. Giraud) and the jury would not offer sufficient guarantees of seriousness, without referring to the control of a Commission des Thèses des Universités **Parisiennes** (Sic).

<sup>58</sup>(\*\*) To this responsibility I should add that of not having ensured, during the two years that followed (before my departure from the mathematical scene), that Verdier actually kept to the contract he had signed. It has to be said that my energies were so focused on pursuing the foundational work I had taken on myself, not to mention motivational reflections and the like, that I didn't have to think too much about the unpleasant task of reminding others of their obligations. I had to learn of Verdier's decision to abandon the publication of the planned work in the early '70s, at a time when I was absolutely no longer into maths, and when the idea would not have occurred to me to "react".

<sup>59</sup>(\*) (May 30) These somewhat dubious forms of style are in fact out of place. As Zoghman Mebkhout confirmed me (who paid to find out), what I dubiously put forward about the status that was given to homological algebra "Grothendieck style" corresponds well to reality.

volume SGA 4<sup>1</sup> (see note n° 63<sup>1</sup>) - so ten years after the defense of his thesis, and at a time when (to my knowledge(\*))  $\frac{1}{2}$  Mebkhout was the only one to make use of derived categories in his work, against the fashion of the seven years that had preceded. Unless I'm mistaken(\*) , he remained the only one to do so, right up to the time of the great "rush" around the famous "Riemann-Hilbert correspondence" at the Colloquium already mentioned, where Deligne alias Riemann-Hilbert appeared as the father of this "correspondence" - sic, and Verdier (with his providential Etat 0 abundantly quoted by his generous friend) appeared as the father of derived categories and algebra.

p. 323 homological style 2000, with no mention of my humble self and even less of Mebkhout<sup>60</sup> (\*\*).

□ In the light of these events, I believe I understand the reason for the unexpected publication of this State 0 which (as it says in the introduction to SGA<sub>2</sub> 4<sup>1</sup> by the same friend) "had become unobtainable" and that nobody cared about "finding", except (perhaps) Zoghman Mebkhout<sup>61</sup> (\*). So there was just this one unfortunate fellow who, in his own corner and against all odds, persisted in using these notions of a bygone age, without anyone really knowing what he was getting at - so stubborn, in fact, that we began to doubt whether he wouldn't one day come up with something that would do the trick, you never knew... . After all, the man to whom he sometimes imprudently referred as one of his sources of inspiration (alongside the Master's works) had, in the past, proved or found things with all that, things that couldn't be ignored even if the author - and the Master himself - were forgotten, Jean-Louis Verdier himself, had he not made his start to stardom with this "Lefschetz-Verdier" formula, which he would have been hard-pressed even to write down, let alone prove, without all these notions fit for the dustbin. . .

While my influential ex-student of almost ten years (since he had rid himself of a certain annoying formality. . . ) **was betting against** derived categories and would continue to bet against them until time X (of the famous Colloquium), he must have thought it prudent (you never knew. . . ) to pre-empt events that might occur. . . ) to anticipate events that might occur, in short, to take out "all-risk insurance", by publishing (not the large-scale work that was one day supposed to constitute a thesis, but) a "text-witness", a sort of exhibit "just in case. . . ."; a text that would "attest to his claim to paternity over an **orphan** whom he] had taken a liking to, and whom he continued, pending events, to disown<sup>62</sup> (\*\*).

p. 324 **Note 81<sub>1</sub>** □ The contributions in question are: 1) Foundations of a duality formalism in the context of locally compact spaces and 2) that of Galoisian modules (in collaboration with J. Tate); 3) the Leschetz-Verdier **fixed-point** formula; 4) duality in locally compact spaces.

Contributions 2) and 3) are "unforeseen" compared with what was known. The most important contribution seems to me to be 3). Its demonstration follows easily from the duality formalism (both for "discrete" and "continuous" coefficients), which does not prevent it from being an important ingredient in the arsenal of "all-purpose" formulas available to us in cohomology. The existence of this formula was discovered by Verdier, and came as a (pleasant!) surprise to me<sup>63</sup> .

<sup>60</sup>(\*\*) Compare with the comments in the notes "Le compère" and "L'Iniquité - ou le sens d'un retour" (n° s 63<sup>1</sup> and 75).

<sup>61</sup>(\*) In any case, it was while perusing the bibliography of a work by Z. Mebkhout that I had just received, towards the end of April, that I learned of the publication of this "Etat 0", when I had even forgotten the existence of this text from another age. ...

<sup>62</sup>(\*\*) If J.L. Verdier had really wanted to make known the yoga of derived categories, which has been buried for seven years, he would have chosen to publish the introductory text which constitutes his thesis, rather than a technical text which nobody cared about and which only acquires interest in the background of yoga and its many uses. But it's understandable that he had no desire to append to the 50-page text the 17 pages of his thesis, containing now embarrassing statements about the role of the one who must not be named. ...

<sup>63</sup>(\*) (April 19, 1985) I return to this beautiful formula, its role and its strange vicissitudes during the Burial, in the three notes "Les vraies maths. . . .", "... and "nonsense", "Tricks and creation" (n° 169, 169, 169), in the fourth part of Récoltes et Semailles.

The formalism of duality in the context of locally compact spaces is essentially the "necessary" adaptation of what I had done in the context of scalar cohomology of schemes (and without the difficulties inherent in this situation where everything was still to be done). He did, however, contribute an interesting new idea, that of the direct construction of the functor  $f^!$  (without prior lissification of  $f$ ) as a right-hand adjoint of  $Rf_!$ , with an existence theorem to boot. This procedure was taken up by Deligne in étale cohomology, enabling him to define  $f^!$  in this framework, without any lissification hypothesis.

These comments make it clear, I think, that by 1967 Verdier had demonstrated his capacity for original mathematical work, which of course; was the determining factor in the credit he received.

**Note 81<sub>2</sub>** As another example, I would point to the detailed development of the duality formalism in the context of locally compact spaces, in the spirit of the "all-purpose" formalism of the six operations and derived categories, of which Verdier's presentation at the Bourbaki Seminar would constitute an embryo. Even in the context of topological **varieties** alone, there is still, to my knowledge, no satisfactory reference text for Poincaré's duality formalism.

□(June 5) There are two other directions in which I note with regret that Verdier did not see fit to go all the way.

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the end of work that he had started off strongly enough to **take credit for** (I mean, by starting up a duality formalism in the context of discrete coefficients and locally compact topological spaces), whereas the essential ideas are not due to him and he doesn't care (any more than for the derived categories) to make himself the **servant of a task** and put at the user's disposal a complete formalism (as I tried to do in the three seminars SGA 4, SGA 5, SGA 7).

The duality program I was planning and suggested he develop was set in the framework of general topological spaces (not necessarily locally compact) and applications between such that are "separated" and locally "lissifiable" (i.e. locally the source is immersed in a  $Y \times \mathbb{R}^n$ , where  $Y$  is the goal space). This was clearly suggested by the analogy with the framework of the stale cohomology of **arbitrary** schemes. Verdier was able to see, in the context of locally compact spaces, that the assumption of local smoothness of applications was unnecessary (which came as a surprise). Nevertheless, the context of locally compact spaces (which excludes "parameter spaces" that are not locally compact) is visibly short-sighted. A more satisfactory context would be the one that would cover both the one chosen by Verdier, and the one I was planning, i.e. the one where topological (or even topos?) spaces are (more or less?) arbitrary, and where applications  $f: X \rightarrow Y$  are subject to the restriction of being 1) separate and 2) "locally compactifiable", i.e.  $X$  plunges locally into a compact  $Y \times K$ ,  $K$ .

In this context, the fibers of an "admitted" application would be locally compact quel- conical spaces. Another step would be to admit that  $X$  and  $Y$ , instead of being topological spaces, are "topological multiplicities" (i.e. topos that are "locally like a topological space"), or even topos of any kind, by restricting the applications in a suitable way (to be made explicit), so as to find fibers that are **locally compact multiplicities**, subject if need be to additional conditions (close perhaps to the point of view of Satake's  $G$ -varieties), for example (and lastly, to the point of view of Satake's  $G$ -varieties). rigorously!) to be locally of the form  $(X, G)$ , where  $X$  is a compact space with **finite** operator group  $G$ .

To my knowledge, even the "ordinary" de Poincaré duality has not been developed in the case of multi-smooth compact topological cities (smooth: which are locally like a topological variety). The case of a classifying space of a finite group seems to show that we can hardly hope to have a duality theorem (absolute global) other than module torsion, more precisely, by working with a ring of coefficients that is

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a Q-algebra. With this restriction, I wouldn't be surprised if Poincaré's duality ("six operations" style) worked as is in this context. No wonder nobody ever looked at it (except unrepentant differential geometers, pretending to look at the cohomology of the "leaf space" of a foliage), given the general boycott on the very notion of multiplicity, instituted by my cohomology students, Deligne and Verdier in the lead.

To put it bluntly, what's missing is a fundamental reflection of the following type: describe (if you can) in the context of any topos and bundles of "discrete" coefficients on them, notions of "cleanliness", "smoothness", "local cleanliness", "separation" for a morphism of topos, enabling us to derive a notion of "admissible morphism" of topos  $f : X \rightarrow Y$ , for which the two operations  $Rf_!$  and  $Lf^!$  make sense (one adjoint of the other) so as to obtain the usual properties of the six-operation formalism. Here, topos are considered as non-ringed, or perhaps as provided with Rings (which are assumed if necessary to be constant or locally constant), assuming (initially at least) which ringed topos morphisms  $f : (X, A) \rightarrow (Y, B)$  are such that  $f^*(B) \rightarrow A$  **is an isomorphism** (81<sub>3</sub>). The foregoing reflections suggest that when we restrict ourselves to Rings of coefficients of zero characteristics (i.e. which are Q-Algebras), we can be considerably broader in the notion of "admissible morphism", so as to encompass "fibers" which are e.g. (topological or schematic) multiplicities, rather than ordinary (topological or schematic) "spaces".

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A first step in this direction (apart from the cases treated by me, then by Verdier on the same model) is due to Tate and Verdier, in the context of discrete or profinite groups. The memory of this first step encouraged me to pursue a reflection along these lines last year, in the context of small categories (generalizing discrete groups) serving as homotopic models. Without going very far, this reflection was nevertheless enough to convince me that there must be a complete formalism of the six operations in the context (Cat) of the category of small categories. (See on this subject the □ "Pursuit of Fields", Chap.VII, par.136, 137.) The development of such a theory in (Cat), or even in Pro(Cat), just like a theory of this type in the context of topological or schematic spaces and multiplicities, would for me have as its main interest to be a step towards a better understanding of "discrete duality" in the context of general topos.

Illusie told me last year that he had struggled with duality perplexities in the case of semisimplicial spaces (or schemes). It seemed to me to be the same old tobacco - to be able to detect the existence of a six-operation formalism in a particular case, and to understand it. But it would seem that the mere prospect of a fundamental reflection has the gift of freezing each and every one of my former students - at least among my cohomology students. If I went to any trouble with them, it was with the conviction that they would not stop right where they had left off (in terms of conceptual work) with me, and remain wringing their hands every time a new situation showed that the work they and their buddies had done with me was insufficient. The conceptual work we do is **always** insufficient in the long run, and it's by taking it up again and going beyond it, and not otherwise, that mathematics progresses. Between 1955 and 1970, each year again I found that what I had done in previous years was not sufficient to the task, and I went back to the drawing board, at least when someone else (e.g. Mike Artin) with the point of view of "algebraic varieties" in his sense) hadn't already done so. But it would seem that my students have also buried the example I set them, along with myself and my work.



**Note 81<sub>3</sub>** I seem to recall that in the formalism of the six variances in (say) staggered cohomology, the assumption that the ring bundles serving as coefficients are locally constant is unnecessary - the essential assumption is that they are prime torsion bundles with residual characteristics, **and** that  $f^1(B) \rightarrow A$  is an isomorphism. When we abandon this last assumption, we have to enter a theory (never yet made explicit, to my knowledge) that "mixes" the "discrete space" duality, and the "coherent" duality (relative to the rings of coefficients and their homomorphisms). As a result, we envisage replace, on the diagrams (or more general topos)  $X, Y$ , the Rings with coefficients  $A, B$  by relative (not necessarily affine) schemes  $X', Y'$  on  $X, Y$ , and ringed topos morphisms  $\square$  p. 328  $(X, A) \rightarrow (Y, B)$  by commutative diagrams of the type

$$\begin{array}{ccc} X' & \xrightarrow{\quad} & X \\ \downarrow & & \downarrow \\ Y' & \xrightarrow{\quad} & Y \end{array}$$

with a "six operations" formalism in such a context. When  $X, Y$ , etc. . . are the ponc- tual topos, we should find the usual coherent duality.

## 15.2.2. The right references

**Note 82** (May 8) This is J.L. Verdier's article "Classe d'homologie associée à un cycle", published in Astérisque n° 36 (SMF), p.101-151 in 1976. In a way, this rather unbelievable article (although nothing should surprise me any more. . . ) is a counterpart to the "perverse article" by Deligne et al. With one reservation: it practically consists in **copying** over fifty pages, in a slightly different context, notions, constructions and reasoning that I had developed at length ten or fifteen years earlier - terminology, notations, everything is there verbatim! I'd have thought I'd returned to a session of the APG 5 seminar held in 1965/66, where these things were spelled out (apparently to the satiation of the participants<sup>64</sup> (\*) for an entire year. After this seminar, at least, all these things became part of the "well-known" for people in the know<sup>65</sup> (\*\*). Verdier had attended, of course, as had Deligne (the only one who was never left behind, even though it was the first time he'd set foot' at my  $\square$  séminaire<sup>66</sup> (\*) - it took p. 329 do it... ). It's true, well, well, that in 1976 it had been ten years since the "writing-sic" of this famous seminar by "volunteers-sic" who were fed up with it was dragging on - I see now that one of these "volunteers" took charge of the "writing-sic" in his own way, even before the publication of SGA 5 in

<sup>64</sup>(\*)For comments along these lines, see notes n° s 68, 68' "Le signal" and "Le renversement", in which I examine the strange vicissitudes of the writing of this seminar, and the relationship between these and Deligne's "SGA 4 operation<sup>1</sup>". The following reflection reveals to me another unforeseen aspect of these vicissitudes and of the dismemberment of the mother seminar by the combined care of Verdier and Deligne. Verdier's and Deligne's publications on this dismemberment date from 1976 and 1977 - they constitute the "green light" given to Illusie to prepare (eleven years later. . . ) the publication of  $\square$ SGA 5 (which, as Deligne says in SGA 4<sup>1</sup>, "may be seen as a series of digressions, some of them very interesting").

<sup>65</sup>(\*\*) For a reflection on this "hasty" impression, see the note on "Silence" (n° 84).

<sup>66</sup>(\*) The year of this seminar was (I think) the year I met Deligne, who must have been nineteen at the time. He "got into the swing of things" very quickly, and even took on the task of writing my lectures on étale duality from the previous year (which he must have known from my explanations and notes), and also the lecture on the cohomology class associated with a cycle, which was discussed in the note quoted n° 68' ("Le renversement"), and which will be discussed a little more in this one. The fact that with the means at his disposal, and a complete mastery of the subject, he waited eleven years to write the essay, to then include it in his  $\square$ SGA 4<sup>1</sup> without informing me, now shows me, in retrospect, that as early as 1966 (and not just 1968 as I may have assumed - see note n° 63, "L'éviction") - i.e. from the very first year of our meeting, there was a profound ambiguity in my friend's relationship with me, expressed from that moment onwards in a perfectly clear way, which I have refrained from learning about to this day!

1977 ! It would appear that the vicissitudes of this unfortunate seminar were not only to Deligne's advantage, as he took advantage of a situation of debandade in his own way. But at the time, Deligne still took care to mention in his essay (on the cohomology class associated with a cycle) "d'après un exposé de Grothendieck" (after a paper by Grothendieck), while dismantling SGA 5 from one of his key lectures and attaching it to his SGA 4<sup>1</sup> as a matter of course. (It's true that this compensated him for being able to present me as his "collaborator"! - see note "Le renversement", n° 68').

Coming back to the **homology** class (not to be confused!) associated with a cycle (which, according to the title, is the subject of Verdier's article), I had developed this formalism in great detail, over several presentations, during the oral seminar, before an audience that, incidentally, begged for mercy (except always the only Deligne, always dashing and fresh... ). It was one of the innumerable "long exercises" I developed that year on the formalism of duality in the étale frame, feeling the need to arrive at a complete mastery of all the points I felt needed to be thoroughly understood. The interest here was to have a valid formalism on an ambient scheme that is not necessarily regular - the passage to the **cohomology** class in the case of

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regular, and the link with my old construction using cohomology with supports and immediately giving compatibility with cups-products, being immediate. I also found that □ this part of the seminar does part of the lot that didn't make it into the published version - no doubt Illusie (on whom all the hard work of preparing a releasable (hmm) edition eventually fell) must have been quite happy that Verdier took care of it, mutatis mutandis (i.e. here: without changing a thing!).

As the saying goes, "it hardly needs saying" that my name does not appear in either the text or the bibliography (except implicitly by the ever-present reference SGA 4, which we'd still have to find a replacement for... ). There's no allusion to a "Seminar on Algebraic Geometry" under the acronym SGA 5, which the author might have heard of - although I seem to remember seeing him busy taking notes (like everyone else, except Deligne of course. . . ).

Incidentally, I've exaggerated just a little by saying that my name is absent from the text - it makes a single, mysterious and lapidary appearance on page 38, section 3.5, "Fundamental cohomology class, intersection" (here we come, the crux of the matter!). The reference consists of a cryptic sentence whose meaning escapes me, I confess: "The idea of systematically using weight complexes (??? those damn weights again!) is due to Grothendieck and was put into shape by Deligne" - without any further explanation of these mysterious "weight complexes" whose idea I had and of which I hear mention here for the first time. There will be no further mention of them in the rest of this article (nor was there any mention of them in the 37 pages before). Understand who can! As for the content of the said section, it is copied without further ado from the SGA 5 seminar which had taken place ten years earlier (and by which time this construction was already five or six years old, see note n° 68'), a seminar which he is careful not to quote. The reference to Deligne (who is said to have "perfected" an idea that had already been perfected when my friend was still in high school!) is a "flower", the idea of which no doubt came to the author because the young and newcomer Deligne had indeed taken on the task of writing my paper on this subject (and refrained from doing so for eleven years, for the benefits we know, see note cited). This "flower" is part of the exchange of courtesies between inseparable friends.

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There is, however, one (undoubtedly) new and very interesting result in the article (th.3.3.1., page 9) on stability of discrete bundles analytically constructible by direct higher images through an analytic and proper morphism. Verdier had learned about all-round constructibility from me one day, fifteen years earlier, as well as □ the stability conjecture, which I had asked myself (and told who would listen) in the late fifties, before I'd had the pleasure of making his acquaintance. Reading the article, the idea wouldn't occur to an uninformed reader (but those are becoming rare. . . ). I

I'm still repeating myself, I'm afraid) that the author isn't serving up hot-off-the-press notions and statements he's only just discovered. He doesn't have to say it's him - because it's self-evident. It's the famous "thumb" style that's so obviously catching on.

Apart from this detail (which, I feel, is in line with the new canons of the trade), there must still be around ten pages (out of fifty), around this interesting result, that present the author's personal work. All things considered, what strikes me most about Verdier, as with Deligne, is that he's perfectly capable of doing beautiful mathematics. Even in this sad article, there's a hint of it in the theorem quoted. But by keeping himself (like his friend) in a gravedigger's mood, he operates, like his prestigious friend, on a paltry fraction of his means. A sign (which astounded me) of apparent mediocrity, in a mathematician who nevertheless gave proof of astute and flair, was the total lack of instinct to feel the scope of the work of his "pupil-sic" Mebkhout, whom he took pleasure in treating from the height of his greatness, without ever having been able to do himself a work of comparable depth and originality<sup>67</sup> (\*). Not that he isn't just as capable as Mebkhout or me. But he has never given himself the chance to do great things, that is, to let go of a passion - rather than **using** mathematics and its gifts **to** dazzle, dominate or crush. Up until now, he's been content to take up as is the fruitful notions and points of view that have already been baked in. Indeed, he seems to have totally lost touch with the meaning of **mathematical creation**.

Yet I seem to remember that when he worked with me, that sense was still there. Nothing external about him prevents that sense from resurfacing. Just as in his friend, in whom I often felt this same eclipse of something delicate and lively, obturated by the same fatuity. .332

This incredible 50-page article, which appeared in a standing magazine, sheds new light on the "The note - or the new ethic" incident (s.33). where a note to the CRAS of **a few pages**, summarizing a solid and **original** work, on an important subject (in my humble opinion), the fruit of **two years' work** by a highly gifted young mathematician, was rejected by two eminences as "devoid of interest"<sup>68</sup> (\*). One of these eminences was none other than Pierre Deligne - the same Deligne who did not disdain to copy in toto and in person the humble doctoral thesis of one of my students (whom he made a point of quoting). (This duplicate, enhanced by a prestigious signature, makes the largest article in the "memorable volume" LN 900 of a no less prestigious collection! See end of notes (52), (67)).

The "tableau de mœurs" is growing by the day, without my having to come out of retirement and hit the streets to mingle with the "big world". A few hours spent here and there leafing through a few well-chosen "great texts" were enough to edify me...

### 15.2.3. The joke or the weight complex

**Note 83** ( May 8-9) I've been thinking about this "weight complex" referred to in the "reference - thumb" in Verdier's memorable article<sup>69</sup> (\*\*) - a reference that's sheer nonsense. As soon as I saw this ludicrous reference, an association came to mind that kept running through my head. This isn't the first time, far from it, that I've found myself faced with something

<sup>67</sup>(\*) The same astonishing lack of flair was evident on this same occasion in Deligne, who didn't "feel the wind" (the importance of Mebkhout's ideas) until 1980, it seems, even though Mebkhout had been working in this direction since 1974. On more than one occasion, I've had occasion to observe my friend's natural flair obturated by sufficiency, especially since 1977 (or 78), which seems to have been a first "turning point" (see notes "Two turning points" and "Les obsèques", n°s 66,70).

<sup>68</sup>(\*) For details, see the note "Casket 4 - or topos without flowers or wreaths", n° 96.

<sup>69</sup>(\*\*) See previous note "Good references".

that seem to defy rational explanation - even though the meaning is clear and unmistakable and clearly perceived, but at a different level from that of conventional logic. This was the only one on which I had functioned at a conscious level for most of my life - with the result that I was constantly overwhelmed by "bizarre", incomprehensible events - distressing in their irreducible saugrenousness! My life changed a lot from the moment (less than ten years ago) when I started

p. 333 to live on a wider register of my faculties. I've come to understand that every absurdity, every so-called "nonsense" has a **meaning** - and the mere fact of <sup>□</sup>knowing this, and from then on being curious about the meaning behind the nonsense, often opens me up to its obvious meaning.

In this nonsense about "weight complexes", I think I sense an act of **bravado** of the same nature as in the appellation "faisceaux pervers"<sup>70</sup> (\*) - the pleasure, in this case, of proving to oneself that one **can afford**, in a journal of standing and in a text that claims to be a standard reference text<sup>71</sup> (\*\*), to say a related nonsense, and that **nobody** will dare to even ask a question! And I'm convinced that the wager contained in this bravado, in the eight years since the article appeared, has **been won to this** very day: that I was the first today to put the naive question to the author.

Of course, the time (or place) at which a saugrenuité appears, in this case at the precise moment of the one and only reference to my person, is by no means coincidental; nor is the form it takes, here by allusion to a type of notion, "weight", entirely foreign to the theme of the entire article, and by the improvisation of a composite notion "weight complex" that never existed! The association that immediately presented itself to me could well provide the key to the more precise meaning of the saugrenuité, beyond the bravado, the demonstration of power. It's the association with an allusion just as sibylline and just as purely formal (but without yet having the added dimension of saugrenuité!) in Deligne's article quoted at the beginning of note (49)<sup>72</sup> (\*\*\*). It was an obscure allusion, in an article where the word "weight" was rigorously absent and where nobody but Serre or I would have been able to see them, to "weight considerations" which had led me to conjecture (in a less general form, it is clearly stated) the main result of the work. As I explain in the more detailed note "The eviction" (n° 63), behind this rhetorical allusion lies the intention to **conceal** both my role, and the ideas (concerning "weights" and "weights") that I'd been working on.

p. 334 their relationship to cohomology in general, and Hodge's in particular) which he intended to reserve for himself alone. This intention must have been all the more clearly perceived by Verdier as he himself <sup>□</sup>me "operates" on the same diapason (in his relationship with me, at least, which seems to me to be the main cement between the two inseparable friends). In either case, an honest presentation would have consisted in starting the article by clearly indicating the source(s) for the main ideas, or for the question(s) that motivated the article.

Having said this, here's the meaning I see behind the symbolic language of apparent nonsense: I can allow myself, without the slightest embarrassment, to display patent **nonsense** in front of everyone, and at the same time express through this nonsense my true intention, with this absurd allusion-reference to the "weight complex": that is, I have no more intention of revealing anything about the role of Gr. in this work, any more than Deligne had such an intention with his empty allusion to "**weight** considerations"-which allusion made no more sense to the reader then than it does now with the imaginary "**weight-complexes**" I've just described.

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<sup>70</sup>(\*) See "Perversity", n° 76.

<sup>71</sup>(\*\*) And it seems that this text is indeed a standard reference today - at any rate, for years it was one of Zoghman's bedside texts (he recently sent it to me). It was there that he learned about the notion of constructibility (which plays an essential role in his theorem), and for a long time he was convinced that Verdier was the brilliant inventor of this crucial notion for him.

<sup>72</sup>(\*\*\*) This is the note "Poids en conserve - et douze ans de secret". For a more detailed examination of this Deligne article from the point of view that interests us here, see "L'éviction", note no. 63, quoted below.

to invent right now, for the sake of the cause and my own pleasure!

I've just posted this note, written yesterday - I was interrupted earlier by a phone call from Verdier, whom I'd tried to reach during the day, to ask him just that question. I explained that I was trying to learn a bit about cohomology, which he knew I'd never understood, and that Mebkhout had passed on to me for my instruction an old article by him, Verdier, a work that had long served as his bedside text. I was now trying my best to read it, but there was this cryptic reference - it was nice of him to quote me, of course - but I had no idea what he was talking about.

He was quite happy, even a little flattered, but yes, with a broad smile that protruded behind an air of paternal joviality, that I'd end up like this in my old age, learning cohomology on this old paper of his. I didn't expect him to contradict me when I said that he knew I'd never understood anything about cohomology - obviously that had been agreed long ago. ... As for those famous "weight complexes", I could feel his broad smile again at the end of the line (you'll say I'm making it up!), delighted that someone (and the addressee himself, no less) had finally picked up on his point.

something that had gone by the wayside for so long. At the same time, there was also a hint of embarrassment - more the embarrassment (I think) of not having been able to hide from a  $\square$  pleasure (like the pleasure one would take in ap . 335

slightly salacious story... ), than not knowing what to answer. Dumped as I was, he really didn't have to worry about that! Without a moment's hesitation, he turned to Deligne (whose name I hadn't mentioned), who had given a demonstration in one of his articles, in which he also quoted me (he couldn't quite remember where) - in any case, it was a question of weights, but yes, he'd forgotten a little, of course - but not arithmetical weights, because I was quite right, it wasn't the same....

His tone was jovial and unapologetic, and he made it clear that he'd already given me quite a bit of his time - with a slightly hurried air, but without losing his debonair, slightly protective tone. I apologized for bothering him like that, with a rather stupid question, and thanked him for his explanation. My apologies were sincere and so were my thanks - he had indeed taught me everything I wanted to know.

<sup>73</sup>(\*) .

## 15.3. IX My students

### 15.3.1. The silence

**Note** 84  $\square$  (May 9) I was perhaps a little brisk yesterday, writing that in "the correct reference" (see note (82)) p. 337 what the author and ex-student shamelessly recopied as "part of the realm of the 'well known' for those in the know". I tried to explain who these "people in the know" were - with the conclusion that **they were no more, no less, than the dear listeners of this SGA 5 seminar** in 1965/66 - listeners, as I've had occasion to say, and judging by the vicissitudes of the writing of this seminar in the hands of volunteers whose lack of conviction I didn't want to sense, it was often rather "more" than "less" (always with the exception of the same Deligne ; of course). Indeed, there was no risk of other people "getting involved" as long as SGA 5 had not been written and published, precisely to enable people to "get involved" by reading it! This seminar was in fact published (as fate would have it) after the two "memorable

<sup>73</sup>(\*) Even with my droopy airs, I didn't really feel like I was putting on an act (I don't have the gifts for it), it was perfectly natural - in truth, I'm a bit droopy in all this stuff I haven't handled in nearly fifteen years! But I think that even when I'm old and ripe for the hearse, I'll still be able to feel the difference between an empty walnut and a full one. .

publications" by two of my dearest students and comrades-in-arms, namely the article in question by Verdier in 1976 (in which he says nothing about the origin of the ideas he develops, published there under his pen and for the first time), and Deligne with SGA 4<sup>1</sup>, which has already been discussed at length<sup>74</sup> (\*). After that, we cordially invite Illusie to take care of publishing the rest!

I can't remember in detail who took part in this seminar - whether Artin was there or not, for example. I think that more or less all my students from the first period must have been there in any case.

- with the exception of Mrs. Sinh and Saavedra (whom I hadn't met at the time) and perhaps Mrs. Hakim. There was also Bucur (since deceased). Houzel, Ferrand - I'm not counting Serre - who never had a taste for big cohomological fuss, and who came to put his feet up cautiously from time to time. While no one except Deligne perhaps had a good sense of where all this was leading,

it seems to me that there must have been ten or twelve listeners (not very involved) who were at least

p. 338 following enough to be considered "in the loop" ☐ <sup>the</sup> thought that ran through my head since yesterday, it's that among all these people "in the know", thus representing cohomological expertise (if not all "luminaries" like Illusie and Berthelot, with their "cohomological" theses that were decidedly weighty), and even apart from Verdier and Deligne - there must be quite a few who've had Verdier's article in their hands! A certain air in Verdier gives me the conviction that nobody ever suggested to him that something might be wrong. And I also know that nobody ever drew my attention to it - I learned of the existence of this article on May 2. exactly a week ago today, thanks to Mebkhout, who had of course known about the scam for years.

This gives concrete meaning to the euphoric observation of the "Unanimous Agreement" (to bury my modest self) made ten days ago (note (74))! This agreement encompasses many (if not all) of my "pre-1970" students, i.e. many of those who today set the tone in the mathematical world; and it includes (or has included) my friend Zoghman himself, treated as a Cinderella by the beau monde and clinging against all odds to a kind of "fidelity to my work" (to use his own expression<sup>75</sup> (\*)), which he has had the temerity and obstinacy to claim for himself at times, with the consequences that we know. Go figure!

In short, I was wrong to suggest that such and such a standing journal published a sort of "boilerplate" article that merely copied what was "well known". What the author was copying in full view (if not of everyone, but) of many witnesses was neither published nor "well known" (except for the cohomology class of a cycle in the coherent framework, where I had published it ages ago); and these were additional ideas that I would be remiss to play down, given that I didn't consider it a waste of my time to spend a year developing these and other ideas in a seminar, in front of a large audience. Verdier's article is probably a useful and well-done "digest" of a small part of the ideas and techniques I had developed: precisely so that they can be passed on to a wider audience.

the realm of the "well-known", the daily bread of those who use cohomology (or homology) for objects that

p. 339 more or less deserve the name "varieties" ☐ From this point of view, then, Verdier has done what was useful to do<sup>76</sup> (\*), and in the end I have no reason to be unhappy. However, from what I sensed from my ex-student

<sup>74</sup>(\*) See notes n° s 67, 67', 58, 68'.

<sup>75</sup>(\*) (June 7) Reading all the notes on L'Enterrement during a recent visit. Zoghman pointed out to me that the expression he had used of "reliability in my work" did not really capture his thoughts. Rather, he had in mind a ~~confidence~~ <sup>confidence</sup> in his own powers of judgment and mathematical instinct, which told him that my work provided him with some of the ideas he needed. It's all about **self-confidence, which is** essential if you want to do something truly innovative.

<sup>76</sup>(\*) He did so, it's true, at the expense of the "dismantling" of the original SGA 5 seminar, of which he and Deligne were the main players and "beneficiaries".

(June 7) The reflection of May 12, three days later (see note "The massacre" n° 87) made it clear that Illusie was even more directly associated than Verdier with what appears to be more a "massacre" indeed than a dismantling - even if he

and friend even today, on the phone, and through many other things I've sensed from him (the "biggest" of which, or at least the most "spectacular", is the mystification of the Pervers Colloquium) - I can feel that **something is amiss**. That memorable Colloquium was certainly brilliant, mathematically speaking, in many respects. What's "wrong" is at a completely different level. I could try to define it in words, but I'm afraid that wouldn't make much sense. Anyone who doesn't feel what's wrong with this Colloquium - and I'm sure with many other Colloquia too, without mystification or anything - won't feel it a hair more, once I've made this attempt to "pin it down" and even succeeded in doing so to my complete satisfaction... ..

The question that remains open for me is whether this "sign" represented by what is undoubtedly a relatively common occurrence today (of an author presenting as his own the unpublished ideas of others) - whether this sign is that of a general degradation of morals, So, is it just a typical sign of a "spirit of the times" in today's mathematical world, or does it have more to teach me about myself - about the person I once was, and who is now coming back to me, through the attitudes towards me of those who were my students?

The two possible meanings are by no means mutually exclusive. My ex-students' relationship with me could not have found this way of expressing itself, if a certain state of morality didn't encourage them to do so. In fact, even before this "sign", I saw many others that seem to me to be even more eloquent in terms of a "picture of morals". What struck me about this sign is the particularity that sets it apart from all the others: it seems to **involve most of my former pupils at the same time**.

Such a circumstance cannot be fortuitous. To simply put it down to a "deterioration of the mores" (all that's real) would be a way of evading its more personal meaning, which implies me as a it involves each and every one of my ex-students. If I say "each", which seems to go beyond the actual amplitude of this

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sign, I'm weighing my words carefully. For this sign is a timely reminder that it is scarcely conceivable that one of my former students has not at least been confronted with situations of this kind. For years now, I've felt a certain "wind" about me, blowing through the world of mathematicians I've left behind (a wind whose origin and reasons I can now clearly see, it seems). There is no way that any of them could have failed to feel the breath of this wind, whether on the occasion of an "incident" such as the publication of this gravedigger-article, or on any other occasion. Whether he wanted to or not, such an encounter inevitably raised (or raised again) the question of his relationship with me, who had taught him his trade. And the sign I've noticed, beyond the one that just brought me to this point, is that **I haven't heard a word on the subject from any of my students**<sup>77</sup> (\*). This is a "coincidence" the meaning of which still escapes me - but which cannot fail to make sense (84 ).<sub>1</sub>

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The day is dawning - I feel it's time to stop. I'm not sure that this is the time and place, in Harvest and Sowing, to pursue the meaning of this striking coincidence. It's a harvest perhaps reserved for other tomorrows, if my reflections of this night meet with an echo in one or other of those who were my students.  
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**Note 84<sub>1</sub>** (May 16) This perfect agreement between my former students, in this complete silence towards me, goes in the same direction as other signs. One is the complete silence that also greeted the episode "Les étrangers" (see section 24) - a silence I have already pondered somewhat in note n° 23v.

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was not the "beneficiary" and that he acted on behalf of others.

<sup>77</sup>(\*) (May 31) Interestingly, the one and only person who ever hinted at the existence of a funeral was an African friend of mine who had passed a 3° cycle thesis with me some ten years ago (so "post-1970 student", and of modest status), with whom I have remained on friendly terms. The letter in which he implied this must have been written two or three years ago, at a time when I was not at all surprised. I did not then ask for details of his impressions, which he has only recently returned to.

On the other hand, with the exception of Berthelot, who sent me numerous separate prints, and Deligne, who sent me four (out of some fifty publications) and one from Illusie, I haven't received separate prints from any of my former students. That says a lot about the ambivalence in their relationship with me. Send prints to

p. 341 part, even though it was doubtful whether I would ever make use of it in my work<sup>78</sup> (\*), would have been the way to

p. 341 to let the person who had taught them their trade know that this trade in their hands did not remain inert, that it was alive and active. But it is also true that for at least some of them, their publications also testify to their participation in a tacit burial of which it was better not to inform the anticipated deceased, trade or no trade... . On the other hand, I have received numerous offprints from several authors working in crystalline cohomology<sup>79</sup> (\*\*), and even a good number of offprints from fellow analysts whom I hardly know by name, when their work takes up (and sometimes solves) questions I asked thirty or more years ago, when it was obvious that I would not return to the subject I had left, and that from a "utilitarian" point of view, they were wasted offprints. But these colleagues must have sensed something that my students didn't want to sense. - Of course, in the sixties, my students were the first to be served for all my publications, both my articles and the great EGA and SGA series, and every one of them (except Mrs. Sinh and maybe Saavedra) must be in possession of my complete work published between 1955 and 1970 (in the ten thousand pages I presume).

It's true that my ex-students are in good company: none of my former close friends in the mathematical "big world", including those whose work is closely related to mine or who played a role in the development of my work program in the sixties, have seen fit to conti-

continue to send me separate prints after my departure from<sup>80</sup> (\*\*\*). Only recently, among

p. 342 the fifteen or twenty friends of yesteryear (including a few students) to whom I sent the Esquisse d'un Programme

p. 342 (which, among other things, announced the resumption of intense research activity, after a hiatus of fourteen years and on research themes closely related to those we used to pursue together), only two (Malgrange and Demazure) took the trouble to send me a few lines of thanks. The few more detailed (and, what's more, warm) echoes I've received have come from young mathematicians I've only recently met, and from my old friend Nico Kuiper, who is in no way connected to the kind of things I do. He found out about the text via intermediaries, and was delighted with my unexpected "homecoming"<sup>81</sup> (\*).

<sup>78</sup>(\*) (May 31) This may even have seemed out of the question until 1976, when I made it quite clear in the early '70s that I had no intention of ever returning to mathematics. The lecture I gave at the IHES in 1976, on De Rham complexes with divided powers, showed quite clearly that I was still interested in mathematics.

<sup>79</sup>(\*\*) (May 31) These are young authors whom I don't know personally, and I presume they've followed Berthelot's example, who for them must be a figurehead. The strange thing here is that, for at least the last two years (since the Colloque de Luminy, September 6-10, 1982), Berthelot has been actively trying to bury me (see the b. de p. note of May 22 to the note "les cohéritiers. . .", n° 91) - could this be a recent turning point in his relationship with me? I don't remember receiving the reprint of the Survey article on crystalline cohomology and consorts, in which he passes my name over in silence - he must have been careful not to send it to me!

<sup>80</sup>(\*\*\*) (May 31) Of course, the psychological reasons that might have prompted them to send me some were far less strong than in the case of my students - but, one might naively think, far stronger than among my fellow analysts, or even among the many algebraic geometers whose prints I received separately, and whom I know little or nothing about personally. Clearly, after my departure from the common milieu, the fact of having been friends created or reinforced, among my former friends in the mathematical world, the automatisms of rejection that I have had occasion to observe. (On the subject of these attitudes, alluded to in passing here and there in Récoltes et Semailles, see the note "Le Fossoyeur ou la Congrégation toute entière" of May 24, n° 97.)

<sup>81</sup>(\*) (May 31) This is almost the only echo from one of my old friends (or one of my former students) in the sense of an acquiescence to my "re-entry". This is hardly surprising, given that the appearance of the deceased unseemly interrupts the normal course of a funeral ceremony... ..

(June 17) However, I recently had the pleasure of receiving a warm letter from Mumford, who says he is "thrilled" and "very excited" by the ideas sketched in the Esquisse, and who confirms that the key technical result I needed was



## 15.3.2. solidarity

**Note 85** (May 11) This story of the ill-fated SGA 5 seminar keeps running through my head. The "good reference"<sup>82</sup> (\*\*) definitely sheds new light on this story, and at the same time gives new meaning to the brilliant "SGA 4 operation".<sup>1</sup><sub>2</sub>

The more I think about it, the **bigger** the SGA 5 story seems. My first impression, when I "disembarked" just a few weeks ago (see notes n°s 68, 68'), was that a situation of debandade among the poor ex-auditors of this seminar in 65/66 had been put to good use in his own way by my friend Pierre, for his famous operation, and that no one else had anything to do with it. And as for the misfortunes of SGA 5, this was neither he nor anyone else, but rather "ut other than myself, who had not, alas, known how to enthuse my volunteer-editor listeners, nor do for them the □work they stubbornly refused to do while saying they p. 343 were about to get down to business. Then it turned out over the last few days that there was one, after all, whose enthusiasm was reawakened ten years later, to publish (without reference to the seminar) what he liked to take from it, thus creating a good reference for his own account, at a time when the other "volunteers" still hadn't decided to get going.

What's become increasingly clear to me since yesterday is that it's not just two "villains", but **every single one of my "cohomologist" students** who are directly involved in the cover-up that took place at this seminar. Unless I'm mistaken, every single one of them attended this seminar - namely (in chronological order of appearance of my "cohomologist" students): Verdier, Berthelot, Illusie, Deligne, Jouanolou. (I'm not counting Jean Giraud, who operated on quite different registers from those mostly discussed in SGA 5 or its predecessor SGA 4).

This seminar, which I did **for the benefit of my students** in the first place, and even though they sometimes asked, grace - **I consider that it wasn't crap**. Every one of them, during that year, learned a good deal about his job as a "cohomology user mathematician"! The things I was doing to them, taking up in the spread-out framework and in a much more circumstantial way ideas I'd first developed in the coherent framework - these things they couldn't find anywhere else but in that one seminar made for their benefit, given that nobody before me had ever bothered to do them - and that nobody but me even felt what there was to do, and why. (Except always Deligne, who learned it over the months in this very seminar, being quicker on the uptake than the others). It was having taken this seminar (and the previous one) and having worked on it at home as best they could, and nothing else, that meant they were now "in the know" about duality formalism, and they were **the only ones** to be. This **privilege**, it seems to me, created an **obligation** for them: to ensure that this privilege did not remain in their hands alone, and that what they had learned from me, and which has been indispensable baggage in all their subsequent work up to the present day, was made available to all, and this within reasonable and customary deadlines - of the order of a year at most, or even two at a pinch.

□We'll say, not without some reason, that it was up to me above all others to see to that. But if I accepted in good faith when students and other listeners offered to help with the writing (which, for those who took it seriously, could only be good for them) - not for the benefit of being able to twiddle my thumbs while they did a job that fell to me. I've

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for my combinatorial description of Teichmüller's tower is well and truly proven. This is the first time since 1978 that an old friend of mine has latched on to my "Anabelian" ideas, whose exceptional scope (comparable to that of pattern yoga) has been obvious to me since the very beginning. ...

(March 28, 1985) Since these lines were written, I have also received a very warm letter from I.M. Gelfand (dated Sept. 3, 1984), in response to the Esquisse.

<sup>82</sup>(\*\*) See note n° 82.

continued, with the help of Dieudonné and others (including, incidentally, Berthelot and Illusie in 1966/67) to develop' basic texts that seemed equally urgent to me, and that no one else would have done in my place or without my assistance<sup>83</sup> (\*). These texts have themselves become indispensable references, including for my "cohomology students", who are just as happy as everyone else to find them ready when they need them.

With the mastery of cohomological ideas and techniques they had acquired through their work with me and the seminars they had attended or participated in, writing this seminar through their joint efforts represented a task of derisory dimensions, if we compare it with the service that was being rendered to the famous "mathematical community", or perhaps also, later on, with an obligation of loyalty they might feel towards me. I've already said that for me (the one with the helping hand), it must have been a job of the order of a few months to write the entire seminar. By dividing the work between the five of them, with the writing experience they had each acquired in those years, and with my detailed manuscript notes at their disposal, the investment for each of them was of the order of a month or two at the very least. They were much better equipped to do this than other editors, such as Bucur, who would have liked nothing better than to entrust a task, which was clearly beyond him, to younger, more directly motivated hands.

As long as I was around (so in the three years that followed), I can see how a "leave it to me" reflex might have come into play - I was supposed to coordinate everything and deal with the "volunteers". It's likely that if I'd asked each of them to give two or three presentations

within a short space of time, it was up to me to do the same, and finally get it over with, they wouldn't have recused themselves. It was from the moment I withdrew from the mathematical world that the situation changed completely. They

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found themselves **the sole trustees of a certain inheritance**, both implicit (in the absence of a will) and very concrete. It's true that, from a practical point of view, my departure was tantamount to a **disappearance** - I was indeed "deceased", in the sense that there was no one outside them to know about the inheritance, to be able to use it and to be concerned (for better or for worse. . . ) about its fate.

If, for the seven years following my departure, this heritage remained hidden (apart from "the good reference" in 1976!), it's because **my students didn't want it to become public during that time**. All things considered, the situation doesn't seem very different to me from that of the "yoga of motives", which was thoroughly known only by Deligne (apart from myself), and which he saw fit to keep secret for his own benefit. If there is a difference at first sight, it is that in this case there is only one "beneficiary" instead of five, and that there is no common measure between the depth of what was, concealed by one, and what was jointly concealed by the five.

I certainly don't know everyone's deeper motivations - even in Deligne's case I have an apprehension that remains hazy and no doubt will remain so. But on a "practical" level, Deligne's game (with the SGA 4 "s operation - and all the rest) is quite clear. And what's also clear is that these operations couldn't have been carried out **without everyone's solidarity**. It seems to me that Jouanolou isn't too much in the picture - he doesn't strike me as a "luminary", I have the impression that he has long since left the cohomological quagmires (85<sub>1</sub>). But I can't imagine Illusie and Berthelot not having had their hands on both SGA<sub>2</sub> 4<sup>1</sup> and "la

good reference", and they can read as well as me and are no stupider than I am.

If Illusie suddenly became involved in the publication of SGA 5, at the precise moment when Verdier used, where

where Deligne needs a logistical base for his famous SGA 4<sup>1</sup> (by unpacking it) the two seminars from which this text and all his work derive), whereas Illusie had

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<sup>83</sup>(\*) Between the 1960s and 1970s, I had to work at an average rate of a thousand pages a year of texts (EGA, SGA, articles), almost all of which were to become standard references (something that was quite clear to me when I wrote them, or when I encouraged a collaborator to do so with my assistance).

had ten years to do it, it's surely no coincidence. If the closing presentation on open problems and conjectures that I gave made in 1966 "has unfortunately not been written, any more over [sic] than p. 346 his very fine introductory talk, which reviewed the formulas of Euler-Poincaré and Lefschetz in various contexts (topological, complex analytic, algebraic)", it's surely no coincidence either - but that's a funeral I don't know anything about. And it's no coincidence either that it seemed as natural to Illusie as it did to Deligne (and just worthy of a mention in passing among the "changes of detail") to amputate the seminar from one of its key presentations, which passes into SGA 4<sup>1</sup> without further ado<sup>84</sup> (\*).

I don't know what were the intentions (conscious or unconscious) of Luc Illusie, whom I like like Pierre Deligne, and who (like him) has always shown me great kindness<sup>85</sup> (\*\*). But I've noticed that, alongside Deligne, he's become the co-actor of a **shameless mystification**: that which passes off the SGA 5 mother seminar of 1965/66 (the very one in which Deligne first heard of schemas, stale cohomology, duality and other "digressions") as a kind of shapeless, vaguely ridiculous appendix to a collection of texts with the misleading name SGA 4<sup>1</sup> written eight years later, which pretends to present itself as anterior (both by the number in its title, and by the number of publication in the Notes readings, and finally by the author's unusual comment "Its existence (of SGA 4<sup>1</sup>) will soon allow us to publish SGA 5 as it stands"<sub>2</sub> - emphasis added) - and which, moreover, affects to treat with undisguised disdain the works from which this meagre collection is entirely derived.

Without these works, treated with this beautiful casualness, **none** of Deligne's great works, which are the foundation of his career, would have been possible. its well-deserved prestige, would not be written now, nor in a hundred years (and the same without doubt for Illusie and my other cohomology students). There is in the esprit of this "SGA 4 V operation a p. 347 **impudence**, of which Illusie is the guarantor (without even realizing it, no doubt), and which could only have been displayed with the tacit approval of a **consensus**. The first people involved in this consensus, apart from Deligne himself, are the very people who were my students and the main beneficiaries of a certain heritage, handed over before their very eyes to the vagaries of the jockeying for position and disdain.

And those airs of peremptory smugness, those paternal, protective airs that I was able to appreciate in my ex-student as recently as the day before yesterday in our telephone conversation<sup>86</sup> (\*), and also those more discreet airs of condescension that I was able to appreciate in my friend Pierre in the aftermath of the brilliant double operation "SGA 4<sup>1</sup> - SGA 5" (of which I was far from having the slightest suspicion at the time and for another seven years) - these airs are **not the** products of solitude, but the signs of a consensus that has **never been called into question**. These tunes tell me something not only about Verdier and Deligne, but also about all those who were my students, and before all others, about those who were (by virtue of their work themes and the tools they wielded every day) the first to be concerned.

The term "mystification", which came to me without having sought it out, opportunely reminds me of that other mystification, in which the same cynicism is on display - that of the so-called "Pervers" Colloquium. The two now seem **intimately, indissolubly linked - it's the same spirit that made both possible**. With the possible exception of Jouanolou, who is no longer so much a part of the "big world", I consider these same ex-students to be the same.

<sup>84</sup>(\*) (May 16) In fact, as I discovered the very next day (see note n° 87), there had been a veritable "massacre" of the mother seminary (or father!) SGA 5, at the hands of Verdier, Deligne and Illusie.

<sup>85</sup>(\*\*) Even after I left in 1970, Illusie showed me a lot of kindness - for a long time he sent me beautiful Christmas cards at the end of the year. I'm afraid I didn't have to write back very often to thank him and give him some sign of life - these signs of a faithful friendship came to me like messengers from a past that seemed so infinitely distant, and with which I'd lost touch.

(May 16) On the other hand, there has been no inclination on Illusie's part to continue or resume contact on a mathematical level, and even last year (when I contacted him about mathematical questions) I sensed his reluctance. In the fourteen years since I left, I have received one and only one offprint from him, dated 1979.

<sup>86</sup>(\*) For this conversation, see the note "Jokes - or 'weight complexes'" (n° 83).

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cohomologists jointly and severally responsible for this disgrace. As far as Berthelot and Illusie are concerned, nothing allows me to prejudge malice or bad faith (which cannot be doubted in the case of Verdier as in that of Deligne). But at the very least, I note a blindness, a blockage in the use of healthy faculties, the underlying reason for which, of course, escapes me. If it weren't for a deliberate intention of indifference and disdain, surely Zoghman Mebkhout, as the only person in the '70s to openly claim to be an admirer of my work, and on subjects that were close to both of them (without that they deign to notice it), would have had the benefit of the minimum "favorable prejudice" so that they would at least be aware so  $\square$  so it little of what he does, and hence realize the interest of the direction in which he had been engaged since 1974, an interest that was **obvious**! Neither of them bothered to notice anything, coming from a vague stranger who still pretends to be a Grothendieck. I don't know if they've opened it, or if they've gone through the shorter, more digestible texts that explain what it's all about - in any case, they haven't deigned to acknowledge receipt of it (nor has Deligne, who obviously sets the tone).

That didn't stop them and the other participants in the memorable Colloque<sup>87</sup> (\*) from learning about the remarkable "Riemann-Hilbert correspondence", without the slightest thought of questioning its origin or authorship, or at least (as solid mathematicians) where it was demonstrated (85 ). But here I trust that Deligne was happy to explain this demonstration, which is surely quite obvious to people like them - precisely the kind of demonstration, using Hironaka-style resolution of singularities, that they learned a long time ago from none other than me (85<sub>2</sub> ). Riemann-Hilbert, Hironaka abracadabra - that was it!

Clearly, like Verdier and Deligne, they've completely forgotten what **mathematical creation** is all about: a vision that gradually unravels over months and years, bringing to light the "obvious" thing that no-one had seen, taking shape in an "obvious" statement that no-one had thought of (even though, in this case, Deligne had been trying in vain for a whole year. . . ). ) - and which anyone can then demonstrate in five minutes, using the ready-made techniques he had the advantage of learning sitting on the benches of a distant seminar he doesn't deign (or hasn't kept) to remember. ...

If I've spoken bluntly of Berthelot and Illusie, it's not because I particularly want to smear them (after an initial settling of scores with their two friends). I know they're no "worse" or dumber than most of their dear colleagues or me, and that the lack of flair and sound judgment that

I see in them in this instance (and sometimes also, that of the necessary respect for others. . . ) is by no means inveterate, but the effect of a **choice**. This choice undoubtedly offered **them**  $\square$  **returns** that pleased them - and may have done so.

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Perhaps this other "return" that comes with my reflection will be unwelcome to one or the other. If it were, it would simply be that he's still reproducing the same choice, which is also that of operating on a tiny part of his faculties, even if it means mistaking bladders for lanterns and vice versa, and hopelessly confusing empty nuts (from the boyfriend) and full nuts (from a vague stranger). To each his own! ( $\Rightarrow$  86, 87)

**Note** 85<sub>1</sub> Jouanolou is the only one of my students, along with Verdier, who did not publish his thesis. This seems to me to be a sign of disaffection with the foundational work he had developed, namely that of  $\chi$  - *adique* cohomology from the point of view of derived categories. Since most of his work on this theme took place **after** my departure, i.e. at a time when my students, Deligne and Verdier in particular, were in the process of completing their studies, I'd like to take this opportunity to thank him.

<sup>87</sup>(\*) (June 12) I have since learned that neither of them took part in this Colloque (Luminy, June 1981). See, however, the note "La mystification", n° 85'.

head, had signaled a general disaffection with the ideas I had introduced into homological algebra, particularly that of the derived category, the context hardly encouraged Jouanolou to identify with his work and do him the (well-deserved) honor of publishing it. As these same Deligne and Verdier, in the wake of the work of Zoghman Mebkhout (aka Elève Inconnu (de Verdier) aka élève posthume (de Grothendieck)), have come to discover (with great fanfare and mutual publicity) the importance of derived categories (see notes n°s 75,77,81), Jouanolou's scorned thesis has, since the Colloque Pervers, regained all its topicality ; a relevance it would never have ceased to have, had the development of the cohomological theory of schemas continued as normal after my departure in 1970. A striking detail that illustrates a certain drastic "turn" in Deligne's options after my departure: it was Deligne himself (who had clearly understood the importance of developing the formalism of  $\chi$ -adic cohomology in the context of triangulated categories) who provided Jouanolou with a key technical idea for a formal definition of the triangulated  $\chi$ -adic categories he was studying, an idea that is developed in the thesis. (See my 1969 "Report" on Deligne's work, par.8.)

(May 30) See also, on the subject of Jouanolou's work, the note "Les cohéritiers. . . ", n° 91.

**Note 85<sub>2</sub>** Significantly, it was in this same SGA 5 seminar that everyone learned this demonstration principle, used to prove the biduality theorem in cohomology.

(in cases where the resolution of singularities is available), that the  $\square$ -finitude theorems for the  $Rf_*$  without any cleanliness assumption on  $f$ , and similarly for  $RHom$ ,  $Lf^!$ . (These finiteness theorems were also omitted from the published version of SGA 5, to be appended to SGA 4 $\frac{1}{2}$ , without Illusie even seeing fit to point this out in his introduction - I only realize this as I write these lines!) Zoghman, who didn't have the advantage of attending the seminar (he got the "right reference" instead), learned the procedure in another place where I had used it (for De Rham's theorem for smooth schemes on  $\mathbb{C}$ ).

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He could also learn it from "the good reference", where my demonstrations are copied into the analytical framework, to establish what my students and listeners at SGA 5 have since liked to call the "Verdier duality" (which was known to me before I had the pleasure of making his acquaintance). It all adds up! **The same demonstration** (copied from me at the same time as the statement) is used by Verdier as a title of authorship for a duality he learned nowhere else than in that dislocated and scorned SGA 5 seminar - and it is used **against** Mebkhout, becoming (by its very "obviousness") a (tacit) pretext and means for shamelessly robbing him of the credit for an important discovery.

(May 30) It seems to me that the first time I used singularity resolution à la Hironaka, and understood the extraordinary power of resolution as a demonstration tool, was for a "three spoonfuls" demonstration of the Grauert-Remmert theorem, describing a complex analytic structure on certain finite coverings of a complex analytic space, and the analogous statement in the case of finite-type schemes on  $\mathbb{C}$ . (It's not impossible that the principle was blown to me, on this very occasion, by Serre). This last result is the main ingredient in the proof of the comparison theorem between étale cohomology and ordinary cohomology (the rest being reduced to unscrewing, thanks to the formalism of  $Rf_!$ , plus a bit of solving to go from  $Rf_!$  to  $Rf_*$  . . . )

### 15.3.3. The mystification

**Note! 85'** (June 3) In fact, I'm learning that they didn't have to wonder about this paternity, since both Berthelot and Illusie learned the theorem from the mouth of Mebkhout, the first...

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in February 1982, the second as early as 1979 (the year Mebkhout defended his thesis). Although neither of them took part in the Colloquium in question, they are nonetheless in solidarity with the mystification that took place at the Colloquium.

place at this Colloquium, because it is impossible that they were unaware of the cover-up that took place of Mebkhout's authorship of the *theoreme du bon Dieu*. I can imagine that, along with all the other Colloque participants, they were the first to be fooled by the collective mystification organized by their friends Verdier and Deligne (a mystification in which four of my five cohomology students appear to be involved). As far as Illusie is concerned, at least, I was struck, during a telephone conversation with him after Mebkhout's visit to my home last summer, by how little he obviously thought of him - he was quite astonished (almost pained on the part of his old master, in whom he would surely have expected better judgment... ) to see me give Mebkhout a leading role in restarting the cohomological theory of algebraic varieties. Consensus of considerable force had decided to rank Mebkhout among the vague unknowns, and my friend Illusie blithely lived with this triple contradiction, without asking himself any questions: the leading role played by the theorem of the good God and the philosophy that goes with it; the evasion surrounding the authorship of these things (an evasion in which he himself participates in the company of many); and the low esteem he has for the format and role of Mebkhout (who he knows perfectly well is the unnamed author of these things, which have renewed a field of mathematics in which he, Illusie, is a figure of eminence).

I find here again the complete blockage of common sense and sound judgment, even in something as seemingly impersonal as judgment on scientific matters, a blockage to which I have had occasion to allude more than once already, and which each time again baffles me. And this contradiction I see here in Illusie's (and surely many others') relationship with Mebkhout, my "posthumous pupil", is surely no more than one of the many effects of a more crucial contradiction to be found in his relationship with me. It is this contradiction, in him more particularly and in my other pupils too, that becomes increasingly clear in the reflection pursued in the notes of the present procession to the Funeral, formed by my pupils of yesteryear...

#### 15.3.4. The deceased

**Note 86** ( May 11) As is so often the case, it was with some reluctance that I set about this task.

new reflection, on the theme "SGA 5 - SGA 4<sup>1</sup> - Perversity", which might have seemed to have been examined and

re-examined over and over again: "It's going to make a deplorable impression on a reader who's probably had enough of it ever since he heard about it; It doesn't look elegant at all to go into details again, SGA 5 this SGA 4<sup>1</sup> that, it's and doesn't deserve any more toast... .".

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□ I'm glad I didn't let myself be intimidated by that well-known refrain, which would like to prevent myself from getting to the bottom of something (at least as far as I'm able to go at the time), on the pretext that it's decidedly "not worth it", that there's nothing to do but let it run its course... . If there have been times when I've discovered things that I consider useful and important, it's always been in moments when I've known not to listen to what presents itself as the voice of "reason", or even "decency", and follow this indecent urge inside me to go and see even what is supposed to be "uninteresting" or of poor appearance, or even messed up or indecent. I can't remember a single time in my life when I've regretted having looked at something a little more closely, against inveterate reflexes that would have prevented me from doing so. These inhibition reflexes were even stronger in *Récoltes et Semailles* than on other occasions, because this reflection is destined to be made public, which immediately imposes certain

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finally, that these constraints never prevented me from either tackling something I wanted to tackle, or going as far into it as I felt I wanted to. In the cases that may at one time have seemed like borderline cases, I forged ahead with the assurance that, should the need arise, I would always have the resource of not including in Harvest and Sowing anything that would "escape" my indiscreet reflection. These "borderline cases" arose exclusively when I hesitated to involve others, and never when it came to involving myself. But even in the first case, as it happens (and this came as a surprise to me), I never had to make use of this "resource": the text of Récoltes et Semailles represents the complete version of my reflection - at least of the part of this reflection that found its way into writing to express itself.

I feel that with the brief reflection of the previous note<sup>88</sup> (\*), the situation has become considerably clearer. I mean that a certain essential aspect of a situation which had been confusing to pleasure, and which I have just evoked by the triple name of a "theme" (SGA 5 -  $\text{SGA } 4^1$  - Perversity), has appeared to me in full light: that of a "solidarity", a "connivance" which had only been confusedly perceived until then. This doesn't mean that I believe I've fathomed and understood all the ins and outs of a "solidarity", a "connivance" that had only been dimly perceived until then.

complex situation, directly and particularly obviously involving at least seven people: Zoghman Mebkhou (acting in a sense as a "revealer" of a certain situation), my five ex-students cohomologists, and myself. I don't even flatter myself to have perceived all the springs and motivations that have been at play in my own person, in relation to the "SGA 5 etc. ... .", in the nearly twenty years since that "unfortunate seminar" took place! But I feel in a much better position than I was yesterday (or even just this morning), to understand and situate the echoes that I hope will reach me on this subject from at least one or other of the main parties involved.

The main question that arises for me (it seems to me that it was already present at another stage of reflection, and now reappears with new vigour) is (it seems to me) this: is what happened with this Burial by my students, (more or less) in their entirety, something completely **atypical**, linked to certain particularities of my person and my singular destiny (such as my departure from the mathematical scene nearly fifteen years ago, the circumstances surrounding it, etc. ) ? Or is it, on the contrary, something "quite natural", due to a simple combination of circumstances - following the principle that "opportunity makes the thief"? I hesitate to think so, though I can't at the moment discern, or even glimpse, what particular aspect of myself has had the virtue of creating such perfect and unanimous **agreement** among my former students, to bury both the "master", and those who claim to be his followers or whose work clearly bears his mark (without, however, being "theirs"). Is it this sort of "aura" of Father that surrounds me, and which I've had occasion to mention? Or is it the challenge posed to each of them by the mere fact of my departure? At the moment, I wouldn't be able to say, for lack of eyes that can see... . Perhaps the coming months will teach me something on this subject<sup>89</sup> (\*).

More than once in the last three weeks, I've thought of this other strange "coincidence": it's that the discovery of the Burial "in all its splendour" (with the four-stroke LN 900 -  $\text{SGA } 4^1$  -  $\text{SGA } 5$  - Colloque Pervers, then back to  $\text{SGA } 5$  and  $\text{SGA } 4^1$ ) - that this discovery came at a time when I had just completed an in-depth reflection on my past as a mathematician and my relationship with my students. It was a time, therefore, when I had just come "to terms with myself" on the subject of my students.

of this past, to the best of my ability, and as far as the facts then known to me allowed, as they were often hazy memories. Or to put it another way: it was lep

rendered by

<sup>88</sup>(\*) This refers to the note "La solidarité" n° 85, dated the same day.

<sup>89</sup>(\*) (May 30) For a reflection along these lines, see the note "Le Fossoyeur - ou la Congrégation toute entière", n° 97.



the exact moment when I was finally **ready** to learn and profit from it.

Chance" did things so well that there wasn't even a break in the meditation. The reflection that had begun with this short retrospective on the fate of the most important notions (in my opinion) that I had introduced<sup>90</sup> (\*) (a reflection that remained in a certain vagueness, where only a certain basic tone emerged insistently... ) - this reflection continued quite naturally on Thursday April 19. It was, it's true, still in the throes of the emotion aroused by that impression of "impudence" (to use the term from earlier, which also aptly describes something I felt at the time), on reading the "memorable volume" LN 900.

In this new departure from "the same" thinking, the main driving force was "the boss" - my self-esteem and sense of decency were affected, and by writing down my emotions I freed myself from them to a certain extent. It was indeed "me", "the boss", who visibly led the dance in the ten days that followed - days marked by the absence of smiles and laughter, by unfailing seriousness. I had to go through this ten-day detour before the reflection returned to the center it had left - to myself. I still remember the relief of that return - like coming out of a tunnel when daylight appears again! It was then that I found myself laughing and smiling again, as if we'd never left each other. It was April 29th. The next day, the 30th, the last day of the month, I was happy to put the finishing touches to this ultimate stage of reflection.

It was also the moment, surely, when I was ready to receive the next "package", this time sent by my friend Zoghman - the "Colloque" package received the day after tomorrow. Today is the tenth day I've been working on assimilating the substance of this package. But at this stage, while I've been gnawing at the bit to get to the bottom of this rebound that just kept on rebounding, the smile hasn't faded for a single day. And today, I truly believe (for the thirtieth time, it's true!), is finally the day of closure.

Five days ago I'd already had that same feeling that I'd reached the end of my rope, that there was nothing left but work to be done.

of stewardship: adding a few footnotes here and there, retyping pages too overloaded with ratures (each time a sign of a thought that had remained somewhat confused, and which needs to be put into This seemingly mechanical work, from which the text always emerges with a new face. ... ). . . This was when I had just written what is now the note "Mes amis" (n° 79), which spontaneously flowed into "final chords". However, I ended up separating these chords from the beginning of the note. In fact, it turned out that this famous stewardship work had broken down: the "footnotes", typed without line spacing, became real notes (**not** footnotes) of nice dimensions, which had to be retyped with line spacing, and then tried as best I could to fit in here and there. It took days before I realized that another procession, after the one called "The Colloquium", was forming to join the procession - and that the last of the processions would not (as I had decided in my head) be the said Colloquium, but would be led by **the Student**. And just today, as the first procession, reduced to a single note, was enriched by a second ("A feeling of injustice and powerlessness"), I also knew who would lead it: it was "**L'élève posthume**". So the procession, opened by a Pupil (posthumous and lower-case, as befits his humble state) and closed by yet another Pupil (by no means humble this time), seems to me to be complete at last!

It's also time, it seems to me, after a first "false arrival", to return to the chords of a final De Profundis, which are more appropriate today than they were five days ago. Here they are, as I wrote them down then, and which also express my feelings at the moment.

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<sup>90</sup>(\*) See notes "My orphans" and "Refusal of an inheritance - or the price of a contradiction" of March 31 (n° s 46,47).

(May 31) Finally, it was another "false arrival" - the "final agreements" were premature this time too! Twenty days went by, during which the "housekeeping work" continually broke down into rethinking this and that aspect that had been neglected. Six more notes joined the "L' Elève" procession, which was supposed to close the parade. The Fourgon Funèbre appeared in the wake of the Elève, carrying four coffins accompanied by the Fossoyeur. He was definitely lacking to give body and meaning to a funeral convoy that didn't seem to be conveying anyone.

Having become cautious through experience, I'm waiting for events to unfold and won't take any chances for the time being.

to predict whether the procession is finally complete, or whether a forgotten procession will sneak in again at the last minute, so as not to miss the ultimate Ceremony<sup>91</sup> (\*).

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### 15.3.5. The massacre

**Note** 87 (May 12)<sup>92</sup> (\*\*) For the edification of the somewhat cohomologist reader, and above all for my own, I would like to review the details of this plundering of a splendid seminary, in the hands of two of my former cohomologist students and under the benevolent eye of the others<sup>93</sup> (\*\*\*) - of the same seminary where they learned, twelve years before anyone else and from the hand of the workman himself, the basics and finesses of the trade that made their reputation.

Two of my oral presentations have never been made available to the public in any form. One is the closing lecture on open problems and conjectures, which "unfortunately was not written up", given how little - and indeed, the author of the introduction to the murder-edition deemed it unnecessary even to mention **what** open problems and conjectures it was. And why should he have taken the trouble, when they were merely problems (which everyone is free to pose as they please!) and conjectures (not even demonstrated!) (87<sub>1</sub>). The other was the lecture that opened the seminar, placing it right from the start in a broader context (topological, complex analytic, algebraic) and reviewing formulas of the Euler-Poincaré, Lefschetz, Nielsen-Wecken type, some of which constituted one of the seminar's main applications. The ". . . any more than. . . ." with which the author of the introduction follows up to signal, in the nick of time, the disappearance of this presentation, says a lot about the **casual attitude** that was clearly self-evident at the time, even though the author of the seminar had been out of circulation for seven years.

There's a whole series of talks I gave on the formalism of homology and cohomology classes. logie associated with a cycle (regular ambient pattern in the cohomological case)<sup>94</sup> (\*\*\*\*). They have been equally divided: cohomology for Deligne, homology for Verdier - who nevertheless overflows a little on cohomology, even if it means making the small reference to Deligne with the famous "complexes". p. 357 weight"<sup>95</sup> (\*). (Not to mention the finiteness theorem for RHom and the biduality theorem, copied verbatim from the seminar - the lion's share of which will go to Deligne, which is to be expected. ... ) The author of the introduction does not see fit to mention the homology lectures alone. Indeed, there was no need, since the previous year his friend Verdier had taken on the task of providing the missing "good reference" (without alluding to a seminar, or to me).

<sup>91</sup>(\*) (June 12) Caution was the order of the day, as a new "Mes élèves" procession separated from the one originally called "L'Elève", which became "L'Elève - alias le Patron".

<sup>92</sup>(\*\*) This note follows on from the previous day's reflection on "Solidarity" (n° 85).

<sup>93</sup>(\*\*\*) Further reflection reveals that one of these "others" lent a hand efficiently to this operation on behalf of others.

<sup>94</sup>(\*\*\*\*) For details, see note no. 82 "Good references".

<sup>95</sup>(\*) See note (83) "The joke - or weight-complexes".

There were oral presentations on finiteness theorems for the operations  $Rf^!\wedge$  ( $f$  not proper), and as a corollary, for the operations  $RHom_*$  ; and  $Lf^!$  . The key theorem was proved using a Hironaka-style singularity resolution technique (valid only in cases where the resolution is available).

These arguments, which I used, have come into common use since the seminar (see note (85<sub>2</sub>)). Deligne has managed to prove these finitude theorems, as well as the biduality theorem, under other, more helpful hypotheses, which have already been verified in most applications. It might have been expected that he would ask to include these refinements in the seminar where he had the privilege of learning étale cohomology, and the ideas and techniques at the basis of all his subsequent work. But this circumstance is used as a "reason" for amputating this part of the seminar. As for the biduality theorem, under Illusie's pen (and within the framework of the diagrams) it became "Deligne's biduality theorem" (introduction to Lecture I). This was only fair, since in the analytical case, Verdier had already claimed it as his own the previous year (without even having to go to the trouble of finding another demonstration).

Then there's Illusie's paper developing a "generic Kûnneth formula". No one before had thought of developing this kind of statement, inspired by the intuition that "generically" i.e. in the vicinity of the generic point of the basis, a relative scheme behaves like a "locally trivial fiber" in the topological context. In an elegant demonstration similar to the one mentioned above, Deligne manages to eliminate the singularity-resolving hypothesis I had made. It's awarded - presentation deleted and "replaced" by a reference to a presentation by the same Illusie in the so-called "earlier" seminar.  
SGA 4<sup>1</sup>.

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□ There is a series of lectures on the formalism of non-commutative traces, developed as a means of explicating the local terms of the Lefschetz-Verdier formula in cases that had never been treated. These lectures were eventually written, it seems, by Bucur, whose manuscript "got lost in a providential move" - it's turning into a vaudeville!<sup>96</sup> (\*) In the introduction to SGA 5, written by Illusie, these lectures become "Grothendieck's theory of **commutative** traces, generalizing [brilliantly] Stallings'" (which were non-commutative!). The slip of the tongue<sup>97</sup> (\*\*) can only be due to a badly (or too well. . . ) inspired secretary, who must have been involved with my friend Ionel Bucur's movers. (The word "brilliantly" is an interpolation of my pen, to better render the thought infallibly suggested by this slip of the tongue, also providential ).

I can't complain, since Illusie has taken on the job of redoing the work (and even, he tells us, a "more sophisticated" version, given that it's put in the Beamtique sauce - I seem to remember, though, Illusie, that you made more "sophisticated" innovations than this in my day... ). It must have taken a long time even, if I remember that I spent weeks putting the machine together; my manuscript also got lost in the same providential move, and God knows if one of the dear listeners, overwhelmed by my oral faconde, was at least able to take comprehensible notes... .

Remarkably, and this is something I hadn't noticed before, he doesn't insert this talk in the place in Lecture XI where it was intended (which no doubt also corresponds to the place it had in the oral seminar), preferring to leave a gaping hole there and make his talk an apocryphal one, called "Calculs de termes locaux" ("Calculations of local terms"). The title does, however, seem to correspond to what I seem to remember him doing in the oral seminar.

<sup>96</sup>(\*) It was this circumstance, no doubt, that inspired Deligne's unexpectedly brilliant criticism of SGA 5, in which the local terms of Lefschetz Verdier's formula (which "remained conjectural", remember!!!) were not even calculated! (See note "la table rase", n° 67, about the absurdity of this criticism, which for an informed reader is similar to that of Verdier's famous "weight complex" the previous year (see note n° 83). So it was Verdier who became the schoolmaster! )

<sup>97</sup>(\*\*) This is the slip of the tongue attributing to me the authorship of a theory of "commutative" traces (for which I had not been expected) instead of "non-commutative" ones. That it survived into the published edition is all the more remarkable given that Illusie was perhaps the most meticulous of my students, down to the last detail.

strange. But right from line 1 of his introduction to this talk, the author is quick to <sup>□</sup>detrompt us: "Cetp . 359 exposé, written in January 1977, **does not correspond to any oral presentation in the seminar**". And he continues with Lefschetz-Verdier formulas (that name rings a bell, though, and I thought I'd actually developed a theory of non-commutative traces, in order to calculate "local terms" in certain cases. . . ), then on a formula by Langlands and on a demonstration by Artin-Verdier in 1967 (this was a year after the final agreements of the oral seminar, which must have influenced these authors, at least one if not both of whom followed him). Towards the end of the page, we learn as if in passing, contrary to what was announced at the beginning, that there is also a "second part of this talk, of a much more technical nature" (I've read this language somewhere. . . ) which is (admire the nuance) "**inspired by the method used by Grothendieck** to establish the Lefschetz formula for certain cohomological correspondences on curves", with a reference to Lecture XII of the same seminar and above all to the indispensable SGA 4<sup>1</sup>; Obviously, there was no reason, for so little, to include this lecture in place of the gaping hole - the "more sophisticated version" of earlier will have done things right. It was even nice of Illusie and Deligne to cite me as a source of "inspiration", when the example of their friend Verdier the previous year had clearly shown that there was absolutely no need for such scruples.

I return to Illusie's introduction to the volume that goes by the name of SGA 5. In it, we learn once again - as Deligne had already announced in his introduction to SGA 4<sup>1</sup> - that it was indeed **thanks to his friend** that the seminar has finally been published:

"I would like to thank P. Deligne for having convinced me to write, in a new version of Lecture III, a demonstration of the Lefschetz-Verdier formula, **thus removing one of the obstacles to the publication of this seminar**".

Once again, we're in the middle of a farce - repeated as is by the docile Illusie in the introduction to SGA 4<sup>1</sup> ! If the seminar remained unpublished for more than ten years, it's because no one (until Deligne saved the day in 1977) had yet considered the possibility of publishing it.

good idea to write a demonstration of the so-called (and rightly so) "Lefschetz-Verdier formula", of which none other than <sup>□</sup> his inseparable friend and my ex-student Verdier himself has proudly borne the paternity **since aup** . 360 **minus** 1964 (87<sub>2</sub>), i.e. for at least two years by the time my seminar ended, and was just waiting to be made available to everyone!

Finally, as another and last (?) mutilation of the seminar, there was the disappearance of the fine talk Serre had given on the "(Serre-) Swan module" - a talk entitled "Introduction à la théorie de Brauer". It's fortunate that Serre, seeing the turn events were taking, had the good sense to include his talk in his book "Représentations linéaires des groupes finis" (Hermann, 1971), and make it available to the mathematical public.(87)<sub>3</sub>

This time, I think, I've come full circle. The picture of the fate of a seminar in which I had put the best of myself (88)<sup>98</sup> (\*), and which I find twenty years later unrecognizable, butchered by the very people who had been its exclusive beneficiaries - or at least by three of them, and with the assent of all the other participants.

I don't regret having taken the trouble, once again, to follow through on what had gradually come to my attention. This "return of things"<sup>99</sup> (\*\*) that I noticed, at the end of a long retros-

<sup>98</sup>(\*) For the meaning of this expression "of the best of myself", see the following notes "La dépouille. . . ", ". . . and the body", n° 88, 89. The first of these situates the SGA 5 seminar, with SGA 4 inseparable from it, as the masterpiece of the part of my work "entirely completed".

<sup>99</sup>(\*\*) See note of this name (n° 73) dated April 30.

pective about my relationship with one of my former students, sensing even then that he wasn't the only one to "bury me with gusto" - I've only now become aware of his breath, his "smell" (to use an expression that then appeared in one of my dreams) - the breath of **violence**. This breath is concealed and revealed at the same time by the speech<sup>100</sup> (\*\*\*) (seemingly detached and impassive) presenting a highly technical substance. What this violence is aimed at, through a "corpse" delivered at mercy,

is the very person of the one who was the "master", the "Father" - at a time when the "pupils" have long since taken his envied place, without encountering any resistance; and that □ also long ago they have elected from among themselves the new "Father", called to replace the old and reign over them.

I feel this breath, and yet it remains for me a foreign thing, misunderstood. To "understand" it, this breath would have to live in me, or have lived in me. But four years ago, for the first time, I felt and measured the significance of something in my life that I had never thought about, that had always seemed self-evident: that my identification with my father as a child **was not** marked by conflict - that at no time in my childhood **did I fear or envy my father**, while at the same time devoting unreserved love to him. This relationship, perhaps the most profound that has marked my life (without my even realizing it before this meditation four years ago), which in my childhood was like a relationship with an other self both strong and benevolent - this relationship was not marked by division and conflict. If, through all my often-torn life, the knowledge of the strength that lies within me has remained alive; and if, in my life by no means free of fear, I have not known fear either of a person or of an event - it is to this humble circumstance that I owe it, ignored until well into my fifties. This circumstance has been a priceless privilege, for it is the intimate knowledge of the creative force within one's own person that **is** also that force, which enables it to express itself freely according to its nature, through creation - through a creative life.

And this privilege, which has exempted me from one of the deepest marks of conflict, is at the moment also like a hindrance, like a "void" in my experience of life. A void that's hard to fill, where many others have a rich web of emotions, images and associations, offering them the path (provided they're curious enough to take it) to a profound understanding of others as well as of themselves, in situations that I manage (by dint of repetition and cross-checking) to apprehend as best I can, but in the face of which I remain like a stranger - with the desire for knowledge within me still hungry.

**Note 87<sub>1</sub>** (87<sub>1</sub>) (May 31) This closing presentation, surely one of the most interesting and substantial with the opening talk, was obviously not lost on everyone, as I see from reading Mac Pherson's paper "Chern classes for singular algebraic varieties" (Chern classes □ for singular algebraic varieties, *Annals of Math.* (2) 100, 1974, pp. 423-432) (received April 1973)-In this paper, under the name of "Deligne-Grothendieck conjecture", I repeat one of the main conjectures I had introduced in this paper in the schematic framework. It is taken up by Mac Pherson in the transcendental framework of algebraic varieties over the field of complexes, the Chow ring being replaced by the homology group. Deligne had learned this conjecture<sup>101</sup> (\*) in my talk in 1966, the same year he had appeared in the seminar where he began to familiarize himself with the language of diagrams and cohomological techniques (see the note "L'être à part" n° 67)-It's nice of you to have done me the honor of including me.

<sup>100</sup>(\*\*\*) These are mainly the introductory texts accompanying SGA 5 (written by Illusie) and SGA 4. <sup>1</sup>  
(written by Deligne).

<sup>101</sup>(\*) (June 6) In a slightly different form, see the rest of today's note.

(March 1985) For further details, given by Deligne himself, see the note "Les points sur les i", n° 164 (II 1).

in the name of conjecture - a few years later it would no longer have been appropriate... .

(June 6) I'd like to take this opportunity to explain the conjecture I'd put forward in the seminar in the schematic framework, while surely pointing out the obvious variant in the complex analytic (or even rigid-analytic) framework. I conceived it as a "Riemann-Roch" theorem, but with discrete coefficients instead of coherent coefficients. (Zoghman Mebkhout told me, incidentally, that his view of D-Modules should make it possible to consider the two Riemann-Roch theorems as contained in a single crystalline Riemann-Roch theorem, which would thus represent in zero characteristic the natural synthesis of the two Riemann-Roch theorems I introduced into mathematics, one in 1957, the other in 1966). We fix a ring of coefficients  $\Lambda$  (not necessarily commutative, but noetherian for simplicity and moreover of prime torsion to the characteristics of the schemes under consideration, for the purposes of stable cohomology... ). For a scheme  $X$  we denote by

$$K(X, \Lambda)$$

the Grothendieck group formed by constructible étale bundles of  $\Lambda$ -modules. Using the functors  $Rf_!$ , this group depends functorially on  $X$ , for  $X$  noetherian and morphisms of schemes that are separated and of finite type. For regular  $X$ , I postulated the existence of a homomorphism of groups canonical, playing the role of the "Chern character" in the consistent RR theorem,

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$$ch_X : K(X, \Lambda) \rightarrow A(X) \otimes_{\mathbb{Z}} K(\Lambda), \quad (15.1)$$

where  $A(X)$  is the Chow ring of  $X$  and  $K(\Lambda)$  the Grothendieck group formed with  $\Lambda$ -modules of finite type. This homomorphism was to be determined solely by the validity of the "discrete Riemann-Roch formula", for a **proper** morphism  $f : X \rightarrow Y$  of regular schemes, which formula is written as the consistent Riemann-Roch formula, with Todd's "multiplier" replaced by the total relative Chern class :

$$ch_Y(f_!(x)) = f_*(ch_X(x) c(f)), \quad (15.2)$$

where  $c(f) \in A(X)$  is the total Chern class of  $f$ . It's not hard to see that in a context where we have the resolution of singularities in Hironaka's strong form, RR's formula does indeed determine the  $ch_X$  uniquely.

Of course, we assume that we're in a context where the Chow ring is defined (I'm not aware of anyone having even attempted to write a theory of Chow rings for regular schemes of finite type over a body). Alternatively, we can also work in the graduated ring associated with the usual "Grothendieck" ring  $K^0(X)$  in the coherent context, filtered in the usual way (see SGA 6). Alternatively, we can replace  $A(X)$  by the even  $l$ -adic cohomology ring, the direct sum of  $H^{2i}(X, \mathbb{Z}_l(i))$ . This has the disadvantage of introducing an artificial parameter  $l$ , and giving formulas that are less "purely numerical" fines, while the Chow ring has the charm of having a continuous structure, destroyed by switching to cohomology.

Already in the case where  $X$  is a smooth algebraic curve over an algebraically closed field, the calculation of  $ch_X$  involves delicate local invariants of the Artin-Serre-Swan type. In other words, the general conjecture is a profound one, the pursuit of which is linked to an understanding of the higher-dimensional analogues of these invariants.

Remark. Designating in the same way by  $K(X, \Lambda)$  "the Grothendieck ring" formed with the construc-

of finite tor-dimensional  $\Lambda$ -spreads (which ring operates on  $K(X, \Lambda)$  when  $\Lambda$  is commutative. . . ), we must likewise have a homomorphism

$$ch_X : K(X, \Lambda) \rightarrow A(X) \otimes_{\Lambda} K(\Lambda)$$

again giving rise (mutatis mutandis) to the same Riemann-Roch (RR) formula.

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□ Let  $cons(X)$  now be the ring of constructible integer functions on  $X$ . We define less tautological canonical homomorphisms

$$K(X, \Lambda) \rightarrow Cons(X) \otimes_{\Lambda} K(\Lambda),$$

$$K^*(X, \Lambda) \rightarrow Cons(X) \otimes_{\Lambda} K^*(\Lambda),$$

If we now restrict ourselves to schemes **of zero characteristic**, then (by using Euler-Poincaré characteristics with proper supports) we see that the group  $Cons(X)$  is a covariant functor with respect to finite-type morphisms of noetherian schemes (in addition to being contravariant as a ring functor, which is independent of characteristics), and the preceding tautological morphisms are functorial. (This corresponds to the "well-known" fact, which I believe was not proved in the SGA 5 oral seminar, that in **zero characteristic**, for a locally constant bundle of  $\Lambda$ -modules  $F$  on an algebraic scheme  $X$ , its image by

$$f_! : K(X, \Lambda) \rightarrow K(e, \Lambda) \rightarrow K(\Lambda)$$

is equal to  $d\chi(X)$ , where  $d$  is the rank of  $F$ ,  $e = Spec(k)$ ,  $k$  the base field assumed to be algebraically closed. . . ). This immediately suggests that the Chern homomorphisms (1. ) and (1' ) must be derivable from the tautological homomorphisms (2. ), (2' ) by composing with a "universal" Chern homomorphism (independent of any ring of coefficients  $\Lambda$ )

$$ch_X : Cons(X) \rightarrow A(X),$$

so that the two " $\Lambda$ -coefficient" versions of the RR formula appear to be formally contained in an RR formula at the level of constructible functions, which is always written in the same form.

When working with schemes on a fixed basic body (again, of any characteristic), or more generally on a fixed **regular** basic scheme  $S$  (for example  $S = Spec(\mathbb{Z})$ ), the form of the Riemann-Roch formula most in line with the usual writing (in the coherent framework familiar since 1957) is obtained by introducing the products

$$ch_X(x)c(X/S) = c_{X/S}(x)$$

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(where  $X$  is in a  $K(X, \Lambda)$  or  $K^*(X, \Lambda)$  indifferently), which we might call **the Chern class of  $X$  relative to the basis  $S$** . When  $x$  is the unit element of  $K^*(X, \Lambda)$  i.e. the class of the constant bundle of value  $\Lambda$ , we find the image of the total relative Chern class of  $X$  with respect to  $S$ , by 1 "canonical homomorphism of  $A(X)$  into  $A(X) \otimes K^*(\Lambda)$ . This being the case, RR 's formula is equivalent to the fact that the formation of these relative Chern classes

$$c_{X/S} : K^*(X, \Lambda) \rightarrow A(X) \otimes K^*(\Lambda),$$

for a regular variable scheme  $X$  over  $S$  (of finite type over  $S$ ), with  $S$  fixed, is functorial by

with respect to eigenmorphisms, and similarly for the variant (5'). In null characteristic, this reduces to the functoriality (for eigenmorphisms) of the corresponding application

$$c_{X/S} : \text{Cons}(X) \rightarrow A(X) .$$

It is in this form of the existence and uniqueness of an absolute "Chern class" application (6), in the case where  $S = \text{Spec}(\mathbb{C})$ , that the conjecture in Mac Pherson's work is presented, the relevant conditions (here as in the general zero characteristic case) being a) the functoriality of (6) for proper morphisms and b) we have  $c_{X/S}(1) = c(X/S)$  (in this case, the total "absolute" Chern class). Compared with my initial conjecture, however, the form presented and proved by Mac Pherson differs in two ways. One is a "minus", in that it is placed, not in the Chow ring, but in the whole cohomology ring, or more precisely the whole homology group, defined by transcendental means. The other is a "plus" - and it is here perhaps that Deligne has made a contribution to my initial conjecture (unless this contribution is due to Mac Pherson himself<sup>102</sup> (\*)). It's that for the existence and uniqueness of an application (6), we don't need to restrict ourselves to regular schemes  $X$ , provided we replace  $A(X)$  by the entire homology group. The same is likely to be true in the general case, where we denote by  $A(X)$  (or better, by  $A_*(X)$ ) the **Chow group** (which is no longer a ring in general) of the noetherian scheme  $X$ . Or to put it another way: while the heuristic definition of *ch-invariants* <sub>$X$</sub>  ( $x$ ) (for  $x$  in  $K_*(X, A)$  or  $K^*(X, A)$ ) makes essential use of the assumption that the ambient scheme is regular, as soon as we multiply it by the "multiplier"  $c(X/S)$  (when the scheme  $X$  is of finite type over a fixed regular scheme  $S$ ), the product obtained (4) seems to retain a meaning without any assumption of regularity<sup>about</sup>□, as an element of a tensor product

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$$A_*(X) \otimes K_*(\Lambda) \text{ or } A^*(X) \otimes K^*(\Lambda) ,$$

where  $A_*(X)$  denotes the Chow group of  $X$ . The spirit of Mac Pherson's demonstration (which does not use singularity resolution) would suggest the possibility of an explicit "computational" construction of the homomorphism (5'), by "making do" with the singularities of  $X$  as they are, as well as with the singularities of the coefficient bundle  $F$  (whose class is  $x$ ), to "collect" a cycle on  $X$  with coefficients in  $K_*(\Lambda)$ . This would also be in the spirit of the ideas I had introduced in 1957 with the coherent Riemann-Roch theorem, where I did self-intersection calculations in particular, taking care not to "move" the cycle under consideration. A first obvious reduction (obtained by immersing  $X$  in an  $S$ -scheme) would be to the case where  $X$  is a firm subscheme of the regular  $S$ -scheme. . .

The idea that it should be possible to develop a **singular** (coherent) Riemann-Roch theorem was familiar to me, I can't say how long ago, but I never tried to test it seriously. It was more or less this idea (apart from the analogy with the "cohomology, homology, cap-product" formalism) that led me in SGA 6 (in 1966/67) to systematically introduce the  $K_*(X)$  and  $K^*(X)$  and the  $A_*(X)$ ,  $A^*(X)$ , instead of just working with the  $K^*(X)$ . I don't remember whether I also thought of something like this in the SGA 5 seminar in 1966, and whether I hinted at it in the oral presentation. As my handwritten notes have disappeared (in a removal perhaps?) I'll probably never know... .

(June 7) Looking through Mac Pherson's article, I was struck by the fact that the word "Riemann-Roch" is not mentioned - which is why I didn't immediately recognize the conjecture I made in the SGA 5 seminar in 1966, which was for me (and still is) a "Riemann-Roch" theorem. It seems that when Mac Pherson wrote his article, he didn't even realize that it was a "Riemann-Roch" theorem.

<sup>102</sup>(\*) (March 1985) This is indeed the case, cf. note no. ° 164 cited in the previous footnote.



of this obvious kinship. I presume that the reason for this is that Deligne, who after my departure put this conjecture into circulation in the form he liked, took care as far as possible to "erase" it.

p. 367 the obvious kinship with the Riemann-Roch-Grothendieck theorem. I think I sense his motivation for doing so. On the one hand, it weakens the link between this conjecture and myself, and makes it more plausible to call-

the "Deligne-Grothendieck conjecture" under which it is currently circulating. (NB I don't know if it's in circulation in the schematic case, and if so, I'd be very curious to know under what name). But the deeper reason seems to me to lie in his obsessive desire to deny and destroy, as far as possible, the fundamental unity of my work and my mathematical vision<sup>103</sup> (\*). This is a striking example of how, in a mathematician of exceptional means, a fixed idea entirely unrelated to any mathematical motivation, can obscure (or even completely block out) what I have called the "healthy mathematical instinct". This instinct cannot fail to perceive the analogy between the two "continuous" and "discrete" statements of the "same" Riemann-Roch theorem, which I had of course emphasized in the oral presentation. As I indicated yesterday, this kinship will no doubt soon be confirmed by a statement in form (conjectured by Zoghman Mebkhout), at least in the complex analytic case, allowing both to be deduced from a common statement. Clearly, given Deligne's "gravedigging" attitude towards the Riemann-Roch theorem<sup>104</sup> (\*\*), he was not likely to discover the unique statement that links them in the analytic framework, and even less likely to raise the question of an analogous statement in the general schematic framework. Nor was he able, in such circumstances, to identify the fruitful point of view of D-Modules in the cohomological theory of algebraic varieties, arising all too naturally from ideas that had to be buried - or even to recognize, for years, the fruitful work of Mebkhout, succeeding where he himself had failed.

**Note** 87<sub>2</sub> (May 31) This is the year of my Bourbaki lecture on the rationality of *L-functions*, in which I heuristically use Verdier's result (???) (and especially the expected form of local terms in the case of d'espèce), without waiting for Illusie to demonstrate it thirteen years later, at Deligne's invitation. It seemed to me, moreover, when Verdier showed me his ultra-general formula that came as a surprise, that he demonstrated it with a few lines of "six operations" formalism - this is the kind of formula where (almost) to write it is to demonstrate it! If there was any "difficulty", it could only be at the level of verifying one or two compatibilities<sup>105</sup> (\*). What's more, both Illusie and Deligne knew perfectly well that the demonstrations I had given in the seminar for various explicit trace formulas **were complete**, and in no way depended on Verdier's general formula, which had simply acted as a "trigger" to encourage us to make explicit and prove trace formulas in as general a range of cases as possible. The bad faith of both is obvious here. As far as Deligne is concerned, it was already clear to me when I wrote the note "La table rase" (n° 67) - but probably not to an uninformed reader, nor of course to an informed reader who renounces the use of his healthy faculties.

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<sup>103</sup>(\*) Compare with the commentary in the note "La dépouille" (n° 88) on the deeper meaning of the SGA 4 operation<sup>1</sup>, similarly aimed at shattering into an amorphous set of "technical digressions" the deeper unity of my work around staggered cohomology, through the "violent insertion" of the foreign text SGA 4<sup>1</sup> between the two indissoluble parts SGA 4 and SGA 5 which develop this work.

<sup>104</sup>(\*\*) These dispositions, with regard precisely to the Riemann-Roch-Grothendieck theorem, are particularly clear in the "Funeral Eulogy"; see the note "Funeral Eulogy (1) - or compliments", n° 104.

<sup>105</sup>(\*) (June 6) It would also appear that, via the biduality theorem (in the meantime promoted to "Deligne's theorem"), the initial demonstration of the Lefschetz-Verdier formula depended on a singularity resolution hypothesis, which Deligne manages to dispense with in the case of *fi ni* type schemes over a body. This is a good opportunity to fish in troubled waters and give the impression that SGA 5 is subordinate to the "seminar-sic" SGA 4<sup>1</sup> which "precedes" it (and which was published well and truly before...). him!).

(June 6) As for Illusie, he played right into his friend's hands, trying to muddy the waters to give the appearance of an ultra-technical oral seminar that didn't even give complete demonstrations of all the results, especially the trace formulas. These were, however, demonstrated there (and for the first time) in 65/66, and it was there that both he and Deligne had the privilege of learning them, and the delicate technique that goes with it<sup>106</sup> (\*\*).

□ This reminds me that, of course, I had taken the trouble to demonstrate Lefschetz-'s formula.

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Verdier in the seminar - it was the least I could do, and a particularly striking application of the formalism of local and global duality that I set out to develop. The question came to me these days: why on earth, when there were ten or so papers still being written by my dear students, so that Deligne and Illusie were spoilt for choice when it came to naming their technical "obstacle" to the publication of SGA 5, they chose the theorem of their good buddy Verdier, who at the time was taking credit for it as his own, just as he had never bothered to write (or at least make available to the public) the theorem of derived and triangulated categories. There's a kind of **defiance** in the absurdity (or in a kind of collective cynicism in the group of my ex-cohomology students, whom I consider to be in solidarity in this operation-massacre), which reminds me of the "weight-complexes" brilliantly invented by Verdier the previous year (see the note of that name, n° 83), or (in the iniquitous register) with the "perverse" name given by Deligne to bundles that should be called "Mebkhout bundles" (see the note "La Perversité", n° 76). I sense such inventions as acts of domination and contempt towards the entire mathematical community.

- and at the same time a **gamble**, which was clearly won until the unexpected appearance of the deceased, who appears almost as the only one awake in a community of sleepers...

**note** 87<sub>3</sub> (June 5) After this assessment of a massacre, we will appreciate the value of Illusie's statement in line 2 of his introduction to the volume entitled SGA 5:

"Compared with the original version, the only significant changes concern Lecture II [generic K nneth for- mules], which has not been reproduced, and Lecture III [Lefschetz- formula], which has been reproduced.

Verdier], which has been completely rewritten and expanded with an appendix numbered III B<sup>107</sup> (\*). A part some changes of □ detail and additions of footnotes, the other presentations have been left **as they are**" (emphasis added).

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Here again, Illusie complacently echoes another well-sent joke from his inenarrable friend, namely that the existence of SGA 4<sup>1</sup> "will soon make it possible to publish SGA 5 **as is**" (see the "Clean slate" note n° 67) - and Illusie does his utmost in his talks and introductions to lend credence to this.

<sup>106</sup>(\*\*) In the second paragraph of the Introduction to the volume published as SGA 5, Illusie presents as "the heart of the seminar" the three lectures III, III B, XII on Lefschetz's formula in stale cohomology, whereas we have seen that in the introduction to lecture III B., he takes care to specify (contrary to reality) that "this lecture corresponds to no oral lecture of the seminar" and that in the introductions to lectures III and III B., he does his utmost to give the impression that these are "the heart of the seminar", he is careful to specify (contrary to reality) that "this lecture does not correspond to any oral lecture of the seminar" and that in the introductions to lectures III and III B, he does his utmost to give the impression that these are subordinate to SGA 4<sup>1</sup>, and that lecture III is presented as "conjectural"! ! In fact, the entire SGA 5 seminar was technically independent of Lecture III (Lefschetz-Verdier formula), which played the role of a heuristic motivator, and Lecture III B is no more than the "hole" (Lecture XI) created by the move to Bucur, which was the welcome pretext for this further dismemberment.

To lend credence to the version of a seminar of "technical digressions" (blown up by his friend Deligne), Illusie was careful to skip the introductory presentation, in which I had painted a preliminary picture of the main themes that were to be developed in this seminar, a picture in which the trace formulas form only a small part (taking on particular importance because of their arithmetical implications, in the direction of Weil's conjectures). For an overview of these "major themes", see sub-note n° 87 .5

<sup>107</sup>(\*) Which is presented as part of the "heart of the seminar"! (See previous b. de p. note.)

imposture (that SGA 5, where he and his friend learned their trade, would depend on the pirate-volume SGA 4<sup>1</sup>, made of bries and bracs gleaned or plundered over the following twelve years), by a luxury of cross-references to SGA 4<sup>1</sup> every page turn...

The final word goes (as it should) to Deligne, who wrote to me a month ago (May 3), in response to a laconic request for information (see the beginning of the note "Les Obsèques", n° 70):

"In short, if it had been seven years since you were doing maths [?!] when this SGA 4 text<sup>1</sup> appeared, this simply corresponds [?] to the long delay in editing SGA 5, **which was too incomplete to be usefully published as it stood.**

I hope these explanations please you."

If they haven't "approved" me, at least they've edified me. . .

**Note 87<sub>4</sub>** (June 6) Perhaps it's time to indicate what the main themes were that were developed in the oral seminar, and of which the published text gives an idea only by cross-checking.

I) Local aspects of duality theory, whose essential technical ingredient is (as in the coherent case) the biduality theorem (supplemented by a "cohomological purity" theorem). I have the impression that the geometrical meaning of the latter theorem, as a local Poincaré duality theorem, which I had explained so well in the oral seminar, has since been entirely forgotten by my former students<sup>108</sup> (\*).

II) Trace formulas, including "non-commutative" trace formulas more subtle than the formula of the usual traces (where both members are integers, or more generally elements of the ring of coefficients, such as  $\mathbb{Z}/n\mathbb{Z}$  or an  $\ell$ -adic ring  $\mathbb{Z}$ , or even  $\mathbb{Q}$ ), placing ourselves in the algebra of a  $\square$  finite operating group on the considered scheme, with coefficients in a suitable ring (such as those considered in the previous parenthesis). This generalization came very naturally, since even in the case of Lefschetz formulas of the usual type, but for "twisted" bundles of coefficients, we were led to replace the initial scheme by a Galois coating (usually branched) serving to "untwist" the coefficients, with the Galois group operating on it. Nielsen-Wecken" type formulas are thus naturally introduced into the schematic context.

III) Euler-Poincaré formulas. On the one hand, there was a detailed study of an "absolute" formula for algebraic curves, using Serre-Swan modules (generalizing the case of moderately branched coefficients, giving rise to the more naive Ogg-Chafarévitch-Grothendieck formula). On the other hand, there were novel and profound conjectures of the "discrete" Riemann-Roch type, one of which reappeared seven years later, in a hybrid version, under the name of the "Deligne-Grothendieck conjecture", proved by Mac Pherson by transcendental means (see note n° 87 ).<sup>1</sup>

The comments I couldn't fail to make on the profound relationships between these two themes (Lefschetz formulas, Euler-Poincaré formulas) were also lost without trace. (As was my habit, I left all my handwritten notes to the volunteer-writers, and no written trace remains of the oral seminar, of which I did, of course, have a complete set of handwritten notes, even if some of them were succinct).

IV) Detailed formalism for homology and cohomology classes associated with a cycle, derived naturally from the general duality formalism and the key idea of working with the cohomology "with supports" in the cycle under consideration, using cohomological purity theorems.

<sup>108</sup>(\*) This geometrical interpretation has at least been preserved in Illusie's writing.

V) Finiteness theorems (including generic finiteness theorems) and generic K nneth theorems for cohomology with any support.

The seminar also developed a technique for passing from torsion coefficients to l-adic coefficients (lectures V and VI). This was the most technical part of the seminar, which as a rule worked with torsion coefficients, and then "go to the limit" to deduce the corresponding l-adic results.

dants. This point of view was a <sup>provisory</sup> pis-aller, pending Jouanolou's thesis (still unpublished at p. 37)

(at present) giving the formalism that was needed directly in the l-adic framework.

I don't include among the main "themes" the calculations of some classical schemes and the cohomological theory of Chern classes, which Illusie highlights in his introduction as "one of the most interesting" of the seminar. As the program was full, I hadn't felt it necessary to dwell on these calculations and this construction in the oral seminar, as it was sufficient to repeat, virtually verbatim, the reasoning I had given ten years earlier in the context of Chow rings, on the occasion of the Riemann-Roch theorem. On the other hand, it was obvious that it had to be included in the written seminar, to provide a useful reference for the user of étale cohomology. Jouanolou took on this task (Lecture VIII), which he had to regard not as a service to the mathematical community, while learning basic techniques essential for his own use, but as a chore, since its writing dragged on for years<sup>109</sup> (\*). It was no different, it seems, for his thesis, which remains a ghostly reference just like Verdier's. ... The "passage à la limite" section shouldn't be counted as one of the seminar's "main themes" either, in the sense that it isn't associated with any particular geometrical idea. Rather, it reflects a technical complication peculiar to the context of étale cohomology (distinguishing it from transcendental contexts), namely that the main theorems on étale cohomology concern in the first place **torsion** coefficients (prime to residual characteristics), and that in order to have a theory that corresponds to rings of coefficients of zero characteristic (as is necessary for Weil's conjectures), we need to go to the limit on rings of coefficients  $Z/t^n Z$  to obtain "*l-adic*" results.

All this said, the only one of the five main themes of the oral seminar that appears in complete form in the published text is theme I. Themes IV and V have simply disappeared, absorbed by SGA 4. Themes IV and V have disappeared altogether, absorbed by SGA 4<sup>1</sup>, with the advantage of being able to refer to them extensively and give the impression that SGA 5 depends on a text by Deligne that appears to predate it. Themes II and III appear in the published volume in mutilated form, still maintaining the same imposture of dependence on the SGA 4 text<sup>1</sup> (which in reality emerged entirely from the mother seminar SGA 4, SGA 5).

### 15.3.6. The skinning

**Note 88** □(May 16) The set of two consecutive seminars SGA 4 and SGA 5 (which for me are like p. 373 a single "seminar") develops from nothingness both the powerful instrument of synthesis and discovery represented by the **language of** topos, and the perfectly perfected, perfectly effective **tool of** stale cohomology - better understood in its essential formal properties, from that moment on, than even the cohomological theory of ordinary spaces<sup>110</sup> (\*). This whole represents the most profound and innovative contribution I have made to mathematics, at the level of a fully completed work. At the same time, and without wishing to be, while at every moment everything unfolds with the naturalness of

<sup>109</sup>(\*) (June 12) Going through the presentation in question, I was convinced of Jouanolou's perfect connivance with my other cohomology students.

<sup>110</sup>(\*) Even if we restrict ourselves to the spaces closest to "varieties", such as triangulable spaces.

Obviously, this work represents the most far-reaching technical "tour de force" I've accomplished in my work as a mathematician<sup>111</sup> (\*\*). For me, these two seminars are indissolubly linked. They represent, in their unity, both the **vision** and the **tool** - the topos, and a complete formalism of scalar cohomology.

While the vision is still rejected today, the tool has, over the last twenty years or so, profoundly re-imagined algebraic geometry in its most fascinating aspect for me - the "arithmetic" aspect, apprehended by an intuition, and by a conceptual and technical baggage, of a "geometric" nature.

It was certainly not only the intention of suggesting that his cohomological "digest" **had anteriority** over the SGA 5 part that motivated Deligne to give it the misleading name SGA 4<sup>1</sup> - nothing prevented him from doing so.

after all, while we're at it, to call it SGA 3<sup>1</sup> ! In "Operation SGA 4<sup>1</sup>" I sense the intention to present the work from which all his own work stems (this work from which he cannot detach himself!) - a work of obvious and profound unity, clearly apparent in the two seminars, SGA 4 and (the real) SGA 5, as thing **divided** (as itself is divided. . . ), **cut in two** by this violent insertion of a foreign and disdainful text; of a text that would like to present itself as the living heart, the quintessence of a thought, of a vision in which he had no part<sup>112</sup> (\*), and the two "quarters" that surround it as a sort of vaguely grotesque appendix, like a collection of "digressions" and "technical complements" to the work that Deligne claims to be central and essential, and in which my humble self is graciously admitted (before total burial) to the number of "collaborators"<sup>113</sup> (\*\*).

Chance" had done it right. This "corpse left to mercy - this "unfortunate seminar" always left behind by the "editors", and at the time of my departure in the hands and at the discretion of my cohomology students - this was not **just any** part of the master's work! It wasn't SGA 1 and SGA 2 (where I was developing, in my own corner and without even realizing it, the tools that were to be the two essential technical aids for the "take-off" of the main work to come), nor SGA 3 (where my contribution consisted mainly of incessant - and sometimes arduous - scales and arpeggios to hone the "all-out" technique of schematics), nor SGA 6 (systematically developing my ten-year-old ideas on the Riemann-Roch theorem and the intersection formalism), or even SGA 7 (which, through the inner logic of reflection, stems from the possession of the central tool, mastery of cohomology). It is indeed the **main part of** my work, the writing of which had remained unfinished (and by their care. . . ), that I have left, at least in part, in the hands of my cohomology students. It was this main part of the work that they chose to massacre and appropriate as their own, forgetting the unity that is their meaning and beauty, and their creative virtue (90).

And it's no coincidence either that, equipped with heterogeneous tools and denying the spirit and vision that had brought them into being from nothing, none were able to discern the innovative work where it was being reborn, against their indifference and disdain. Nor that after six years, when at the end of the line the new tool was finally

apprehended by Deligne, they unanimously buried the one who had created it in solitude - Zoghman Mebkhout, <sup>□</sup>the disowned master's posthumous pupil! And it's no more a coincidence that, after the momentum had died down

Deligne's initial work (which in just a few years had led him to launch a new theory of

<sup>111</sup>(\*\*) Some difficult or unforeseen results were obtained by others (Artin, Verdier, Giraud, Deligne), and some parts of the work were done in collaboration with others. This in no way detracts (in my mind at least) from the strength of my appreciation of the place of this work in my body of work. I intend to come back to this point in more detail, in an appendix to the Thematic Outline, and to dot the i's and cross the t's where it's obviously become necessary.

<sup>112</sup>(\*) This line of thought had reached full maturity, in terms of both key ideas and essential results, even before the young man Deligne appeared on the scene to learn algebraic geometry and cohomological techniques from me, between 1965 and 1969.

(May 30) On this subject, see the note "L'être à part", n° 67'.

<sup>113</sup>(\*\*) See notes "Le feu vert", "Le renversement", n° s 68, 68'.

Hodge, and towards the demonstration of Weil's conjectures), and despite its prodigious means and the brilliant means of my cohomology students, I note today this "morose stagnation" in a field of prodigious richness where everything still seems to be to be done. This should come as no surprise, when for nearly fifteen years the main source of inspiration and some of the "big problems"<sup>114</sup> (\*), even though they are present and confronting us at every step, remain carefully bypassed and concealed, like the messengers of the one whom for fifteen years it has been our constant aim to bury.

### 15.3.7. . . and the body

**Note** 89 (May 17) The thought, the vision of things that lived within me and that I had thought I was communicating, I see as a living body, healthy and harmonious, animated by the power of renewal of living things, the power to conceive and engender. And now this living body has become a **corpse**, shared between some and others - this limb or quarter, duly stuffed, serving as a trophy for one, another, butchered, as a puzzle or boomerang for another, and yet another, who knows, as it is, for the family kitchen (we're not that far from it!) - and all the rest is good for rotting in the dump... .

That's the picture I've come to see, in terms that may be colorful but seem to express a certain reality of things. The puzzle may well fracture a skull here and there<sup>115</sup> (\*\*)  
- but never will these scattered pieces, neither trophy nor puzzle nor family soup, have the power so simple and obvious in the living body: that of the loving embrace that creates a new being. ...

(May 18) This image of the living body, and of the "remains" with its pieces scattered to the four winds, must have been forming in me throughout the past week. □ The comical form in which it presented itself under my pen-p

typewriter in no way means that this image is in the least an **invention**, a tad macabre, a burlesque improvisation on the spur of the moment. The image expresses a **reality**, felt pro- fondly at the moment it took material form through a written formulation. I must have been aware of this reality in bits and pieces, here and there, over the fourteen years since my "departure", and perhaps even before. Bits and pieces of information recorded at first on a superficial level by a distracted attention, absorbed elsewhere - but which all pointed in the same direction, and which must have assembled, on a deeper level, into a certain image - an unformulated image that I didn't bother to take note of, when I had other things to worry about. This image has been considerably enriched and clarified in the course of reflection since the end of March, six or seven weeks ago. More precisely, scattered pieces of information, finally examined by the care of a fully present conscious attention, have been gradually assembled into **another** image, at the more superficial level of the thought that examines and probes, through work that might seem independent of the presence, in deeper layers, of the first. This conscious work culminated six days ago in the sudden vision of the "massacre" that took place - when I felt the "breath", the "smell" of **violence**, for the first time I believe in the whole<sup>116</sup> (\*) reflection. It was also the moment when, in the layers already close to the surface, this

<sup>114</sup>(\*) This "main source of inspiration" is, of course, the "yoga of motives". It has been active in Deligne alone, who has kept it to himself for his own "benefit", and in a narrow form deprived of much of its force, denying some of the essential aspects of this yoga. Among the "great problems" inspired by this one, which have been ignored or discreetly discredited, I see right now (outsider that I am) the standard conjectures, and the development of the formalism of the "six operations" for all the usual types of coefficients, more or less close to the "motifs" themselves (which play in their respect the role of "universal" coefficients - those which give rise to all the others). Compare with the comments on this subject in the note "My orphans", n° 46.

<sup>115</sup>(\*\*) (May 31) And it will even be used to prove a theorem "of proverbial difficulty"!

<sup>116</sup>(\*) (June 12) In recent years, I've sensed violent intent on the part of some of my ex-students towards some of my "co-students", but never a violence that could be felt as coming from a collective will (grouping five of them here).

the feeling of a living, harmonious body, which is well and truly "massacred" - and also the one in which the deeper, diffuse image must have begun to surface, perhaps bringing to the image-in-form a carnal dimension, a "smell" that thought alone is powerless to give.

This "carnal" aspect came to the fore again in a dream last night - it's under the impulse of this dream that I now return to the lines I wrote yesterday. In this dream, I was cut quite deeply in several places on my body. First of all, there were cuts on my lips and in my mouth itself, bleeding profusely as I rinsed out my mouth with copious amounts of water (heavily reddened by blood) in front of my face. an ice pack. Then wounds in the belly, also bleeding profusely, especially one from which blood was coming out.

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□ by jerks, as if it were an artery (the Dreamer didn't care about anatomical realism). The thought I pressed my hand in front of the wound and curled up to stop the blood - it did indeed stop flowing, eventually forming a clot and a very large scab. Later, when I carefully lifted the scab, delicate healing had already begun. I was also cut on one finger, and it was surrounded by an impressive dressing doll... ...

I have no intention of embarking on a more delicate and detailed description of this dream, nor of probing it in depth here (or elsewhere). What this dream "as is" already reveals to me with startling force is that this "body" of which I spoke yesterday, and which as I was writing I saw as detached from me, like a child perhaps that I had conceived and procreated and which had gone out into the world to follow its own path - this body remains today an intimate part of my person: that it is **my** body, made of flesh and blood and a life force that enables it to survive deep wounds and regenerate itself. And my body is also, without doubt, the thing in the world to which I am most deeply, most indissolubly linked. ...

The Dreamer did not follow me in the image of the "massacre" and the sharing of the remains. This image was meant to convey the reality of intentions, of dispositions in **others** that I had strongly perceived, and not the way in which I myself experienced this aggression, this mutilation of which I was the object through something to which I remain closely linked. The Dreamer has just given me a glimpse of the extent to which I remain linked to it. This ties in with what I perceived (albeit less forcefully) in the reflection in the note "Le retour des choses - ou un pied dans le plat" (n° 73), where I try to pinpoint the feeling of this "deep link between the one who conceived a thing, and this thing", which appeared in the course of the reflection that day. Before that reflection on April 30 (barely three weeks ago), and for the rest of my life, I have pretended to ignore this link, or at least to minimize it, following the well-trodden path of current clichés. Worrying about the fate of a work of art that has left our hands, and of course worrying about whether our name remains attached to it in any way, is felt to be petty and mean-spirited - whereas it seems natural to us all that we should be deeply touched when a child of the flesh whom we have raised (and whom we believe we have loved) chooses to repudiate the name he was given at birth.

### 15.3.8. The heir

□

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**Note 90** (May 18) I don't know if any student in the sixties (apart from Deligne) was able to sense this essential unity, beyond the limited work he was doing with me. Some may have sensed this in a confused way, and that this perception was lost without return in the years following my departure. What is certain, however, is that from our first contact in 1965, Deligne sensed this living unity. It was this fine perception of a unity of purpose in a vast design that was surely the main stimulus for his intense interest in everything I had to communicate and pass on. This interest

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people) and directed against me, through my work.

demonstrated, without ever wavering, throughout the four years of constant mathematical contact between 1965 and 1969<sup>117</sup> (\*). He gave mathematical communication between us that exceptional quality I've already mentioned, and which I've experienced with other mathematician friends only in rare moments. It was this perception of the essential, and the passionate interest it stimulated in him, that enabled him to learn as if by playing everything I could teach him: both the technical means (zinc strand diagram technique, Riemann-Roch yoga and intersections, cohomological formalism, étale cohomology, topos language) and the overall **vision** that unites them, and finally the **yoga of patterns** that was then the main fruit of this vision, and the most powerful source of inspiration I had yet discovered.

What's clear is that Deligne was the only one of my students, right up to the present day, who at a certain point (as early as 1968, I believe) had fully assimilated and made his own the totality of what I had to transmit, in its essential unity as well as in the diversity of its means<sup>118</sup> (\*\*). It was, of course, this circumstance, felt by all, that made him the designated "legitimate heir" to my work.

work. Clearly, this heritage neither encumbered nor limited him - it was not a burden, but gave him a sense of identity.

wings; I mean: he nourished with his vigor these "wings" <sup>"□which</sup> he had from birth, like other visions and p . 379 other heritages (less personal of course...) would nourish it...

The heritage he had nurtured in those crucial years of growth and development, and the unity that makes up its beauty and creative virtue, which he had sensed so well and which had become like a part of himself - my friend subsequently<sup>119</sup> (\*) denied them, striving relentlessly to hide the heritage, and to deny and destroy the creative unity that was its soul. He was the first among my students to set an example by appropriating tools, "pieces", while striving to dislocate the unity, the living body from which they came. His own creative impulse was slowed down, absorbed and finally dislocated by this deep division within him, driving him to deny and destroy the very thing that made him strong, that nourished his impulse.

I see this division expressed in three interrelated effects. One is the dissipation of energy, scattered in the effort to deny, dislocate, supplant, hide. The other lies in the rejection of certain ideas and means, essential to the subject's "natural" development.

which he has chosen as his central theme<sup>120</sup> (\*\*). The third is the attachment to this theme, of all themes, which is about

to supplant, to oust a master who is present every step of the way and who must be constantly erased - the very theme p . 380

who is most intensely invested with the fundamental contradiction that dominated his life as a mathematician.

What I know at first hand, and a basic instinct or flair that has never deceived me, make

<sup>117</sup>(\*) This period comprises five years, of which my friend spent one (1966) in Belgium doing his military service.

<sup>118</sup>(\*\*) When I say "totality", I mean everything that was essential, both in vision and in means. This doesn't mean, of course, that there weren't unpublished ideas and results that I never thought of telling him about. On the other hand, I don't think there was any mathematical reflection from the years 1965-69 that I didn't talk about "on the spot" to my friend, always with pleasure and profit.

<sup>119</sup>(\*) Strangely enough, this division must have been present as early as the first year of our meeting (already expressed in an ambiguous attitude towards the SGA 5 seminar, which was his first contact with schemes, Grothendieck-style cohomological techniques, and staggered cohomology), and at the latest and in unequivocal form as early as 1968 (see note "L'éviction", n° 63). - at a time, therefore, when mathematical communication was perfect, and when the development of his mathematical thinking seems to me not yet to have been marked by conflict. At the time, he made many interesting contributions (which I take great pleasure in highlighting in the Introduction to SGA 4) on topics that he did his utmost, immediately after my departure, to bury.

<sup>120</sup>(\*\*) This refusal manifested itself in the burial of derived and triangulated categories (until 1981), of the formalism of the six variances (until today), of the language of topos (also), and by a sort of "blocking by disdain" of the vast program of foundations for homological and hoinotopic algebra, which I'm now trying (twenty years later) to sketch out with La Poursuite des Champs, and which he had of course also felt the need for. However, even though it was inspired by the yoga of motifs (buried until 1982), this yoga remained mutilated of part of its force, being detached from the formalism of the six variances which constitutes an essential formal aspect of it. It seems to me that this aspect has also been rigorously banished from



Hodge-Deligne's theory.

it's quite clear to me that if Deligne hadn't been torn by this profound contradiction in his own work, mathematics today wouldn't look like it does<sup>121</sup> (\*) - that it would have undergone, in several of its essential parts, far-reaching renewals like the one I myself had been the main instrument of - the very one that this same Deligne was bent on countering and diverting!<sup>122</sup> (\*\* )

There's also no doubt that he was ideally suited to be the driving force behind a powerful school of geometry, a continuation of the one that had formed around me - a school nourished by the vigor of the one from which it had sprung, and the creative power of the one who was taking over from me. But this school that had formed around me, this nurturing matrix that had surrounded intense years of training - it broke up the day after my death.

p. 381 of my departure. If this was the case, it was precisely because I couldn't find, in the person who was obviously taking over from me<sup>123</sup> (\*\*\*), the person who would also be the soul of a group united by a common adventure, for a common task.

whose dimensions are beyond anyone's means.

I have the impression that, after my departure, each of my students found themselves in their own corner, with a wealth of work to do - there's no shortage of that anywhere in maths - but without this "corner" fitting into a whole, and without this "work" being carried along by a current, by a wider purpose. Surely, as soon as I left, if not even before, the eyes of most of my students or ex-students were focused on the designated "successor", the most brilliant among them and also the closest to me. At this sensitive moment, my friend must have felt, perhaps for the first time in his life, the power over others that was suddenly in his hands, the power of life or death that he had over the fate of a certain school from which he had come, and whose friends he had rubbed shoulders with for four years were no doubt expecting him to ensure its continuity. The situation was entirely in his hands, and it was he who would set the tone... He did indeed set the tone, by destroying the legacy, and first and foremost that confidence and expectation<sup>124</sup> (\*) which those who, with him, had been pupils of the same master, could not fail to bring him. ...

I'm sure many people are impressed by Deligne's work, and not without reason. But I'm also well aware that this work, beyond the impressive initial impetus (ending with the demonstration of Weil's conjectures), is far from "living up to its potential". It certainly demonstrates an uncommon technical mastery and ease, placing him among the "best". But it lacks the humble virtue that

<sup>121</sup>(\*) When I wrote these lines about "mathematics today", I wasn't just thinking about the more or less profound knowledge we have of mathematical things today. I was also thinking, in the background, of a certain **spirit** in the world of mathematicians, and more particularly in what might be called (without sarcastic or mocking intonation) "the big mathematical world": the one that "sets the tone" for deciding what is "important", even "licit", and what is not, and the one that also controls the means of information and, to a large extent, careers. Perhaps I'm exaggerating the importance that a single person, in the position of figure de proue, can have on the "spirit of the times" in a given milieu at a given time. Deligne's career seems to me to be comparable (for better or for worse) to Weil's in the milieu that had welcomed me twenty years earlier, and with which I had identified myself for twenty years.

(May 31) Compare with the (complementary) reflections in the note "Le Fossoyeur - ou la Congrégation toute entière", n° 97.<sup>122</sup> (\*\*) (June 16) I'm convinced that the very fact that the key ideas I've introduced into mathematics are already developing normally, on the momentum gained in the sixties (cut short by the "chainsaw effect" to be discussed in the following two notes. ... ), mathematics today, fifteen years after my departure, would have been different from what it is, in some of its essential parts. ...

<sup>123</sup>(\*\*\*) This **de facto succession** was expressed by unequivocal concrete signs: he took over from me at the IHES (from which I left the year after he joined - see note "L'éviction", n° 63), and he took over, with the means I had developed at this final for some fifteen years (from 1955 to 1970), the central theme of the cohomology of algebraic varieties.

<sup>124</sup>(\*) (May 26) In the course of further reflection, I detected yet another "expectation" regarding my tacit heir, this time coming not from my students alone, but from "the whole Congregation" - see the end of the note "The Gravedigger - or the whole Congregation" (n° 97). I have little doubt that these two opposing expectations, one linked to a very particular moment, the other continuing throughout the fourteen years of the Burial, are both real. What's more, I'd be inclined to think that for many of my former students, the two expectations must have been present simultaneously: that of finding in the most brilliant among them the one who would ensure the continuity of a School and a work in which they had their place and their part - and that of seeing erased (if that were possible) all trace of the one whose departure suddenly called out to them with such force, in the quietude of the well-trodden paths. ...

I perceived in him in his early years - the virtue of renewal. This virtue he carried within him, this freshness or innocence of the little child, has long since been deeply buried, denied. I was about to write that by this "virtue" and by his not very <sup>common</sup> gifts, as well as by the exceptional circumstances of which he benefited from the deployment of his gifts, Deligne was called upon to "dominate" the mathematics of our time, just as a Riemann, or a Hilbert, had each "dominated" the mathematics of their time. Inveterate habits of thought, rooted in common parlance, have suggested to me this image of "domination", which nevertheless gives a distorted apprehension of reality. These great men undoubtedly fully "grasped", "assimilated", "made their own" the mathematics known in their time, which undoubtedly also gave them an exceptional mastery of technical means. But if they rightly seem "great" to us, it's not through their technical prowess, "wresting" difficult demonstrations from surly substance. It's by the renewal each of them brought to several important parts of mathematics, by simple and fruitful "ideas", that is to say: for having turned their gaze on simple and essential things, to which no one before them had deigned to pay attention. This childlike ability to **see** simple, essential things, however humble and disdained by all - **this is the** power of renewal, the creative power in everyone. This power was present to a rare degree in the young man I knew, unknown to all, a modest and passionate lover of mathematics. Over the years, this humble "power" has seemed to disappear from the person of the admired and feared mathematician, enjoying unfettered his prestige, and the (sometimes discretionary) power it gives him over others.

This **stifling** in my friend of something very delicate and very vivid, neglected by all and which has creative power, I've felt it many times since I left, and more and more in recent years. But it took the discoveries of the last few weeks, and the reflection I've been pursuing since the end of March (following on from Récoltes et Semailles), to begin to feel the full extent of the devastating effect of this suffocation in the life of my friend, and among many others I've known closely. Not only on some of my "later" (and assimilated) students, who were subjected to his malevolence (perhaps unconscious in some cases), which was exercised against everyone and weighed heavily on three of them; but also, it seems to me glimpse it now, among my students "before", by the destruction of a **continuity** in the subject, and that of a sense of a whole, a unity, giving a deeper and broader meaning to their work than ce <sup>lui</sup> p. 383 an accumulation of separate prints bearing their names (91)<sup>125</sup> (\*).

More than once in the last seven years, and more than once again in the last few weeks and days, I've felt a sadness, at what feels, on some level, like an immense **waste** - when what is most precious in oneself and in others is squandered or smothered as if for pleasure. Yet I've also come to learn that such "waste" is a staple of the human condition, to be found in one form or another everywhere, in the lives of individuals, from the humblest to the most illustrious, as well as in the lives of peoples and nations. This very "mess", which is nothing other than the action of conflict and division in everyone's life, is a substance of a richness and depth that I have barely begun to fathom, a nourishment that it is up to me to "eat" and assimilate. So this mess, and every other mess I encounter at every step, and every other thing that happens to me at the turn of the road and is so often unwelcome - this mess and other unwelcome things carry within them a  **blessing**. If meditation has meaning, if it has the power of renewal, it's insofar as it allows me to receive

<sup>125</sup>(\*) (June 16) This second aspect only came to my attention in the course of my reflection on L'Enterrement. If ever I saw a prestigious mathematician make use of the "power to discourage", it was the very man who once seemed to me to be my heir apparent. When I wrote the section "The power to discourage", I had thought a lot about him (before the thought came back to me), but without yet having the slightest inkling (at least not on a conscious level) of the extent to which this power had found occasion to be exercised among the very people for whom he must have been (as he was for me) the model of the perfect mathematician. ...

the benefit of that which (through my inveterate reflexes) presents itself as "evil", where it allows me to **nourish** what seems designed to destroy.

Nourishing yourself with your experience, letting it renew you instead of constantly evading it - that's what it means to take full responsibility for your life. I have this power within me, and it's up to me at any given moment to make use of it, or to let it go to waste. It's the same for my friend Pierre, and for each of my students - free, like me, to feed on the "mess" I'm finishing reviewing in these last days of a long meditation. And the same goes for the reader who reads these lines, intended for him or her.

### 15.3.9. The co-heirs

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**Note** 91 (May 19) The echoes that have reached me here and there about my former pupils have been more than sparse.

Hardly any of them gave me any sign of life after my departure, if only by sending me prints<sup>126</sup> (\*). However, by gathering together the few that have reached me, I can form an idea, admittedly very approximate. Perhaps it will become clearer in the months to come, if this reflection prompts some of them to come forward.

I've already had occasion to note the profound rupture in Deligne's work after my departure, even though in some respects he appears, unwillingly, as a successor, and therefore as part of a certain continuity. And I had the feeling that this rupture must have had profound repercussions on the work of all my other students. It's this impression that I'd like to explore a little more closely.

The only one of these students whose work seems to be an obvious (at least at first sight) extension of the work he had done with me, seems to be Berthelot<sup>127</sup> (\*\*). He's also the only one who, for a long time, sent me numerous separate prints - perhaps even all his separate prints. They are all on the difficult subject of crystalline cohomology, the systematic start-up of which is the subject of his thesis. Yet it seems to me that, as with my other (commutative) "cohomology" students, his work is marked by the disaffection of some of the main ideas I had introduced: derived categories (and triangulated categories, cleared by Verdier), six-operation formalism, topos (91<sub>1</sub>). As Zoghman Mebkhout himself says, his own work, so close in theme to Berthelot's (91<sub>2</sub>), is in line with these ideas, combined with those of the Sato school. If they hadn't been repudiated by my cohomology students, led by Deligne and Verdier, there's a good chance that from the very beginning of the seventies,

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Mebkhout's crystalline theory (which he began to develop only from 1975, against the disinterest of these same students) would already have reached the full maturity of a formalism of <sup>six</sup>□operations, which it still hasn't reached today<sup>128</sup> (\*).

Incidentally, I remember talking to Verdier about the question, which intrigued me, of the link between constructible discrete coefficients and continuous coefficients, without it seeming to catch his eye. It must have caught on later

<sup>126</sup>(\*) (May 31) On this subject, see note no. 84, following the note on "Silence" (no. 84).

<sup>127</sup>(\*\*) According to the duality theme that Verdier pursued for a few years after my departure, in the context of analytic spaces close to the one in which I had developed it, there is an impression of continuity as in Berthelot's case. But it seems to me that this has been a bit of a "routine continuity", whereas the one whose signs (or absence of signs) I'm looking for above all is a creative continuity, continuing an initial impulse into the unknown. ...

<sup>128</sup>(\*) (June 7) I hesitated to hazard this assessment, which could be interpreted as minimizing the originality of Mebkhout's theory. This would not be at all in line with my thinking, and all the more so as I have an excellent opinion of the abilities of each of my cohomology students (when they are not blocked by prejudices alien to mathematical common sense). My friend Zoghman himself dispelled any scruples I might have had, saying that he was convinced that "normally", it was my students who should have been developing his theory from the very beginning in the 70s. At a certain level, they're surely the first to be convinced: it's they, or Deligne, who **should have** developed it - and with the general degradation of morals, that's all it takes to behave as if they (or Deligne) really were! On this subject, see the notes "Le Colloque" and "La mystification", n°s 75' and 85'.

(See note "L'inconnu de service et le théorème du bon Dieu", n° '48.) In fact, he was so "blocked" by his burial syndrome that, until October 1980, he failed to perceive the importance of Mebkhout's work - and when he finally did, it was in the grave-digging mood we all know (see notes n° s 75 to 76).

As far as I'm aware, Verdier's work since his thesis defense has essentially been limited to redoing in the analytical context (which sometimes presents additional technical difficulties) what I had done in the coherent schematic framework, without introducing any new ideas. It's even rather extraordinary, with the reflexes he was supposed to have developed, and well-informed as he was, that he didn't come across Mebkhout's theory himself, by dint of turning his crank - and that he didn't at least recognize that his "pupil" was doing some interesting things, which had escaped him (as they had escaped Deligne).

To tell the truth, while intrigued by the question of the relationship between discrete coefficients and coefficients continuous, I hadn't really had any inkling of Mebkhout's crystalline theory, which would blossom in the decade following my departure. On the other hand, there was a vast theme, arising from my <sup>reflexions</sup> of cohomology both commutative and non-commutative of the fifties (1955-1960), and which was just beginning (in the "commutative" context i.e. in terms of additive categories) in Verdier's work, started in the early sixties and left behind after his defense (see note n° 81). The non-commutative aspect was initiated later in Giraud's thesis, which developed a geometric language, in terms of 1-fields over a topos, for non-commutative cohomology in dimension  $\leq 2$ . By the second half of the sixties, the inadequacy of these two primers was quite obvious: both in terms of the inadequacy of the notion of "triangulated category" (teased out by Verdier) to account for the richness of structure associated with a derived category (a notion destined to be replaced by the considerably richer notion of **derivator**), and in terms of the need to develop a geometric language for non-commutative cohomology in any dimension, in terms of n-fields and  $\infty$ -**fields** over a topos. One sensed (or I sensed) the need for a synthesis of these two approaches, which would serve as a common conceptual foundation for homological algebra and homotopic algebra. Such a work was also in direct continuity with Illusie's thesis work, in which both aspects are represented.

Bousfield-Kan's seminal work on homotopic limits (Lecture Notes n° 304), published in 1972, was also in line with this diffuse program, which since at least 1967 had been just begging to be developed. In January of last year, without yet suspecting that I would be embarking on the Pursuit of Fields a month later, I submitted to Illusie some thoughts on the "integration" of homotopy types (familiar to homotopists as "homotopic (inductive) limits"), at a time when I was still completely unaware of the existence of Bousfield and Kan's work, and that this type of operation had already been examined by others than myself. It turned out that Illusie was equally unaware of it, despite the fact that he was supposed to have remained in homologous-homotopic waters for all the time since my "death" in 1970! This just goes to show how far he seems to have lost touch with certain realities that are part and parcel of the fundamental thinking he himself had been pursuing in the sixties<sup>129</sup> (\*). He must have made his own little hole, from which he hardly ever emerges. ...

□ With the disdain that has befallen the very notion of topos and all the "categorical nonsense", it is not surprising

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<sup>129</sup>(\*) This notion of "integrating" homotopy types had come to me again, in the context of unscrewing stratified structures, which I took up at the end of 1981.

that Giraud now has a total disaffection for what had been his first major theme of work. It's true that Deligne, with the exhumation of motives two years ago, pretended to have suddenly discovered the interest of the arsenal of non-commutative cohomology, sheaves, links and consorts, as if he himself had just introduced them, along with motives and motivic Galois groups<sup>130</sup> (\*). It's doubtful that this kind of circus will rekindle a flame that he himself has worked so hard to extinguish. ... In February last year, I sent Giraud a copy of the twenty-page letter that became Chapter 1 of the opening chapter of *La Poursuite des Champs*. It's a non-technical reflection, in the course of which I managed to "jump with both feet" over the "purgatory" that had once stopped Giraud (and many others) from handling the notion of "non-strict"  $n$ -category (which I now call " $n$ -field"), which remained heuristic and yet was visibly fundamental. This was the start of the *Poursuite des Champs*. When we met (on friendly terms) last December for Contou-Carrère's thesis defense, I learned from Giraud that he hadn't even had the curiosity to read the letter! I got the impression that he'd drawn a long line under such things. The idea that there might be some rich substance, in a direction he had long since abandoned, didn't even seem to occur to him. I tried, unsuccessfully I'm afraid, to get him to understand that there was a juicy and vast work here that had been waiting for nearly twenty years to be done, and which I finally got round to in my old age, to at least give a broad outline, under the dictation of things themselves, of a rich substance that the "deceased" me continues to feel strongly about, while my students have long since forgotten about it.

Jouanolou also abandoned a research direction he had just begun with his thesis. This direction had become the object of the disdain of a fashion established by the very person who had provided him with a master technical idea for the theme he had chosen. With the "rush" on triangulated categories with the Colloque Pervers three years ago, this same Deligne suddenly pretended (without laughing) to discover the big job of foundations in perspective, the lack of which is suddenly felt at all ends, and which he had  $\square$ été the first to discourage for ten years - The need for such a job was quite obvious to me as early as 1963/64 with the beginnings of étale cohomology; and for Deligne just as much, from the moment he started hearing about  $l$ -adic cohomology and triangulated categories, i.e. when he arrived at my seminar the following year. Beyond the construction of "constructible triangulated categories" on the ring  $\mathbb{Z}_l$  (above a basic scheme, let's say), and the development of the formalism of the "six operations" within this framework (something accomplished, it seems to me, in Jouanolou's thesis), to make analogous work by replacing the base ring  $\mathbb{Z}_l$  by an arbitrary (more or less?) Noetherian  $\mathbb{Z}_l$ -algebra, e.g.  $Q_l$  or an (algebraic?) extension of  $Q_l$ . This is one of the things for which time has been ripe for some twenty years, and which are still waiting to be done, when the wind of contempt that has blown over them has died down... .

The natural continuation of Ms. Raynaud's work (weak Lefschetz theorems in staggered cohomology, in terms of 1-fields) would have been placed in a context of strictly taboo  $\infty$ -fields, let's not talk about it! The same goes for Ms. Sinh's work, begun in 1968 and completed only in 1975 - a natural continuation would have been the notion of an enveloping  $\infty$ -category of Picard of a so-called "monomial" category, or of triangulated variants of such a category<sup>131</sup> (\*) - let's not! Another was to transpose her work in terms of fields onto a topos - what a horror! As for Monique Hakim, she too had the misfortune to write her thesis on a subject which, in the times since my untimely departure, looks a bit ridiculous on

<sup>130</sup>(\*) See "Souvenir d'un rêve. . . - or the birth of motifs", note n° 51.

<sup>131</sup>(\*) See "Souvenir d'un rêve. . . - or the birth of motifs", note n° 51.

edges - relative diagrams on a locally ringed topos, I ask you! His little book on the subject, published in Grundlehren (Springer), must sell three or four copies a year - no wonder I've got bad press there, and they're not too keen to accept any text I might recommend. For me, it was a first test-step towards a "relativization" of all "absolute" notions of "varieties" (algebraic, analytic, etc. . . ) on general "bases", the need for which is obvious to me (91<sub>3</sub> ). It's true that we've done just fine without them until now. But it's also true that we've done without maths for the two million years we've been around.

The fact remains that Monique Hakim, who was not motivated to write her thesis in the same way as I was to offered it to him, surely had no desire to keep any contact with a theme which (detached from p . 389

In the context of a favorable consensus, or stubborn thought pursuing a tenacious and sure vision against all odds, it can no longer make the slightest sense.

As for Neantro Saavedra Rivano, he seems to have disappeared entirely from circulation - I can find no trace of his name even in the official world directory of mathematicians. What is certain is that his rather categorical thesis subject could hardly have been in good press with the gentlemen who decide what is serious and what is not. The most natural continuation of this thesis, in my opinion, would have been neither more nor less than this "vaste tableau des motifs", a theme decidedly a little broad for this student's more modest aims. Yet he ended up having the unexpected honor of having his thesis redone ab ovo et in toto by one of these great gentlemen himself, barely two years ago. (On this subject, see the notes "L' Enterrement - ou le Nouveau Père" and "La table rase", n° s 52 and 67.)

Finally, the only ones among my twelve "pre-1970" students for whom it's not too clear to me whether or not there was a more or less drastic or profound **break in** their work, compared to the one they had to follow in my contact, are Michel Demazure and Michel Raynaud (91<sub>4</sub> ). All I know is that they've continued to do maths, and that they're part (as you'd expect, given their brilliant means) of what I called earlier "the great mathematical world".

The foregoing brief reflection, based on what is sometimes very little data, is of course largely hypothetical and very approximate. I hope that those mentioned here will forgive me for any perhaps gross errors of assessment, which I'll be happy to rectify if they'd be so kind as to let me know. Here again, I realize that everyone's case is surely different from everyone else's, and represents a much more complex reality than someone as distant as me can reasonably apprehend, let alone express in a few lines. All these reservations aside, I have the impression that this reflection has not been in vain, for me at least, to identify by a few concrete facts, a still vague impression that had emerged yesterday (and which was undoubtedly present at an informal level for many years): that of a **break** that was made p . 390

in many of my students in the aftermath of my departure, reflecting on a personal level the sudden disappearance, overnight, of a "school" to which they must have felt a part during crucial formative years in their mathematical profession.

**Note** 91<sub>1</sub> (May 22) I've just come across an article-survey from the Colloque "Analyse *p*-adique et ses applications" at CIRM, Luminy (September 6-10, 1982), by P. Berthelot, entitled "Géométrie rigide et cohomologie des variétés algébriques de car. *p*" (24 pages), which outlines the main ideas for a synthesis of Dwork-Monsky-Washnitzer cohomology and crystalline cohomology. The initial ideas (and the very name) of crystalline cohomology (inspired by Monsky-Washnitzer cohomology), and the idea of complementing these with the introduction of sites formed by rigid-analytic spaces, ideas that I had introduced in the

In the sixties, they became the daily bread for all those working on the subject, starting with Berthelot, whose thesis developed and fleshed out some of these initial ideas. Nevertheless, my name is conspicuously absent from both the text itself and the bibliography. Here we have a fourth clearly identified pupil-croquemort. Who's next?

(June 7) It's a remarkable fact that more than fifteen years after I introduced the starting ideas of crystalline cohomology, and more than ten years after Berthelot's thesis established that the theory was indeed "the right one" for clean and smooth schemes, we still haven't reached what I call a situation of "mastery" of crystalline cohomology, comparable to that developed for stellar cohomology in the SGA 4 and 5 seminar. By "mastery" (in the first degree) of a cohomological formalism including duality phenomena, I mean no more and no less than full possession of a six-operation formalism. While I'm not "in the know" enough to be able to appreciate the difficulties specific to the crystalline context, I wouldn't be surprised if the main reason for this relative stagnation lies in the disaffection of Berthelot and others for the very idea of this formalism, which makes them neglect (just like

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Deligne's Hodge theory, which remained in its infancy) the first essential "level" to be reached in order to have a fully "adult" cohomological formalism. These are  $\square$  the same kind of dispositions

which have surely led him to overlook the relevance of Mebkhout's point of view to his own research. NB When I speak here of "crystalline cohomology" in a context where one abandons assumptions of cleanliness (as is necessary for a "fully grown-up" formalism), it is understood that one is working with a crystalline site whose objects are (power-divided) "thickenings" that are not purely infinitesimal, but are "proper" (power-divided) topological algebras. The need for such an extension of the primitive crystal site (which for me was only a first approximation for the "right" crystal theory) was clear to me from the start, and Berthelot learned it (along with the initial ideas) by null other than myself. A written allusion to this link can be found in *Esquisse Thématique*, 5 e.

**Note 91<sub>2</sub>** It's quite extraordinary that nobody but me seems to have realized that Mebkhout's unnamed theory was an essential new part of a crystalline theory. As someone who has been completely "out of cohomology" for nearly fifteen years, I realized this as soon as Mebkhout took the trouble to explain to me what he had done last year. In any case, when I mentioned the matter (as a matter of course) to Illusie, he seemed to see it as a rather "kooky" combination of things (*D-Modules* and crystals) that really had nothing to do with each other. Yet I know first-hand that he has a mathematician's flair, and so do my other students (cohomologists in this case, starting with Deligne) - but I can see that in certain situations, he's no longer any use to them... ... The more I think about it, the more I find it extraordinary that in such an atmosphere, Mebkhout still managed to do his job, without letting his own mathematical flair be defused by the total incomprehension of his elders, so far above him. . .

**Note 91<sub>3</sub>** It was especially since my lectures at the Séminaire Cartan on the foundations of the theory of complex analytic spaces, and on the precise geometrical interpretation of "level modular varieties" à la Teichmüller, towards the end of the fifties, that I understood the importance of a double generalization of the common notions of "variety" we've been working with so far (algebraic, real or complex analytic, differentiable - or  $\square$  subsequently, their "moderate topology" variants). One is to broaden-

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the definition so as to admit arbitrary "singularities", and nilpotent elements in the structural bundle of "scalar functions" - along the lines of my foundational work with the notion of schema.



The other extension is towards a "relativization" above suitable locally annelated topos ("absolute" notions being obtained by taking a punctual topos as a base). This conceptual work, matured for over twenty-five years and initiated in Monique Hakim's thesis, is still waiting to be taken up. A particularly interesting case is that of the notion of relative rigid-analytic space, which allows us to consider ordinary complex analytic spaces and rigid-analytic spaces over local bodies with variable residual characteristics, as "fibers" of the same relative rigid-analytic space; just as the notion of relative scheme (which has finally become commonplace) allows us to link together algebraic varieties defined over bodies of different characteristics.

**Note 91<sub>4</sub>** While Demazure's thesis work, like Raynaud's, makes essential use of a consummate schematic technique they learned from me, the essential ideas in their respective works are not part of the "Grothendieckian" panoply, which distinguishes their work from that of my other students of the first period. It's possible that this circumstance resulted in a continuity in their work, free from a rupture due to the effect of the "master's burial syndrome". This doesn't necessarily mean that this syndrome didn't affect one or the other in another way. Three years ago, I was struck by Raynaud's attitude towards Contou-Carrère's work on local relative Jacobians. The results announced are profound, difficult and beautiful, and go far beyond a simple generalization of "well-known" things. There's an unexpected link with Cartier's theory of typical curves, some wonderful explicit formulas - all entirely within Raynaud's (and my) grasp. The freshness of his welcome must have weighed decisively in Contou-Carrère's strategic retreat, abandoning for profit and loss a subject in which he had invested himself wholeheartedly and which, it may have seemed, was only going to get him into trouble. ... <sup>132</sup>(\*). My letter to him, in which I expressed my (pained) surprise at his insensitivity to the beauty of these results, went unanswered.

### 15.3.10. . . and the chainsaw

**Note 92** □ When I moved to the area nearly four years ago, there were not far from p . 393

me a beautiful cherry orchard. Often, when I went for a walk, I'd go and have a look. It was a pleasure to see these thick cherry trees, in their prime, with their powerful trunks, which seemed to have always been one with this piece of land, where wild grasses proliferate freely. They must not have known about fertilizers or pesticides, and in cherry season, that's where I'd go to pick the tasty ones. There must have been twenty or thirty trees.

One day, when I went back there, I saw all the trunks cut down to man-height, the crowns slumped on the ground next to the trunk, stumps in the air - a vision of carnage. With a good chainsaw, it must have been done in an hour or so. I'd never seen anything like it - when you cut a tree, you usually take the trouble to bend down and cut it flush with the ground. There's a shortage of cherries, of course, and this cherry orchard wasn't going to produce tons, that's for sure - but these stumps of trunks said something other than shortages and yields... .

Yesterday I had that feeling again, of a vigorous trunk, with powerful roots and generous sap, with strong, multiple branches extending its momentum - cut clean off, at man's height, as if for pleasure. It was taking the trouble to look at the main branches one by one, and seeing each one cut off, that finally made me see what had happened. What was made to unfold, in the continuity of a momentum,

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<sup>132</sup>(\*) For further details, see sub-note no. ° 95<sub>1</sub> to note "Cercueil 3 - ou les jacobiniennes un peu trop relatives", no. ° 95.

of a deep-rooted inner necessity, has been sliced clean through, with a clean slice, to see itself designated for all to see as an object of derision.

This reminds me of the "misunderstanding" Zoghman referred to, which supposedly took place between me and my students (except Deligne). What's clear, in fact, is that neither impetus nor vision were communicated from me to any of my students (apart from Deligne, who is decidedly "apart" indeed!). Each of them assimilated a technical baggage, useful (and even indispensable) for doing a well-done job on the subject they had chosen, and which could even be of use to them later on. I can't say whether there was any hint of something else, going beyond that. If there was

p. 394 had, it didn't stand a chance against the chainsaw, which quickly trimmed it... ..

I'm well aware that if there are still people doing maths - and unless they give up completely the kind of maths we've been doing for over two millennia - they won't be able to resist reviving each and every one of the branches I see lying inert. Some of them have already been taken over by my friend-with-the-saw, and it's quite possible, if God gives him life, that he'll do the same with some or all of them. Most of them, however, are no longer in his style. But perhaps he'll also eventually tire of constantly substituting himself for someone else, which is surely very tiring and not at all profitable, and just be himself (which isn't bad at all).

# 16. D) IN-GROUND

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## 16.1. X Le Fourgon Funeral

### 16.1.1. Coffin 1 - or the grateful D-Modules

**Note 93** □ (May 21) It's been a couple of weeks now that my thoughts have been lingering on my "bon teint" students, those

"before". Each day, the reflection appeared as a "final addition", as a matter of conscience, to a reflection that seemed (practically) finished. More than once, it was an innocuous footnote, carelessly branching off from the previous day's or the day before's reflection, that grew and grew to the dimensions of an autonomous "note". Each time, it quickly found its name, distinguishing it from all the others, and inserting itself into its funeral procession, just in the right place, as if it had always been there! Every other day, I was there to redo (each time with pleasure) at least the end of the table of contents, which seemed to be closed and which was then lengthened by two or three new participants in the Procession, when it wasn't a whole new procession...

...

This Procession ends up taking on worrying dimensions - no one's ever going to want to read all that! But if it grows so long, it's not, to be honest, for the dubious benefit of a hypothetical reader, but first and foremost for my own benefit - just like when I do maths. I've never had any regrets about having embarked on these "last complements", which I embark on each time as if against my will. By dint of these last supplements, I've learned many things that I wouldn't have been able to learn otherwise, by doing without a

"piecemeal" reflection. And one by one, these things came together to form a vividly colored, vastly proportioned and multi-faceted picture. Even now, I can see that it's not entirely finished - there are still two places that seem to need a final brushstroke.

I think it's time, after my "good-natured students", to talk a little about the **buried**.

p. 396 -of those who "with me are entitled to the honors of this funeral through silence and disdain". No more than I or those who bury with gusto, these buried are not saints and have no vocation for martyrdom. I don't think there's one of them who didn't resent me□for the trouble I was quite unwittingly getting him into (simply because of

that he'd been unwise enough to bet on me, on a certain approach to mathematics and on a certain style. . . ) - or that he had at least tried to distance himself from me, once he'd recognized that the bet was definitely a loser<sup>1</sup> (\*). As I've seen, this is a wasted effort - once spotted, it's all over, and to stand out is to fuel contempt, to provide tacit justification, instead of disarming it. More than once, too, and in many ways, I've seen the roles of burier and buried rubbed together and confused<sup>2</sup> (\*\*). These aspects of ambiguity are undoubtedly the cause of a long-standing reluctance on my part to talk about the "buried" in any more detailed way than the allusions I've already made to them in passing. It's possible that, with the possible exception of Zoghman, none of the other three I know would be grateful to me for giving him "publicity" here, as if I hadn't given him enough trouble as it is.

Like so many times in the course of Harvesting and Sowing, I'm finally overcoming such reluctance in myself. I tell myself that even when it comes to people who have had to suffer because of me (because of a choice they made at a given time and where, for one reason or another, they were happy with it, although they had no more idea than I did of the disadvantages attached to their choice) - even towards them my role is not to help them evade a very real situation, in which they are involved whether they like it or not, and which surely makes sense even if it has serious disadvantages.

Before I embark on the black series of the four coffins of my late co-deceased and co-buried, I should perhaps cheer up the reader with a less funereal note. First of all, in my dealings at the "local" level of my University's Institute of Mathematics, I have by no means had the experience that the good I might say about a candidate for a post, or the fact that a candidate was one of my students (after 1970, needless to say), or that his work was influenced by mine, necessarily worked against him. A

p. 397 such an attitude of systematic boycott uniquely characterizes the relationship of the mathematical "big world" to my person, and by extension, to those who □appear to be linked to me "after 1970". This boycott has been

As far as I've been able to ascertain, his service has been virtually faultless in the fourteen years since I left, albeit with two modest exceptions. One concerns a student who, after a promising start, was supposed to be working with me on a state doctorate thesis on a most tantalizing subject, and whose application for a position as assistant professor at USTL had been rejected by my University's Commission of Specialists. He was "drafted" to the national level, with the help of Demazure, to whom I had written about this student's work<sup>3</sup> (\*). In addition, on two occasions, the journal **Topology** accepted articles by students of mine: an article "Factorisations de Stein et découpes" by Jean Malgoire and Christine Voisin, and a forthcoming article by Yves Ladegaillerie, containing the central result of his 1976 thesis (See note n° 94).

I've already had occasion to talk about Zoghman Mebkhout in particular, and I'll mention him again here "for the record"<sup>4</sup> (\*\*). Mebkhout began to draw inspiration from my work in 1974, I believe, and has continued to do so ever since.

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<sup>1</sup>(\*) (February 1985) I am aware of a total of seven or eight (short) publications, outside my University, presenting (in summary form) work done with me and inspired by me, since I have been in Montpellier. My name is absent from all of them.

<sup>2</sup>(\*\*) (September 2) In different ways from one to the next, each of them has at some point internalized and taken on board the disdain for their work, acquiescing to the consensus that dismisses this work or classifies it as "uninteresting".

<sup>3</sup>(\*) At the "practical" level of promotion or accession to a position and status, the record of my teaching activity since 1970 boils down, in all, to two accessions to a position with status at stake, once as a maître-assistant and another time as an assistant. By a strange irony, on both occasions, these accessions signalled a sudden and radical halt to all research activity on the part of the person concerned.

<sup>4</sup>(\*\*) Apart from Introduction (6) (L'Enterrement), Mebkhout is mentioned in the notes "Mes orphelins", "L'inconnu de service



to this day. I am not aware of any of my "official" students having produced a work of comparable scope - although Mebkhout's work is necessarily affected by the conditions of adversity in which it had to be pursued. As I said in the Introduction (6), for the past four years, Mebkhout's ideas and results have been used by everyone, while his name has been carefully withheld<sup>5</sup> (\*\*\*). It's a mystery to me how my friend was able to continue doing maths, while at the same time

suffering disdain, then iniquity as a kind of inescapable fate - a fate that came to him through people he must have (and still must) felt vertiginously  $\square$  above him<sup>6</sup> (\*), people he p . 398

must have first heard them referred to as the "gods of the stadium", at a time when he (like myself) was a modest emigrant student with precarious resources. When he defended his thesis in 1979, he had an assistant's post in Orléans. He then did everything he could to get into the CNRS, coming back three times - on the third occasion (in October 1982) he was finally given a position as a research fellow (equivalent to that of assistant or maître-assistant at the University). This gave him, if not a statutory guarantee, at least a degree of relative security.

Of the four "co-buried" mathematicians of whom I am aware, Mebkhout is the only one who has continued to pursue his work against all odds, trusting his mathematical instincts without letting himself be stopped by considerations of prudence and expediency that might have inspired a merciless fashion. There was in him, who was not of a combative nature, an elementary **faith** in his own judgment, which is also a **generosity**, and which (much more than cerebral "means") is the primary condition for doing innovative and profound work. My idea of his work is surely incomplete. From what I know of the main part of his work, it seems to me that with the brilliant means at his disposal, placed in an atmosphere of warm and active sympathy, he could have accomplished it, and brought it to greater maturity, in three or four years instead of ten, and in joy and not in bitterness. But three years or ten, and "maturity" or not, the remarkable thing is that the innovative work appeared, and that it could have appeared in many such conditions.

### 16.1.2. Coffin 2 - or cut to size

**Note** 94 Yves Ladegaillerie began working with me in 1974. It was "just by chance", in a hollow moment at his place - I submitted to him some naive reflections on the plunging of topological 1-complexes into surfaces, at a time when I knew nothing about surfaces (except the notion of and he even less so. It was a bit grothendieckery (where I come from, anyway, it always starts-days like that. ... ), and it clicked  $\square$  with him more or less, until one day it finally "clicked", I don't know p . 399 when and why. Perhaps it was when an obviously juicy question was emerging, a certain key conjecture about determining the isotopy classes of a compact 1-complex in a compact oriented-edge surface. True - false? That was the suspense, which lasted a good six months,

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et le théorème du bon Dieu", "L'Iniquité - ou le sens d'un retour", "La Perversité", "Rencontres d'outre-tombe", "La Victime - ou les deux silences", "Le Pavé et le beau monde", "Thèse à crédit et assurance tous risques" (notes n°s 46, 48', 75, 76, 78, 78', 80, 81).

<sup>5</sup>(\*\*\*) Legion of people acted as gravediggers at this funeral, in which practically the entire Colloque de Luminy (June 1981) took part. Apart from my cohomology students (see note "Mes élèves (2) : la solidarité", n° 85), those whose professional good faith is directly and gravely in question here and of whom I am aware are J.L. Verdier, B. Teissier, P. Deligne, A.A. Beilinson, J. Bernstein.

<sup>6</sup>(\*) Of course, Zoghman Mebkhout is no more of an idiot than I am, and he's sufficiently in the know to have a precise idea of the work of each of my cohomologist students, and to realize its scope as well as its limits, without any propensity to idealize it. Nevertheless, inhibitions of considerable power have held him back from even the idea of publicly questioning any of them, even where malice is patent.

a year, during which Yves brought himself (and me) up to speed on the key theorems of surface theory, while pushing on with the "foundation" parts of his work. The known results made the conjecture rather plausible, but were obviously far from the mark - whereas the conjecture implied cow results from Baer and Epstein, and other things that had unusual, even suspicious, aspects. He finally managed to prove the key conjecture in the summer of 1975. Essentially, it is equivalent to a complete algebraic description, in terms of fundamental groups, of the set of isotopy classes of plungers of a triangular compact space (say) in an oriented compact-edge surface<sup>7</sup> (\*).

From the moment Yves had "hooked", he did his thesis in a year, a year and a half, results, writing, everything, and on top of it all. It was a brilliant thesis, not as thick as most of those done with me, but as substantial as any of those eleven theses. I defended it in May 1976.

The thesis is still unpublished today. It may not have been thick, but apparently it was still too thick to be publishable, among many other excellent reasons I've been given. I mention some of them in the note "On n'arrête pas le progrès" (n° 50). The story of my efforts to "place" this unfortunate thesis, one of the best I've ever had the pleasure of inspiring, would make a small book, which would surely be instructive but which I've given up writing. Among the close friends of yesteryear who had such good reasons for forgetting to read the results and burying the whole thing with their eyes closed, there is

are (in order of appearance on stage) Norbert A. Campo, Barry Mazur, Valentin Poenaru, Pierre Deligne

p. 400 -□not counting B. Eckmann via Springer<sup>8</sup> (\*). The central result will finally appear,

nine or ten years later and reduced to the bone, in a short article in Topology (shhh - I have an accomplice on the Editorial Board of this esteemed journal. . . ). The rest of the work, on the one hand, demonstrated things that everyone has always used without demonstration (and we certainly did well without it!), and on the other hand, developed typical grothendieckeries, completely contrary to usage and good morals. I'm well aware that if my friend Deligne doesn't take it upon himself to "discover" them in the next ten years, others won't be able to resist repeating them in thirty or fifty years' time, since my healthy instinct tells me that these are fundamental things. They have been a precious thread in my Anabelian cogitations, and if God lends me life, I'll have ample opportunity to refer to them in the part of Mathematical Reflections developing the yoga of Anabelian algebraic geometry.

This adventure was a revelation for me, the first of its kind - the revelation of something I didn't become fully aware of until the reflection of l'Enterrement. I've tended to forget it ever since, my mind being absorbed elsewhere. Yves Ladegaillerie, one of the most brilliant students I've ever had, understood right from the start that to be accepted in today's mathematical world, it's not enough to put in the hard work and meet all the requirements of excellence. Having more than one string to his bow, for seven years he devoted himself to more down-to-earth tasks and to the

<sup>7</sup>(\*) The "analogous" statement in the non-oriented case is false - decidedly a tricky result, carefully "sliced" into a set of equally "plausible" but nonetheless false hypothesis-conclusions! For further comments on Ladegaillerie's work, see Esquisse d'un Programme, especially the beginning of par.3.

<sup>8</sup>(\*) I don't know Eckmann personally, and my correspondence to have Yves' thesis published by Lecture Notes was with Dr. Peters, in charge of LN at Springer. I believe that through the fifteen or so volumes of LN that were published by me (SGA in particular) or by students (theses) in the sixties, I was among those who contributed by their endorsement to the credit and unprecedented success of this series, still in its infancy. The reason given for rejecting the work I recommended (that they didn't publish theses) was a joke.

My first experience of the New Look in correspondence also dates from this episode: with a truly impressive ensemble, A. Campo, B. Mazur, V. Poenaru and Dr. Peters refrained from honouring me with a reply to a second letter, when naively (I have a slow comprehension. . . ) I returned the charge, after their reticent reply which showed that they hadn't bothered to acquaint themselves with the results set out in the introduction to Ladegaillerie's work.



less problematic returns. He is fortunate to have held a position as assistant professor

eve

n before his unfortunate encounter with me, □ providing him with security that his misadventure did not jeopardize. L'anp

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last a mathematical spark seems to have awakened again, on a theme very close to those I've been interested in over the last few years - hyperbolic geometry à la Thurston and its relationship to the Teichmüller group. It's even possible that we'll go a little way together again, or that he'll take his own personal walk, just for the fun of it, and without expecting any return other than that which mathematics itself can give. He knows that if he expects any more, it's in his interest to change his interlocutor or fellow traveller (and even his past. . . ).

### 16.1.3. Coffin 3 - or jacobiennes a little too relative

**Note** 95 My first encounters with Carlos Contou-Carrère were in the corridors of the Institut de Math, shortly after my arrival in Montpellier in 1973. He'd corner me in some obscure corner and pour a torrent of mathematical explanations on me, before I'd even had time to apologize politely and duck out of the way. What he was pouring out at an impressive rate went right over my head, without him even pretending to notice, or being the least bit bothered when I timidly let him hear. He was in desperate need of someone to talk to, and I wasn't his only "unwilling interlocutor". This was at a time when I was absolutely not into maths. For a year or two, I would run away as soon as I saw his (easily repaired) silhouette appear at the end of a corridor. It was like that until Lyndon, who had been at Montpellier for a year as an associate professor, let me know that Contou-Carrère had some unusual resources and was about to be shipwrecked for lack of knowing how to use them. Until then, the question of whether or not what Contou-Carrère was spouting at me held water, and whether or not he had the means, hadn't even crossed my mind, so far away was all that. Perhaps Lyndon's suggestion came at a time when I was beginning to take an interest in mathematical questions again. In any case, I took the bit by the teeth and asked Contou-Carrère if he would explain something he had done, so that I could understand him. I suspect I was the first to ask him such a thing, at least in the many years he'd already been in France. It wasn't easy to get him to explain something, but it wasn't easy.

by no means impossible, and it was well worth the effort. I soon realized that Lyndon □ hadn't been wrong - that Contou-Carrère was full of ideas that just needed to be carefully worked out and developed, and he had an immediate and very sure intuition in practically any mathematical situation that could be put before him. This rapidity and certainty of intuition, even in things with which he was completely unfamiliar, was beyond me and impressed me - the only other pupil where I experienced it to a comparable degree was Deligne<sup>9</sup> (\*). On the other hand, he had an almost total block against writing! Incredibly, he could do maths **without writing** - God knows how he managed to do even that little, not to mention communicating with others, where he was a total "shipwreck" (see above).

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If I had something urgent and useful to teach Contou-Carrère, it was the art of writing, or more frus- tely, just making him understand that maths is done by **writing** it. I must have tried for two years, maybe three, until 76 or 77<sup>10</sup> (\*\*), without being quite sure whether I really succeeded entirely. His first major work written entirely in black and white was his thesis on the cycles of

<sup>9</sup>(\*) I'm not sure I've come across it in any other mathematicians, except Pierre Cartier (who impressed me in his youth with this remarkable ability) and Olivier Leroy, mentioned in the following note.

<sup>10</sup>(\*\*) (June 7) Checked, it was until February 1978.

p. 403 Schubert, defended only last December (1983)<sup>11</sup> (\*\*\*). Between 1978 and the present day, our relations have been very episodic, my role being practically confined to supporting him as best I can in the nom- many occasions when he had found himself trapped in one way or another in his professional life, constantly suspended on the most precarious assistant-delegate positions.

For two or three years, I had been trying to provide Contou-Carrère with the foundations of a precise and flexible mathematical language and some principles of systematics. With this background, and his resources and wealth of ideas, he was spoilt for choice as to what to branch out into. Rather than start with ideas of his own, he branched off into the theory of relative local and global Jacobians, which I had mentioned to him as a possible thesis topic. Once I'd left him to his own devices, in the space of barely a year he produced some very fine work, part of which is announced in a note to CRAS (95<sub>1</sub>). Going all the way would have meant a few years of exciting, highly motivating work, while at the same time learning all the finer points of schematic technique. I had no doubts at the time - it was obvious to me that Cartier, Deligne and Raynaud would all give a warm welcome to the work already done, which was profound, difficult and unexpected in many ways. Cartier was delighted to see some of his old ideas take on new relevance. Raynaud, on the other hand, was indifferent, as was Deligne, who kept the complete manuscript in his drawers for six months, without deigning to give any sign of life<sup>12</sup> (\*).

It was two against one - enough to feel the wind. The slightly too relative Jacobians are sine die written off. The chainsaw did its job...

That didn't mean we didn't have a few mishaps at Contou-Carrère, a detailed account of which would be useful.

well another little book, which I willingly give up writing. It's around this time, I think, that for the first and only time since I left (in 1970) the institution I'd been the only one to work in for four years (1958- ), I decided to write another little book.

p. 404 62) to represent and make credible "in the field", during the years when it still had no roof over its head to her - it's the only time I've ever taken it upon myself to recommend someone for an invitation (for a year, in this case), at a time when Contou-Carrère was in danger of finding himself out of a job and on the street. I knew that the person I was recommending, just as unknown as Hironaka, Artin or Deligne had been when I warmly welcomed them to the IHES, would do as much honor as they had to the institution that welcomed him. Of course, I didn't fail to say so. Fortunately for Contou-Carrère, his position as assistant delegate (admittedly unworthy of the honor of an invitation to such a select institution) was finally renewed<sup>13</sup> (\*).

I wasn't that surprised by this episode, knowing Deligne's disposition even then, and given that

<sup>11</sup>(\*\*\*) It's a long piece of work (which I haven't read) in which he carefully develops ideas in which I have no part, giving, among other things, an explicit resolution of the singularities of all "Schubert"-type cycles - something nobody has been able to do before him. For once, when he did write a formal essay, he was criticized for it being too detailed (not to mention that his statements were too general. . . )! For my part, if I have a criticism to make, it would be in the opposite direction: while Contou-Carrère asserts that his methods should apply to all types of semisimple groups and Schubert cycles, he has only done the work in the case of the general linear group - so he hasn't gone all the way with the work that needs to be done on the precise question: description of the resolutions of equivariant singularities of universal Schubert cycles, **and** of the singular loci of said Schubert cycles. This lacuna seems to me to be a legacy of this "block" against piecework and writing, which had long been his main handicap.

<sup>12</sup>(\*) Contou-Carrère had been ahead of the game, however, and didn't breathe a word in his note about me, who had provided him with the initial program. It was all in vain - no matter how much he put into it, there's an unmistakable "style" attached, whether you like it or not, to certain themes, which it's best to avoid if you want to make a career in maths today. (June 7) After checking with the person concerned, I note that I am confusing here two different episodes concerning Contou-Carrère's work on Jacobians. See the following note (n° 95) for further details and precise references.

<sup>13</sup>(\*) I can't complain, since five or six years later, on the occasion of IHES's twenty-fifth jubilee last year, I was indeed honored with an invitation, and even given the choice between the solemn reception with the minister's speech, or a subsequent week's stay at IHES, again all expenses paid (I was assured). I told my old friend Nico Kuiper that it was very kind of him to have thought of me like that, but that I didn't travel anymore at my age... ..

Nico Kuiper had warned me that everything depended on him in this particular case. (It didn't even occur to me to suggest to him that this might also apply to the other members of the Scientific Council, given the case in point... .) The episode that touched me the most, however, of all Contou-Carrère's misadventures (my "protégé", as Verdier had taken it for granted to call him in a letter....), takes place in October 1981, in connection with his application for a teaching post in Perpignan. Colleagues in Perpignan (where he had his position as assistant delegate) surely appreciated the presence among them of someone who was at ease and could be consulted in virtually all branches of mathematics. When a teaching post became vacant, they put him forward as the sole candidate for the position. - a rare move, which made it clear that they wanted him and no one else in the job. C.C. had relatively few publications apart from his doctoral thesis in Argentina with Santalo, mostly notes to CRAS, announcing results (some of them profound), but without demonstration. No one had ever suggested to him that, in these times of unprecedented competition, it's better to have articles with complete demonstrations as "exhibits".

which I'd preached to him enough myself, but from a less utilitarian point of view<sup>14</sup> (\*\*). Still that Contou-Carrère's candidature was deemed inadmissible by the Comité Consultatif des Universités and the p. 405 case referred. The thing that struck me at the time was that neither the President of the CCU (the national body that made the decision), on behalf of the Committee, nor any of the members in a personal capacity, had the minimum respect to write, either to the main interested party Contou-Carrère himself, or at least to the Director of the Institut de Mathématiques de Perpignan, to give a few words of explanation on the meaning of this vote, which in the absence of any explanation could only be received as a stinging disavowal of the choice of the Perpignan colleagues, and as a disavowal of their only candidate as suitable to honorably fill the post for which he was proposed. The Council included three of my former students, two of whom knew Contou-Carrère personally. Of course, they knew that he had been a pupil of mine as well as theirs, especially as the dossier included a particularly glowing report by me on the candidate's work. None of them, nor any of the other members of the Council, thought about the affront represented by this vote-couperet without any other form of trial, and the torpedoing of a mathematician just as honorable as any of them.

It was this incident that, for the first time in my life as a mathematician, made me feel that "breath" I've spoken of more than once in the course of my reflections. I had already felt it four years earlier, with the episode of the<sup>15</sup> (\*) strangers. But it wasn't within the world that had been mine, blowing on **one of the theirs** - on someone who unreservedly identified with this world. It made me feel sick, for weeks; perhaps months. To free myself from an anguish that then embraced me without that I cared p. 406 to read it<sup>16</sup> (\*), I became restless, writing letters left and right, and a thirty-page text "Le Cerveau et le Mépris" (The Brain and Contempt), in a darkly humorous vein, which I finally decided not to publish<sup>17</sup> (\*\*). With hindsight, I realize that it was the perfect time to **meditate** on the meaning of this

<sup>14</sup>(\*\*) The year before, Contou-Carrère had applied for a professorship in Rennes, where he knew Berthelot and Larry Breen. His application was considered admissible by the CCU, but the position was awarded to another candidate. No one bothered to warn him that if he was to have any chance of a position, he would have to publish detailed demonstrations of the results he was announcing. The following year's disavowal by the CCU came as a complete surprise to Contou-Carrère, his Perpignan colleagues and myself. With hindsight and in the light of the present reflection, I doubt whether the situation will really change with the writing of his thesis (already declared "unpublishable" as it stands) and its defense, and whether he will have any chance of finding a teaching post in France.

<sup>15</sup>(\*) See "My farewells - or strangers", s.24. - 406

<sup>16</sup>(\*) I became aware of this anguish only during a long period of meditation the following year, when I discovered the role of anguish in my life, whose presence (chronic until 1976, and occasional after 1976) had been "the world's best-kept secret" all my life. There were highly effective mechanisms in place, which concealed all the generally recognized signs of anguish, which remained ignored by myself and those close to me.

<sup>17</sup>(\*\*) I was discouraged from publishing it by the very people for whom I was about to go to war, to whom I had the good sense to say: "We're going to war!"

that was happening. The funny thing is that what was "preventing" me from even realizing the need for in-depth meditation, was a long meditation I was engaged in at the time<sup>18</sup> (\*\*\*) and about which I've had occasion to speak) - and a meditation, what's more, on my relationship to mathematics (if not on my past as a mathematician)! She was troubled by an episode in which life was calling out to me forcefully - and I evaded the challenge by getting agitated, then diving back into "meditation". Looking back, I realize that this "meditation" didn't fully deserve the name, that it lacked an essential dimension of true meditation: attention to myself at the **very moment**. I was "meditating" on the meaning of certain more or less remote events, while ignoring a repressed anguish (perfectly controlled, it's true, as a result of a long habit of such control), a sign of my refusal to take cognizance of the message this rejected "breath" was bringing me.

But I'm getting away from my point. The torpedoing, of course, had the effect it was bound to have. The Perpignan colleagues were called to order once, and that was enough. Apparently, they don't even have an assistant delegate position any more, at least not for Contou-Carrère. He has found a replacement at short notice in Montpellier, for the current year, whose incumbent will return next year.

I'm not too worried about his future, though, as it's been a while since Contou-Carrère had the wisdom to get ahead of the curve, and plugged into IT. With the means

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his brilliance, he must have been dominating the subject from on high for a long time now, while doing the math he's been

loves in his spare time. He's a father with two children, and maths in these times, with the past hanging over him, is decidedly hazardous, not to say violent. It's in his interest to have a brilliant career as a computer scientist, where no one will hold it against him that he was even the slightest bit my pupil.

**Note 95<sub>1</sub>** (June 7) It was towards the end of '77 that I submitted to Contou-Carrère a detailed work plan for a theory of local and global relative Jacobians, including, in the local case, the suggestion of "screwing back" Cartier's Jacobian and ind-group, in order to find a "complete" Jacobian with a more beautiful universal property, and which would be "autodual". I had no idea of a demonstration to propose, and didn't bother with his work after February '78, having realized that my presence inhibited rather than stimulated his abilities. He managed to "get going" within a year, and his first note "The generalized Jacobian of a relative curve, construction and universal factorization property" (concerning the global case) appeared on 16.7.1979 (CRAS t.289, Série A - 203).

The following month he found the decisive results for the local Jacobian, but didn't publish anything on the subject for a year and a half, when he published "half" (the universal property of the ordinary local relative Jacobian, unrevised with the Cartier group), in a note to CRAS dated March 2, 1981, under the name (not very convincing at first sight) "Corps de classes local géométrique relatif" (CRAS t.292, Série I - 481). As for the theory of the complete local Jacobian, even more interesting in my opinion, there is a draft note to the CRAS, which was never published, under the title: "Local Jacobian, universal Witt bivector group and tame symbol". Of course, I was informed of his results as early as 1979, i.e. a complete realization of the provisional program I had proposed to him, for which it had been necessary to overcome considerable technical difficulties, requiring a great deal of imagination and technical power. I was aware (unless I was mistaken) only of the first note, and was astonished that he didn't publish the rest, i.e. the local part, without ever giving a clear explanation for it - but he was obviously disappointed by the reception given to this work.

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first note. After his unsuccessful application to Rennes in 1980, and given that my letter of support attached to his application file reported remarkable results on global and local relative Jacobians,

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show my text before attempting to make it public.

<sup>18</sup>(\*\*\*) See "The troublemaker boss - or the pressure cooker", s.43.

he must still have considered it prudent (in preparation for his candidacy the following year in Perpignan) to publish at least one more note on the local Jacobians, or else empty his entire bag. It was only two months later, in May '81, that he sent the draft of his third note to Deligne and Raynaud (no doubt Cartier had known about it for a long time), presumably to test the waters. (I don't think he would have had the slightest difficulty in getting Cartan to present this third note, at any time since August 1979 when he had the results in hand). Neither Raynaud nor Deligne gave him any sign of life - but in March 1982 Deligne sent him the manuscript of an article "A remark on tame symbols", dedicated to Deligne, by Kazuya Kato, which makes Contou-Carrère's theory in the case of a basic body, and conjectures its validity on any basic ring. Contou-Carrère told me about it, saying he was convinced that Deligne had communicated his results (without naming him, or giving any indication of a demonstration) to K. Kato. At the time, the thing seemed so incredible that I didn't take Contou-Carrère seriously - although now I realize that it would be quite in the usual "thumbs up!" style of my brilliant friend Deligne. Contou-Carrère seemed genuinely outraged that anyone would "presume to conjecture" about something he seemed to regard as a kind of private property. Yet Contou-Carrère himself took his conjectures from me, without even thinking it necessary to allude to me in any of the three notes<sup>19</sup> (\*)! From him to me, it must have seemed self-evident, whereas the mere presumption that Deligne would do the same to him outraged him, but he didn't dare breathe a word of it to the interested party. (I had urged him to explain himself to Deligne, which he did not. . . )

In a way, I imagine he's had to do violence to himself over the years not to publish some very fine results, in which he's had to invest his heart and soul in making them. If he's done himself such violence, it's out of concern for an economic climate that's clearly not conducive to this kind of grothendieckery. In the last few days, he was astonished to receive a letter from the same Deligne, expressing surprise that he had not published

his note on "total" Jacobians, and asking him for everything he possessed on the subject and even on others. Zoghman Mebkhout had already told me a few days before that Deligne was using these things etp that he had even named Contou-Carrère in this context. It would seem that the time is ripe for Contou-Carrère to finally recognize a child of his own, whom he has been careful to bury for nearly five years. Perhaps, who knows, the time has even come for a reconciliation between the two "pupil-enemies"; these two most brilliant of my pupils, one a medal-winning academician and the other a delegated assistant, and yet (whether they reconcile or not) for a long time two **brothers**.

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#### 16.1.4. Casket 4 - or topos without flowers or wreaths

**Note** 96 (May 22) I'd hardly be exaggerating if I said I'd never met Olivier Leroy. What is certain is that from the moment he heard about me, he decided to avoid me like the plague. His reasons, I confess, escape me. Perhaps an instinct told him that I was only going to get him into trouble, or maybe Contou-Carrère (who was very friendly with him for a long time) told him so - I may never know. All the same, I had the honor and pleasure of two substantial conversations with Leroy, which I remember very well.

The first time must have been in '76, '77, when Contou-Carrère and I went to see him at his place, out of the blue, just to talk math a bit - I don't know if we had any ulterior motives. But perhaps it was understood that Olivier was thinking of embarking on a 3° cycle doctorate, and I certainly had plenty of subjects up my sleeves. Having seen him once or twice at Contou-Carrère's, and from what Contou-

<sup>19</sup>(\*) On the subject of a certain conniving role I've often played in this kind of situation with some of my students, see the note "Ambiguity", n° 63".

As Carrère himself suggested, I had a feeling that Olivier must be a quick study, and not just in maths. It was a memorable evening for the three of us. I soon had to tell Olivier about a program for a fundamental group theory of a topos and van Kampen theorems in the toposic framework, and he seemed interested. He must have had some toposic background from Contou-Carrère's algebraic geometry seminar, and he seemed interested in having an opportunity to "get his teeth into" the language of topos on an example of concrete theory. For a good two or three hours, I had to pour over him a detailed masterpiece of the theory I had in mind to develop, which was fleshing out

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as I talked about it, and as a host of concrete situations in algebraic geometry and topology came up in my mind - situations that had to be expressed in the  $\square$ cadre toposique, and that with each times I first had to "remind" someone who was hearing about it for the first time. More than once in the course of the evening, Contou-Carrère (who has read almost everything and has a strong stomach) looked vague and disoriented, and even for him it was a lot to take in - and more than once I thought it prudent to ask Olivier if it wouldn't be better to stop for today and start again another day. I could have saved myself the trouble - Olivier was visibly fresh, bright-eyed and perfectly at ease, and I even laughed, because I couldn't believe he wasn't cracking up, but he wasn't! He was a young guy, maybe twenty years old, who must have had just a tincture of schematics, a bit of topology and topos, and he'd worked quite a bit with infinite discrete groups, I think... . It was three times nothing, to tell the truth, and with that he still managed to fill in all the blanks and "feel" effortlessly what I, an old veteran, was telling him at top speed in two or three hours on the basis of a fifteen-year familiarity with the subject. I'd never come across anything like it, or at most with Deligne, and perhaps Cartier, who had also been quite extraordinary in this line in his younger days.

Olivier was going to write his 3<sup>o</sup> cycle thesis on the subject in question. Little did he know what was waiting for him at the end. In any case, I never saw him again during the two years he worked on the project and beyond. His official boss was Contou-Carrère, but it would have been a pleasure to chat with such a hip guy. In fact, I wasn't even notified of the defense, and don't think I ever received a copy of this thesis - but I remember

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having held a copy in my hands, from someone who had been entitled to it<sup>20</sup> (\*). I couldn't say whether the defense was  $\square$ done before or after the "pouring" of the note to the CRASs where Olivier summarized his work. I discusses this casting, in some detail but without naming anyone, in the section "La note - ou la nouvelle éthique (1)" (s.33). The two mathematicians who took care of this casting were Pierre Cartier (the same one whose incredible speed of intuition had come back to me when talking about that of his young non-colleague, whom Cartier was casting so kindly and with all the regret in the world), and the other was Pierre Deligne,

<sup>20</sup>(\*) All this secrecy is all the more unusual in that I was surely, along with Contou-Carrère, the only person in the whole Languedoc region who could understand anything of Olivier Leroy's work. Needless to say, I never got my hands on Leroy's draft note to the CRAS either. Perhaps I'm deluding myself, but it seems to me that if I hadn't been so drastically sidelined that it was practically impossible for me to intervene, I would have found a way to get this unfortunate note published, through Cartan or Serre if need be, who aren't connected, but who would have trusted me if I had guaranteed the seriousness of the work. (June 7) I must have learned long afterwards that Leroy had passed his thesis, and was too busy on my side to think of asking myself then how it was that I hadn't even been informed. It didn't "click" until after Contou-Carrère's own thesis defense, for which I'm supposed to have been the thesis supervisor (x). He found a way to ensure that, as the only member of the jury, I was not entitled to the definitive, official copy of his thesis! I finally just received a copy today - he had thought (he wrote) that I "wasn't interested" in having one. . .

(x) More precisely, for a year or two, C.C. had cautiously played on two "directors" at once (you never knew. . . ), each of them unaware of the existence of a "parallel" director. I was informed of Verdier's role as director in extremis, when C.C. finally fell back on me in the spring of 1983, when it became clear that Verdier decidedly wanted his hide anyway!

with his historic remark that mathematics "didn't amuse him", (although it did "amuse" him at a young age. . . ) I should add Contou-Carrère himself, who didn't lift a finger to defend his pupil.

- This exposed him to the risk of upsetting powerful men. He must have suggested to Olivier Leroy that it would be better to forget the episode of his unfortunate thesis. What is clear, however, is that Leroy has indeed drawn a line under this episode - even if the opportunity arose to publish not just a note to the CRAS, but his entire work, I doubt he would make use of it<sup>21</sup> (\*). This time again, the chainsaw has done its job<sup>22</sup> (\*\*).

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Despite this misadventure, I still had the pleasure of seeing Leroy regularly for several months in early 1981. It was at a micro-seminar I was giving on the algebraic-arithmetical theory of Teichmüller's tower (discussed at some length in *Esquisse d'un Programme*). The only literal listeners were Contou-Carrère and Leroy. Even for an ultra-selective Parisian audience (and I know what I'm talking about), there wouldn't have been three or four of them in a whole room, so as not to be left out. To tell the truth, the reason I was doing this seminar, at a time when Contou-Carrère was entirely taken up with fine-tuning his ideas on Schubert's cycles, was for Leroy, thinking that maybe he'd catch on to such a splendid subject. He obviously "sensed" what I was doing, but had decided in advance (I think) that he wouldn't "catch on". Strange that he even bothered to come - something must have fascinated him, just as it did me, and he wasn't too clear himself about what he really wanted. When I realized he wasn't going to go for it, I called it a day. I wasn't interested in continuing a monologue in front of two spectators, no matter how brilliant they were. It was at this point that I had my second and last conversation with Leroy. I don't think I've seen him since.

There's been no real mathematical discussion between Leroy and me, apart from the one seven years ago. - which explains why I know virtually nothing about the work he did, apart from his unfortunate topological work. His misadventure must not have increased his confidence in people like me, or even Contou-Carrère or other people from the beau monde □ mathématique. I heard he was doing a seminar

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at the Faculté des Lettres, where there's a group of sympathetic mathematicians who get on well with each other. There are

<sup>21</sup>(\*) An eloquent sign of this "gros trait": in Olivier Leroy's application for an assistant's position at Montpellier; submitted during a vacancy two years ago, Leroy mentions neither the title of his post-graduate thesis, nor the name of Contou-Carrère who had been his boss, nor does he mention any personal work whatsoever. Clearly, he hadn't decided whether he wanted the job or not - which is why, despite his impressive credentials, it was awarded to another candidate with a solid track record and for whom there was no doubt as to his intentions.

<sup>22</sup>(\*\*) By an interesting coincidence, I recently heard that Cartier had dedicated one of his Bourbaki lectures to me (the first time such a thing has happened to me, I believe), and that the lecture was devoted to the theory of topos - the very topos that Cartier had deemed unworthy of inclusion in a note to the CRAS. Sign of a change in the fashion winds of recent years? Certainly not, and it all ties together again: the presentation in question concerned the use of topos in logic! My friend Cartier's touching dedication seems to me to be in the same vein as the Funeral Eulogy delivered on a special occasion last year (see the note "L'Eloge Funèbre - ou les compliments", n° 104), in which the word "topos" is pronounced (among other well-sent compliments), only to add (as a unique and eloquent comment) that they are "used today in logic" - and nowhere else, need I say, at least as long as my friends who are lavish with compliments can prevent it, by the power in their hands. . .

(Reference to Cartier's talk: *Catégories, logiques et faisceaux, modèles de la théorie des ensembles*, Séminaire Bour- baki n° 513, Feb. 1978).

I sense in the condescending (and boycotting. . . ) attitude of some (such as Deligne, Cartier, Quillen, among those who set the tone. . . ), towards innovative and profound notions such as that of topos in geometry, a phenomenal **haughtiness**. Even supposing that just one of them had the stuff (or the innocence. . . ) to draw out of nothing, as I have done with the introduction of étales and crystalline topos, a new topological vision of algebraic varieties (and from there, the means for a profound renewal of algebraic geometry and arithmetic, while awaiting topology) - there's no doubt that the very attitude of contempt that he likes to cultivate in himself and arouse in others, defuses this power of vision and renewal, for the sole benefit of a fatuity.

would expound ideas of combinatorial topology - a subject that's been right up my alley for nearly ten years. As I'm by nature a discreet person (but yes, yes!), I didn't ask any questions about what he was talking about, and I don't know whether he intended it for publication. As far as his situation is concerned, he leads a most illegal existence (although he's neither a foreigner nor in an illegal situation), doing tutorial work right and left, paid for (shhh. . . ) by I don't know what slush funds and under the noses of the treasurer-payer and the Cour des Comptes. I don't think he's made up his mind whether or not he's finally going to pursue a career in mathematics, and it must be an uncomfortable situation in the long run, Cour des Comptes or not. I'd be delighted if my edifying painting of a Funeral, in which he appears as the fourth assistant coffin, could help him dispel his perplexities, this time with full knowledge of the facts.

### 16.1.5. The Gravedigger - or the entire congregation

**Note** 97 (May 24) It was against a certain reluctance on my part that I finally decided to mention by name some of my close friends and colleagues in the mathematical world of yesteryear, whom I have seen act as "gravediggers" (or "chainsaws"), cutting short, from the outset, the attempts made by certain mathematicians of modest or precarious status, to take up some of my ideas and develop them according to their own logic, or only (as in the case of Yves Ladegaillerie) to follow in their footsteps.

p. 414 an approach and style that bears the stamp of my influence. As I've said over and over again, such reluctance to involve others, or only to name them<sup>23</sup> (\*) without <sup>a□voir</sup> consulting them, was not uncommon during Récoltes

and Semailles. In each case, I eventually examined the reticence and realized that it was unfounded, that its source was not delicacy but confusion, not to say pusillanimity. In all the cases (it seems to me) where I mentioned by name the acts or attitudes of others, these were in no way of a "confidential" nature. They concerned the professional life of the person concerned, with all the repercussions that implies in the professional lives (and by extension, in the lives of all) of other colleagues, including myself. Each of those I involve is just as responsible for his or her actions and attitudes, and for the full range of their implications (whether or not he or she likes to ignore them), as I am for mine. He has no right to take offence if some of the consequences of his actions come back to him in one form or another, for example that of a public "mise en cause", in this case through me. If at times my language is colourful and harsh, my intention is in no way polemical, nor to offend or outrage anyone, but rather to describe the facts and the way I feel about them, as an incentive for everyone (and first and foremost for those I imply) to examine them on their own, rather than dismissing them one way or another (as I often did myself before the Harvest and Sow reflection). If the person being questioned chooses to be offended, that's his or her choice. This choice may pain me, coming from people I hold in esteem or even affection, but it doesn't weigh on me. The reluctance I mentioned, a sign of a certain confusion in my vision of things, vanished without a trace as soon as it was

<sup>23</sup>(\*) I was reluctant, for example, to include a note (note n° 19) in which mention would be made by name of all the students who had prepared and completed a state doctorate thesis with me. This hesitation in me must have stemmed from the reluctance of many of my students to see themselves associated with me, a reluctance I must have perceived at an informal level for some years already. The only ones among my former students (with or without quotation marks) where the desire to distance themselves from my person had been clearly perceived by me at the time, were Contou-Carrère (with whom I had only just discovered it), and Deligne (where the thing had already been quite clear since 1968, without my suspecting however how far this desire would lead him). In Deligne's case, I was particularly reluctant to name him as having been "more or less" a pupil, as I didn't want to seem like I was taking advantage of such a brilliant "pupil", when he himself didn't want to reveal the link that bound him to me and my work. My reflection made me realize, moreover, that this link had taken on an infinitely greater significance in the life and work of my young friend than I had ever suspected.

(June 1) See the note dated March 27 (three days later) "L'être à part" (n° 67').



understood and, by the same token, surpassed.

At no time during the reflection on l'Enterrement did I feel that some vast "conspiracy" had been hatched against my work and against those who had the temerity to draw inspiration from it (rather than simply borrowing tools, keeping quiet about the name of the worker who had fashioned them and placed them in their hands). There is no conspiracy, but there is a **consensus** which, in what I have called "the great mathematical world",

has so far appeared to me to be without fault. This consensus, except at most in rare exceptions, is in no way fueled by any conscious "malice" toward my person or my work. In p. 415

Only in exceptional cases did it express itself in unequivocal malevolence towards one or other of the four "co-sitters" mentioned in the previous notes<sup>24</sup> (\*). But surely such malevolence could not have proliferated in any of my former pupils, and could only have been expressed unhindered by the encouragement of general consensus.

In most, if not all, of my former friends and students, this consensus manifests itself not in "malicious" attitudes, but in (I believe) entirely unconscious mechanisms of unrolling uniformity and flawless efficiency, sweeping aside like chaff common sense and healthy mathematical instinct, to make way for purely automatic **attitudes of rejection**<sup>25</sup> (\*\*). Such automatic attitudes, I suspect, are not only aroused by me and those whose mathematical "smell" recalls it in some way - but also by any mathematician who does not present himself as already invested with the **tacit endorsement** of a certain "establishment"; either he himself is already part of it, or

that he appears as the "protégé" (to use an expression from Verdier's pen) of one of these.

It seemed to me □ that in almost all mathematicians, dispositions of a minimum "openness p. 416

mathematical instinct" (necessary for this "common sense" and "healthy mathematical instinct" to come into play) **can only be triggered in relation to someone who has already been granted such a guarantee.**

This kind of mechanism must be practically universal, not only in the mathematical world, but in all sectors of society without exception. It goes far beyond any individual case. If (as it seems to me) there is an exceptional situation in the case of my person, and of those who in the eyes of the establishment are "my protégés", it is because in the past I was invested with the status of "one of theirs", with the usual effect of a "minimum of openness" towards me and "mine". This status was taken away from me when I left in 1970. Or more precisely, by my own choice, clearly expressed on more than one occasion in the years following my departure, and by my way of life to this very day, I have indeed ceased to be one of "them". In fact, I myself no longer felt "one of them", and I left a world that was common to us with no spirit of return. Even today, my "return to maths" is by no means a return "among them", to the establishment, but a return to mathematics itself; more

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<sup>24</sup>(\*) Only in the cases of Deligne and Verdier did I learn of what I consider to be unequivocal acts of malice.

<sup>25</sup>(\*\*) These attitudes of rejection, of course, never present themselves as such, even in extreme cases like those of my friend Deligne, or Verdier. They are almost invisible in terms of conscious attitudes towards me, which (as I've already had occasion to say) are almost always (perhaps even always), in the case of my friends and pupils of yesteryear, attitudes of sympathy (from which some of them sometimes try as best they can to defend themselves) and respect. Such dispositions of sympathy and respect are present, not only at the superficial level of conscious "opinions", but even at the deeper level of real attraction (or repulsion), and of real knowledge of others (independently of the images in which we strive to enclose them).

Here we find ourselves in a typical situation **of ambivalence** (collective, I'd almost be tempted to say) where, as far as the eye can see, we "see" nothing! (Compare with the reflection in "The Enemy Father (1), (2)" (sections 29, 30), where for the first time in Harvest and Sowing I address this ambivalent aspect that has marked many relationships in my life, and not only in the mathematical milieu). And yet, at the level of concrete manifestations (examined at length in Burial), the "resultant" of these ambivalent forces no longer has anything ambivalent about it, it seemed to me, but it does present itself, with "baffling uniformity and unfailing efficacy", as the "attitude of automatic rejection" that I'm about to examine more closely.

precisely, a "return" to a continuous mathematical investment, and to an activity of publishing my mathematical reflections.

I'm only just beginning to realize how much my departure was felt as a kind of "desertion", even as an "outrage" by my former friends and pupils<sup>26</sup> (\*). It must have been the easiest way to evacuate the meaning of my departure, the questioning it could arouse in them, by such a diffuse feeling of a **wrong received**, and the automatic reaction of a grudge, expressing itself in an act of **retaliation** (which rarely had to be perceived as such, or even as an act, on a conscious level) : since he has cut himself off from us, we cut ourselves off from him - we cease to grant him and "his kind" the benefit of the "automatism of attention" reserved for "ours" - he and his kind will be entitled, like the first-timers, to the rigours automatic rejection!

p. 417     □The situation is further complicated (for my former friends and students) by the fact that not only was I doing

part of the establishment, but that moreover it is impossible for any of them to do their job as mathematicians without using at every step notions, ideas, tools and results of which I am the author. I don't know if there has ever been, in the history of our science or any other science, an example of such an embarrassing paradox! Seen in this light, the chainsaw effects (by no means limited to my friend Deligne) to cut off any velocity of development for ideas that bear my imprint (when such development could only increase this perplexity) now present themselves to me as driven by an implacable inner logic, as a **necessity** from a certain choice already made - the choice of rejection. And the same is true of the efforts I see being made just about everywhere to sweep under the carpet the origins of those notions, ideas, tools and results that have become part of our common heritage, and which we can no longer do without, whether we like it or not. This "indifference" that I've noticed in the face of Deligne's very large "operations", pretending to claim, one by one, the paternity of a certain number of my main contributions to mathematics (or, for the crumbs, generously attributing them to some inseparable friend) - this is by no means indifference, but **tacit approval**. Deligne is simply doing what the establishment's collective unconscious expects him to do: **erasing** the name of the man who cut himself off from everyone else, and thus resolving the intolerable paradox **by replacing a real but unacceptable paternity with a tolerable fake paternity**.

Seen in this light, the main Deligne officiant appears, no longer as the one who would have shaped a fashion in the image of the profound forces that determine his own life and actions, but rather as the designated **instrument** (by virtue of his role as "legitimate heir") of a **collective will** of flawless consistency, committed to the impossible task of erasing both my name and my personal style from contemporary mathematics.

p. 418     I have little doubt that this vision of things essentially expresses the reality of things: everything at least on a collective level. Surely my "return," which unexpectedly ends a funeral that was going on so satisfactorily for all, or (if it doesn't end it) at least disrupts so unseemly and inadmissible the unfolding of a ceremony that seemed to have been settled in advance - this return is going to inconvenience and displease not only this or that among the principal officiants, but also embarrass the entire congregation assembled for this funereal occasion! And I have no idea, of course, what "parade" this famous collective unconscious will come up with, to clear up the mess created by the untimely return of the deceased, suddenly (and unacceptably scandalously) stepping out of the cozy coffin intended for him, and pretending to officiate his own funeral in his own way. I trust, however, that the congregation will find

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<sup>26</sup>(\*) My friend Zoghman Mebkhout expressed this way of seeing and feeling things particularly eloquently. It's because of this desertion that I'm responsible for his difficulties with the mathematical world, since he alone has been deprived of the "protection" and support that those who now treat him like a troll once found with me.

a good way of evacuating this little additional contradiction in the mathematical edifice!

I seem to see quite clearly now, at the level of the images and attitudes of each individual, the reflection and general form taken by the collective consensus, and the collective will to erase, to bury. It's the universally used system of "two mutually contradictory tables" on which we operate simultaneously, and which I first had occasion to discuss in Harvest and Sowing in the case of my own person. (See the section "Merit and contempt", s. 12.) I doubt there's anyone who would say flatly and plainly: "Grothendieck has done nothing but bogus mathematics, let's not talk about it any more and get down to business". As it stands, this would be too explicitly contrary to the axioms of the establishment, for the time being at least. In the foreseeable future, in twenty or thirty years' time, the question won't even arise, as the name will no longer even be mentioned. The common tactic, both individually and collectively, is that of silence: we don't think of the deceased, not as a mathematician at least, we don't talk about him, and we don't mention him (except, when we can't do otherwise, by the providential acronym SGA or EGA, until these references are replaced by others from which all trace of the deceased is absent).

Yet there are occasions, exceptional no doubt, when complete silence becomes impracticable. One such occasion, I imagine, was my application for admission to the CNRS, which must have embarrassed more than a few of my colleagues.

from a<sup>27</sup> (\*). An autre will be the preliminary distribution of Récoltes et Semailles<sup>28</sup> (\*), pending its publication

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in volume 1 of *Réflexions Mathématiques* (if my publisher doesn't crack and refuse to blame the entire scientific establishment). These are opportunities created by the inadmissible deviations of the deceased himself, unhappily stepping out of the role assigned to him. Another occasion (perhaps more instructive for an understanding of the Burial, before its disruption by an unruly deceased) is the twenty-fifth anniversary jubilee of the IHES, which was celebrated last year "with great pomp". As "the first of the four Fields medals of the IHES", it would have been difficult to pass over me entirely in silence on this solemn occasion - even if my role in giving the IHES a real existence in the four heroic years of its existence was overlooked. The eulogy concocted in my honor, in the brochure published to mark this jubilee (to which I have already referred twice), seems to me to be a model of its kind - as an elegant and discreet way of resolving, to everyone's satisfaction, this "little contradiction" in contemporary mathematics... .

And suddenly I'm all revived - like the horse that's starting to smell the stable! Almost two weeks ago, I began to reflect on this instructive episode, in a note that immediately took the name "L'Eloge Funèbre - ou les compliments". After some hesitation as to where to place this note (taken from a late footnote to the first of the notes written for the Funeral), it appeared that the most natural place to insert it was (not the "chronological" place, but) in the "Funeral Ceremony" which is to complete the Funeral. And so, without having looked for it, the "thread" I've been following for the last three weeks, through the last three processions "The Colloquium", "The Student" and finally "The Funeral Van", which has only just joined the convoy, connects with the final part of the Burial, namely the Funeral Ceremony;

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<sup>27</sup>(\*) (May 26) I've just learned that my colleagues on the Comité National at the CNRS have made an effort on my behalf, by offering me a two-year "position d'accueil". I don't know whether they did so enthusiastically - but none of my friends on the Committee went out of their way to give me a call or a note to tell me the good news (dated May 15).

(September) I was informed of this in a letter from the CNRS dated August 16 - it was a one-year (not two-year) appointment to a research associate post.

<sup>28</sup>This is a limited edition (150 copies) produced by my university for distribution among my closest colleagues and friends.

this ceremony marked above all, precisely, by this □chef d'oeuvre Funeral Eulogy which I began to examine on May 12, and which now constitutes the note following quite naturally from this one<sup>29</sup> (\*).

I'm finally (again?) on target! And at the same time, this beginning of reflection on a eulogy suddenly takes on a new dimension. It's no longer just the ingenious invention of a powerful brain at the service of a fixed idea, expending itself before the indifference or commanding attention of the distinguished guests at an official "grand occasion" - but it's above all the perfect, deftly served response, made on this delicate occasion of all, to a collective expectation, about the attitude that should be taken towards my person. If anyone of his generation has earned the unreserved gratitude of the entire congregation, it's my friend Pierre Deligne, who fulfilled the role expected of him with his unmistakable perfection.

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<sup>29</sup>(\*) (November 1984) Following an unforeseen episode of illness, the note in question (n° 104) is separated from "this one" by a new procession - "The deceased - still not deceased" (n° s 98-103).

**Part three.**

**BURIAL (II)**  
**or the Key to Yin and Yang**

2. A walk through a work or the Child and Mother

# 17. The deceased (still not deceased. . . )

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## 17.1. The incident - or the body and the mind

**Note 98** □(September 22) The latest notes for the Burial (apart from a few footnotes page) is dated May 24 - that's four months ago. The two weeks that followed, until June 10, were mainly spent re-reading and completing or reworking the notes already written, not to mention a visit of a day or two from Zoghman Mebkhout, who came to read all the notes for L'Enterrement before I entrusted it to the typesetter, and to give me his comments. I was confident that the final manuscript would be ready by early June, and that it would be typeset and printed (that was optimistic, after all. . . ) before the university vacations. I really wanted to send my "five-hundred-page letter" to everyone before the vacation rush!

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In fact, the text of L'Enterrement is still unfinished as I write: as it was four months ago, it still lacks the final two or three notes - plus a<sup>1</sup> that has been added in the meantime: the one I've just started with the lines I'm writing, as a quick account of what's happened in the meantime. On June 10, a new contingency burst upon the writing of Récoltes et Semailles, which is rich in unforeseen events: i got sick! A stitch in my side, which appeared suddenly (when the minute before I'd suspected nothing), pushed me onto my bed with a peremptory force, without reply. Standing or even sitting up suddenly became very painful for me, only lying down seemed to suit me. It was really silly, especially at a time when I was just about to finish an urgent job, and I didn't want to talk about it anymore! Typing while lying down was out of the question, and even handwriting in this position is no picnic...

<sup>1</sup>(September 23) In fact, it appears that this planned "note" was split into three separate notes (n° s 99-101)

It took me almost two more weeks, during which I tried as best I could to carry on with my work against all odds, to realize the obvious: my body was exhausted and insisting, without me even pretending to hear, on complete rest.

I'd had such a hard time hearing it, because my mind had remained fresh and alert, wriggling to keep up its momentum, as if it had an autonomous life, totally separate from that of the body. It was even so fresh and wriggly that it had the greatest difficulty in taking into account the body's need for sleep, constantly refusing, to the point of exhaustion, the deadline for sleep, that impediment to going round in circles!

Throughout my life, and up until three or four years ago, the unlimited capacity to recuperate through deep, prolonged sleep had been the solid, salutary counterpart to sometimes inordinate investments of energy: when sleep is secure, you can no longer fear anything, and you can afford (without it being madness) to throw yourself headlong and to the point of exhaustion into orgies of work - even if it means making up for it with orgies of restorative sleep! This ability, which all my life had seemed as much a matter of course as the ability to work, the ability to discover (and surely the two are intimately linked. . . ), has in recent years ended up being eliminated, and sometimes even disappearing, for reasons that I can't quite discern at present, and that I haven't really made the effort to fathom yet. More and more, when, after a long day spent at my typewriter (or on handwritten notes) and obeying the injunctions of my body, which refuses to go on, I resolve to go to bed, the reclining position (and the partial relief it provides from the tension of sitting) immediately revives my thoughts. The thinking starts all over again, for hours or even the whole night (or rather, what's left of it. . . ). I realize that the system is unprofitable (assuming it's **sustainable** in the long run), given that (for me at least) prolonged reflection without the support of writing ends up going round in circles, often becoming a kind of rehashing - the bad bent is well taken, and tends to get worse. It had become, it seems to me, the great focus of energy dispersal in my life in recent years, while other dispersal mechanisms have been eliminated one by one, gradually, over the years.

If this mechanism has taken root in my life with such tenacity, if I've been willing to pay such a price for the last few years, it's surely because something in me has found its reward, and would find its reward when the time came. It would be no luxury for me to examine the situation closely - more than once in the past four months I've been on the verge of doing so.

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□ This was undoubtedly an urgent task. Eventually, however, I realized that there was something even more urgent. He

The first thing I had to do was to deal with the most pressing problem: to re-establish contact with my body, to help it recover from the state of exhaustion I had come to feel and admit, and to regain the vigour it had lost. I realized that to do this, I'd have to give up all intellectual activity for an indefinite period - even meditating on the meaning of what was happening to me. Today's notes bring to an end this long and salutary "parenthesis" in my major investments, which for a time (since February of this year) had come together in the writing of "**Récoltes et Semailles**". The present note is a first reflection, or at least a kind of summary report, on this four-month "parenthesis".

By the time I understood, at the end of the ends, the need for complete rest, great fatigue had become profound exhaustion. Having failed to listen to the peremptory language of my body, the derisory few pages of comments and retouching in *l'Enterrement*, snatched from a state of physical exhaustion in those first two weeks, were done at the cost of an outlay of energy which, with hindsight, seems to me demented! In any case, after these feats, I had to lie in bed for weeks on end, getting up only a few hours a day for the indispensable practical tasks.

Remarkably, once I finally **understood** the need for complete rest, I didn't experience the slightest



I found it difficult to give up intellectual activity altogether, without any desire to "cheat". I didn't even have to make a decision per se - just by understanding, I'd already quit. The tasks that only the day before had kept me on my toes suddenly seemed very distant, as if they belonged to a very distant past... .

The present, however, was not empty. While for weeks and months sleep remained reluctant to come, and I lay for long hours, seemingly in total inactivity, I don't remember a single time when I found time long. I was reacquainting myself with my body, and also with my most immediate surroundings - my bedroom, or sometimes the patch of grass or dry grass bathed in sunlight right before my eyes, wherever I happened to lie down, near the house or during a short (and cautious. . . ) walk. I'd spend long moments following the dance of a fly

in a ray of sunlight, or the peregrinations of an ant or tiny green translucent bugs

or pink along endless strands of <sup>her</sup>□<sup>be</sup>, in inextricable forests of such strands tangling beneath

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my eyes. These are also the moments when, in silence and in a state of great fatigue, we follow with solicitude the hesitant wanderings of the slightest wind through our guts - the moments when we get back in touch with the elementary and essential things; the moments when we can fully appreciate all the benefits of a restful sleep, and even the marvel of simply pissing without any problem! The humble workings of the body are an extraordinary marvel, and we only become aware of them (sometimes reluctantly) when they are disturbed in one way or another.

It was quite clear that, "technically", the root of my "health problem" was sleep disturbance. The deeper reasons for this disruption eluded me and still do. By trial and error, I tried first and foremost to get back to sleep, the good old sleep I'd known, which mysteriously slipped away just when I needed it most! I've only recently found it again. Needless to say, the idea of relying on pills would never have occurred to me, and if I tried herbal teas or orange blossom water (which I was introduced to on this occasion), I knew deep down that they were expé- dients. More seriously, I took the opportunity to make some major changes to my diet: a reduction in starchy foods in favor of green vegetables and fruit (both raw and cooked), a (moderate) reintroduction of meat as a regular ingredient in my diet, and above all, a drastic reduction in the consumption of fats and sugars, where I (and many others in affluent countries) had been systematically unbalanced since at least the end of the war. My son-in-law Ahmed, who practises Chinese medicine and has a very good "feeling" for these things, helped me a great deal in realizing the importance of such a change of diet in restoring a disturbed life balance. He was also the one who insisted, without tiring, on the importance of significant bodily activity, on the order of a few hours a day, to keep up with intense intellectual activity. Intellectual activity otherwise tends to exhaust the body, drawing available vital energy towards the head and creating a strong yang imbalance.

Ahmed didn't content himself with lavishing me with good advice, accompanied by a yin-yang dialectic to which I'm quite sensitive, in the four or five years I've had ample opportunity to familiarize myself.

with this delicate dynamic of things. As soon as I was well enough to garden, □and seeing myself

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Ahmed took it upon himself to get the shabby mini-garden up and running again, by clearing new strips of land, bringing in soil, transplanting and sowing, building terraces and retaining walls, rearranging the compost heap and so on. As the days and weeks went by, I saw unfolding before me, under the impetus of my indefatigable friend, landscaping tasks sufficient to keep me busy for years, if not for the rest of my life.

for the rest of my life!

That's exactly what I needed, and what I'll need in the long term to counterbalance my over-enthusiastic intellectual activity. In this respect, the daily walks I could take myself, as suggested to me a long time ago, wouldn't be much help: my head continues to grind during the walks as it does in bed, undisturbed by the beauties of the landscape, which I cross without seeing much of anything! On the other hand, when I'm watering the garden - I'm responsible for making sure it's doing well - and even better, when I'm hoeing a bed of vegetables, I can't help but pay attention and get into it a little - to see the texture of the soil, how it's affected by the hoeing, by the vegetable plants and by the "weeds" that grow in it, by the compost and by the mulch.

- and also, as time goes by, become aware of the state of the plants I'm supposed to be caring for, a state which reflects to a large extent the greater or lesser attention I've been able to give them. This gardening activity, and all that revolves around it, responds to two strong aspirations or dispositions within me: the one that pushes me towards an action where I see **something coming out of my hands** on a daily basis (which is by no means the case for walking, and even less so for the weights suggested to me by a colleague and friend. . . ); and the one that also pushes me towards an action where I see something coming out of my hands on a daily basis (which is by no means the case for walking, and even less so for the weights suggested to me by a colleague and friend. . . ). ); and the one that pushes me towards an action where, at every moment, I have the opportunity to **learn from** contact with things. It would seem that I'm best disposed to learn in situations where I'm actually "doing" something - "something" that takes shape and transforms itself under my hands... .

p. 426 Once I'd got over the state of exhaustion itself, my convalescence was, it seems to me, aided by two types of activity, or rather, two types of important and beneficial factors in my day-to-day activities, both at home and in the garden. On the one hand, there was **physical effort**.

that I often felt tired and listless before setting to work - the "harder" the job, involving me wielding a heavy pickaxe or large stones, let's say, the fitter, heavier I felt afterwards

a good dose of fatigue. And then there was the contact with **living things**: the plants that had to be cared for; the soil that had to be prepared for them, then mulched or hoeed; the food that had to be prepared and that I ate with as much pleasure as I had had in preparing the meal; the cat demanding its pittance, and its share of affection; the various utensils and tools too, and right down to the rough and often badly licked stones that had to be turned and turned in all directions, in order to assemble them into low walls that would stand upright. . .

Physical effort and contact with living things - these are precisely the two aspects that are lacking in intellectual work, and which mean that such work is by nature incomplete, piecemeal, and ultimately, if not supplemented and compensated for by something else, dangerous or even harmful. This is the third time in just over three years that I've had the opportunity to realize this. It's even become quite clear now that I'm facing a drastic deadline: to change a certain lifestyle, to find a balance where the yin pole of my being, my body, is not constantly neglected in favour of the yang pole, the mind or (to put it better) the head - or else lose my skin in the very next few years. That's what my body has been telling me, as clearly as it can be told! I've now reached a point in my life where the need for a certain elementary "wisdom" has become a matter of **survival**, in the literal sense of the word. This is surely a good thing - otherwise "wisdom" would be perpetually postponed, in favor of the kind of bulimic intellectual activity that has been one of the dominant forces in my entire adult life.

Faced with such a clear deadline: "change or die! - I didn't have to probe myself to find out what my choice was. That's why, for almost four months, I was able to abstain from all intellectual activity, maths or no maths, without ever feeling like I was doing myself any violence. I knew, without having to tell myself

In other words, a living gardener is even better than a dead mathematician (or a dead "philosopher" or "writer", for that matter! ). With a little malice, we could add: even better than a living mathematician! (But that's another story... )

I don't think I'll ever find myself in such a "borderline" situation, where I'll have to give up all intellectual activity, whether mathematical or meditative, in the long term. Rather, the task practice □ the most immediate, the most urgent in the years to come, seems to me to be precisely that of achieving

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a balanced life in which the two types of activity coexist on a daily basis, that of the body and that of the mind, without either becoming all-consuming and crowding out the other. I make no secret of the fact that my most powerful investments since childhood have been in the "spirit", and that the two main passions which have continued to dominate my life in recent years still lead me in this direction today. Of these two passions, the passion for mathematics and the passion for meditation, it seems to me that it is the first named above all, if not exclusively, that acts as a factor of imbalance in my life - as something that still has an unfortunate tendency to "devour" everything else for its own sake. It's no coincidence, surely, that the three "episodes of illness" in my life that have marked a situation of imbalance, since June 1981, have occurred precisely at times when it's the mathematical passion that has taken centre stage.

It could be said that this is not quite the case for this latest episode, which occurred during the writing of *Récoltes et Semailles*, a period of self-reflection, not to say meditation. But it's also true that this reflection on my mathematical past was constantly fuelled by my mathematical passion. This was particularly the case in the second part, *l'Enterrement*, where the egotic component of this passion was particularly strong and constant. Yet, even in retrospect, I don't feel that at any point this reflection took on an all-consuming, even demented, rhythm and pitch, as on the two previous occasions when my body was finally forced to let out an unanswered "ras le bol! Seen separately from the context of an entire life, my intellectual activity over the last year and a half (since "resuming" with the writing of *La Poursuite des Champs*, followed by *Récoltes et Semailles*) appears to have continued at a most reasonable pace, without forgetting to eat or drink (but sometimes, just a tad, to sleep. . . ). If it finally led to a third "health episode" (to use a euphemism), it was undoubtedly the result of a whole life marked by the eternal imbalance of a head that is too strong, imposing its rhythm and its law on a robust body that has long endured without flinching<sup>2</sup> (\*).

□ In the past two months, I've had ample opportunity to realize the irreplaceable benefit of bodywork, in intimate contact with humble living things, speaking to me in silence about the simple, essential things that books or reflection alone are powerless to teach. Thanks to this work, I found sleep again, that even more precious companion than eating and drinking - and with it, a renewed vigor, a robustness that had suddenly seemed to have vanished. And I've come to realize that, in my season of life, if I want to continue this new mathematical adventure I've been on since last year for a few more years, I can't do it without endangering my health and my life, except with my two feet firmly planted in the soil of my garden.

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The coming months will be those in which a new way of life will have to be put in place, in which the work of the body and that of the mind find their place and are reconciled on a daily basis. There's plenty to do!

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<sup>2</sup>(\*) I should make an exception here for the five years from 1974 to 1978, which were not dominated by any major task, and where manual occupations absorbed a not inconsiderable proportion of my time and energy.

## 17.2. The trap - or ease and exhaustion

**Note** 99 (September 23) Last night, I had to cut myself short, lest I keep going until two or three o'clock in the morning and get caught up in a spiral I know only too well. I was feeling refreshed, and if I'd followed my natural inclination, I'd have gone on into the wee hours! The trap of intellectual work - at least that which you pursue with passion, in a subject where you end up feeling like a fish in water, following a long familiarity - is that it's so incredibly **easy**. You just pull and pull, and it always comes, you just have to pull; it's only sometimes that you have the feeling of an effort, of friction, a sign that it's resisting just a little. . .

I remember, however, from my early years as a mathematician, a persistent feeling of heaviness that had to be overcome by stubborn effort, leaving a feeling of fatigue in its wake. This corresponded above all to a period in my life when I was working with insufficient or even inadequate tools; or to a later period, when I had to acquire tools more or less painstakingly, under the pressure of a milieu (essentially, that of the Bourbaki group) that used them routinely, without their *raison d'être* becoming apparent to me as I went along, or even sometimes for years. I've already had occasion to talk about these sometimes rather painful years (see "L'étranger bienvenu" s.9, and

"cent fers dans le feu, ou: rien ne sert de sécher!", note n° 10), in the first part of *Récoltes et Semailles*. It was mainly the period from 1945 to 1955, which coincides with my period of functional analysis. (He

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It seems to me that in the students I had later, between 1960 and 1970, this resistance to learning without sufficient motivation, where notions and techniques are swallowed up on the faith of the authority of elders, was much less strong than it was in me - to tell the truth, I didn't perceive any at all).

To return to my subject, it was from 1955 onwards that I often had the impression of "flying" - of doing maths by playing with myself, without any feeling of effort - just like some of my elders whom I had once envied so much for such almost miraculous ease, which had seemed well beyond the reach of my modest, ponderous self! Today, it seems to me that such "facility" is not the privilege of some exceptional gift (as I've encountered in others, at a time when such a "gift" seemed entirely absent in me), but that it appears of its own accord as the fruit of the union of a passionate interest in a given subject (like mathematics, say), and a more or less long-standing familiarity with it. If "gift" does indeed play a part in the emergence of such ease, it is undoubtedly through the factor of time, more or less long from one person to the next (and sometimes from one occasion to the next in the same person, it's true.... ), to arrive at a perfect ease in working on this or that subject<sup>3</sup> (\*).

Still, the more things go - as the years go by - the more I get this impression of "ease" when I do maths - that things are just waiting to reveal themselves to us, if only we take the trouble to look, to scrutinize them just a little. It's not a question of technical virtuosity - it's quite clear that from this point of view, I'm in much worse shape than I was in 1970, when I "quit maths": since then, I've had the opportunity above all to unlearn what I'd learned, "doing maths" only sporadically, in my own corner, and in a spirit and on themes quite different (at first sight at least) from those of yesteryear. Nor do I mean that it would be enough for me to get to grips with some famous problem (Fermat's, Riemann's or Poincaré's, let's say), to make my way in a straight line to its solution, in a year or two.

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<sup>3</sup>(\*) Yet I know several mathematicians, each of whom has produced a profound body of work, who have never seemed to give me the impression of ease, of "facility" referred to here - they seem to grapple with an omnipresent heaviness, which they have to overcome with effort, at every step. For one reason or another, the "natural fruit" just mentioned didn't "appear of its own accord" in these eminent men, as it was supposed to. Which goes to show that not all unions bear the fruit one might expect...

even<sup>4</sup> □trois! The ease I'm talking about is not the ease that sets out and achieves such and such a **goal**, fixed in advance :

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proving such and such a conjecture or giving it a counterexample... . Rather, it is that which allows us to dart into the unknown, in such and such a direction that an obscure instinct tells us is fruitful, with the intimate assurance, never to be denied, that every day and every hour of our journey cannot fail to bring us...

its harvest of new knowledge. Exactly **what** knowledge the next day, or even the next hour on this very day, holds in store for us, we certainly sense - and it's this "presentiment", constantly caught short, and the suspense with which it's bound up, that constantly launches us forward, while the very things we're investigating seem to draw us into themselves. What becomes known always surpasses what was foretold, in precision, flavor and richness - and what is known in turn immediately becomes the starting point and material for a renewed foreknowledge, dashing forward in pursuit of a new unknown eager to be known. In this game of discovering things, the **direction** we're taking at any given moment is known to us, while the **goal is** forgotten - assuming we started out with a goal, in fact, that we set out to achieve. This "goal" was in fact a **starting point**, the product of an ambition, or of ignorance; it played its part in motivating "the boss", setting an initial direction, and triggering this game, in which the goal has no real part. As long as the journey undertaken does not last a day or two, but is a long one, what it will reveal to us as the days and months go by, and where it will lead us at the end of a long cascade of unknown adventures, is for the traveller a total mystery; a mystery so remote, so out of reach in fact, that he hardly cares! If he happens to scan the horizon, it's not for the impossible task of predicting a point of arrival, and even less to decide on one as he sees fit, but to take stock of where he is at the moment, and from among the directions open to him for continuing his journey, choose the one he feels from then on to be the most burning. ...

Such is the "incredible facility" I mentioned earlier, in connection with the work of discovery in a wholly intellectual direction, such as mathematics. It is not **held back** by inner **resistance**<sup>5</sup> (\*)

(as is so often the case in meditation work such □as I practice it), nor by **physical effort** to

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The result is fatigue, which eventually gives an unequivocal signal to stop. As for **intel- lectual** effort (assuming we can even speak of "effort", having reached a point where the only "resistance" left is the time factor... . ), it doesn't seem to generate either intellectual or physical fatigue. More precisely, if there is any physical "fatigue", it is not really felt as such, apart from occasional aches and pains caused by sitting too long in a fixed position, and other similar incidental problems. These are easily eliminated by a simple change of position. Lying down has the unfortunate virtue of making them fade away, and thus encouraging a revival of intellectual work, instead of much-needed sleep!

I've come to realize, however, that there's a physical "fatigue" that's more subtle and insidious than muscular or nervous fatigue, which manifests itself as such in an unmistakable need for rest and sleep. The term "exhaustion" here (rather than "fatigued") would be more accurate, although it's understood that this state is not perceived as such, in the common sense of the term, which designates extreme fatigue, manifesting itself as

<sup>4</sup>(\*) Yet I know several mathematicians, each of whom has produced a profound work, who have never given me the impression of ease, of "facility" referred to here - they seem to grapple with an omnipresent heaviness, which they have to overcome with effort, at every step. For one reason or another, the "natural fruit" just mentioned didn't "appear of its own accord" in these eminent men, as it was supposed to. Which goes to show that not all unions bear the fruit one might expect... .

<sup>5</sup>(\*) Yet I know a remarkably gifted mathematician whose relationship to mathematics is typically conflictual, hampered at every step by powerful resistances, such as the fear that a certain expectation (in the form of a conjecture, say) might turn out to be false. Such resistance can sometimes lead to a state of genuine intellectual paralysis. Compare this with the previous footnote.

in particular by the great effort required just to stand up, walk a few steps, etc. It's more a question of "exhaustion" of the body's energy for the benefit of the brain, manifested by a gradual lowering of the body's general "tonus", its vital energy level. It seems that this exhaustion by excessive intellectual activity (by which I mean: not compensated by sufficient bodily activity, generating physical fatigue and the need for rest) - this exhaustion is gradual and **cumulative**. These effects depend on both the **intensity** and **duration** of intellectual activity over a given period. At the level of intensity at which I pursue intellectual work, and with my age and constitution, it would seem that the cumulative exhaustion in question reaches a critical, dangerous threshold in my case, after a year or two of uninterrupted activity, without compensation through regular bodily activity.

In a way, the "ease" I'm talking about is only apparent. Clearly, intense intellectual activity involves considerable energy: energy is taken from somewhere and "spent" on work. It would seem that the "somewhere" is in the body, which "takes in" (or rather, **disburses**) as best it can the (sometimes dizzying) expenses that the head spends without counting the cost. The normal route to recovery

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of the energy supplied by the body, is sleep. It's when the head becomes bulimic that it ends up encroaching on sleep, which amounts to eating up energy capital without renewing it. The trap and the danger

of the "ease" of intellectual work, is that it relentlessly encourages us to cross this threshold, or to remain beyond it once crossed, and that moreover this crossing is not signalled to our attention by the usual, unmistakable signs of fatigue, or even exhaustion. It takes great vigilance, I realize, to detect the approach and crossing of the threshold in question, when we're fully engaged in the pursuit of an exciting adventure. To perceive this emptiness of energy at the level of the body requires a state of listening to the body, which I have often lacked and which few people have. Indeed, I doubt that such a state of communion of conscious attention with the body could flourish in anyone at a time of life dominated by purely intellectual activity, to the exclusion of all physical activity.

Many intellectual workers instinctively feel the need for such physical activity, and arrange their lives accordingly: gardening, DIY, mountains, boating, sports... . Those who, like me, have neglected this healthy instinct in favor of an over-invasive passion (or too much lethargy), sooner or later pay the price. That's three times in three years that I've paid the bill, and I've done it without complaint I might add, or better still, with gratitude, realizing with each new episode of illness that I was merely reaping the rewards of my own negligence, and what's more, that it was also teaching me a lesson that no doubt only he could give me. Perhaps the most important lesson I learned from the last of these episodes, which has just come to an end, is that it's high time I took the initiative and made such reminders unnecessary - or, more concretely, that it's high time I cultivated my garden!

## 17.3. A farewell to Claude Chevalley

**Note 97** In my reflections yesterday and today, I have deliberately left out an event that took place right in the middle of the illness-episode, in the early days of July, at a time when I was still bedridden. This was the death of Claude Chevalley.

I found out about it from a vague article in *Libération*, more or less devoted to the event, which a friend had passed on to me on the off-chance that it might interest me. There was almost nothing about Chevalley, but a bit about Bourbaki, of which he was a founding member. I felt quite stupid when I found out about the news. For months I'd been picturing myself about to finish with *Récoltes et Semailles*, mint paperback and all - and go up to Paris dare dare to bring him a still-warm copy! If there were

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one person in the world I was sure would read my pamphlet with real interest, and often with pleasure, was him - and I wasn't at all sure there'd be anyone else but him!

Right from the start of my reflection, I realized that Chevalley had brought me something, at a crucial moment in my itinerary, something sown in effervescence, and which had germinated in silence. What I felt connected to him wasn't so much a **feeling of** gratitude, let's say, or sympathy, or affection. These feelings were surely present, as they are also present towards this or that other "elder" who had welcomed me as one of their own, more than twenty years earlier. What made my relationship with Chevalley different from my relationship with any of them and with most, if not all, of my friends, was something else. It's the feeling, I think, or better said, the perception, of an essential **kinship**, over and above the cultural differences and conditioning of all kinds that marked us from an early age. I don't know if there's anything of this "kinship" in the lines of my reflection where it's mentioned<sup>6</sup> (\*). In the period of my life to which these lines refer, Chevalley appears perhaps more as an "elder", this time on the level of an understanding of certain elementary things in life, than as a "parent". It's a distance, however, that my subsequent maturation must have reduced and perhaps abolished, as had been the case for a long time at the mathematical level, in my relationship with him as with my other elders. If I now try to put into words the meaning of this kinship, or at least one of its signs, it comes to me as follows: both of us are "cavaliers seuls" - travelers, both of us, on our own "solitary adventure". I write about mine in the last "chapter" (of the same name) of "Fatuité et Renouvellement"<sup>7</sup> (\*\*). Perhaps, for those who knew Chevalley well (and even for others), this part of the reflection is more apt to suggest what I'd like to express, than that which concerns him by name.

Meeting him and talking with him would surely have given me a better understanding of this friend than I had in the past, and a better grasp of both this essential kinship and our differences. If there were, apart from Pierre

Deligne<sup>a</sup> person for whom I felt an eagerness to be able to hand-deliver the text of Récoltes et Semailles was Claude Chevalley. If there was one person whose comments, whether mischievous or sarcastic, would carry particular weight with me, it was him again. On that day in the first week of July, I knew I wouldn't have the pleasure of bringing him the best I had to offer, nor the pleasure of still hearing the sound of his voice.

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The strange thing - and which no doubt contributed to making me feel so **stupid** on the spur of the moment - is that more than once over the past few months, as I talked about an upcoming meeting with Chevalley, I remembered that he was struggling with health problems - and there was a worry in me, constantly brushed aside, that this meeting might not take place, that my friend might perhaps disappear before I came to see him. The idea of course occurred to me to write or telephone him, if only to ask about his health and how he was, and to say a few words about the work I was engaged in, and my intention to go and see him about it. The fact that I brushed it off as silly and unwelcome (that there was really no reason why. . . etc.), as one so often does in situations of this kind, illustrates just how much I, like many others, continue to live "below my means" - brushing off the obscure foreknowledge of things that blows me knowledge that I'm too busy and lazy to hear. . .

<sup>6</sup>(\*) See "Rencontre avec Claude Chevalley - ou : liberté et bons sentiments" (section 11 ), and the last paragraph of the following section, "Le mérite et le mépris".

<sup>7</sup>(\*\*) See especially, in this sense, the two sections "The forbidden fruit" and "The solitary adventure", n° s 46, 47.

## 17.4. Surface and depth

**Note** 101 (September 24) After the digression of the previous two days around the "illness episode" of the past few months, it's time to pick up where I left off in June. I foresaw that there would be two final notes yet to be written: a "Funeral Eulogy (2)" (which would follow on from and complete the note "Funeral Eulogy (1) - or compliments" of May 12), and a final "De Profundis", in which I intended to sketch out an overview of my thoughts on the Burial.

The planned substance of these two notes was still very much alive when I fell ill - I was just about to throw it all down on paper, just long enough to finish putting the finishing touches to all the previous notes, so as to feel that I was working on solid, tidy "backsides". ... During the three full months (since June 23rd to be exact) that I've practically stopped all work on Burial, apart from the occasional typing correction, it has, alas, slipped my mind a little. I feel

p. 435 even a little foolish, embarrassed in any case, to wisely set about filling in the blank pages waiting behind titles-pensums, on the pretext that these figures went in a provisional table of contents, and that I had the imprudence of alluding to it here and there in a certain text intended for publication. This is especially the case for "L' Eloge Funèbre (2)", and even rereading the first juice of "L' Eloge Funèbre (1)" (aka "les compliments") wasn't enough to warm up for me a substance that for months had had the leisure to chill in its corner!

And yet, from the day after May 12, when I wrote this note, and throughout the month that followed, my hands were tingling with the desire to delve deeper into this new mine I'd stumbled upon, without even suspecting it. When Nico Kuiper was kind enough to send me the jubilee brochure celebrating the twenty-fifth anniversary of the IHES last year, I must have spent half an hour going through it (including the two half-page articles on Deligne and me), without finding anything in particular. The only thing that struck me was the absence of any allusion to the difficult early years of the IHES, when its reputation was established in a makeshift room, myself (with the first Algebraic Geometry Seminars) being the only one to represent it "in the field". I reflected on this months later, when writing the note

"L'arrachement salutaire" (n° 14), in March 84. Not being sure of my memory, I have, as a matter of conscience asked Nico to send me another copy of the booklet (as I couldn't get my hands on the first one again). This was a second opportunity to go through the two topos again, perhaps with a less hasty eye. And yet, this time I'm definitely not hip. I note in passing, with some surprise, that the topo on Deligne states that "The main thrust of his work is to 'understand the cohomology of algebraic varieties'" - who would have thought! I forgot about it for a month or two (until I was reminded of it while writing the note "Refusal of an inheritance").

- or the price of a contradiction", n° 47). On the other hand, I don't notice that the word The word "cohomology" is not mentioned, nor is the word "schema". In my inattentive state at the time, there's still nothing to make me suspect that this anodyne text, a little overloaded with hyperbolic epithets, functions as a Funeral Eulogy, "served" (moreover) "with perfect fingering"! A fingering so perfect, in fact, that I wonder if any of the readers of this booklet (a little dull around the edges, by dint of deliberate pomposity on all sides, as the occasion demanded, it would seem. . ) noticed it more than I did, on my first and second readings.

p. 436 This immediately ties in with an observation that keeps coming back to me, whenever, for one reason or another, I am led to look at something with a □ somewhat intense and sustained attention that I had previously been content to look at "in passing" with the "usual", routine attention I pay to the big and small things and events that go on in my life from day to day. Such a situation



is a frequent occurrence during meditation, which often leads me (more often than not "one thing leading to another" and without deliberate intent) to examine more closely certain events of the day or night (including dreams), which had passed more or less unnoticed in my customary state of attention, or whose meaning (often clear and obvious) had entirely escaped my conscious attention at first.

When I speak here of "somewhat intense, sustained attention", what I really mean is an **alert**, fresh look, a look that is not weighed down by habitual thinking or the "knowledge" that serves as a facade for it. If, for one reason or another, we are led to take an alert, attentive look at things, they seem to transform before our very eyes. Behind the apparent flatness of the dull, smooth surface of things presented to us by our everyday "attention", we suddenly see an unsuspected **depth** open up and come to life. This profound life of things didn't wait for us to take the trouble to get to know them - it's always been there, part of their intimate nature, whether we're talking about mathematical objects, a garden lawn, or all the psychic forces at work in such and such a person at such and such a time.

**Thought** is just one of the instruments we use to reveal and fathom the depth behind the surface, the secret life of things, which is only "secret" because we're too lazy to look, too inhibited to see. It's an instrument that has its advantages, just as it has its drawbacks and limitations. But in any case, thought is rarely used as an instrument of discovery. Its most common function is not to discover the secret life in ourselves and in things, but rather to mask and freeze it. It is a multi-purpose tool at the disposal of both the child-worker and the boss. In the hands of the former, it becomes a sail, capable of capturing the forces of our desire and carrying us far into the unknown. In the hands of the other, it becomes an immovable anchor, which neither turbulence nor storms can shake... .

The thought process was getting a bit lost, and now it's back to a starting point - which is the observation I **was** ☐ **arresting** on yesterday: to what extent, through habits and conditioning p. 437

I live below my means! (In which, moreover, I find myself in very good company... ). It was through a gradual discovery of L'Enterrement, based on facts as large as the LN 900 volume<sup>8</sup> (\*), that my lazy attention was finally awakened. A reading of the note "Refus d'un héritage

- ou le prix d'une contradiction" (n° 47) led me on May 12 to reread for a third time (!) the two famous "topo". This time, however, I noticed a rather unusual detail: there was no mention whatsoever of "cohomology" (or algebraic varieties or diagrams), in the little text in dithyrambic style devoted to me in the jubilee brochure! The whole thing struck me as funny enough to merit a footnote, which I set about writing as quickly as possible. Along the way, I became aware of one or two other "funny" details that hadn't yet caught my attention: even though this was a third reading, it too had remained superficial and mechanical - I'd pretty much just **repeated** and **reproduced** the readings I'd done previously. It was only when I wrote what was supposed to be a footnote, and which became the note "L'Eloge Funèbre (1)", that I gradually became hooked on the game, that a **curiosity** was awakened, which led me to return to these texts once again, this time looking at them a little more closely. It was only then that the transformation I mentioned earlier took place - that a "depth" opened up, an intense life behind the flat façade of a dithyrambic discourse, served up in the florets of a grand occasion! It was this curiosity that transformed a mechanical, repetitive, distracted gaze into an "awakened" one. . .

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<sup>8</sup>(\*) See the note "Souvenir d'un rêve - ou la naissance des motifs", n° 51, and the following note "L'Enterrement - ou les Nouveaux Pères".

The "awakening" in question wasn't instantaneous, moreover; it came gradually, with the progress of the reflection pursued in this note-de-bas-de-page-sic. To tell the truth, it wasn't complete until the final point of this note, when the hour was late (I seem to remember) and prompted me to "get it over with"<sup>9</sup> (\*\*).

p. 438 But no sooner had I placed this point, or at least by the next day, than I realized that I was far from exhausting the subject of the Funeral Eulogy. It was only then that I felt completely to how these two texts, so short and innocuous on the surface, were rich in meaning, veritable mines in fact! And that I was far from having exhausted what they had to say, if only I would listen... .

(September 25) Last night, I had to cut my thoughts short again, even though they had only just started, it seemed to me. I'd been sitting in front of my typewriter for three and a half hours straight, and little signs were beginning to show me that it was time to get up and get moving.

I well remember the first time I was led to direct "intense, sustained attention" to written texts, and experienced day after day, for months on end, the amazing metamorphosis of a dull, flat "surface", coming to life and revealing a rich, precise meaning, an unsuspected "depth". It was also, at the same time, my first long-term meditation, in the spirit of a journey into the unknown, which would last as long as it lasted... . The starting material was the voluminous 1933 / 34 correspondence between my father (who had emigrated to Paris) and my mother (still in Berlin at the time, with me then aged five). My aim was to "get to know" my parents. I had discovered the previous year that the admiration I had devoted to them all my life, which had eventually congealed into a kind of filial piety, covered up and maintained a very great ignorance about them. This phenomenal ignorance, in which I had been happy to maintain myself all my life, only became fully apparent to me during the long-term meditation of the following year, from August 1979 to March 1980.

I had begun to "prepare the ground" throughout the month of July 1979, in particular by doing a first reading of the whole of this correspondence, alongside work on a "poetic work of my own composition"<sup>10</sup> (\*) which I was then putting the finishing touches to. Every evening, I spent a few hours reading three or four letters-answers, certainly with interest and, I would have said at the time without hesitation, with a great deal of pleasure.

attentive way. Yet I was dimly aware that I remained a stranger, an outsider to what I was doing.

p. 439 that the true meaning escaped me. What I was reading was often quite crazy, as if this man and this woman I saw living and parading before my eyes had nothing in common with those I had thought I knew - those whose clear, intangible image my memory restored to me. In the absence of patient, meticulous, demanding work on what I was reading, which I would have pursued as I went along, I was simply stunned, without more, by the (relatively) little in these letters that was "big" enough to catch my superficial attention. What was recorded in this way was superimposed on the "well-known", which had been the invisible and unchanging foundation of my life, of my sense of identity, since my early childhood and right up to the present day (without my ever realizing it, of course). Assuming I'd stuck to that first reading, surely the thin layer of new, undigested "facts" that had thus been superimposed on the master layers would soon have been eroded and washed away without leaving much trace, in the months and years that followed.

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<sup>9</sup>(\*\*) All the more so, I'm sure, as on the same day I had already gone through the long and substantial reflection "Le massacre" (n° 87), to which, incidentally, I refer towards the end of the note "L'Eloge Funèbre - ou Les compliments" which had followed on from it.

<sup>10</sup>(\*) Allusion is made to this work and the episode in my life it represents, at the end of the section "Le Guru-pas-Guru, ou le cheval à trois pattes", n° 45, and in the note n° 43 to which it refers.

At the time of this preliminary work, my main investment was elsewhere, in the writing of a work that was absorbing most of my energy. I was well aware of the limits of a work done in parallel with another, and that I'd have to come back to it from start to finish, through a piecework project in which I'd put all my energy. I anticipated that it would be a matter of a few weeks - in fact I spent seven months in a row, devoted to a meticulous examination of the letters and writings left by my parents, of which the most "burning" part is surely the 1933 / 34 correspondence. Seven months, moreover, at the end of which I finally cut it short, realizing that the subject ("getting to know my parents") was as inexhaustible as ever. It had become more urgent to **get to know myself**, with the help of all the things I'd learned about my parents, and thus, indirectly at least... about my own forgotten childhood... .

I've just spent nearly two hours going through the beginnings of the notes of this meditation on my parents, begun on August 3, 1979. Contrary to what I thought I'd hastily remembered, I didn't then realize, except perhaps very dimly, the need to review thoroughly, "from beginning to end" (as I wrote earlier), the letters and other written traces of my parents that I'd read over the past month.

At least, I don't suggest anything to that effect in my notes. After a recapitulatory reflection by a

day or two, taking provisional stock of my multiple, tantōtinet confused impressions, aroused by p . 440

In no way do I pretend that I'm resuming this reading with meticulous piecework. Instead, I follow it up (as would be expected) with an equally brisk reading of **other** letters (notably a voluminous correspondence from my parents in the years 1937/39), and with a parallel reflection fuelled by my reading impressions. One thing leading to another, over the course of August and the following month, I began to learn what it's like to **work** on a letter (or any other written testimony of a life) in such a way as to apprehend its true, sometimes striking, meaning - a meaning, however, that the person writing often likes to ignore, to conceal from himself and from others, unseen and unknown! while managing to display it "between the lines" in a sometimes ostentatious, incisive way. And it must be rare for an insinuation or provocation (sometimes ferocious. . . ) not to reach the addressee, for it not to be perceived and "cashed in" by him at a certain level, while he too is careful not to let this perception, this knowledge penetrate the field of his gaze, and he too enters with all sails unfurled, into this same game of "neither seen nor known!". It is unfailingly the most obscure passages, those that seem to border on debility (or insanity. . . ) and defy all rational interpretation, that to the curious eye reveal themselves to be the richest in meaning: veritable mines, providing irreplaceable keys to penetrate further into the simple and obvious meaning behind the accumulation of apparent nonsense. Passages like these, frequent in the correspondence between my parents, and especially in the letters from my mother, who led the way, of course went completely "over my head" when I first read them in July. I began to pick up on them, here and there, over the course of the following month. It was only in September that various cross-checks made me realize that I'd definitely missed something essential in what I had to learn from the 1933/34 letters, and brought me back to them, prompting me to do a first "in-depth" reading of some of them. This reading immediately changed my childhood image of my parents and their relationship with me and my sister.

## 17.5. In Praise of writing

**Note** 102 (September 26) It's been two days since I've been in the midst of "autobiographical reminiscences", as I set off to write ("cold") the sequel to a certain note on a certain Eloge Fu- phique.

p. 441 nèbre. I don't know whether this digression will have warmed my ardor even a little! At the very least, it's about time I got to the point I had in mind when I launched into it <sup>before-□hier</sup>, a little in the direction of: "On the art of reading a message that pretends not to say what it has to say". This kind of text-message is much more frequent than I would have imagined... ..

It goes without saying that the question of the "how" of this "art" doesn't arise, as long as you're prepared (as I was for most of my life) to take at face value and to the letter everything you're told or written, and not to look for or see, in anything or anyone, any intentions other than those expressly expressed by the person concerned. On the other hand, the question arises when you are confronted with the indefinable expression that something is "not right" in a statement, tirade or narrative, that there's something fishy about it, that something has "passed", somewhere, that isn't supposed to have been said (what would you expect?). Sometimes, too, it's the elementary and disconcerting perception of an incoherence, an absurdity, sometimes so enormous and at the same time seemingly elusive, that it seems to defy all formulation, to the point of appearing to be debilitating or delirious. Such situations are often overloaded with anguish - and it was indeed by an instantaneous influx of anguish, never recognized as such but blurred and immediately retracted under a wave of violent, distraught anger, that I invariably reacted to such situations, where absurdity suddenly burst into my life: an inadmissible, incomprehensible absurdity, fraught with threats, each time shaking my serene vision of the world and of myself to the foundations! And so it was, at least until I discovered "meditation", when an intrepid, enterprising curiosity defused and took over from these waves of anger and anguish... ..

It was curiosity, i.e. the desire to know, that made me spontaneously find, under pressure of need, this "art" of deciphering a scrambled testimonial text - or more modestly speaking, a method that suited my limited means and cumbersome nature. No matter how hard I tried, and no matter how curious I was, on first reading (or even on second) of these weighty letters, all the essentials went right over my head - "I couldn't see a thing". Sometimes, commenting on a few often confused impressions, perhaps about this or that particularly obscure and disconcerting passage, I managed to penetrate further into the meaning of a text that had seemed hermetic. Along the way, I sometimes found myself copying, for quotation purposes, passages of greater or lesser length, distinguished either by obscurity, or because they gave me the impression of being "important", for one reason or another. □

p. 442 other. As the days and weeks went by, I realized that the simple act of **copying** a passage in extenso of the text I was examining, surprisingly altered my relationship with the passage, opening me up to an understanding of its true meaning.

This was completely unexpected, whereas my initial motivation (at least on a conscious level) had been a matter of pure convenience. I even remember that for a long time, there was a certain restrained impatience in me, to devote precious time to acting as a copyist, nothing more, nothing less, I'd eat my heart out to get to the end and write as fast as I could... . But there's no comparison between the speed of the eye reading written lines and the hand transcribing them word for word. No matter how fast you write, the "time factor" is absolutely not the same. And I suspect that this "time factor" does not act in a purely mechanical, quantitative way - or, to put it better, that it is only one aspect of a more delicate and richer reality. Nor, for me at least, is there any common measure between the action of the eye that scans lines that someone else has thought and written, and the act of the hand that letter after letter, word after word, rewrites those same lines. Surely, there is a profound symbiosis between the hand and the mind or thought; and at the very rhythm of the hand that writes, and without any deliberate purpose, the mind cannot help but reform, rethink the same words, assembling themselves into sentences laden with

meaning, and these in discourse. Provided that a desire to know animates this hand that reproduces letters, words and phrases, and that it animates this mind which, in unison, also "reproduces" them, at another level, - surely this double action creates a more intimate contact between myself and this message of which I make myself the scribe-writer, than the act, above all passive and without support or tangible trace, of the eye that is content to read.

This groping intuition is in line with a long-held observation - that for me, rhythm is the key to success. of working thought (be it mathematical work or any other, including the work I call "meditation") is most often (if not always) that of the hand that writes, and in no way that of the eye that reads<sup>11</sup> (\*). And the **written trace** *laid* by my hand (or sometimes, by the typewriter maneuvered

by my hands. ... ), at the pace of unhurried, never dawdling thought, is the indispensable material support of that thought - both its "voice" and its "memory". I suspect that the same is more or less true (though perhaps to a lesser degree) of most, if not all, "intellectual workers".

## 17.6. L'enfant et la mer - or faith and doubt

**Note** 103 (September 27) In any case, the fact remains that, just as I can only "enter" a mathematical theory by writing, I can only begin to enter a text-message, the "between the lines" of a message, by **rewriting** it. My first work of meditation "on texts" was transformed, an apparent platitude began to open onto a living depth, and the absurd to find a meaning, **from the** moment I began to rewrite in extenso the message, or (in the case of one of prohibitive dimensions) the passages that a flair made me feel were crucial.

In the absence of reliable "objective" criteria for guaranteeing the validity of an "interpretation", presented as the result or outcome of (so-called?) "work" on a text, let's say, we can make any text or discourse say exactly what we want, inventing whatever "message" we like to attribute to it. Nothing could be further from the truth, and sure-fire examples abound! Moreover, I doubt (except perhaps in a delimited discipline like history - and even then. . . ) that it would be possible to identify such criteria. It wouldn't do much good anyway: it wouldn't stop anyone inventing fanciful interpretations, nor would it enable anyone to fathom and discover the true meaning of a message, a situation or an event. Rules and criteria are the ingredients of a **method**, which has its own usefulness and importance (often overestimated, to the detriment of other factors and forces of a completely different nature), as a tool for discovery and consolidation in the development of scientific or technical knowledge, and in that of any kind of know-how: driving or repairing a car, etc. On the other hand, at the level of self-knowledge and discovery of oneself and others, the role of method becomes entirely incidental: it's the "stewardship" that certainly follows, when the essential is there. And being inspired by a method, or even clinging to it, does nothing to encourage the emergence of that more essential thing.

- quite the contrary!

To put it another way: he who sets out to find such and such a thing *decided* *advance* (which he will call "true",

or "truth") will have no trouble finding it, and even proving it to his complete satisfaction - and surely

<sup>11</sup>(\*) This circumstance, which seems to affect me to a greater degree than most of my fellow mathematicians, once made it difficult for me to fit into the collective work sessions of the Bourbaki group, finding myself unable to keep up with the readings as they went along. In fact, I've never really enjoyed **reading** mathematical texts, even beautiful ones. My spontaneous way of understanding maths has always been to **do** it, or to **redo** it (with the help, here and there, of ideas and indications provided by colleagues or, for want of a better word, books. . . ).

along the way, he'll find one or another, if not a whole crowd, happy to form an alliance with him and share his convictions and satisfaction. He's like the butterfly hunter, who sets off with a beautiful butterfly in his net (a stuffed one, as it happens), and takes it out all happy (and to his own satisfaction) when he returns from his "hunt".

And then there's the one who finds himself placed in front of an unknown, like a naked child in front of the sea. When the child wishes to know it, he enters and knows it - whether it is warm or cool, calm or agitated. Anyone who is attracted by something unknown, and sets out to discover it, will surely know it to a greater or lesser degree. With or without a net, he will find the truth, or at least **some** truth. His mistakes and his discoveries are all stages in his journey, or rather, in **his love affair** with what he wants to know.

I know what I'm talking about, because in my life I've been both this butterfly hunter and this naked child. There's no difficulty in distinguishing one from the other. I doubt that "objective criteria" will be of much help here, it's much simpler than that! All you have to do is use your eyes...

And there's no difficulty in distinguishing the successive stages, the successive decanting stages, in this journey I've just been talking about, from this "dead" stage where no presentiment surfacing to consciousness yet gives rise to the suspicion of "something", beyond a certain flat, amorphous surface presented to us by sleepy eyes, and which through successive "awakenings" leads us towards an increasingly delicate, more intimate, more complete apprehension of this "something". It's not essentially different in nature, whether we're talking about the discovery of mathematical things, or the discovery of ourselves and others. The feeling of **progress in knowledge**, which deepens little by little (even through an accumulation of errors, patiently, tirelessly corrected) - this feeling is as indisputable in the latter case as in the former.

This **assurance** is one side of an inner disposition, the other side of which is an **openness to doubt**: an attitude of curiosity, excluding all fear, towards one's own mistakes, which enables us to detect them.

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ter and constantly correct them. The essential condition of this double foundation, of this **faith** indispensable to welcoming doubt as well as to discovery, is the absence of all fear (whether apparent or hidden).

about what will "come out" of the research we're undertaking - fear, in particular, that the reality we're about to discover will upset our certainties or convictions, that it will disenchant our hopes. Such fear acts as a deep paralysis of our creative faculties, of our power of renewal. We can discover and renew ourselves in sorrow and pain, but not in fear of what is about to be known, what is about to be born (any more than a man can know a woman and make her conceive, in a moment when he is afraid of her, or of the act that carries him into her). Such fear is no doubt relatively rare in the context of scientific research, or any other research whose theme does not involve our own person in any profound way. It is, however, a major stumbling block when it comes to self-discovery or the discovery of others.

However, the feeling that accompanies a discovery, large or small, is as compelling in the case of self-discovery or the discovery of others, as it is in the context of impersonal research, such as mathematics. I've already alluded to this feeling. It's a reflection, at the emotional level, of a perception of something that has just happened - the appearance of something **new** - and this "something" appears as tangible, as irrefutable (I apologize for the repetition!) as the appearance of a mathematical statement, let's say, or a notion or a demonstration, that we'd never thought of before. In fact, I find it difficult to distinguish or separate this feeling, which accompanies a particular discovery, from the feeling of progression I mentioned earlier, which accompanies an entire research project. Discoveries "big and small" are like the successive **steps** that materialize a progression, like

successive **thresholds** we have to cross. Progression is nothing other than this sequence of thresholds crossed, of accessions from each of these levels to the next.

The "feeling" or, better still, the perception that reflects and restores this process, is a sure and unmistakable "criterion".

- I don't remember it ever misleading me, either in maths or in meditation: that I had to realize, with hindsight, that this feeling would have been illusory. It often enables me, without any residual doubt, to distinguish the true from the false, or to discern the true in the false, and the false in what is supposed to be true. But above all, it is an irreplaceable **guide** in any true search - a guide ready to inform us at a moment's notice (provided we take the trouble to consult it) if we are on the wrong track, or are on the right track.

The willingness to listen to this sure guide is nothing more' <sup>it</sup> seems to me, than what in another p . 446

<sup>12</sup> (\*) I have named "rigor". It seems to me that this rigor is no different in essence from the demands of mathematical research, or from those of self-knowledge, without which there can be no such knowledge. But it goes without saying that this in no way means that the presence of this rigor, at the level of such intellectual work, is a guarantee or sign of its presence for the knowledge of oneself and of others. In fact, the opposite is true, as I have observed on countless occasions, starting with myself. In this respect, the "rigor" I'm talking about here came into my life at the same time as meditation. Or to put it another way, I can't really distinguish between one and the other. The moments of meditation in my life are none other than those when I examine myself (most often through my relationship with others) in such a state of extreme exigency with myself.

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<sup>12</sup>(\*) In the section "Rigor and rigor", n° 26, where I refer to "rigor" as "delicate attention to the **quality of understanding** present at each moment" in a search.

2. A walk through a work or the Child and Mother



# 18. XII The Funeral Ceremony

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## 18.1. The Eulogy Funeral

### 18.1.1. (1) The compliments

**Note** 104 □(May 12)<sup>1</sup> (\*) Remarkably, in the little "topo" on my work that is done in this

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even in the brochure<sup>2</sup> (\*\*), the word "cohomology" or "homology" is never uttered! Nor is the word "schematic". Certainly (as circumstances demanded, when I was acting as the "first Fields Medal to be awarded to the IHES"), there's mention of the "titanic aspect" of my work, the number of volumes published, the essential problems identified, with the greatest natural generality (funny French for that), very careful terminology, allusion to "Grothendieck groups" (another of those greatest natural generalities, I bet!), and even topos and their usefulness in logic (but certainly not elsewhere!) . . . But there's no hint of a **result**, or a **theory** I'd developed that might have been useful - it must be that these twenty titanic volumes were rigorously empty, or just collections of problems (never solved) and notions, with the greatest natural generality it's understood: Grothendieck's group is awarded (since my name is already stuck to it afterwards), presented as the "ancestor" of algebraic K-theory (!) (which has nothing to do, of course, with topological *K-theory*, of which nothing is said)<sup>3</sup> (\*\*\*) ; as for the Riemann-Roch theorem, it must have been the descendants of the "ancestor" who took care of it - those who make the real theorems, the serious stuff!

At a time when it is fashionable to disregard generalities (persifuted by the ridiculous phrase "greatest natural generality"). . . ), □ the anonymous pen that took care of my praise here p. 448  
funèbre gratified me abundantly with what today is delivered to the disdain<sup>4</sup> (\*). I also appreciated (perhaps I'm the first. . . ) the humor of the same anonymous pen in this passage from the eulogy:

"He created a school of algebraic geometry at the IHES, gathered around the seminar he led and **nourished by the generosity with which he communicated his ideas**" (emphasis mine). Unfortunately, just like my "titanic work", this "school of algebraic geometry" that I nurtured so well is rigorously empty - not a single name is mentioned, and no one has come to complain that it has been forgotten, at least not to me.

Yet I seem to remember seeing the young Deligne faithfully haunting this (presumably empty) seminar between 1965 (when he must have been nineteen) and 1969, and learning in this seminar and in our tête-à-tête not only the technique of schemas, but also cohomological techniques and staggered cohomology - in other words, the very tools used on every page of his work (among those I've seen, at least). In

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<sup>1</sup>(\*) (May 18) The following note is "from a footnote (to note no.° 47) that has grown prohibitively large". I've inserted it here, believing that this order is more natural than the chronological one.

Since the very moment this note was written, I've felt the need to develop it a little further - this will be done in a follow-up note to this one, which has not yet been written at the time of writing. The two notes together have now taken on the appropriate name: "L'Eloge Funèbre" (The Funeral Eulogy)!

<sup>2</sup>(\*\*) (May 18) This is the brochure published in 1983 by the IHES (Institut des Hautes Etudes Scientifiques) to mark its twenty-fifth anniversary. Reference is already made to it in the footnote "L'arrachement salutaire" (n° 42), and again at the beginning of the note "Refus d'un héritage - ou le prix d'une contradiction" (n° 47), to which the present note (L'Eloge Funèbre (1)) refers (see previous footnote).

<sup>3</sup>(\*\*\*) My work on the Riemann-Roch theorem is the first strong start of **algebraic** K-theory, and null-an "ancestor". **Topological** K-theory was born the same year (1957) that I proved the Riemann-Roch-Grothendieck theorem, following my presentation at the Hirzebruch seminar. The "ancestor" of this unspoken "descendant" hadn't another year! Algebraic K-theory (with Bass's introduction of the *K* functor in addition to the *K* functor I had introduced) developed in the years that followed, under the dual influence of the "ancestor" and his first "descendant".

In fact, as early as the second half of the sixties, I had already taken a step towards a description of the higher  $K^i$  (for a "monomial" category, e.g. additive), along the lines of Mrs. Sinh's thesis. This remained heuristic, being based on the intuition of the **enveloping  $\infty$ -category of Picard**, while no one at that time (or since) had taken the leisure to develop the notion of a (non-strict)  $\infty$ -category, i.e., the  $\infty$ -category of Picard. The notion I now call by the name of  $\infty$ -field (on the punctual topos) With the sketch of foundations for a cohomologico-homotopic formalism of fields I'm about to develop in the Pursuit of Fields (in the right file of the ideas I developed between 1955 and 1965), this "geometrical" approach towards a theory of higher *K-invariants* would enfin be available.

<sup>4</sup>(\*) (May 18) And I've gone on and on! For a complete quotation from my Eulogy, see the note "The Eulogy (2)".

p. 449 "topo" devoted to Deligne in the same brochure, no allusion either that might make the reader suspect he might have learned something from me. Yet, remarkably, my name is pronounced three times in this eulogy (by no means funereal) of Deligne ("third Fields medal of the IHES"). And even in a periphrase there's a reference, with the vague rigor that must surround every appearance of my modest person, to the fact that I would have "constructed the theory of cohomology in geometry over any body" - and surely still "with the greatest natural generality", it smells of grothendieckery<sup>5</sup> (\*). The full context quote is worth giving, it's a little masterpiece of the genre:

"Starting from there [classical Hodge theory] and from  $l$ -adic analogies suggested by Grothendieck [one wonders where Gr. found the time to learn such serious things, while redigging his twenty volumes of greater natural generalities], he [Deligne] derived the notion of mixed Hodge structure and equipped the cohomology of any complex algebraic variety with it. In  $l$ -adic cohomology, i.e. [?] for varieties over a finite field, he proved Weil's proverbially difficult conjectures. This result seemed all the more surprising [!!] since Grothendieck, after having constructed the theory of cohomology in geometry over any field [one wonders what else he went looking for there], had reduced the remaining conjecture [??] to a series of conjectures that are as unapproachable today as they were then."

Clearly, far from having contributed in any way whatsoever to proving this surprising result of such proverbial difficulty, these grothendieckeries (with a name that would scare off the most hardened generalist-naturalist) have been no more than good enough to encumber us again with **conjectures**, as is only right (he never makes any others!) and unaffordable what's more (one would have guessed), just as much today as when he had the preposterous idea to make them.

p. 450 ☐ Yet I seem to remember tackling them, these unapproachable conjectures, but that was probably because... that I was misinformed. It was around the time I left, I mean died, and my posterity, better informed than me, was careful never to put its nose in that stuff, since Deligne was formal: it was unapproachable!

I recognize the style: we've done our homework, quoted Grothendieck extensively (neither he nor anyone else will be able to claim that we're burying him on this solemn day), and even hinted at the " $l$ -adic analogies" that had played a role in getting the mixed Hodge theory off the ground. This must be the second time since the famous lapidary half-line thirteen years before<sup>6</sup> (\*); both allusions bear a strange resemblance to the "weighty considerations" of a certain 1968 article<sup>7</sup> (\*\*): one is "thumbed", and one has led the reader by the nose at the same time! Here, the solemn occasion helping, the thumb reference does more than drown the fish - the impression this text wants to suggest about this famous Grothendieck is precisely the one carried by this "wind" of fashion that I've been feeling for a few years - the one I've already had the opportunity to feel today<sup>8</sup> (\*\*\*), no longer in the tones of eulogy and special occasions

<sup>5</sup>(\*) (May 18) In the Eulogy, reference is made to my "great attention" to terminology. In the use of saucy expressions like "the greatest natural generality" or "the theory of cohomology in geometry over any body", I clearly perceive the intention to mock this attention.

The extreme care I take with the names given to things stems naturally from the respect I have for these things, whose name is supposed to express their essence, or at least some essential aspect of them. I've been shocked on more than one occasion by the disdain with which this attitude of respect seems to be treated today, a disdain which is sometimes expressed by the use of abracadabra names for important notions. On this subject, see also the note "Perversity" (n° 76).

<sup>6</sup>(\*) This "lapidary half-line" can be found in Deligne's report "Hodge Theory I" to the Nice International Congress in 1970. See comments in note n° 78<sub>2</sub>.

<sup>7</sup>(\*\*) On this subject, see the beginning of the note "Canned weight and twelve years of secrecy" (n° 49), and the more detailed review in the note "Eviction" (n° 63).

<sup>8</sup>(\*\*\*) See the note of the same day "The massacre", n° 87.



in front of a large audience, but in those of the massacre...

I'll continue the quote, it's worth it:

"This theorem (ex-Weil's conjectures) has helped to make *l-adic* cohomology a powerful tool... needless to name the brilliant and modest inventor of such a powerful tool... . . applicable to questions seemingly far removed from algebraic geometry, such as Ramanujam's conjecture.

More recently, he studied Hodge cycles on abelian varieties, taking the first step towards a "motivic" theory such as Grothendieck had dreamed of. He also demonstrated the algebraic mechanism of "intersection cohomology", the topological theory of Mac Pherson and Goresky. This made it possible to transpose it to *l-adic* theory, where it proved surprisingly useful."

So, one year after the publication of the "memorable volume"<sup>9</sup> (\*\*\*\*), an anonymous pen (I'm guessing the same one) has finally repaired a small "oversight" in the said volume. Perhaps someone had to ask a question anyway.

question, and Deligne is here to make up for the omission in his own way (it's nice of you to quote this dreamer, though

□ de Grothendieck, when it comes to, well, serious mathematics! ). And always deceiving the reader, p. 451  
given that the "first step" was taken as early as 1968 with Deligne's launch of the Hodge-Deligne theory, rooted in the yoga of motives which he had indeed "nourished" through my contact, throughout the four years that had preceded. This yoga, from which his work stems, from which he has never known how to detach himself while denying it, is in fact dispatched in the periphrase of the first quotation under the name of "*l-adic* analogies". An uninformed and attentive reader would certainly not suspect a link between these "*l-adic* analogies", which would have played a role as a starting point (but certainly not beyond. . .) for Hodge-Dixon's theory. ) for the Hodge-Deligne theory<sup>10</sup> (\*), and a "motivic theory" of which I had indeed dreamed (and a devilishly precise dream at that) - if not this link, that it's still this same dreamer Grothendieck who manages (by dint of greater natural generalities) to suggest analogies to real mathematicians, on condition that they do some real work.

As for the famous "algebraic mechanism of 'intersection cohomology'", here we are in the middle of the Colloque Pervers<sup>11</sup> (\*\*) (although the word "perverse" is not used). We've certainly taken the gloves off with the one

of the "four Fields □ medals of the IHES", given the solemnity of the occasion - but we don't have to embarrass ourselves with the student p. 452

posthumously by the same Grothendieck. My own burial on this exceptional occasion in the limelight, ministerial speech and all, is not burial by silence, but by **compliment**, skilfully measured and administered. But where Mac Pherson and Goresky are named, it goes without saying that for posthumous pupil Zoghman Mebkhout, silence is de rigueur, as it was two years earlier at the Colloque Pervers, and as it still is today.

<sup>9</sup>(\*\*\*\*) This is the Lecture Notes volume n° 900 published in 1982, referred to in the notes "Souvenir d'un rêve - ou la naissance des motifs" and especially "L'Enterrement - ou le Nouveau Père" (n° 51, 52). This is the volume in which the motifs are "exhumed" (after a twelve-year deathly silence on their subject), under an (implicit) alternative authorship.

<sup>10</sup>(\*) This Hodge-Deligne theory is still in its infancy, due to the failure to develop the notion of a "Hodge-Deligne complex" on any finite type scheme over  $\mathbb{C}$ , and the formalism of the six operations for these "coefficients". The need for such a theory was obvious to Deligne as much as it was to me, even before his first work on mixed Hodge structures, and it followed obviously from the yoga of patterns. But as soon as I left the mathematical scene, Deligne developed a "block" against the key ideas I had introduced into homological algebra (derived category, six operations, not to mention topos), which prevented the natural development of a theory that had got off to a spectacular start.

<sup>11</sup>(\*\*) On this Colloquium, see Cortège VII, "Le Colloque - ou faisceaux de Mebkhout et Perversité".

## 18.1.2. (2) Strength and the halo

**Note** 105 (September 29) The "previous" note, "L' Eloge Funèbre (1) - ou les compliments" (n° 104), is dated May 12 - more than four months old. It had begun as a footnote to "Refus d'un héritage, ou le prix d'une contradiction" (note n° 47, from the end of March), just to note in passing a little "funny" fact I'd only just become aware of. But as I was writing it, it became clear to me as I went on

lines and pages that these two short, seemingly innocuous texts on which I was commenting, without really planning or looking for it, were a real "mine"<sup>12</sup> (\*). It was also the day when I had already

to paint a picture of a massacre (note n° 87), a picture that had gradually emerged from the mists of time. of the past few weeks. There, it had suddenly materialized, had taken shape by the mere virtue of an enumerative description, and now it was calling out to me forcefully. The massacre and the "compliments" - a eulogy for the late man - were like two complementary parts of the same striking picture, both appearing on the same day!

It was certainly enough to satisfy me! The very next day, "my hands were tingling" to keep up the momentum and, in particular, to probe further into this little gem of a mine I'd just unexpectedly got my hands on. It became clear that the first thing to do was to quote in extenso the two passages in question from the jubilee booklet - at the same time, it would also be the best way to make contact.

with these texts and get a better grasp of their real message, the message "between the lines". . . <sup>13</sup>(\*\*).

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Without even having had the <sup>□</sup>loisir yet to copy the two texts, the previous day's contact had already been enough to arouse or awakened in me several associations of ideas, which I felt were juicy. I couldn't wait to pursue them, although I wasn't sure yet where they would lead. ...

In the end, I didn't keep up the momentum in the days and weeks that followed, though I did promise myself that I'd be back to it in the next few days. An unforeseen "health incident" put an end to all work on Harvest and Sowing for over three months, and indeed to all intellectual work whatsoever<sup>14</sup> (\*). The "hot moment" for pursuing this direction of reflection, which had just opened up in those days, has now passed. It's not certain that it'll come back, or even that I'll want to make the effort to "blow" (the heat!) to bring it back at all costs. To tell the truth, my real desire now is to come to the ultimate note, drawing a provisional **balance sheet of the** whole reflection named L' Enterrement - and to draw a **final line!** As far as this note is concerned, I'll at least give the full quotation I promised myself (and to the reader, for that matter); and perhaps at least a few summary indications, too, about certain associations of ideas that these two texts (and perhaps also the fact of rewriting them in black and white) will have aroused in me.

The two texts in question (pp. 13 and 15 respectively, from the 1983 jubilee brochure entitled "Institut des Hautes Etudes Scientifiques") are part of a series of "minute portraits" of the "permanents" and "long-term guests" who have passed through the IHES since its foundation in 1958, arranged in chronological order of entry. These are fairly brief texts, each about half a page in length, and each includes the dates on which the person joined the IHES, their position (professor or long-term visitor), their main honors, their main areas of interest and their most important contributions, with (where appropriate) the names of some of their collaborators. For my humble self, however, there's a remarkable gap in the last three.

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<sup>12</sup>(\*) For some retrospective comments on this subject, see the beginnings of the September 24 note "Surface and depth" (no. 101).

<sup>13</sup>(\*\*) On this subject, see the note "Sur l'art de déchiffrer un message - ou éloge de l'écriture" (n° 102), which follows the note quoted in the previous footnote.

<sup>14</sup>(\*) On this subject, see the notes "The incident - or body and mind" and "The trap - or ease and exhaustion" , n° s 98, 99.

objective "aspects of a work and a personality - areas of interest, main contributions, principal collaborators or pupils - which void is filled by these "compliments" in dithyrambic style, some of which have already been noted and quoted in the previous note. ...

The series in question, which I have the honor of opening, is made up of the following mathematicians and physicists:

A. Grothendieck, L. Michel, R. Thom, D. Ruelle, P. Deligne, N.H. Kuiper, D. Sullivan, P. Cartier, H. Epstein, J. Fröhlich, A. Connes, K. Gawedzki, M. Gromov, O. Lanford.

I thought I remembered that Dieudonné had been a professor at the IHES at the same time as me, and I see from this list that this is not the case - he had been content to take on the role of director of Publications Mathématiques. However, I now realize, on page 3 of the brochure, in the IHES "Curriculum Vitae", that this is not the case, and that Dieudonné, like me, has been a "permanent professor" since 1958 (and until 1964), at least theoretically. A strange little contradiction! I'm copying here the beginning of the "Curriculum Vitae", at the first two "dates", 1958 and 1961:

**1958** Creation of the Institut des Hautes Etudes Scientifiques association in Paris, by Léon Motchane, assisted by world-renowned scientific advisors and a group of European industrialists.

The scientific activity was launched by two mathematicians: Jean Dieudonné (→1964) and Alexandre Grothendieck (→1970) appointed permanent professors. Issue no. 1 of "Publications Mathématiques de l'IHES" is published.

**1961** Recognized as a public utility.

.....

Incidentally, in this brief Curriculum Vitae, I thought it would be useful to mention the (somewhat symbolic) publication of issue no. 1 of Publications Mathématiques (consisting of a 24-page article by G.E. Wall, the author of which had no particular connection with the newly-born association), but not the algebraic geometry seminars (well known under the familiar acronyms SGA 1 and SGA 2). Wall, whose author had no particular connection with the newly-born association), but not the algebraic geometry seminars (well known under the familiar acronyms SGA 1 and SGA 2) through which I began to single-handedly ensure the scientific reputation of an institution, during years when it barely existed "on paper". Indeed, until around volume 24 of Publications Mathématiques, the bulk of these publications consisted of the successive volumes (1 to 4) of the "Eléments de Géométrie Algébrique"<sup>15</sup> (\*), all the other volumes tour<sup>16</sup>nant around fifty pages each (of high scientific standard, that goes without saying). Moreover, on page 19 (after the series of "minute portraits" from which Dieudonné was absent, God knows why<sup>16</sup> (\*)), we read, in a very "placard publicitaire" layout (with a tantalizing photo of the impressive stack of volumes in their entirety from the prestigious Publications):

#### Publications Mathematics

It was Jean Dieudonné alone [!] who, from 1959 onwards, took Publications Mathématiques to the pinnacle of world excellence.

Since 1979, they have appeared as a regular 400-page annual publication, under the direction of an editorial board headed by Jacques Tits.

Distribution is handled by... (etc)

<sup>15</sup>(\*) Of which I am the author, in collaboration with J. Dieudonné.

<sup>16</sup>(\*) (September 30) It occurred to me that the reason might well be this: so as not to have to say that during the years in question (1958-1964), Dieudonné's time was essentially divided between writing Eléments de Géométrie Algébrique (in which I unfortunately appear as principal author) and Bourbaki essays - apart from the piano and cooking (Dieudonné was both a musician and a cook), which, alas, could not be mentioned in this brochure, too selective for a passing smile to find its way

2. A walk through a work or the Child and Mother  
in. . .

The reason why Publications Mathématiques is singled out in this way - **here**, in this jubilee presentation of a prestigious institution whose main vocation has never been that of a periodical publisher - is undoubtedly to make people forget a certain fact that is unpleasant to some<sup>17</sup> (\*\*): that the said institution would undoubtedly have been written off and forgotten long ago, if for three or four critical years a certain quidam, stubbornly pursuing in his corner ideas of his own (which had the good fortune to catch the eye), had not been able to come up with a solution.

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some, including in the "big world"), had only brought it against all the odds<sup>18</sup> (\*\*\*) a caution and credibility that the "plus beaux statuts d'association du monde, et même les plus beaux "conseillers world-renowned scientists" (sic), are powerless to deliver.

(September 30) The style of this jubilee booklet (which I'm going to get to know very well!) is certainly not that of my friend Pierre, nor of Nico - they surely have other things to worry about, both of them, than composing this kind of occasional text. On the other hand, it's obvious that the two minute portraits I'm interested in, one of me and the other of Deligne, weren't written without Deligne at least providing the words.

-And it's equally clear to me that these two texts, at least, were not delivered to a printer without Deligne having first read them and given the go-ahead. So it seems clear to me from the outset that the two texts in question reflect in any case and first and foremost my friend's dispositions and intentions - the image he strives to give of myself and himself, both to himself and to the mathematical public. It is in this respect, of course, that these two passages interest me. This interest does not depend on whether or not Deligne is the author of these revealing lines, or whether the author is someone else (the one who undoubtedly "thought up" the brochure as a whole), who for one reason or another would have espoused this "message" that my friend wanted to get across.

At the end of these pages, you'll find the two minute portraits, taken from the portrait gallery (pp. 13-19) entitled "Activities of permanent and long-term visiting professors".

ALEXANDRE GROTHENDIECK, mathematician, professor at IHES from 1958 to 1970, Fields Medal.

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During his 12 years at the institute, A. Grothendieck renewed the foundations and methods of algebraic geometry, and opened up new applications for it, notably arithmetic. At the IHES he created<sup>a</sup> a school of algebraic geometry, based on the seminar he led and nourished by the generosity with which he communicated his ideas. The titanic aspect of his work is reflected in his publications, including the treatise "Eléments de géométrie algébrique", in collaboration with Jean Dieudonné (8 fascicules) and the 12 volumes of the "Séminaires de géométrie algébrique du Bois-Marie", in collaboration with numerous students.

In algebraic geometry, he identified the essential problems and gave each concept its greatest natural generality. The notions introduced have proved essential far beyond algebraic geometry. They often seem so natural that it's hard to imagine the effort they cost. If

<sup>17</sup>(\*\*) With all due respect to my friend Nico (who at the time had been director of the aforementioned jubilee institution for twelve years), who surely (on this occasion as on others) saw nothing but fire. ...

<sup>18</sup>(\*\*\*) Against all odds: throughout those four years, I didn't let myself be impressed by the persistent warnings and rumors of imminent bankruptcy of an "adventure" (as some well-informed friends would have us believe. . . ) that was entirely unrealistic, not to say a bit of a smokescreen! The fact is that, at the time, IHES had no financial or land base whatsoever, and its life was constantly dependent on short-term donations from a few more or less well-disposed industrialists. I didn't worry much about this, confining myself to trusting the founder-director Léon Motchane, who managed year after year to "save the day" through feats of financial prestidigitation and "public relations". After all, in these clement times, if things fell apart, I had a good chance of quickly finding a less problematic fallback position! On the other hand, if I won the bet I'd made on IHES (with the encouragement of Dieudonné, who knew Motchane and in whom I had every confidence), my position at IHES suited me better than any other I knew of.

This was undoubtedly facilitated by the great attention he paid to terminology. Let's not forget that the "Grothendieck groups", linked in algebraic geometry to the theory of intersections and used in topology, are the ancestors of algebraic *K-theory*. The topos introduced in algebraic geometry on a general base field to transpose results previously proved on  $\mathbb{C}$  by way of are now used in logic.

He left the IHES in 1970, at a time when his passion for mathematics was waning. Are we to believe that the problems he was tackling along the lines he had set himself had become too difficult?

.....

PIERRE DELIGNE, mathematician, professor at IHES since 1970, Fields Medal, Henri Poincaré Gold Medal, Foreign Associate of the Académie des Sciences.

The main thrust of his work is "understanding the cohomology of algebraic varieties". If the complex algebraic variety  $X$  is nonsingular projective, the theory of harmonic integrals provides a Hodge structure on  $H^*(X)$ . Based on this and on  $l$ -adic analogies suggested by Grothendieck, he derived the notion of mixed Hodge structure and equipped the cohomology of any complex algebraic variety with it. In  $l$ -adic cohomology, i.e. for varieties over a finite field, he proved Weil's proverbially difficult conjectures. This result seemed all the more surprising given that Grothendieck, having constructed the theory of cohomology over any field, had reduced the remaining conjecture to a series of conjectures that are still as unapproachable today as they were then.

This theorem has helped make  $l$ -adic cohomology a powerful tool, applicable to questions in seemingly far removed from algebraic geometry, such as Ramanujam's conjecture.

□ More recently, he has studied Hodge cycles on abelian varieties, taking a first step towards a "motivic theory", just as Grothendieck had dreamed. He also demonstrated the algebraic mechanism of "intersection cohomology", the topological theory of Mac Pherson and Goresky. This enabled it to be transposed into  $l$ -adic theory, where it proved surprisingly useful.

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He is currently interested in non-commutative harmonic analysis (theory of functions on real or  $p$ -adic Lie groups - or finite classical groups - and certain homogeneous spaces), as an extension of his work on automorphic forms (Ramanujam conjecture) and, with G. Lusztig, on representations of finite groups.

He is quick to assimilate and penetrate all mathematics, and as a result has enlightening and constructive reactions to every question put to him.

These two texts need to be supplemented by a third, in which Deligne and I appear in one breath. I found it in a loose leaf inserted in the brochure, under the same title "Orientation des recherches à l'IHES" as the chapter in which the "galerie de portraits" is inserted, with the subtitle: "Note sommaire sur les 'perspectives des activités scientifiques'". This is essentially a draconian "shortening" of the portrait gallery, reduced this time to just the "permanent professors" (present or past)<sup>19</sup> (\*), with two or three lines devoted to each. These are (in the order in which they are cited) myself, Deligne, Michel, Thom, Ruelle, Sullivan, Connes, Lanford III, Gromov. This is the order of the more detailed portrait gallery, except that this time Deligne has "moved up", for the benefit of being quoted in one breath with me. Amusingly, in this text the proper names of the eminences reviewed all appear underlined, with the sole exception of my modest self<sup>20</sup> (\*\*)! Here's the passage about my friend and me:

<sup>19</sup>(\*) (October 1) To keep things in perspective, we've also included Connes (although he's only a "visitor"), so that's one more "Fields Medal" for the collector. On the other hand, my friend Nico Kuiper has been left out. He wouldn't have made a difficult of stepping aside for the occasion. ...

<sup>20</sup>(\*\*) (October 1) The typographic effect of this brilliant process (which may not have been consciously intended) is that

Alexandre Grothendieck's theories of legendary depth and Pierre Deligne's brilliant discoveries (both Fields Medals) have linked topology, algebraic geometry and number theory in "interdisciplinary" ways (cohomology). More recently, this has enabled G. Faltings from Germany (who had previously worked at IHES) to prove a landmark theorem in number theory, which sheds light on the famous "Fermat theorem".

Incidentally, in this mini-gallery, the "Fields medals" have been given a capital M - and "interdisciplinarity" has been a favorite theme of its founder-director since the early days of the IHES. It is perhaps thanks to this circumstance, moreover, that this digest finally seems to imply that my person might have something to do with a certain "interdisciplinary means" called "cohomology" (which also happens to be the "guiding axis" of Deligne's work, by who knows what coincidence).

But here I am, taking this text by the scruff of the neck! The occasional reference to Faltings, who had overnight risen to the top ranks of scientific actuality with his sensational result (described here as "arduous", as if that's what it was all about - but that's beside the point... ) - it too is part of the "little bit" of the text: the scribe's "signature" in short, and hardly worth my attention. It's the first sentence about Deligne and me that obviously contains the essential "message" of the passage. It tells me a lot about certain dispositions in my friend and ex-student - and above all about a profound "Unsicherheit" (insecurity, lack of assurance, deep inner grounding)<sup>21</sup> (\*). Here, no more than in any of the published texts signed by him<sup>22</sup> (\*\*), or in the two minute portraits that preceded it, nothing could make one sup-

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I don't think my friend could have learned anything from me at some point. But here he is, in no uncertain terms, presenting himself as **another father** of a vast unifying vision "taken" from others<sup>23</sup> (\*), as if subjugated by the intimate conviction of his profound inability to conceive himself and allow **his own** visions, as vast or even more vast, to blossom within him ; and as if, in order to be and appear "great", all that remained to him was the derisory resource of **reclaiming for his own account** the halo with which it had pleased him since his youth to surround a prestigious and now defunct elder (or at least, declared as such by a providential consensus . . . ). To take hold of a **halo**, rather than let the still unformed and nameless things within him germinate and blossom, waiting to be born and named - rather than to live out **his own strength**, which lies within him, and which is also waiting. . .

(October 1) Last night, it seemed to me that I was once again at the heart of the conflict - the very one I mentioned in general terms at the very beginning of Harvest and Sowing, eight months ago (in the section

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this passage, which is about to be quoted, appears to be dedicated to PIERRE DELIGNE (whose name appears typographically as the head of the "permanent" line, to the exclusion of my own), and that I am somewhat of a **collaborator**, a stranger to the establishment! Chronological order is certainly respected, nothing to say for sure - and yet the effect produced (and surely intended) is that of a role **reversal**, arousing familiar associations in me (evoked in notes such as "le renversement", "L'éviction", "Pouce", n°s 68', 63, 77). As a result, I've also rediscovered a certain **style of** appropriation - the "Pouce! - style, which clearly identifies me as **the real** author of the message.

<sup>21</sup>(\*) The German word "Unsicherheit" that came to me here has no equivalent in French, nor (I believe) in English. Its literal translation "insecurity" can hardly be applied to a psychological trait. The negative term "lack of assurance" is another makeshift approximation. It's understood that what we're talking about here is "assurance" at a deep level, the lack of which can be perceived on certain occasions, while superficially the impression of assurance, of perfect ease prevails; they form like a protective carapace, of an often considerable, foolproof inertia and "solidity". ...

<sup>22</sup>(\*\*) At least the ones I've seen so far.

<sup>23</sup>(\*) There's a particular irony in the fact, moreover, that this vision, taken here from others as a "halo" for himself, has in fact been scorned and systematically opposed since the master's "death", by the very same person acting as heir while at the same time standing out and repudiating the inheritance. On this subject, see the three notes "L'héritier", "Les cohéritiers. . .", ". . . and the chainsaw" (n° 90,91,92); and for further illustrations, the X procession (Le Fourgon Funèbre), made up of the four "coffins" 1 to 4 and the Gravedigger (notes n° 93 to 97).





"Infallibility (of others) and contempt (of self)", n° 4), and which I found again "in an extreme and particularly striking case", towards the beginning of the Burial (in the note "the knot", n° 65, April 26). This was again an unexpected encounter, at the turn of a quotation that I ended up including in the wake of the other two, out of a sense of conscience! I'd spotted the passage a few days ago, while leafing through the famous booklet, and it struck me at the time, but I didn't dwell on it. But yesterday, once I'd written it down in black and white, it immediately struck me as more meaningful, and more striking, than the two circumstantial passages I'd just copied down and which were supposed to form the main theme of the note I was writing. Yet there was no shortage of places that clicked in these two passages, eliciting associations that I would not have failed, even four months ago, to develop as dryly □ over another ten pages if not not twenty. But it suddenly seemed to me that what I could have developed in this way was basically, with one exception at most, something **already known** that I was finding confirmed, perhaps from a somewhat different angle, and above all: that these were **accessory** aspects after all, the kind of aspects I had dwelt on extensively in the previous "Compliments" note of May (and even throughout my reflection on the Burial). The third passage, on the other hand, brought me back to something **essential**, which I'd tended to lose sight of during the long "investigation" that was (among other things) my work on the Burial.

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I was tempted to leave it at that then, without at least trying to put into words what this single, pithy four-line sentence was saying to me, and which on some level was indeed "heard". In the end, I ignored it. The words came slowly and hesitantly, while the impression, diffuse at first, became clearer as I wrote. Once I'd written it down in black and white, and pruned away what seemed unnecessary, I knew I'd captured what I'd "heard" as well as I'd ever be able to.

It was getting prohibitively late, and I really had to stop there. I went to bed happy, but not yet sure whether I'd include what I'd just written in my testimonial for publication. After all, I might as well leave it to the reader, if he was interested in going beyond the surface of a message, to find out for himself what **he** heard in it! It was only today that I knew I would include this passage, which does express a certain perception or understanding I have (or think I have) of something that seems to me important, even crucial as the mainspring of this Burial.

## 18.2. THE KEY TO YIN AND YANG

### 18.2.1. (1) Muscle and gut (yang buries yin (1))

**Note** 106 (October 2) I'd still like to pursue at least one of the associations of ideas aroused by the three-part Funeral Eulogy (which I finally quoted in full yesterday). This association had occurred to me the day after May 12, when I had just written the note "L' Eloge Funèbre (1) - ou les

compliments" (n° 104). It touches on a certain aspect of things that often breaks unnoticed, and that I only began to really realize □ five or six years

ago  
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Between the lines in the texts examined, we see the cult of certain **values** asserting itself. Thus, what is emphasized about Weil's conjectures, proven by Deligne, is their "**difficulty**"<sup>24</sup> (\*)-not their

<sup>24</sup>(\*) (October 3) Diffi culty described as "proverbial", no less! It makes little sense, other than to impress those not in the know! The "diffi culty" of a conjecture can only really be appreciated once it has been demonstrated - it's its fruitfulness, on the other hand, that can be sensed from the outset, and which often manifests itself objectively, even before it has been demonstrated, through the work it has inspired. The "great" conjectures are not distinguished from the others by their "diffi culty" (which is unknown - even supposing the term has any meaning. . . ), but by their **fecundity**. I note in passing that this is one aspect

**beauty**, their simplicity, the vast perspectives they opened up from the very moment they were enunciated by Weil. I'm thinking, too, of the fruits of these glimpsed perspectives, long before they were demonstrated, and of other glimpsed fruits that now fall at the right moment, once the last step in the long journey to their demonstration has been taken. It is the beauty, the extraordinary internal coherence of these conjectures, and the previously unsuspected links they reveal, that have made them such a powerful and fertile source of inspiration for two generations of geometers and arithmeticians. The most profound part of my work (both the "fully completed" work and the "dream of motifs") is directly inspired by them (through Serre, who was able to capture and communicate the full force of the vision expressed in his conjectures). Without them, neither *l-adic* cohomology nor even the language of topos would probably have seen the light of day. To put it another way, the "vast unifying vision" of (algebraic) geometry, topology and arithmetic that I've been striving to develop over the last fifteen years of my life, is to be found in these "Weil conjectures". And as the vision gained in breadth and maturity, it was this vision itself, and the previously hidden things it enabled me to apprehend one by one, that told me step by step what to do, by which end to "take" what was at hand. The last step in the demonstration of Weil's conjectures was neither more nor less than one of the steps in a long and fascinating journey that began, I can't say when, long before I was born, and which, after my death, will not be completed any time soon!

p. 463 □ But following the spirit one detects in the quoted text, one could believe that Weil's "conjectures" were a question of weights: here's the weight to lift "à l'arrachée"! Two hundred kilos is no mean feat, the difficulty is proverbial, many have tried it and not one yet has been able to do it - until "H-day" (like "Hercules")! The result was astonishing (106<sub>1</sub>), just think of the two quintals - no one would have believed they'd ever manage it. ... The same spirit can be perceived in the laconic commentary on the "difficult theorem" proved by Faltings: here again, in the very designation of this new stage in our knowledge of things, it is the **difficulty** again that is highlighted, to arouse the admiration of the crowds - not the perspectives that open up, starting from a new summit crossed<sup>25</sup> (\*). It didn't even seem worth mentioning the name "Mordell's conjecture" (admittedly unknown to a non-mathematical audience) - as if the apprehension and formulation of the conjecture (here, by Mordell) were an accessory, because "easy". Instead, a biddon perspective on "Fermat's theorem" (which is supposed to be "enlightened"). It's true that the latter is universally known (even outside mathematical circles) as a weight of well over three hundred kilos (which has withstood three centuries of effort).

The first point I wanted to make is that the values exalted in these texts (with the discretion befitting the occasion, of course) are those that can be called the **values of muscle**, of the "cerebral muscle" in this case: the one that makes it possible to surpass, by sheer strength of wrist, proverbial records of "difficulty".

These are not just the values of the hero in the spotlight, like those of the author of a certain jubilee brochure (an anonymous author whom I think I recognize). They are also the values increasingly (does it not seem) dominate the mathematical world, and more generally, the scientific world. Even beyond this world, which is still relatively small, □ <sup>we</sup> can say that these are also, and increasingly, the values of a certain "culture", described as "Western"<sup>26</sup> (\*). Nowadays and since

p. 464 of a thing, while "diffi culty" is a typically "yang", "masculine" value.

<sup>25</sup>(\*) What struck me most, from the moment I held in my hands Faltings' preprint in which he proves three key conjectures, including Mordell's (discussed here), was on the contrary the extraordinary **simplicity** of the approach, by which he proves in some forty pages these results, which were supposed to be "out of reach"! (Compare note n° 3.)

<sup>26</sup>(\*) When I refer here to the "values" of our culture as they appear today, I'm of course referring to the "values" of our culture as they appear today.

It's been a long time since this "culture" and its values conquered the surface of our planet, wiping out all others as irrefutable proof of their superiority. The planetary symbol, the heroic embodiment of these values, is the cosmonaut in his waterproof armor, the first to set foot on some unimaginably remote and desolate planet, in front of millions of breathless TV viewers slumped in front of their screens.

These values, which in the absence of a closer definition I've limited myself to a brief term of symbolic value, "muscle", are not new. In ethnologist's jargon, we could also call them "patriarchal". One of the first written texts, it seems to me, in which their primacy is forcefully asserted (force without reply!) is the Old Testament (and more particularly, the book of Moses). And yet, one only has to read this fascinating document from a remote era to realize that the primacy of "patriarchal" values, that of man over woman, or that of "spirit" over "body" or "matter", was far from going as far as the negation or contempt of complementary values (which were perhaps not yet perceived as "opposed" or "antagonistic")<sup>27</sup> (\*\*). I don't know if the history of the vicissitudes of these two sets of complementary values has ever been written - and it must be a fascinating thing to pursue this history, through centuries and millennia, from the time of Moses to the present day. It's also the story, no doubt, of the gradual degradation of a certain balance of "values", "patriarchal" or "masculine" on the one hand, "matriarchal" or "feminine" on the other - of "muscle" and "guts", of "spirit" and "matter"; a degradation that has visibly moved in the direction of "male" values (or "yang", in traditional Eastern dialectics), to the detriment of "feminine" values.

□ It seems to me that our era is characterized as one of excessive exacerbation of this degradation. The last acts of this history include the closely interrelated "space race" between the two antagonistic superpowers (imbued with essentially identical values), and the arms race (nuclear in particular). As the final act and probable outcome of this relentless evolution in the outbidding of a certain type of "force" or "power", we can already foresee some nuclear holocaust (or other, there's an embarrassment of riches to choose from. . . ) on a planetary scale. Perhaps it will have the merit of solving all problems at once and once and for all... .

My intention here, however, is not to paint a tantalizing picture of the "end of the world" (they didn't wait for me to do that), and even less to wage war against "muscle", or "brain" (aka "mind"). I know that even my "guts" would have nothing to gain from it! I value my muscles and my brain, which I'm sure are very useful, just as I value my "guts", which are no less useful. Rather, I'd like to say a few words here (if I may) about how this profound conflict between these two types of values, conveyed by the surrounding culture, has played out in my own life. In more down-to-earth terms, it's also about the history of my attitudes (of acceptance or even exaltation, or rejection) of two equally real and tangible **aspects** or **faces** of my person, inseparable and complementary by nature, and in no way antagonistic in themselves. I could call them "**the man**" and "**the woman**" in me, or also (to take less "loaded" appellations, and which therefore offer less risk of misleading), the "**yang**" and the "**yin**".

It would seem that for most people, the "chips are down" from early childhood, when the

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"official" values - those conveyed by schools, the media and the family, and which are the subject of a general consensus in various professional circles. This does not mean that these values are unreservedly accepted by all, nor that they form the basis of everyone's attitudes and behaviour. Indeed, it is with distress that honest people, the media and competent professional literature (from the pens of educators, sociologists, psychiatrists etc.) speak of a "certain youth" in particular, who decidedly don't "fit in" and who don't fit in with a certain picture!

<sup>27</sup>(\*\*) Mother worship, for example, is a deeply rooted tradition in Judaism, which no doubt serves to compensate for the "official" values (so to speak) emphasized in the sacred texts. This tradition is found again, in a modified and more exalted form, in the Catholic tradition, with the cult of (the Virgin!) Mary.

in place The essential mechanisms which, for the rest of our lives, will silently dominate our attitudes and behavior with the efficiency of a perfectly tuned automaton. At the heart of these mechanisms are those of affirmation or rejection of such and such traits in us, or of such deep-seated impulses, with either a yang or yin "signature", or of such and such "packages" of traits and impulses with a given signature, or even of the entire "yang" or "yin" package. It is these mechanisms which, to a very large extent, determine all the others. choice mechanisms (affirmation or rejection) structuring our "self".

p. 466 □ For reasons that still remain mysterious to me, in my own case the history of relationships (both (both in the "boss" himself and in the "worker", both of whom are dependent on the double yin-yang aspect of all things) - this history has been more eventful than usual. I can distinguish three periods. The last, in a sense, is the same as the first, which spans the first five years of my childhood. This third period, which I can call that of **maturity**, can be seen as a kind of "return" to that childhood, or as a gradual reunion with the "**state of childhood**", with the harmony of the uneventful marriage of "yin" and "yang" in my being. This reunion began in July 1976, at the age of forty-eight - the same year I made the discovery (three months later) of a hitherto unknown power within me, the power of meditation<sup>28</sup> (\*).

The dominant values of both my parents, my mother and my father, were yang values: willpower, intelligence (in the sense of intellectual power), self-control, dominance over others, intransigence, "Konsequenz" (which means, in German, extreme coherence in (or with) one's options, particularly ideological-logical), "idealism" in both political and practical terms... . In my mother's case, this valorization took on an exacerbated force from an early age, and was the flip side of a genuine hatred she had developed towards "the woman" in her (and from then on, towards the feminine in general). (I myself only discovered these things five years ago, three years after meditation came into my life). In such a parental context, it's a mystery (and yet a fact that's beyond doubt for me) that I was able to blossom fully during the first five years of my childhood - right up to the moment when my parents, my older sister and I were uprooted from our parental environment and my family of origin (made up of my parents, my older sister and myself) was destroyed by my mother's will and the political events of 1933.

p. 467 **Note 106<sub>1</sub>** □ (October 3) Neither I nor Deligne have ever had the slightest doubt that Weil's conjectures may not be valid, and I don't recall anyone expressing such doubts. Describing the "result" (i.e. the proof of these conjectures) as "surprising", again shows a deliberate intention to impress. In fact, at no time since the introduction of "topology" and scalar cohomology have I felt that these conjectures were beyond my grasp, but rather (from 1963 onwards) that they were bound to be proved within the next few years. When I left in 1970, I had little doubt that Deligne, who was in the best position of all, would soon prove them (which he did), along with the stronger "standard conjectures on algebraic cycles" (which, on the other hand, he set out to discredit).

Indeed, Deligne is right to express reservations about the validity of the latter conjectures, of which I'm no more convinced than he is. But the scope of a conjecture doesn't depend on whether it turns out to be true or false, any more than its so-called "difficulty" renders it "out of reach".

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<sup>28</sup>(\*) See the two sections "Desire and meditation" and "Wonder", n°s 36 and 37.

- is entirely subjective. It depends solely on whether the **question** on which the conjecture puts its finger (and which had not been perceived, before it was asked) - whether this question touches on something truly essential to our knowledge of things. It's obvious (to me, at least!) that there can be no question of having a good understanding of algebraic cycles, nor of the so-called "arithmetic" properties of the cohomology of algebraic varieties (or, indeed, of "pattern geometry"), as long as the question of the validity of these conjectures remains unresolved. Even today, as at the Bombay Congress in 1968, I consider this question, along with that of the resolution of singularities, to be one of the two most fundamental issues in algebraic geometry. I'm well aware of the significance of both! This potential fruitfulness cannot fail to manifest itself, as soon as we no longer limit ourselves to bumbling around a conjecture that has been declared "too difficult", and someone finally takes the trouble to roll up their sleeves and get to grips with it!

## 18.2.2. (2) Story of a life: a cycle in three movements

### 18.2.2.1. (a) Innocence (the marriage of yin and yang)

**Note 107** □ (October 4) I have already had occasion to mention an important aspect of these first five p. 468 years of my life, as a "privilege" of great price<sup>29</sup> (\*): a deep and unproblematic identification with my father, which has never been touched by fear or envy. I became aware of this circumstance, and of the very existence, as well as the silent strength, of this identification with my father, only four years ago (during the meditation on my childhood and my life that followed the one from August 79 to March 80 on my parents). This identification was like the peaceful and powerful core of an identification with the family we formed, my parents, my sister (who was four years my senior) and me. I had boundless admiration and love for both my father and my mother. For me, they were the measure of all things.

This in no way means that my attitude towards them was one of automatic approval, of blissful admiration. I probably didn't know that they were the measure of all things to me, but I knew full well that they were fallible like me, and there was no fear in me that would have prevented me from noticing a disagreement and making it clear. In the conflicts that surrounded me, I wasn't afraid to take sides in my own way. This had nothing to do with a certain faith and self-assurance that formed the deep, unshakeable foundation of my being - rather, it flowed spontaneously from that very faith and self-assurance. Sometimes my father, in fits of impotent anger when my sister (without seeming to) took pleasure in provoking him, would strike her brutally - and each time I was outraged, in an outburst of unreserved solidarity with my sister. I think these were the only big clouds in my relationship with "my father" (there were none with my mother). It's not that I approved of my sister's sometimes pitiful tricks, nor, I think, that they really troubled me - **she** wasn't the measure of things for me. Her tricks (the reason for which surely escaped me as much as it did my father, who always "worked", or my mother, who never intervened either before or after) - in a way, these tricks didn't really have any consequences for me. She was my sister, just the way she was, that was the trick. But **my father** let himself be to such blind brutality...

The three people closest to me, who together formed the matrix of my early years, were torn apart by the conflict between <sup>each</sup>□<sup>of</sup> them and themselves, and between them and the other two: an insidious flict, aup between my mother and my sister, and violent conflict between my father and my mother of a

<sup>29</sup>(\*) See the note "The massacre", n° 87.

on the one hand, and my sister on the other, who each on her own account (and without anyone during my parents' lifetime ever pretending to notice. . . ) made it work in their own way. The mysterious, extraordinary thing was that, surrounded by conflict in these most sensitive, crucial years of my life, it remained **outside** me, that it didn't really "bite" into my being in those years and settle there permanently.

The division in my being, which has marked my life as much as anyone else's, didn't take hold in those years, but in the two or three that followed, from my sixth to my eighth year or so. At a certain point (which I thought I could pinpoint to within a few months of my eighth year), there was a certain **turning point**, after more than two years of separation from my parents (who didn't bother to give me any sign of life) and from my sister. It was above all a **break with my childhood**, "buried" from that moment onwards by effective forgetting mechanisms (which have remained in place, more or less, to this very day). At some deep level (not the deepest though. . . ) my parents were then declared by me to be "foreigners", just as my childhood was now declared to be "foreign". I **abdicated**, in a sense: to be accepted in the world that now surrounded me, I decided to be like "them", like the adults who made the law there - to acquire and develop the weapons that command respect, to fight on equal terms in a world where only a certain kind of "strength" is accepted and prized... .

It was this strength, too, that was favored by my parents, who had surrounded me in my early years. And here I come back to that "mysterious thing" (from which I've just moved away, following the thread of another association aroused by this thing), the **absence of division in me**, in those early years of my life.

Perhaps the mystery for me no longer lies in this absence, but rather in this: that my parents, both my father and my mother, then **accepted me in my totality**, and totally: in what in me is "virile", is "man", and in what is "woman". Or to put it another way: that my parents, both torn apart by conflict, each denying an essential part of their being - each incapable of a loving openness to himself and to each other, as of a loving openness to my sister... . that nevertheless they found such openness, such unreserved acceptance, toward me □ their son.

To put it another way: at no time in these first five years of my life have I felt **ashamed of who I am**, whether in my body and its functions, or in my impulses, inclinations and actions. At no time have I had to deny anything about myself in order to be accepted by those around me and live in peace with them.

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Of course, there were times when I did things that just didn't "fit": like all children, I was bound to be a pain, even unbearable, when I got down to it - and it was clear from time to time that I needed to put things right. I didn't lay down the law, nor was I tempted to, not having to compensate for some secret mutilation. And in my parents' love for me, there could have been no room for adulation, for indulgence in whims - for unconditional approval. But while I was bound to be "sent packing" by my father or my mother (just as the reverse could sometimes happen), neither of them ever made me feel ashamed of an act or behavior that didn't please them.

Against the backdrop of a deep, unambiguous identification with my father, I see myself as a child, imbued with both virility and femininity, both strong.

It seems to me that in each being and in each thing, in the indissoluble and fluctuating marriage of the yin and yang qualities within it that make it what it is, and whose delicate balance is its profound beauty,

the harmony that lives in this being or thing - that in this intimate union of yin and yang there is often (perhaps always) a background note, a "dominant", which is either yin or yang. This background note is not always easy to detect in a person, because of the more or less effective and complete mechanisms of repression, which distort the game by substituting a borrowed image for an original harmony. So my "brand image" for forty years was almost exclusively masculine - without ever being questioned or even detected as such, either by myself or (it seems to me) by others, until my forty-eighth year. I tend to believe, however, that the background note present at birth remains present throughout one's entire life, at least in deep layers that will perhaps never find their way to the outside world.

the opportunity to come out into the open. In my own case, strangely enough, I still don't know what to make of it. say what this dominant note is, the one therefore □ which permeated my early childhood and which was "mine"

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when I was born. Various signs have made me suspect more than once that this note is "yin", that it's the "feminine" qualities that dominate in my being, when it finds the opportunity to manifest itself spontaneously, in the moments when it's free from all kinds of conditioning that have accumulated in me since childhood. To put it another way: it could be that what is the creative force in my body and mind, what I've sometimes called "the child" or "the worker" in me (as opposed to the "boss" who represents the structure of the self, i.e. what is conditioned in me, the sum or result of the conditioning accumulated in my person) - that this force is even more "feminine" than "virile" (whereas by nature and necessity it is one, and the other).

This is not the place to go into all these "signs". The important thing is not whether this deep dominant note in me is "feminine", or whether it is "virile". Rather, it's that I know how to **be myself** at every moment, welcoming without reticence both the traits and impulses within me by which I am "woman", and those by which I am "man", and allowing them to express themselves freely.

When I was a child, in those early years, it wasn't unusual for strangers to mistake me for a girl - without this ever creating the slightest sense of unease or insecurity in me. It was mainly my voice, I think, that had this effect, a very clear, high-pitched voice - not to mention the fact that I had long hair (mostly disheveled), perhaps simply because my mother (who had plenty of other things to worry about) didn't often take the time to cut it for me. I was also as strong as a Turk, and I didn't mind playing violent or daredevil games, although I did have a penchant for silence, even solitude, and a penchant for playing with dolls<sup>30</sup> (\*). I don't remember anyone making fun of me for this, but it certainly happened here and there. If such incidents passed without leaving a trace of injury or humiliation, it's surely because they were not echoed or amplified by any feeling of insecurity in me, while the acceptance of who I was, by those who alone really mattered to me, was beyond question. Mockery couldn't have reached me, it could only be turned against the one who must have seemed so foolish to me, to pretend to find fault with the most natural thing in the world.

□ I was well aware, moreover, that this kind of rather strange silliness is by no means an uncommon thing, that the mere sight

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nudity can cause scandal! Yet for as long as I could remember, I'd had every opportunity to see my mother, father and sister naked, and every opportunity to satisfy my legitimate curiosity as to how each of them and myself were made. It was quite obvious that there was no cause for scandal in the conformation of either men or women, which seemed to me decidedly fine as it was.

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<sup>30</sup>(\*) If this inclination seems rare in little boys, I think it's mainly because it's systematically discouraged by those around them.

it was - and more particularly (I made no secret of it) that of women.

### 18.2.2.2. (b) Superpère (yang buries yin (2))

**Note** 108 (October 5) It was in 1933, when I was in my sixth year, that the first crucial turning point in my life took place, which was at the same time a crucial turning point in the lives of both my mother and father, in their relationship to each other and to their children. It was the episode of the violent and definitive destruction of the family we four formed, a destruction of which I was the first and only person, forty-six years later, to acknowledge and follow the events, in my parents' correspondence and in one or two exsanguinated, enigmatic and tenacious memories, patiently probed and deciphered - long after my father's death and that of my mother<sup>31</sup> (\*).

It's not my place to expand here on what I've learned and understood in the course of this long work, about the significance and meaning of this episode. Three days ago, I already alluded to this turning point<sup>32</sup> (\*\*), as marking the abrupt end of the first of the three great periods in the history of the marriage of yin and yang within me. In December 1933, I found myself hurriedly dumped into a foreign family that neither I nor my mother, who had brought me there from Berlin, had ever seen. In fact, these unknown people she was taking me to were simply the first people who would take me in as a "boarder" for a very modest pension, and with no guarantee whatsoever that it would ever be paid.

p. 473 my mother was preparing to join my father as soon as possible, who was moping around waiting for her in Paris. It was an *en-tendu* thing between my parents that everything was going to be for the best so much for me in Blankenese (near Hamburg),

than for my sister, who for a few months had been dumped at the end of the day in an institution in Berlin for handicapped children (where she had been accepted, even though she was no more handicapped than me or our parents).

At the end of six strange months, heavy with dull menace and anguish, I found myself overnight in a world totally different from the only world I'd known in my life, the one formed by my parents, my sister and me. I found myself as one of a group of boarders, eating separately from the family and looking like second-class children to the children at home, who formed a world of their own and looked down on us. From my mother I received a hasty, stilted letter from time to time, and from my father never a line in his hand, during the five years I stayed there (until 1939, on the eve of the war, when I finally rejoined my parents under the pressure of events).

The couple who took me in quickly endeared themselves to me. Both he, a former pastor who had left the priesthood and lived on a meagre pension and private lessons in Latin, Greek and mathematics, and his vivacious and sometimes mischievous wife, were unusual people, endearing in many ways. He was a humanist of vast culture who had lost his way a little in politics, and had run afoul of the Nazi regime, which eventually left him alone. After the war, I renewed my acquaintance and remained in close contact with them until both died<sup>33</sup> (\*).

From him and especially from her, as from my parents, I received the best as well as the worst. Today, with the benefit of hindsight, I am grateful to them (as I am to my parents) for the "best", as well as for the "worst". It was this best and worst that I received, first from my parents, then from them, that formed the bulk of the voluminous "package" I received as a child (as everyone receives the

<sup>31</sup>(\*) My father died in Auschwitz in 1942, my mother in 1957. The work I'm talking about here took place between August 1979 and October 1980.

<sup>32</sup>(\*\*) See the end of the note "Yang buries yin - or muscle and guts", n° 106.

<sup>33</sup>(\*) She died at the age of 99, two years ago, and I was able to see her dead again, face to face with her, the day before the funeral.



his. . . )j, which it was up to me to unpack and examine. They are part of the substance, the richness of my past, and it's up to me to nourish my present.

My new environment was all very "proper" and conformist in many ways, with in any case the repressive attitudes de rigueur for everything to do with the body and, more particularly, sex. It took □pourtant several years, I think, before I internalized and took p . 474 back to myself. these attitudes, like the shame of showing myself naked, go hand in hand with an ambiguous relationship with my body. This shame, inculcated from an early age, is one aspect of a deep-seated division, where the body is the object of tacit contempt, while so-called "cultural" values (confused with intellectual capacity for memorization and the like) are held in high esteem. This division within me remained ignored until my forty-eighth year, when it began to be resolved. This was the second great turning point in my life, marking the advent of the "third period" in the history of my relationship to myself, i.e. if that of my relationship to my body, and to the "man" and "woman" in me. But before that, I had ample opportunity to help pass on this division to my children<sup>34</sup> (\*), whom I could see passing it on in turn. ...

I alluded yesterday<sup>35</sup> (\*\*) to the "changeover" that finally took place within me. Delayed by more than two years after the uprooting from the initial family environment (or, better said, after the **destruction of** this environment), this shift consecrates the setting up of the common repressive mechanisms, from which my childhood had until then had the rare good fortune to be exempt. So far, I've detected two major forces of a repressive nature, which dominated my adult life and a large part of my childhood (108<sub>1</sub>). I think it's fair to say that they didn't emerge gradually, but that in my case they appeared more or less overnight and in their full force, as the consequence of a deliberate **choice**, at an unconscious level. I described this choice earlier as "abdication", but at the same time it was also a powerful principle of action: "I'll be like 'them'" (and not "like me") also meant; I'm going to "bet" on "the head", no worse in me than in anyone else after all, and fight and "them" fight with their own weapons!

One of these mechanisms, and the one I'm most interested in here, is one of the most common: the **repression of my "feminine" traits** (or those felt as such by common consensus), in favor of "masculine" values. The other side of the coin was, of course, to invest fully in my traits and aptitudes. and the over-development of these, which have taken on a disproportionate role.

□If anything here is out of the ordinary, it is not of course the mere **presence of** this double mechanism, p . 475 nor (it seems to me) the strength of the "repressive" component itself, the strength of the repression of "yin" traits, attitudes and impulses. There's no comparison here with what happened to my mother, whose life (and that of her loved ones) was devastated by her lifelong hatred of what made her a woman. At no time, I think, were my ways entirely devoid of a certain gentleness, even tenderness, which stubbornly rounded out the character I'd carved out for myself as a child, and often won me sympathy and affection. The exceptional side of me is rather in the excessiveness of my investments, in the **excessiveness of** the energy I invest in my tasks, without letting myself be distracted by a glance to the right or to the left! When I'm not working, my mind is constantly focused on the completion of some stage of the job. This attitude ("Zielgerichtetheit" in German, "aimdirectedness" in English) is par excellence a yang attitude, an attitude of **tension**, of **closure** to everything that doesn't seem directly linked to the task.

This excess was likely to conjure up in others the image of a kind of "super-man" or "super-male",

<sup>34</sup>(\*) At least, the four of them I helped raise. The fifth and last one is being raised by his mother, and so far there hasn't been a single opportunity for us to get to know each other.

<sup>35</sup>(\*\*) See the beginning of the previous note "Ecllosion de la force - ou les épousailles", note n° 107.

admirable, alas (given prevailing values), but immediately arousing (at a level that remains unconscious most of the time) instinctive reactions of defense or even antagonism in the face of such a display of force, perceived as threatening or even aggressive, or in any case dangerous (108<sub>2</sub>). Above all, this image irrevocably evokes the image of the "**super-father**", and immediately sets in motion the ambiguous multiplicity of reactions of attraction and repulsion knotted around the age-old conflict with the father. ... This is **my** contribution to the **ambiguous** relationships that have been so common in my life, and with which I found myself confronted so many times in the course of Harvest and Sowing. This ambiguity is reinforced, not diminished, by the persistence of yin traits in me, which fuel a sympathy that the mere hypertrophy of yang traits into a kind of gigantic "superman" would be powerless to arouse.

And once again I can see, in these endless "relationships of ambiguity", that I'm still only reaping what I've sown myself, even if each time the harvest turns out to be unexpected (and unwelcome. . .)! For hasn't the motivation (or at least **one of** the motivations) that drives "the boss" in me to constantly surpass himself in the accumulation of works, been precisely to constantly force and boost the esteem of my peers (in the first place) and of my blunders (moreover); to hear some of the best lament that they can't keep up with me, at the rate I'm going?! Yes, there has been in me this secret desire to arouse in others (as in myself) this "larger-than-life" image, disproportionate-like the very one it reflects-and which obstinately returns only through the other: in clear, lofty words, through the praise expected (and cashed in like a due) - and **also**, through the deep, obscure channels of muted enmity and conflict... ...<sup>36</sup>(\*)

**Note 108** □ (October 6) I mean that the forces of a repressive nature at play in my life, seem to take mostly, if not exclusively, one of two specific forms: burying the past, and emphasizing my "virile" features to the detriment of my "feminine" ones. I don't mean to say that these two "forces", both repressive in nature (i.e., aimed at "repressing", at suppressing a certain reality), are the only ones that have "dominated my life"! To do so would be to forget the whole non-egotic aspect of my being, the drive for knowledge expressing itself at the level of the body as well as the mind. (On this subject, see

in particular "My passions", section n° 35.)

Even among the forces structuring the ego, emanating from the "boss", there is at least one which is not repressive in nature, which predates the forces of repression by far, and whose role in my life has been even more essential: identification with my father, who has been like "the peaceful and powerful heart" of the feeling of my own strength. This identification in no way exalted certain values or qualities (let's call them virile) to the detriment of others ("feminine"). Irrespective of the values my father professed, his personality (until 1933, when a shift took place in him<sup>37</sup> (\*)) was marked by a strong yin-yang balance, in which intuition and spontaneity were no less important than intellect and willpower.

Finally, another important "force" of an egotistical nature, intimately linked to repressive mechanisms (or, to put it better, of a "repressive" nature itself), is **vanity**, which has played as heavy a role in my life as in anyone else's. But this "force" is so universal in nature, as is the dominant role it plays in everyone's life (in more or less coarse or subtle form), that there's hardly any need to mention it. But this "force" is so universal in nature, just like the dominant role it plays in everyone's life (in a more or less coarse or subtle form), that there's hardly any point in expressly including it in a list of specific forms.

<sup>36</sup>(\*) (October 6) To tell the truth, the "secret desire" to which I've just put my finger, has not yet been consumed, even if it has been detected in the meantime (just a few years ago. . .), and is less all-consuming today than it was in the past.

<sup>37</sup>(\*) Remarkably, this "shift" in my father (then aged 43) was towards a **super-yin** state, towards a kind of pasha-like passivity, in close connivance with my mother, playing a super-yang role. She took charge of him instead of her children. (They were left to "profit and loss", at least until 1939, the year in which, under the pressure of events and against her will, she finally took me back to her... .) This relationship of dependence on my father, and the yin-yang role reversal between my parents, lasted until my father's death in 1942.

the forces and mechanisms that structure the ego and give it its particular physiognomy and foundation.

**Note 108** □(October 6) In this "deployment of force" there is no "aggressive" intention in the common sense p. 477 of the term, neither conscious nor unconscious, only an unconscious desire to impress, to force esteem. It's true that the term "forcing esteem", which comes back to me spontaneously, already carries a connotation of **constraint**, close to that of "aggression". This unconscious intention to coerce, also perceived at an unconscious level, must often be experienced as a kind of aggression (even though this experience remains hidden, as do the antagonistic reactions it triggers). At the same time, this experience must often be conflated with similar childhood experiences involving the father as the protagonist, where the latter appears as the main holder of repressive authority, or even as a crushing rival, envied and hated.

Even without such an amalgam, and independently of any perception in others of a "constraint" intention in me, there must often be the perception of a strong **imbalance**, a fundamental disharmony, in this exclusively yang "deployment of force" (in spirit and intention, at least). This excess is detrimental to the main person concerned, myself, and in fact "dangerous" to his very physical survival (as the health incidents of recent years have clearly shown me!). This is undoubtedly what I was thinking about when I wrote that "such a display of force" was felt "in any case to be dangerous" - dangerous "by nature", an example not to be followed... ! Surely such a feeling is enough to provoke "defensive reactions", even in the absence of any aggression or intention to aggress.

It's true that such relationships of ambiguity recurred after 1976, with some of my students in particular, at times when any mathematical investment was absent, and when there was no apparent "de- poyment of force" in my life. It's also true that the "deployments" in question in the **past** have created a **reputation**, which continues to stick to me, especially in my professional life, and which to some extent replaces the perception of who I am **in the present**. What's more, I've acquired such an ease in dealing with certain mathematical subjects that, even outside my mathematical periods and with the help of my reputation, this natural ease or mastery can already have the effect of a "deployment of force", on unmotivated students, and make them feel me to be (in spite of certain pleasant, even reassuring) as a kind of Superman (a little Superpère around the edges!).

□ Besides, as a flip side of the ease I'm talking about, I often tend to underestimate the difficulty that This tends to place them at odds with my expectations. (On this subject, see the note "Failed teaching (1)", n° 23 iv.) Such a situation must quite often be one of the important ingredients of a false relationship. to the father... p. 478

### 18.2.2.3. (c) The reunion (the awakening of yin(1))

**Note 109** (October 9) Four days ago, as I finished my previous note<sup>38</sup> (\*), I felt very happy. I found myself unexpectedly reconnecting with an intuition that came to me on a certain Sunday, October 17, 1976 (eight years ago, give or take a few days) - the intuition of the devastating effect, in my life as well as in my mother's, of a "certain force" within me. It was the first time in my life that I had given any thought, however brief, to what my life, and above all my childhood, had been like. It was also the day after

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<sup>38</sup>(\*) See note "Yang buries yin - or the Superfather", n° 108.

of the day I had discovered the power of meditation<sup>39</sup> (\*\*), and it was the first time since then that I had made use of this power, so long ignored. It was not by design, but by the effect of a profound impulse, as if moved by a very sure instinct, that the reflection that day ended up being directed towards my childhood. Only in retrospect can I appreciate the extent to which it was the source of my true strength, as well as of the conflict and division within me, that a deep need to know had carried me to that point. For almost three years, I would not return to it, distracted as I was by the "order of the day", without realizing that I remained on the periphery of the conflict in my life, while stubbornly staying away from the heart of it: from that childhood drowned in mists, which seemed so infinitely far away... .

p. 479 I've just gone through again, "diagonally", the eighteen exceptionally dense leaves of this crucial meditation in my life. It was during the night that followed this meditation, or rather in the early hours of the morning after this night of meditation, that I had a dream of shattering force - the first dream of my life too, whose message I probed passionately. I was no more aware then of where I was going and what was happening, than I had been the day before when I was "discovering meditation". Du□rant four hours I delved into the meaning of this experience, this dream-parable, through successive layers of increasingly burning meaning, before arriving at the heart of the message, its simple and obvious meaning.

It wasn't like a sudden understanding of "intelligence", nor even like a sudden light in a darkness or half-light. Rather, it was like a deep wave born within me and suddenly surging through me, and in its vast waters bringing me that sense which had eluded me until then: that I was at this moment reuniting with a very dear and precious being, whom I had lost since childhood... .

That moment felt like a **birth**, like a profound renewal. This feeling remained strong throughout that day, and again in the days that followed. Looking back over eight years, that moment still seems to me to be the most creative of all in my life, and an essential turning point in my spiritual adventure. It had certainly been prepared by many other "moments" in the days and months leading up to it. Perhaps the first precursor was that "salutary uprooting", more than ten years earlier, from an institution where I had intended to end my days<sup>40</sup> (\*). These earlier moments seem to me to be the ingredients, or rather the **means** at my disposal, with which I was able to cross this other "threshold" that lay before me unnoticed, at a deeper, more hidden level than others I had crossed. Everything was in place, for a few days or hours, for me to cross it - and I could cross it, just as I could not cross it, day after day for the rest of my life. ...

And with this threshold well and truly crossed, the way was opened to other crossings, to other "awakenings", each of which by its very nature is also a renewal, and to some extent, a "new birth", a re-birth. I've avoided some of them for months, even years, before finally taking the plunge, relieving myself of some nagging illusion that for a lifetime had stood between me and the full flavor of my life and the world around me. And surely, there are some I continue to evade, even as I write these lines. ...

p. 480 □ In the light of reflection over the last few days, it's that moment of reunion with my childhood, believed lost and dead for a long life, marking the end of the "second period" of my spi- ritual itinerary: that of the predominance, in my personal life, of **egotistical mechanisms**, against the creative forces, the forces of knowledge and renewal, which had gone through an almost complete stagnation of forty years. It was also the time of the predominance of a "certain force", of a force of an almost exclusively "virile" nature, in the image of the values in honor in the surrounding world, at the expense of the "masculine".

<sup>39</sup>(\*\*) See "Desire and meditation", n° 39.

<sup>40</sup>(\*) See note n° 42, of the same name.

deep "feminine" aspects and forces of my being, ignored and repressed (with never complete success, thank God!).

The very first intuition about the destructive nature of this force, which had dominated my life as well as my mother's, and that of other women who had been important in my life - this intuition made a brief appearance in these days of intense maturation, thanks no doubt to the re-emergence of yin, "feminine" energy, in my conscious apprehension of things. Contrary to my hasty recollection earlier, this appearance did not take place during meditation on the eve of the reunion, but a few hours afterwards, in a short meditation on the meaning of what had just happened. The intuition is born and takes shape at the very end of the few pages of notes from this meditation. I perceive the destructive nature of this "force" (which today I would call "superyang force", i.e. excessively yang-dominant) first in my mother, then in other women, and then in these final lines:

"As for the 'strength' in myself, it was certainly this that made me the target and object, expected throughout a young life, of the secret hatred and resentment of M., then J., then S. - of a hatred deposited in them long before they knew of my existence, in the distraught days of a childhood deprived of love."

In fact, the word "childhood", in the last line again, which bears witness to one of the most important days of my life, appears for the last time for almost three years! As for the intuition about the nature of the superyang force in me, as provoker of antagonistic reactions, even of hatred and resentment, it tended (it seems to me) to sink a little into oblivion until the very last few days. To be more precise, it remained present only in my perception of certain important relationships in my life (and especially, relationships with women I've loved). On the other hand, she hardly penetrated really p. 481 situations.

<sup>41</sup> (\*), with certain students in particular, as I've had to examine or evoke many times in the course of Harvest and Sowing. Throughout all this reflection, the fact that I myself, by a kind of involuntary "provocation", made my own contribution to the conflict situations I evoked or examined here and there - this fact often remained completely hidden, whereas the protagonist's contribution was quite clear to me. This is, of course, a very common reflex, not to say universal! My reflections over the last few days have defused this reflex and, at the same time, brought it back into focus for me - by bringing me face-to-face with myself - with **a certain** me, at least - at the end of the road (a reflection on yin and yang. . . ).

My brief reflection of four days ago barely scratches the surface of the many aspects of my personality that made the yang imbalance in the "character" I'd been playing since childhood felt; and the crushing effects this imbalance could sometimes have on others. On those in particular where the yang-type strength was still lacking - and first and foremost on my own children. I'm thinking here above all of a certain "mode" of peremptory assurance on which I operated, in all the things (and there were many) about which I had, rightly or wrongly, a way of seeing or feeling, or strong opinions. Of course, the idea would never have occurred to me to impose these ways of seeing on anyone, least of all on my children - and thanks to this absence of any desire for constraint within me (at least on a conscious level), I was unable for most of my life to realize the extent to which these ways of being within me (which seemed spontaneous and natural to me, and whose complex nature I was far from discerning. . . ) - the extent to which they had an impact on my life. ) - to what extent they had the same effect on my children and others as a constraint; or rather, an even more insidious effect: that of arousing or maintaining in others an **insecurity** about the value of their own feelings, ways of seeing, opinions - as though

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<sup>41</sup>(\*) Or treated as such. ...

these (in the face of my unwavering assurance, even my pained astonishment) didn't even **need to exist**.

I have a feeling that the development of this propensity in me, particularly in relation to my children, could well be quite complex, intertwining intimately with the vicissitudes of my married life. This is not the place to try and follow its mysteries; nor is it the place to make a more or less inventaire  
There were many other aspects of my person that manifested this imbalance, one of which I tried to identify in the previous note: the "deployment of force".

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We shouldn't think that this imbalance, cultivated over a lifetime, and the multitude of psychological mechanisms through which it manifested itself, disappeared overnight as if by magic. I didn't expect anything like it, not on that day of our reunion, nor in the days and weeks that followed.

(October 10) These were days of melting ice, buoyed by a powerful influx of new energy - days of inner work and wonder, before the new worlds I saw opening up day after day, taking birth in the humble weave of daily minutiae and unfolding under the intense action of eyes eager to see. These were also the days when the first inkling of the richness of this unknown suddenly calling out to me, which I had ignored only the day before, began to dawn on me. I apprehended it through these "bits" that had just made themselves known to me, in the very instant of the reunion, and in the unpredictable and unforeseen journey that had followed it. I felt that this "birth" through which I had just passed was just the **beginning of** something entirely unknown, or rather the **recommencement of** something that had been interrupted, cut off or stifled one day, and then mysteriously started again. To tell the truth, this intense "becoming" had already been in motion again in the preceding months, but at a level where introspective **thinking** had hardly had any part to play. ...

One of the most profound aspects of this new-found becoming, this new-found work, was the gradual restoration of the original balance of "woman" and "man", of yin and yang within me, over the course of days, weeks and years. In a way, I can say that since the moment of my reunion, "childhood" or the state of being a child has remained present, "in power", through a deep and indelible knowledge within me of my own nature, of my essential, indestructible unity, beyond the effects of a certain "division".

which often continues to agitate the surface of my being. The very word "child" or "childhood" to designate the thing, this unity of being, <sup>didn't</sup> appear until years later, around the time I began to become acquainted, at the level of conscious thought, with the dual yin-yang aspect of all things. This was also the moment when the knowledge (or at least, the presentiment) emerged that the state of childhood, the creative state, is that of the perfect balance of yin and yang forces and energies, that of the "marriage" of yin and yang, manifesting itself in a state of creative harmony.

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It seems to me that, at a certain level, this knowledge of my fundamental unity is present at all times, and that it **acts** at all times. It is also true that this action is more or less sensitive and effective according to the moment, and that it is by no means in the nature of a more or less permanent elimination, or even a wholesale destruction of the egotic forces, of the "boss" therefore - nor even of an elimination of the forces of repression (which form a good part of the "I", if not quite its totality. . . ). These are the forces of surreptitious concealment of the reality that surrounds me and the reality that unfolds within me - the forces silently and obstinately at work to maintain against all odds the tenacious illusions, which without them would immediately collapse under their own weight... . Some of these repressive mechanisms have been identified one by one and have disappeared. I got rid of certain **illusions** that had weighed heavily on me, and I cleared up the few stubborn **doubts** that, over a lifetime, had been relegated (by the care of the

"boss") rotting in garbage-undergrounds, never examined. Their message finally heard, these doubts have disappeared, leaving a peaceful and joyful knowledge. I've also spotted some very powerful mechanisms of repression, deeply rooted in the ego, which I've come to realize (over the last few years) have had a considerable impact on my life, today as never before. They go in the direction of yang imbalance, in the direction of the occultation of certain yin forces and faculties. I don't know if these mechanisms will ever be defused - and I know it's up to me. No doubt they will vanish on the day, and only on the day, when I have entered into the origins of the conflict in my life much more deeply and fully than I have done so far.

For the moment, with my life currently focused on a major mathematical investment, I can safely say that it's not going anywhere!

#### 18.2.2.4. (d) Acceptance (the awakening of yin (2))

**Note** 110 □(October 11) I've been wanting for a day or two to take stock, in a few words, of where p .484 (after eight years) this "gradual restoration of the yin-yang balance" in me.

Perhaps the most important change of all is in a much greater **acceptance** than in the past of who I really am from moment to moment. Another way of putting it is that the mechanisms of repression within me have softened considerably. As I said yesterday, some have disappeared after being discovered and understood, and others, which I had ignored all my life, have become familiar to me in their everyday manifestations. I see them in action, not as enemies I must try to extirpate at all costs, but as part of the multiplicity of facets of my conditioned being, and thus of the richness of the present "given", which faithfully reflects my past history; both the "ancient" history of my conditioning and the roots of division in my being, and the more recent history of my maturation, the work by which I end up unpacking, "eating" and assimilating the initial package bequeathed to me by my parents and their successors. This "acceptance" within me therefore includes not only the impulses and traits of the "child" that I had long ignored and repressed (particularly those reflecting the feminine aspects within me), but also the repressive mechanisms specific to the "boss", i.e. the inveterate mechanisms of "non-acceptance"! Accepting the latter has nothing in common with "cultivating" or fortifying them. On the contrary, it's an indispensable first step towards unravelling them or defusing them to some extent, through the effect of curious and loving attention. The experience of the last eight years has convinced me that, if this attention goes deep enough and to the very root of the repression, the latter can be resolved and disappear, releasing a considerable amount of energy - that which until then had been immobilized to maintain against all odds such and such a set of repressive mechanisms, and the habits of thought and other things that serve to maintain them.

But it wasn't in relation to the inherently "knotted" aspects of myself that this new acceptance of myself first appeared in my life. It came without fanfare, even before the discovery of meditation, and therefore even before the "reunion" that closely followed it. It was in July 1976, during a brief love affair with a young woman, G., perhaps a little more "homely" in her ways than the women I'd loved previously. As fate would have it (?), Les

The material circumstances surrounding these loves were such that I saw myself placed in a typically "feminine" role. I did the housework and prepared the□evening meals, waiting for the spouse to return from a p .485 long and tiring working day: tending a herd of one hundred and fifty goats in the hills, which she also had to milk in the evenings. It just so happened that this unusual role of housewife suited me like a glove. It may seem a small thing, but it clicked. I made the connection with some of my friends.

impulses and desires in my love life, expressing themselves for the first time in certain love poems, where the experience of love appears, without any ambiguity, as "feminine". I understood then, without reflection or "effort", without any hint of reticence or embarrassment, that in my body as well as in my desires, in my feelings and in my spirit, I was a woman, as well as a man - and that there was no conflict of any kind between these two profound realities in my being. In those days, the dominant note was feminine - and I accepted this gratefully, in mute astonishment. When I thought about it, there was a quiet, gentle joy in me.

This joy was self-sufficient, it didn't need to be expressed in words, either to myself or to others. I don't know if I spoke about it to the woman whose lover I was, or perhaps whose lover I was... . Surely, on some level she knew, without my having to say so.

This joy has not faded, it has remained alive to this day. It comes from living knowledge, like the fragrance that accompanies a flower. In some moments or periods of my life, this knowledge, and the joy that is a sign of it, is more present than in others, more strongly active. But I don't think it ever leaves me.

When I have spoken of this experience and knowledge here and there, in the weeks and years that followed, it has always been as if I were communicating something of great value to others, in a moment when I felt open to receive, if only for a few moments, something of this joy within me. I've never felt embarrassed to talk about it, as if it were something a little scabrous (perhaps there would have been such embarrassment at times, however, if the reality and strength of the "man" in me hadn't been above suspicion!) And I remember one occasion when I was really showing off, showing off and winning on both counts - all I needed was to get my period like everyone else and give birth to a kid that dry.

My new feminine identity, superimposed on my masculine one, had an immediate renewing effect on my love life. It aroused a very strong echo from the women I subsequently fell in love with, by awakening masculine impulses in the lover, which had been carefully repressed throughout her life, and had hitherto only found expression "on the sly", as a kind of burr, unworthy of appearing in the conscious experience of love.

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The unconscious experience of love is rich in archetypal impulses, one of the most powerful of which is that of returning to the Mother, to the original bosom. Such an archetype is present in the deepest layers of the amorous experience, in both men and women. In women, resistance to the satisfaction of such an impulse in the couple's experience of love is even stronger than in men, where it comes up against a key taboo, and not two as in men. In both cases, the satisfaction of these impulses in the shared experience often remains more or less symbolic and, above all, hidden from awareness. When such an archetype and this experience rise up from the deepest layers to the light of day, in the field of conscious awareness, this experience is immediately transformed, acquiring a new dimension. At the same time, considerable energies previously compressed by repressive mechanisms or bound by repressive tasks are released. The effect is an immediate **liberation of** the erotic impulse, manifesting itself in a renewed intensity and a new fullness in the experience of love.

From the foregoing, it's already clear that this new acceptance of myself has gone hand in hand with an acceptance of others. The two are inextricably linked. Of course, I'm talking here about "acceptance" in the full sense of the word, which in no way means a (often bittersweet) **tolerance** of such and such "foibles" or "faults", felt as a sadly unavoidable evil for which one is obliged to "make do". What I sense in such an attitude is resignation, not to say abdication,



and certainly not a source of joy, nor a surge of awareness of something worth knowing: the presumed, unknown depth behind the flat surface of such "faults" or "shortcomings" that we're willing to tolerate... ..

The fact that this is a joyful, creative acceptance in no way implies that this acceptance is total. An attentive reader will already have noticed this for themselves

more than once in the course of Harvest and Sowing, as I happened to en□rendre in passing,

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(But when it comes to myself, this mechanism more often than not has the effect of not even taking note of the unpleasant thing in question. . . )

The acceptance I'm talking about is rooted in an **interest** in the thing being "accepted", whether in oneself or in others. Whereas acceptance is in itself a typically "yin" inner disposition, this connotation of "interest" that it takes on in me is of a "yang" nature - it's the "yang in the yin", in the delicate Chinese dialectic of the infinite interweaving of yin and yang. ... I was about to venture that there was a pure and simple identity between acceptance (the real thing!) and this interest, this curiosity. And yet, as I reflect a little on the matter, I realize that there's another way of accepting, one that's more totally yin in nature than the one I'm used to. It's like **welcoming** the accepted thing, rather than rushing towards it to probe it. (This nuance of welcome seems to me to be the "**yin** within the yin", here we go!) The impulse of interest, and the attitude of welcome, can both form the keynote of acceptance of others or of oneself. What both have in common is **sympathy**. This, too, is one of the forms of love. If there is any profound identity to be identified here, it would be the observation that **acceptance is a form of love**. Love of self, love of other, both indissolubly linked. ...

Except in rare moments, my interest is more intensely involved in my own person than in that of others. It's this passionate interest in myself that has animated the long periods of meditation over the last eight years. It's true that self-knowledge is at the heart of knowledge of others and of the world, and not the other way round - and I feel that it's towards the heart of things, towards what's most essential, that my new passion, meditation, has carried me and still does. My interest in others has become more fragmented and reluctant over the years, as has the acceptance that comes with it. One of the ways it has manifested itself concretely is in a lesser propensity to talk when I'm in company, and in an attitude of listening. Most of In my life, this ability to listen had been almost entirely lacking. Even after the great turning point of the reunion, I still had to realize quite often □ that I had spoken out of turn, for lack of listening etp . 488 of discernment, before this inveterate propensity began to pass me by. If it has become much less invasive, and has even almost disappeared, it's in no way the result of some self-imposed discipline (such as: you won't open your mouth unless. . . ). It's simply because I've lost the urge to talk, at times when I feel that it's useless, that it doesn't contribute anything to others or to me - at least nothing of any value to me. If I can now often feel such things, it's undoubtedly because I've become more attentive. This too has not come about as a result of discipline ("you'll be careful to open your ears wide when..."), but I can't say how. In any case, I feel better for it, and life is that much more interesting (and, above all, less noisy!). And other people feel better too...

I think I really started to talk less, as soon as this

It's the force within me that drives me to always want to rectify what appear to me (rightly or wrongly) to be "errors" in others - as if it wasn't enough for me to detect and rectify my own! It's the same force that drove me (and sometimes still drives me) to want to convince others of this or that, instead of simply looking at why so-and-so prefers to stubbornly believe this rather than that (which seems like "that" to me, and which I'd like to convince him of!); or why I'm so keen for him to believe that, rather than this. This almost universal force within us, which constantly pushes us to seek in the approval of others (and even just one. . . ) the confirmation of the validity of what we hold to be true - this force deeply rooted in the ego has finally, I believe, let go of me. It was a great relief, the end of a tremendous dispersion of energy. It was when I finally realized, two years ago, the extent of this force in my life, its nature, and the extraordinary dispersal of energy it represented, that it was defused - and I found myself lightened by "a hundred tons of weight". To be aware, without reluctance, of the echoes that others reflect back to us, without being bound by any desire or "need" (however hidden) for approval or confirmation - that's really what it means to be "free of him". It's such a need or desire that really constitutes the discreet, yet unfailingly solid "hook" by which conflict can "hang" in us, and by which we are (whether we like it or recognize it, or not) under the dependence of another, of his good will - by which in short he "holds" us, and (minutely) us maneuvers as it pleases...

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Logically, accepting others should also mean accepting their way of seeing things, whether or not they seem wrong to us, and even when it comes to their way of seeing our own precious selves (including our own ways of seeing. . . ). But that's where the problem lies - it's the crux of our acceptance of others, not the acceptance of more or less embarrassing common "faults" that don't directly involve ourselves. Quite often, moreover, if we reject such "faults" in others, it's above all because they make us feel directly implicated, by the very fact of being confronted with ways of being that seem to us (rightly or wrongly still) to be the opposite of our own. In other words, it's an **insecurity** within us, manifested in reactions (more or less overt or hidden) of vanity, which is the great obstacle, opposing our acceptance of others. But this deep-rooted insecurity, compensated for by the movements of vanity, seems to me to be indissolubly linked to the non-acceptance of ourselves, like an inseparable shadow.

So it's full self-acceptance that appears here as the key that opens us up to acceptance of others. And this link, which has just appeared to me here, links up with another profound link, which I've known for a long time, perhaps for as long as I can remember: that self-love is the heart, peaceful and strong, of love for others.

## 18.2.3. The couple

### 18.2.3.1. (a) The dynamics of things (yin- yang harmony)

**Note** 111 (October 13) Yesterday I didn't continue writing the notes. Instead, I amused myself by reviewing a number of yin-yang "couples". Starting with the ones that popped into my head, a bit luckily, I then got into the game, and ended with a sort of "census" of all the ones I could get my hands on. I'd started because I figured that a lot of what I'd written lately was likely to go entirely "over the head" of a reader who wasn't already at least a little familiar with the double yin-yang aspect of things. Perhaps it wouldn't be out of place to take the trouble to give at least a few striking examples of such couples, in addition to those that had crept in by the tape in recent days. Then, driven by the little devil (or angel, I don't know. . . ) of

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the systematics within me, I ended up digging out my old thoughts on the subject from five years ago. Over the course of a week or two, I had "collected" a hundred or two of these suggestive couples, which had then been assembled by affinity into some twenty groups. As this reflection took place on the bangs of the famous "poetic work" I was writing, I couldn't help arranging these groups as best I could, by affinities and filiations of meaning from one group to the next. Last night, taking a step back, and without a poetic straitjacket around my neck, I came up with eighteen groups (instead of twenty), by grouping them perhaps a little more rigorously. I suspect, moreover, that there must be many more groups, perhaps even an unlimited number, corresponding to modes of apprehending reality that I haven't thought of in the course of the work (nor, perhaps, ever yet).

As for the eighteen groups I have actually identified, I have endeavored to assemble them into a diagram (or "graph") following the main affinities linking them to one another. Some of these links only came to my attention in the course of drawing successive drafts of the diagram. The work here was really very close to the familiar mathematical task of graphically capturing, as strikingly as possible, a more or less complex set of relationships (given, for example, by "applications", represented by arrows) between a number of "sets" or "categories", appearing as the "vertices" of the "diagram" we were endeavouring to construct. Here too, essentially aesthetic requirements, notably symmetry and structural transparency, frequently lead us to introduce (and if necessary, discover or even invent) "arrows" or links that we hadn't thought of at the outset, and sometimes even new "vertices". In any case, after five or six successive drafts, I ended up with a vaguely Christmas-tree-shaped diagram that satisfied me for the time being - especially as it was getting prohibitively late!

I went to bed happy, feeling that I hadn't wasted my time, even if my notes hadn't progressed a hair<sup>42</sup> (\*). But I was back in contact with some decidedly juicy

f - every one of thesep

each of these couples has something delicate and important to tell me about the nature of this world in which I live, and often about my own nature. I've rediscovered with renewed strength a feeling that was already present five years ago: that the delicate interplay of yin and yang, of the "feminine" and the "male" in all things, is an incomparable thread leading to an understanding of the world and of oneself. It leads us straight to the essential questions. Often, too, the very "yoga" of yin and yang - I mean, the very act of paying attention to the aspect of things and events that is expressed in terms of yin-yang balance and imbalance - provides a first key to a better understanding of these questions, and to an answer.

I apologize if, for the last page or two, I have given some readers the impression that I'm talking about the sex of angels, when they wouldn't even really see what these famous yin-yang "couples" I'm talking about are, let alone these "groups" into which some of them come together, which groups are finally supposed to assemble into a "diagram" (maths is useful, after all!). I should give at least one of these groups here - and I feel like taking the one I spontaneously started with yesterday, the one that ended up appearing in the course of reflection as the "primitive" group(\*), from which all the others seem to gradually emerge, through some sort of successive "filiations" (continuing on my famous diagram of eight "generations". . . ). Here's a list of the "couples" I've identified that make up this primitive group (which we could call by the first of these couples, "the **action - inaction** group").

<sup>42</sup>(\*) In compensation, I could apply for a patent on the invention of a new poetic form, namely the "non-linear" or "diagrammatic" poem.

## 2. A walk through a work or the Child and Mother

- Action-inaction
- activity-passivity
- sleep-wake
- subject-object
- generate-design<sup>43</sup> (\*)
- execution-design<sup>43</sup> (\*)
- dynamism-balance
- élan-assise
- perseverance
- enthusiasm-patience
- passion-serenity
- tenacity-detachment.

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I'd like to add the following two couples, among a dozen or so "latecomers" who came to me again this morning, following on from my thoughts yesterday:

- know-how
- explain-comprehend.

Needless to say, in these couples, it's the term "yang" or "masculine" that is put first, following the usage of our patriarchal society, where the man gives the name to the couple? On the other hand, while traditional Chinese society is considerably more patriarchal than our own, when we follow Chinese usage to speak of the relationship between yin and yang, we always put yin ("feminine") first, for example when speaking of "yin-yang balance" (instead of yang-yin). The meaning of this usage surely lies in the archetypal intuition that it is yang that is born of yin, which is the "most primitive" principle of the two, and not the other way round... ..

This is not the place to comment on any of these couples. For the reader who doesn't "feel" anything when he sees them, it would in any case be wasted effort; but for the reader who feels challenged by them, who senses (albeit obscurely) that each of them has something to say to him about the world and about himself - about balance and imbalance, about the internal dynamics of beings and things... . can dispense with detailed commentary, and take this challenge as a starting point for his or her own reflection.

### 18.2.3.2. (b) Enemy spouses (yang buries yin (3))

**Note** 111 There's just one point I'd like to stress here, common to all yin-yang "couples" without exception. It is also the most crucial thing of all, it seems to me, for an understanding of the nature of the relationship between yin and yang, and hence of the nature of each of these two principles (or energies, or aspects, or forces. . . ) in the Universe. It's this: each of the two terms of one of these couples, such action-inaction, **in the absence<sup>44</sup> (\*) of the other term**, constitutes a state of serious imbalance, and at the limit

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<sup>43</sup>(\*) (November 6) In fact, there's an even more primitive group, which can be called the "**father-mother**" group. On this "omission", see the note "Our Mother Death - or the Act and the Taboo" (n° 113). The "engendering-conceiving" and "execution-conceiving" pairs, which I've included below in the (so-called "primitive") action-inaction group, fit in visibly more naturally in the "mother group" formed around the "father-mother" pair.

<sup>44</sup>(\*) (October 16) In fact, this "absence" is never total, it seems to me - in no thing is yin or yang present in a pure state, without the simultaneous presence of its complementary, however infinitesimal. The "imbalance" I'm talking about is therefore characterized, not by the total absence of one of the two complementary terms (something never achieved), but by a state of excessive **weakness** of that term. Another type of imbalance, or morbidity, occurs when **both** terms are "absent", or more precisely, present but very weakly. Thus, in the case of the "action-inaction" couple, a state of **agitation**, which doesn't "act" as such (except to perpetuate itself, to maintain confusion), while dispersing energy,

(when the "absence" in question is almost complete, and prolonged) a state that leads to the destruction of the thing (or being) in which this imbalance takes place, and even of himself and those around him.

Thus, a state of uninterrupted **action**, which does not alternate with sufficient periods of **inaction**, of rest, leads to exhaustion, illness and (ultimately) death - something which has been most topical lately, for me!<sup>45</sup> (\*\*) But conversely, a state of excessive inaction leads to a weakening and sclerosis of the body's or psyche's capacities and functions (depending on the case), and ultimately, to destruction. In the case of my "incident-disease", moreover, I have a simultaneous example of **both** imbalances: excessive action of the mind, inaction of the body (and sufficient rest for neither. . . ).

This "explanation", in this case of the "philosophy" of yin and yang imbalance, remains superficial, in the sense that it does not touch upon an inveterate cultural bias, valuing the term yang, action, in **opposition** to the term yin, inaction. Inaction is seen as a "negative" thing, not productive or interesting from any point of view, accepted at best as a stop-gap measure, which, alas, is imposed even on the best will in the world, since it is necessary to rest from time to time in order to continue to invest oneself in action (on pain, as I have just explained, of overwork and God knows what else. . . ). In short, inaction is seen as the humble handmaiden of action - indispensable, alas, but otherwise unworthy of attention or esteem.

□ Bien entendu, une telle valorisation "officielle" de l'action au détriment de l'inaction, a a immédiatement comme p. 494

The consequence is to set in motion resistance mechanisms (which often remain hidden or at least very blurred), expressing themselves through an **opposite** valorisation: action, as a result, appears as something imposed by the hard necessities of life, like **work** in short, boring as can be, in the office or the factory or even in the fields, and exhausting in any case, even if it's not too boring. The real raison d'être of action is to earn a crust and make a living (that's the essential), and beyond that and above all, to have fun (during your working life), and a nice retirement and pleasant permanent leisure activities later on, when you're freed from the regrettable obligation of "work". This time, it's inaction (aka "leisure") that's more or less consciously valued, and it's action that's its humble servant. There is thus **a reversal of roles**, but always with the same imbalance: that which consists in **the antagonism** established by the interested party (under the pressure of cultural conditioning) between two essential aspects or poles of his life; an antagonism which is expressed and perpetuated by a state of despotic preponderance of one of its aspects, and servitude of the other.

It seems to me that, more often than not, the two attitudes and values overlap in the same person, one dominating the pavement at the conscious level, the other at the unconscious level. The superimposition of these two opposing imbalances obviously doesn't produce balance! Balance, on the other hand, flows naturally from an understanding of the true nature of action and inaction (even when such an understanding remains purely "instinctive", manifesting itself directly in balanced behavior, and in no way in verbalized "knowledge"). **In action in the full sense of the word, there is also inaction - in the very moment**, I mean, and not just "after", because you have to rest after action! This "inaction" within the "action", the "yin within the yang", is like a deep calm that serves as a foundation for a movement that would take place on the surface. It is manifested, for example, by the impression of perfect relaxation that emanates from a feline in motion, whether it's the first alley cat to come along, or a lioness with a powerful build... .

And **in true inaction**, even total **inaction**, **there is action**. So **sleep** is rich in its dreams that speak to us about ourselves, through which we live <sup>another</sup> □ more intense and delicate **life**, p. 495

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can undoubtedly be seen as such a "default" imbalance (of yin and yang).

<sup>45</sup>(\*\*) On this subject, see the first two notes (n° 98, 99) of Cortège XI, "Le défunt (toujours pas décédé. . . )".

that we're often too sleepy or too pusillanimous to live in the waking life. And it's enough to contemplate a sleeping baby, or just to be roused from a deep sleep, to feel that even without dreams, truly good sleep is **work** in its own way: something that absorbs us completely, "replenishing" in short, an energy that has been dispersed and that we come to **replenish at** its source... . Once again, this is the "yang in the wine", without which yin itself would be destructive.

Similar considerations could surely be developed for **waking** inaction, outside sleep time. All we have to do is observe carefully, on the spot, any state we perceive as "inaction". You'll realize that in inaction, there is action, even the sterile cackling of a thought that continues to go round in circles, even though it has stopped working. But in truth, it's a misnomer to call this purely mechanical movement "action", a movement that continues purely because of inertia - because of the inability to stop the machine! And it's certainly not this inner agitation that will bring yin-yang harmony to "inaction", making it beneficial. On the other hand, this can be the case with various activities designed to fill leisure time (when these are nonetheless experienced as a state of inaction). But even in the state of complete rest of a convalescent state, let's say, there can be action, otherwise this rest or "inaction" becomes **sluggishness**, certainly not conducive to convalescence (i.e. the restoration of a disturbed equilibrium!). For example, this state of rest can give rise to attention to one's own body and its immediate surroundings (which are like a second skin. . . ), an awareness or even a communion, which in itself has an authentic character of "action"; for there can be no doubt that **learning** is indeed an **act** (since it has an irrefutable **effect**: the appearance of knowledge. . . ).

Looking at the fourteen couples I've included in the action-inaction group (and I'm sure we could find many more that fit in naturally), we can see that for all but perhaps one, it's the first term, "masculine", that is invested with prestige, with "value", according to the attitudes-reflexes.

and inculcated since childhood. It's the sign of the same inveterate imbalance in our culture'□the imbalance marked by the exclusive valorization of yang, to which I happened

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<sup>46</sup> (\*). The same can be said for almost all the yin-yang pairs I've come across - a very striking fact indeed, which I'd never before taken the time to verify in such detail.

Among the pairs written earlier, the only one that seems to me to be an exception is the **passion-serenity** pair, given that in common usage, the word "passion" is often associated with the image of unleashing, violence, or if not **laissez-aller**, annoyingly bordering on the cloud of associations surrounding a word like "**turpitude**". As if by chance, sloppiness and turpitude refer to states of psychic imbalance characterized by an excessive **yin**, feminine preponderance! And symmetrically, following the same push-button mechanisms (which reveal our current conditioning, and by no means the nature of something like "serenity"), the word "serenity" is associated (as opposed to "passion") with the image of **self-control** - a quality, therefore, that is appropriately **masculine** in essence. (In fact, the yin counterpart of "control" is by no means "passion", but "abandonment").

What's happening here, then, is that as a result of a general confusion in people's minds about the nature of certain things, expressed by an equal confusion in the use of certain words, supposed to designate them, there is a confusion of the yang-yin couple "passion-serenity" with the whole of the two notions

release - control,

whose terms are yin-yang (but not a "couple", as the two terms have no desire to marry!). It seems to me, then, that the so-called "exception" to the rule (of valuing

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<sup>46</sup>(\*) See note "Yang buries yin (1) - or muscle and guts", n° 106.

yang) is, on the contrary, a particularly interesting confirmation! And I wouldn't be surprised if the same were true of the other few examples I've come across, where in a yang-yin pairing, it's the yin term that seems to be valued.

In fact, I'm not at all sure that this distortion in worldview that I see in the civilisation, stemming from this systematic bias in favour of the masculine as opposed to the feminine.

- that this distortion, this imbalance are so much less in the Chinese tradition, □ or even in the p . 497

Chinese world (or, more generally, the "Oriental" world) today. There's no sign of this in everyday life, either through my Oriental friends or through the echoes I've heard of tradition and life in China or other Far Eastern countries - quite the contrary. Rather, it seems to me that a fine perception of yin-yang dynamism has been confined almost exclusively to the **practice of certain arts** - such as calligraphy, poetry, the culinary arts and, of course, the medical arts<sup>47</sup> (\*).

It is the latter in particular, under the name of "Chinese medicine" and thanks to the spectacular successes of acupuncture, that over the past twenty years has come to be regarded as a prestigious discipline. Yet many people are still unaware that, in Chinese medicine, the alpha and omega of understanding the body, the circulation of energy in the body and its disturbances (which constitute the morbid states we call "illnesses"), lies precisely in a very fine dialectic of yin and yang. The fact that this dialectic "works", since "Chinese medicine" based on it is effective (including in many cases that escape the means of the Western panoply), can be seen as a kind of "proof" of the reality of yin and yang "principles" or "aspects" or "modes" (of apprehension, or existence) - that they are not pure speculations out of the hats of certain philosophers and other poets (not to say fumists).

We may well ask what is the meaning of such proof, and indeed of any "proof" whatsoever of the validity of such and such a worldview. Even supposing that the proof was convincing (i.e., that the interested party was willing to allow himself to be convinced), and even and above all, that the vision in question was valid. is profound and therefore beneficial - the best proof in the world is powerless to **communicate**

□ **une vision**, let alone a vision of the world. It does you good to be "convinced" mordicus p . 498

of a vision that remains foreign, misunderstood. In fact, it doesn't even make sense - or more precisely, the true meaning of his "conviction" is no more understood by the interested party than the vision he pretends to incorporate into his heavy cultural baggage.

When the vision is understood and assimilated, the very question of "proof" seems strangely preposterous. - a bit like proving that the sky is blue when you can see it's blue, or that the scent of a flower you love is good... .

### 18.2.3.3. (c) The half and the whole - or the crack

**Note 112** (October 17) My first thoughts on the dual aspect of "feminine" and "masculine" came from a reflection on myself. It was around the beginning of 1979, at a time when I still didn't know the Chinese words "masculine" and "feminine".

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<sup>47</sup>(\*) (October 21) I've left out the **divinatory art** of the **I Ching** or "Book of Changes", which today enjoys great popularity in certain circles in both Europe and America. The 64 "hexagrams" that constitute the basic "words" of the divinatory language of the I Ching are none other than the 2, <sup>6</sup> possible combinations of sequences of six yin and yang "signs", from pure yin (six repetitions of yin) to pure yang (six repetitions of yang). There seems to be a kind of fine alchemy of yin and yang combinations, which (it seems) had fascinated Jung. The interest of this alchemy (as a "collection of archetypes" in particular) seems to me a priori independent of its use in divinatory art, and of the credit one is willing to give to such use.

"yin" and "yang", and the existence of a kind of subtle "philosophy" of the incessant interplay of yin and yang, in the Chinese cultural tradition. I learned about this towards the end of the same year, I believe, from my daughter and especially from my son-in-law Ahmed, who was just beginning to take an interest in Chinese medicine, which he latched onto strongly in the years that followed. Most of what he told me overlapped and confirmed the vision I'd arrived at, which came as no surprise. If there was any surprise, it was rather in the few cases of "couples" where the "natural" yin-yang role seemed to me to be reversed, in the Chinese tradition. My reflex (strongly "yang" in this case!) had been a skin-deep conviction that this "reversal" must be due to a cultural distortion, without actually looking too closely<sup>48</sup> (\*) - it was at one point where my past games on the feminine-masculine seemed far away, while I was engaged in an otherwise more personal meditation on my parents' lives and my childhood. It was months

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or years later, I think, that, through a certain amount of cross-checking, I realized that in some cases my apprehension of the yin and yang roles in such and such "couples" had remained a tad superficial; that I had hastily lumped together situations of a different nature that Chinese yin-yang dialectics were careful to distinguish (112'). Now, I realize that my apprehension of yin and yang is still relatively crude and static, especially when compared to the finesse required for the practice of certain traditional Chinese arts such as medicine (also closely linked to dietetics and the culinary arts), where this apprehension ends up becoming like second nature.

More than once, I've had the impression that among practitioners of these arts, whether Eastern or European, this finesse of apprehension remains fragmentary, in the sense that it remains, to a very large extent, carefully confined to the practice of the art. In everyday life, it acts more like ordinary "knowledge", simply superimposed on the "knowledge" of cultural (and other) conditioning, and remaining more or less a dead letter vis-à-vis the latter. To put it another way, I've had the impression that the vision of the world and of oneself, and the mechanisms of repression in the perception of reality, are in no way different in these "well-informed" people than in ordinary mortals.

This impression overlaps with another, which I had while perusing a couple of texts, written by Europeans supposedly "in the know", who: rest to give an overview of the traditional Chinese philosophy of yin and yang. (One of the authors is a well-known French orientalist, whose name now escapes me). The thing that struck me was that in these texts, yin and yang are presented as "**opposing**" (or "**contrary**") or even **antagonistic** principles (the latter term recurs several times in one of these texts), rather than **complementary**. This "opposition" or "antagonism" would have its typical expression in that which would take place between man and woman within human society, and within the couple instituted by society.

Antagonism between husband and wife is a reality in both East and West. It is deeply rooted in culture, so much so that it can sometimes appear as one of the (sometimes confusing!) aspects of the human condition, or even as the root of conflict in man or in human society. The reality of this antagonism is incontrovertible, and it certainly goes beyond the common clichés that try to exorcise it 500% as best they can. This "social" reality is the product of conditioning.

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<sup>48</sup>(\*) This reaction of peremptory assurance towards a thousand-year-old tradition, which could have encouraged me to be more cautious, is the same one that, as a child, led me to reject the formula (quite complicated, my goodness!)  $\pi = 3, 14 \dots$  taught by books, in favor of  $\pi = 3$ , which I had convinced myself by my own means. (See the note "Squaring the circle", n° 69.) It's true that with this story of yin and yang, I'd had ample opportunity to realize the extent to which the apprehension of the nature of the "feminine" and the "masculine", and of their interrelationships, is distorted by inveterate cultural distortions of considerable force. What I didn't realize, however, was the extent to which a precise and delicate apprehension of these relationships was also essential in the practice of certain traditional Chinese arts, and pushed to a degree of great finesse.



It's an immemorial reality, which very early on takes root in and structures the developing "I". And yet, beyond this reality, there is a deeper reality, coming from much further back, which is decisive in the love drive itself. It's the reality of a profound, essential **complementarity** between the sexes, where there's no room for any kind of "antagonism". It's also the reality that is clearly evident in all living species, with the sole exception of our own, where it is largely obscured by cultural antagonism, and thus by a state of **division** specific to man and human society.

The common romantic clichés, such as "Nous Deux", which dominate much of literature and the media, make a mockery of "complementarity", while casting a modest veil over the troubling male-female antagonistic aspect, or (at best) treating it as a kind of spicy accident, welcome to spice up a meal that's otherwise a little too bland or syrupy. As soon as you get beyond these reassuring clichés, you're immediately confronted with the reality of this male-female antagonism - a reality that's apparently universal, and, what's more, tough as nails, tough as weeds! But to start from this omnipresent and irrefutable reality, to institute a kind of cosmic antagonism of yin and yang, of "feminine" and "masculine", is to project onto the entire Universe the state of tearing apart, of profound division of human society and the individual, a disease therefore peculiar to our species. It's also perpetuating our own ignorance of **another** reality within ourselves (joining this cosmic reality of complementary harmony), a reality just as tenacious (or, to put it better, indestructible), but more hidden. This reality runs counter to the conditioning that tacitly establishes a de facto antagonism between woman and man, wife and husband, as well as between that in ourselves which is "woman" and that which is "man".

To tell the truth, this **dualistic** or **warlike** vision of the Universe, where one aspect of things is at constant war with an equally essential "symmetrical" aspect - this vision is in no way the fruit of **reflection**, which would "start" (as I just wrote) from the reality of conflict in the human couple and in human society, and then "deduce" (or "institute", as I wrote more aptly) it in the entire Cosmos. It's no more, no less, than the faithful, automatic expression of cultural conditioning, and it's in line with one of the 501 essential functions of this conditioning: **the maintenance of conflict, of division in the very person**, visibly, the maintenance of this instituted antagonism between "woman" and "man" in me would be impossible, or rather, this antagonism would already be resolved, **as soon as** I took the time to contemplate the Universe with the eyes I received at birth, and where I note that everywhere, except (apparently . . .) in myself and among my fellow human beings, the "feminine" and the "masculine" are your indissoluble complements; that it is from their marriage and union that harmony, creative force and living beauty are born in all living and "dead" things of Creation. On the other hand, if I claim to "see" "oppositions" and "antagonisms" everywhere in the Universe where they don't exist (and even though in doing so I'd be following a venerable tradition that goes back thousands of years), I wouldn't be using my eyes at all, but rather confining myself to **repeating** (like everyone else) what has been repeated from generation to generation since perhaps the dawn of time ; and, in any case, to obey the silent, imperative injunction of cultural consensus - the very injunction that has firmly established within me a division, a conflict that I would claim to rationalize (and thereby perpetuate) as a "cosmic necessity".

There's certainly a lot to be said about antagonism in couples, and more generally about female-male antagonism - and I trust my peers that much has been written on the subject, including some relevant stuff. This is not the place to dwell on this most interesting of themes, particularly on the particular form this antagonism takes in our patriarchal society. It seems to me that among those who have

Clearly, many hold the structure of society, reflecting and embodying the preponderance of men over women, to be responsible for this antagonism. They're probably right - and I suspect that in a society with a pronounced matriarchal tendency, we must find a similar antagonism, manifesting itself more or less symmetrically. What I would like to add, however, is that this causality appears to me to be **indirect**, that it seems to be exercised through the intermediary of a more hidden causality, touched upon in today's reflection. This more hidden and more essential cause of division in the couple is the state of division **within the person**, both woman and man, with regard to his or her own impulses (and in particular those of sex) and faculties. I see this as the real root of the antagonism between man and woman, as well as their **mutual spiritual dependence**,

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□ I mean the **lack of inner autonomy** of both.

This division within ourselves consists in the intimate and secret conviction, in both of us, that we are only **half**. One of the signs of this conviction is this diffuse and insidious feeling, never examined, of **cracking**, of **mutilation** perhaps, from which only the partner of the other sex could deliver us, temporarily at least. Behind the circumstantial airs of "macho" or "Circe" (and many others), everyone, men and women alike, find themselves in the position of a **beggar** vis-à-vis their potential or actual partner, of one who expects an ephemeral deliverance from the other's (more or less) goodwill, which he hopes will be complete, but which always turns out to be lame, from his pitiful state as a cracked, not to say broken, pot - **half a pot**, in short, looking for another half to glue him back together as best he can (and rather badly than well, as you might guess). . . ).

This feeling of fracture, or rather, this **ignorance** of our true nature, of our fundamental **unity** beyond the physiological specificity linked to our sex - this deep division within us seems to me to be the product of social conditioning alone. There's no trace of it, at least in the first days and months of infancy. This conditioning is by no means reduced to valuing the "masculine" to the detriment of the "feminine", or vice versa. After all, if I feel, accept and am accepted as both "male" **and** "female", with a "background note" that can vary from one facet of my person to another, and which is by no means limited to the dominant (albeit very important) genitalia - then it doesn't really matter whether it's the "masculine" or the "feminine" that is valued around me. At the level of my sexual drive, my personal "valorization" would in any case tend to gravitate towards the sex opposite to my own (sorry, complementary I meant), without feeling inferior (any more than superior) in front of this being **different** in body, towards whom I'm drawn by a deep and imperious drive. Moreover, whether it's a question of gender or any other kind of value, the importance of the "value" or prestige attributed by social consensus (to oneself or to others) is relatively secondary, not to say minimal, in a person who is not (or not very much) affected by this feeling of "fissure" I'm talking about - in a person, therefore, in whom lives this spontaneous **self-confidence** that is not, in fact, a "flaw".

It's a manifestation of an intact knowledge of one's own nature.

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□ A sign, among others, that the "crack" or division<sup>49</sup> (\*) in the person is not only The product of a In other words, it's the one who's supposed to be the "beneficiary" of this consensus that claims to "enhance" him, while (in a certain sense) it's breaking the backs of both him and his partner. We can see that this division is all the more acute, all the more violent, the stronger and more ruthless the repression of one sex for the "benefit" of the other. It could be said that the principle followed by "Society" (the source and instrument of repression) in establishing

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<sup>49</sup>(\*) I'll refrain here from using the rather fashionable expression "castration", a term of great violence (superyang for that matter!), which has the added disadvantage of suggesting the image of an irremediable, irreversible mutilation, and thereby stimulating reactions of dismay, revolt or resignation likely to reinforce a state of blockage, rather than encouraging its evolution in the direction of a gradual resolution.

of repressive mechanisms is: "**divide and conquer**"! But this "division", created by the Consensus to break and enslave both men and women, is played out on **two levels at once**. The most visible picture is that of **division within the couple**, achieved<sup>50</sup> (\*\*) by instituting a more or less tyrannical preponderance of one of the sexes over the other - of man over woman, or vice versa. One is supposed to reign over the other - and both end up as slaves<sup>51</sup> (\*\*\*). Because when the wife or husband is scorned, it's **both** of them who are scorned - sometimes by others, but more profoundly and above all, **by themselves**.

And here we come to the second, more hidden aspect of the game of division. This is the **division within the person himself**, the hidden springboard of the couple's division. It is accentuated by the latter, without however being reduced to it, and it is by no means produced solely by the valorization of one sex to the detriment of the other. Rather, it's the product of a silent, incessant **constraint**, exerted on us by those around us from our earliest years.

young years. This constraint pushes us to deny, on pain of rejection, an entire "side" of our identity.

our person (the □ "yin" side, or the "yang" side<sup>52</sup> (\*)), dismissed as ridiculous or unseemly, and in p. 504 in any case, as unacceptable.

#### 18.2.3.4. (d) Archetype knowledge and conditioning

**Note 112** <sup>53</sup> (\*\*) Thus, in the **matrix-embryo** and **vagina-penis** pairs, the distribution of yin-yang roles is unmistakable, and the yin term surrounds and contains the yang term. This had led me to hastily conclude that in the **container-content** pair it was the "content" that was yang, without being put on guard by the **form-ground, exterior-interior, periphery-center** pairs (where, as I had clearly sensed, the first term is indeed yang, as well as being the "container"). In fact, in the matrix-embryo and vagina-penis pairs, I had wrongly emphasized the "geometrical" or configurational aspect of the relationship between the two terms, a secondary aspect to the main one, which in this case determines the distribution of roles: **that which nourishes** is yin in relation to **that which is nourished**, which is yang, and **that which penetrates** is yang in relation to **that which is penetrated**, which is yin (as is **that which gives** in relation to **that which receives**).

My reflections on yin and yang, however limited they may be, have founded an intimate conviction in me that, beyond differences in individual apprehension about yin-yang role distributions (or, let's say, about the yin or yang "background note" in a given person), an apprehension highly subject to "cultural distortion", such a "natural" distribution (or "background note") does indeed exist. It is as irrefutable, "cosmic" and immutable (as far as the distribution of roles in universal-nature couples such as those discussed so far are concerned), as a physical law, or a "natural" relationship.

mathematical, even if it cannot be "established" either by experience (in the sense in which this term is understood in the practice of the natural sciences), or by a "proof" or even a "demonstration". This reality □ au ying et du p. 505 yang is apprehended by direct perception, which can be developed and refined (among other things) by sufficiently deep reflection.

It seems to me that one of the main effects of this kind of thinking is to take us beyond

<sup>50</sup>(\*\*) (October 21) At least on the surface. But as suggested above, if we go deeper into things, we realize that this division in the couple, maintained by the preponderance of the man over the woman, has a deeper "root", which I'll come back to a few lines later.

<sup>51</sup>(\*\*\*) Slaves, moreover, who for nothing in the world would part with Their chains, which are dearer to them than life. . .

<sup>52</sup>(\*) In principle, and barring accidents, the sense of constraint pushes men to deny their yin side, and women to deny their yang side. The situation is more delicate for the woman, who is supposed to deny the very traits in her that are given prestige by social consensus, and which she would therefore feel encouraged to cultivate. She thus finds herself subject to two opposing pressures, and the task of structuring an "operational" identity for the unconscious is further complicated.

<sup>53</sup>(\*\*) This footnote is taken from a footnote to the previous footnote (see reference in the first paragraph of the previous footnote).

cliché reflexes, programmed into us by the surrounding culture, to regain contact with reality itself. This, it seems to me, is already present in deep layers of the psyche, as a kind of archetypal knowledge, beyond the reach of cultural conditioning. The role of reflection is to enable us to regain contact with this knowledge already present, and to carefully decant it from superficial "knowledge", i.e. from cultural conditioning.

The work I've begun in this direction has been important for my understanding of the world and of myself, and hence for my daily "doing" and the conduct of my life. This work (as on many other occasions) seems to me like a **first breakthrough**, like a door I've just pushed open onto a vast panorama, which I still have to explore. I've got everything I need to do it - but I don't know if I ever will<sup>54</sup> (\*). Mathematics aside, there's no shortage of equally "juicy", even more personal and even more burning topics for reflection, which will undoubtedly take precedence over the deepening of a more general reflection on yin and yang. ...

## 18.2.4. Notre Mère la Death

### 18.2.4.1. (a) The Act

**Note** 113 (October 21) Three days have passed without writing any notes. My days have been absorbed by other tasks and events. One of these was the visit of Pierre, accompanied by his granddaughter Nathalie, who arrived last night. He plans to stay until tomorrow evening, and by then to read what's been written about the Funeral. It's likely to be a bit short, for a text that took me almost three months to write...

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The time I was able to devote to reflection, I spent continuing □ to play with "couples". yin-yang and the groups they form. It's a fascinating subject, combining the very special flavor of the investigation of a mathematical "structure", whose very nature gradually becomes clearer as the work progresses, with that of a reflection on the world and on existence. Each of the main yin-yang pairs represents a kind of "**keyhole**" (among an infinite number of others), revealing a certain aspect of the world, or of a corner of it. The "groups" of couples I've identified so far seem to correspond rather to different possible ways of apprehending things in the Universe, like so many **doors** opening onto it and showing it to us from so many different angles. Each of these "doors" has a large number of keyholes, perhaps even an unlimited number, through which to look - until perhaps we simply push the door open? For the time being, I've confined myself to detecting a good number of these holes (I've found well over two hundred), and sticking my eye to each one, if only for the space of a few moments, while realizing each time that there would be something to look at for a good while without wasting my time - quite the opposite, in fact! But I'm even more impatient to go and have a look at such and such a hole through which to look again, and also to go round all these doors and orientate myself as best I can how they are arranged in relation to each other, and perhaps also according to which "patterns" are arranged in one or other of these holes that made me detect their existence... .

Finally, the eighteen "doors" I'd detected a little over a week ago were augmented by three more, making a total of twenty-one, laid out in a diagram (which I'd described as "vaguely Christmas-tree-shaped"), now comprising a "trunk" of nine "vertices" (or "doors", or "gates").

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<sup>54</sup>(\*) Just as I don't know if the kind of work I see opening up before me has ever been done before (the study, in short, of a kind of local and global "map" of the qualities of things in the Universe and their modes of apprehension, in the light of the harmony of complementary yin-yang). It's not a question of presenting a doctoral thesis on this or that, but of deepening an understanding of the world and of oneself, which can only be the fruit of personal work.

"groups", or "angles"), connected by vertical "edges" or "links", with on each side of the trunk six other vertices connected to it and to each other, so as to form the "branches"(\*).<sup>55</sup> □ Somewhat comical, among thep . 507

three "new" groups that have appeared in recent days, one is the most obvious, the most primordial or primitive of all: it's the one that corresponds to the very first intuition of yin and yang as the "feminine" or "female", and the "masculine" or "male". It seems to me most strikingly expressed by the "**father-mother**" archetypal couple (in preference to "man-woman", which is part of the same group). This group is highly charged with sexual connotations, appearing in pairs such as "**beget-conceive**" or "**penis-vagina**", themselves part of the cloud of associations around **the act** par excellence, the Act-archetype: the creative embrace that transforms (potentially at least) the woman into a mother and the man into a father through the appearance of **the child**, the Work resulting from the Act.

These connotations of the love drive were constantly at the forefront of my thinking five years ago. What's more, they were given almost uninterrupted lyrical emphasis throughout the 130-odd pages of the famous "poetic work" into which the reflection had then been condensed, producing a wearying effect on even the best-disposed reader. It's surely a reaction of annoyance towards this double "deliberate intention", poetic and erotic<sup>56</sup> (\*) in my only reference text for my reflections over the last few days, that I simply "forgot", among the famous groups of yin-yang couples, the one who of course opened the procession (and rightly so) in this text of woe.

The title of the work in question, "Eloge de l' Inceste", was a tad□provocative too, and of a nature to give a false idea of his intentions and his "message". In fact, these evolved quite considerably as I wrote - the poetic straitjacket didn't prevent the work from going deeper and decanting. My first and main aim was to probe a certain aspect (which I felt to be profound and essential) of the love drive, as I knew it from my own experience. It was primarily a question of the erotic drive in men, or more precisely: the "**yang**" drive, which corresponds to the "male role" in the game and in the act of love, but which is present with varying degrees of strength<sup>57</sup> (\*) in women and men alike. For a long time, perhaps forever, I've known that this drive, by its very nature, is "**incestuous**": it's also the drive to "**return to Mother**", to return to the original womb.

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<sup>55</sup>(\*) (October 24) I'd be at a loss to predict whether or not yin-yang couples will emerge that don't fit naturally into any of the groups I've identified so far, i.e. whether there are **other** yin-yang groups or "doors" opening onto the world, or even an unlimited number of them?

The fact that I couldn't find another would in no way mean that there couldn't be an infinity of others, perhaps even an infinity of others that escape human experience, our means of perceiving the Universe. This reminds me that more than once in recent years, I've been struck by the intuition that, from the ant or the tiny aphid, to the mammals already close to us, every animal species has means of perceiving and apprehending the Universe that escape all other species, including our own ; so that, in terms of the wealth of sensory apprehension (let's say) of what surrounds us, our species does not "cover" or "contain" any other, any more than any other contains us.

The "no more than" I've just hazarded seems hasty, even overcautious, given that in terms of the richness and fineness of purely sensory perception, the evolution of our species would tend to go in the opposite direction, **to regress**. It seems to me that it's only at the level of the intellect, of the fineness of mental images, and in particular those linked to language, that we excel over other species. It's no coincidence that most of the yin-yang pairs that spontaneously came to my attention belong to this register, specifically "human", while only a handful have (among others) an obvious sensory connotation, such as shadow-light, cold-hot, low-high, and a few others.

<sup>56</sup>(\*) (October 24) This deliberate statement in form reflected an inner attitude, the choice of a certain role - the role of **apostle** of a message. On this subject, see the end of the section "The Guru-not-Guru - or The Three-Legged Horse" (n° 45), and the related note n° 43.

<sup>57</sup>(\*) (October 24) This presence is often more or less totally suppressed by powerful repression mechanisms. I have the impression that in men, this yang drive tends to predominate over the complementary y in drive, and that the opposite is true in women. But cultural conditioning, and the various ways in which it is internalized, both "positive" and "negative", interfere so drastically (and often complexly) with the play of original drives, that it's sometimes difficult to detect them behind sporadic, furtive and often degraded manifestations.

This great return is "staged" and relived in the course of the amorous game, culminating in an **annihilation**, an **extinction** of being, a **death**. To experience the fullness of the act of love is also to experience **one's own death**, like a "birth in reverse" that returns us to our mother's bosom.<sup>58</sup> (\*\*)

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But it also means transgressing **two** extremely powerful **taboos** at once: the **incest** taboo, which excludes "the Mother" as an object of amorous desire, and the one too that (in our culture at least) separates and opposes, like irreconcilable enemies, **life** and **la mort**, **being born** and **dying**. Yet I already knew, that the act of love is **both** a **death**, accomplished in the orgasmic spasm, and a **birth**, a renewal of being, **emerging** from this death. ... like a new shoot delicately springs up from the nourishing earth, itself formed from the creative decomposition of beings that have been damaged in it. ...

It was during this reflection on the meaning of the act of love, five years ago, that I finally understood that "death" and "life" were the wife and husband of the same tightly entwined couple<sup>59</sup> (\*), that eternal life was born of death, to be eternally abysed in it. Or, to put it better, that life eternally abysed in Death, to be eternally reborn from Her, the Mother, fertile and nourishing - She herself nourished and renewed ceaselessly by the eternal return to Her of the countless bodies of Her children.

And the human couple of wife and husband, lover and lover, when they live to the full the impulse that draws one into the other, is like a **parable** of these endless espousals of life and death: at the end of each night of love, the lover abysed and dies in the lover, to be reborn with her from this death in their common embrace... .

At the beginning of this same reflection, I visualized an essential aspect of division within the person, as a kind of "cut", a "**horizontal**" cut: that established by the taboo of incest, which "cuts" the child from the mother, just as it cuts life from its mother Death, and just as it also cuts a generation from the one that precedes it.

If I saw this cut first, it's probably because it's the very one I was exempt from. However, my life, like everyone else's, has been profoundly marked by this other great cut, which I saw later in the course of reflection and which I called the "**vertical**" cut: the one that separates, to set them against each other, the two "halves" of the feminine and masculine in each being, tolerating only one to the exclusion of the other. This is precisely what we've been talking about in this long digression on yin and yang, which I've been involved in for the past week or two.

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□ It now seems to me that this division ("vertical") is even more crucial than the other ("horizon-"). tale"), that in some sense it implies or "contains" it. After all, to **separate** the child from the mother, and life from death; to associate with death, as with the impulse that links the child to the mother, a feeling of **defilement**, **repulsion** or **shame** is also to **cut off** from each other, to set them against each other, the husband and wife in those two indissoluble and primordial cosmic couples: mother - child, death - life<sup>60</sup> (\*).

<sup>58</sup>(\*\*) I'm convinced, moreover, that this content of the yang love drive is present in all living species and even beyond; that it corresponds to the same profound dynamic of all things in the Universe: that every creative process (or "act") is an embrace of yin and yang, of "the Mother" and Eros the Child, returning to and abysing in her. From this "death" (or "birth in reverse") of the child returning to the Mother, emerges, as from a nourishing womb, the **fruit of the act**, the "work". It is the appearance of the "child", the **new** thing, through the act of death and renewal of the "**old**" that gives birth to it. In this cosmic dimension, the original sex drive has been present since time immemorial, long before the appearance of the human species and even before the appearance of life (in the biological sense) on our planet.

<sup>59</sup>(\*) (October 24) It's strange, then, that among the yin-yang pairs I'd noted a few weeks later, the "death-life" pair doesn't appear. Perhaps this was due to confusion with the related pairing "death - birth" (or better still, "dying - being born"), so that the former might seem to duplicate the latter.

<sup>60</sup>(\*) I've written the pairs here in "natural" yin-yang order, starting with the yin term, the "original" term.

On the subject of the "mother-child" couple, note that the term "mother" also figures in a second important archetypal couple, the primitive "mother-father" couple, giving its name to the group it describes. (The

Interestingly, these last two couples are not among those I had identified in the "Eloge".

The "death-birth" couple, on the other hand<sup>61</sup> (\*\*)'□ <sup>more</sup> directly linked to my love experience, is included. The couples

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"Mother-child" and "death-life" only came to mind in the course of my reflections over the last few days, among many others that had previously escaped my attention, one of the most interesting of which is "evil-good". This is one of those couples (like "death-life") that can be called "difficult", in the sense that such powerful conditioning makes us apprehend the two terms as antagonistic "opposites", rather than as inseparable complements. Clearly, these conditionings were stronger in me five years ago, when I wrote the Eulogy, than they are today. Yet there were already a good number of "difficult couples" in the Eulogy, including "chaos-order" and "destruction-creation" . .

In retrospect, a somewhat deeper understanding<sup>62</sup> (\*) of the nature of the various yin-yang couples, as forming a harmonious entity of inseparable complements, now appears to me as so many "thresholds" to be crossed in our journey of discovery of the world and ourselves. Such a "threshold" is all the more notable the more "difficult" the couple in question, i.e., the more its apprehension as a "couple" comes up against stronger inner resistance, an expression of cultural conditioning.

#### 18.2.4.2. The beloved

**Note** 114 (October 26) Yesterday's reflection<sup>63</sup> (\*\*) was a little difficult to get going. This is no doubt due to the many interruptions of the last few days. However, since the day before, there was something still warm inside me that I was anxious to put down on paper, if only in a few lines. Afterwards, I was ashamed to realize that it had been lost along the way, ousted by everything that came along! Today, I couldn't bring myself to part with it prematurely, as if by misunderstanding, before I'd even really got to know it.

In the recent reissue of "Zupfgeigenhansl"<sup>64</sup> (\*\*\*), I leafed through this classic old song

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In fact, the "mother-child" couple is a different group, the one I call the "cause-effect" couple). Moreover, the yang term "child", of this same "mother-child" couple, is also part of another archetypal "old-age-child" couple, close to the very interesting "maturity-innocence" couple. These two couples are part of the group I call "high-low", which is the richest (if only numerically) of all those I've detected so far. It contains many other remarkable pairs, such as **decline-essence, dying-birth, destruction-creation, forgetting-learning, finding-beginning**... . In enumerating these few couples, I had to go to great lengths to name them in yin-yang order, against ingrained habits. On the face of it, the new order looked a little zany, even bizarre - the world turned upside down! On closer inspection, however, we realize that this unusual order reveals **another** aspect of the relationship between the two terms, a complementary aspect to the usual one where (for example) "to be born" precedes "to die". - whereas we've just seen that "dying", in a deeper sense, precedes "being born".

The same goes for the overall name of my reflection, "Harvest and Sowing", which is undoubtedly a yin-yang pairing (which I'm just discovering!). It's named again in reverse order to the usual yang-yin order, with harvesting supposed to **follow** sowing, not the other way round. Yet the name came to me without any ambiguity whatsoever, and without at any time even the idea that it might have been the reverse, "Sowing and Harvesting". It was being confronted with unwelcome harvests that each time drew my attention back to the sowing from which they sprang; as if the profound meaning and function of the harvest had been to stubbornly **bring me back** to the long-forgotten sowing of my own hand... .

<sup>61</sup>(\*\*) Please note that in this "death-birth" pairing, the term "death" does not have the same meaning as in the "death-life" pairing: in the former it designates an **act** (synonymous with "death"), in the latter a **state**. In German, there are two different words: "Sterben" (without the rather cavalier connotation of "trépas") and "Todt". In French, I think it's preferable to refer to the couple as "mourir-naître", which eliminates the ambiguity surrounding the meaning of the term "mort".

<sup>62</sup>(\*) By this I mean an understanding that is not purely intellectual, but manifests itself concretely in a changed relationship to others, to the world or to ourselves, in changed ways of being.

<sup>63</sup>(\*\*) This is the reflection in yesterday's note (n° 116), which I have placed **after** today's.

<sup>64</sup>(\*\*\*) In Wilhelm Goldmann Verlag (1981).

compiled and published around the turn of the century. I'd heard it had become impossible to find, but some German friends □ who were passing through my house had brought me a copy. That day (so the day before yesterday) I had

I had a quick look around before getting down to work, a bit like shaking hands with an old friend in passing. I came across the song "Wohl heute noch und morgen", which I skimmed through without really stopping, in a hurry to get back to my work. Still, something clicked. I sensed that these simple, seemingly naïve words were delicately touching something deep inside me - something, moreover, very close to what I had been trying so hard to evoke three days before. I was just about to rewrite my notes on the subject. Perhaps I had a vague feeling that the stanzas I had just gone through were more faithful and convincing messengers of what I would have liked to communicate, than my notes of preemptory brevity, written in the rush yet again towards something else, as if in passing, while the emotion of an immediate experience remained absent.

When I got up this morning, I tried to translate into French these stanzas, whose tune I didn't know, yet which had been singing inside me for two days. Surely it was a way of finding them better, of letting their flavor and melody penetrate me. To my surprise, it wasn't too difficult to find some of the rhythm and music of the German text in another language, which at first seemed reluctant, while remaining very close to the literal meaning. So here are the seven stanzas, rendered as best I could<sup>65</sup> (\*).

"This day again and  
tomorrow by your side will  
be  
but as soon as the third day dawns,  
I'll be on my way."

"But when will you come again  
m'amour, my sweet beloved?"  
"When the snow falls on red roses  
et quand pleuvra vin frais!"

"Roses do not snow and  
wine does not rain  
so, m'amour my sweet beloved  
neither do you return!"

I lay down in my father's  
garden, and a lovely  
dreamlet came to me as I  
slept.  
white snow on me snowing.

□ And when I awake, here is pure  
emptiness pure nothingness -

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<sup>65</sup>(\*) (October 29) The following version was revised over the following three days. In the evening we sang and I was able to learn the tune of the song. Most of the changes to the initial version were made to take into account the requirements of rhythm and tonal accent in the sung text. Even if it's necessary to divide the syllables appropriately between the notes of the tune, it can be sung with the French text, without at any point having to do violence to the tonic accent (as is unfortunately common in some recent French consonants).



it was the pretty red roses on top of  
me blooming...

Come back boy and pass, all soft  
inside the beautiful garden  
wears a wreath of roses and  
a goblet of wine.

With his foot he stumbled,  
gentle to the pretty  
monticulet  
fell - and snow roses also  
rained fresh wine. ...

There was a joy, a happiness inside me, as I groped for a way to render what I was reading, which with each passing moment became like a part of me. There was this gentle, bare beauty, at once calm and poignant, a grave beauty made up of joy and sadness intimately entwined. I don't think there are many people who aren't touched to some degree by a song like this, even though they'd rather not be - as so often we defend ourselves against an unexpected emotion, when something deep inside us that we didn't know existed suddenly resonates, and speaks to us in silence about what we'd rather not know.

It's the dream, above all else, that has the power to make that resonance in us which must remain hidden, ignored, that which must remain silent. Perhaps only the language of dreams has the power to touch those secret chords within us and make them sing in spite of ourselves. And when, for a moment, you have allowed them to sing, even if it's a song of pain or heavy sorrow, you suddenly feel light and as if new - **washed** clean, as if abundant water had passed through your being and dissolved and carried away all that in faith that is knotted and hard and old. ...

When the poet is about to strike one of those chords whose song unleashes the inner waters, he instinctively borrows the language of dreams, at once limpid and charged with mystery - a language of images and parables, which baffles reason by its apparent absurdity, and by its secret obviousness goes straight to where it wants to touch!

There's no need here for the word "death" to be uttered, or any other word that for the awakened mind would be related to it.

reports. Yet **she** is present, and her misty face is that of the Beloved. The distant, sleeping Beloved you left behind long ago, and yet so close - both snow and falling roses.

into snow and is born of the snows. ... The force that draws you in□It is like a very deep and powerful wave, a wave coming from She who calls and bringing you back to Her. And the call is poignant sadness and the return is joy that sings in a very low voice, and joy and sadness are **one** and the same, and **are** this wave that carries you into the Beloved, with the unreplicated strength of childbirth. p. 514

And there was no need to evoke, even in a single word, this longing and the surge of desire for you, **the child** - for the "boy" that the Beloved calls to Her. All it took was for a dream to speak of Her sleeping in her father's garden, dreaming of snow and waking up to roses, for that long-forgotten wave to awaken in you too, responding to the longing of Her who dreams and wakes, calls and waits... .

#### 18.2.4.3. The messenger

**Note 114** This old Silesian song is one of many love songs, old and not so old, singing of the mysterious and poignant amalgam of the **beloved** and **death**. The one I

I've just transcribed is perhaps exceptional for the profusion of images charged with meaning, and for the wealth of associations it elicits. It's not my intention here to go through them one by one, after evoking one or two that struck me most strongly. When, yesterday and the day before, my thoughts returned to these hastily-read stanzas, it was not then in the sense of deepening an emotion, which at first remained epidermal. Rather, it reminded me of the extent to which the themes of love and death, or of the beloved and death, appear to be linked, as if by some mysterious spell! And beyond the theme of death on the beloved's face, they join that of birth - of awakening - roses out of sleep - snows, both mysteriously united in the poignant image of roses falling in snow, on Celle who is both dreaming and awakening, asleep in her father's garden.

The taboo may well inculcate the repulsion of death, its incompatibility with life as with love! You'd have to believe that it runs counter to some deeply-rooted knowledge, or an impulse as powerful as it is secret, for what must be separated at all costs to seem so tenaciously to want to come together, taking the circuitous routes of symbol and dream, through songs and myths handed down from generation to generation, century to century.

p. 515 No doubt many scholarly volumes have been written on the subject of these troubling amalgams. to exorcise them as best we can. *Nobstant de tels efforts, sûrement aussi, "quelque part" en chacun de nous, le sens profond de ces associations tenaces est perçu bel et bien - en Les moments, tout au moins, où* we don't deliberately close ourselves off to the emotion within us that welcomes these messengers, speaking to us about ourselves in the elusive and powerful language of dreams.

This "deeper meaning" is revealed to us anew, directly and with elemental force, by the experience of love, provided we dare to live it fully and listen to its obvious message. It speaks to us of the mystery of death and birth, indissolubly linked in the Act that transmits life and renews lovers.

No doubt I'm not the first person in whom this "deep-rooted knowledge" has risen from the obscure depths where it had long been exiled, to become fully conscious and permeate all the more strongly my relationship to death and life, to the world and to myself. I have the impression, however, that written and published testimonies of such knowledge on a conscious level must be rare. The only ones I've come across so far are three or four stanzas from Lao Tseu's Tao Te King<sup>66</sup> (\*). On the other hand (and somewhat paradoxically), I also have the impression that the amalgam "love-death" must, at some point, have become a kind of romantic poncifix, a very safe "cream pie" to draw a tear - complacent from even the most reluctant eyes. It's a fact that the process, over time, has

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<sup>66</sup>(\*) (October 30) I came across these passages from the Tao Te King towards the end of 1978. It was a striking, entirely unexpected confirmation of things I had felt strongly about (some for a long time, others only recently. . . ), and which I seemed to be alone in feeling this way. This "encounter" was experienced as a great goose, a silent exultation. This joy and exultation carried the gestation and writing of *L'Eloge de l'Inceste* over the next six or seven months. The conception took place in the days or weeks following this encounter. On a more modest or humble note, I've felt a similar joy in recent days, "recognizing" the emotion that had animated an anonymous poet (dead for centuries) when he sang of those roses falling in snow, born absurdly, miraculously from "l'auter Nichts" - from "pure emptiness, pure nothingness"; or to put it better, in rediscovering, through my own intimate experience, this **same** emotion, a sign of the same knowledge. It's the same knowledge that can be found in the Tao Te King, over more than four millennia - with the difference that, in the Chinese text, this knowledge is expressed in the pictorial, but by no means symbolic language of a highly awakened consciousness, and not in the language of dreams (which is also the code-language of the deepest layers of the psyche).

The content I recognized in these few stanzas from the Tao Te King has obviously escaped the translators of the five or six different versions (in French, German and English) I've had in my hands. I'm not surprised. Such messages, expressions of an understanding that runs counter to millennia of conditioning, communicate their true meaning (beyond the words and images used to express it) only to those who already know it through what they have assimilated from their own experience, or to those in whom the work of assimilation is ongoing and who are already very close... .

has come into disrepute - so much so, alas, that even among people with delicate sensibilities, there is a tendency to confuse pure gold with its crude tin counterparts. Some see an old-fashioned or even ridiculous air, even where there is a keen perception of a hidden reality, and a delicate expression, foreign to all "fashion". Here, a consensus of "good taste" comes to the aid of all kinds of inner resistance, which automatically screens out the eruption of any vivid, authentic emotion, be it joy or sorrow, pleasure or torment, that comes to shake up the familiar routine.

It's the same mechanism that so often blocks the original force of the love game and its orgasmic outcome. Fortunately, the mere fact that they remain hidden, banished from the field of consciousness, in no way prevents the archetypes that drive the love drive from being present - from making what must disappear vanish, so that the meaning of the love game can be expressed and fulfilled, and the final act can be a creative act, a renewal. Often, however, a secret **fear** stands in the way of the very "pleasure" we think we're looking for, frightened as we are by the very presence of an unknown and formidable force, which risks (if we're not careful. . . ) sweeping away like chaff the one in us who insists on keeping "control" at all costs. Such fear cannot tolerate that pleasure never approaches that threshold of poignant intensity where it is both pleasure **and** torment, united to each other in a long and intolerable embrace that seeks deliverance, to finally resolve itself and sink into orgasmic nothingness... .<sup>67</sup>(\*)

(October 27) I think I've understood the secret message of songs and dreams like "Ce jour encore et tomorrow... .", in the **essentials** they share. The question then remains: what is this force that

□ pushes with such insistence to give voice to this "deeply rooted knowledge", older

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that our species; to express it against all odds, nobosting the vigilance of the surly, narrow-minded **Censor**, by taking the key to the fields and giving free rein to the symbolic language of dreams, with its unlimited resources?

If myths, songs and dreams breathe the same message with countless faces, it's also true that the prisoner to whom they're addressed never tires of hearing them! It's true that he's a willing prisoner, but he never **listens**. He's frustrated with air, space and light, yet reassured by the four walls that surround an existence devoid of any great surprises or mysteries, except perhaps death, which lies at the end, infinitely far away... . His prison protects him from **the Unknown** that lies beyond these walls, and which he pretends to ignore. It both frightens and fascinates him. It's because the Beyond its walls frightens him, that his prison-refuge is dearer to him than life itself. And yet it fascinates and attracts him, unwillingly, just as the messengers who come from far and wide to tell him about it attract and fascinate him. And sometimes he gives in to this unusual attraction, as long as it's in secret from the Censor-Supervisor General: while he lends an ear, he is nonetheless "thumbs up" - he hasn't heard anything, and above all, he hasn't listened to anything!

The question I was asking myself just now seems to have disappeared, swallowed up by a convincing image. It reappears, as soon as I remember the **effect of** the message - that **emotion** that comes before the message, and the **benefit of** that emotion.

But the truth is, **any** emotion that strikes a deep chord is a messenger from beyond the four walls, a messenger from the open sea. Even though we would strive to erase all trace of it the next moment, it is beneficial, it has already left its mark, like a delicate perfume - as if these dull walls

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<sup>67</sup>(\*) (October 28) It's this same fear, manifesting itself as a kind of **refusal** of pleasure, which at the same time pushes us to **isolate** pleasure from the whole experience of love, reducing it to the finality of love (sometimes tacit, sometimes clearly expressed). "Love" is thus reduced to a "search for pleasure" - an exchange of courtesies, in short, between two partners, like inviting each other to dine in four-star restaurants, if not at the Folies Bergère. This "pleasure", fearfully kept on a leash, is just as foreign to the original impulse, as dry paint chips scraped off a painting by the hand of the Master would be to the painting; or as a hairdryer is foreign to the great offshore wind, laden with the perfumes of the sea and the land... .

or as if, through some unsuspected opening in the sanitized air, we could catch even the slightest whiff of the scents of woods and fields.

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□(October 28) Over the last fortnight or so, I've been reluctantly giving the matter some thought. It's a move in a direction I hadn't planned, with no apparent connection to the theme of L'Enterrement, or even (it might seem) to myself. I know deep down that this is not the case, that I continue to be involved in these notes as much and more than ever. Nevertheless, I'm torn between the desire to "get it over with", and the desire to delve into what is glimpsed day by day, to follow the most compelling associations.  
- A desire that goes hand in hand with my concern not to let anything slip that might shed light on my "investigation" into the Burial. What seems most distant is sometimes also most intimately close. ...

The fact remains that for the past fortnight, if not ever since I resumed writing after my illness, I've had the impression (a little painful at times) of doing things "in a hurry", as if each new note were another parenthesis that I opened (in front of an imaginary reader who would cry for mercy) and that I had to close as quickly as possible! I'm sure it's this attitude, perhaps even more so than the unusual number of friends I've had over the last few weeks, that has led to my writing being hasty and a little jumbled at times. I've had to rewrite most of the notes I've written recently as I went along, retyping them on the net. This further slowed down progress, and kept my impatience to see the work move forward!

It's also true that these themes - which I sometimes pretend to want to deal with at once, as if they were "well known" and that I'd take the trouble to spell them out for the benefit of a reader who'd just "arrived" - are both too delicate and too far-reaching to bear such casual dispositions. I couldn't help noticing this as the pages went by, and "rectifying" the situation, by which I mean readjusting my inner attitude, under the weight, so to speak, of what I claimed to be able to tackle on the sly!

It reminds me that this long reflection on y in and yang, in which I've been engaged for almost four weeks and which is by no means finished yet, is in fact merely **the clarification of** an instantaneous intuition, which seemed to me quite simple, not to say obvious; an intuition that came "in flash" the day after May 12, when I had just written the first note on a certain "Eloge...".

Funèbre". When I took up the rest of this note, a month ago<sup>68</sup> (\*), willing myself to follow this association of ideas, in preference to others which seemed to me of lesser interest' □<sup>1</sup> foresaw that it would engage me in another five or six pages at the most. Now I'm over sixty. ...

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Yesterday I pondered the meaning of the symbolic evocation of the links between love and death, or death and birth, or life and death - and the meaning, too, of the emotion such evocation arouses in us. What is the force at work in myth, or song, or dream, that drives them to "breathe into us without tiring of the same message with countless faces", - and what is the force in **us**, willing prisoners of reassuring prisons, that so often responds to them with this emotion, going ahead of the evocation and showing that it has "hit the nail on the head", that it has touched where it wanted to touch? And also: where does this strange power of dream language come from, language that evokes without naming, that communicates what no other language knows how to communicate?

Pursuing these questions also means delving deeper into the role of the love drive and the dream, and the profound links that bind them; each nourishing the other and nourished by it, each expressing itself, and communicating with the other, through a language that is common to them and that escapes the Censor. It is also

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<sup>68</sup>(\*) In the note "Muscle and tripe (yang enterre y in (1))", n° 106.

further explore the role of archetypes and symbols in the love drive, and the role of "symbolic" satisfactions of the drive.

All this is taking me far beyond the limits of what I can reasonably hope to "fit" into this "digression" on yin and yang, continuing (it's about time I remembered) right in the middle of a certain Funeral Ceremony! I think it's time to leave this new "thread" there, and return to another "thread" left hanging three days ago<sup>69</sup> (\*), which brought me back to myself.

#### 18.2.4.4. (d) Angela - or farewell and goodbye

**Note** 115 □(October 30) For a day or two a few lines have been running through my head, from a poem written yp . 520

three years ago. I wrote it first in German, and the next day took it up again in French. It was the first two stanzas that had come to the fore - the third and last seemed to have faded from memory, except for the first line "Ein Kreis schliesst sich" - "A circle is perfected". (And apart from the last line, which repeated that of the first stanza). When I woke up last night, my thoughts returned to it again, and I finally got up to rummage through my papers. I found the poem with no trouble at all - every cloud has a silver lining! And here it is.

Dense, ripe,  
heavy fruit  
my life is bending  
over backwards  
in Her

The sweet, thick juices  
soaked me  
have blossomed  
fragile milk flowers become  
fruit and wine

A perfect circle -  
from my lap  
mounts softly  
describes its orbs  
and bends over to turn  
in Her...

I think this is the only poem I've ever written in which the thought of death<sup>70</sup> (\*) is clearly present. Here it appears under the name "She". In the primitive version of the previous day, it was evoked by the German word "Erde", earth. The German "translation" of the three stanzas is far from literal; the first came as follows:

Voll und schwer

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<sup>69</sup>(\*) In the note "Le paradis perdu" (n° 116), placed after the present note (n° 114).

<sup>70</sup>(\*) I should rather write: the thought of my death. Two poems (each a few lines long) written in 1957, the year of my mother's death, are imbued with the presentiment of this death.

## 2. A walk through a work or the Child and Mother

rei fe Frucht  
neigt sich me in Leben  
gen Ende  
Der Erde zu

Die sussen Säfte  
die mich durchtränken  
haben geblüht  
weiche Blilten und wurden Frucht und Wein

Ein Kreis schliesst sich  
aus meinem Schoss  
steigt Süsse  
kreist  
und neigt sich  
gen Ende  
der Erde zu...

□

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Finally, rewriting the original German version just now, I couldn't help but write it all the way through, so much so that the next two stanzas seemed to flow spontaneously from the first! For me, these three stanzas are a love poem (in fact, I've hardly written any poems other than love poems). If this one is addressed to anyone other than myself, it's to **Her** - to She who waits in silence, ready to welcome me...

On the same day, I had written two other poems, one before and one after. They were addressed to a flesh-and-blood "beloved", Angela, "l'Ange" - a tall, slender, blonde girl, very much alive, whom I'd met the week before, on the hot summer road where she'd been hitchhiking. In an hour or two we'd had time to say a lot to each other, and we'd parted on that note. I would have liked to give her the poems she had inspired, including another one written on the very evening of the day I had met her, and yet another (always in German, our common language), which came the day after the "three (almost) at once". And I wish we'd loved each other too... . But I lost track of her, just as she must have lost mine.

One thing the poems inspired by this encounter have in common is that each is either very strongly "yang" or very strongly "yin". They are some of the most intense I've ever written, and each came in one go, almost without retouching - as if they'd been there all ready and waiting only for the signal of this encounter to take shape in tangible words.

At first glance, it may seem strange to find among these poems charged with intense erotic tension, this other poem in autumnal tones, about to enter the long sleep of winter. There was, in those lonely days, an intense perception of life, amplified by erotic emotion and by the profusion of archetypal images that underlie it - and at the **same time**, the serene detachment of a life fully lived, nearing its end, ready to "return to Her".

Such dispositions of communion with death, our silent Mother, felt as a friend and very close, are surely favored by a state of great fatigue of the body, bringing us back to simple and essential things: our body, love, death... . I was just coming out of a "long period of mathematical frenzy",

which I have already mentioned in the introduction to *Récoltes et Semailles*<sup>71</sup> (\*). I was just beginning to recover from unp. 522 state of physical exhaustion in which this somewhat demented period had left me. It had just come to an end (as suddenly as it had come) under the impact of a dream-parable of lapidary force, which I kindly then listen to the message<sup>72</sup> (\*\*). These were days of availability, of listening - a "sensitive period" of an in-between wave: behind me a long and ample "mathematical" wave, and in front of me a no less ample "meditation" wave that was already announcing itself... . It would gain momentum ten days later, with another dream, the account of which opens the introduction to *Récoltes et Semailles*, this vision of myself "as I am".

These were weeks of intense inner work, of silent gestation, of change. And these love poems, different in tone from any I'd written before, are a fruit and a testimony of this intensity, this plenitude.

They're also the last love poems I ever wrote. Perhaps there was a prescience in me that this was the last time I would be in love, and that the great fireworks of songs for the beloved would unfold! A prescience that these poems addressed to an unknown girl, whose beauty I could feel intensely without having known her, were at the same time a farewell to love songs and to the women I had loved.

- a farewell to my passion for love, which was about to be consumed in this sparkling spray, and which was about to leave me. And, more secretly and profoundly still, that it was a farewell (or a goodbye, perhaps) to **all** women, merging and becoming **One** under a new face. A more distant face perhaps, drowned in mist, at the other end of the road - but at the same time very close, and very sweet. ...

## 18.2.5. Refusal and acceptance

### 18.2.5.1. (a) Paradise lost

**Note 116** □(October 25)<sup>73</sup> (\*) Again three days have passed without my finding the time to continue on my momentum. The first day, Monday, was taken up mainly by a visit from Pierre and his two-year-old daughter Nathalie, whom I saw off late in the evening to catch the night train to Orange. In a few days' time, I'll be able to take stock of what I've gained from this visit - a visit I wasn't counting on any more. . . For the moment, I'd prefer to continue my rambling reflections on yin and yang.

This reflection may seem like a philosophical digression, suddenly bursting into a certain investigation where it would have no place - except that it emerged unannounced from some vague associations of ideas around a certain Funeral Eulogy. ... However, I feel that it is precisely with this "digres- sion" that I'm beginning to go beyond the stage of uncovering all the "**raw facts**" that make up *L'Enterrement*<sup>74</sup> (\*\*), to finally approach, if only a little, the **forces** at work behind the "facts".

<sup>71</sup>(\*) See "Dream and Fulfillment", especially page (iii). The "period of frenzy" in question extends from February to June 1981. It is also that of the "long march through Galois theory" (see "Galois' legacy", n° 7). It leads into a long period of meditation on my relationship with mathematics (see the sections "Le patron trouble-fête - ou le marmite à pression" and "Le Guru-pas-Guru - ou le cheval à trois pattes" n° s 43 and 45). This runs from July 19 to December 1981. The poems to Angela (and the poem to "Elle") are dated July 8 and 9 (except for the very first, dated July 1).

<sup>72</sup>(\*\*) See the beginning of note no. ° 45, quoted in the previous footnote.

<sup>73</sup>(\*) (November 1) This note predates the two preceding ones, written between October 26 and 30, which form a direct continuation and deepening of the one that immediately precedes them, "L'Acte" (n° 113, October 21). The present note, "La moitié et le tout - ou la fêlure" (The half and the whole - or the crack), is more closely linked to the end of the preceding note of October 17 (n° 112). From this point onwards, the reflection had split into two parallel tracks: one (on the feeling of death and its link to the love drive) continuing in the three notes (presented as consecutive) 113, 114, 115, and the other initiated with the present note n° 116.

<sup>74</sup>(\*\*) (November 14) This assertion, made "in the heat of the moment", has not been carefully weighed up, and is only partially founded. For a more detailed and nuanced overview, see the note "Rétrospective d'une méditation - ou les trois volets", d'un tableau", n°

acts and behaviors that seem strangely aberrant... . It's surely no coincidence that it was precisely through this "digression" that I was also led, without having planned it, to involve myself in a deeper way than at any other time in Harvest and Sowing. This is one of the unexpected fruits of the recent illness-episode, which occurred at a time when I was about to bring the seven-week investigation to a swift conclusion... ..

This "digression", then, in which some will see a kind of intimate confession, and others a metaphysical speculation, is for me (more than any other part of Harvest and Sowing) at the very heart of Burial, at the **heart of the conflict**. It's only the optics that have changed, the "point of view" from which the thing is seen. is looked at - but suddenly, changed so drastically, that the thing we'd just been examining seems to suddenly disappear!!! I think it won't be long before we □recover the contact that might have seemed lost in road, with the news item l'Enterrement.

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But we can also forget about the news item, whose main merit will then have been to encourage "digression". . .

Part of yesterday was spent retyping the draft of the previous note, written four days ago, which I finally named "Our Mother Death - or the Act". Much of this draft was quite heavily crossed out, a sign that the wording had remained a little muddled, while some important and delicate themes had crept into the reflection a little "by the by", in the stride towards something else. To tell the truth, when I began this note, I was mainly intending to pick up the thread of the previous note, entitled "The half and the whole - or the crack", written just a week ago. But in the end, this thread remains unfinished, and it's about time I picked it up again.

For this note too, I had to retype a large part of the text, essentially for the same reasons, rectifying clumsiness and obscurities along the way. This is the beginning of a reflection on the **division in the couple**, intimately linked to the **division in the person**, and pi us precisely to what I called (in the "Act" note of four days ago) the "vertical cut": that which "cuts", or subtracts, one of the yin or yang "halves" of the original "whole" within us.

At a level which now remains that of an intuitive, non-verbalized understanding, I "understand", it is "clear" to me, that it is the division within the person himself (a division created from scratch, it would seem, by conditioning) which is the root cause of the omnipresent conflict in human society; be it the conflict within the couple or the family, or the conflict within larger groups or that which sets such groups against each other, right up to the armed confrontation of peoples and nations against each other. The conflict within a couple, which pits two antagonist-types against each other, distinct and easily recognizable as such, could not without reason appear as **the** fundamental parable, as **the** elementary, irreducible case of conflict in human society. The "point" of the "Crack" reflection was

above all, to bring the case of conflict within a couple back to that other more fundamental, even more "elementary" conflict: that of conflict within □each person himself, which pits one "part" of himself against another part.

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In the light of this reflection of seven days ago, it was natural to think first of the conflict between the yin and yang "parts" within us, one of the two being accepted and duly put forward and inflated, the other more or less completely rejected and repressed. I was aware, however, that there were other antagonisms within the person, linked to taboos other than that of **the univocity of sex**. It's true that this last taboo, just as strong as that of incest, is even more insidious because of the obviousness with which it is clothed, which seems to obviate the need even to formulate or name it, so much so does it seem to go without saying! Although I haven't yet taken the time to verify it step by step, I have the impression (since the reflection on the Praise) that this taboo is the most crucial of all; that the division or "cut" it institutes in the person is the most important one.



the ultimate root of each of the multiple aspects of inveterate division in the human person. Carefully clarifying the extent to which this is the case would surely be a most attractive starting point for a "journey of conflict discovery". This is not the place to launch into it, however - not to mention the fact that, as far as the journeys ahead of me are concerned, I can think of more burning starting points than this... .

In retying the text of this note "The half and the whole - or the crack", I realized that in writing it, I hadn't thought to explain in the slightest **why** I saw conflict within the person as the root cause of conflict within the couple, and of conflict within society. As I said earlier, this is one of the things I've "understood" (without ever having had to "explain" it to myself), which have been taught and confirmed by the silent and eloquent language of a thousand little everyday facts, over the days and years<sup>75</sup> (\*). I'm not saying that it's pointless to spell out or "explain" here the "why" and the "how", whether in a few pages, or in imposing volumes perhaps. And no doubt a few pages on the subject, here, would be no more or less "out of place" than any other page on yin and yang and conflict, which has already found its place in these notes. I'm sure I'd learn lots of things, like I'd also learn more by pursuing this other theme of reflection, on the conflict instituted in us between yin and yang, and yang as the ultimate cause of division within us. □ One of these themes visibly extends p. 526 the other, which makes both even more appealing! However, that's not the direction I'd like to pursue right now, if at all. It's not the "thread" I've been wanting to pick up for a week now, and which is still unresolved.

When I finished the reflection in this note<sup>76</sup> (\*), a week ago, I suddenly felt very happy and delighted: the reflection had unexpectedly reconnected with something important, which I had somewhat lost sight of in the previous days: **acceptance**. It was through the negative that this contact was re-established, by virtue of the word that ends this reflection like an unexpected climax - the word "**unacceptable**". It's because a whole "side" of us is rejected as "unacceptable" by those around us, and first and foremost by our parents who set the tone (or by those who take their place, when parents fail) - it's through this **non-acceptance** that conflict sets in within us. The conflict and division within us is nothing other than our **abdication of** a repudiated part of ourselves - the abdication of our undivided nature. This abdication is the price we pay, the price we **must** pay, to be "accepted" both in our own right and by others.

by those around us. This "acceptance" is not acceptance in the full sense of the word, acceptance of who we really are. It is, rather, □ the **reward** for our submission to p. 527

This is the reward for a **deformation** and **mutilation** of our being, just as those around us have undergone from an early age.

In the reflections in the previous notes, there was mention of acceptance of repeated games, and both times the ac-

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<sup>75</sup>(\*) This "understanding" or conviction is not really contradicted, it seems to me, by the observation I've made many times, that the division in the couple formed by mother and father, and the antagonistic attitudes that express it, leave a deep mark on the child, and often dominate adult attitudes and behaviour. It is surely fair to say that, to a large extent at least, the division within us is the mark and legacy of the division that, in the days of our childhood, pitted our mother against our father. So the question of whether the division in the person is more fundamental or "elementary" than that in the couple, or vice versa, may seem a little like deciding whether the chicken comes out of the egg or the egg out of the chicken!

I'm convinced, however, that in a couple where one of the spouses is "one", not in conflict with himself or herself, and even if his or her spouse maintains an antagonistic attitude towards him or her, the conflict would **not** be transmitted to the couple's children. The reason for this belief, I believe, is that the child in such a case would be totally **accepted** by one of the parents. The appearance of division in the young child seems to me to be no more and no less than the effect of the **rejection** of a part of his being by those around him, and first and foremost, by **both** parents.

<sup>76</sup>(\*) The note "The half and the whole or the crack", n° 112.

ception appeared as a crucial thing, the first time being in the note "Innocence (The marriage of yin and yang)" (n° 107), where I take up an observation that goes back to a meditation four years ago: that the blossoming and full flowering of an undivided force in me could take place in the context of from a family torn apart by conflict and latent hatred, **simply because I was fully accepted by my parents** and those around me, conflict didn't take root in my being until later, after the age of five, in a much more "peaceful" environment than my birth family. Conflict between close relatives was certainly far from reaching (in my day at least) such exacerbated (if veiled) intensity as in my family of origin. And yet, in my family of origin, I myself remained **outside the conflict**. Even when I sometimes took sides, it wasn't a heartbreak, it was the spontaneous expression of an undivided being, who had never known the bite of rejection by his own kind, or the fear of rejection.

I realize now, with half a century's hindsight, that even in my new environment, this force of innocence in me exerted a radiance, a kind of fascination I'd say; like that of a **lost paradise**, infinitely far away, for which we might be nostalgic for a lifetime, and which suddenly calls out to us through the voice and gaze of a child. It won me strong and lasting affections, which followed me into my adult life, and right up to the death of those who loved me in this way<sup>77</sup> (\*). But **at the same time**, it was obvious that this force **could not be tolerated** - any more than one tolerates it in a tidy pleasure garden, in a vigorous, exuberant tree or bush that one thinks one loves, while stubbornly pruning it into the shape of a cube, cone or sphere. ...

According to my reconstruction of events<sup>78</sup> (\*\*), this force held out for perhaps two, two and a half years, before plunging deep, relegated into the underground, after I had finally decided to be and do what everyone else was doing: all muscle all brains, you guessed it, and never mind the guts - and be left in peace! I ended up following suit, **rejecting** and denying (while ignoring) everything that needed to be rejected and ignored, thanks to the unwavering consensus of all the adults around me. And also by the consensus of my parents themselves, who had almost given up, living their true love as far away from their children as possible... ...

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### 18.2.5.2. (b) The cycle

**Note 116** (November 1) I'm resuming the thread interrupted exactly one week ago, when I unexpectedly (on October 26) launched into a kind of "poetic digression" on the feeling of death in love and in the song of love.

I've just reread the previous pages from October 25 and retyped the last one. I seem to see a circle that began two weeks ago with the note "Ecllosion de la force - ou les épousailles" (n° 107). This trace ends with the preceding pages, which repeat and amplify the final "climax" of the note of October 17, "La moitié et le tout - ou la fêlure" (n° 112). This climax, or "final word", which brings that day's reflection to a close, is summed up in the categorical imperative of the final word, **"unacceptable"**.

Among the bewildering multitude of conditioning of all kinds that have shaped our lives, this fine word seems to me to perfectly encapsulate **the** decisive cause of division within us: it's the **non-acceptance, the rejection** of ourselves, in the first years of our lives<sup>79</sup> (\*). It takes the form of non-acceptance, of the rejection of certain forces and impulses within us, which are an essential part of our being, of our power.

<sup>77</sup>(\*) I see seven people who have given me their affection in this way, only one of whom is still alive today.

<sup>78</sup>(\*\*) I made this reconstruction of the key events of my childhood in March 1980.

<sup>79</sup>(\*) My own case was exceptional in this respect, as I was only exposed to such attitudes from my immediate entourage from the age of six.

to know and create. Their repression, taken into our own hands by a worried and implacable inner Censor, is a mutilation of this power within us. Its effect is often to paralyze our creative faculties<sup>80</sup> (\*\*).

□ This unacceptable power, or these "faculties," are also nothing other than the humble ability to be ourselves. p.

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It also means living our own life, through the humble and full use of our own faculties, rather than a stereotyped, programmed life, driven above all (and often exclusively) by reflexes of **repetition** and **imitation**. These enclose us and isolate us like a heavy, stiff, impermeable shell from which we would never part<sup>81</sup> (\*).

The shell is built up from our earliest years, growing thicker as the years go by. Its initial function was undoubtedly to protect us from aggression (often well-intentioned) by those closest to us, and to ensure a more or less benevolent tolerance on their part. But this shell doesn't just protect us from the outside world - it also has, and perhaps more profoundly and essentially, the function of isolating us, of protecting us from **ourselves**: from that knowledge and strength within us, declared "unacceptable", having no place, by the mute consensus that rules around us. It was in our childhood, and has become more and more over the years, a **two-sided** shell, one "outer", the other "inner". They protect the "me", the "Boss", on the one hand from the aggressions he fears from the outside world (and he tends to become more fearful with each passing year!), and on the other hand and **above all**, from the disturbing and inadmissible fantasies and incongruities of the "Worker"; the dirty **brat** to put it better, unpredictable or possible, worrying still even though he's kept at bay by a triple layer of thick horn, guaranteed to resist fire and water... .

(November 2) After the note "Innocence" (n° 107 ), highlighting the role played by my acceptance by my immediate entourage in my early years, there was a second moment when "acceptance" and "non-acceptance" were at the center of reflection. It was in "Acceptance the yang

in the yin," (note n° 110), where I make a partial assessment of the changes that have taken place in me since the day a "reunion" with the child king. They are moving towards a gradual "return" to a "state of childhood".

□ This return is by no means a "regression" to a previous state, which would have the virtue of erasing the traces in me, p. 530

the traveler, of the path that was once mine. It's only through **maturation**, the fruit of inner work, that we can regain contact with an innocence that seemed to have disappeared, with a child in us that seemed long dead and buried. And there's no maturing that isn't also a return in some small way - a return to the child, and to the simplicity, the innocence of the child. This is how a life fully lived is like a circle still "perfecting" itself; it's old age returning to childhood, it's maturity returning to innocence - and ending in a death, perhaps, that prepares a new birth, as a winter prepares a new spring... .

In this sort of "balance sheet" of an unfinished road back, it became clear that the "final word" was **acceptance**, just as the final word of my path of rupture, of the path of departure, was that of **non-acceptance**, rejection, refusal. My maturation was nothing other than the process, the inner work, by which I gradually accepted, welcomed, the things in me that for a long time I had refused, eliminated as best I could, ignored.

This is by no means a "backtracking", a path travelled once that I would retrace again in the opposite direction; a "regression" therefore, to use the expression from earlier. Rather, it's like the upper arc of a cycle, extending and continuing the lower line already traced, **born** from it, which has become like its

<sup>80</sup>(\*\*) (November 2) Often, and more ostentatiously, it manifests itself in "blocking" effects - the inability both to "function" in such and such a situation in which we are engaged, and to disengage ourselves from this dead-end situation. ...

<sup>81</sup>(\*) Apart from the hours of sleep and dreaming, when the carapace becomes lighter: sometimes even disappears. ...

and the springboard for new momentum. ...

(November 3) Yesterday's notes ended with an unexpected image, springing from reflection without my having called for it. I greeted it with some reluctance at first, out of concern that the vision of reality that the image in turn immediately suggested might be artificial; that the image might "force my hand" and make me say things that would be "far-fetched". But once the last lines had been written and I'd stopped to think about them for a moment, I knew I'd put my finger on an unexpected and important aspect of a certain reality; an aspect I may know about, but haven't fully assimilated, an aspect I tend to neglect or forget.

p. 531 □ I've tended for many years (118) to value what goes in the direction of "acceptance", and, on the contrary, to see in a mostly negative light what goes in the direction of "refusal". Although perhaps not always clearly expressed, I felt that these two attitudes, acceptance and refusal, were "opposites", one of which would be "good" for myself and for everyone, and the other "bad".

In this informal way of apprehending things, I remained trapped (without realizing it, of course) in the age-old "dualistic" vision of things, the one I had also previously called the "warrior" vision, which opposes as antagonists things that a deeper vision reveals to us as **complementary** and inseparable **aspects of the** same reality. When I began (on October 25, ten days ago) the present reflection on Acceptance and Refusal, I had just realized that these are the wife and husband of one of those famous yin-yang or "cosmic" couples we've been talking about for the past month - since the beginning of this "digression" on yin and yang. So I anticipated that this aspect of things would come to the fore. Over the past couple of days, it may have seemed as if we were moving away from it. But now, the lines that conclude yesterday's reflection, with the image of the two arcs of the same cycle extending each other, have unexpectedly brought me back to this initial intuition, which had remained unexpressed.

I've tended to see the **rejections** that dominated my life from my eighth to my forty-eighth year in a predominantly (if not exclusively) **negative** light: as a sometimes crushing **weight** that I dragged around for quarter-century years of my life, and which I finally got rid of (or rather, **started to** get rid of) over the past eight years. This "day" began to reveal itself to me after the discovery of meditation and the "reunion" with the "child" in me. This was precisely the moment when I began to discover the process of refusal in my life, expressed in a kind of "superyang conformism". This aspect of things is by no means imaginary. To perceive it where before there had been a kind of "blank", a total emptiness, has been one of the fruits of the maturation that has continued over these eight years. Yet there is another aspect of the same reality, no less real and important, the "positive" aspect of the **"powerful principle of action"**. This aspect appears for the first time (and very discreetly) in the October 5 meditation "Yang buries yin - or the Superpère" (n° 108), when I write:

□ "The "I'll be like them" (and not "like me") also meant: I'm going to "bet" on "the head", not worse in me than in anyone else, after all, and 'beat' them with their own weapons!"

p. 532 It was this motivation that was the driving force behind my disproportionate investment in mathematics from 1945 to 1969 - the force that fuelled a quarter-century's worth of discoveries<sup>82</sup> (\*). Whether one chooses to view such investment in a "positive" or "negative" light, what's clear is that there was indeed **momentum**, intense **action**. On the learning side of life, there was that "sometimes crushing weight", never examined, so to speak.

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<sup>82</sup>(\*) It was, more precisely, the egotic component of this impulse, the **egotic** "factor" of this "living force".

not to say total stagnation - and yet this same "weight" at the same time fuelled a surge of knowledge, gave it its living strength.

Since my "departure" in 1970, I've tended to downplay, and sometimes deny, the "value" that should be attributed to such an impulse, in the direction of a so-called "scientific" discovery and understanding of the outside world. I've tried several times in the course of Harvest and Sowing to identify the common aspects of such discovery and self-discovery, and how they differ<sup>83</sup> (\*\*). It's certainly true to say that the impulse to discover in a scientific direction (be it biology, or "psychology". . . ) leads us away from ourselves and an understanding of ourselves. When the role of such an understanding is fully understood, we might be tempted to see the impulse to scientific discovery (and any other that "distances us from ourselves") as an "evil", or at the very least, an "obstacle" to maturation, and hence to full self-fulfilment (at least in the case, which has been mine for a long time, where this impulse mobilizes most, if not all, psychic energy). Yet it's also true that **everything** we experience is raw material for learning about life and ourselves. It's a **material that it's up to** us to allow to transform into knowledge, by allowing a process of maturation to begin and continue within us. That's also why I don't regret anything I've experienced, seeing in the end that "it's all good, and there's nothing to throw away"; including the deserts of long periods of spiritual stagnation, which were the main reason why I'm so happy with my life.

price I paid without skimping (and with my eyes closed. . . ) for my inordinate investment in a passion  
□devouring

. Now I see that these very deserts had something to teach me, that only they can- p . 533  
be could teach. I couldn't have done without it - at the very most, I might have been able, after a few years, to start this "second arc" of the cycle, which I'd been putting off for several decades.

It was on this day, too, that it became clear that the acceptance of myself and of others, which was born and developed in the years of my maturity, was "nourished" by the refusals that had marked the longest part of my life - this "lower arc" of the cycle evoked yesterday, and its "nourishing foundation". Certainly, in the first six years of my life, there was indeed a total acceptance of myself, which had in no way needed previous "refusals" to be, and to unfold and assert itself. On the contrary, it was able to blossom precisely because it was not countered, not trimmed by the scissors of a certain refusal. But this "acceptance" that was in me as a child is not **"the same"** as that of my mature years. It lacked a dimension that the mere acceptance of me, by those who had surrounded my childhood, could not have given it. It was a **knowledge of refusal**, of the rejection of myself (or a part of myself) by others, or by myself. This knowledge came to me through the experience of rejection, and also through the experience of contempt, which is one of its many faces.

Perhaps some people are born with a knowledge, an understanding of refusal, that enables them to remain **one**, innocent and knowing, despite the refusals to which their childhood is exposed. I am well aware that this was not my case. I could not do without the experience of refusal and contempt by others and myself, as a breeding ground for the blossoming of an understanding (however imperfect) of refusal and contempt.

### 18.2.5.3. (c) Spouses - or the "Evil" enigma

**Note** 117 I've just probed an unexpected aspect of the relationship between refusal and acceptance in my own life, which had appeared unexpectedly in yesterday's reflection. The "refusal" in question here is not, however, a refusal in the full sense of the word; I mean, a fully assumed refusal - not by a long shot. This refusal was also a

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<sup>83</sup>(\*\*) See in particular "Desire and meditation", "The forbidden fruit", "The solitary adventure", n° s 36, 46, 47.

long **flight** in the face of the thing refused. It consisted in **not seeing it, ignoring it**, and thereby, to a certain extent, making it disappear from the field of my conscious apprehension and also, from the field □ visible to others. It

was both the cause and the outcome of a state of disharmony, of imbalance - in this case, a "superyang" imbalance, which marked my adulthood, and certain crucial mechanisms of which remain in action to this day. This "refusal" therefore in no way appears here in a role of symmetry, or even yang-yin complementarity, opposite the "acceptance" (of myself and others) mentioned earlier. The latter, on the contrary, is part of a process of self-awareness, aimed at re-establishing a disturbed harmony. It's an acceptance in the full sense of the word - and not just another flight, in the opposite direction to the flight sometimes called "refusal".

There is, however, a more obvious relationship between "refusal" and "acceptance" than the one probed earlier. It appears when both are taken "in the full sense of the word". They are then **simultaneous** and complementary aspects of the same harmony, of the same fully-assumed attitude (whereas sometimes they were two **consecutive** aspects of a path or progression, passing through a state of imbalance, of disharmony, on the way to renewed equilibrium). From this point of view, there is no such thing as "true" acceptance, which would exclude or close itself off to refusal. And there is no "true" refusal that is not born of acceptance, that is not a tangible manifestation of it; that is not one of the two "faces" - the "yang" face - of the same indivisible thing that comprises two, and whose "yin" or "mother" face is acceptance<sup>84</sup> (\*).

An "acceptance" that excludes refusal is not acceptance, but complacency (to others or to oneself, or both), or complicity or connivance (when it comes to the "acceptance" of others).  
trui). Total acceptance of a being, whether oneself or another, in no way implies approval.

□ unconditional approval of his actions, habits and inclinations. Such unconditionality is in itself an **escape**, a refusal to acknowledge (often eloquent) reality, and by no means an acceptance of it. Far from creating a "force field" conducive to renewal, to reconnecting with a forgotten unity, it reinforces inertia, and helps to keep us in a rut.

A refusal that is not at the same time an opening, that is not also like a hand (or "a perch") extended to others, or like a start that marks a point of rupture and renewal in one's relationship with oneself.

- such a "refusal" is truly a cut, which "cuts" and isolates both the one who refuses, and the one who is refused. It is yet another flight from a reality felt to be unpleasant, even disturbing, fraught with threats to our well-established lives, to our conveniences - a reality we believe we can escape from with a slashing blow: "there's no point"... . And yet **it is!** And our imperative "refusal" in no way prevents things from being what they are, even at the risk of displeasing us. On the contrary, just like the complacency of automatic approval, such a refusal reinforces inertia against creative change, and is like a **verdict**: unacceptable you are, and such you will remain... .

I don't pretend to realize in myself the harmony of fully assumed acceptance and rejection. On the contrary, I know I can't - and I'm not sure I've ever met anyone who could. To achieve it is also to have solved, in my own person, the great enigma of "evil": of iniquity, of lies, of wickedness, of spinelessness, of contempt - and of the suffering of those who are "evil".

<sup>84</sup>(\*) It's interesting to note that this "natural" distribution of yinyang roles in the acceptance-rejection couple (expressed in French by the feminine and masculine genders of each term of the couple) is **reversed** in the image that had spontaneously presented itself to me at the end of the previous day's reflection. That there can be such reversals is hardly surprising - just as in a lover-lover couple, where the love relationship is undefined, there are bound to be moments when the roles in the love game are reversed, giving free rein to the "yang" erotic impulses in the lover and the "yin" erotic impulses in the lover. I talk about the importance of such occasional role reversals in the note "Acceptance (the yang in the yin)" (n° 110, last paragraph of the first part of this note).

stricken and speechless. It is also, surely, to have fully understood the "good" that lies in what an inner awakening so often designates as "evil".

To refuse war, while seeing and accepting that it is everywhere and in everyone; that the very people I love carry it within them and propagate it, just as I myself have taken it up, carried it, propagated it and passed it on. To refuse war, while accepting that it exists, while loving its countless blind soldiers. This and nothing else, surely, is what it also means: to have come out of the war, to have emerged from the conflict - to have stopped propagating war.

#### 18.2.5.4. Yang plays yin - or the role of Master

**Note 118** □(November 4)<sup>85</sup> (\*) The appearance of this "tendency"<sup>86</sup> (\*\*) was in the early '70s, i.e. in the years following my "departure" from the mathematical scene. Under the influence of an environment and friends quite different from those before, there was a drastic turnaround in all "values" of which I was a member. With hindsight, I can describe this shift as a passage from a "superyang" or "patriarchal" value system, to an almost opposite one, with a strong "yin" dominance - a "matriarchal" system. Among the influences that played a part in this reversal were sporadic readings of

Krishnamurti - on this subject, see the note "Krishnamurti - or liberation become hindrance" (n° 41).

If I then allowed these influences to play a part, which were to lead me to such an "ideological" turn, it's undoubtedly (without realizing it at the time) that there was a deep and urgent need for renewal within me, and first and foremost, the need for liberation from the weight of inveterate "superyang" attitudes. This same need had surely already come into play in 1969, when in the midst of intense and fruitful mathematical activity, I suddenly "dropped out" of maths to take an interest in biology<sup>87</sup> (\*\*\*); then the following year, leaving (with no spirit of return) the mathematical scene and even scientific research. There was then a sudden and drastic change of environment and activities, to which I've had occasion to allude several times during "Fatuity and Renewal" (the first part of Harvest and Sowing).

However, it would be inaccurate, or only partially true, to consider these spectacular changes of environment, activities and finally "values", as a "renewal", a "liberation". I've already said I've made this point quite clear in the section entitled "Rencontre avec Claude Chevalley - ou liberté et bons sentiments" (n° 11). In the more penetrating light of the present reflection on yin and yang, I can say that the change that appears to be the most significant of all, that of yang values evacuated (even before myself, let alone examined) in favour of yin values - this change, however, in no way altered the structure (superyang) of the "I", and at most somewhat tempered the attitudes of the "I".

and behaviors that followed. It's true that my understanding of the outside world □s'était consid- But this transformation remained fragmentary, limited almost exclusively to the intellectual level, that of "options". It could not have been otherwise, precisely as long as this transformation was limited to my vision of the "outside world", in which my own person did not figure, or figured only incidentally or superficially, above all through my "social role" and its ambiguities and contradictions. No more than in the past, I had not the slightest suspicion that in **my own person**, there could be ambiguities and contradictions! On the contrary, I was driven by an unshakeable conviction that **my** person was free of all contradictions (even though I was beginning to discern contradictions in others, all around me),

<sup>85</sup>(\*) This note is taken from a footnote to the "Cycle" note (n° 116). See the reference at the beginning of the November 3 notes.

<sup>86</sup>(\*\*) The tendency to value "acceptance" as opposed to "refusal".

<sup>87</sup>(\*\*\*) I first became interested in "molecular biology", under the influence of my biologist friend Mircea Dumitrescu, who introduced me to this fascinating world.

that there was a perfect match between my conscious desires and my conscious knowledge of things on the one hand, and my unconscious (if there was one in my case, if it wasn't a mere carbon copy of my conscious. . . ).

The first crack in this conviction only appeared in the spring of 1974, when I finally understood that something must be wrong with **me** too, and not just with others, as the cause of this inexorable deterioration in my relationships with all those close to me (to which my life then seemed to have been reduced, throughout my adult life). The effects of this salutary crack remain limited, in the absence of a genuine **curiosity** about myself, which would have been a feast to dig into, to look at what was behind it, and to see in the process the collapse of a heavy edifice, made of abracadabra illusions and never examined... .

This stubborn blockage of a natural curiosity surely stemmed above all from the fact that I had never before encountered such curiosity in others, which might have made me suspect that in life as in maths, whenever a problem presents itself, there's plenty to look at and, in the process, learn lots of unexpected and very useful things - in other words, that there was such a thing as **self-discovery**. I had been reading Krishnamurti at the time, and had come to realize that some of the things he said were deep and important. So I tended to take him at face value all the way.

88□

p. 538 I had more or less tacitly adopted the Krishnamurtian(\*) worldview .

I'm talking about, this baggage has indeed acted as a "hindrance" to true liberation, to renewal in the full sense of the word. I explain myself on this subject in the aforementioned note (which I've just reread), where I try to pinpoint the role of the "Teachings" (of Krishnamurti) in my own itinerary. The first "awakening" in the full sense of the word came only two and a half years later, with the discovery of meditation. It was also the discovery of self-discovery; that there is an **unknown thing** that is "me", and that I have the power to penetrate this thing, to know it. This crucial discovery was made at a time when all teaching (whether capitalized or not) had been forgotten. It was also the moment when, for the first time, the "edifice", built of received ideas and "teachings" of all kinds, held together by immense inertia, collapsed - and the moment when an active curiosity appeared, often mischievous, and always caring.

It was after this turning point, with the blossoming within me of a curiosity about myself first and foremost, and about "life" in addition and as a natural fruit, that I was able to see both Krishnamurti and his message with new eyes. With hindsight, I was able to appreciate the richness of the message, and at the same time discern its limitations and shortcomings, as well as certain fundamental contradictions in the Master ("the Teacher", for his disciples and followers). The most serious of these shortcomings and contradictions seems to me to be the one I've just touched on again: the absence of any curiosity in the Master himself. There's nothing in his writings to suggest that, in those distant days, this vision was **born** in a **person** - a person caught, like you and me, in the net of ready-made ideas and contradictions never spotted; that the vision was **decanted from error** in the course of intense, sometimes painful **work**, against the current of

p. 539 immense forces of inertia; that the stages of this work, or the "thresholds" crossed in the course of these labors, were so many unexpected **discoveries**, each overturning a whole set of inveterate ideas'□ **perpetuated** by

<sup>88</sup>(\*) (November 5) The effect in my life of this "adoption" of a vision, becoming a kind of cultural baggage, has remained most limited. My attention was drawn to certain aspects of reality that had previously escaped me entirely, but without any in-depth work of sorting and assimilation, with the power of renewal. If Krishnamurti was important in my itinerary between 1970 and 1976 (between my "departure" from the mathematical scene and the discovery of meditation), it was not so much because of the "baggage" I borrowed from him, as because he had become (unbeknownst to me, of course) a tacit **model**, to which I conformed without wanting to appear to do so - the model, in short, of the "Guru-not-Guru", of the Master who denies himself to be one.



the universal mechanisms of imitation and repetition<sup>89</sup> (\*).

All these things, the child one day knew them, and even experienced them, having lived them intensely. But the Master has forgotten them, and never remembers. Rather than being a child, who passionately discovers and **learns**, and transforms himself in the process, he wanted to be the unchanging **Master** who **knows**, of unchanging infused science, and who devotes his life to spreading his **Teachings**, for the benefit of ordinary mortals. He made himself what his followers and disciples, those who believed in him, wanted him to be: the embodiment of a static, repetitive and therefore reassuring message, the apostle of a new ideology. A **Guru-not-Guru**, in short, like myself (emulating his example, perhaps<sup>90</sup> (\*\*)) once was... .

(November 15) I have named the preceding note (from November 4) "Yang plays the yin - or the Master". As befits a meditation on myself, the main name of the note concerns my own person, referring to a certain "game" I played, however, for a few years after my departure from the scientific world in 1970<sup>91</sup> (\*\*\*). As for the second name "The Master", it can be interpreted indifferently as referring to myself, through a designation of the role or pose I held in this game of "yang playing yin", or to that of Krishnamurti, who served as my tacit model.

In fact, the values that emerge from Krishnamurti's books are almost exclusively yin values. When I first read Krishnamurti (in 1970 or 1971), it was the first time I'd seen such values put forward, and the limits and flaws of my (and "everyone else's", with variations) yang vision of the world identified with penetration. This must have been the reason for the very strong impression this reading of a few chapters made on me. Six or seven years later, I also had the opportunity to read Mrs. Luytens' fine biography of Krishnamurti. It confirmed a

certain impression of his person that already emerges from his books (nobosting the fact that he never appears in them in person). Today I would express it by saying that the basic tone in his temperament is □ strongly yin. p. 540  
What's more, throughout all his writings, we see, like a constant leitmotif, the emphasis on yin qualities, attitudes and values, and the devaluation (explicit or by omission) of yang qualities, attitudes and values.

Krishnamurti's life and teachings thus embody the quite exceptional attitude of "**yin buries yang**", which runs in the opposite direction to the by far more common attitude of "yang buries yin", of which my own life (until my forty-eighth year at least) offers an equally extreme illustration. Krishnamurti's "superyin" options<sup>92</sup> (\*) have the great merit of running counter to the basic values of the surrounding culture. Nevertheless, they seem to me to be no less repressive (of one part of him by another part) than mine have been.

There is, however, a very pronounced and striking "yang" aspect to Krishnamurti's life, which was undoubtedly first imposed on him by the role of figurehead, of (future) "spiritual master", decided upon by his prestigious theosophist tutors when he was still a child. Subsequently, after the great turning point in his life marked by discoveries that radically altered his vision of things (discoveries that became by

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<sup>89</sup>(\*) (November 5) These mechanisms are clearly part of the basic mechanisms of the psyche, in both humans and animals. They pre-exist all conditioning, all learning (such as that of language by the young child, and that of almost all the acts of daily life), which could not be established or carried out without them. They were no less present and efficient in the young Master-to-be than in anyone else.

<sup>90</sup>(\*\*) (November 5) Decidedly, the dubious nuance of this "maybe" is out of place! See the penultimate footnote written today.

<sup>91</sup>(\*\*\*) The moment of the discovery of meditation, in October 1976, marks a sharp decline in this game, which continues as best it can, in a more discreet register, until 1981, when it is finally detected and defused. On this subject, see the section already quoted "Le Guru-pas-Guru - ou le cheval à trois pattes", n° 45.

<sup>92</sup>(\*) These "options" undoubtedly go back to his childhood, and more precisely to his first contacts with his theosophical tutors.

The role of "master", or "guide", was (it seems) entirely internalized, and taken over by the propagation of a doctrine that was personal to him, and not taken over from his theosophist masters. This propagation represents an intense, even exhausting activity. It hardly seems to me to be in keeping with a **balance of** yin and yang, but rather a **constraint** imposed on an eminently contemplative temperament, by an "ego" as strong and invasive in the master as in anyone else. Seen in this light, the present note "Yang plays yin", which deals mainly with Krishnamurti, could also be called "**Yin plays yang**".

Thus, on two different occasions and in two different ways, I have played "games" in my life that are like an **inversion of** the attitudes that dominated the life of the person who, at a certain point in my life, was to become the tacit model of my (equally tacit) brand image, and of certain attitudes.

p. 541 and poses within me. But through styles of expression that are inverses of each other, I now recognize an obvious kinship. One is in the presence of **repression** (unconscious, it goes without saying), general<sup>93</sup> trice of a disruption of the natural balance of yin and yang<sup>93</sup> (\*). The other is to be found in the choice of a **role**, and in the **weight of this role**, its effect of slowing down, even blocking, in a blossoming, in a maturation, in the progression of an understanding or knowledge. This role (or pose) was the same for me as it was for my role model, from whom I may have simply borrowed it as it was. This is the **role of the Master**.

## 18.2.6. Yin and yang mathematics

### 18.2.6.1. (a) The most macho of arts

**Note** 119 (November 5) I've been meaning to talk about yin and yang in mathematics for some time now. The two aspects of yin and yang in mathematical work, or in an approach to mathematics, only became apparent to me in the course of my reflections on yin and yang over the last few weeks. I foresaw that probing this dual aspect to some extent in these notes would be the most natural way of "getting back to my sheep", in these notes which are supposed to be a retrospective on "a mathematician's past".

What's been clear to me from my first thoughts on yin and yang (five years ago) is that "doing maths" is perhaps **the most yang**, the most "masculine" of all human activities known to date. In fact, any entirely intellectual activity, such as scientific research in particular and, more generally, any activity commonly described as "research", is a very strongly yang-dominated activity. I was going to write: "marked by a strong yang imbalance", and this is indeed the case when this activity absorbs almost all a person's energy. This yang predominance (or imbalance) is evidenced by a number of yin-yang couples, where it is clear that it is the yang term above all, not to say exclusively, that is "present" in intellectual work. I'll just mention a few, all of which belong to the same "group" (or "door to the world"), which I call the "vague - precise" group. (NB in this last couple and those that follow, the term yin comes first).

- sensitivity - reason (or intellect)
- instinct - reflection
- intuition - logic
- inspiration - method
- vision - coherence

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<sup>93</sup>(\*) We're certainly in very good company!

- the concrete - the abstract
- the complex - the simple
- the vague - the precise
- dream - reality
- indefinite - definite
- the unexpressed - the expressed
- the informed - the trained
- the infinite - the finite
- the unlimited - the limited
- the whole - the part
- global - local (or parcel-based).

I've just gone through my yin-yang repertoire, and come up with a whole bunch of other pairs that give a sense of the superyang character of pure intellectual activity. I'll just mention the first of all those I thought of earlier: the **mind-body** pair.

Having said this, it seems to me that among the various types of intellectual activity, mathematical work represents the ultimate extreme-yang. This is undoubtedly due above all to its character of extreme abstraction, the fact that it is, to a very large extent, independent of any "support" by sensory experience and reasoned observation of the external world, the world in which we live and in which our bodies move. This extreme abstraction distinguishes mathematics from all other sciences, and mathematical work from all other intellectual work, making it a science or work "of pure reason". In contrast to the experimental and observational sciences, it is also the only science whose results are established by **demonstrations** in the most rigorous sense of the term, following a rigorously codified and in principle infallible **method**, the so-called "**logical**" **method**, to arrive at **certainties** that leave no room for doubt or reservation, or for the possibility of exceptions that would have escaped the cases observed so far. These are the extreme-yang features of mathematical work, and mathematical work alone.

Certainly, these traits had a way of attracting me from childhood, me who had opted for "the head" and for "the head".

the extreme yang!<sup>94</sup> (\*) Particularly after the experience of war and the concentration camp, faced with discriminations and prejudices that seemed to defy even the most rudimentary reason, what fascinated me most was

in mathematical activity (from what little I knew of it □ during my high school years), it was this **pou-**

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**I could see that**, by virtue of a simple demonstration, I could win over even the most reluctant of people, in short, **force** their assent, whether they were well-disposed or not - provided only that they agreed with me to the mathematical "rules of the game". When I first came into contact with school mathematics in 1940 at the Lycée de Mende (where I was able to attend, despite being interned at the Rieucros camp five or six kilometers away), I seemed to know these rules, to feel them instinctively, as if I'd always known them<sup>95</sup> (\*). Surely, I felt them better than the teacher himself, who unconvincingly recited the usual platitudes about the difference between a "postulate" (in this case, Euclid's, the only one he and we had ever heard of... ) and an "axiom", or "**the** demonstration" of the three "cases of equality of triangles", by following the textbook like a First Communion pupil would follow his breviary.

Five years later, seduced by the sudden prestige of atomic physics, it was for studies in

<sup>94</sup>(\*) Except, however, for the military and warlike variant, with parades, uniforms, standing to attention with a bulging torso, and impeccably organized massacres and mass graves. ...

<sup>95</sup>(\*) These first contacts took place shortly after my childish reflections on squaring the circle, referred to in note no. ° 69.

physics that I first enrolled at the University of Montpellier, with the idea of learning about the mysteries of the structure of matter and the nature of energy. But I soon realized that if I wanted to learn about mysteries, I wouldn't be able to do it by taking courses at university, but by working on my own, alone, with or without books. As I didn't have the flair, nor the equipment, to learn physics that way, I put it off until a more propitious time. I then started doing maths, while following a few courses "from afar", none of which could satisfy me, nor bring me anything beyond what I could find in current textbooks. But I still had to pass my exams. ...

### 18.2.6.2. (b) The beautiful

**Note** 120 (November 6) Looking back over yesterday's notes just now, I was able to confirm that I had made-

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tention to avoid a certain confusion between mathematical **work**, a very yang-dominant activity, and "mathematics". It's surely no coincidence that in both French and German, the word for it is feminine, as is "**la science**", which encompasses it, or the even broader term "**knowledge**"<sup>96</sup> (\*), or also "**substance**". For the mathematician in the true sense of the term, by which I mean the one who "does mathematics" (as he would "make love"), there is no ambiguity about the distribution of roles in his relationship to mathematics, to the unknown substance that he therefore knows, that he knows by penetrating it. Mathematics, then, is as much a "woman" as any woman he has ever known or even desired - whose mysterious power he has ever felt, drawing him into her, with a force that is both gentle and unrelenting.

I first became aware of the profound identity between the impulse that drew me to "women", and that which drew me to "mathematics", a few months before my encounter with the stanzas of the Tao Te King that were to set me off on the Eloge de l'Inceste (and, along the way, on my first systematic reflection on the "feminine" and the "masculine", whose Chinese names "yin" and "yang" I didn't yet know). That was six years ago, when I wrote a two-page text entitled "En guise de programme" (by which I meant: for the (C 4) "Initiation à la Recherche" course, of which this text constituted an introduction, or more exactly a declaration of intent about the spirit of this "course". After writing this text, which came to my pen most spontaneously, I was struck by the abundance of images springing from one another, charged with erotic connotations. I was well aware that this was no accident, nor the result of a simple literary intention - that it was an unequivocal sign of a profound kinship between the two passions that had dominated my adult life. Without thinking at the time of deepening the matter through systematic reflection (which would appear only a few months later, when I wrote the Praise), or even (I think) of formulating clearly for myself what was suddenly perceived, I think I can say that in that moment I learned, without fanfare, something important - I had "discovered" something<sup>97</sup> (\*\*), something that had entirely eluded me before.

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Of course, like everyone else, I'd heard of Freud and libido sublimation and all that, but that's beside the point. Even tons of books on psychoanalysis and whatnot can't do the trick. to spare such moments, when all theory, all "baggage" is forgotten, and suddenly some□que thing "clicks!". It's at moments like these that our knowledge of things is renewed. It has nothing to do with

<sup>96</sup>(\*)On the other hand, "**le savoir**" is masculine, and is indeed "l'époux" in the yin-yang couple "la connaissance - le savoir". The German is less clear-cut here, since both "Kennen" and "Wissen" are **neuter** (as substantivized verbs).

<sup>97</sup>(\*\*) It was then a "discovery" in the "yin", "feminine" mode - which takes place by welcoming new knowledge into ourselves, in a state of silent openness to what comes to us. Such moments have been rare in my life, I think. In any case, the moments of discovery I remember are almost all yang, "masculine" in tone.

read books, listen to lectures, i.e.: increase knowledge<sup>98</sup> (\*).

When I think of "mathematics", I certainly don't mean the totality of **knowledge that** can be described as "mathematical", recorded from antiquity to the present day, in publications, preprints or manuscripts and correspondence. Even if you eliminate the repetitions, that's probably a few million pages of compact text; a dozen tons of books perhaps, or even a few thousand thick volumes, enough to fill a spacious library: nothing to get a hard-on about, quite the contrary! Talking about "mathematics" only makes sense in the context of a **vision**, an **understanding** - and these are essentially personal, not collective. There is as much "mathematics" as there are mathematicians, each of whom has a certain personal experience of it, more or less vast or limited, one of the fruits of which is his own understanding, his own vision of "mathematics" (the one he has known), always more or less fragmentary. It's a bit like "**the** woman", which may seem to some as

a mere abstraction, or as a hollow formula and yet has a deep, powerful, irrefutable (for me at least) "reality", of which every woman met or known is an embodiment and represents p. 546

one aspect; and the **same** woman in another's experience undoubtedly represents yet another incarnation, yet another aspect.

My intention here is in no way to confront the difficulty of "integrating" this vast multiplicity of experiences, understandings and visions of "mathematics" into a totality, a unity - and this, moreover, at a time when we are witnessing (it seems to me) a kind of relentless "divergence" in mathematical production, and when no mathematician can flatter himself that he knows, even if only in outline, the totality or essence of what has been accomplished in our science. Rather, my aim was to examine the interplay of yin and yang in mathematical **work**, i.e., in the relationship between the mathematician (or any mathematician, starting with myself) and "mathematics". The thing under scrutiny, then, is "the mathematician" or "that mathematician" (in his or her relationship to mathematics), rather than "mathematics" itself.

### 18.2.6.3. (c) Desire and rigor

**Note 121** (November 7) At the level of our intellectual faculties, of reason, to "know" something is, above all else, to "**understand**" it. And in a work of discovery that takes place in this register of our faculties, the impulse to know that drives the child in us (independently of the motivations of the "I", the "Boss") is the **desire to understand**. This is perhaps the main difference between the drive for intellectual knowledge and its elder sister, the drive for love. This desire to understand pre-exists any "method", scientific or otherwise. The latter is a tool, fashioned by desire to be used to

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<sup>98</sup>(\*) This observation is not contradicted by the fact that it is quite possible, and even probable, that this "awareness" (the passage to the conscious level of something perceived in the unconscious) was facilitated by the existence of the Freudian consensus, of which I had heard without it really making me feel either hot or cold. Knowledge can encourage the emergence of new knowledge, but it's much more common, it seems to me, for it to nip in the bud any hint of emergence - in the same way that ready-made "answers" nip in the bud the emergence of a (good) question. ...

It's remarkable that, while "everyone's heard" a little about the role of the erotic drive in creativity (artistic or scientific, let's say), there was no trace of it in the consensus that prevailed in the circles to which I belonged at one time or another. And yet, there was no shortage of striking facts that could have tipped me off a long time ago. Up until three years ago, periods of intense creativity in my life, and especially periods of inner renewal, were also marked by a powerful influx of erotic energy. Nevertheless, my mathematical activity has never been accompanied by conscious erotic images or associations. But I do remember being somewhat disconcerted, in the '50s, during a work session of the Bourbaki group, by a colleague and friend who mentioned to me, as the most common thing in the world, a peculiarity in his mathematical work: when he had reached the end of a difficult task, he felt an imperious urge to make love (with or without a partner) - and this all the more strongly the more satisfied he was with what he had just done.

its ends: to penetrate the unknown accessible to reason, for the purpose of understanding. Knowledge is born of the desire to know, and therefore of the desire to understand, when it is reason that wants to know. **Method**, as an instrument of desire, is by itself powerless to give birth to knowledge - any more than the forceps of a doctor, or even the expert hands of a midwife, give birth. But they can sometimes usefully assist the birth of the newborn, when the time is ripe and they know how to come at the right moment... .

Many, if not all, high school and university students must feel the rigor of mathematics.

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tic, preached to them by sullen masters, as a kind of a priori entirely external to their humble selves, incomprehensible and arbitrary, dictated by a peremptory and impitoyable God to a Euclid promoted to Grand Censor-in-Chief, with the mission of making countless generations of schoolchildren pale at the task of swallowing Culture with a capital C as best they could. I must have been one of the few not to have passed through this stage in my relationship with school mathematics - to have instinctively sensed, from the very first encounter and within the narrow confines of a sixth-grade math book, the original function and meaning of rigor: that it was a flexible and astonishingly effective instrument in the service of an understanding of those things called "mathematics" - things that reason alone can fully know. This "rigor" is also like the soul and nerve of what I called, in the reflections of the day before yesterday, "the rules of the mathematical game", and what earlier I called "the method". Having only glimpsed them, it was as if I had always known them - as if it were my **own** desire that had delicately, lovingly shaped them, like a key that had the power to open up for me an unknown, mysterious world, whose foreboding richness would prove inexhaustible... . And it was my own desire that continued to refine this tool throughout my high school and university years, before any encounter could lead me to suspect that somewhere there were **fellow human beings** - people who, like me, found pleasure in probing the In-known that this key, apparently unknown to all (including my teachers), alone had the power to open up<sup>99</sup> (\*).

#### 18.2.6.4. (d) The rising sea. . .

**Note** 122 (November 8) It's been three days since I started thinking, in principle, about "yin and yang in mathematics", and I have the impression that it's still going on, even though I'm partially absorbed in other occupations and tasks. By dint of preliminaries, I still haven't come to the point I wanted to make from the start: that in my own <sup>□mathematical</sup> work, it's the **yin** note, "feminine", which dominates!

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I realized this a few weeks ago, on the bangs of the present reflection on yin and yang, and in connection with the "association of ideas aroused by the three-part Funeral Eulogy", which was the starting point for this long digression (see the beginning of the note "Yang buries yin (1) - or the muscle and the gut"). To tell the truth, this association of ideas (to which I'll return later) was more or less based on the intuition that my approach to mathematics was strongly yang-dominant. This intuition was quite natural, since it was my superyang options that had motivated my long-term investment in mathematics. However, this intuition, or more precisely, this idea, was wrong - all it took was

<sup>99</sup>(\*) Still, the little math I'd learned in high school and university might have been enough to make me realize that, in the past at least, there must have been people like me, those in fact who were called "mathematicians". Mr. Soula (one of my professors at university) had told me about Lebesgue, who would have solved the last open problems in mathematics, including measurement theory (which I'd been working on since leaving high school in 1945). But in those years (1945-48), my desire to clarify by my own means the questions I had been asking myself was so exclusive, that it excluded any kind of curiosity about the existence, work or person of mathematicians past or present.

that I take the time to examine it a little and realize that the opposite is true.

For a surprise, it was: a surprise! I didn't mention it "on the spot" in my notes, so as not to cut off the train of thought as I tried to work out how I perceived yin and yang and the philosophy that emerged for me. But here we are at last!

This misconception about the nature of my approach to mathematics must have crept into me, unexamined and as a matter of course, from the time I started paying attention to the yin-yang aspect of things, five or six years ago. It must be a residue of my yang, virile brand image - a residue that has continued to linger there, out of sheer inertia, because I haven't bothered to sweep it up. ...

Perhaps the reader will have the impression that I'm taking him for a ride, given that as recently as three days ago, I explained at length that mathematical work was the most superyang of superyang activities - that in the relationship to mathematics, mathematics was "the woman", and the mathematician as an enterprising lover - and now, all of a sudden, I raise the question of whether, in the case of my humble self, my work or my "approach" is yin or yang, and conclude (as the most natural thing in the world) that it's yin, who'd have thought! If there's any apparent confusion here, it stems from a lack of understanding of this universal fact: that in everything, be it the most yin or the most yang in the world, the dynamics of yin and yang are at play, through the marriage of the two original forces. Thus fire, the most yang of all things and the very symbol of yang, is yin in some of its aspects (this is the "yin in yang")' □ and conversely water, which is the very symbol of yin, is yang in some of its aspects and p. 549 functions (it's the "yang in the yin"). There's no need to develop these two particularly instructive examples here - Surely, the reader intrigued by these observations (which may seem peremptory or sibylline) need only follow the associations of ideas with fire and water, to discover for himself the reality of yin in yang, and yang in yin. And if he's a mathematician, or just familiar with intellectual work (even if he's not a mathematician, or even a scientist), he'll have no trouble discerning the existence of complementary yin and yang modes of approach to any kind of intellectual work, however "yang" it may be in comparison with other, less fragmented types of activity.

A possible starting point would be to go back to the fifteen or so yin-yang couples mentioned at the beginning of the reflection three days ago<sup>100</sup> (\*), when I noted that for each of these couples, it was the predominance of the yang term that took place in intellectual work (especially mathematical work), when we compare such work with other types of activity, such as making love, singing, painting (a picture or a wall, for that matter), gardening and so on. This doesn't alter the fact that, if we stay within a given activity such as doing maths, let's say (all things yang, it's understood), we can distinguish a balance (or sometimes, an imbalance) of either yin or yang traits, varying from one mathematician to another and sometimes, within the same mathematician, from one job to another.

For example, in some works it is the **logical** structure of the theory developed that is emphasized, in others it will be the **intuitive** aspects, there is an imbalance, manifested in the reader or listener by a familiar feeling of **unease** (and sometimes in the author too), when one of these indispensable aspects is grossly neglected, to the "benefit" of the other. (When both are grossly neglected, we throw the book in the garbage can, or leave the room slamming the door!) When each of the two aspects is strongly present, whether explicitly or between the lines, this manifests itself in a very familiar feeling of unease. also familiar with harmony, beauty, balance and satisfaction. So it is, irrespective of

"basic □ tone" that dominates the approach taken, whether that tone is in the "logical", or "intuitive" (or p. 550

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<sup>100</sup>(\*)See "Le plus macho des arts", note n° 119.

also "structure" or "substance"). There's no need to expand on this instructive example, to describe where the problem lies (i.e., to identify the "malaise" referred to earlier), when one or other of the two aspects is neglected; the reader already knows this from his or her own experience! Similar observations are bound to emerge for most of the yin-yang couples considered three days ago. Perhaps even for all of them, even if some are more delicate than the intuition-logic pair, and will undoubtedly require more in-depth examination to be fully understood.

I now had to try and make this fact a little more explicit, or rather "get it across" - that in my way of doing maths, it's my "feminine" yin traits, more than my "masculine" ones, that are running the show. If the idea here was to take this impression to its logical conclusion, testing it in as many ways as possible, the natural thing to do (which did occur to me yesterday) would be to go through the yin-yang pairs I'm aware of, those that might represent (among others) an aspect or mode of apprehension of intellectual work (there must be about fifty of them, I suppose), and see for each of them which of the two "spouses" of the pair predominates in me. I anticipate that in every case, one of the two will, on examination, prove to be predominant.

So, in the intuition-logic pairing, I notice at first glance that both aspects are strongly present in my mathematical work. This is a sign of balance and harmony, among other signs pointing in the same direction. As befits a yin-yang couple, for me (in my work, I mean), the two spouses are truly inseparable - the logical structure of a theory develops step by step and in conjunction with the deepening of an **understanding of the things it deals with**, i.e., also in conjunction with the development of an ever finer and more complete **intuition of them**. Perhaps in my published works, in keeping with the canons of the mathematician's craft, it is the yang aspect, the "structure" or "logic" or "method" aspect, that is most apparent, most obvious to the reader. Yet I'm well aware that what drives and dominates my work, what is its soul and *raison d'être*, are the mental images that are formed in the course of the work to apprehend the reality of mathematical things.

p. 551 ☐ Certainly, I've never skimmed when it comes to identifying, as meticulously as possible, by means of the language. It is in this continual effort to formulate the unformulated, to clarify what is still vague, that the particular dynamic of mathematical work (and perhaps also of all creative intellectual work) may be found. It is in this continual effort to formulate the unformulated, to specify what is still vague, that perhaps lies the particular dynamic of mathematical work (and perhaps also, of all creative intellectual work) - in a continual dialectic between **the** more or less unformulated **image**, and the **language** that gives it form and, in the process, gives rise to new, more or less blurred images that deepen the previous one, and which in turn call for a formulation to give them form. ... . In fact, it's this perpetual effort to use language to define, as precisely and perfectly as possible, what at first appears as an indefinable and unformed "presentiment", as an informal "feeling", as an image shrouded in mist. ... it's this work that, since my childhood and still today, fascinates me the most in the work of mathematical discovery. But if the "effort" here always seems to be on the "language" side, i.e. on the formulation, structure and logic side, which form the key ingredients of the mathematical **method** ; and if (by force of circumstance) **this is where** the visible aspect of a mathematical **text** (or at least its fruits) is to be found, this doesn't mean that (for me at least) this is not where the soul of mathematical understanding lies, nor the driving force or motivation behind mathematical work. I believe that very few of my works have reversed this relationship, that I have developed a "formalism" by allowing myself to be guided solely, or above all, by its internal logic, by desiderata of coherence, or by other aspects of the formalism itself, rather than by a content, by a substance, manifested in images, intuitions of a "geometric" nature.



trique". In any case, all my life I've been unable to read a mathematical text, no matter how trivial or simplistic, when I can't give it "meaning" in terms of my experience of mathematical things, i.e. when it doesn't arouse in me the mental images, the intuitions that would give it life, just as a living flesh of muscles and organs gives life to a body, which without it would be reduced to a skeleton. This inability sets me apart from most of my mathematician colleagues, and (as I've already mentioned) it's what often made it difficult for me to fit into the collective work of the Bourbaki group, particularly during the joint readings, where I was often left behind for hours on end, while everyone else followed along at ease.

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□ I have just followed some associations of ideas about my mathematical work, linked to the "intuition-" couple. p. 552

logical", and to a few neighboring couples who introduced themselves in the wake of this one; the informs  
 - the formed, the undefined - the defined, the informal - the formulated, the vague - the precise, inspiration - method, vision

- coherence. ... It would surely be instructive to review one by one (as I had been thinking of doing) all the possible and imaginable "couples" in relation to intellectual work, and probe for each one in what way and to what extent either of the two spouses is present in my mathematical work, and whether or not one of the two seems to "set the tone", and which one. Over and above a more delicate apprehension of the particular nature of **my** mathematical work, such "piecework" will surely deepen my understanding of the nature of mathematical work in general, and also my apprehension of each of the couples thus reviewed. But such systematic work would obviously take me too far, and would go beyond the reasonable limits of the present reflection. It seems more natural to me to try and find here, and "pass on" if I can, the associations of ideas and images that have convinced me (without having to go any further) that in my mathematical work, it is indeed the "feminine" traits of my being that tend surreptitiously to set the tone, and thus to find a kind of unexpected "revenge" (where one would least have expected it!) for the repression they had to endure in other spheres of my life.

Take, for example, the task of proving a theorem that remains hypothetical (to which, for some, mathematical work would seem to boil down). I see two extreme approaches. One is the **hammer-and-chisel approach**, where the problem at hand is seen as a large, hard, smooth walnut, whose interior needs to be reached, the nourishing flesh protected by the shell. The principle is simple: place the cutting edge of the chisel against the shell, and strike hard. If necessary, repeat the process in several different places, until the shell breaks - and you're happy. This approach is especially tempting when the hull has asperities or protuberances, through which to "take it". In some cases, such "bits" are obvious, in other cases, you have to carefully turn the nut over in all directions, prospecting carefully, before finding a point of attack. The most difficult case is when the shell is perfectly round and uniformly hard. No matter how hard you tap, the chisel's cutting edge slips and barely scratches the surface - you end up getting bored with the task. Sometimes, though, you manage to do it, by dint of muscle and endurance.

□ I could illustrate the second approach, keeping the image of the walnut that needs to be opened. The first p. 553  
 The parable that came to mind earlier is that you dip the nut in an emollient liquid - why not just water? From time to time, you rub it so that it penetrates better, and for the rest, you let time do its work. The shell softens over weeks and months - when the time is ripe, a squeeze of the

just hand it over and the shell opens like a ripe avocado! Or let the nut ripen in the sun and rain, and perhaps also in the winter frosts. When the time is ripe, a delicate sprout emerges from the flesh and pierces the shell, as if playing with itself - or to put it another way, the shell opens up on its own, allowing it to pass through.

The image that came to me a few weeks ago was even different: the unknown thing we're trying to get to know appeared to me as some compact expanse of earth or marl, reluctant to be penetrated. You can go at it with pickaxes or crowbars or even jackhammers: that's the first approach, that of the "chisel" (with or without a hammer). The other is the **sea**. The sea creeps in insensitively and noiselessly, nothing seems to break, nothing moves, the water is so far away you can hardly hear it... . Yet it eventually surrounds the restive substance, which gradually becomes a peninsula, then an island, then an islet, which is eventually submerged in its turn, as if it had finally dissolved into the ocean stretching as far as the eye can see. ...

Readers who are at all familiar with some of my work will have no difficulty in recognizing which of these two modes of approach is "mine" - and I have already had occasion in the first part of *Récoltes et Semailles* to explain myself on this subject, in a somewhat different context<sup>101</sup> (\*). It's the "sea approach", by submersion, absorption, dissolution - the one where, if you're not very attentive, nothing seems to happen at any moment: everything at every moment is so obvious, and above all, so natural, that you'd almost scruple to write it down in black and white, for fear of seeming to combine, instead of tapping a chisel like everyone else... . Yet this is the approach I've instinctively practised since my youth, without ever really having had to learn it.

This was also, in essence, Bourbaki's approach, and my encounter with the Bourbaki group was providential in this respect, by confirming me, by encouraging me to be **giant** in this "style" that was spontaneously mine, and in

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otherwise I risked finding myself more or less alone of my kind<sup>102</sup> (\*). It's true that this situation (being alone of my kind) had long been familiar to me, and didn't bother me that much. As for whether my instinctive approach to mathematical work was going to be "efficient", i.e. above all (according to the criteria in force, and especially for judging a beginner mathematician) whether I was going to be able to solve "open questions" that nobody had yet been able to answer, I couldn't know in advance, and I didn't worry too much about it. My natural inclination was to ask my own questions, rather than trying to solve those that others had asked themselves. In fact, it's mainly through the discovery of new questions and **notions**, new **points of view** and even new "**worlds**", that my mathematical work has proved fruitful, even more so than through the "solutions" I've been able to provide to questions that have already been asked. This very strong impulse to discover the right questions, rather than the answers, and to discover the right notions and statements, much more than the demonstrations, are also strong "yin" traits in my approach to mathematics<sup>103</sup> (\*\*). That's why I'm particularly sensitive when I see the best of what I've contributed to mathematics being treated casually or with disdain by some of my students, i.e. by the very people who benefited from it in the first place.

In any case, it was only in retrospect that I realized that my natural approach

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<sup>101</sup>(\*) See "Dream and demonstration", n° 8.

<sup>102</sup>(\*) In this extreme-yin approach, I tended to go further even than most of my friends in Bourbaki were willing to go. This is probably one of the reasons why I ended up leaving the group towards the end of the '50s.

<sup>103</sup>(\*\*) Incidentally, I have the impression that it's no different for any other research work I do, including what I call "meditation".

of mathematics also "worked" when I felt attracted, inspired by a question that others had asked when, in short, it "clicked" and at the same time the question became "mine". If I tried to draw up a more or less exhaustive list of such cases, I suspect it would be quite long.

long. On the face of it, there are four such situations that seem to me to "stand out" for their scope<sup>104</sup> (\*\*\*).

In all four cases, the hypothetical theorem ended up being proved, for the most part, by the "from the sea rising", submerged and dissolved by some more or less vast theory, going far beyond the results it was initially intended to establish. In fact, I've noticed that the ideas, notions, formulas and methods I developed in these situations (and in others too) have long since entered the realm of "well-known" mathematics, which "everyone" knows and uses to their heart's content, regardless of their origin<sup>105</sup> (\*).

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#### 18.2.6.5. (e) The nine months and the rising sea

**Note** 123 (November 9) There's another point in common with the four cases mentioned yesterday, of open questions that were resolved (or rather, "dissolved") by "the approach of the rising sea". This is the role played by **J.P. Serre** in each of these four cases. It was above all a role of "detonator", to get me "started" on these questions, to use the expressions of a footnote in the Introduction mentioning this role (see "The end of a secret", section 8 of the Introduction). In fact (as I now realize), it appears that Serre played just such a role in the genesis of the main ideas and tasks I developed between 1955 and 1970, i.e. between the time I left functional analysis for geometry, and the time I left the world of mathematics.

I could say, with a slight exaggeration, that between the early fifties and around 1966, in other words for some fifteen years, everything I learned about "geometry" (in a very broad sense, encompassing algebraic or analytic geometry, topology and arithmetic), I learned from Serre' when I didn'tp learnt by myself in my mathematical work. I think it was in 1952, when Serre came to Nancy (where I stayed until 1953), that he began to become a privileged interlocutor for me - and for years, he was even my **only** interlocutor for themes outside functional analysis. I think the first thing he talked to me about was Tor and Ext, which I'd made my own, and yet, look, it's as easy as pie... . ... and the magic of injective and projective resolutions and derived and satellite functors, at a time when the Cartan-Eilenberg "diplodocus" had not yet been published. What attracted me to cohomology at that time were the "A and B theorems" he and Cartan had just developed on Stein's analytic spaces.

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<sup>104</sup>(\*\*\*) The questions I have in mind here are, in chronological order of their solution, the following:

1. Validity of the Riemann-Roch-Hirzebruch formula in any characteristic.
2. Structure of the fundamental "prime to characteristic" group of an algebraic curve over an algebraically closed body of any characteristic.
3. Rationality of *L-functions* of fi ni type schemes over a fi ni body (part of the "Weil conjectures", and an important step towards the proof of these conjectures, completed by Deligne).
4. Semi-stable reduction of abelian varieties over the fraction field of a discrete valuation ring.

<sup>105</sup>(\*) I myself have often practised this carelessness about the origin of the "well-known" I used, except in cases where I knew the origin at first hand, having more or less witnessed the birth, or when I was the father myself. As I have seen many times over the years, and especially in the course of my reflections on the Burial, this elementary delicacy has often been lacking in some of my students or close friends in the mathematical world, even when it has been a question of things they have learned from none other than me, and whose origin they know beyond any possibility of doubt. On this subject, see the reflection in the note "Le Fossoyeur - ou la Congrégation toute entière", n° 97.

or two tête-à-tête with Serre that I sensed all the power, the geometric richness concealed in such simple cohomological statements. At first, they had gone completely over my head, before he told me about them, at a time when I hadn't yet "felt" the geometric substance in the beam cohomology of a space. I was so enchanted that for years I had been intending to work on analytic spaces, as soon as I had completed the work I was still doing on functional analysis, where I was definitely not going to linger! If I didn't really follow through on these intentions, it's because Serre had in the meantime turned to algebraic geometry and written his famous "FAC" foundation article, which made comprehensible and highly seductive what had previously appeared to me to be unbearably boring - so seductive, in fact, that I couldn't resist these charms, and so turned to algebraic geometry, rather than analytic spaces.

If I hadn't held back, I'd have been off, one thing leading to another, making the history of my relationship with Serre, which would also be little more than the history of my mathematical interests, from 1952 to 1970. This is not the place. I would only add that, as is only right and proper, it was from Serre that I was introduced to the four questions mentioned above. Of course, it wasn't a matter of pointing out the precise wording of the question, period. The essential thing was that Serre each time strongly sensed the rich substance behind a statement that, offhand, would probably have neither warmed nor chilled me - and that he managed to "convey" this perception of a rich, tangible, mysterious substance - a perception that is at the same time

**desire** to know this substance, to penetrate it. This is perhaps the most crucial moment of all in a work of discovery, the moment when  $\square$  "it clicks", despite having no idea yet so vague

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This is the real moment of "conception". This is truly the moment of "conception" - the moment from which gestation work can and does take place, given the right circumstances. ...

If Serre played an important role in my work and in my mathematical output, it seems to me that it was more in the appearance of those crucial moments, when the spark passes and obscure, invisible work begins, than in the technical means, unknown to me, that he sometimes provided me with at the right moment, or in the ideas that I borrowed from him at later stages of my work.

One of the reasons, no doubt, for the special role played by Serre, is my reluctance to keep abreast of mathematical news by reading, or even to learn the ABCs of a particular "well-known" theory by reading the books or dissertations that deal with it. As far as possible, I like to get my information from the living word of people who are "in the know". From the time I first came into contact with a mathematical environment (in 1948) until I left in 1970, I was fortunate never to lack a competent and willing interlocutor to keep me abreast of things that might be of interest to me. This may have created a dependency, but I never felt that way<sup>106</sup> (\*). In fact, the question of "dependence" could hardly arise, as long as my interlocutor and I were equally interested in what he was teaching me. Teaching the eager-to-learn is beneficial for both parties, and an opportunity for the "teacher" to learn, as well as for the one being taught.

The "reason" given earlier does explain the importance of interlocutors in my mathematical past, but not the exceptional role played by Serre, which seems to me to far exceed that of all my other "interlocutors" combined! What's certain is that Serre and I complemented each other perfectly. We had similar interests.

<sup>106</sup>(\*) The first and only exception was in 1981, long after my "departure" from the mathematical world. It was when I turned to Deligne as the ideal interlocutor for my Anabelian reflections, after my "Long walk through Galois theory". At the time, I clearly sensed the intention to take advantage of this unique interlocutor situation, to "turn me on my head" - and I ceased all mathematical relations with him, until today. On this episode, see the note "Two turning points", n° 66.

We had many strong points in common, and I sensed in him the same high standards and rigour that I put into my work. Apart from that, we worked in very different "styles". I have the impression that our approaches of the  $\square$  mathematics and our work complemented each other, without really ever encroaching on each other. The genre

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The kind of work I was doing (and the way I was doing it) was very different from Serre's kind of work. He might lay the first foundations of a theory in a fifty-page text, or even spend a year writing a medium-sized book elegantly and concisely expounding some subject that inspired him - but certainly not spend the best part of five years of his life, or even ten years or more, developing at length and in volumes a whole new language (which had been quite dispensable until then), to found a new and fertile approach to algebraic geometry, let's say. He introduced a number of fertile new ideas and notions, without letting himself be drawn into "carrying" them through to the end. On more than one occasion, however, these ideas and notions served as a starting point for a large-scale work that suited me perfectly, and for which there would have been no question of Serre himself taking the plunge.

An association comes irresistibly to me here. In the light of the reflections of the last few days, I see my relationship to mathematical work and to my "works" more as "**maternal**" than "paternal". The moment of conception, crucial though it is, represents for me a tiny portion of the "work" during which the thing in gestation, the "child" to come, grows and develops. This work is very much like that of a pregnant woman's pregnancy, which begins when the child is conceived, and continues for nine long months... . The time it takes to bring what was a foetus to term and to **give birth** - that is, to bring a **child** into the world, a living, **complete** child, not just a head or a torso or a baby skeleton or whatever. The mother's role is obviously very different from that of the father (even the best father in the world. . . ), who does little more than cast a seed, then goes off to do other things.

Clearly, Serre's mathematical work, his approach to mathematics, is strongly yang-dominant, "masculine". His approach to a difficulty would rather be that of the chisel and the hammer, very rarely that of the sea that rises and submerges, or that of water that soaks and dissolves. And he seems content to cast a seed, without much concern for where it will fall, or whether it will trigger conception and labor, or even whether the child that might be born of it will be in his likeness or bear his name.

$\square$  An image can help us apprehend an important aspect of a certain reality, but it does not exhaust reality. Reality is always more complex and richer than any image that tries to express it, and so it is with the images that came to me, without having sought them out, to express two different approaches to mathematics - Serre's and mine. Serre sometimes brought to fruition work that needed breathing space, just as I sometimes sowed ideas, some of which germinated and were brought to fruition by others. No more than in my approach to mathematics, I lack "virility" (whereas the background note is "feminine"), any more than Serre lacks "femininity" in his, balancing his "virile" background note.

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The same cannot be said of a creative approach to an unknown substance, be it mathematical or otherwise: there is no discovery, no knowledge, no renewal, except through the joint and inseparable action of the original yin and yang energies and impulses within a single being. It is in the intimate fusion of the two that lies the **beauty of** a being, or of a work of art - that delicate, elusive quality which signals itself to us through that special feeling of harmony, of satisfaction. This quality is present in all Serre's work that I have known, whether in person or through the texts he has written. I have known few mathematicians where it is so consistently present, and with such force.

### 18.2.6.6. (f) The Yin funeral (yang buries yin (4))

**Note** 124 (November 10) Yesterday's and the day before's reflections are far from exhausting the set of characters strongly marked in my mathematical work, which are of a yin nature. To probe them further, on the lines of the present reflection on yin and yang in mathematics, would also be an excellent opportunity for me to deepen an understanding of the nature of mathematical work in general. This theme of yin and yang in mathematics, which I thought I'd get round to in a day's reflection, and on which I've already spent five consecutive days feeling as if I'd only just begun, has just revealed itself as one of those many seemingly innocuous themes, which become broader and deeper the closer you get to it and the more you enter into it. There's no way I'm going to rush through this juicy theme (or even just "get around to it"), in the middle of a Funeral Ceremony that I've just started.  
don't want to drag this out of proportion!

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□ I'd just like to point out two more of these "strong characters" (without comment, I promise!).  
marked" in my mathematical work that go in the "yin", feminine direction. One is a predilection for the **general**, rather than the particular (which makes a "pair" or "couple" with it). The other trait seems to me to be even stronger, or to put it better, more essential, more neuralgic, and broader too (in the sense that it **contains** the first). If there is one "quest" that has run through my entire life as a mathematician, from the age of seventeen (fresh out of high school) right up to the present day, an incessant quest that has marked all my work (published or unpublished) since its inception, it is that of **unity**, through the infinite multiplicity of mathematical things and possible approaches to these things. Detecting and discovering this unity beyond the often bewildering richness of diversity (without taking anything away from this richness), recognizing common traits beyond differences and dissimilarities, and going right to the root of analogies and resemblances to discover profound kinship - this has been my passion throughout my life. The very differences, the expression of unlimited and elusive diversity, have come to seem like the infinitely branching branches and twigs of the same vast tree, where each and every branch and twig shows me the way to the trunk that is common to them all. Instinctively and by nature, my path has been that of **water**, which always tends to **descend**, the path towards this trunk, towards these roots. And if I liked to linger along the way, it was rarely at the ridge to explore leaves and delicate twigs, but above all at the large branches, the trunk and the master roots, to get to know their texture and feel through the bark the rising flow of nourishing sap.<sup>107</sup> (\*)

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□ To tell the truth, I'm still not sure what to make of this newly discovered fact, how to situate it - that in my approach to mathematics, in my way of "doing math", the basic tone with me is strongly yin, "feminine". This is in line with a certain intuition I've already alluded to - that the

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<sup>107</sup>(\*) I believe I can discern in this quest for unity through diversity, a distinctive trait common to the three passions that have marked my life, including passion for love and meditation. Perhaps even, apart from any passion, it's a **way of** apprehending reality in which I tend to see, pay attention to and give weight to common traits and kinships, rather than differences (without being tempted to overlook the latter). I've noticed that the most common tendency of many is the opposite tendency, the yang tendency, which often goes so far as to ignore or deny deep-rooted kinship. (Superyang tendency, characteristic of our culture. It is often accompanied by a reflex to level out differences, to align everything on the same supposedly "perfect" or "superior" model, for the sake of a false "unity", which is an excessive impoverishment as well as violence). These differences in accent between an interlocutor and myself have often been the cause of dialogues de sourds, in which two parallel monologues are developed that never meet. ...

basic tone of my deepest being, I mean the "child" in me or the "Worker", i.e. that which is creative and beyond conditioning (i.e. beyond the "I", the "Boss") - that this basic tone is also "feminine" rather than masculine. Perhaps I now have everything I need to clarify what this really is, by carefully examining all the signs that point either in one direction or the other<sup>108</sup> (\*), to recognize the significance of each, and what emerges from them as a whole. And if such work doesn't lead to a tangible "yes" or "no", surely it won't have been in vain, in order to get a better grasp of my ignorance, which at the moment is still hazy, unsettled, because I haven't meditated on it. Perhaps I'll do the same, once I've finished my work on Harvest and Sowing, and on the momentum of this one. But then again, this is not the place.

But if I've been led to this reflection on yin and yang, it's been in the course of a reflection in which I've been striving above all to understand certain relationships, between myself and others (among those who were my students, in particular). So it's the possible repercussions of the "new fact" that has just appeared, on my relationship with others and on others' relationship with me, that I'm mainly interested in here. And it's here, too, that I find myself at a loss to "place" or exploit this fact. Perhaps it has something to do with the fact that, apart from me, probably nobody has ever noticed such a thing - not on a conscious level, at least on a formulated level. In any case, I've never received any echoes that I could interpret in this sense, as far as I can remember - any more than I've received any echoes that I could interpret in this sense, as far as I can remember.

that I don't remember any echoes that would give me an image of myself.

"yin," while the character I've camped out since my <sup>infancy</sup> (if not early childhood) has been strongly p. 562  
yang; so much so that even now, this "virile" character seems like a second (?) nature, which continues to dominate my life in many ways.

It's true that the mere fact that a trait in someone (me, in this case) is not perceived on a conscious level doesn't necessarily prevent it from affecting relationships with others, and that this trait is indeed perceived in the mathematical world, among mathematicians who are more or less familiar with my work, and that this perception has "spread" among a much wider mathematical public.

- I have no doubt about it. When I wrote, in "L' Eloge Funèbre (1) - ou les compliments" that "the anonymous pen that has taken care of my eulogy here has gratified me abundantly with what today is delivered to disdain", I wouldn't have known at the time, to identify in a lapidary formula exactly what was "today delivered to disdain" by the mathematical fashion, among the things to which I attach value. But the very next day, through an "association of ideas" that I'll have to come back to<sup>109</sup> (\*), I had sensed (without perhaps having formulated it, and without it yet appearing as clearly as it does now) that "this something" was none other than everything that was recognized (at an often informal level) as being a "yin" or "feminine" way of doing mathematics, "feminine" way of doing mathematics - a way that was tacitly equated with "bombing", "nonsense" (to borrow the compliment paid by my pupil and friend Pierre Deligne, to the text that forms the basis of all his work), "cranking", "ease" and so on.

Certainly, in the Eulogy (delivered by this same friend Pierre), including in the passage where I am quoted in a breath with him<sup>110</sup> (\*\*), compliments were de rigueur! There was no question of nonsense or

<sup>108</sup>(\*) Several of my strongly marked yang traits seem to me to be **acquired** traits, stemming from conditioning, and more precisely, from the superyang brand image dating back to my childhood. These traits include an inordinate investment in action; a strong projection towards the future, i.e. towards the accomplishment of my tasks; a predilection for discovery work that is above all intellectual, and the invasive role of thought; a closed attitude towards anything that doesn't appear to be directly linked to my current tasks, and in particular my inattention to landscapes, seasons and so on. There is, however, one yang trait that seems innate and not acquired, and that's my strong affinity with **fire**, unlike my relationship with water, which is decidedly not "my element". In fact, it seems that my astrological chart is marked by a very strong yang imbalance, with all the signs that enter it being "fire signs", to the exclusion of all water signs.

<sup>109</sup>(\*) See the beginning of the note "Muscle and tripe" (n° 106), where this association is first mentioned.

<sup>110</sup>(\*\*) See note "L'Eloge Funèbre (2) - ou la force et d'auréole", n° 105.

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of bombast, but rather of "**titanic** aspect", "twenty volumes", "cleared **essential problems**", "greater **natural generality**" (sic), school "**nourished** by the **generosity** with which he communicated his ideas", "theories of **legendary depth**", "renewed **foundations**", "**opened** new applications", notions "so **natural** that it's hard for us to imagine the effort they cost" (not to say that they were "easy" - but that's something I took care to make clear myself<sup>111</sup> (\*\*\*)' □ "great attention to terminology" (not to say "bombing"), "**ancestors** of algebraic K-theory", "topos introduced... .. on a **general** base body", "**analogies suggested** by Grothendieck", "**conjectures**... .. still as unapproachable... .", "as Grothendieck had **dreamed** it". ... .

I've underlined the key words in these quotations - they're all words that denote a yin approach to things. The "perfect fingering" in this burial by "well-dosed compliment" consisted in the systematic use of hyperbole with regard to those qualities which, on the one hand, are "delivered to disdain", and on the other hand, are real and are of great value to me; and this **while** passing a complete and radical eraser stroke on the complementary aspects, which today have the exclusivity of honors, the "virile" aspects, as strongly present however in my work as in anyone else's, with very few exceptions.

In fact, it's these "virile" aspects and values, to the exclusion of the slightest "feminine" note, that are highlighted in the text on Pierre Deligne, both by the choice of a few epithets ("proverbial **difficulty**", "**surprising result**", "makes *l-adic* cohomology a **powerful** tool", "**first step**", "**astonishingly useful**", "**speed**", "**penetration**", "**enlightening and constructive reactions** to each question", "**brilliant discoveries**"), than by the detailed enumeration of tangible results (whereas not a single result of mine is mentioned in my portrait-minute, nor is it suggested that these results may have played a role for those of Deligne).

I'm not sorry I took the trouble to make this quick compilation of epithets - the effect is truly striking! If, at the level of structured knowledge, few people have any notion of yin and yang, we have to believe that in the unconscious of my friend Pierre, as in that which served as his scribe, there is a perception of flawless certainty. Here, it is put to good use for a certain cause: to deliver to scorn he who must be delivered to scorn, and to designate a hero to the admiration of the crowd.

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In fact, I doubt that these three short texts I've just looked at had very many readers. But whether or not they did seems to me to be an incidental question. As far as I'm concerned, these texts were addressed, not not to hypothetical potential patrons (after all, it's not my friend Pierre's concern to find patrons to finance his institution), but to the "Congrégation toute □ **entière**", which appeared in the reflections at the time. course of the note of the same name (aka "Le Fossoyeur" n° 97). The message they carry is like a shortcut and masterly of innumerable messages in the same vein, from my friend Pierre and others among those who were my friends or students, and others still perhaps, messages captured and approved by this same Congregation. If there is such a thing as a collective unconscious (and I'd be inclined to think there is now), there's no doubt that in the unconscious of this Congregation (aka the "mathematical community"), as in that of the Grand Officiant at my solemn funeral, there's the same flawless perception of what's yin (pissed off!), and what's yang (hats off!).

And all of a sudden, this funeral appears to me in a new, unexpected light, in which my own person has become an accessory, a **symbol of** what must be "handed over to disdain". These are no longer the funerals of a person, nor of a work, nor even of an inadmissible dissidence, but the funerals of the "mathematical feminine" - and even more profoundly, perhaps, in each of the many participants applauding the Eulogy, **the funerals of the disowned woman who lives within himself**.

<sup>111</sup>(\*\*\*) See note "The trap - or ease and exhaustion" n° 99.



### 18.2.6.7. (g) Supermaman or Superpapa

**Note** 125 (November 11) Exceptionally (for once. . . ), I woke up early this morning, after barely four or five hours' sleep. The unexpected outcome of yesterday's reflection immediately set in motion intense work to "place" and assimilate the new fact that had just emerged, just enough time to warm up a hearty soup and have a snack before going to bed at three o'clock last morning. And early in the morning, this same work dragged me out of sleep, then out of bed... ..

If I speak of an "unexpected" outcome and a "new" fact, I must add that from the very beginning of this interminable "digression" on yin and yang, there had been in me a kind of restrained expectation of a "denouement", or at least the expectation of a "junction" that was to take place with a certain procession, which had assembled in a Funeral Ceremony. It might have seemed that I was drifting further and further away from the scene of the funeral, or even that it had been definitively forgotten - and yet no, it had. always there, as if in mute or filigree. I had never really left them. Their silent presence manifested itself in this discreet and constant expectation, this feeling of tension, of suspense, which carried me towards p . 565

that still nebulous point where the "junction" was finally to be made. I could sense the approximate location of this junction point - it was around a certain "association of ideas" (evoked more than once, but still not formulated) which had been the starting point, the initial motivation for this unforeseen journey through yin and yang and through my life. All in all, this journey was to be like another great cycle, returning (more or less. . . ) to its point of departure; or rather, like a turn in a downward spiral, taking me a notch deeper into the thing probed, "to the very heart" (if my premonition wasn't deceiving me) of this Funeral.

But just as I'm getting ready to "land", and at the turn of a final paragraph of a "note" that's still all about "digression" or even "rehashing", here I am, suddenly in the middle of a funeral ceremony and right in the heart of it, a bit like an extraterrestrial who's catapulted himself right in front of the priest in his chasuble and the congregation of the faithful ; or even worse, like a deceased person, believed dead and (almost) already buried, who suddenly lifts the lid (and wreaths and touching epitaphs come tumbling out!) and there he is in person, in his white shroud and sparkling eyes, like a living imp emerging from his box when you least expect it!

Thus, the culmination of yesterday's reflection was at the same time the denouement of that suspense I spoke of, a very particular suspense that is very familiar to me in "sea-spreading" work, be it mathematical or any other. But in the very wake of this relaxation of a long suspense immediately appeared a **perplexity**. I think it's this perplexity above all that has absorbed me ever since, and which, at ungodly hours, has drawn me from bed to the typewriter. That there should be perplexity is hardly surprising - it happens, more or less, every time a situation suddenly appears in a new light, which at first sight would seem to contradict an old vision. The very first thing to do, then, is to carefully probe these contradictions, to examine to what extent they are real, or only apparent, i.e. expressions of an inertia of the mind which is reluctant to recognize the "same" thing under two different lights. This indispensable work is complete, when all the dissonances have been resolved into a new harmony (albeit a provisional one), into a vision that encompasses and brings together previous partial visions, correcting or adjusting them as necessary, and eliminating those that would turn out to be fundamentally wrong. In such a renewed vision, the "old" which gave rise to it, p . 566 i.e. the more fragmentary visions that come together in it, itself acquires a new meaning<sup>112</sup> (\*).

To return to my "perplexity", here it is. The "denouement" or "new day" consisted of an image

<sup>112</sup>(\*) Compare with the reflections in the two sections "L'Enfant et le bon Dieu" and "Erreur et découverte", n° s 1 and 2.

suddenly appeared - that of the burial with great pomp of the "symbol" of the "mathematical feminine", incarnate in my person, and projection at the same time of the "disowned woman" in each of the participants at the funeral; or to put it another way, it's the image of the symbolic burial of a kind of **Super-Mother**, as an expiatory victim in short and in place of the woman-but-rarely-mother who vegetates in the obscure underground of each of the participants who came to applaud at the funeral. This image seems to contradict **another, opposite**, still hazy, one that had gradually formed in the course of the pre-June reflection (culminating in the note "The Gravedigger - or the entire congregation"): that of a **Super-Father** both admired and feared, both attractive and hated, "massacred" by his children, whose mutilated remains are delivered to derision during the "same" funeral. Placed side-by-side (if that were even necessary), these vividly colored images will seem to border on the zany and delirious, and I can easily imagine the scalp dance that these psychoanalytical fantasies are bound to provoke, assuming there are any readers who have had the breath to follow me this far!

I'll leave them to their dance, which will add an exotic note to this unusual funeral, and in the meantime I'll follow up on an association that arose last night, which I believe will reconcile these two supposedly antagonistic, even irreconcilable, images or facets, and even make them love and marry.

## 18.2.7. The reversal of yin and yang

### 18.2.7.1. (a) The reversal (1) - or the vehement wife

**Note** 126 (November 12) I had thought of pursuing in my notes that association mentioned at the end of yesterday's notes, of a nature to "reconcile" and "make love" the two seemingly antagonistic images that had emerged from my funeral. As I was about to start the notes in this direction, I sensed a reluctance, which I wouldn't want to ignore.

The association concerned my mother's relationship with my father, and the meaning of the destruction of the family that had led to the death of my mother.

p. 567 took place in 1933, by my mother's will overcoming my father's acquiescence (reluctant and embarrassed at first, then eager and total). This crucial episode marked a kind of reversal in the couple ☐ **formed by** my mother and father.

My father had been the ostentatiously adulated heroic embodiment of virile values, while my mother (a strong-willed, domineering character if ever there was one) strutted her stuff in the colors of the subjugated and happy woman, over a daily life marked by constant confrontation. Acquiescence to the children's sacrifice marks the moment when the God and Hero **collapses**, followed by a veritable orgy of "triumphant contempt for the woman who, only the day before, had played the role of swooning adulteress, and who now took the place of the fallen hero, emasculated and happy to be so, reduced to the despised role of "woman", from which she herself was relieved at the same moment... .

The little I've said is so schematic, so quintessential I'm afraid, that it's more likely to give rise to innumerable misunderstandings than to help us understand the hidden motives behind a certain burial. However, I feel that this is not the place to expand on what I have just outlined in a few words. To render with a minimum of finesse a complex reality, blurred at will by the two protagonists, would require a new and lengthy digression, of a magnitude that the context does not justify. I don't feel inclined to delve into it at present, and all the less so as this is a situation that involves others than myself, and where my own responsibility (as co-actor) doesn't really seem engaged. I, and my sister, figure not as actors, but as **instruments** in my mother's hands to bring down the ardently admired and envied Hero, in order to take his place and make him an object of derision.

If this scenario, patiently uncovered five years ago<sup>113</sup> (\*), is the most extreme and violent of its kind that I've ever experienced, I've nevertheless had ample opportunity since then to detect very similar scenarios in other couples. The work I did on my parents' lives helped me open my eyes to things that had previously escaped me entirely. I was stunned at the time, and with good reason! Today, I would tend to believe that, apart from the particular violence of the colors, the kind of antagonistic relationship I uncovered in the couple formed by my parents is more or less typical of couple relationships, or at least extremely common. So the reader who, like me, has ended up using his or her faculties to fathom the hidden springs of couple antagonisms, or of female-male antagonism, won't be otherwise surprised (or even shocked) by what little I've said here.

□ If I try to disregard what is peculiar from one case to another, and draw out the common points to the male-female antagonisms I've been able to see up close and where I've understood something, there's this.

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1. In women, admiration and envy of men, due to the (often overrated) prestige they enjoy as a result of their status (as males, in particular) and the qualities (real or supposed) that justify it.
2. Often there's an element of resentment, even hatred, due to an amalgam (unconscious, as it happens) between the man (lover or husband, for example) and the father. The antagonistic relationship between mother and father is taken up by the daughter, who identifies (more or less completely) with the mother. More direct motives for resentment (towards the father) are often added (his tyrannical attitudes, lack of affection, attention or concern, etc.). Subsequently, these feelings of antagonism (and others), "ready to use", are projected as they are onto the partner (actual or potential), whether or not the latter has "the head for the job".

So when earlier (in 1°) I wrote that women's dispositions (of admiration and envy no- This is only partly true. It seems to me that, more often than not, the **driving force** behind these dispositions **comes from the relationship with the father** (even if the latter is long dead and buried), and that its entry into action depends only to a limited extent on the particular personality of the partner.

3. To compensate for her feelings of inferiority (entirely subjective, needless to say) and veiled antagonism, or even animosity or hatred, there's a fear of exercising power over her partner (even though it's he who, by more or less tacit general consensus, is supposed to hold authority). The woman exercises power by any means at her disposal (the most powerful being her body, and above all, her children,<sup>114</sup> (\*)), and it is almost always hidden. The gratification that accompanies it is therefore mostly unconscious, but no less real and important. The power game often becomes all-consuming, becoming the main content of a woman's life, absorbing almost all her energy, and to which everything else (including the love drive and children) is subordinated, even sacrificed, without hesitation.
4. □ The most extreme case, the most torn, is when admiration and envy towards the male, which it is to dominate while appearing to submit to him, is accompanied by contempt, even disgust and hatred, for what is feminine - for her own condition as a woman. Yet it is only by playing on her "femininity" that she can hope to subdue the man, or at least maneuver him to her will.

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<sup>113</sup>(\*) On this subject, see the two notes "La surface et la profondeur" and "Eloge de L'écriture", n°s 101 and 102.

<sup>114</sup>(\*) The main common "means", however, is not mentioned here, being of a more subtle nature, difficult to describe in a few words. It consists of a certain all-purpose "tactic", examined in the later section "The claw in the velvet" (notes n°s 137-140) of the reflection on yin and yang.

willingly! So, to satisfy her strongest egotic impulse, that of "making her partner work" (or even subduing him, or breaking him. . . ), she finds herself forced into a role that is hated, felt as contemptible, as unworthy of her. It is in this extreme case of refusal of her own condition and nature, that of a superyang and anti-yin option, that she will seek an illusory escape from the conflict she carries within her, using all her strength to achieve a **role reversal**: herself substituting for the man, the hero and master, once admired and envied and now fallen, reduced himself to the role she had long worn as an abject livery, the despised role from which she would finally be delivered... .

The sketch I've just made is also schematic, capable at best of **evoking** a certain reality for those who have already perceived it for themselves here and there, without perhaps having yet tried to define it as best I could through a summary description like this one. If I wanted to give it some relief, I should at least try to specify the different **levels** (almost all unconscious) on which this set of mutually antagonistic feelings and desires are played out. Moreover, in this tangle of inexorable egotic mechanisms, from which the love drive seems rigorously absent, I'd also have to try and find out whether and to what extent it contributes to the never-ending round-about (like the force of the wind, perhaps, captured by the wings of an ingenious mill to make a heavy millstone turn forever... . ), and to what extent it also happens that the cogs sometimes stop and fall silent, to give free rein to **something else**.

And finally, I've left out entirely what's going on inside him, the "partner" or protagonist, as if he existed only in relation to her, as the **object of** attraction and repulsion, admiration and envy of the woman facing him. This is undoubtedly one of the reasons for this omission: in this merry-go-round of the couple, who play the active role, investing themselves in it wholeheartedly, often finding their true *raison d'être* in it (if not their own).

p. 570 better), while **he** sees nothing but fire, busy as he is elsewhere and □ moreover as naive as any<sup>115</sup> (\*), reacting one after the other without trying to understand, and (what's more) without understanding indeed, not even (it seems to me) on an unconscious level. At least, that's the impression I've always had, ever since I started paying attention to the couple's merry-go-round! But it's also true that I'm much less familiar with the man's role, since I've only been able to observe it up close in the case of my modest self, whereas I've had the opportunity more than once, on the other hand, to get to know the woman's role from the very front.

In any case, even if I were to take great care, over ten pages or a whole volume, to flesh out my rather schematic description, it would still be wasted effort for a reader who has not yet "used his faculties" in this area, and who has never seen or smelled anything of the kind. As for the reader who is a little "in the know", surely the little I've said about it, notwithstanding clumsiness and obscurity, will be enough to put him back in the bath of things he had already perceived for himself, and to arouse in him images and associations no less rich than those that were present in the background, at the time of writing my lapidary description.

It doesn't take much more, it seems to me, to see the "missing link" between antagonism to the "Superpère" (finding its expression in the symbolic burial of the latter), and contempt, rejection of the "feminine", and more profoundly, the denial of "the woman" in oneself (which may find expression in the symbolic "Burial" of a "Supermère", under a plethora of dithyrambic double epithets).

<sup>115</sup>(\*) (November 23) Of course, if the merry-go-round is spinning, it's because (however "naive" he may be) **he's** enjoying it just as much as she is - and she's making it her job to see to it! It seems to me that the two main "hooks" by which she "holds" him (and by which she too is held. . . ) are vanity, and a need for emotional and love security, guaranteed by a stable partner. And then there are the children...

use. . )<sup>116</sup> (\*\*).

### 18.2.7.2. Retrospective (1) or the three parts of a picture

**Note** 127 □(November 13) The time seems ripe now for me to try to trace in a few largep lines a vision both sharper and more nuanced of the Burial, which (as I wrote the day before yesterday) "encompasses and reunites the earlier partial visions, correcting or adjusting them as necessary. . . ". I see three such earlier visions, which we must recognize as partial aspects of a single **everything**.

The first aspect to emerge, the most obvious and also the most simplistic, is the "**reprisal for dissent**" aspect, which was the aspect most emphasized in the note "Le Fossoyeur - ou la Congrégation tout entière" (cf. note 97 p.) - the last note before the illness episode. It's also the note, among those from processions I to X (those before the incident), that seems to me to capture the **collective motivations** most deeply, those of "Le Fossoyeur" alias "La Congrégation (quasiment) tout entière".

The second aspect, which I might call "**massacre** (more than just symbolic) and **burial** (symbolic) of the **Superfather**", doesn't appear. Perhaps this is because this component of the funeral's motivations doesn't really concern "the entire congregation", which was the focus of my attention at the time, but mainly (if not exclusively) "those who were my pupils". It's true that, even leaving aside their undisputed leader, my friend Pierre, they played a leading role in the implementation of the Burial, which would not have been possible without the contribution of

of some, and without the acquiescence of all. (On this subject, see the note on "Silence" (see note no.° 84 p.). It is It is through them, above all, that the "Superpère" aspect seems to me crucial to an understanding of Burial.

The first aspect, the "retaliation" aspect, came to my attention from the time of Yves Lade- gaillerie's setbacks in 1976<sup>117</sup> (\*); since then, I've tended to forget this aspect, but periodically it has come back to my attention, over the following years. It has finally gone beyond the formless stage of being "felt" without more, and by becoming the substance of a clear and nuanced understanding, in the quoted note on the "Gravedigger".

The second aspect'□or "Superpère" aspect, only began to appear in the course of reflection in Ré- coltes et Semailles<sup>118</sup> (\*), and first<sup>119</sup> (\*\*) without any connection with the Burial as such, which I was to discover only in the following months. This aspect gradually emerges from the mists throughout the reflection on the Burial, finally taking striking form with the notes "Le massacre", "La dépouille. . . ", ". . and the body" (87, 88, 89). These notes are dated May 12, 16 and 17, the "Gravedigger" note is dated May 24; the illness episode appears on June 10, and puts an end for over three months to the continuation of the notes, which resume on September 22. At the very least, it's likely that if this episode had not occurred, at a time when I was about to take stock of the whole and draw a final line, my vision of the Burial would have stopped at that which had emerged in the two weeks between May 12 and 24 - a vision in "two parts", each of which remained in its own corner, without the idea ever occurring to me to try to put them together.

Yet there was a vague feeling, like a barely perceptible mist, that the final word was still to come.

<sup>116</sup>(\*\*) (November 23) This was so hasty that a week later, this conclusion and this "missing link" were entirely forgotten! For the "missing step" in arriving at a more convincing "missing link", see yesterday's note "The reversal of yin and yang (2) - or revolt" (n° 132).

<sup>117</sup>(\*) see the two notes "On n'arrête pas le progrès!" and "Cercueil 2: les découpages tronçonnées", n° s 50 and 94.

<sup>118</sup>(\*) (November 29) To tell the truth, this aspect had already been present in the form of an epidermal intuition for a number of years in my relations with Deligne, but without me ever dwelling on it before the Harvest and Sowing reflection.

<sup>119</sup>(\*\*) In the two sections "The Enemy Father (1) (2)", n° s 29, 30.

not really grasped; the feeling of one "groping in the shadows" (the expression must have appeared once or twice in the course of my notes on Burial). The Gravedigger's final note must have had the effect of a light gust of wind in the fog, which can give the illusion that the fog has dissipated, when in fact it has only shifted a tad. Or to put it another way: the aspect taken up in this note appeared there in such clarity and with such relief, that the impression (by no means illusory) of a tangible, penetrating understanding of this aspect, and the feeling of satisfaction that accompanied it (a feeling, that this impression and this feeling created a kind of euphoria, of one who feels ready to reach the goal, and made me more or less forget the other, nonetheless significant, aspect, the "Superpère" aspect, which had remained "on the back burner"!

The third installment appeared just three days ago (five months to the day after the appearance of the unfortunate episode-illness). It's □ the (symbolic) **Funeral and** (very real) **Burial** aspect of the

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"This **"feminine"** is visualized as a kind of **"Supermom"**, herself embodied in my modest person! This aspect came to light at the end of a long and entirely unforeseen "digression" on yin and yang, in which an effort had finally been made to express intelligibly a certain "association of ideas" stemming from a certain "Funeral Eulogy", which was supposed to close the funeral ceremony. This famous "association" or "intuition" (to which I first alluded at the very beginning of the note "Le muscle et la tripe" (yang enterre yin (1)), ) has still not been made explicit - but everything is ready to go, and I've been promising for a while that I'll get to it!

Along the way, a whole host of facts and intuitions have come to light, some of them new and unexpected for me, and all of them helping me to reconnect with important aspects of my life, and of existence in general. One of these facts - that the "basic tonality" of my mathematical work is "feminine" - seems to contradict one of the intuitions at the root of this association, which is still waiting for its time: the intuition that as a mathematician (as for everything else), I was a very **yang** character; an intuition therefore linked to the "Superpère" aspect of Burial. And this same fact, which seems to contradict this association (from which all the thinking on yin and yang stems!) also brings up in a flash the third aspect that had eluded me until then, the "Supermother" aspect. At the same time (at the end of the endings), the link is made with a "Burial" that seemed to have been forgotten for nearly a hundred pages!

For "rising tide", it's rising tide - let's hope that the end result, I mean this promised "vision" that I'm about to bring out of limbo, will be equal to the means, namely a whole sea of philosophical-Freudian digressions on yin and yang... . The tide has turned (with the

note-coup-d'envoi "Le muscle et la tripe") on October 2, the crucial "new fact" made its appearance in the following days<sup>120</sup> (\*), while □ any day now I'm getting ready to finally put this famous

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"association" (which appeared five months earlier, on May 12 or 13, after reflection on the note "L' Eloge Funèbre

(1) - or compliments", on the same day as the crucial note "The massacre"). But this fact wasn't "revealed" in the notes until five days ago, on November 8, after three preliminary notes on yin and yang in maths (written over the previous three days). This is the note "The rising sea... ." (122). The very next day, November 10, with the note "Les obsèques du yin (yang enterre yin (4))" (124)), the "Supermère" makes her appearance (but the word is only used in the following day's note, "Supermaman ou Superpapa?" (125)). And so we have the "third part" of L'Enterrement!

It's without any deliberate intention that I've committed myself, on the spur of the moment, to this retrospective of

<sup>120</sup>(\*) I seem to remember that as early as the day after tomorrow, in the note "Innocence (the marriage of yin and yang)" (n° 107), the fact in question had appeared, and was part of the "various signs" referred to in that note (without any further details about them), which "have made me suspect more than once that... .. it is the 'feminine' qualities that dominate in my being... .".

the reflection on Burial, from the perspective of the successive appearance of its three main aspects (as I see things now). Such occasional retrospectives, in the course of a long-term meditation, have each time proved most useful, giving an overall view of the process of reflection, and at the same time a fresh perspective on some of these main "résultats"<sup>121</sup> (\*). Perhaps the most striking thing for the hypothetical reader of this retrospective is that I have makes the detour through such a long digression, rather than get straight to this famous "association" (still to come) and that we don't talk about it anymore, to finally get to the famous "trait final," under the Burial; p. 575 which I was in such a hurry to draw in the note "L'Eloge Funèbre (2)" of September 29, when I was just getting back into the harness of the reflection left in abeyance in June! It was with this in mind, moreover, that I began the following note three days later, "Le muscle et la tripe", which begins with an allusion to this association, without giving any details about it.

If I didn't do it then, and put it off from day to day and week to week for a month and ten days already, it's by no means because of a deliberate intention, which would have appeared at some point. If I try to fathom the cause, I'd say that I must have felt instinctively, without even having to tell myself, that at the point I'd reached then, writing the association in question outright would have made no sense; that it would have been like a mere "statement", purely formal or verbal, while the rich substance covered by words that would have come to me by a pure effect of memorization, would remain ignored, unperceived. If you're a mathematician (or a scientist, if you're not), you're bound to have experienced such a situation many times, and the discomfort it arouses, when confronted with a statement that you can easily see is perfectly precise, and where, moreover, you know the meaning of each of the terms used as best you can, and yet feel that the "meaning" and substance totally escape you. The situation is perhaps even more frequent with non-technical texts, which nevertheless express a tangible substance, strongly perceived by the author; with the difference, however, that it is much rarer for the reader to realize with any clarity that the meaning of what he is reading escapes him. In this case, there was even more - for **myself** too, who for months hadn't been "in the bath" of the Funeral Eulogy and the associations associated with it, and who for years hadn't really "plunged" into the reality of yin and yang (while brushing up against it at every step. . . ).

- Even for **me, anything** I might have written to "say" this association would have been a verbal thing, not really felt or perceived. Resolving to do so, or to put it another way, forcing myself to do so, would have been a purely formal way of discharging a kind of obligation, out of a sense of conscience, "completing" in short, a penum, while taking care to "give good weight", not to lose along the way such an "association" which (as I well remembered!) had been juicy and steaming, and which had long since had time to cool down.

and molder in a corner of memory!

□ If what I remembered were indeed to serve the deepening of an understanding that remained

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I could not, however, do without these hundred pages of "digressions". They form the most profound part of the whole reflection pursued throughout Récoltes et

<sup>121</sup>(\*) This kind of retrospective seems to me to be very rare in mathematical work, and I've only been practising it myself since the writing of "Pursuing Stacks" (started in spring last year). A common working practice, on the other hand, and one which has a similar effect, from the point of view of a "fresh perspective" on the ideas and results of a mathematical work in progress, is to take up "ab ovo" all the notions and statements of the theory one is developing, in the order that presents itself as the most natural, at the point at which one's understanding is at that moment. Often such work, which may seem purely routine, leads to a substantial deepening of understanding, for example by bringing to light, through the demands of internal coherence of the new ordering, equally "natural" notions, properties, relations etc., which had not been seen previously. Sometimes, too, by revealing the fortuitous or artificial nature of certain hypotheses, or the narrowness of an entire initial context, the work of "restatement" leads to an unsuspected broadening of the initial purpose, giving the theory initially developed a new dimension and scope.

Sowing. I cannot yet predict whether the vision of Burial that I am about to unravel in their wake will leave me with a sense of complete satisfaction, or whether there will remain obscure corners or dissonances, which I may give up trying to illuminate or resolve, at least for the time being, or in Récoltes et Semailles. But in any case, just as in my mathematical work, I know that each of these hundred pages, like each of the six hundred (give or take a few) of the text of Récoltes et Semailles now being written, has its own unique place and message and function, and that I could not have done without any of them (whether or not there are readers to follow me this far!). While the goal was far away (if not totally forgotten. . . ), each of these pages brought me its own harvest, which it alone could bring me.

### 18.2.7.3. Retrospective (2) or the knot

**Note** 127 (November 17) I've just gone through four rather difficult days, with a lot of turmoil around me. There could be no question of continuing in the same vein, and my work on the notes has been limited to a little housekeeping: rereading the part of the text to be given over to typesetting, and correcting the part that has been done. Between the "first draft" of the text for each note, reread before starting on the next note, and the final net text, ready for duplication, I make at least three careful readings, making adjustments of expression during the first two at least. I'm going to get to know the text of Récoltes et Semailles pretty well! But above all, I'm doing what's necessary to make sure that the text that will be entrusted to me for duplication will be the best I have to offer, including in its form. With the exception of one of the Funeral notes, for all the sections and notes of Récoltes et Semailles that I've written and reread, the last reading left me with a feeling of complete satisfaction. I felt that each time I had managed to say what I had to say as clearly and as nuanced as I was capable of doing, without hiding anything that was clear, understood, known to me at the time of writing, nor anything that remained obscure, blurred, misunderstood or even entirely mysterious, unknown... .

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□ The only exception is the note "The half and the whole - or the crack" of October 17, from which the "thread"

of the meditation split into two, on the two themes I've called (subtitled in the rest of the notes "the key to yin and yang") "Our Mother Death" and "Refusal and Acceptance"<sup>122</sup> (\*). This is the last part of this note, the two or three pages where I speak of division within the person as the ultimate root of division and conflict in the couple, in the family and in human society. This is an intuition that first appeared to me in the early years after my "departure" from the world of science, and which has developed, confirmed and deepened over the years, right up to the present day. It has become so "self-evident" to me (without my ever having bothered to examine it carefully and in all its facets), that it has crept into my thinking somewhat as a matter of course, without any effort to present it in such a way as to make this "self-evidence" even slightly apparent. But if reading these pages leaves me with an impression of vagueness and dissatisfaction, it's surely not simply a question of clumsy "presentation". Rather, I feel I've jumped in with both feet over a substantial reflection on this complex theme, a reflection for which I feel I have all the elements in hand to make it, but which isn't done for all that! In the note of October 25th ("Paradise Lost" (116)), which is directly linked to the note of the 17th (to develop the theme of "Refusal and Acceptance" from there), I first try as best I can to "make up for" the gaps I had noticed in the "Refusal and Acceptance" note.

<sup>122</sup>(\*) The need to group the notes that make up the "digression" on yin and yang under subheadings was felt only a few days ago. This also led me to readjust the names I'd given to these notes, which are now quoted in some places under names that are a little different from their original names (but with the right number, nonetheless). At the same time, the name of this set of notes, "The key to yin and yang", also appeared.



earlier note - but without saying much more than simply this: that as for a possible "voyage of discovery into conflict", "that's not the direction I want to go in right now", too bad, that's for another time!

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□ In the previous note of four days ago, I had surveyed three aspects, or "strands", of the p. 578 table. Burial, that have emerged so far. Afterwards, I remembered that at two points already during the reflection on Burial, I had felt, and written, that I was touching the "knot" of the conflict. It was in the notes "Le noeud" and "L' Eloge Funèbre (2) - ou la force et l'auréole" (, ). These notes echoed reflections (seemingly "quite general") in an early section of Récoltes et Semailles,

"Infallibility (of others) and contempt (of self)" (section n° 4). It's **self-contempt**, ignorance of one's own It is also the source of **contempt for others**, of the eternal reflex-compensation of "proving" one's worth by placing oneself above others, using (for example) the derisory power to demean or crush, or simply to cause pain or harm.

As I wrote this note, I certainly had no shortage of examples. The one most vivid in my mind at the time was Pierre Deligne, whom I'd seen use his power to discourage and even humiliate in ways that often seemed inexplicable. It was only two months after writing this note that I began to discover "L'Enterrement in all its splendor", as witnessed by the notes of April 19 ("Souvenir d'un rêve - ou la naissance des motifs", and "L'Enterrement - ou le Nouveau père" (51) (52)). Gradually, too, I discovered my friend Pierre's role as Grand Officiant at my funeral and burial. Most of the pre-June notes on the funeral (Cortèges I to X) focus on him. It's also the one about which I have incomparably richer and more personal material than for any of the other numerous participants. So, on the two occasions when I had the feeling of "touching the heart of the conflict", it was he again, the only one with whom regular contact has been maintained to this very day, who was at the center of my attention.

#### 18.2.7.4. (d) Parents - or the heart of the conflict

**Note 128** ( November 18) Twelve hours of sleep last night - I needed it, after several rather short nights! I feel that I've regained some of the energy that was beginning to fray a little - I'm more energized than yesterday, to pick up the famous "thread" where I left off.

□ In the two moments I was talking about yesterday there was a kind of "flash" in me so clear and strong, that the idea p. 579 that it wasn't something purely subjective, the product (let's say) of a simple, deliberate intention to apply some psychological "theory" that was close to my heart - that it was, in short, the "butterfly" providentially caught in his net by the butterfly hunter<sup>123</sup> (\*)! To doubt such signs, whether in meditation or maths or elsewhere, would simply be to abdicate my power to know and discover. I'm lucky enough to know what that power is, and if there's one thing I have every confidence in, it's in him.

I could see in this "flash", in what it taught me, a fourth "part" of the picture of

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<sup>123</sup>(\*) For this image, see the note "L'enfant et la mer - ou foi et doute" n° 103.

l'Enterrement, which would be added to the other three (reviewed in the November 13 note). But right away I see it as intimately linked to the two aspects "Superpère" and "Supermère" - and this obvious link goes far beyond the person of my friend. This misunderstanding of the "power to know and create" within us, which I mentioned again yesterday, is nothing other than a misunderstanding of our fundamental unity, the fruit of the marriage in our being of "yin" and "yang", "feminine" and "masculine" qualities, energies and forces. For what is "man" in us, on its own, does not make us capable of knowing or creating, any more than what is "woman" in us, on its own, gives us this power. It is not a factitious and derisory **half** of our being that has the power to know and create, but it is the **whole**, the **totality of** our being, that has this power. It has it, not as the outcome of a quest, a long journey, a becoming, which we would go through in a state of temporary powerlessness that would gradually amass "power" along the way; but this power is ours by nature, we have received it as a free gift, from the day we were born<sup>124</sup> (\*\*).

p. 580 And this "self-contempt", or "lack of self-knowledge", is nothing other than the **refusal** of this gift, the refusal of this fundamental unity, and of the power that is its inseparable companion. Or rather, it is like an inseparable shadow of this refusal, it is the **knowledge of an impotence**<sup>125</sup> (\*), introduced by this refusal; a shy, blurred, unassumed knowledge, which takes great care to stop at the known (which is very poorly known. . . ), afraid as it is to plunge deeper, to become aware of the hidden unknown power, and blocked by this deliberate, cultivated powerlessness.

The most common form this denial of our unity takes, in our superyang society, is the burial day after day, hour after hour of the "yin", the "feminine" in us. This is precisely what the "supermother part" was all about, aka "Funeral and burial of the feminine" and, more specifically and **above all**, of the feminine within **ourselves**.

But I also feel that there is a direct and profound link between self-contempt and the "Superpère component", aka "massacre and burial of the father". It's this strongly presaged link that I'd now like to try and identify. To put it another way, there must be a direct and profound link between the division within us and our antagonism to the father.

p. 581 It goes without saying that this "antagonism" finds occasion to express itself as much in relation to the bio-logical father, as in relation to the person who took his place in childhood, or in relation to any other person who, at one time or another and for one reason or another, takes the place of a more or less symbolic "spare father", onto whom the original antagonistic drives are projected. My aim, then, is to identify the root cause of these antagonistic drives and attitudes, so common that we might sometimes be tempted to regard them as universal; a cause that goes deeper than a simple set of concrete grievances, often all the admittedly tangible grievances one might have against the author of one's days. More than once, I've found that these grievances are often more in the nature of a plausible and welcome rationalization, to an antagonism whose real root, the cause of its vehemence and tenacity, lies elsewhere.

I could formulate the intuition I'm trying to pin down in another way, in the form in which it spontaneously presents itself to me: it's that I have the intimate conviction that in the one who is "**one**", undivided, in the one who accepts himself in the totality of his being - in him, the conflict with the father, or with the mother, is resolved. He is **autonomous**, "**free**" from either parent. The umbilical cord that continues to link us to our parents,

<sup>124</sup>(\*\*) And probably even long before we were born. ...

<sup>125</sup>(\*) As I explain a line further on, this knowledge is "blurred", in its essential content it remains unconscious. Often, however, a small piece of it emerges (like the tip of an iceberg whose base remains carefully submerged... ), in a sort of **profession of faith of impotence**, which more than once has left me speechless. They are made in the tone of a peremptory and unanswerable **statement**, behind which one senses a kind of vehement, fierce closure - as if this impotence, which is thus claimed as an intangible and sacred "fact", were the most precious asset, which one would not relinquish at any price. . .

long after childhood and adolescence (and more often than not, throughout adulthood and into death) - this link is broken. The moorings are broken, which until recently held us back from truly setting out on **our own journey** to discover our Mother World<sup>126</sup> (\*).

This intimate conviction is not just wishful thinking, it's not the projection of a wish. (renamed "conviction" for the occasion). Its origins lie in my own experience, first and foremost. place in □ what I've seen in my relationship with my own parents. I'm thinking here of the transformation This was marked by the "awakening of the yin" in me, followed by the discovery of meditation in the months that followed, and finally by the "reunion" with my childhood two days after<sup>127</sup> (\*). I realize that this turning point was marked by an immediate **autonomy**, in contrast to an earlier dependence on received and adopted ideas. The most profound of all these dependencies was my dependency on my parents, whose values and options had shaped mine and my own vision of the world, and whose Epinal image of themselves, of the couple they formed and of their relationship with their children I had also taken on board "en bloc" and as it was, without any change whatsoever. Since childhood, I'd been "operating" on this set of values, options and images, which were in no way the fruit of my own life experience and assimilation work, but simply "baggage". Much of this baggage was made up of clichés and self-indulgent illusions, which I had "trusted" from my parents, and which very often in my life replaced a direct and living perception, a creative perception of the things around me.

It's true that this "autonomy" I'm talking about appeared immediately with the discovery of the power of meditation. It was total (I think) in everything I took care to examine. That doesn't mean that many preconceived ideas, particularly and above all those that came to me from my parents, initially remained in place through sheer inertia, because they hadn't yet been examined. There were so many things to look at, there was no question of looking at everything at once! Not to mention the fact that, after a few months of intense work, I allowed myself to be distracted by "life going on" - especially love affairs, as you can imagine<sup>128</sup> (\*\*). During

<sup>126</sup>(\*) It's a strange thing that in French, the notes "le monde", "l'univers" and "le cosmos" are all masculine. The equivalent words in German, "die Welt", "das Ail", "der Kosmos", are of the three genders feminine, neuter (which is often a kind of "super-feminine" in German), and masculine. This seems to me to correspond better to the nature of the things designated by these terms. When we speak of the "cosmos", the connotation (apart from space cells and extraterrestrials, of recent invention) is that of an **order**, governed by laws - ideas which correspond well to the masculine (in which the two languages concur). On the other hand, "the world" and "the universe" suggest the idea of a **whole** of which we and everything else are a **part**; of something, moreover, that it's up to us to **discover**, to **penetrate**, to **know**. In these aspects, which seem essential to me, these two terms designate things that are "yin", "feminine" in nature, and particularly so in relation to us. I'd be hard-pressed to understand why the French language nevertheless assigns them the masculine gender.

In this connection, I'd like to point out another strange (?) "anomaly", this time apparently in German, where "le soleil" and "la lune" are called "die Sonne", "der Mond". Their genders are reversed from those used in French, which would seem to be the most "natural". Thus, the sun is immediately associated with the idea of heat and fire, which are typically yang in nature. Perhaps this "anomaly" is common in Nordic languages, because in cold countries, where the sun's heat is never felt as torrid, burning, but is expected as a blessing, a source of life, the sun is felt (along with the earth) as a kind of nurturing mother, lavishing creatures with the warmth they "feed on" as much as with the nourishment they receive from the earth... ..

<sup>127</sup>(\*) I talk about these crucial episodes in my life in the notes "The reunion (the awakening of yin (1))" and "Acceptance (the awakening of yin (2))", n°s 109 and 110, and in the section "Desire and meditation", n° 36.

<sup>128</sup>(\*\*) My love life in the years following my discovery of meditation in 1976 was more intense, and also more eventful, than at any other time in my life. It surely represented a dispersion, a diversion from the initial impetus of meditation, which was not to be resumed (with its due breadth) until August 1979, with the long-term meditation on my parents' lives. (For this, see the notes "La surface et la profondeur" and "Eloge de l'écriture", n°s 101 and 102.) Yet, with hindsight, I realize that I could not yet "spare" this dispersion - a certain passion, a certain hunger within me had to be consumed, and along the way, I had to continue to learn, through those I loved, what I had learned only imperfectly in my past life. At this point, I had my doubts.

For the next two years or so, my meditations were confined to a few occasional reflections of very limited scope, when I found myself confronted with some acute conflict situation, and urgently felt the need to get to the bottom of it. It was only after August 1979 (almost three years after my discovery of meditation) that I began the "great cleansing" of the preconceived ideas about my parents and myself, which continued to clutter me up and block my view of this fascinating world in which I live. The work on my parents' lives absorbed me for seven months, until March of the following year. I was then on the eve of my fifty-second birthday. It was with this work that the autonomy I spoke of, which in a sense had remained only "potential" for three years, became fully actual, complete and irreversible. It was also through this work, and only through it, that I was able to **love** my parents in the full sense of the word, that is to say: **to accept** what they were, or had been, with all that this had implied (and that I was then beginning to glimpse), and in particular, implied for me, their son.

If I felt the need to do this work (128<sub>1</sub>), and if I was able to do it, it was because three years earlier, I had been able to accept the gift of life received at birth, and denied for forty years - the gift of my unity. Or, to put it another way, it's because I'd been able to **accept my own nature**. It was through accepting and loving myself that I was able to accept and love my parents<sup>129</sup> (\*).

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I can also say that it was through this work alone that the **conflict with my parents** was "resolved"-a conflict whose existence I hadn't even suspected a few years before, when my parents were □ dead one by one and the other for over twenty years. It's true that the basic note in my attitude towards my parents, since my early childhood, had been one of admiring respect, appreciation, unreserved identification, and after their death, a kind of tacit cult of their person and memory. This is not the kind of relationship we usually refer to as "conflict", suggesting a basic note of antagonism, of enmity. Of course, my parents were happy with the way I made them feel, they thought it was all very well and in the right order - and there can't be many parents who wouldn't like to be in their place, or who don't congratulate themselves when they are! It was only after this work on my parents, and even more so after the work on my childhood that followed, that I was able to realize fully, with full knowledge of the facts, just how **false**, fake, not "**real**" this idyllic relationship I'd had with my parents had been. It could only have survived by stubbornly **erasing** from a touching canvas a host of things that didn't "fit", including painful periods (of acute antagonism, often felt as a **heartbreak**), or chronic "blunders", which recurred in the relationship between my mother and me with the same implacable regularity (even if less frequently) as had once been the case between her and my father. Not to mention things that had entirely escaped my conscious awareness, such as the "big cross" I had drawn over my parents at the age of eight, after two years spent in a foreign environment, with a hasty letter from my mother three or four times a year as any sign of life from either of them... ..

But the profound reason, the **real** reason, why I call my relationship with my parents "conflictual" between the summer of 1933 (when I was five years old) and the winter of 1979/80 (when I was fifty-one), is not that during those forty-six years there were conflicts that pitted me against one or the other or both of them jointly - whether these conflicts were frequent or rare, violent or latent, conscious or unconscious. It's rather that this relationship wasn't **and couldn't** be **assumed** (as it was, I mean, without profound transformation). It could only be lived and seen as I lived it and as I saw it, through the effect of a constant, tenacious **repression** of my faculties of knowledge and understanding; through a **refusal**

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that meditation on the past alone could have taught me.

<sup>129</sup>(\*) This is in line with the reflections at the end of the note "Acceptance (the awakening of yin (2))", n° 110.

stubbornly of a □ cognition of the true nature of this relationship, or at least, of certain

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of this relationship, involving in an essential way each of my parents as much as myself, and the image I maintained of us. To put it another way, the form this relationship had taken was perpetuated by a stubborn, incessant **flight** from a reality that was all too tangible, a reality that was just as stubborn in making itself known to me again and again, without me ever really learning anything from it while my parents were still alive. The episodes, sometimes heart-rending, of clear and undeniable conflict opposing me to one or the other, were only some of the more or less eloquent signs of the "conflictual" nature of the relationship with my parents, i.e. of this repression and evasion taking place **within myself**.

To put it another way, a "conflictual" relationship with others, in the deepest sense of the term, is one that is "divided", one that perpetuates itself equal to itself through a process of repression, of escape from reality, and which conversely helps to perpetuate these processes in itself. The signs of "conflict", of "division" in the relationship, can be as much in the nature of antagonism, as in that of allegiance; it can be a deliberate utterance of criticism or even disdain, as a deliberate utterance of approval or admiration.

And here I am again, without having sought it or planned it, with what might be called my philosophical "dada": that conflict between people is only the "sign" of conflict in each of the protagonists, or again: that the "source" of conflict in society is conflict, the division in the person. (The parents in all this ended up disappearing without a trace!).

This view seems to overlook entirely the more simplistic and by far more common view: that conflict between two people is the result of "interests" or desires in one and the other, which are "objectively" antagonistic, i.e., such that the satisfaction of one can only be achieved at the expense of the other. This is the universally accepted way of seeing things, whether we're talking about conflict between two distinct people, or internal conflict within the same person. So (in the first case) these "desires" are incompatible may be the desire to dominate, to set the tone, to steer the ship.

certainly the most □ current cases, including between parent and child (and just as much, between wife and husband, or between

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lover and lover). I'm not denying that this way of looking at things is real and useful, at least in some cases. But I see that it only concerns a superficial reality, while a deeper reality escapes it entirely. To give you an example, I'd like to point out that the desire to dominate (or to shine, or in general, to put oneself above others) is rooted precisely in this "self-contempt", in this "lack of self-knowledge" mentioned earlier, which we try to escape by adopting attitudes and behaviour that **blur** and **compensate for** this secret lack of self-esteem. So, beyond the "objective" conflict of antagonistic desires, we see in this case the conflict within the person, as the creator of desires of such a nature that they can only arouse and feed antagonisms to others.

Of course, with these few comments I'm not going to exhaust the delicate and important question of the relationship between the two aspects of conflict, which I'd like to describe as "superficial" and "deep" - and that's probably not the place here. Rather, I feel the need to return to the theme of conflict with the father, or conflict with the parents, from which I was moving away. At one point, I may have given the impression (and even let myself be carried away by it for a few moments!) that conflict with a parent, or with Pierre or Paule, was all the same. But I know it's not! I know that **conflict with the father, conflict with the mother, is at the heart of the conflict within ourselves**.

I spoke earlier, in this sense, of my "intimate conviction" (which I would also call a **knowledge** within me, a thing well understood), that in the one who is not divided within himself, the conflict with the parents is resolved. This knowledge, I said, comes to me above all (I believe) from the experience of conflict resolution in my

p. 587 relationship with my parents<sup>130</sup> (\*). Another way of putting it is that **accepting our parents** (i.e. ending the conflict with our parents) **is part of accepting ourselves**. They are (in relation to us) and our **origins**, and our **conditioning** (or a good part of it, at least). The first of one (our origins) is inseparable from who we are, whatever our path and destiny; the other (our conditioning) is deeply rooted in us, and as such is as much a part of who we are as our origins. To deny the true reality of our mother or father, whether expressed in antagonism or allegiance, is also to deny an essential part of ourselves and of what our life has been, as far back as we can remember. ...

And there's more. It was through our mother and father, before all others, that the conflict in both of them was transmitted to us. (This is what was expressed a few moments ago by the pithy term "our conditionings"!)

This is how they are linked to the conflict in ourselves, more closely than any other people in the world. And the first external projection of this conflict within us, and the oldest and most crucial of all, is the conflict with our mother and father. So it seems to me that the conflict within ourselves, and the conflict with either of our parents, are indissolubly linked - they are like one and the same conflict. Sometimes I've expressed the "intimate conviction" that when the conflict within us is resolved (or at least, when it's resolved at its root, in the "yin versus yang" division), then our conflict with our parents is resolved too; or, to put it another way, that the resolution of the conflict within us passes through that of the conflict with our parents. But I'm convinced that the opposite is also true: that as soon as the conflict with our parents is resolved, the conflict within us is resolved at the same time<sup>131</sup> (\*). This is why I see the relationship with our parents as a **key role** in our spiritual adventure, a unique role that belongs to no one else in our family, whether spouse or child, friend, teacher or pupil.

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p. 588 **Note 128**<sub>1</sub> (December 1) <sup>132</sup>(\*) The importance for me of "getting to know my parents" was reiterated vided by a dream, which came to me on October 28, 1978. It was a dream about my father's agony. This agony stretched over days and nights of painful struggle, surrounded by the busy indifference of those around him, while by the tacit consensus of all he was considered "already dead" - "it was like a verdict, which would have made his death effective, cutting short all doubt". When I woke up, I recounted the dream, but during the

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<sup>130</sup>(\*) See footnote below.

<sup>131</sup>(\*) I can give the impression here of posing as "the one who has resolved the conflict within himself". It's true that I say without reservation that the conflict with my parents has been totally resolved. It's also true that the conflict within myself continues to be felt in many ways, it hasn't disappeared. It's something that's certainly apparent on every page of Harvest and Sowing, and it's something I've had occasion to point out on more than one occasion, in one case or another. It would therefore seem to contradict the assertion commented on in this footnote, "that as soon as the conflict with our parents is resolved, the conflict within us is resolved at the same time". And yet, in a certain sense (the one I had in mind when writing these lines), it is indeed true that "the conflict within me is resolved". At least, something essential in this conflict, at its very root, is well and truly resolved, by this knowledge of my unity, by this acceptance of myself. If the conflict is likened to a tree with strong, deep roots, we can say that when the root is cut or withered, the tree is already dead, whereas through acquired inertia, the trunk and main branches remain in place, just long enough to wither and disintegrate little by little. I can feel this gradual "drying up" of the conflict as the years go by, like a once strong and vivacious hold that is gradually loosening. I see the writing of Récoltes et Semailles as one stage in this process, among many others over the past eight years. Another image to try and describe this same reality is that of a deep calm that gradually spreads out, like the calm of a deep sea, unaffected by the upheavals that shake the surface. I explain this in more detail in the two notes "The reunion (the awakening of yin (1))" and "Acceptance (the awakening of yin (2))", n°s 109, 110.

<sup>132</sup>(\*) This note is a b. de p. to the previous note° 128 "Parents - or the heart of the conflict".

In the three months that followed, I avoided thinking about it at all, to the point where it sank into the penumbra of half-forgetting. In short, I "buried" my father's death, about which this dream had spoken to me, and in this dream (which evoked a crucial aspect of my waking life) I "buried" my living father. There was considerable resistance to the clear and penetrating message of this overwhelmingly beautiful dream. They were resolved at the end of a first night of stubborn meditation on the meaning of the dream, on the following January 31st, followed by four more meditations in the three weeks that followed.

This dream made me realize that my relationship with my father and mother was a frozen, "dead" relationship, cut off from a living reality whose perception was being repressed - just as (in the dream) the perception of agony declared null and void, and the spontaneous action that followed from it, was repressed: to help the one who, painfully and abandoned by all, was struggling to live.

□The first thing to end this isolation in me was to get to know my parents. I

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I had no idea at the time of the dimensions of the task, but I imagined that "in a few hours" I'd be able to get "to the heart of the matter"! The idea of getting to know myself, particularly through my childhood, didn't occur to me at the time. I felt the need to do so later, as a spontaneous consequence of the journey I was about to embark upon. This journey began only six months later, in August 1979, because of the long digression (though by no means pointless in many respects) that constituted the episode "Eloge de l'Inceste". (For the latter, see note "L' Acte (113).

Along with the dream of October 18, 1976 (which triggered the "reunion"), this dream about my father's agony is one of the two that most strongly influenced the course of my life. The resistance to his message was much stronger, it seems to me. The message of the first was received within hours of awakening, while that of the second was put off for months. It only began to be fulfilled nine months later, with my departure on a voyage of discovery that continues to this day. ...

It's only in the last few days that I've been able to make the connection between the meaning of this dream and the reality of the funeral I'm trying to penetrate in the present reflection. This funeral, in which I appear as the "principal deceased", appeared to me a short while ago as a "return of things" (see the note of the same name),

(73)). This time, I see a "return of things" again, but from an entirely unexpected angle. In L'Enterrement, I appear alternately as "The Father" and "The Mother". The idea had never occurred to me that I'd ever been in the analogous position of a son, "burying" alive (be it symbolically, or by tacit consensus) his father or mother - quite the contrary! And indeed, I had strong reasons to be convinced of the contrary, reasons I first mention at the end of the note "the massacre" (in the context, it's true, of the Father's **massacre**, not his burial). (I come back to this in more detail in the note "Innocence (the marriage of yin and yang)" (107). In writing these last two paragraphs about my early childhood, in the note "The Massacre", I must surely have given the impression (and indeed, been under that impression myself at the time) that my relationship with my father had been free of conflict throughout my life. This is what

might also suggest a superficial look at this relationship. But already in the note commented on here, "Parents - or the heart of the conflict", □where

I do not confine myself to such epidermal impressions, it appears clearly that this is not the case, that this view of things (which was indeed mine until January 31, 1979) was one of the illusions that I was happy to entertain for most of my adult life. This illusion became clear to me the moment I finally took the trouble to examine the meaning of the dream about my father's agony - the most **beautiful** of all the dreams life has given me to date. This dream presents the grip of conflict on my relationship with my father with striking realism - and it also lets me experience the **resolution** of this conflict. The conflict is resolved by a **break** in me with the consensus that my father is dead, a break that suddenly opens the door to **something else** - and by a gesture of love from my father, giving me the chance to live with him.

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meaning that he had heard the cry that my constricted throat was unable to let out towards him. . .

The deep kinship between this dream experience, a striking parable of a frozen relationship with my parents (which suddenly comes back to life. . . ), and the reality of the Burial I've been probing for nearly nine months, is now so obvious to me. It's remarkable that throughout this long period of reflection, and right up until the last few days, the thought of this kinship never crossed my mind. I finally "stumbled upon it" by pure chance, in connection with a footnote in which I intended to point out, for all practical purposes, the role that this time too (in triggering a reflection on my parents) had played a certain **dream**, among so many others over the last eight years that have been like providential beacons on my path. This comment had the effect of putting me back in touch with the experience and substance of this dream, which I'm still a long way from having exhausted. Once this contact had been re-established, it was hardly possible, given the context, for the relationship with Burial not to become apparent.

It's true that this kinship, for the moment, only concerns a certain "knot", whereas in this dream and in the reality it transcribes, there is the knot, and its resolution. This resolution, moreover, which the dream had brought to life for me, the flavor and strength of which I'd known from that night on, it was up to me and no one else to make it a lived reality in my waking life too, in my relationship with my father and mother. I was free to do it, or not to do it - and for months, it was the latter alternative.

p. 591 that was my choice! Today - five years after that resolution - it's surely still the same, in□this sort of symmetrical situation where I'm involved, while I'm the one who acts as Father buried by a consensus-verdict, where I had been the son who devoutly buried his flesh-and-blood father alive! And perhaps this time too, it's by meditating on the meaning of my life - in this case, on the meaning of this burial - that I'll resolve this other knot in which I find myself, and perhaps dissolve yet another part of the weight of my past.

As to whether this meditation will be of any use to anyone other than myself - to such and such a protagonist perhaps of this funeral where I am not the only one to be buried, and where legions of mourners have flocked to the funeral - that need not be my concern; nor whether such and such a knot as I see in others will resolve itself or not. That's his job, I've got enough of my own! But if by any chance it should resolve itself while I'm still alive, I'll surely be one of the first to know\*, and I'll be glad of it... ..

### 18.2.7.5. (e) The Enemy Father (3) - where yang buries yang

**Note** 129 Decidedly, in the preceding pages<sup>133</sup> (\*), I barely touched on the theme of **conflict with parents**, and not even that of conflict with the father, which had been my starting point. The associations of ideas I followed from there seem to have distanced me from it, rather than deepening it. In what I've just said about conflict with parents, the roles of mother and father are interchangeable, just as it makes no difference whether the "we" referred to in these pages refers to a man or a woman. However, in our relationship to parents, mother and father are far from symmetrical, and the role played by each of them depends crucially on whether "we" are boy or girl (now man or woman).

In this case, the conflict with the father (expressed through his symbolic burial, or even his massacre) is of primary interest to me in the case of those I know to have actively participated in my funeral, all of whom are **men**. In the structuring of the ego, then, the father is the one to whom

p. 592 We **identify with**, and **model ourselves** on, in our relationship with others (and more specifically, with women), and in our relationship with ourselves. Rarely does this identifica□tion take place without major "burrs", and the antagonism to the father is one of the traces, a tenacious one if ever there was one. This is not the place to try to go into all the details.

<sup>133</sup>(\*) Those of note no. 128, of which this is an immediate continuation.



Nor do we examine the way they tend to express themselves in the relationship with the father. Indeed, my own experience in this area is so atypical that I'd perhaps be less well placed than anyone else to make such an inventory, even though I don't feel intimately, from my own experience, the ins and outs and the particular "flavour" of any of the main cases<sup>134</sup> (\*). My experience here is mostly indirect, based on what I've observed around me, and first and foremost in my children's relationships with me.

Over and above the particular nature of the "blunders", and the grievances and resentments towards the father that stem from them, there is one common aspect that I have strongly perceived on many occasions, when any deliberate "explanatory" statement was entirely absent. This is that the boy's or man's antagonism towards the father who has served him as a model, and whom he reproduces, whether "positively" or "negatively" (by imitation, or by opposition), whether he likes it and recognizes it or not - this antagonism is nothing other than an aspect, particularly eloquent and crucial, of an antagonism towards **himself**. More precisely, it is the outward sign, through the (more or less clearly expressed) rejection of the father, of the **rejection of a part of himself**; of that, surely, by which (unwittingly, or against certain conscious or unconscious options) he resembles his rejected model - his father.

As a result, I'm back on my feet - I can see the link between "self-contempt" (or "self-denial") and "antagonism to the father" becoming clearer - but I'm back on an unexpected side. I was prepared to find a more or less direct link between this antagonism to the father, and self-denial in the form of the refusal (or "burial") of the feminine in one's own person. Instead, I seem to have falls (as I should have expected, in "good logic") on the rejection of the **masculine**. And yet, I know although this refusal, less obvious and more hidden <sup>in□the</sup> man than the refusal of the feminine in him (of which I have especially p. 593

It's not so rare for a person's ego to be in conflict with his or her own, and it weighs just as heavily on him or her. Often, it is superimposed on the other, so that, however the ego is structured, whether in yin or yang colors, we are sure to be unacceptable to ourselves! Or to put it another way, this rejection of the father, or the rejection of what is "masculine", "virile" in oneself and makes us resemble the father, often goes hand **in hand** with the unreserved adoption (in the absence of a "yin" counterweight, rejected) of a "yang", "macho" value system with a touch of zinc!<sup>135</sup> (\*)

The idea comes to me that this contradiction (truly appalling indeed, once said and written in black and white!) is undoubtedly also the real **nerve** in this merciless **competition**, which is one of the hallmarks of our supermacho society (and this just as much in the upper echelons of science, as anywhere else. . . ). For if "climbing" and "surpassing" are superyang values par excellence, these values would undoubtedly not be internalized with such vehemence, nor would they be put into practice with such brutality (however subdued, when it comes to "high spheres" . . . ) if in the rival in the best position, the latter was not the only one. ) if we didn't also see the formidable shadow of the Father, at once admired, envied and secretly hated - the one who was there before us, and whose very existence, as far back as we can remember, has been **the great challenge** in our lives.

#### 18.2.7.6. (f) The arrow and the wave

**Note** 130 (November 19) I found myself impatient to continue where I left off. It's been a week, in fact (since the note of November 12, "L'épouse véhémence (le renversement du

<sup>134</sup>(\*) Compare with the reflections at the end of the note "The Massacre", n° 87.

<sup>135</sup>(\*) (November 29) At least, this is by far the most frequent case of which I'm aware.

p. 594 yin and yang)" (126), that day after day I have the feeling that I'm on the verge of getting "to the heart of the matter" - of coming to the overall picture of the Burial that I had promised myself, which would bring together the partial "strands" that had been de□gaged in the course of reflection - and a week, too, that the "point" in question found itself postponed from day by day. Each day, as I finish my note (since I have to stop and go to bed, as the hour advances), I feel that I've done a job I couldn't avoid doing, that I've "advanced" a notch - but at the same time I feel that the "point" I'm trying to get at has receded by the same amount! The obvious temptation here is to just keep going until I've got to the crux of the matter. But after the "health incidents" of the last three years, I also know that this is the blunder to avoid.

In fact, I know deep down that I'm right in the thick of it. I'm just gnawing at the bitter end. This impatience to get to the end of a task, this drive towards a "point" or "crux", intensely perceived in front of me - close by, or far away, it doesn't really matter - this attraction of the "goal" to me that throws me forward, like an arrow hurtling towards its target - this aspect that seems to me the most intensely "**yang**" of my person, characterizes my way of being **outside work time**. It's a striking aspect of the "**boss**", of what is conditioned, acquired in me. Nothing in what I knew of my early childhood could have foreshadowed this character, which appeared later in my childhood, and which has so strongly marked my entire adult life to this day.

p. 595 In the workplace itself, this aspect seems to have all but disappeared. I have the impression that the little that remains here and there is no more and no less than the sign of the boss's occasional interference, discreet it must be said, in the course of the work (where, to tell the truth, he has nothing to do!). The work itself, at the whim of the Worker who, through my hands, works at his own pace, follows a completely different rhythm. Impatient ardour is replaced by peaceful, stubborn calm. There is no longer an arrow hurrying towards a target, but a wave that stretches out far and wide, moving who knows where, wherever the moving force that drives it takes it - a wave followed by another wave, followed by yet another... . There is no hesitation in this movement, in every place and at every moment it has its own direction that carries it, or draws it forward. In each moment there is a progression, one cannot say towards what, there is a "work" accomplished in a movement that ignores effort.

- and there is no goal. The very idea of a "goal" here seems strangely preposterous - where on earth would we want to place it?! The goal has disappeared, and so has□the arrow. If there is an arrow, it's not a vibrating one that shoots out

in the heart of a target to come and plant themselves in it - but in **each** place of this moving mass of waves following one another there is an unequivocal movement and force, there is a direction in a progression, as precise and sharp as an arrow, invisible and yet imperious that would mark this direction, this force, this movement.

So, it seems to me that in my work, I'm as "yin", as "sea and motion", as one can be. This has been true, I believe, of all the work of discovery in my life, of all the work I've thrown myself into with passion, and above all, of my mathematical and meditative work. And now that I've unexpectedly described how I feel about this work in a sudden, compelling image, it seems to me that this image also describes the **movement of my life**, from the day of my reunion with myself, and perhaps even before, from the moment of my "salutary uprooting" from a cosy<sup>136</sup> (\*) fold. At the very least, it describes the "how" of my life on a deeper level, that of the "calm" I spoke of (just a few hours ago) in one of the footnotes to yesterday's note - a calm that is unaffected by the agitation that takes place on the surface. In this deep calm, there is movement and progress, but there is no goal - the goal has disappeared.

And I also remember now that it was this same image that came to me in March, when I

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<sup>136</sup>(\*) See note of the same name, n° 42.

speaks of the manifestations of my two passions, meditation and mathematics, as "the up-and-down motion of waves following one another, like the breaths of a vast and peaceful respiration... . "<sup>137</sup> (\*\*). Now, eight months on, I believe I recognize in these images the spontaneous movement of my being, in what is most spontaneous, in what is truly original in me - in what comes from the child eager to know, before it is touched by the preoccupation with appearances and the frenzy of becoming... .

#### 18.2.7.7. (g) The mystery of conflict

**Note 131** □ (November 20) Yesterday evening was spent almost entirely rereading the previous day's notes, p. 596 correct them on the way, retype a page that was decidedly too overloaded, write the footnotes (planned the day before) - and already it was midnight! I was anxious to get on with the evening's work, however little it might be, and went back to my typewriter to resume the previous day's interrupted "thread". And then something else came to mind - the image of the arrow and the wave. For a long time, I'd recognized myself in the arrow, whereas the wave seemed to correspond to a temperament quite different from my own. It's one of the surprises that came up in the course of this reflection on yin and yang, that it's this image of the wave that expresses most strikingly, and most accurately, the "basic tone" that prevails in my being, when "the boss" is far away, or at least when he gives way to something else. The image took shape, as if it had been there all ready, just waiting for the words that would finally give it form. They came without haste or hesitation, as I simply tried to **describe**, as faithfully as possible, without glossing over or distorting anything, what still remained a vague feeling.

The description completed, it was around two o'clock in the morning. I reread these two pages that very night, so there was no need to make any alterations. The trickiest part was when I tried to describe this intuition of a continuous infinity of "arrows", closing like a "field" of forces. It was an idea that presented itself forcefully, and which seemed reluctant to let itself be evoked by language. Yet I felt that this was an important aspect of the whole image, the "yang in the yin" aspect. In the wave there's "the arrow", there's a **momentum** that carries it forward, following a movement of its own, which is not that of **an** arrow, but rather that of a whole multiplicity, a **continuous** multiplicity that smoothly restores the wave's movement. And I also knew that in my work I was **also an** "arrow"; but I was doing so in a different mode from the one I'd imagined until now, because I hadn't taken the time to ever look at this work with any kind of attention, to immerse myself in it as if it were someone other than myself, in order to perceive its tonality. If I hadn't done so earlier, in the eight years that it's been happening to me

to meditate on, is undoubtedly that I have remained the unwitting prisoner of an inveterate deliberate intention: that of identifying myself with the □ "boss" in me, rather than with the Worker-child; that is to say also, when I speak of "me", of p. 597

think first and foremost (perhaps even exclusively, very often) of the person I am when it's the "boss" who takes center stage. In a way, these are also the times when I'm not at work.

The necessities and vagaries of teaching (among other things) have nevertheless, since the discovery of meditation, drawn my attention to **certain** features of my work - namely, those features which I felt were universal in nature, that they should be present in **all** creative work, in all work of discovery<sup>138</sup> (\*). But before this reflection on yin and yang, I hadn't yet thought of discerning

<sup>137</sup>(\*\*) See the end of the "My passions" section, n° 35, from which these lines are taken.

<sup>138</sup>(\*) The first written text, I believe, in which I evoke some of these traits, is that of October 1978, "En guise de Programme" (alluded to in the note of November 6, "La belle inconnue" n° 130). After this text, I won't bother to spell out and expand on my observations on this subject in black and white before this year's Harvest and Sowing reflection. Its first eight sections are essentially devoted to this theme, not to mention numerous other comments throughout the course of the year.

in my own work, which makes it different from that of any other. One of these traits, which seems to me the most crucial of all, is finally identified in the November 8 note "La mer qui monte. ... "

(122). The image first evoked in that note, in the typical context of a conjecture to be proved, is taken up again in yesterday's notes, in a different light, outside any particular context. At last, I'll pick up where I left off the day before yesterday. I left<sup>139</sup> (\*\*) with the intention of trying to pinpoint the root cause of antagonism to the father, beyond the specific grievances we may have against him. Following the associations of ideas that came to the fore, I initially strayed from this line of thought, being led above all to speak of conflict with parents, father or mother indifferently. This "conflict" can take the form of allegiance (as was the case with me), as well as that of "conflict" between parents.

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of antagonism. Since my work on the lives of born parents, this "conflict **with parents**" appears to me to be truly "at the heart of the conflict" within ourselves. Resolving the latter, I'm convinced, is neither more, nor less than resolving the conflict with the parents, i.e.: being free of them, being fully autonomous spiritually, pursuing **one's own** journey... .

Returning once again to antagonism to the father, in man, I've reconnected with an intuition that has occurred to me many times over the last few years: it has occurred to me that the deeper meaning of this antagonism to the father is the rejection of that in us which makes us like the father, of the appearance and **virile** traits of our person. I've made this last part of yesterday's reflection<sup>140</sup> (\*) a separate note, with the name

"The enemy father (3) - or yang buries yang" - thus also suggesting, by this name, the link with the two sections "The enemy father (1), (2)" (n° s 29,30), where this "enemy father" theme appears for the first time.

Thus, the aspect of the Burial discussed at the start of yesterday's reflection, namely the aspect of "self-contempt", or "self-unrecognition" or "self-denial", appears as a kind of hyphen, or better still, a "**hinge**", between the two preceding sections, the "Supermother - or burial of the 'feminine'" section and the "Superfather - or massacre and burial of the father" section. This hinge-like nature becomes apparent as soon as it becomes clear that, in the first of these strands, "the feminine" is, above all else, "the feminine **in us**" (as indeed was perceived as early as the November 10 note "Les obsèques du yin (yang enterre yin (4)", where the "Supermère" strand makes its appearance); and furthermore, that "the father" is, above all, the symbolic substitute for "the masculine in us". Thus, the two aspects in question are perfectly aligned.

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symmetrical, corresponding to the two obvious "cases de figure" of the "refusal of self" - namely, the refusal of "the woman" (aka Mother) in us, and the refusal of "the man" (aka Father) in us<sup>141</sup> (\*\*). And the theme of conflict at the

parents, which is a kind of conjunction or superposition of the two distinct themes of conflict with the mother, and with the father, also appears as a kind of hinge. Or to put it another way, as seen in yesterday's reflection<sup>142</sup> (\*), this theme appears to be inseparable from that of self-denial, the one and the other being two distinct aspects of the same undivided reality, that of **conflict within ourselves**.

In all this, it would seem that the original aim of "identifying the root **cause** of antagonism to the father" is still unfulfilled. I could say that antagonism to the father is one of the **forms** taken by antagonism to oneself, or self-denial. The initial question therefore seems to split into two. On the one hand, for what "causes" does self-denial take on this particular form in certain cases? Probing it also means

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of this reflection.

<sup>139</sup>(\*\*) In the note "Parents - or the heart of the conflict", n° 128.

<sup>140</sup>(\*) In fact, this is not the previous day's note, but the day before, which I'm about to follow up on.

<sup>141</sup>(\*\*) I would remind you that it is by no means rare for the two kinds of "symmetrical" refusal to overlap in the same person. Given the devaluation of yin in our society, it must be quite rare, in any case, for yin refusal not to be present in a more or less pronounced form. So I'd be tempted to see in the antagonism to the father a sign (at least presumptive) of a double refusal of yin **and** yang.

<sup>142</sup>(\*) See penultimate footnote.

We'd like to take a closer look at a number of different typical situations that could give rise to such antagonism.

On the other hand, we're back to the even deeper and more crucial question of the "**cause**" of self-denial, i.e. the cause of conflict and division within us. I think I've at least grasped the common **mechanism** by which the generational conflict is transmitted - the rejection of ourselves within us is nothing other than the internalization of the rejection of us by those around us from our earliest years - of the rejection at least of certain aspects and impulses within us, which form an essential part of our original being, of our creative faculties. I touch on this aspect of things (among others) in the "Refusal and acceptance" section of "The key to yin and yang", and more particularly in the first two notes "Paradise lost" and "The cycle" (116), (116').

Having grasped this common "mechanism" of conflict transmission, however, does not mean having understood the **cause** of conflict in ourselves and (through us) in human society. **Why**, from time immemorial in all places (according to the unanimous testimonies that have come down to us through the ages), "Society" does not tolerate

Is it not the case that those who make it up are **whole** beings? That is to say, beings in full □possession of their creative faculties, who do not repress at great cost a part of what they are, considered so shameful (or so fearsome... ) that it is better to ignore that it is, and tacitly rule that it **is not**... .

For me, this is one of the great mysteries of existence, perhaps the greatest mystery of all<sup>143</sup> (\*).

There was a time, just a few years ago, when my attitude towards the universal reality of repression and conflict was one of militant **revolt** - revolt against this "**sword**", which claimed to cut in two what, by its very nature, should be one, **was** one. This was my attitude when I wrote the Eloge five years ago<sup>144</sup> (\*\*). It was through the long-term meditation that followed, on the lives of my parents, that this attitude changed. Through this work, which day after day brought me back into intimate contact with the manifestations of the conflict in my parents, and which patiently led me from the manifestations to their meaning and cause - through this work I finally came to feel the **mystery** of the conflict. The rebellious attitude had disappeared, as if it had never been there. It had been an epidermal reaction, a simple dispersion of energy. A revolt - against whom? Not against a person or a group of people, against the famous "Them... ." ! We're all in the same boat, and we've been here a million or two years... . Revolt against "God"? That's all it would have taken.

□ Deep down, I've known for a long time (I couldn't even say how long, although for a long time p . 601 I pretended to ignore it. ... ), that everything in this world has its good reason for being, and even, if we understand the bottom line, surely everything is **good** as it is. Death and the "beyond" of death (if there is such a thing) is one of these things. It's a mystery, and if there's a "**faith**" in me about it, it doesn't consist in "articles of faith" about the existence (or non-existence) of an afterlife and its particularities, but simply in this simple assurance: that things are perfect as they are, including everything to do with death, and also everything to do with birth, which is just as mysterious. For

<sup>143</sup>(\*) This suggestion is purely subjective, it simply reflects the fact that, of all the "great mysteries of existence", this is the one I feel particularly strongly about, in a way that goes beyond mere intellectual curiosity. It's the only one that arouses in me a **desire** to fathom it, to know it, to know "the last word" (insofar as it can be known, with the limited faculties that are mine). The difference is the same as in mathematics, between the open-ended questions that "I can feel" (into which I could dive straight away), and those that I "understand" in the technical sense of the term, whose scope I perceive (at a **superficial** level), but which "neither warm nor cool me". The Riemann hypothesis is one of the latter (no doubt due to my great ignorance of analytic number theory), and Fermat's theorem was another until a few years ago. It's my "Anabelian" reflections that have changed my attitude towards the latter, while my ignorance of the work it gave rise to is still as great as ever.

<sup>144</sup>(\*\*) This episode is mentioned several times in Récoltes et Semailles, most recently in the note "L'Acte", n° 113.

For a long time, however, I had excluded "conflict" from this list - I saw it as a kind of "burr", an inadmissible bleat, a stubborn and bizarre (even revolting) "kink" in the concert of Creation. All it took was for me to become intimately acquainted with the conflict, instead of wasting my time pretending to fight with it, for my relationship with it to be profoundly transformed.

The mysteries of death and "after death", of birth and "before birth", are not unique to our species. The questions they raise have meaning for all living beings, perhaps even for everything from the electron to the nebula. The mystery of conflict, on the other hand, seems to me to be unique to man, to

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the human species<sup>145</sup> (\*). It appears to me as **the** great mystery about the particular meaning, the particular destiny of **our species**. The "explanations" that have been given, by ethnologists and the psychologists, those altogether at least that I've heard of, are clearly no more than **rationalizations**, to **justify** the repression suffered and internalized, as indispensable to the smooth running and very existence of society; rather like in a society of one-armed or one-legged people, there will be no shortage of eminent theorists to prove A plus B (without anyone thinking of contradicting) that a society where people have the use of both arms (or both legs) could in no way function<sup>146</sup> (\*). These are all convoluted justifications, attempting to conceal a mystery with explanations that purport to be "scientific". In fact, the question of the origin and meaning of conflict (or repression) in human society remains purely rhetorical, as long as those who pretend to ask it have not gone through an intense and in-depth process of understanding conflict **itself**, and the origins of conflict **within it**. In the absence of such self-knowledge, this question (like questions about the nature of freedom, or love, or creativity) is a modern equivalent of the medieval question about the "sex of angels" - an exercise in style without more, to manage to "fit in" what needs to be fitted in anyway. Strictly speaking, this question is not a "scientific" one, and its examination does not presuppose **maturity**, but simply a certain preliminary knowledge, and a certain level of intellectual power or agility<sup>147</sup> (\*\*).

In this case, it's not a question of trying to guess the mechanisms by which repression is established in human <sup>society</sup>, i.e. to find an **explanation** for the fact of repression. A

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Even supposing we could come up with a plausible, even convincing scenario, I wouldn't feel much further ahead. It might shed light on a certain interesting aspect of the mystery - the "mechanical" aspect, in short - without penetrating it. No more than the detailed results of paleontology and the

<sup>145</sup>(\*) (December 3) I may be right to object that conflict, in the form of aggression and confrontation between individuals or groups of individuals, exists within species other than our own. When I speak of "conflict" here, I'm thinking of the specific form it takes in human society, and in particular of its profound links with **division** and **repression** within the individual - repression of the major part of his being, and in particular repression of his means of perceiving reality, and of perception itself. The various forms of repression seem to me to be rooted in the one that seems to me to be the most crucial of all, the so-called "sexual" repression, which inculcates shame of one's own body and bodily functions and drives (or at least, of some of these functions and drives). These are mechanisms unknown outside the human species, as far as I know. Perhaps I'm wrong in using the terms "conflict", "division" and "repression" almost as synonyms, or at least as terms that designate different aspects of the same reality. I'll explain a little more about the meaning of the word "conflict" for me in the note "Parents - or the heart of conflict", n° 128.

<sup>146</sup>(\*) Just as in the days of slave societies, for "the best minds" (who were also served by slaves) as for the rest, it was taken for granted that "no society without slaves". Apparently, it wasn't until Plato had the unexpected good fortune to find himself a slave that he began to see things differently.

<sup>147</sup>(\*\*) (December 3) The fact that the question of the meaning of conflict does not fall within the remit of science might give rise to the expectation that answers can be found in myths and religions. "It seems to me, however, that this is not the case. From what I know, it would seem that one of their essential functions, not to say their main function, is to establish a "law" which, for the most part, consists of a "package" of prohibitions through which repression materializes in a particular society. This law, presented as sacred in essence, is not required to justify itself, or to explain its "meaning", let alone the meaning it shares with other laws governing other societies.

molecular biology, nor even Darwin's profound ideas, really penetrate the mystery of the appearance of life and its creative flowering on earth over the past three or four billion years. What interests me in the mystery of conflict is not the mechanical, scientific aspect - an aspect that's as **external to me** as Fermat's famous "theorem". But it's the question of the **meaning** of conflict. This meaning **concerns me** in an immediate and essential way, just as it concerns each and every one of the countless men and women who have torn and killed each other over countless generations, and who have passed on to their children the conflict taken up from their parents.

That there must be a **meaning** to the conflict, and that I can know what that meaning is, is surely part of the "faith" I was talking about earlier. It's obvious to me - and that familiar "sense of mystery", that there's something deep here to fathom, tells me at the same time that this "something" **is** precisely **that meaning**. The "faith" in question overlaps with a faith in my faculties, when they reveal to me, here without the shadow of a doubt, that there is a "meaning" before me to discover.

Perhaps one day, this meaning will become apparent, as if I had always known it! This mystery doesn't seem distant or unapproachable. It presents itself to me as something very close at hand, which it would be up to me to know more intimately. And surely I can already see a way of approaching it, or rather an aspect that already seems to be beckoning me in a friendly way. After all, conflict has much to teach me, and has already taught me a great deal... .

#### 18.2.7.8. (h) The reversal (2) - or the ambiguous revolt

**Note** 132 ( November 22) That makes two notes in a row where I see myself embarking on excursions anything out of program - this time □ I'll be careful to start first with this

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which, for once, was **expected**. I'd like to look at one of the "typical situations" mentioned (without further clarification) in the previous note, situations likely to give rise to antagonism towards the father, and more profoundly, to a (more or less radical) rejection of virile traits in oneself (which rejection finds its symbolic expression in the rejection of the father). I had remembered the situation in question as early as the November 18 reflection, ending with the note "The enemy father (3) - or yang buries yang". My intention then was to point out, in this "typical situation" at least, a **direct link between rejection of the masculine and rejection of the feminine**.

The case in point closest to me, and on which I had worked at length in addition, was that of my mother. All her life, she had indulged in a barely disguised contempt for everything feminine, she had modelled herself on masculine values to excess, and at the same time her relationship with men had been, since her adolescence, a "viscerally" antagonistic one<sup>148</sup> (\*). I was very fortunate that my mother spoke to me very freely about her life from childhood onwards, and that I had access to detailed autobiographical notes up to the early years of her life with my father, not to mention a voluminous correspondence. In addition to my own experiences with her, this is exceptionally rich material, which I'm far from having exhausted. I've worked with her enough to feel, without a doubt, that the double refusal in her that I've just mentioned - refusal of the feminine and an- tagonism towards men - was rooted in a torn relationship with her father. The latter, an endearing man in many respects, generous, honest and affectionate, had become embittered during a long social decline in post-war Germany (I mean, the Germany of 14-18), of which there were so many. In fact, this downward spiral had begun even earlier, from a well-to-do, carriage-riding man to a

<sup>148</sup>(\*) Unlike his disdain for the feminine, this visceral antagonism, reflected in a vehement and turbulent love life, remained unconscious throughout his life. I only became aware of it during my work from August 1979 to March 1980.

itinerant shoe-shiner. Under the sting of worries and disappointments, his short-tempered temper sometimes turned to family tyranny, for which his wife, in frail health, fai□s mostly to blame. My mother, deeply attached to her father as she was to her mother, was repulsed by these episodes of paternal tyranny, suffered in silence by her mother, who sometimes couldn't take it but never complained. The child was passionately identified with her mother, the victim of paternal arbitrariness, and at the same time the role played by her mother (the role of victim, the passive role - "the role of a woman". . . ) seemed intolerable to her. There was this identification with the mother, expressed in a revolt, a visceral antagonism towards the father, and at the same **time** there was this startle that "I'll never be like her" (who suffers without rebelling), a startle that could only mean at the same time "I'll never be like women".

But even more profoundly, there was also a longing for the power of the father, of the man, to dominate at will. And my mother's life was dominated and devastated by this all-consuming passion to dominate; and above all, to dominate and break **man** - the very man who aroused in her such a surge of raging revolt, the man who by his very nature was supposed to dominate her - just as her father had dominated her mother, suffering, pale and powerless, his power.

I was going to write here that the reflection now "joins" that pursued in the note "The vehement wife (the reversal of yin and yang)", of November 12 (126). As I didn't remember this note very clearly, I've just reread it. Strangely enough, I had forgotten that this note was prompted (like today's) by my mother's "case study". Ten days ago, I had felt reluctant to develop this case at all. If I've come back to it today, overcoming this reluctance (which I'd also forgotten in the meantime!), it's undoubtedly because there was an aspect of the situation that had remained unclear. I'd also forgotten that the starting point for today's note, "the intention to put my finger... on a direct link between the refusal of the masculine and the refusal of the feminine", had already been the initial motivation for the reflection of ten days ago, following naturally on from the question that ended the previous day's note "Supermaman or Superpapa?" (125). In fact, the last sentence of the 12 :

"It doesn't take much to see the 'missing link' between... .", would seem to say that I had then thought I had accomplished my task for the day (of éta□blir such a link). If I had entirely forgotten that I had already updated this link, and even though I had already asked myself this question before the note of four days ago (on which I followed up today's reflection), it's undoubtedly because I hadn't yet been fully convinced by the brilliant conclusion I've just quoted, formulated no more than six days before this note "The enemy father". (3) - or yang buries yang". The situation becomes clearer by quoting the whole sentence:

"It doesn't take much to see the "missing link" appear between antagonism to the Superfather (finding its symbolic expression in the burial of the aforementioned), and contempt, rejection of the "feminine", and more profoundly the denial of "the woman" in oneself (which will perhaps find expression in the symbolic "Burial" of a "Supermother", under a plethora of dithyrambic epithets of double use... .)."

In this conclusion, there was one step missing, which made it hasty: it was the link between "antagonism to the Superfather" and the refusal of the "masculine", a link that only appeared in the reflection with the quoted note of November 18 "The Father the enemy (3) - or yang buries yang". Antagonism to the Father then appeared to me as the symbolic expression of the far more crucial reality of refusing the yang, "masculine" side of oneself. In the "symmetrical" case of the rejection of the feminine, this link between the symbolic expression and its deeper meaning had already been perceived when the "Supermere part" appeared, in the November 10 note "The funeral of yin (yang buries yin (4))" (124). This is how the two "opposing" strands that appeared in the "Supermaman or Superdad?" note of November 11th - the Father's funeral and the Mother's funeral - came to be seen.



before yesterday as symmetrical manifestations of self-denial (or self-contempt), taking the dual form of the **rejection of the masculine and the rejection of the feminine in their own person.**

In my note of the 18th, "The Enemy Father (3) - or yang buries yang", I confined myself to the case of a **male** "subject" - although the most extreme case known to me is that of my mother! In fact, my mother had been entirely forgotten in this reflection, even ten days earlier (if not hidden under the heading "my parents" in the note of November 17).

It's the knowledge I have of my children and their relationship to me, that made me feel four days ago a link between antagonism to the father, and the rejection of the masculine in oneself. In fact, for each of the four

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(among my five) children whom I've had the opportunity to know fairly closely, I've more than once in recent years sensed, behind attitudes of inveterate antagonism towards me, their father, a rejection of the virile side of their being, and above all, of the **impetus** within them which launches them out to meet the world - and which makes them resemble a father who's been rejected! I'd never asked myself whether this was a general fact; or rather, there was a sort of unspoken presumption in me that it must be so, without my ever feeling the need, before the reflection of four days ago, to formulate the thing clearly, let alone examine it with any care. To tell the truth, this kind of "general" question was not at all one of those I asked myself in meditation, whose purpose had been more down-to-earth: to understand myself, above all through my relationships with others - and thereby also, to some extent, to understand "others", i.e. those with whom I came into relationship.

Of course, in my reflection of four days ago, when I suggested that there must be this link, that antagonism to the father was the expression of a deeper conflict, namely the rejection of "the man" within oneself, it was still a simple presumption, suggested by my very limited experience. This link seems to me at least plausible, and more particularly in men, but I don't claim to "see" this link in general. I don't have this "intimate conviction" about it, which I so often choose as my very sure guide. In my mother's case, for example, I can see that antagonism to the father was the source of an occult and virulent antagonism to virile traits **in a man**, but by no means to such traits in a woman, quite the contrary. It's true that the mere fact of fully valuing virile traits, and cultivating them to excess in oneself, may not necessarily mean that one fully accepts the yang side of one's being; that would, after all, **also** mean accepting the "yin within the yang" that is spontaneously found in any yang "dominant" trait, which of course was not the case with my mother.

But this is taking a rather dialectical turn, which doesn't inspire me with confidence! I prefer to refer instead to my direct perception of my mother, as refined by my reflection on her life and that of my father. I don't remember ever having the feeling  
un refus chez elle de quelque chose, **en** **she** that is fundamentally "virile". On the other hand, I strongly perceived

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in her, this contradiction, or rather, this **heartbreak**, of one who cultivates within herself (like so many **weapons**), and who cherishes more than her life, the very traits that, in man, arouse in her such vehemence, and whose life has been crumbled (and prematurely consumed) by the fever of constantly meeting and confronting, and reducing to mercy in others, the **same** force on which she has staked her all, and which devastates her own life, as it devastates the lives of all those dear to her.

## 18.2.8. Masters and Servant

### 18.2.8.1. (a) Velvet paw - or smiles

**Note** 133 (November 24) The cases mentioned in the previous note of the day before yesterday are not, to my knowledge, the only ones to confirm this presentiment that a superyang imbalance in the father (whether or not this imbalance takes despotic forms), is reflected in the children by a rejection of yang, which in turn can express itself in many different ways. In boys, in the cases I'm aware of and which are on my mind at the time of writing, this rejection takes the form of a (more or less complete) repression of the virile side of himself - and this rejection will surely follow him throughout his life (unless profoundly renewed, which is certainly rare). My mother's case shows me that it's not always the same with a daughter - unless my mother also had a certain rejection of the virile side of her being, expressed in a more subtle way that I've never been aware of until now<sup>149</sup> (\*). What is striking in her case, however, is the opposite extreme effect - that of an overdevelopment of virile traits in her (in addition to an aversion to everything feminine). I know of other cases in the same vein, in **men** (my mother's father, for example) - that of a **revolt** against the father, expressed by the development of a strongly virile personality, able to confront the father "on equal terms". As I haven't had the opportunity to experience such a case up close, I'm inclined to believe that it must be rarer. But it doesn't really matter.

□ If there's one common thread running through all the cases I've come across from near and far, it would be this: a The father's superyang imbalance is reflected in the child's **imbalance**, which may be in the yin direction (perhaps the most common case), or in the yang direction<sup>150</sup> (\*). In every case I can think of (though I wouldn't dream of systematically listing all the cases I've come across), this imbalance is accompanied by **an antagonistic relationship with the father**. I have the impression that it is also accompanied by a visceral antagonistic attitude towards **male** third persons, in whom yang traits are strongly marked, at least when these are not balanced by complementary yin traits - that is, towards men in whom a superyang imbalance prevails, reminiscent of that of the father.

A superyang imbalance of this kind (like the opposite imbalance) is bound to arouse **unease** in anyone, as I've already seen<sup>151</sup> (\*\*). But this uneasiness doesn't necessarily translate into an automatic antagonistic attitude - it's not uncommon, for example, for it to be resolved (or at least to disappear from the field of consciousness) by an attitude of submission, more or less unconditional admiration, or allegiance.

The association comes to me here that it was these tones that were surely the most common, in relations to my person (haloed by prestige), within the mathematical world - at least among those colleagues (or students) who (as I wrote elsewhere) "did not feel protected by a comparable renown", or (I'll add here) those in whom a certain inner balance, a certain spontaneous knowledge of their own

force, did not exclude such cantilevers. But it is undoubtedly in the nature of such a relationship of "allegiance" that it conceals a hidden antagonism, which <sup>se□manifests</sup> itself (openly, or in a way that still remains occult)

<sup>149</sup>(\*) A related situation is that of a mother with a domineering, invasive temperament, a sign of superyang imbalance. In both cases, which I know first-hand, this resulted in the daughter's repression of her "virile" traits.

<sup>150</sup>(\*) When I speak here of "imbalance in yin direction", this does not mean a (perhaps excessive, one-sided) development of yin traits, but rather a **repression** of yang traits, which is not at all the same thing. In the opposite case, described as "imbalance in yang direction", it does mean an "excessive development" of yang traits, which often goes hand in hand with a more or less thorough repression of certain yin traits.

<sup>151</sup>(\*\*) In the note "Le Superpère (yang enterre yin (2))", n° 108.

when the right opportunity arises...

I've just followed a few associations, which pick up on and complete my thoughts of the day before yesterday (in the previous note "The reversal of yin and yang (2) - or revolt"), and by the same token, those of the note of November 18, "The enemy father (3) - or yang buries yang". They make me realize that the relationship between a certain state of yin or yang imbalance in one of the parents (in this case, a yang imbalance in the father), and the repercussions it has on the child, is by no means univocal, as I hastily suggested. Undoubtedly, the form in which parental imbalance, in this case of the father, is transmitted must depend on many other factors, both the family environment (and more particularly, the mother's person and attitude), and the child's birth temperament<sup>152</sup> (\*).

But to tell the truth, that wasn't the direction I had in mind when I started thinking about this earlier. Rather, I was thinking of pursuing a completely different association of ideas, which has been present since the reflection of November 12, when the dynamic of **the reversal of** yin and yang roles was introduced into the reflection for the first time (in the note of the same name, "- or the vehement wife", (126)). Perhaps the reader will have made the connection on his or her own - in any case, when I raised this question on November 12, then the day before yesterday on November 22, somewhere in the back of my mind, as if in mute tones, was the thought of two other occasions when "reversal" had already been mentioned, in the course of this reflection on Burial. The first was in the note of the same name in Cortège V, "Mon ami Pierre" (note (68') of April 28). The second occurrence is in a footnote, in the reflection of September 30, part of the note "L' Eloge Funèbre (2) - ou l'auréole et la force". There's even a third such occasion, but between the lines, at the beginning of the reflection due the day after tomorrow, which opened the reflection "The key to yin and yang".

(This is the note "Muscle and tripe (yang buries yin (!))" (106), from October 30.) □ This is the content of p. 611.

the famous "association d'idées, suscitée par l'Eloge Funèbre en trois volets", to which it alludes

- the very one that set me off that very day, to start the digression on wine and yang that I've been pursuing for almost two months. Now might be the perfect time to let the cat out of the bag, since I've been talking about it, not to mention thinking about it since the day after May 12, after the note "L' Eloge Funèbre (1) - ou les compliments", more than six months ago.

What these three situations have in common is that they involve a "reversal" of roles between my friend and ex-student Pierre, and myself. In both cases, which I mentioned a moment ago, I appear as my ex-student's "collaborator" (if not outright pupil!). The first time, it's as if I'd contributed (in a messy, admittedly, but sometimes interesting way) to the development of the "powerful tool" of *l-adic* cohomology by my brilliant predecessor and friend. The second time, when we are quoted in a breath (for having "linked topology, algebraic geometry and number theory by 'interdisciplinary' means ... ."), the same reversal of reality is cleverly suggested by a typographical "oversight", as if by the greatest of coincidences<sup>153</sup> (\*). The meaning of this reversal becomes even more tendentious than a simple question of precedence (in this case, within an institution that Dieudonné and I were the only ones to "start" at the scientific level, but which I had left a long time ago), when we pay attention to the choice of eulogistic epithets ("theories of legendary depth" for one, "brilliant discoveries" for the other, who is also entitled to the underline, along with everyone else except me). This meaning was "strikingly" clarified in the reflection "Les

<sup>152</sup>(\*) In this way, I can see that each of my mother's three brothers (all younger than her) developed in a very different way from my mother (who was a bit of a swan in a brood of ducks), and from the other brothers too.

<sup>153</sup>As I realized earlier in the note "The Massacre" (n° 87), chance often makes things right, as long as the typographers and movers get involved!

obsequies of yin (yang buries yin (4)) ((124), November 10), in which reflection on yin and yang suddenly "landed" in the midst of a funeral ceremony: to one the accumulation of epithets (dithyrambic at times) yin and superyin, to the other yang and superyang. ...

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That's what struck me the day after the note □ "Les compliments" of May 12, even before that. to have had the opportunity to explain it in as much detail as I did two weeks ago. According to the way I felt at the time (which I'll have to revisit here), there was a real **reversal of** reality, or more precisely, a "reversal", taken to a caricatural extreme, of a basic reality which I felt to be nuanced and balanced. I saw myself as a person with a strong "yang" or even superyang dominant, at least in my most apparent, most obvious traits, and particularly those that are obvious to others<sup>154</sup> (\*). On the other hand, I sensed in my friend Pierre a basic yin temperament, much more balanced than mine had been in the days when we saw each other often and he was my pupil.

In fact, I believe that this apprehension of reality was essentially correct. If I have sometimes, in recent years, and most recently<sup>155</sup> (\*\*), sensed an original "yin" background note in myself, it seems to me that I was the first and only one to feel it - that it was above all through my yang or "virile" traits, often quite invasive, that I was constantly apprehended by others<sup>156</sup> (\*\*\*), both on a conscious and unconscious level - at least as far as personal relationships were concerned. These relationships (apart from romantic ones) mainly, if not exclusively, involve the "boss" in us, the conditioned. The new fact that emerged in the course of thinking about yin and yang - that **in my work**, my approach to things is predominantly yin, "feminine" - doesn't really contradict what I already knew. He nuanced it, correcting me on a point where I had tacitly put everything "in the same bag". And all things considered, it seems to me that the sudden and strong impression I'd had of a cartoonish "reversal" of a reality, or more precisely, of an **intention** of such a deliberate reversal - that this "intuition" was also essentially correct, albeit sketchy. It's the reality imperfectly grasped by this intuition that I'd now like to delve into more closely.

#### 18.2.8.2. (b) Brothers and spouses - or double signature

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**Note 134** (November 25) I'd first have to try and get a closer grasp of this impression, for me that the "base note" in my friend Pierre is a **yin** note. As I see it, this is true both at the level of the "I", as I've seen it expressed in his relationship with me and others, and in his work, i.e. at the level of the drive for knowledge, the creative faculties within him.

As for the first aspect, he and I were obviously of **complementary** temperaments, with the added nuance that the excessive, "superyang" aspect of mine seemed to disconcert him somewhat at times. It was above all, I think, this constant forward projection towards the accomplishment of my tasks, this **isolation** from everything that wasn't related to them, that aroused in him a kind of incredulous astonishment, in which I sensed a nuance of affectionate regret - the same regret I'd felt many times in my mother, when she saw me so cut off from the beauty of things around me<sup>157</sup> (\*).

<sup>154</sup>(\*) Even more so in the years "before I left" than now.

<sup>155</sup>(\*\*) In the note "The arrow and the wave" (n° 130, November 19).

<sup>156</sup>(\*\*\*) And for myself too.

<sup>157</sup>(\*) My mother, like my father, retained right up to the end of her life a capacity for communion with nature, as well as a keen sense of observation of everything around her, both of which I still lack today. This was perhaps the only "yin" aspect of her being that she didn't repress, that was able to blossom freely. On the other hand,

Strictly speaking, it wasn't a feeling of unease, a sign of rejection of a certain reality. At least, I don't recall a single instance of him feeling uneasy with me, or having the impression of an attitude or movement of rejection, of distancing, or even of a clash between us. And I have no doubt that this was in no way a deliberate "diplomatic" gesture on his part, of one who had decided to let nothing show. On the contrary, he sometimes expressed the "astonishment" to which I alluded, without any trace of embarrassment or irritation. Clearly, the basic tone in our relationship, and one that has never wavered to this day<sup>158</sup> (\*\*), was one of affectionate sympathy, which never crossed no shadows.

□ This remains for me a strange fact, and one that nothing I believe could have made anyone suspect, before p. 614 the episode of my departure from the IHES (and even then, at the level of what "passes" directly in a tête à tête, let's say) the fact that from the very first years after our meeting there was a deep, essential ambiguity in his relationship to my person, through the presence of a hidden antagonism, a desire at least to distance himself from my person, and that of ousting. The latter manifested itself in a particularly brutal way (which left me stunned at the time), though infinitely subdued in manner, during the episode of my departure from the IHES (mentioned in the section "Eviction" (63)). My friend had just been co-opted as the fifth "permanent" member of IHES, thanks in no small part to my heartfelt efforts in this direction. In the "explanation" that took place between us (perhaps there were several, I couldn't say), he never lost that perfect, smiling naturalness, with all the aspects of benevolent kindness, that made him so endearing. He then explained to me, without my detecting the slightest hint of hesitation or embarrassment, and even less of antagonism or enmity, or secret satisfaction, that he had from those early years made the decision to devote his life and all his energy to mathematical work; that this dedication to mathematics, which was his for better or for worse, had to come before anything else; that the reason why I expected the support of my colleagues and, in particular, of himself (to request the withdrawal of funds from the Ministry of the Armed Forces) seemed to him to be entirely unrelated to mathematics; that he regretted, of course, that this was a prohibitive circumstance for me, and that, given life "axioms" different from his own, I was about to leave the IHES for a cause which, from his point of view, seemed inconsequential; but that, to his great regret, he could not associate himself, any more than my other colleagues, with a request which was foreign to him, and the outcome of which was entirely indifferent to him (134 ).<sub>1</sub>

In essence, I have given the "manifest", explicit content of my friend's speech, as I remember it, without any effort to try and restore a style of expression, or the atmosphere of an interview, of which I have retained no particularity beyond what I have said here.

The episode takes place at a time when I hadn't yet the slightest suspicion that, behind the overt content quite anodyne (and sometimes strangely absurd) speech, often expresses itself in □ sourdine, and quite clearly, p. 615 a completely different message. This one was surely perceived on an unconscious level, but fiercely rejected, repressed from the conscious field. It's in this note, however, written over fourteen years later, that I take the trouble for the first time to subject this episode to conscious attention, and clearly formulate its long-rejected meaning.

Here I followed one of the threads, probably the strongest, of the associations that came my way. I made it

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as for "projection towards a goal", which is one of the dominant traits of my "self", it's also perhaps the only aspect of my person through which I've managed to be even more yang than my mother!

<sup>158</sup>(\*\*) (November 26) Although the basic tone has remained one of sympathy and attraction, since I left, over the years and more and more, this relationship has become more and more *strained*, sclerotic, drained of what gave it quality of life. I feel as if I'm standing in front of a "shell" that's so perfectly watertight, that nothing gets through either way. See "Two turning points" and "The tomb", n°s 66, 71.

However, I realize in retrospect that this is not the case. Without doubt, the image of a person and their temperament that emerges spontaneously from the description of concrete situations in which they find themselves involved, is more vivid and convincing than an enumeration of "traits" that are supposed to define them. Rather than launch into this, I'd prefer to note yet another association, and engage in -another digression, by comparing the relationship examined here with that between Serre and myself. In terms of the relationship between our persons, the impression that prevails for me is by no means that of a "**complementarity**" as with Pierre, but rather that of an **affinity** between two temperaments, each strongly "yang" to the other. More than once, in the eighteen years of close mathematical communication, this affinity has manifested itself in occasional frictions, expressing itself in passing chills, none of which have lasted long. As I remember it, these episodes were caused by casual impatience on Serre's part, which didn't sit well with my own susceptibility. Sometimes Serre was annoyed by my stubborn pursuit of an idea against all odds, when it seemed important to me. I'd bring it up at every opportunity, without worrying whether it would "pass" or not, strengthened as I was by the conviction (which was rarely mistaken) that I had "**the** right" point of view. I don't know why Serre had developed an aversion to my cohomological "big fuss" - perhaps he was simply allergic, like André Weil, to all "big fuss". On the other hand, when I began to develop "my" cohomological yoga, in the second half of the fifties, Serre was practically my only occasional interlocutor.

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so it wasn't looking good! I  $\square$  crois that he only consented to take a cautious interest in these works, and only began to realize they were getting somewhere, with the development of staggered cohomology from 1963 onwards, followed the same year by my sketch of a demonstration ("in four spoonfuls") of the rationality of  $L$  functions<sup>159</sup> (\*).

It seems to me that the relationship between Serre and me was typical of a yang-yang affinity, unlike the relationship with Deligne, which was a yin-yang complementarity. In terms of mathematical work and approach, however, the situation was reversed. As I said in a previous note ("Les neuf mois et les cinq minutes", (123)), I feel that Serre's approach and my own are **complementary**, in the sense of yang-yin complementarity. It was this very complementarity that gave rise to occasional friction, due to the strongly yang temperaments of both him and me.

The relationship between Deligne's and my approaches to mathematics was quite different, to be sure. I can say, without reservation, that it was with Deligne, more than with anyone else, that I had the experience of a perfect **affinity** in our ways of seeing and approaching the mathematical questions that interested us both. This experience has been renewed every time there has been a mathematical dialogue between us. It's clear to me that this is by no means a coincidence - due, for example, to the influence I exerted on him during the decisive years of his apprenticeship. This affinity didn't develop over a long period of familiarity - on the contrary, it was present right from the start.

our first contacts, which was the force at work in creating, almost overnight, a bond of such strength, rooted in our shared passion. It's about a deep affinity between two approaches  $\square$  de

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<sup>159</sup>(\*) Another point of friction I remember, probably even more episodic, was my insistence on linking the theory of quotient transition in algebraic groups and formal schemes (still poorly understood in the 1950s) to questions of the "effectiveness" of flat equivalence relations, or even (later) to quotient transition in the context of fpqc bundles. These views, first taken up by Gabriel and Manin, are now commonplace just about everywhere in algebraic geometry and even elsewhere. It seems to me that Serre's reluctance dissipated, as soon as I took the trouble (as no one else seemed willing to do) to prove in black and white the first effectivity theorem, for flat and finite equivalence relations.

mathematics, pre-existing at the time of our meeting, and which express (I'm convinced) an important aspect of the original temperament in both of us - a yin "basic tone" in the apprehension and discovery of things<sup>160</sup> (\*).

There's no question of "demonstrating" such an intimate conviction, any more than I'd dream of wanting to "demonstrate" that the basic tone in my own mathematical work (let's say) is yin, "feminine". At the very most, it's sometimes possible for such things to "pass on" a feeling from one person to another, and trigger in others an awareness of something to which they had not previously paid attention; something that had escaped their conscious attention, yet was already "registered" somewhere, in diffuse form. The situation is surely blurred, as it so often is, by the efforts made by the person concerned to mold himself according to the values in honor, the yang, "masculine" values. Whereas I can see that his mathematical work and the (considerable) influence he exerted are profoundly "masculine".

marked by his ambiguous relationship with me, I doubt, however, that the efforts in question to erase

a basic temperament akin to mine, recused - that these efforts were crowned with success. Certainly the

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The same rigorous attitudes that didn't apply to him before my "departure" have long since prevented him (at least in writings intended for publication) from examining things that are too far beneath him, or things that are now anathema. Yet it seems to me that in what he publishes, he has not been able to refrain from following the style of approach that is spontaneously his own. At least, that's the impression I got as I leafed through the few parsimonious offprints he was still kind enough to send me from beyond the grave, after my "death" fifteen years ago.

But of course, my understanding of Deligne's mathematical approach goes back to the years before my "death", between 1965 and 1969. For five years, we were both strongly connected to the same things, and mathematical communication was uninterrupted (except for a year he spent in Belgium), and more intense than that I had with any other mathematician, including mime (it seems to me) Serre. More than once, I've had occasion to evoke those years<sup>161</sup> (\*), of intense creativity for both of them. In my friend's case, they were marked by an impressive start, which didn't surprise me because it seemed so obvious! It was a time when his very sure sense of substance, of what is tangible behind the most abstract appearances, or in the most "general nonsense" formulations, was not yet obscured by complacency, nor by the burial syndrome that later appeared. He then made numerous contributions to those themes (extreme-yin, I might say) that later consensus (with his unreserved blessing) had long since excluded from the ranks of "mathematics".

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<sup>160</sup>(\*) (November 26) The reflections of the present note, in continuity with those of the notes "The rising sea" and "The nine months and five minutes" (n°s 122, 123), seem to suggest for any person the presence of a "double signature", or a **double** "basic tone": one (undoubtedly the most apparent) concerns the "boss", i.e. the structure of the "ego" and the mechanisms that govern it; the other concerns the "Worker", aka the "child", i.e. also the drive for knowledge, discovery of the world, creation (including, of course, the drive for love). (It's true, it's the most common thing in the world to take the boss for the worker and vice versa, in other words, to take bladders for lanterns - but that's another story... ) So for me, this basic double tone is yang(boss)-yin(child), for Serre it's yang-yang, for Deligne it's yin-yin (without my having any feelings of doubt or hesitation about it). Against the background of sympathetic relations with one and the other, it's this "distribution" of "signs" (or "tones") which means that, in terms of relations between people, my relationship with Serre is one of affinity and my relationship with Deligne is one of complementarity.

approaches to mathematics.

Of the four possible "distributions", only the yin-yang double tone remains. Given the disfavor of yin in our macho society, a disfavor that will tend to play especially on the first tone (the "boss tone"), I presume that the yin-yang double-tone must be less frequent than yang-yang. Yet I know at least one notorious mathematician who seems to fit this signature. Of course, the second tone, or "original tone", is trickier to pin down, as it will often be "blurred" by outside influences, by the desire to be and do "like everyone else".

<sup>161</sup>(\*) See in particular the notes "L'enfant", "L'enterrement", "L'éviction", "L'investiture", "Le noeud" (in Cortège V, Mon ami Pierre), and the note "L'héritier" (in Cortège IX, Mes élèves).

p. 619 serious"<sup>162</sup> (\*\*): topos formalism, cohomological "big stuff"... . I review and edit pins these contributions, with obvious pleasure, in the introduction to SGA 4<sup>163</sup> (\*). Other such contributions (among even more "muscular" ones, which immediately placed him among the "big stars") can be found in my 1968/69 double report, referred to in the note "The investiture"<sup>164</sup> (\*\*).

**Note** 134<sub>1</sub> (November 26)<sup>165</sup> (\*\*\*) Typical detail: these military funds, about which nobody wanted to lift a finger, as long as there was talk that they would be the cause of my departure, were suppressed the very year of my departure to general indifference! You never know, in case it upsets a distinguished guest who's a bit fussy about the matter... . The funds in question represented only a small part of IHES resources (5%, if I remember correctly). Without having to consult each other, my four colleagues at IHES (not counting the director) were unanimous in seizing the opportunity to get rid of me (almost at the same time, in fact, as the director himself). And I thought I was indispensable, and loved!

(December 6) The two physicists at IHES, Michel and Ruelle, were unhappy that the "Physics" section at IHES was somewhat of a poor relation, next to the mathematics section, represented by Thom, Deligne and myself (including two Fields medals!). This imbalance had just been exacerbated by Deligne's co-optation (which, incidentally, had been done with the unreserved agreement of Michel and Ruelle, unanimously in of the IHES Scientific Council, with the exception of Thom). There had been consultation between physicists and mathematicians at the IHES, to put pressure on the director, Léon Motchane, to re-establish fairness.  
p. 620 balance between the two sections, as far as possible. I assume, however, that my physicist colleagues must not have been unhappy to see this imbalance effectively offset, and much sooner than they would have hoped, with the sudden prospect of my departure.

As for Thom, he was incensed that Deligne had been co-opted against his formal opposition. He had described Deligne's contributions, all unpublished, which I mentioned in my glowing "investiture" report, and which obviously went over his head, as mere "exercises"! What shocked him about Deligne's accession to "permanent" status at IHES, on an equal footing with himself, was that the young Deligne - he was 25 at the time - wasn't already covered in honors. According to Thom, such a position should come only as "the crowning achievement of a career". It was a far cry, less than ten years later, from the heroic years when I welcomed a still unknown Hironaka into my makeshift premises... . In any case, Thom's bitterness was such that (according to what he told me himself) he was thinking of leaving the IHES, to return to his professorship in Strasbourg, which he had been careful (more cautious than I had been, when I left the CNRS for the IHES) to keep. Through my warm sponsorship of Deligne, I had been the first and foremost cause of his frustration, and I presume that Thom must have felt, in his heart of hearts, that I had only got what I had deserved through my impertinence, by

<sup>162</sup>(18.5.4.4\*\*) (November 26) Incidentally, some of this mathematics was exhumed, loudly and without my name being mentioned, at the "Colloque Pervers" in 1981, and the following year with the "memorable volume" LN 900. On this subject, see the notes "L'Iniquité - ou le sens d'un retour", "Thèse à crédit et assurance tous risques", "Souvenir d'un rêve - ou la naissance des motifs", n°s 75, 81, 51.

<sup>163</sup>(\*) (November 26) These comments had been added to a second, completely revised edition of SGA 4 (especially concerning sites and topos). They may give the impression that Deligne had been associated with the birth of the main ideas and results that constitute the "powerful tool" of étale and *l-adic* cohomology. So I've added my voice to that of Deligne and my other cohomology students, sharing (ten years later) the remains of a deceased master!

<sup>164</sup>(\*\*) This double report is reproduced in volume 1 of *Réflexions Mathématiques*.

<sup>165</sup>(\*\*\*) This sub-note to the previous note ("Brothers and spouses - or the double signature" n° 134) is taken from a footnote to that note. (See reference at the end of the third paragraph of this note).



seeing himself forced to leave the IHES just a few months after having introduced my brilliant "protégé"!

As for the director, at a time when he found himself cornered by the unanimous desire of the permanent staff to leave, he then (according to a tried and tested tactic he wielded to perfection) played the "divide and rule" game, using the question of the military funds as a convenient means of distraction, and at the same time getting rid of the most troublesome of his permanent staff. (A masterly reversal of fortune, since the secrecy he had maintained around the presence of these funds seemed to me to be an additional and compelling reason to force him to leave!

and his departure from the IHES closely followed mine - from someone who, like him, had been part of the IHES from its precarious and heroic early years, and who' <sup>□</sup> with him and according to p. 621 its own resources, had ensured its credibility and durability.

### 18.2.8.3. (c) Yin the Servant, and the new masters

**Note 135** (November 26) Among the many affinities between Deligne and myself, in the years before my departure, was the pleasure he took, as I did, in developing (when the need arose) what I call "gros fourbis". Most, if not all, of my energy as a mathematician was devoted to such tasks. If it were a question of building a house, doing "big jobs" would mean : not just making a tantalizing sketch of the house, or even two or three from different angles, or even detailed plans, with dimensions and all; but bringing and cutting the stones that are to be used to build it, one by one; assemble them into walls, lay the beams, rafters and tiles or lozes; fit doors and windows, washbasins, sinks, drains and gutters; and install (if you're really going to live in it yourself) right down to the curtains on the windows and the drawings on the walls. It can be a large house, or a one-room cottage - but the spirit of the work is the same. And as long as you live in it, no matter how thoroughly you've done everything, you soon realize that the work is never finished, that there's always something new to come - at least when the "grosse fourbis" - sorry, the house - is vast.

Most of my energy as a mathematician, between 1955 and 1970, was devoted to starting up and developing à brin de zinc four **big** "gros fourbis" - without, of course, having reached the end of any of them, see above. These are, in chronological order, the cohomological tool, schemas, topos, motifs<sup>166</sup> (\*). These four master themes are, moreover, intimately connected one <sup>□</sup> to the other, as would be distinct buildings p. 622 all part of the same farm or hamlet, all working towards the same goal. And each of these "big edifices" led me, without any intention on my part, to develop other "big edifices" that were already much smaller - a bit like building a large house, or even a whole hamlet, which leads you to install a lime kiln, a carpentry and joinery workshop, and so on. A few examples,

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<sup>166</sup>(\*) The "cohomological tool" didn't wait for me to exist. I'm referring here to a certain personal approach, which led in particular to the "mastery of étale cohomology" (which seems to me the main technical and conceptual ingredient in the demonstration of Weil's conjectures, completed by Deligne). Twenty years later, in "A la Poursuite des Champs", I'm pursuing this line of research in the direction of "non-commutative" (or "homotopic") cohomology. For the "commutative cohomology" direction, I give some details about this approach in the beginning of the note "My orphans" (n° 46). The four "big fourbis" referred to here correspond essentially to the five "key notions" in the note quoted, except that the "cohomological tool" corresponds to **two** such notions or ideas (namely, derived categories, and the "six operations" formalism).

Interestingly, the only one of the four "big fourbis" (or main research themes) named in my Eulogy (see notes n° 104 and 105) are topos. As chance would have it, this is also the one of the three buried by my cohomology students, the one that had not yet been exhumed under the paternities of rechanges, at the time of the Funeral Eulogy. (The latter takes place in 1983, the derived categories are exhumed in 1981 at the Colloque Pervers, and the motifs in 1982 in the "memorable volume" LN 900).

every year, the need was felt once again to augment the arsenal of categorical notions and constructs with two or three (small) additional "big eats". People who've come ten or twenty years later, who've found everything ready-made and are comfortably settled in (and even others who basically know what they're dealing with), shrug their shoulders condescendingly at so much unreadable "nonsense" (Deligne dixit) and eh-four hair-splitting ("Spitzfindigkeiten", as an illustrious German correspondent called them, albeit well-disposed towards me<sup>167</sup> (\*)). These are people who have no idea what it's like to build a house on level ground, and who will probably never build one, content to play landlord in those that others have built for them, with both hands and with all their heart.

p. 623 □ I was a bit brisk just now, seeming to put my friend Pierre in the bag of those who "have no idea of what it's like to build a house...". Not only did he see me at work, but it was with pleasure that he built houses of his own, as if he'd never done anything else in the twenty years he'd been in the world. Incidentally, this story of "big forks" and house-building and all that (in case the reader hasn't already noticed. . . ) is yet another aspect, or another image, to capture something I had previously tried to grasp as best I could with the image of "the rising sea", then with that of a train of waves following one another<sup>168</sup> (\*). This is the "yin mode", or "feminine" mode, of apprehending reality, and the corresponding approach to immersing oneself in it and extracting an image that renders this reality with suppleness and fidelity. So here I am, taking a detour via my own person, to return to my initial purpose - that of "conveying" this strong perception within me of a kinship, an essential affinity between Deligne's approach to mathematics and my own. But in this aspect of Deligne's work, which I've just tried to define with the help of an image, there was a complete "blurring", it seems to me, after my departure-death in 1970 - I believe that the "big fourbis" are totally absent from his "after" publications. Certainly, he could not reasonably have used this trait in his disowned master, to debunk the latter, while tolerating the same trait flourishing in himself, in accordance with his own nature.

It's true that if it's not a question of following an inner need, the expression of an elementary impulse, but simply of increasing one's prestige through the accumulation of **results** that "make a mark", my friend really had no interest in continuing to embarrass himself with (more or less) "big stuff". Even in my day, and outside the Bourbaki group (itself involved in a sizeable "gros fourbis"! ), it was already a rather frowned-upon thing to do. This is hardly surprising, given that the "superyang" blinkers, in our society and in the world around us, have become the norm.

p. 624 in the consensus of the scientific world, didn't start yesterday. Perhaps this was the main reason why the houses I took pleasure in building remained uninhabited for <sup>long</sup>□years, except for by the bricklayer himself (who was also the architect, carpenter etc.). And to this day, even the part of my work that has long since become common heritage (and even where there is still no other reference available than my writings), remains surrounded (at least for those who are not part of the "beau monde" and who make no point of looking down on it) by an almost awe-inspiring halo, as if entering it would require almost superhuman faculties. It's true that it's often long, and it couldn't be otherwise, given that everything is well and truly done, by hand and in detail, from start to finish, with even

<sup>167</sup>(\*) My correspondent kindly assured me, just to please me, that he was well aware that my work was "largely free of such defects" ("weitgehend frei von diesen Übeln"). For him, these were the "tares" into which one could not fail to fall (like the "Spitzfindigkeiten" of categorists of all stripes), if one were to develop a theory (as I suggested with regard to motifs) on foundations that would still remain conjectural. Here we find the visceral rejection of the "mathematical dream" discussed in the section "The forbidden dream" and in the following three sections (sections 5 to 8). It's yet another aspect of an automatic repression of any "yin", "feminine" approach to mathematics.

<sup>168</sup>(\*) See the two notes "The rising sea" and "The arrow and the wave", n°s 122, 130.

at every turn of the chapter, explanations of where we're going<sup>169</sup> (\*). It didn't seem to me that my students, when they were working with me, had much trouble getting into the swing of things. But that was at a time when "tangible results" had already won over the mathematical establishment, and my students worked with the confidence of playing a "safe" card. I have the impression that, since then, more and more people have taken pleasure in accrediting the "unreadable" version<sup>170</sup> (\*\*), in accordance with a fashion that is even more tyrannical today than it was in my day.

But even setting aside the desiderata of fashion, when it comes to calculating profitability and "returns", we're bound to be careful to avoid "big stuff" like the plague. Developing "big stuff" and making it available to everyone is a **service** to a scientific community that often accepts it.

reluctantly. I've never been too bothered by this understandable reticence; I've always been a bit

knew that I had "the right stuff", and that sooner or later, people wouldn't be able to stop coming.

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But even as they come, the "returns" in terms of "credit" can only be modest. If I were to draw up a numerical balance sheet, not of the notions, questions and ideas I introduced and developed in the fifteen years from 1955 to 1970, and which have either become part of the common, anonymous heritage, or are buried without music (waiting to be exhumed with great fanfare), but of what might be called "great theorems", I doubt I'd find even ten. Perhaps the total time directly devoted to their demonstration is of the order of a few weeks, or a few months at the most. There wasn't one until 1957 (Riemann-Roch-Grothendieck theorem) - and yet I know I hadn't wasted my time in the three years before that. In fact, none of the "great theorems" would have been demonstrated by now (although this was by no means my main concern), if I hadn't stubbornly followed a passion for understanding within myself for these fifteen years, trusting the approach it dictated, whether or not it was "profitable" (in terms of such and such desiderata), or whether or not it was well regarded in the wider world. Each time, this approach consisted of starting with a strong intuition, or a handful of such intuitions, and taking them as a solid, foolproof thread that pulled me into the unknown; and in doing so, to change the image, I couldn't help but gradually, with the unknown in the process of making itself known, like rough stones that you "know" by cutting them, build houses, some very large, some not so large, and all fit to be lived in - houses where every nook and cranny is destined to become a welcoming and familiar place for more than one person. Doors and windows are plumb and open and close without cracking or creaking, the roof doesn't leak and the chimney doesn't pull. It doesn't have to be Notre Dame de Paris, and there's no "great theorem" hidden in everyone's breadbox - these are simply houses that had to be built, and that I built to be lived in. I found my joy in making them, beautiful and spacious, knowing full well that the work I was doing, alone or in company, had to be done, and that at each moment it was as good as I could make it.

It's this spirit, too, that I found in the Bourbaki group in the 1950s, and which made me feel at ease, "at home", notwithstanding the differences in background and culture, and the difficulties.

that I mentioned earlier. In those days at least, it was a  $\square$  esprit de **service**, still, that

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<sup>169</sup>(\*) It's only over the years, I think, that I've come to realize the need to include such explanations, often purely heuristic, to try as far as possible to communicate to the reader a sense of "direction" and purpose, strongly present in me at the time of writing. Today, this seems far more essential than meticulous writing of key demonstrations, which the reader will be happy to reconstitute or even construct from scratch, as soon as he senses where we're going, and that "where" attracts him... ..

<sup>170</sup>(\*\*) This is obvious only to Deligne, who repeated it to me during his recent visit. This was SGA 4 (more than half of which develops the language of topos with extreme meticulousness), declared "unreadable" by my friend, as justification for his brilliant "SGA 4 operation<sup>1</sup>".

I found there. Service to a **task**, and beyond the task, service to other men, eager like us to understand things small and great, and to understand them thoroughly and to the end. This "service" did not take the form of austere duty or asceticism. It arose spontaneously and joyfully from an inner need, expressing something in common that linked these very different men.

And it's this same spirit that I recognize in the Cartan seminar, where so many French mathematicians got their start, and later (in the 1960s) in my own seminar (which goes by the acronym SGA, "Séminaire de Géométrie Algébrique du Bois Marie"). One of the differences between the two seminars was that mine were strongly focused on the development of the "big fourbis" mentioned earlier (i.e. "**my**" fourbis), for which there were never too many hands, whereas Cartan's themes from one year to the next were more eclectic. What seems more important to me is what the two seminars had in common, and above all, what seems to me to have been their essential function, their **raison d'être**. To tell the truth, I can see two of them. One of the functions of these seminars, close to Bourbaki's purpose, was to prepare and make available to everyone easily accessible texts (I mean, essentially complete), developing in a detailed way important themes that are difficult to access<sup>171</sup> (\*). The other function of these seminars was to provide a **place** where motivated young researchers - even if they weren't geniuses - could learn the trade of mathematician on topical issues, in contact with eminent and benevolent men. Learning the trade - in other words, getting down to work, and in the process, finding an opportunity to make a name for themselves.

It would seem that my departure in 1970 marked the end, in France at least, of the "major seminars".

- **sustainable** places where, year after year, some of the great themes of contemporary mathematics are being worked on - and places that are also **benevolent** and inspiring, for ☐all those who come to put their minds to it.

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hand. I don't know if they exist anywhere else in the world (Moscow, perhaps, under the impetus of I.M. Gelfand?). What is certain is that such places are decidedly contrary to the zeitgeist, just like the "big fourbis", written out in black and white, meticulously, for **all** to see.

It's no coincidence that hardly anyone writes careful and (for the time being) ex-hausive essays on topics that have been ripe for discussion for ten or even twenty years, and which are clearly crucial, but which in the meantime are only accessible to a handful of people "in the know". Those who are part of the mathematical "big world", unless they are also part of the "handful" in question, will have no difficulty whatsoever in being brought up to speed by one of them, who will be only too happy to oblige. As for the rest of us, what the hell! Back in the sixties, I saw a lot of books clamoring to be written. I'd have written them myself, but I couldn't do everything at once. To my knowledge, none of these books has yet been written<sup>172</sup> (\*). However, I know more than one person (even if only among ex-students) who was in the know enough and had the feeling and the knack, to be able to write without difficulty such a book as was needed (and still is). And from the little I've seen of some of their later work, I don't get the impression that it's the abundance and difficulty of their more personal work that's made them so successful.

<sup>171</sup>(\*) "Difficult to access", either because these themes remained imperfectly understood, or because they were known only to a handful of insiders, and the scattered publications dealing with them gave an inadequate picture.

<sup>172</sup>(\*) (November 28) I should make an exception here for theses written at my instigation. The spirit that animated me and which I believe communicated itself to my students, at least during the time they worked with me, was that which animated me for my own work; that is to say, in colorful terms, "to build the houses" that were visibly needed, even if I was often the only one to feel the need for this or that particular "house". I have the impression that, as a general rule (with one exception), this feeling was eventually communicated to the student, making him or her "hook" on a particular subject, and subsequently identify strongly with it. With the exception of Verdier, who didn't deign to make available to everyone the foundation work agreed between us and which is still waiting to be written, the thesis work of all the students who did their state doctorate thesis with me has become what you might call "standard references". They are houses fit to live in, and none of them duplicates any other. ...

would have prevented him ("I'm sorry, but I really don't have the time!") from providing this service to the famous "mathematical community". For more than one, too, it's a safe bet that it would have made him even more notorious, like author of a book read and quoted (even if not everything he expounds necessarily □ from him - but the "how" p. 628 is by no means a negligible quantity. ... ), than by the more or less thick bundle of its separate prints.

Clearly, it's not a simple "lack of time" that's preventing some of us, with impressive unanimity, from making accessible to all what remains the privilege of a few - or even from having (if only here and there, let's say, the time to write a book) an **attitude of "service"**. At this point, I'm irresistibly reminded of the SGA 5 seminar in 1965/66, which, for eleven years, had been ignored for personal gain by the very people who had benefited from it in the first place, my friend Pierre and my other cohomology students! It's true that there was a body to share, and therefore a special motivation in this case. But I'm also thinking of other cases, where the service rendered made up for obvious shortcomings, and was brushed aside by the people in place<sup>173</sup> (\*). People will say that these are still rather special cases, that it was my person who was targeted, when it was obvious that it was I who had inspired the work in question. And yet, I can sense a "zeitgeist" in all of this that transcends any individual case.

The aspect of the "zeitgeist" that I'm trying to pin down here, as best I can, is the **discrediting of an attitude of service** - a discrediting that I perceive through a host of converging signs, and which for me is a patent fact. Everyone is free to deny it, just as they are free to examine it for themselves and see for themselves. My purpose here is not to "prove" it to a reluctant reader, but to try to grasp its meaning.

From the point of view of this reflection, the first meaning is obvious. The attitude of service is typically a "yin", "feminine" attitude, and it's not surprising that it's one of the many that find themselves devalued. The nuance, which I've seen many times, is that such an attitude was just the right one. good for those who couldn't afford a "master" attitude - that work done in this spirit

□ was **subaltern** drudgery, good for the pedestrian among those who coach big ideas and p. 629 brilliant discoveries".

However, I also know that there's more to it than that - because otherwise, why should we prevent at all costs a "pedestrian" of good will (when by chance there are any) from quietly doing in his corner the dirty work that is his by right, finally providing solid references where previously we had to be content with saying (when we deigned to say something. . . ) "we know that. . . " or "we can demonstrate that... .", or more rarely and more honestly "we'll admit that... . " ? !

I was first confronted with this troubling question eight years ago, when Yves Ladegaillerie was trying to "fit in" his thesis<sup>174</sup> (\*). It was, I confess, at a time when my interest in both mathematics and the world of mathematicians was at its most marginal. I was a little bewildered, but I didn't try to unravel the meaning of this mystery. With a few variations, my attitude hardly changed in the years that followed, until last February, when I continued to reflect on the subject in Récoltes et Semailles. And yet, by dint of picking up on signs, and even if I didn't mean to, I couldn't help but gradually pick up on their meaning, or rather, meanings. I can see two of them. One has to do with me - the burial syndrome, which I haven't quite got round to yet. The other has nothing to do with any particular person. It's about an **attitude of exclusivity in the possession and control of scientific "information"**, an attitude that prevails within the scientific community.

<sup>173</sup>(\*) I'm thinking here, of course, of Yves Ladegaillerie's work, and that of Olivier Leroy, mentioned in four previous notes and sections ("On n'arrête pas le Progrès", "Cercueil 2 - ou les découpes tronçonnées", "La note - ou la nouvelle éthique", "Cercueil 4 - ou les topos sans fleurs ni couronnes", notes n°s 50, 94, section 33, note n° 96).

<sup>174</sup>(\*) On this subject, see the two notes °s 50 and 94, cited in the previous footnote.

of the scientific "establishment", making it a kind of ruling caste by divine right, within the so-called scientific "community"<sup>175</sup> (\*\*).

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This is a theme I've already touched on (barely, barely) in the note □ "Deontological consensus - and information control", and a little also in "Le "snobisme des jeunes", ou les défenseurs de la pureté" (25), (27)). I suspect that this is a **new development** in the scientific world, which has come to take hold at a snail's pace over the past two or three decades. I don't think I was among those who propagated and welcomed this unwritten "new ethic" of double standards<sup>176</sup> (\*). If I have any co-responsibility for its advent, it's rather that I didn't see it coming<sup>177</sup> (\*\*). Before these last few years, I had no idea that the all-round information I had enjoyed freely, practically since my first contacts with the scientific world in 1948, had over the years become, I'm not sure when or how, a far-reaching **privilege** shared with a handful of friends - a **class privilege**, to use an overused term, which here seems to express a very tangible reality.

But my aim is not to make a "class analysis" of the mathematical world, and the "relations of force" and the "means of power" in this world - no more than to paint a "picture of manners". It's time to return to □ a more limited purpose - that of understanding, in its essential springs in the main protagonists, the "news" of my early funeral!

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#### 18.2.8.4. (d) Yin the Servant (2), and the generosity

**Note** 136 (November 28) The previous two notes were essentially digressions around the theme of the yin-yin affinity between Deligne and myself, in terms of mathematical work and approach to mathematics. I don't know whether they helped to "get across" my perception of this affinity and its nature, which for me is beyond doubt.

I've written elsewhere that "in my work, I'm as "yin", as "sea and motion", as one can be". On reflection, I'd say that's not literally true - that one "can be" even more so, because (as I see it) Deligne is even more so than I am. Or at least, the "yang in the yin" seems more pronounced in me than in him. What's fiery in me, takes on a more measured air in him. Where I boldly launch myself forward, more often than not he will remain cautious, and often well-founded, in his expectations. As long as I have the beginnings of an idea, a "tip" I can get my hands on, I don't hesitate to throw myself into a mathematical quagmire that I feel is substantial, without bothering to take a closer look at the initial idea ("ihr auf den zahn fühlen", as they say in German. . . ), or to predict the outcome of the melee. Sometimes the idea just doesn't make sense, for some obvious reason that escapes me because I'm so keen to "jump into the fray". Eventually I realize it - sometimes I feel like an idiot, and yet I've rarely regretted taking the plunge. That's how I make contact with an unknown substance - by rubbing up against it, whether "advisedly" or not.

<sup>175</sup>(\*\*) (December 6) Lust for domination is a **superyang** imbalance, and by far the most common form of such an imbalance. It corresponds to an obliteration of the yin term, "feminine" in the yin-yang couple "Master-servant", or "that which dominates (or masters) - that which serves", neighboring the couple "mastery - service".

<sup>176</sup>(\*\*) I don't know if there are many among the elders or colleagues of my generation, or even among younger colleagues and friends, who have seen it. I doubt if there is a single one of "those who welcomed me fraternally, into this world that became mine", to whom Récoltes et Semailles is dedicated - apart perhaps from Chevalley. That's certainly one of the things I'd have liked to talk about with him - but he's no longer here to tell me. ...

<sup>177</sup>(\*\*) I don't know if there are many among the elders or colleagues of my generation, or even among younger colleagues and friends, who have seen it. I doubt if there is a single one of "those who welcomed me fraternally, into this world that became mine", to whom Récoltes et Semailles is dedicated - apart perhaps from Chevalley. That's certainly one of the things I'd have liked to talk about with him - but he's no longer here to tell me. ...

My friend, on the other hand, first probes and examines - and then launches out, when he feels sure, if not of the point of arrival, which would be asking too much, but at any rate that there's somewhere to land, and that he won't come back empty-handed. I've never had the impression in his work of any kind of **dispersion of energy**, as I often had - but rather that with him, **all the strokes work**. From this point of view, his style of work carried

the mark of **maturity**, whereas mine was more the mark of **youth**, sometimes muddled by the sheer force of it. to be feisty. When we first met, however, I was approaching forty, p. 632

when he was twenty years old. And more than once, I sensed in him a kind of smiling indulgence towards me, the kind of indulgence a benevolent adult would have towards an affectionate child, when he saw me still embarking on some (small) "big job", without ever doubting anything... ..

The aspects I'm evoking here are no doubt difficult to detect in published works "au net", which present a final, or at least advanced, stage of reflection. I'm no less exacting in my work than he was, and I hardly ever entrusted notes to a typist or printer until they had reached a stage where they satisfied my need for complete clarity. On the other hand, in the style of writing I follow in "Réflexions Mathématiques" (and particularly in "A la Poursuite des Champs"), the original approach to the work is apparent on every page. Readers will notice many "misses". They are all small in scale - usually spotted the next day or two, if not the very same day, and rectified in the pages that follow. (That this should be so surprised me - it's one of the signs of the extraordinary "ease" of my mathematical work, which I've mentioned elsewhere<sup>178</sup> (\*).). One of the reasons for the "little misses" is, of course, my lack of familiarity with a subject I hadn't touched for seven or eight years - and these absent-mindednesses become rarer as the work progresses, as the contact gradually lost is re-established. Nevertheless, this way of taking at face value, without hesitation, what a rather nebulous memory gave me of things I knew more or less well at the time, illustrates well this "go-getter" aspect, and sometimes scrambling, which constitutes (among others) the "yang in the yin" aspect of my mathematical (or non-mathematical) work. I'm convinced that an equally spontaneous text from Deligne's pen would be much closer to what is commonly considered "publishable" - indeed, publishable by his own exacting standards.

□ If I insist here on the character of "maturity", of "yin very yin" in the working style and approach of p. 633 I don't mean to suggest that my friend's work is in any way unbalanced, that it lacks or is lacking in "yang" or "virile" qualities. If this were so, his work would not, like Serre's or mine, bear the delicate, unmistakable mark of **beauty on** every page. But this is not the place, any more than I did in the case of Serre or mine, to follow the delicate harmony of yin and yang, of "feminine" and "masculine", in his published work, which is known to me, and in what is known to me of his work through the personal contact I've had with him for nearly two decades.

Nor should we think that my observation of a balance between yin and yang is some kind of truism, that it applies immediately to every man who, in one capacity or another, is considered a "great mathematician". This perception of beauty that I just mentioned is not equally present, nor to the same degree, in the work of all mathematicians who leave a lasting imprint on the mathematics of their time. Among these, I know of two who, like Deligne, appear to me to be predominantly yin in both their work and their personality, and whose work has never failed to impress me.

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<sup>178</sup>(\*) See note "The trap - or ease and exhaustion", n° 99. It seems to me that this "ease" is even greater now than it was before I "left". This seems to me to be linked to a maturation that has taken place in me over the past fifteen years, and which is felt in my mathematical work as elsewhere.

The yin imbalance is so extreme in one of these colleagues that he seems entirely unable to even formulate the slightest definition or statement clearly and correctly. The yin imbalance is so extreme in one of these colleagues that he seems entirely incapable of formulating even the slightest definition or statement clearly and correctly (let alone an idea. . . ) - even though he has a deep intuition for many things, and has introduced a number of important and fruitful ideas. In each case, these ideas have taken shape through the work of others. It's clear that he has a rare repression of "yang" traits and forces, both in his work and in his way of being. This repression takes on the proportions of true powerlessness, even in his work, where he would be incapable of completing anything by himself.

p. 634 means. He compensates for this impotence of being with an attitude of megalomania, internalizing at the same time the defects he delights in cultivating in himself, as if it were **thanks to them** that he could ☐conceive ideas that

(in his eyes) make him **the** great scientist of the millennium. ... <sup>179</sup>(\*)

I sense a repression in the opposite direction in my friend Pierre, evacuating certain "yin" traits and leading him (more or less successfully) to model himself on a superyang image. Admittedly, this repression is a long way from the opposite extreme I've just described. It does not go so far as to erase from the reader or interlocutor the feeling of beauty, of satisfaction without any aftertaste of unease, which are the signs of a true understanding, giving at every moment their fair share to both clarity and shadow, to mystery. In other words, the "superyang" brand image chosen by my friend should hardly encroach on his work itself, at times of work I mean, when the presence of the "boss" should be as often effaced as it is (I think) with Serre, or with me<sup>180</sup> (\*\*).

On the other hand, it seems to me that the role of the boss becomes important, even invasive, when it comes to choosing work **themes**. There's this fixed idea of standing out from my person, and by the same token, the refusal to follow certain inclinations of his own nature which are too strongly associated in him with the image of the disowned master. So, while he may, like anyone else of great means, be able to demonstrate difficult theorems (or even "proverbially difficult" ones), and even to introduce beautiful ideas and develop them, he would never dream of naively "rethinking", in his own way and even if only in broad outline, an entire science (such as topology, which could really use it.... ) - or even to create a new science from scratch, to "unearth new worlds" (as I wrote elsewhere) (136<sub>1</sub> ). And yet, if there's anyone I have no doubt he has what it takes, it's him. If there's anything he's lacked until now

p. 635 to do this is **generosity** - true generosity, which is at the same time a calm assurance, which makes us follow the impulse of our own nature wherever it carries us, without us ☐soucier or encouragement, nor "returns".

But there's also the simple joy of "building houses", big or small, for others to live in, without necessarily having to be on the scale of a "whole science" or a "new world" - the joy of lugging stones and beams around like the first bricklayer or carpenter who comes along, without fear of being mistaken for this or looking like that - or of bringing within everyone's reach what (according to some) should remain the preserve of the very few. It's an attitude of service, a certain humility, another expression of the same generosity mentioned earlier, of the same fidelity to one's own nature. My friend has exchanged it for an attitude of self-importance ("me - to do such a job!") and a caste attitude<sup>181</sup> (\*), in terms of choice.

<sup>179</sup>(\*) I'm talking about attitudes and ways of being that I'd seen before I left, when I had the opportunity to meet this prestigious colleague on a familiar basis. It's not impossible that something has changed since then (although that would be a rare occurrence. . . ).

<sup>180</sup>(\*\*) I revise this hasty impression at the end of sub-note no. ° 136<sub>1</sub> (dated December 4) to this note.

<sup>181</sup>(\*) This "class" attitude, in my friend and in the "great mathematical world", appears in my thoughts first in the two notes (from March) "Deontological consensus - and control of information" and "Youth snobbery - or the defenders of purity" (n°s 25, 27), and it reappears in last week's note "Yin The servant, and the new



acceptable" work themes.

Finally, there is a third attitude or force by which "the boss" weighs in on my friend's choice of work themes, the substance he gives himself to probe, a force that sets him imperative barriers. It's the "master's funeral" syndrome, or **gravedigger's syndrome**. It's not just a question of refraining from naming the one who must remain ignored. It's also a matter of burying the work itself, or more precisely, of "**cutting**" it off cleanly, as if with a **chainsaw**, in one's own work as in that of others, at the level of each of the master branches sprouting from a vigorous trunk<sup>182</sup> (\*\*). As I reminded you the day before yesterday (in the previous note, "Yin the Servant, and the new masters"), among the four major themes that I identified and developed during my period as a "surveyor" between 1955 and 1970, only one was "taken" and used in broad daylight by my brilliant pupil and <sup>succ</sup>cessor, the other three have been "truncated" - muted, p. 636 it goes without saying. One of the themes was partially exhumed in 1981, and another the following year - as if stunted shoots had sprouted from the scarred stumps of severed main branches, surrounded by brightly-colored garlands and neon lights, just to give the impression...

**Note** 136<sub>1</sub> (December 4)<sup>183</sup> (\*) My own approach has constantly led me to "rethink" from top to bottom everything that stands in my way as a mathematician, whether it's the most insignificant thing, or whether it's on the scale of "an entire science". It's true that, having only two arms like everyone else, I haven't always been able to go so far in carrying out a program of work to remake "an entire science from top to bottom", as I did in the case of algebraic geometry, starting from a few very simple key ideas around the notion of the schema. Even in this case, where I invested a large part of my mathematical energy for twelve years in a row, I was far from "completing" the planned program - for that, I would have needed twelve more years! (And no one after I left bothered to carry on with the task, which must have seemed (wrongly) thankless... .)

Other cases in which I have rethought a science, but certainly without going that far, include **holographic algebra** (both commutative and non-commutative - the latter, incidentally, did not yet exist when I first started thinking about it in 1955), and **topology**, with the introduction of the notion of **topos**, which is still waiting for its time to become the daily bread of the geometric topologist, along with the various notions of "topos".

of "spaces" and "varieties" commonly used today<sup>184</sup> (\*\*). No doubt certain important parts of current topology will hardly be touched <sup>by</sup> the systematic development of the p. 637 point.

toposic viewpoint in topology. So this point of view seems to me to be the crucial element in the "creation from scratch of a new science" - a science that achieves a synthesis (entirely unexpected when I arrived in the 1950s) of algebraic geometry, topology and arithmetic<sup>185</sup> (\*). Over and above the construction of the new algebraic geometry, and through the "mastery of stale cohomology" (and that of the l-adic cohomology that follows from it), it was the elaboration of a master builder of this new science still in the making, and the development of solid technical foundations, that was in my eyes my main contribution to the mathematics of my time. The "yoga of **patterns**", which remains

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masters", n° 135.

<sup>182</sup>(\*\*) I was first confronted with the reality of "the chainsaw" on May 19, during the reflection in the double note "Les héritiers... .", " . . and the chainsaw" (n° s 91, 92), then in the four coffin notes that follow (and which, with "Le Fossoyeur", form the "Fourgon Funèbre" or Cortège X de l'Enterrement), on May 21 and 22 (notes n° 93-96).

<sup>183</sup>(\*) This sub-note to the preceding note ("Yin the servant (2) - or generosity", n° 136), is taken from a footnote to this one. (See footnote in third paragraph before end of footnote).

<sup>184</sup>(\*\*) Compare with some of the comments in the second part of the March note "My orphans" (n° 46), and in his sub-notes n° s 46<sub>5</sub> to 46<sub>7</sub>.

<sup>185</sup>(\*) See previous footnote. (March 11, 1985) The term "entirely unexpected" is no doubt excessive, for the prescience of such a synthesis is already to be found in Weil's conjectures, which acted as a powerful source of inspiration.

still conjectural, seems to me to be the soul, or at least the neuralgic part among all, of this new science, so vast that until today I hadn't even thought of giving it a name. We could call it, perhaps, **arithmetic geometry**, suggesting by this name the image of a "geometry" that we would develop "above the absolute base" Spec  $\mathbb{Z}$ , and which admits "specializations" both in the traditional "algebraic geometries" of the various characteristics, and in "transcendental" geometric notions (above the basic bodies  $\mathbb{R}$ ,  $\mathbb{C}$  or  $\mathbb{Q}_l \dots$ ), via the notions of analytic or rigid-analytic "varieties" (or better, **multiplicities**), and their variants.

I see yet another "new science" that I had glimpsed as early as the 1960s, originating in my reflections on homological algebra begun in 1955. It's a vast synthesis of ideas coming from homological algebra (as it developed in contact with the needs of geometry

algebra, or better said, "arithmetic geometry"), homotopic algebra, the topos version of "general topology", and finally the theory (in limbo since the sixties) of (non-strict)  $\infty$ -categories, or, as I prefer to say now,  $\infty$ -fields. I had expected, as something going that this synthesis was going to be taken in hand by some of my cohomology students, in the same way that I would-

p. 638 mencer par Verdier  $\square$  d dont la fameuse thèse<sup>186</sup> (\*) était censé justement aller dans ce sens. It seemed to me that the

The development of a satisfactory common language, with all the desired generality and flexibility, was to be a matter of a few years' work, surely exciting, by a small nucleus of motivated researchers. After a few very fragmentary beginnings in this direction by some of my cohomology students, my departure in 1970 signaled an immediate abandonment of this work program, among many others close to my heart. That's why I came back to some of my ideas, in a 1975 correspondence with Larry Breen, in the hope of reviving a vision of things that I felt were "in the way", and that "everyone" took care to carefully circumvent, whenever confronted with them. In my letters to Larry Breen (reproduced in chap. I of "A la Poursuite des Champs"), I proposed to call by the name **topological algebra** this science still in gestation, which for a decade or two I alone had been glimpsing<sup>187</sup> (\*\*). Finally, in February 1973, with "A la Poursuite des Champs" (The Pursuit of Fields), I set to work, wearily and despairingly, to see someone other than myself tackle a task that had been burning to be undertaken for twenty years.

Clearly, there's no common ground between the "arithmetic geometry" mentioned earlier, and topological algebra, one of whose main roles, in my view, is that of "logistical support" in the development of this new geometry. For this new geometry to reach the stage of full maturity attested (let's say) by a mastery of the notion of pattern, comparable to the mastery we have of stale cohomology, we must no doubt expect several generations of geometers to have tackled it, more

dynamic and bolder than those I've seen at work; not to mention a comparable mastery of **Anabelian  $\square$  algebraic geometry**, which appears to me (along with the motifs) as one of the two neuralgic" parts of arithmetic geometry, now discernible<sup>188</sup> (\*).

p. 639 <sup>186</sup>(\*) On this subject, see the note "Credit thesis and comprehensive insurance", n° 81.

<sup>187</sup>(\*\*) With the exception, at most, of Deligne, to whom I had thought I had communicated a vision, which he hastened to bury with the rest in the wake of my departure. I allude several times in Récoltes et Semailles to this part, the earliest of all, of my overall program to lay the foundations of a kind of "all-round geometry" - notably in "Le Rêveur" (section n° 6) and in the notes "Mes orphelins", "L'instinct et la mode - ou la loi du plus fort", "Le compère" (n° s 46, 48, 63").

<sup>188</sup>(\*) (For some key ideas in Anabelian algebraic geometry, see Esquisse d'un Programme, par. 2 and 3).

By "neuralgic", I mean here a part of this "arithmetic" geometry that brings it intuitions, conductive threads and problems that are entirely new in relation to the "acquis" of the sixties (this "acquis" essentially consisting of a framework and a language, and a homological and homotopic formalism common to the three disciplines encompassed by the "arithmetic" geometry).

Finally, there's a fourth direction of thought, pursued in my past as a mathematician, moving in the direction of a "top-to-bottom" renewal of an existing discipline. This is the "moderate topology" approach to topology, on which I expand somewhat in the "Outline of a Program" (par. 5 and 6). Here, as so often since the distant years of high school, I seem to be alone in sensing the richness and urgency of the work to be done on the foundations, the need for which here seems more obvious than ever. I have the distinct feeling that the development of the moderate topology point of view, in the spirit evoked in *Esquisse d'un programme*, would represent for topology a renewal of scope comparable to that which the schema point of view has brought to algebraic geometry, and this, without requiring energy investments of comparable dimensions. What's more, I believe that such a moderate topology will eventually prove to be a valuable tool in the development of arithmetic geometry, in particular for formulating and proving "comparison theorems" between the "profinite" homotopic structure associated with a stratified scheme of finite type over the field of complexes (or more generally, to a stratified schematic manifold of finite type over this body), and the corresponding "discrete" homotopic structure, defined by transcendental means, and moduli of suitable hypotheses (equisingularity in particular). This question only makes sense in terms of a precise "unscrewing theory" for stratified structures, which in the context of "transcendental" topology seems to me to require the introduction of "moderate" context.

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□ To return to the person of my friend Pierre Deligne, he had ample opportunity, during the years 1965-1970 of close mathematical contact with me, to become thoroughly familiar with this set of geometrical ideas and visions, which I have just reviewed in broad strokes. (With the exception of the ideas of moderate topology, which began to germinate and intrigue me only from the early '70s onwards, as I recall). His role vis-à-vis this vast program was twofold, and in two opposite directions. On the one hand On the other hand, drawing on the ready-made tool of *l-adic* cohomology, and on the ideas (which had remained hidden) of pattern theory, he made remarkable contributions to the development of the arithmetic geometry program. The most important of these are undoubtedly the start of a theory of mixed Hodge coefficients, and above all his work on Weil conjectures and their *l-adic* generalization. On the other hand, apart from the **tools** and ideas that he directly needed for his work (and whose origin he systematically endeavored to make people forget), he did his utmost to thwart the natural development of everything else: this is the "chainsaw effect", of which I had ample occasion to speak in the course of my reflection on Burial, including (allusively) in the preceding note (n° 136). This chainsaw effect has been partially blurred by partial exhumations (in 1981 and 1982), "like stunted shoots that have sprouted... "under the sudden pressure of immediate needs. (These occasional exhumations have just been mentioned again at the end of the previous note). He also did his utmost to constantly give the impression (without ever making it clear...) that the paternity of the ideas, notions, techniques and results he used, and whose origin he was careful to conceal, was his own, when he wasn't generously attributing it to some other of my former students or collaborators.

All in all, after this quick retrospective of what has been so tenaciously truncated and buried by

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arithmetic geometry). Perhaps a third such "neuralgic part", intimately linked to the motives, should be added to the previous two, namely the Langlands-style theory of **automorphic forms**. If I have refrained from mentioning it, it is because of my regrettable ignorance of the theory of automorphic functions. (I don't know whether the opportunity will present itself to remedy this ignorance... )

my friend, I return to the impression that prevailed in the previous note, where I suggested that the interference of the "boss", of egotistical greed in his work, was essentially limited to the choice of work **themes**. After all, the gravedigger's gravedigger's gravedigger's gravedigger's gravedigger's gravedigger's gravedigger's gravedigger is apparent in his work, with very few exceptions

p. 641

And I realize that these "opportunities" are countless! **This gravedigger syndrome** (intimately linked, I'm sure, to the emphasis on superyang values) seems to me □avoir

had a truly "invasive" effect on his work and his oeuvre, in no way comparable to that of his pro-yang options; and this effect is by no means limited to the mere choice of themes, which the "boss" would make available to the "worker-child", only to withdraw on tiptoe. On the contrary, it seems to me that the "boss" hardly detaches himself from the "worker" at all during the work, so worried is he that the latter might forget the imperative instructions; in other words, that the work itself is often invaded by **inner dispositions** entirely foreign to the nature of the work of discovery, which is thrust into the unknown. This is something that was strongly felt many times during the reflection on Burial, and which I tended to lose sight of during my long reflection on yin and yang.

## 18.2.9. The claw in the velvet

### 18.2.9.1. (a) Velvet paw - or smiles

**Note** 137 (December 7) It's been over a week since I've continued with the notes, apart from some housekeeping work (including sub-notes to two of the previous notes). I had to have three teeth pulled (that's what it's like to be approaching sixty. . . ), a necessary but brutal intrusion, which has meant that I've been operating at a slightly reduced speed lately. I took advantage of the situation to fall back on some overdue correspondence. Now everything seems to be back to normal...

In the four preceding notes (from November 24 to 28), I tried above all to identify more closely the relationships of affinity or complementarity between temperament and mathematical approach in Deligne and myself, in order to situate this "reversal" of yin and yang roles, which I thought I had perceived in my friend's presentation of himself and me, at least at the level of their "mathematical" personalities. Along the way, other aspects of reality came to light, concerning my friend or myself, and beyond our personalities, aspects of the world of mathematicians or, quite simply, of the world of men. In the end, it seemed to me that it was the attitude of service, and the signs of the disappearance of such an attitude in the scientific world, that was the most striking new thing to come out of this stage of reflection, as I'm trying to suggest by the name "Maîtres et Serviteur" (Masters and Servants) that I've given it.

p. 642

□To get back to the original point of "situating" a certain reversal, I now feel as if I've closely enough the real situation concerning my friend and me, to follow it up. The first thing to note is that this initial intuition of a reversal of yin and yang roles, which came to me the day after the May 12 reflection "L'Eloge Funèbre (1) - ou les compliments", was indeed correct. It had already become clear, as early as the November 10th reflection in the note "Les obsèques du yin

(yang buries yin (4))" (n° 124), that my friend endeavors to give a supervirile image of himself, and superfeminine me. The question raised in the November 24 note "Le renversement (3) - ou yin enterre yang" (n° 133), was whether this presentation actually constitutes a "reversal" of reality. The "new fact" in the note "The rising sea... ." (n° 122) was that, just as in my friend's work, the tonality my basic approach to mathematics was yin, "feminine", could at times make me doubt it.

The reflection of the last three notes, however, dispelled this doubt. It was already clear from the outset that I had always been perceived by Deligne (as by my other students and ex-students), on a conscious level at least, as very strongly (too strongly perhaps. . . ) virile<sup>189</sup> (\*). But it also became clear that, in the mathematical relationship between Deligne and myself, and against the background of a strong yin-yin affinity, there was also a yin-yang **complementarity** at play (which could be called "secondary", as opposed to this affinity playing the "primary" role), in which it's really me who plays the "yang", virile role, through a "yang in the yin" component that's much more pronounced in me than it is in him.

The deliberate intention I have observed in Deligne, and which seems to me to be eagerly echoed from many quarters<sup>190</sup> (\*\*), seems to me to be ☐ a **deliberate intention to reverse roles**, and more specifically, **yin-yang roles**<sup>191</sup> (\*). It seems to me that this is another important aspect of Burial, adding to the four already reviewed earlier (in the notes of November 13 and 17 "Rétrospective (1), (2)", n°s 127, 127'). It is these five aspects, all of which are undoubtedly intimately linked, that we now need to assemble into a coherent overall picture of l'Enterrement.

To be convincing, such a painting would also need to bring together, in a common perspective, **three successive "planes"**. In the foreground, there is the lone Deligne, Grand Officier à mes Obsèques, non-pupil and non-heir of the master, declared deceased and having no place to be or to have been. . . Apart from the deceased himself (who is, however, only a deceased, a tacit extra), this is clearly **the** central figure in the funeral ceremony. He is closely followed, in the background, by "the bustling group of my ex-students, carrying shovels and ropes" (to quote from memory the enumeration of the Cortèges, in "L'Ordonnement des Obsèques"). Finally, in the third shot, there's the (almost) entire congregation, who have come to celebrate my funeral (and those of the four co-deceased, standing tall in their "solidly screwed oak coffins"), and to lend a hand at the burial.

☐ Between these three planes seems to reign a perfect harmony, a "**Unanimous Accord**," like those we see. The same atmosphere reigns at any other funeral celebrated in the proper manner, between the priest filled with pious compunction, the family of the deceased displaying the tunes of the occasion, and the bulk of the audience, intoning where it should be intoned, and remaining silent where it should be silent, without ever, ever making a mistake.

To continue with this last image, I now see myself placed in the situation (less comfortable than that of the dear departed, decidedly out of the loop. . . ) of one who, faced with such a touching ensemble, would impertinently propose to try to guess the true thoughts and motivations that animate and agitate the one and the other, priest, family and common people of the faithful, behind the airs of solemnity or contrition sedants

<sup>189</sup>(\*) Besides, current values being what they are, I doubt that any scientific prestige can be carried by an image (generally accepted and received) that is not necessarily a "yang" or even superyang image. It's only at an unconscious level, it seems to me, that the "feminine" nature of my approach to mathematics has been perceived both by my friend and ex-student, and by the mathematical public in general (those, at least, who have had any contact with the kind of thing I've been working on).

<sup>190</sup>(\*\*) I'm thinking here of the "puffs of insidious disdain and discreet derision" mentioned in the Introduction (see Intr. 10, "An act of respect"). I shouldn't be surprised, when I see some of the most prestigious of my students setting the tone themselves. The thing that seems common to me in the many "puffs" that have reached me over the years is precisely an affection for condescension towards the strongly "yin" traits in my approach to mathematics and in my work. On this subject, see also the comments in the footnote of June 23, in note no.° 96 "Cercueil 4 - ou les topos sans fleurs ni couronnes".

<sup>191</sup>(\*) The first time this deliberate reversal of roles appears in my thinking, it's the reversal of roles in the master-student relationship, as I'm presented as my student's "collaborator", taking on the role of the **true** founder and master of *l-*adic** cohomology. (See the two notes on this subject, "Le renversement" and "L'Eloge Funèbre (1) - ou les compliments", n°s 68', 104.) It's interesting to note that in the "master-student" "couple", it's indeed the master who plays the yang role (as the giver, or speaker), "active", and the student the yin role (as the receiver, or listener), "passive". Here again, the brilliant reversal effected by my ex-student can be seen as a yin-yang role reversal, in the same direction (yin-yang becoming yang-yin) as that which constitutes the main message of my Funeral Eulogy, a message that appeared in the note "The funeral of yin (yang buries yin (4))".

for the occasion.

The reflection has been going on for some time now, with the main unspoken thread being to prepare what is necessary to apprehend the closest of these three "planes" of the painting - that of the priest in chasuble, sorry, of my friend Pierre Deligne I mean. It is on this plane that I would now like to turn my attention.

Let me say at the outset that one of the aspects (or "strands") of the picture that featured prominently in the note "Le Fos- soyeur - ou la Congrégation toute entière" (n° 97), namely the "reprisals for dissent" strand, seems to me to play only the most effete of roles with my friend, if it even comes into play at all. I've never had

p. 645 At the time, I had the impression that my friend Pierre felt in the least "implicated" by my "dissidence". On the contrary, it was a great opportunity - one he would probably never have dared to dream of - to elegantly rid himself of the presence of a master who was a little too present, in this institution where, at the age of twenty-five, he had just achieved one of the most envied (or at least, the most enviable) positions in the mathematical world. The fact that this dissidence became more pronounced in the months and years that followed, was experienced, it seems to me (perhaps not on a conscious level, but it doesn't really matter), as an even greater godsend, which gave him free rein, without any hint of resistance coming from anyone (as in he gradually came to realize over the years), an impressive "legacy"<sup>192</sup> (\*). He wouldn't have pretended to complain, even inwardly or unknowingly, about this inescapable boon.

pereated! And it seems to me that the same observation must be valid, all things considered, for most of my students "before" (my departure), and in any case, each of my five cohomologist students. If any of them, whether inwardly or more or less clearly expressed<sup>193</sup> (\*), have hinted at a feeling of dissatisfaction, of frustration with my dissidence, I tend to believe that this is in the nature of a **rationalization** of a grave-digging attitude towards his providentially departed master, rather than a **cause** (albeit one of many) of it. What strengthens me in this conviction, as much for my cohomology students "in general", as for their undisputed leader Deligne, is that the forerunner signs of the Burial that was about to occur (if the right opportunity appeared - and, oh unexpected miracle, it did!), are that these signs appeared in the first place. - these signs were already apparent before I left in 1970, and in any case after the famous SGA 5 seminar of 1965/66, destined for the massacre I know. It's no coincidence, surely, that with such a perfect set-up, all five<sup>194</sup> (\*\*) lost interest in the fate of this seminar where they learned their trade, and at the same time, some beautiful mathematics that they were almost the only ones, for twelve years, to have the privilege of knowing and using. I've gone into enough detail on this subject in the course of discussing the fate of SGA 5, to make it worthwhile to say more here. I will only point out, as far as Deligne is concerned, that in three of the four articles he wrote before I left in 1970, the intention to hide, or at least to conceal and minimize as far as possible the influence of my ideas, is clearly apparent, without having waited

<sup>192</sup>(\*) See, on the subject of this "inheritance", the note "The heir" (n° 90) and the sub-note (n° 136.)) of the note "Yin the Servant (2) - or generosity" (n° 136).

<sup>193</sup>(\*) The only one of my ex-students to have expressed a sentiment in these tones (with a certain reproving nuance to boot) was Verdier, about a year ago. Back in the days of *Survivre et Vivre*, however, he seemed to sympathize with my dissent. There was even an episode of cordial collaboration with his wife Yvonne, on the occasion (if I remember correctly) of the organization of a traveling exhibition on the initiative of Robert Jaulin (of whom Yvonne had been a student), which I joined as a survivor...

<sup>194</sup>(\*\*) (December 12) I should, however, single out J.P. Jouanolou, who ended up writing three consecutive seminar papers, developing notions and techniques that he would need directly and immediately for his own thesis work.

my "dissent".

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□What, then, is the root and particular nature of this attitude of antagonism, of avid competitorp to supplant, to erase, in my friend towards me - an attitude that coexisted with an affectionate and trusting sympathy, and a communion at the mathematical level, from the very first years of our meeting? I I'm even convinced that it must have been present in the background from the moment we met, and probably even before; and also, that it arose much more from the role I was to play for him, than from any particularity in me - if not all the "particularities" that made it possible for me to play this role for him. It's also the role he's been trying to erase for the last twenty years, surely it implied, without any attempt on either side, and by force of circumstance, a "parernal" aspect. And there's no doubt in my mind that it was around this aspect that the conflict arose - a conflict that already existed in him, long before he ever heard my name or even (doubtless) the name of our common teacher, mathematics.

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This conviction, to tell the truth, is not the fruit of reflection, and even less would I pretend to "demonstrate" it. Rather, it came to me over the years, after I'd left - I can't really say when or how myself; little by little, I think, by dint of signs large and small, none of which I dwelt on, even for a moment, but all of which, together, ended up leaving the trace of a knowledge, diffuse and imperfect to be sure, but a knowledge nonetheless, that was there one day... . I could undoubtedly, through painstaking work uncovering half-buried memories and probing them one by one, deepen and materialize this knowledge that remains somewhat imponderable; and it is quite possible (and even probable) that such work would hold many surprises in store for me. Yet I don't feel motivated to do it. This is probably because (rightly or wrongly) it seems to me that this is not really my job, but my friend's - that what I'm probing here concerns him much more than it concerns me. As far as I'm concerned, this intuition or "knowledge" or "conviction" that I've just formulated is enough for my present desire to understand, and I rely on it without reservation.

As so often in my life, I am confronted here with a relationship□of antagonism to the father, where I figure surrogate father, "adopted" father (much more so, it seems to me, than "adoptive" father<sup>195</sup> (\*)). This, plus my friend's deliberate reversal of yin-yang roles, is immediately associated in my mind with the situation evoked in the note "Le renversement (2) - ou la révolte ambiguë" (n° 132) - a situation of which my mother's relationship with her father is for me the most extreme prototype. Yet the differences between the situation in question, and that of my friend Pierre's relationship with me, are immediately obvious. In his relationship with me, I never perceived the slightest hint of "revolt", or even antagonism in the form of virulence, aggression, showing claws or teeth, even in a smile. Of course, there was no lack of smiles on either side, but on his part, they were either smiles of

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<sup>195</sup>(\*) (December 12) As I write these lines, I'm aware of the need for caution when asserting the "non-symmetry" of roles, especially as these are played out on an unconscious level. I presume that at this level, and apart from the mathematical communication itself, I must at some point have entered into the "paternal" role, all prepared by the context. But this role was clearly not of comparable weight, in my life and in my relationship with my friend, to that of my mathematical passion; it remained episodic, and there should be no trace of it after my "departure" from the mathematical scene in 1970. On the other hand, my ex-student's attachment to me, for better and (above all) for laughs, continued to manifest itself over the next fifteen years, both in his work and in the way he maintained, against all odds, an ongoing personal relationship with me.

sympathy (as I felt them), or sometimes innocent surprise, and sometimes almost pain, when he could see (and I ended up feeling the nuance of intimate satisfaction) that certain blows, delivered with a velvet paw, had hit the mark where it was intended.

To put it another way, this antagonism, whether expressed towards myself or towards third parties (when it was a question of reaching out through them to the deceased master, yet still very much present in him. . . ), has always, and without a single exception, taken the extreme-yin form: that which delights (and excels) in reaching out and wounding, even eliminating or crushing, with all the appearances of the most exquisite delicacy. While his deliberate choices for his brand image as a mathematician are superyang (as mine undoubtedly have been, albeit without any more success than his), it seems to me that on the relational level, the basic tone (towards me at least, and those he considers to be related to me) is decidedly and across the board superyin. (I would make one reservation on this subject, however, an important one, on which he I'll have to come back).

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□ Another "eye-popping" difference between Pierre's relationship to me, and that of the "ambiguous revolt": From what little I know of his family, I understand that Pierre's father is a mild-mannered, modest man, hardly the "profile" that would provoke a reaction of revolt, later transferred to a surrogate father.

### 18.2.9.2. (b) The reversal (4) - or the conjugal circus

**Note** 138 (December 8) As I finished the reflection last night, I had the somewhat painful impression of one who understands less and less. Before going to bed, I stayed for a while following the associations generated by last night's reflection. I thought I saw a few points of light appear, which I think will serve as luminaries in today's reflection.

Perhaps the most important of these associations relates to the "velvet paw" aspect of my friend, who likes to scratch (and sometimes deeply and mercilessly) with the most innocent airs in the world, and "with all the appearance of the most exquisite delicacy". This image, which came up in the course of a comparison (with a situation of "revolt" mentioned earlier) that had been shipwrecked, immediately struck me as rich in meaning, as an essential aspect of the "antagonism" I had set out to explore. And in retrospect, the image of "innocent smile and velvet paw" - the quintessence of an experience of almost twenty years - seems to me **the** "sensitive point" in yesterday's reflection, **the** unexpected "point of light" as I groped in the dark. If this impression of groping and darkness prevailed beyond that, it's because, too caught up in the ideas I'd had in my head the moment before and which I had to pursue or place, I hadn't been able to pay attention to the delicate "tilt" that had taken place in me, as soon as the image appeared. And in the half-hour that followed, as I pursued a few associations related to this image and one or two other moments of the past reflection, my attention became scattered once again. It's only now, with a day's hindsight, that I can see the thread of my interrupted reflection coming into focus, a perspective that had escaped me only a short while ago, as I reread yesterday's notes.

If I take care to follow the strongest association of all and the one most intimately linked to my lived experience, discarding for the moment others more □ "structured", more "intellectual", it comes this. I see myself back

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suddenly, as if in a single impression that would sum them all up, to this multitude of particular cases (experienced either as a co-actor, or as a close witness) of the **conjugal circus** - the circus of the woman-man couple. The circus of the couple, married or not, with or without children, young or old or young-old or the opposite, in the doldrums pulling the devil by the tail or in ease driving a carriage, it's all the same the circus of the couple doesn't change for all that. Suddenly, I'm back at it again, with one aspect of this circus that struck me



of all things (it took me a long time, it has to be said, before I saw anything but fire in it). . ): it's the very particular, very "innocent mine", "I said nothing and did nothing", "velvet paw" tactic played by the woman, in a certain game where it's always she who leads with perfect dexterity, and where it's always he who follows (and often, cashes in) without realizing anything. I've seen very few couples that didn't work to this tune, with infinite variations it's understood, left to the improvisation skills of each of them, not to mention their particular temperaments and other circumstances. I had the opportunity just today to see a particularly dazzling demonstration of this, which I won't go into here.

It's a somewhat colorful and nuanced description of these circus games, at least in the broad outlines, or even just the evocation of the tones (velvet paw, precisely, on the "her" side) in which which was largely absent from the November 12 reflection I've just reviewed, in the note "Le renversement (1) - ou l'épouse veéhémente" (n° 126). Clearly, I was pursuing this reflection against the grain of a reluctance, so much so that it ended up taking on the appearance of an austere "forces and motivations" - I was definitely not in good form that day! It was also the first time, in "The key to yin and yang", that the "reversal of yin and yang" was mentioned. The extreme case that had obsessed me My mother's was then, and still is as recently as yesterday (see note of November 22, "Le renversement (2) - ou la révolte ambiguë", n° 132). However, in my "four-point analysis essay", I took care to identify the first of these three "points" in such a way as to apply to all of them.

to the vast majority (if not all) of couples I've come to know even remotely closely, without the necessary predominance (albeit in occluded form) of the vehement tone of (ambiguous) "revolt". This p. 650

However, there's another thing in common that I missed that day. It only began to dawn on me last night, during that well-spent half-hour when I let my thoughts wander in the wake of the "in shape" reflection. This important common thing, which I had previously only perceived in the extreme case of "vehement wife", is the subtle play of **yin-yang role reversal**.

I'm not sure whether I should write that this game is the "spring" of the power game I alluded to earlier, or that it's **identical** to the latter. Surely, what for her (and often for him too) constitutes the quintessence of the masculine role, of the role devolved to the man, is the **possession of power** - a possession often fictitious, admittedly, but which in any case draws an element of reality from the social consensus. Perhaps I've tended to underestimate the strength of this element of reality, the strength of the **symbol of** the man as representing **authority** in relation to the woman - and in particular, its strength as a driving force in the woman's motivations. I suspect that for her, "being a man", or "being the man", means above all else, **exercising power**. The "reversal of roles", at the level of egotistical motivations<sup>196</sup> (\*), is probably no more, no less, than **the exercise of power by the woman over the man**.

Given the existing consensus, this exercise of women's power can scarcely take place in any other way than covertly.

It does not consist in ordering, nor in ☐faire pretending to decide (with the expectation that the decision will be followed), p. 651

but to **keep things running** - and, above all, to keep things moving, without ever seeming to do so. That's the famous marital merry-go-round, spinning and spinning! Tactics to keep it moving,

<sup>196</sup>(\*) The reversal of yin-yang roles in the erotic drive and in the game of love has been discussed elsewhere (see note on "Acceptance (the awakening of yin (2))"). The erotic impulse is by its very nature foreign to the games of the ego, and in particular to the games of power, even though the ego is eager to make it an instrument to serve its own ends, and skilful at doing so (within certain narrow limits at least, and by distorting and mutilating the original impulse). If there is a relationship between the two types of yin-yang "reversal", i.e. between, on the one hand, the free play of the two impulses yin and yang in the lover **and** in the lover, and on the other, the obsessive play of an incessant and insidious demonstration of power by one of the spouses over the other, it seems to me that this relationship can hardly be other than this: that each of the two types, at every moment, excludes the other.

passed on wordlessly from mother to daughter, from woman or girl to girl, from generation to generation, is the **"velvet paw" tactic** mentioned yesterday at the bend in the road. If you pay close attention, you'll recognize it in an infinite variety of guises, from the extreme-yang case of the vehement wife, embodied for me by my mother, to the extreme-yin case of the dolent (even overwhelmed) wife, which I saw embodied by another close relative.

It seems to me that there are very few women who don't practice this age-old tactic, and master it thoroughly<sup>197</sup> (\*). It's a daily practice, especially in the conjugal circus, but not limited to it. It seems to me that it's rarely practised between women (perhaps simply because it's harder to "get" a woman than a man). On the other hand, for some women, this tactic becomes second nature, in their relationship with **all** men, or at least those who are perceived by them as having a markedly virile character.

If I'm talking about "tactics" here, it's only an accessory aspect, the "tactical" aspect, of a more important reality: that of an inveterate inner attitude, towards "men" in general, or at least towards those, father, lover or husband in particular, who in her life play a privileged role as a **man**, invested (by social consensus, or by her own choice) with **authority**. This attitude is by no means always in the nature of a thirst for domination (as in the case of the "vehement wife") - at the very least, it's an attitude that's in the nature of a thirst for domination (as in the case of the "vehement wife").

p. 652 At least not in the sense in which the word "domination" is usually understood. It's more a case of a craving, which sometimes becomes all-consuming, **to constantly exert an** action  $\square$  sur l'autre, to "keep it moving" (implied: in

movement around her own person... ). To achieve this, all means are often good. One of these means of exerting action, and thus power, is to **hurt**, and sometimes to hurt as deeply as possible, to knock out outright, and at the limit, to destroy, physically or psychically, if only the occasion is propitious; and this, always, without seeming to touch, with "all the appearances of the most exquisite delicacy". On more than one occasion, I myself have been "sent packing"! Often too, caught off-guard as a co-actor or witness, I've had my breath taken away by the apparent gratuitousness of the act that wounds or destroys, with an innocent smile or an absent air, but always looking as if nothing has happened, seizing with an infallible instinct the moment and the place to touch the other where he or she can be most deeply affected - whether this "other" is the father or the lover, the husband or the child, or a mere acquaintance or even a stranger (provided only that the opportunity is there to strike and to reach...). . . ).

### 18.2.9.3. (c) Ingenious violence - or passation

**Note** 139 (December 9) This is the extreme, yet by no means rare, case of **violence for violence's sake**, of **gratuitousness** in violence and malice. This kind of violence, whether it strikes a stranger or the person closest to us and supposedly loved, is not characteristic of either men or women; it is neither "yin" nor "yang". But the disconcerting and insidious **form** in which I encounter it here, under the mask of an air of distracted absence or even ingenuous gentleness - this form, which has come to be quite familiar to me, seems to me to be peculiar above all to women. This is no doubt linked to the "patriarchal" social consensus, which invests men with authority and power over women<sup>198</sup> (\*). This form is

<sup>197</sup>(\*) It's also true that there are very few men who don't "walk the walk" when this tactic is applied to them. For most of my life, I've walked without a hitch. That only really began to change with the appearance of meditation in my life, at the age of forty-eight (it's never too late to do the right thing). Even today, I sometimes get caught up in it (not often, admittedly, and never for very long...).

<sup>198</sup>(\*) This consensus, moreover, and the authority of the man in his relationship to the woman, have been greatly eroded over the last few generations, and more and more so these days. I'd be the last to complain! It doesn't seem, however, that this superficial change in laws and mores has changed much in the deeper workings and "style" of relations between the sexes, and in particular in the visceral and carefully concealed antagonism between women and men. This is undoubtedly due to

its own means of satisfying a will to power which, in order to be forced (by the force of things) to follow paths other than those open to man, is no less imperious, no less devouring within her - quite the contrary! It would seem that not being able to unfold in the light of day, being condemned in advance to an occult existence, only serves to exacerbate and further proliferate this craving within her, to the point, in many cases, of truly "devouring" her life and that of her loved ones.

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Fortunately, this craving does not always reach the level of gratuitous, all-out violence, and the registers in which it is deployed are not all in tones of violence. While tones of discreet derision are more often than not the rule, giving vent to a veiled antagonism or secret enmity, simply mischievous tones of indulgent affection, a little mischievous on the edges, are not excluded. And while it's true that the tried-and-tested tactic of the "velvet paw" is a woman's privilege and weapon of choice, this privilege is by no means exclusive. Many times I've

could, and very closely, see this weapon wielded by men<sup>199</sup> (\*), with equally perfect mastery<sup>200</sup> (\*\*). Remarkably, in all these cases, the man who had appropriated this woman's

wea  
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someone who tended to repress certain virile sides of his being, and (by the same token, no doubt) to mold himself according to the **maternal model**.

This same tactic is frequently observed, and is virtually the rule, in the power games played by children, girls and boys alike, vis-à-vis their parents, or other adults in their stead. This immediately gives rise to an association with the situation of writers or journalists in countries (past or present) where direct or indirect censorship is rife, making direct, unvarnished public expression of one's true ideas and feelings impossible or risky. The main difference between this last case and the previous ones is that, in this case, recourse to indirect, veiled and sometimes symbolic expression of one's true feelings is no longer the work of the unconscious, but of conscious thought. The reason for this, surely, is that there is then a sufficiently widespread consensus in favour of unorthodox ideas and feelings (which need to be "passed on" without appearing to be so), so that the person concerned no longer feels obliged to hide them from himself, for fear of appearing as a hideous misrepresentation in his own eyes. Only in extreme cases of ferocious political or religious terror (such as existed in the Middle Ages, or in the Soviet Union and its satellite countries in Stalin's time) are unorthodox desires forced (by some, at least) to plunge an even deeper notch, evading the gaze of the Internal Censor, as well as that of the censorship instituted in mores and police apparatus.

All these examples seem to suggest that the "velvet paw" style (or "I said nothing, thought nothing, wanted nothing") appears, more or less automatically, in any situation that is even remotely sustainable, where

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The fact that this attitude of antagonism, and its means of expression through a certain power play (or reversal of power), is much more the result of the **transmission of an "inheritance"** from generation to generation, than of "objective" conditions within the family.

<sup>199</sup>(\*) I note, however, that in the cases known to me, when there is apparently "gratuitous" violence (by which I mean unprovoked) towards a close person or friend, it is always a person towards whom the person concerned harbors (albeit unwittingly) a long-standing grudge or animosity, materializing in concrete grievances (even if these remain informal more often than not). The only exception to this is my friend Pierre Deligne, in his relationship with me and those he assimilates to my person, as belonging to my "sphere of influence". This is an attitude of antagonism and violence (muffled, admittedly!) with no "personal" cause, by which I mean no cause in grievances (real or imaginary) that he harbors against those he's trying to reach. On the other hand, this is a behaviour that many women display, and not only (as in this case) towards close friends, acquaintances or even strangers, but also towards those closest to them, such as their lover or husband (of course, and as a matter of priority), or their brother or even their own child.

<sup>200</sup>(\*\*) It would seem, moreover, that this tactic, implemented by the unconscious, always inherits from it that "tact" and almost infallible sureness, so rarely present in fully conscious action. I don't think I've ever seen this tactic used without mastery.

a balance of power to our disadvantage makes it impossible, or at least dangerous, for us to express candidly, directly, our feelings, desires, ideas, intentions - and, more particularly, feelings of animosity or enmity towards those who are perceived as exerting a constraint on us (and no-tamment, la contrainte justement qui prétendait empêcher nous d'exprimer nos sentiments véritables)<sup>201</sup> (\*).

p. 655 This is not the only <sup>□</sup>case in which the style in question appears, and the inner dispositions that it recover. Very often, this "balance of power" is more or less fictitious, corresponding much less to an "objective" reality, taking into account the real dispositions (or means of power) of those perceived as "oppressors", than to the **idea** (conscious or unconscious) that we have of them. This idea is rarely the fruit of a careful and intelligent examination of a given reality, but is almost always part of the "package" of conditioning of all kinds that we receive at a young age, taking into account certain fundamental choices that have been made in us since that early period. Thus, whether in a girl or a boy, the choice (unconscious, of course) to identify with **the mother** implies the adoption of a whole set of attitudes and behaviours (such as those expressed by the "velvet paw" style), and at the same time the ideas (unconscious most of the time, but it doesn't matter) that underlie them (such as ideas about a certain balance of power, and the antagonistic reflexes that accompany these ideas). In the opposite case of identification **with the father**, but when the father himself has integrated into his person certain typically "feminine" traits (or which are such in our society, at least), it's conceivable that the effect could be quite analogous to that in the first case.

The point I'm getting at here is that in our society today, and in the circles I've been part of at least, it seems to me that this style ("velvet paw"), and this "feminine" inner attitude I'm examining here, are only to a very limited extent the spontaneous individual reaction to relationships of that it's an objective force, instituted by society or by the particular circumstances surrounding our childhood (or even, our <sup>age</sup> <sup>□</sup>adult at a given moment); that it's rather an "**inheritance**" taken from one or other of our parents

p. 656 (if not both at once?), who himself had taken it over from one of his own parents. Clearly, this type of inheritance tends to follow the **maternal** line, being transmitted primarily from mother to daughter. But on more than one occasion, I've seen mother-to-boy transmission up close. There's nothing to suggest that it can't also be transmitted, exceptionally, from father to son, or even from father to daughter.

#### 18.2.9.4. (d) The slave and the puppet - or the valves

**Note 140** (December 10) I'd like to return to some associations around the theme of gratuitous **violence**. This was the theme with which yesterday's reflection began, and from which I drew back, in order to return to an examination of the "feminine" (or "velvet paw") style in power games, and as a means of expressing a disposition of antagonism towards others (and above all, towards men perceived as strongly virile or as being, in any capacity whatsoever, in a position of authority, prestige or power). As I reminded you yesterday, (seemingly) gratuitous violence, violence "for its own sake", is no more than a form of violence.

<sup>201</sup>(\*) As I was writing these lines, the thought occurred to me that the situation I've just described is precisely the one we were confronted with in the early years of our childhood - all of us, without exception. A large part of our unconscious (the part we might call "the oubliettes", generally perceived at the unconscious level as a sort of "garbage pit"), is nothing other than the response of our child psyche to this pressure from the environment, which forces us (it's practically a question of survival) to bury away from our own eyes, as a sign of disavowal, everything in us that falls under social censorship. This censorship is soon internalized in an inner Censor, whose sullen presence guarantees the permanence of this premature burial. Yet, despite the Censor, unorthodox impulses, knowledge and feelings, duly buried, manage to express themselves, sometimes with exacerbated and fearsome efficiency, in indirect, often symbolic, yet perfectly concrete ways. The "velvet paw" section is a particularly "striking" - and often disconcerting - example... ..

for both men and women. Everyone has had the opportunity to be confronted by it suddenly, at the turn of the road, whether in the guise of the "most exquisite delicacy", or in the form of a boot or a burst of machine-gun fire in the belly. The latter style, the "yang" style, is certainly rarer these days, in so-called "peaceful" times, and in civilized countries like ours. For most of us, well-bred and more or less well-placed people in a country of affluence, this violence that-says-well-its-name is not part of everyday life, as is the case with the other, muffled violence with its ingenuous airs. And yet, all you have to do is peruse the "news" column of any major daily newspaper, or listen to the news at<sup>202</sup> (\*), to realize that gratuitous violence "lasts", even here, still roams the streets. It doesn't always go so far as to slit the throat par-dessus le marché, the anonymous little old lady

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you've taken it upon yourself to break into. But when young people in search of adventure "borrow" a car left carelessly open in front of the house, it's rare that they don't leave it in a ditch ten or twenty kilometers away, having first carefully ransacked it. Even in the peaceful countryside where I have the good fortune to live without worrying too much about anything, the smallest farmhouse or cottage doesn't remain unoccupied for long, before it's already been looted from top to bottom (that's for utility) and, what's more, copiously vandalized (that's for pleasure). In all these cases, the gratuitousness of the violence is particularly striking, given that the person struck is a stranger, often someone we've never seen before and never will.

This is what we might call "**anonymous**" violence. Since time immemorial, no doubt, wars have been collective orgies of such violence - times when the opportunity to kill for free is king, and when the life of a vague individual is worth zero before the pleasure of pulling a trigger and testing one's power to make a nameless, bland figure slump before one... .

If there's one thing in the world, as far back as I can remember, that has left me bewildered and speechless every time, it's seeing myself confronted once again with violence beyond comprehension, violence that strikes and destroys for the sheer pleasure of striking and destroying. If there's one thing in the world that imbues us with this indelible sense of "evil", it's not death or the suffering the body can endure, but it's this thing. And when such violence (whether it's harsh or gentle, whether it seems "big" or "small") comes at you unexpectedly from one of your loved ones, it's bound to hit hard and deep, to bring out (or resurface. . . ) a nameless anguish. The root of this anguish goes deep down, when it finds the soft, fresh soil of childhood, or even infancy, in which to take root. This anguish, "the best-kept secret in the world" in my life as a child and as an adult, appeared in me at the hands of my mother, in my sixth year.

It was at the age of 51, during the month of March 1980, that I uncovered the episode of the implantation of anguish in my life. Anxiety's hold over me had been defused beforehand, to a large extent in my life. sure at least' with the appearance of meditation in my life (in 1976), gradually taking a place in it growing. A third decisive turning point in my relationship with anguish came in July and August 1982, during a careful examination of the mechanism of anguish in my everyday life. The anguish-creating situations, from childhood to middle age, were those which, in unknown depths of my being, made me re-experience "that which is beyond comprehension". These were also the times, to be precise, when I saw myself confronted once again with the familiar signs of violence, seemingly inexplicable, elusive, irreducible... . The sudden eruption of this violence suddenly brings back a wave of distraught anguish, which is immediately controlled and repressed. This visceral reaction remained identical to itself.

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<sup>202</sup>(\*) These are things, it's true, that I stopped doing a long time ago, contenting myself with occasional information through intermediaries.

<sup>203</sup> (\*). If anything has changed over the last few years, it's the emergence of a way of **thinking** in the wake of anguish, one that makes comprehensible, and often obvious, what had appeared under the threatening mask of "that which is beyond comprehension", of the delirious ; and above all, in the last two years, by the appearance of a **look at myself**, a look of interest and concern for this anguish itself, which a reflex movement of peremptory force would have me hide from myself. Or, to put it another way, my relationship with anguish has become, especially over the last two years, no longer one of visceral refusal, or of taming wild beasts or gravediggers, but rather and increasingly, one of attentive and affectionate **welcome to** the message it brings me about myself - about my present, my past and its action in my present. This, it seems to me, is the last step I've taken so far, towards an increasingly complete inner **autonomy in** relation to others, that is to say, above all else: in relation to my family and friends<sup>204</sup> (\*\*).

p. 659 □ It seems to me that it's the violence-that-doesn't-say-its-name, the "feminine"-style violence, that is most powerful source of anxiety, far more so than the more spectacular violence of a punch in the face. Those who play with muffled violence - and thereby also play with the secret valves that release nameless, faceless waves of anguish in others - hold in their hands a weapon more formidable than any authority or simple power of coercion. And to manipulate these floodgates of anguish at will and at whim, with an air of innocence, represents a **power that is** undoubtedly more incisive and more formidable - even if it remains hidden - than any de facto or de principle power instituted by social consensus. This is woman's "just revenge" on man, in a society where man claims (or has claimed) to dominate her; and this is also the price "he" pays for his illusory supremacy (present or past). If she is a **slave** (and in our country, she is less and less so), he is a **puppet** in her hands, or very nearly so (and he is as much a puppet today as he ever was).

For some years now, whenever I'm confronted with a situation of gratuitous violence (be it directed at myself or others, be it brutal or insidious), the association with **self-contempt** - or rather, I **see** this self-contempt - comes to me with unstoppable force.

even in one who affects, openly or inwardly, to despise others. I have no doubt that this is □ not a simple push-button mechanism in me, a "philosophical" or "psychological" dada that I

p. 660 I'd be quite happy to take the occasional step out, perhaps as a way of exorcising the anguish I was talking about with a convincing formula, by casually sticking a boilerplate label on a threatening stranger. It's simply a **knowledge of** an essential, profound and (once seen) obvious relationship.

<sup>203</sup> (\*) (December 14) It would be more accurate to say that this reaction remained "more or less the same" **right up to the time of** my meditation in July and August 1982. While the "provocations" that caught me off guard have been numerous since then, the "visceral reaction" in question has only appeared once, a year ago. It was the occasion for a short meditation, lasting a few hours, which completely clarified the situation. As soon as a confused inner situation is confronted with simplicity and acceptance, the anguish that accompanies it and brings us the message of our confusion, disappears without a trace, except that of knowledge and renewed calm.

<sup>204</sup> (\*\*) This "last step" has already been discussed at the end of "L'acception" (n° 110), in the somewhat different light of a "final step".

liberation from the need for **approval** or **confirmation**, which "is really the hook, discreet and ironclad, by which conflict can 'hang' in us, and by which we are. . . dependent on others. . . by which, in short, it "holds" us, and (in a minor way) manoeuvres us as it pleases... ". (This passage could well have been written on this very day - but I swear I didn't copy it!)

I can't say whether there are still other such "steps" ahead of me, which will give me the hindsight to see my current autonomy as still relative, and not complete (as I would tend, however, perhaps a little naively, to believe. . . ).

The emergence and blossoming of a relaxed, attentive relationship with anxiety represents a **liberation** in our relationship with others. In fact (as the following paragraph states), it is the possibility for others to "turn the floodgates of anguish" in us as they please (by alternating between gratification and rejection, in a measured and deftly administered manner), that represents their principal means of power over us.

This knowledge doesn't "evacuate" anything, it simply allows me to **situate** an unknown. It is in no way a sentinel, placed there to block the path of anguish, or to expel it from the square. This is not the nature of knowledge, as I understand it. Knowledge is part of an inner **calm**, and helps to ground it. It is a restlessness within us, on the other hand, that constantly pushes us to want to block the way to "intruders", lest they upset a "calm" of composition. The calm I'm talking about doesn't fear intruders, it welcomes them. And the surface agitation created by the new encounter with anguish does not disturb this calm, but rather contributes to it.

## 18.2.10. Violence - or games and the sting

### 18.2.10.1. (a) The violence of just

**Note** 141 (December 13) With my "joke" in the previous note, about the "slave" and the "puppet", I'm sure I've found another way to displease everyone, and (if I'm read. . . ) to be called every name in the book! Unless the hypothetical reader applauds contentedly, who knows, convinced that the image is well sent and applies to the whole world, except to himself (or herself); and except perhaps again, at most, to the sarcastic author. By this supposition, moreover, he would be giving my modest person a credit that in no way belongs to him. At the very most, I'd venture to admit that in the last few years (and especially since a certain meditation on anguish in July and August 1982), I've begun to get out of, or even be out of, the famous "circus" - the conjugal circus, of course, but also the others that resemble it.

like brothers. In the first part of *Récoltes et Semailles*, there's even a section in this vein, called "Fini le manège!" (No. ° 41, last March). It wasn't about the conjugal circus, but about a certain mathematical circus, into which I had more than enough time to turn.

a good part of my life, like everyone else. But it's also true that a few weeks after this section □au nom prometteur, on April 29, appears a note "Un pied dans le manège (n° 72), whose name p . 661

would seem to herald a different tune! The difference with before, perhaps, is that if it happens to me again here and there to turn in some merry-go-round (and I hardly see any more than the mathematical merry-go-round which continues to attract me. . . ), it's myself (or at least someone within me) and no one else who pulls the threads that keep me going round and round, and these have ceased to be invisible to me.

Having made these reservations, I can say that for most of my adult life (and more precisely, until I discovered meditation), I "walked" on the quarter-turn (like everyone else, again), both in the conjugal carousel (it spun merrily for no less than twenty years!), and in the others. I don't regret it, because the knowledge I have of carousels of all kinds I owe first and foremost to the ones I've been on myself. If I've been on them for so long, it's because the student was slow to learn - and also, surely, because in more ways than one I found bait. In the end, I suppose, they lost their strength and charm. ...

It seems to me that in all these carousels, I was always the one who "walked", and never the one who "made walk". Or to put it another way, I don't think I've ever had the slightest propensity for the famous "velvet paw" style - I've sometimes played hard with my claws, but never, I think, with claws drowned in velvety down. This is one trait, among many others, that attests to the fact that at the level of the structure of the ego, of the "boss", of that in me which is conditioned, the basic tone is strongly "masculine", without any ambiguity whatsoever. The yin, "feminine" tones, on the other hand, dominate at the level of the "child", the original in me, i.e. also in the drive for knowledge and in the creative faculties.

I'd like to say a few more words about the "gratuitous violence" in my life. In the previous

note (from three days ago), I evoke it in the light of the one who finds himself the target of this violence, or at least the one who is confronted with it in others (even as a mere witness), when I write :

"If there's one thing in the world, as far back as I can remember, that each time has left me bewildered and speechless, it has been to see myself confronted once again with that violence that passes understanding, the violence that strikes and destroys for the sheer pleasure of striking and destroying... . "

p. 662 □ These lines, and those that follow them, correspond well to reality, to the reality of my own experience in all I'm sure this is also the case for countless men and women who, like me, have been confronted with this kind of violence. They could give the impression that the person who wrote them is himself a complete stranger to such violence, that all his life he has been free of such delusions. But this is not the case. I recall four relationships in my life, three of which took place in my childhood or adolescence (between the ages of eight and sixteen), marked by an enmity based on no specific personal grievance, and expressed in the form of systematic and merciless mockery, or in roufflés and other brutalities. On the first occasion, the victim, a classmate (still in Germany), was the whipping boy of the whole class. The situation dragged on for years, I seem to remember. The next two cases took place during the war, when I stayed (just out of a French concentration camp) in a Secours Suisse children's home in Le Chambon sur Lignon, "la Guespy", between 1942 and 1944. This time, the "horrors" were one of my classmates (whose parents, like mine, had to be interned as German Jews), and one of our two supervisors, both of whom spoke German like me. They were both a bit of a pushover for a sometimes ruthless group of young boys and girls of which I was a member - but I think I gave them a harder time than anyone else in the whole gang. Living together under the same roof, and as refugees with a precarious status, under the constant threat of the Gestapo rounding up Jews, could have aroused in me feelings of solidarity and respect, but it didn't happen.

In all three cases, the person I took as a target of malice was of a gentle, rather shy, non-combative nature, which I then classified as "soft" or "cowardly", and which therefore formed part of the traits that were supposed to make him or her a less-than-stellar character. In an age devastated by the breath of violence and contempt for the individual, and myself filled with aversion to war or concentration camp violence, and everything that goes with them, I nevertheless felt entirely justified in the contempt and violence I inflicted on others, for the simple "reason" that I had taken pleasure in classifying them as "unsympathetic" (and other adjectives to match. . . ), after which everything (or almost everything) became permissible, not to mention the fact that I had to be careful not to get too close to them. not to say, highly commendable. I used to pride myself on being "logical" and fair-minded,

p. 663 □ I didn't see then that my behavior, and its justification by an antipathy (which I wouldn't have I'd certainly thought to probe their true nature), were exactly the same as those of the good-natured German of the thirties vis-a-vis "dirty Jews" (things I'd seen up close in my childhood); and that it was these too that made possible the unprecedented outburst of violence that was then sweeping the world. Of course, I pretended (following in my parents' footsteps) to distance myself from this violence as if it were some strange aberration (sometimes even "beyond comprehension"). I was full of haughty condescension towards all those, soldiers or civilians, who in one way or another consented to be active or passive cogs in the heroic mass graves and accompanying abominations. And at the same time, at my modest level and within my own limited sphere of action, I was doing what everyone else was doing... ..

If I try to discern the cause of such a strange blindness in the service of a deliberate purpose of contempt and violence, it comes down to this. The violence I myself had had to endure during my childhood since the age of five, without ever having been designated as such to my attention as a child, had ended up in my mind's eye.



by creating a state of chronic tension, unconscious and carefully controlled by a strong will. This tension, or accumulation of aggression with no particular target, created the need for a release of aggression. This "need", however, was not bodily in nature - there was no shortage of opportunities to let off steam through appropriate bodily activity in any of these cases - but **psychic**. Surely there must have been some accumulated resentment, mostly unconscious of course, and not materialized in palpable grievances towards a particular person (one of my parents, let's say, or one of the people who took their place), on whom I could then have transferred feelings of resentment, and given them concrete, perhaps violent, expression. There must have been a "vacant" violence in me, a diffuse, wandering violence, looking for a target on which to vent. It often seems to be animals (insects, toads, dogs or cats, even oxen or horses. . . ) that bear the brunt of such wandering violence, in search of a victim. This was not the case for me; I don't remember ever having martyred an animal, big or small, in my life. Apparently, I needed a scapegoat closer to home, a **person**! When you're looking for one, it's always easy to find one.

□ I have no doubt that what I have just written describes a certain aspect of reality. Yet I feel that this description still remains on the surface of things, only identifying a certain "mechanistic" aspect, without really going any further into the unconscious experience. For the moment, in place of this experience, there is a kind of great "blank", a void. This is not the time or place to go beyond that, to probe further into what this "white" covers, what dissolves in this "void". Is it this famous "self-contempt", which was so peremptorily asserted in the note three days ago, and which suddenly, now that it's **me**, seems to have vanished without a trace? Now or never, at last, would be the time to get to the bottom of it, to elucidate this tenacious and ambiguous "vagueness" that continues to mark my knowledge of myself, just as the "vagueness" that once surrounded the role and very existence of anguish in my life. It seemed to me that anguish was the "best-kept secret" of my entire life. Is there another, even better-kept secret, one that I've barely touched on here and there, on two or three occasions, since I've been meditating? I feel like I've got everything I need to get to the bottom of it - including this sudden surge of familiar interest, which tells me that the time is ripe to get started! However, I have a feeling that I'm not going to do so here, in this meditation that is in some way "public", or at least intended for publication. This one, among many others, will at least have had the virtue of unexpectedly bringing to maturity a question that has suddenly become very close, recognized at last as crucial to an understanding of myself, whereas previously it had seemed like one question among a hundred, on a long waiting list whose end I may never see... .

It's not out of the question that I'll still have the opportunity to meet one or other of the three men (two of whom are about my age) who were once the innocent targets of my own violence and aggression; or if not, at least that I'll have the chance to write to one of them. It will be good for me to be able to make amends, and to do so with full knowledge of the facts. Perhaps it will be good for him too. Strangely enough, though, I don't get the impression that any of the three of them ever really held a grudge against me, or that my violence had triggered in him any personal animosity towards me in particular. a kind of calamity, from which there could be no escape, and which my own person has was perceived more as one among extras in this calamity, than as □ a ruthless tormentor (that I was) and hated. Of course, I may be wrong, and I may never know - just as I may be lucky enough to be confronted one day with this karma, which I sowed in blindness.

I think there must have been a maturing in me in the years following the "Guespy" episode, with no "Guespy".

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And yet, as far as I can remember, there was no reflection whatsoever on the subject. Still, there were effective reflexes in me afterwards, which would have prevented me from ever again associating myself with acts of collective violence by an entire group against one of its members. I don't think it ever happened again in my adult life, nor that I was ever tempted to play such a role again, which I must have sensed to be false and spineless under its cheerful, "sporty" exterior. But even after the war, life was full of situations full of veiled violence and anguish, perpetuating the deep tensions that had marked my childhood and adolescence. This is the context of a fourth relationship, marked by occasional movements of animosity and violence that I can call "gratuitous" - not founded or provoked by concrete grievances, nor even (I believe) by acts that could pass for "provocative". It concerns my relationship with one of my sons. I know that I was no less attached to him, and that I "loved" him no less than my other children. But at some unconscious level, I must have rejected certain aspects of him, precisely those that made him softer and more vulnerable, and harder to apprehend, than his brothers and sister. Decidedly, he didn't "fit in" at all, even less than my other children, with the beautiful superyang images I would have liked to find in my children - and all the more so, as certain very harsh circumstances that had surrounded his first two years and left a deep impression on him, made it more difficult for him to form trusting relationships with his parents. During the time he lived with me under the same roof, right up to his tenth year, I sometimes subjected him to humiliating punishments, imposed in a thunderous voice. These were things that had faded entirely into oblivion, as had a certain atmosphere that had come to permeate the family air - a few dialogues with his sister and two brothers, two or three years ago.

p. 666 years, which very conveniently brought back so□soit little of these things in my memory. Perhaps the day The time will come when he, too, will be ready to talk about it with me - he who, perhaps, among my children, has borne the brunt of a family atmosphere charged with hushed anguish and unassumed tensions; or at the very least, the one who has most "coped" at the hands of his father, while each of them has had his ample share of the parental "package". At the very least, I know - and I'm glad to know it - that what's preventing any of my children from having a simple, trusting relationship with me, their father, and from talking together about a heavy past and probing it, is not some fear they've kept from me, and which they've tried hard to hide.

But then again, this is not the place in these notes to delve further into a complex situation, which involves six or seven other people as much as myself. What I wanted to do, above all, was to make an unvarnished observation of the occasional appearance, here and there in my life and in my own actions, of the same apparently gratuitous violence, which so often "left me bewildered and speechless", when I encountered it in others. This observation is not made with any particular "intention", nor is it meant to "explain" or "excuse" gratuitous violence in anyone, any more than it is meant to explain or excuse mine. It's not impossible - indeed, it's probable - that, with further reflection, the two violences - that in others and that in me - will end up shedding light on each other. It's the kind of thing that eventually comes on its own, out of the blue, without being sought out. If I've made this observation, it's simply because it was in the way, and (on pain of ceasing to be true) I couldn't not make it here.

### 18.2.10.2. (b) Mechanics and freedom

**Note** 142 (December 14) Last night's reflection was a timely reminder of something we all tend to forget, and especially (in this case) of something **I** tend to forget: that I'm not "better" than anyone else, that I'm cut from the same cloth as everyone else.

like so many of my friends whom I'm about to put in the hot seat, the focus of uncompromising attention. ...

Yesterday, I gave a sort of description of the appearance of (seemingly) "gratuitous" violence, such as the discharge of tension and aggression □ accumulated on some scapegoat who, for one reason or another another, finds herself in charge. This "mechanistic" and superficial description, surely "well known", can lend credence to an equally "mechanistic" **attitude** towards this violence, in oneself or in others. It is then seen as a kind of inescapable inevitability, rooted in the very structure of the psyche - alas, what can we do about it! Such an attitude, under a "rational" or "scientific" guise, seems to me to be nothing other than the rationalization of an **abdication**: an abdication before the presence of a creative **freedom** in ourselves and in others, which opens up the option for each of us **to assume** the situations in which we find ourselves, instead of passively following the slope lines of ready-made mechanisms, ready to take us over at any moment. While it's true that we rarely make use of this "freedom" option, the very **presence** of this option and of the creative possibilities within us, whether we choose to make use of it or not, changes the nature of things completely. It is in **this way**, and in no other, that situations involving relations between people, or between a person and himself or the world around him, have a dimension that is absent when, instead of people, we're dealing with (say) computers, however sophisticated they may be. This is also where the privilege of **responsibility** for our actions and the motivations behind them comes into play for each and every one of us. This responsibility is in no way removed by the fact that we often resort to the convenience of hiding our own motivations. p. 667

To return to the case in point as an illustration, if I was able to play the great soul while making use of my power to torment a comrade who had done me no harm, it's because behind a surface "good faith", I had chosen an attitude of crass, phenomenal bad faith, which was just as obvious at the time as it is now, forty years later. It was indeed a **choice**, which nothing forced me to make, and which amounted to turning a blind eye to the tensions and aggression that had built up inside me (while, of course, claiming to have nice "non-violent" ideas), and "sneaking" (sic) them out onto the scapegoats at hand. Violence of this kind - which is to say, almost all violence and violence against abominations raging in the world of men - can only take place, and their secret function can only be fulfilled, on **condition** that it remains rigorously secret precisely (while □ even as it croaksp . 668

We're encouraged to do this by the air we've always been surrounded by, while we've always seen those around us eager to endorse our subterfuges, no matter how crude. It's true that we've always been encouraged to do this by the air that surrounds us, while we've always seen those around us eager to endorse by consensus subterfuge, however crude, in the service of fictions that had their assent. And my own subterfuge, in the cases I have mentioned, did indeed have the assent or tacit encouragement of those around me, without which I could not have maintained it and continued my game.

Assuming a situation, on the other hand, is no more and no less than approaching it **in good faith**, in the full sense of the word, i.e. without taking advantage of the facility offered to us to conceal the obvious ins and outs of the situation through crude subterfuge. It also means, quite simply, making use of our healthy faculties of perception and judgement, without taking care to conceal them for the needs of one cause or another. It may seem strange, but it's also simple and obvious - when we approach a situation with such an "innocent" attitude, it is immediately and profoundly transformed, however confused and knotted it may have seemed. Or, to put it another way, if it was indeed "knotted" and hadn't moved a muscle for a long time, it's because we ourselves were preventing it from evolving, from "flowing" according to its own nature; that we were obstructing its spontaneous movement, following in the footsteps of the "innocent".

This is the concordant example of all those who have surrounded us since our earliest childhood. It's enough to **stop** stiffening, to **stop** obstructing, for things that seemed frozen to start moving again, for what was stuck to become unstuck, and for the hard accumulated tensions to finally free themselves and resolve themselves in a new and ample movement, finally reappearing.

This "ease" or "convenience" we have, with everyone's encouragement, of "taking bladders for lanterns", and thereby blocking what is made to sink, is in fact nothing "comfortable"! We're paying an exorbitant price for the cushy inner immobility we've been allowed to enjoy - the price of inner tension,

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and the enormous investment of energy required to maintain both this tension and the fiction of bladders = lanterns. That said, everyone does as they please, at any given moment - that's our privilege. And at any moment, because of what we

What we do, we sow, for ourselves and for others. And the harvest of what we **sow** begins right now.

### 18.2.10.3. (c) Greed - or the wrong business

**Note** 143 Perhaps it's time to return to the "foreground" of the Funeral, i.e. the ins and outs of the role played by the Grand Officiant at my funeral, my friend Pierre. I had already returned to it a week ago, in the note "Patte de velours - ou les sourires" (n° 137, December 7), only to move away from it.

again with this digression (on five consecutive notes) on "la griffe" and "le velours". I feel that this This "digression", like many others that preceded it, was not in vain.

If I've been led to this conclusion, it's precisely because the most striking apparent trait, perhaps, in the way my friend has taken on his role, is the persistence, without any hint of rupture at any time, of the purest "velvet paw" style, in the service of a flawless antagonism that never says its name<sup>205</sup> (\*). Another salient fact is that, behind the pleasant, well-tempered appearances of a knowing smile and friendly airs, my friend often expressed an unequivocal, and seemingly gratuitous, intention to **harm** or **injure**, **either** myself or those he considered "mine" (in terms of mathematical work). I have gone into enough detail on concrete facts in this sense in the first part of Burial, for it to be worth returning to them here. What we're talking about here is malevolence (strictly circumscribed within the realm of scientific activity, it would seem), "**violence**" in the strongest sense of the word, even though it remains rigorously concealed - the claw always drowned in exquisite downy silks. And this violence, this malevolence, has all the appearance of the most disconcertingly **gratuitous** - it

It would seem that they are exercised for the sole pleasure of harming and injuring,

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□As <sup>with</sup> any situation, it seems so unbelievable that often we hesitate to believe the testimony of our healthy faculties<sup>206</sup> (\*). Denying this testimony, as is common practice, is one of the innumerable ways of not taking on a situation, and thereby perpetuating it, so it's surely preferable to put the matter down, to look around it, perhaps in search of aspects that may have escaped us and which provide an approach to it, enabling us to integrate it into our experience. It seems to me that very few of us have ever been in such a state of causeless malevolence - and to be willing to remember this is already a possible step **towards approaching** a factual situation, which common reflexes would rather encourage us to evacuate straight away. It's also a good idea to

<sup>205</sup>(\*) As I've already had occasion to point out elsewhere, the fact that antagonism, or deliberate rejection or derision, "never says its name" is in no way peculiar to my friend Pierre, but (as far as I'm aware) applies to all participants in the Funeral, without exception. And so, in this "Yin funeral" of derision, the underlying note in each of the participants (and as befits such a funereal occasion) is itself - yin!

For the "occult" nature of the Burial, see also the note "The Gravedigger - or the entire Congregation", n° 97.

<sup>206</sup>(\*) On this subject, see the note "The Emperor of China's robe", n° 77'.

probe further, to see if there might not be some hidden grievance that is the cause and springboard of violence that seemed to have no cause - just as it's good, if need be, to recognize bogus "grievances" for what they are, of the kind (for example) that I myself have practiced, knowing that so-and-so is an awful character who deserves no mercy, etc.

But in this particular case, no matter how hard I probe, I can't see anything that remotely resembles a **grievance** that my friend might (rightly or wrongly) harbor against me, or against any of those he has chosen as targets of malice. He himself has never hinted at anything remotely resembling this; not to mention the fact that, when probed by me on more than one occasion about some of his actions which had left me speechless, he never admitted that there was the slightest hint of enmity in him towards anyone. I ended up sensing a secret gratification in him, during my occasional encounters, when he would serve me his good reasons, all objective, with his very own air of innocent, slightly amused surprise... . In short, I entered into a game that he played as he pleased, and with an intimate satisfaction that took me a long time to perceive. (And yet he was far from the first person to make me squirm in this way!) Still, better late than never, I finally got off that merry-go-round<sup>207</sup> (\*\*)!

If, on the other hand, I probe myself, reviewing my relationship to my □ami since our meeting a Nearly twenty years later (in 1965), I can't find a trace of anything that might have been the cause of any grievance against me. In the conventional, superficial sense of things, I can say that during all this time, and more particularly in the first five years of close contact, I "did him nothing but good".

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But this observation immediately reminds me of another, less superficial one - that of a **complacency** in me towards him, which emerged in the course of reflection in the notes "Being apart" and "Ambiguity" (n° s 67' and 63"). Clearly, this complacency was by no means "a good thing" for him - and also, that the my brilliant young pupil and friend's attitude towards me developed in close symbiosis with my own attitude, and more specifically, with this complacency. It's not impossible, even, that this complacency, at some unconscious level, was (not only perceived, which is obvious anyway, but also) felt by my friend as a "grievance", as a scenario perhaps too well known and replayed over and over again, in his youth as a child who was a bit of a prodigy around the edges, and which was served up to him (albeit discreetly) once again. Perhaps he'd naively believed that, once he'd set foot in the mathematical "big wide world", everything would be different from what he'd known - and then no, it was still the same tobacco! (And by his own deliberate choice, today it's still the same tobacco, and even bigger, which is more... .)

I'll probably never know exactly what's going on here. Besides, it's not my job to find out, assuming I have the antennae to do it on my own. If there was a "grievance", it was, at most, an "auxiliary" grievance, helping to set "something" in motion - a certain **game**, driven by a force of quite another magnitude; a force whose presence I've long felt, but whose nature remains enigmatic to me. Before leaving this "foreground" of the Burial painting, I'd like to at least try to surmise the nature of that force.

Clearly, there's a **greed** to supplant, oust and erase, as well as a greed to **appropriate** the fruits of others' labors and love affairs with lady mathematics. Yet it's clear to me that it's **not** a simple "bulimia" for prestige, admiration, honors, or even power, which is the mainspring of his role in the Burial. How often, in the course of my reflection on this □rôle, have I been It's hard to believe that his **obsession with** burying was actually burying himself! Thanks to his exceptional gifts and an equally exceptional situation, he had been blessed with everything he needed to

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<sup>207</sup>(\*\*) This was in 1981 - the "second turning point" referred to in the note "Two turning points", n° 66.

to far surpass his master, and to leave a profound imprint on the entire mathematical world of his time. All he had to do was let the child in him play to his heart's content, without bothering him with instructions, barriers here and forbidden directions there - simply taking care of what was necessary, strictly stewardship. In doing so, and without having to push, pull or elbow, the "boss" in him, no more or less greedy than in anyone else, would certainly not have lacked all imaginable marks of prestige, admiration, honors, and power to boot, not even knowing what to do with them, whereas it's the dullard who gives it his all, leaving the boss little leisure to play the boss... .

Decidedly, in simply "utilitarian" terms, it was a bloody bad deal to get embroiled in a Funeral that had been sticking to him for fifteen years or more, and was going to stick to him for the rest of his life, if the cumbersome deceased hadn't suddenly decided to disrupt the ceremony, by lifting the lid of his coffin, when (as expected) it was least expected! (All bets are off as to how this unfortunate incident will affect Pierre's future bets... .) Or to put it another way, my friend had the makings (by his intellectual means, at least), and the credentials, to be a mathematical Peter the Great, and chose instead to play the little Peter. Sounds like a bad deal indeed, at least if the goal was indeed, first and foremost, that of vanity.

#### 18.2.10.4. (d) Both knowledge or fear of knowledge

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**Note** 144 (December 15) Towards the end of last night's reflection, I felt the slight uneasiness of someone who, with a peremptory air, serves up a reasoning of impeccable logic, while dismissing the diffuse feeling that there is nevertheless something wrong. This "something" became apparent, moreover, as soon as I was stopped writing. One vague way of putting it is this: the "logic" of the unconscious, the one that presides over our most crucial choices, is by no means that of <sup>conscious</sup>□<sup>ordinary</sup> reasoning, and even less that of orthodox" reasoning. In this case, the perception I have of the "assets" of the young man Deligne in the second half of the sixties (let's say), and the weight I give them (which goes in the same direction, at least, as the weight any reasonably well-informed mathematician would give them) - this perception and this weight (which I would like to call "objective") are unrelated to the dispositions and feelings of the interested party himself; with those, in particular, concerning his own abilities, which certainly form the key asset among all those at his disposal.

I have the impression, however, that on a conscious level at least, and with all the modesty that modesty demanded, my friend had integrated and made his own the flattering echoes that had surely been coming back to him for a long time about his unusual gifts. But there's no doubt in my mind that at a deeper level, where the great choices that dominate a life are made without words, this "objective" version of things became (and still remains today) a **dead letter**. In its place, there is an insidious **doubt**, which no "proof" of value (or of superiority over others. . .) will ever uproot - a doubt all the more tenacious because it remains forever unformulated. I've seen it in my friend, as I've seen it in others less brilliantly gifted, and it's the same. This doubt is the stubborn messenger of an **intimate conviction**, which also remains unspoken, even more deeply buried than this doubt itself: an intimate conviction of powerlessness, fundamental and irremediable. This too is the "self-contempt" of which I spoke at the very beginning of Récoltes et Semailles, in the context of a reflection that remained "general"<sup>208</sup> (\*). It reappeared, again in an impersonal context and under a different guise, a month or two ago, as a "feeling of cracking"<sup>209</sup> (\*\*) - this diffuse feeling of "cracking".

<sup>208</sup>(\*) See "Infallibility (of others) and contempt (of self)", n° 4.

<sup>209</sup>(\*\*) See note "La moitié et le tout - ou la fêlure" (n° 112), October 17.

which I had first noticed in myself, the day after I discovered meditation. And several times during the reflection on Burial, there was a sudden and acute perception of this "intimate conviction of powerlessness" in my friend, throwing new light on a situation that seemed to defy common sense... .<sup>210</sup>(\*\*\*).

□ I know that this intimate conviction, in my friend or in any other, is itself like **the shadow of a knowledge** - the knowledge of a "crack" that actually exists, of a "mutilation" suffered, and sanctioned and maintained to this very day by its own acquiescence. The shadow does not, however, restore the knowledge from which it springs, beneficial in itself like all knowledge - it is rather like a deformed, gigantic caricature of it, a scarecrow version. What deforms knowledge and makes it unrecognizable is **fear** - the fear of making contact with knowledge itself, of letting it rise from the depths where it has always been repressed, and of assuming the humble reality of which it is the faithful reflection.

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To make contact with this dreaded knowledge, to become aware with a fully conscious gaze of this reality known in its deepest layers, and shunned - this is what it really means: to make full contact with that in us (whether we call it "the force", or "the child"), "believed lost and dead for a long life". For it is this strength and nothing else, the strength of childhood, that enables us to take on the knowledge of that in us which is cracked, mutilated, paralysed. Assuming this knowledge also means reconnecting with that **other knowledge**, which predates our mutilation and is even more essential than it: the original knowledge of the presence of this "force" that lies within us, a force that is neither muscle nor brain, and which contains both.

Strangely enough, this lost knowledge of the presence within us of this "force", this **creative power**, as an obvious, indestructible part of our true nature - this knowledge is rediscovered through the discovery and humble acceptance of a **state of powerlessness**, resolved by this very acceptance. The knowledge of a state of powerlessness covers and conceals the even more deeply-rooted knowledge of our creative force. The latter is like the key that opens us to the former, both indissociable in truth, like the front and back of the **same** knowledge<sup>211</sup> (\*), both objects of the **same** fear.

□ When I speak of "the force" buried in each of us, this is by no means an abstract thing and vague, with the verbal subtlety of a "philosopher", or a psychologist with a touch of philosophy about him. It's this strength that allows you to "do math" (or "make love". . . ) like a child breathes - that is, without prudently obliging yourself not to leave the wake left by your predecessors, and to repeat with application the gestures and recipes (or the clichés. . . ) that were theirs. ) that were theirs; and it's also one that gives you the courage and humility, in your own home as well as in that of others, to call a spade a spade and not take bladders for lanterns, even if in doing so you go against the most established consensus, or the most inveterate and well-honed mechanisms within yourself.(\*)

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<sup>210</sup>(\*\*\*) On this subject, see the note "Le renversement (3) - ou yin enterre yang", where (among other things) certain such "sensitive moments" of reflection are evoked.

<sup>211</sup>(\*) In this image, of course, the "**right side**" is the knowledge of powerlessness, of inauthenticity, of the "crack", while the "**left side**", even more hidden, is the knowledge of our undivided nature and creative power. I've found again and again over the years that it's the "flip side", the more deeply buried knowledge of the two, that is the object of the greatest fear, and the most vehement denial. It's not so much the familiar, anodyne state of being a trained and (more or less) "learned" monkey that worries anyone, but rather the innocence of the child who feels things as they are and calls them by their name, and who does and says as he feels, without shame at being different from what's "expected" of him.

<sup>212</sup>(\*) (December 16) The action of the creative force in everyone, the force of renewal (or "child force"), can be recognized by its fruits, both in the works of the hand or mind, and in the facts of everyday life, in our relationship with others and with the beings and things around us. Over and over again, I've noticed that creativity in everyday life is much less of an issue.

The first example I've come across is one that's sure to get the heart racing of any young (or even not-so-young) glory-seeking researcher. Who wouldn't want to be the intrepid pioneer of sciences still in their infancy, and as such feature prominently in every textbook, like Kepler, father of modern astronomy! But when it's a question (as Kepler and others did) of tenaciously spinning one's own yarn in solitude and indifference (if not disdain or hostility), for thirty years or even just one - then suddenly there's nobody left! We don't mind being in the

p. 677 manuals, in good company in short, but one is also **afraid of** being alone, if only for a year or even **seulement** one day. But he who "knows" the presence of the force within him (and to know it he hasn't had

never to speak of it, either to others or to himself. . . ) - he also knows that he's **alone**, and that being alone doesn't worry him. And whether he'll be in the textbooks is the least of his worries - especially when he's working.

As it happens, this same Kepler, in his very work, "went against the best-established consensus" in his science, and established for millennia at that. In his day (when the Inquisition still existed), this was even more inconvenient than it is today, when you have a good chance of losing your job, or not finding one, but without the risk of ending up at the stake. Coming back to Kepler, I don't know what he was like in his everyday life, with regard to the "best-established consensus"; perhaps he kept his nose to the grindstone, like everyone else. What's certain is that today, as in the past, there aren't many people who would deviate even a hair's breadth from this consensus. It's undoubtedly the same old tobacco - the **fear of being alone**, the flip side of a deep-seated need to be alone.

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We're not just talking about "works" (in the conventional sense, i.e. tangible "products" shaped by the hand or mind, of creativity).

The presence of continuous creativity in a person's life is a sign of continuous "contact", however fragmentary and imperfect, with the creative force within him or her. This is something different from the mere presence of "gifts", and of a continuous investment of energy to take advantage of them, expressed by a more or less important production, also more or less "rated", but which does not have, in itself, a creative virtue, a virtue of renewal.

In my intellectual pursuits, and particularly in my mathematical work, with modest "gifts" (but considerable investment), it seems to me that this "contact" with the force within me, i.e. the tacit and profound knowledge I had of it, was virtually intact. In other words, I was pretty much "functioning" on my full (creative) capacity in this (admittedly very fragmentary) area of my life, with virtually no loss, detour or blockage of energy by the usual "friction effects". One of the most common of these is a certain pusillanimity, which so often renders us deaf to the inner voice telling us what to do, when what it's teaching us is precisely "new", that is, leading us down paths that only we tread. This kind of inhibition, almost absent from my relationship with mathematics (and, it seems to me, more and more so as the years go by), has, on the other hand, existed in other areas of my life just as much as in anyone else's, and particularly in "everyday life".

Returning to the subject of mathematics, I see a kind of reversed relationship in my brilliant ex-student. He has "gifts" that have always amazed and enchanted me, in no way comparable to my own. (It's true that the longer I live, the more I see that this **is** by no means the essential thing, to be innovative in science or elsewhere; see on this subject the reflection in the note "Yin the Servant (2) - or generosity" (n° 136).) His investment in mathematics is considerable, as was mine in the past, and from an early age he benefited from exceptionally favourable conditions for the development of his gifts, and for the conception and elaboration of a body of work commensurate with them. Twenty years on, I'm still waiting for this work, and I'm still not satisfied! Surely there is some "contact" with the creative force within him, attested by the beauty of such things he has made - but this contact is disturbed, tormented. My friend's relationship with his work, and even within his work itself, is one of conflict - work becoming, more and more over the years, an **instrument** in the hands of the "boss" to satisfy **his** cravings, alien to the child's thirst for knowledge and discovery.

I doubt that such a conflictual relationship can be resolved without first being assumed - that is, first and foremost, acknowledged. At least, not once in my life have I seen such a thing happen, without the other. This is what led me to write that the knowledge of our powerlessness was "the key" to regaining full knowledge of our creative power, and thereby also full creative power itself. In my mathematical work, the question has not arisen, because there has been no deep blockage in this work, equivalent to partial impotence, which would have made me "function" on only a small part of my possibilities. On the other hand, the question arose for me as it does for everyone else, at the level of my daily experience, in my relationship to others and to myself, to my body and to my body's impulses. It was at this level that I experienced, over and over again, that the realization of a blockage, a "powerlessness", was indeed the **key** to freeing imprisoned creativity.



universal in man: the need for approval, for confirmation by others (and would there be only **one who** approves and confirms). ... <sup>213</sup>(\*)

#### 18.2.10.5. (e) The secret nerve

**Note 145** But I've strayed from my point again! I had started from the realization that my "reasoning" of last night was off the mark, when I wanted to "get across" this conviction of mine, that my friend's motivation for playing the role I know in my Burial, and in the way I know, was not **greed** (for prestige, admiration, honors, power). It's true, of course, that by trading a child's impulse for a **role**, he'd made "a bad bargain", even from the point of view of "returns", prestige etc. But that proves absolutely nothing. But this proves absolutely nothing. Such "miscalculations" are, moreover, the

almost absolute rule, it seems to me, and by no means the exception, in the choices (at the unconscious level) of our principal investments and options. But even though the reasoning is worthless, I nevertheless p. 678

There's no doubt that what I wanted to convey is the perception of a reality: that it's **not** this very real greed, which has taken a growing and truly devouring part in my friend's life, that it's not this **greed** that constitutes the **nerve** in this role played by my friend, as **the** key figure in the implementation of my funeral.

If I take a closer look at this clear-cut feeling (without any further attempt to "establish" its validity!), I come to the following conclusion: it's this **gratuitousness** in the antagonistic or malevolent act, a gratuitousness that has often left me speechless, that doesn't "fit" at all with the all-purpose "explanation": greed. As far as prestige, admiration and honors were concerned, at least, and even "power" in the ordinary sense of the term, my brilliant ex-student and friend gained nothing, either in the moment or in the longer term, by playing on his former master's "discreet and delicately measured disdain", of which he had the secret; or by using the same disdain (perhaps less delicately dosed) towards a researcher of lesser status than himself, or towards his present or past work, in such a way as to discourage the one whose confidence in his own faculties of judgement was not as firmly anchored as it was in me; or for yet another, who had courageously persevered against the general disdain of which my friend set the tone, by robbing him of the fruits of his perseverance against all and sundry. While it's true that in this last case, as in others, my friend pretended to appropriate the fruits ripened by others in solitude (and sometimes in the disdain of his elders), this "benefit" (in the "Pouce" style<sup>214</sup> (\*)) is so derisory, when one considers **who** the appropriator **is**, that the "explanation" put forward goes up in smoke itself!

As far as I'm concerned, I know for a fact that it's not **this** benefit that's the "nerve" of the company. of such appropriations. On the other hand, I sense in it **the intoxication of a certain power** - a power more delicate, and no doubt more exhilarating, than power in the conventional sense, as a man of science and importance.

commonly exercised by sitting on Committees, Councils, Juries and the like, directing an Institute, or p. 679 research of brilliant young researchers, or talking into the ear of a minister. The "intoxication" of which I speak appeared (for the first time in the reflection) in the note "La Perversité" (n° 76), when I suddenly find myself confronted with "an act of **bravado**, a kind of intoxication in a power so total, that it can afford to even to display (symbolically. . .). . . its true nature of "perverse" spoliation of others".

It was a dazzling act of bravado, ostentatious and yet at the same time **hidden**, informal, slipped in there casually, with even a semblance of an explanation for the strange name "faisceaux".

<sup>213</sup>(\*) Here I'm reiterating, by another means, observations that had already appeared in the sections "The forbidden fruit" and "The solitary adventure" (n° s 46,47), and also, in passing, in the note "Acceptance" (n° 110).

<sup>214</sup>(\*) See the notes "Pouce!" (n° 77) and "Appropriation et mépris" (n° 59) about this style of appropriation in my brilliant friend and former student.

perverse", what could be more natural? We're going to shed some light on this in three words, plus a short list of "things that should have found their place" in our modest but brilliant article. ... <sup>215</sup>(\*).

Once again, I recognize the purest "velvet paw" style, a.k.a. "thumb" style! - and behind the uniformity of a **style** that has become familiar to me in more than one person, I also sense the **common nerve**: this imperious, all-consuming **thirst** to wield power; a **certain power**, and in a certain mode - the power of the cat over the mouse, when he plays his Great Game with that perfect grace (which only the mouse is unable to appreciate to its full value), and with "the most exquisite delicacy" for sure - or the power also of a clever wife over her big dodo of a husband. . .

Based on my friend's case, I've already been led to talk about the "style" in question, and its meaning, in the general context of couples of all kinds. This was the subject of our reflection a week ago, in the note "Le renversement (4) - ou le cirque conjugal" (n° 138, December 8). Here, for the first time, the "nerve" of the "velvet paw" game (a.k.a. "Thumb!") appears with all the clarity it deserves, as in a **game of power**. A game of power, however, of a very particular nature: the fascination of the game for those who play it, its often all-consuming charm, consists precisely in the **occult nature of the power** that is exercised through it, this "neither seen, nor known" character, which makes it possible to play the other (**of him, never with him**).

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him. . . ), making him go round in circles as he pleases, always leading the dance, where the other baldly follows shot after shot.

□ **coup**, in pathetic response to those little blows delivered by invisible wires that one wields at one's whim and according to one's enjoy...

All I had to do was write down in black and white what I'd felt obscurely for years, without ever having bothered to formulate it clearly - all I had to do was make this short effort to condense into words what had long remained diffuse, so that what only yesterday seemed "enigmatic" to me (namely, the nature of a "certain force" in such and such a friend), suddenly opened up its obvious meaning to me! This "force" in him, or (as I wrote earlier) the "nerve" behind such acts which may seem "inexplicable" (or even "beyond comprehension"), I had already clearly identified in the reflection of December 8. But while the starting point of this crucial reflection was indeed a certain "enigmatic" game played by my brilliant friend, it was **another** experience, richer and more intense than the one associated with his person, that fuelled this reflection; an experience that had been fully assimilated (or very close to it), and which gave me an already-formed knowledge that the more epidermal experience of my sporadic relationship with my friend Pierre could not then have communicated to me.

Of course, in the end, it was this experience that I had to understand, and thereby fully assume; and if I launched into a digression on the "couple's carousel" without any inner reservations, it was because I felt that this carousel had something to tell me about my relationship with my friend. His thoughts continued to linger in the background, like a discreet background note.

However, the two didn't come together completely that day, or in the days that followed. No doubt the moment was not yet fully ripe. For the junction to take place without reserve or effort, with the ease of the obvious, I first had to "clear the ground", by stubbornly and unhurriedly following, one by one, the most compelling associations that demanded my attention. I didn't rush things, and I knew that this was what I had to do - attend to what was calling me insistently, without letting myself be diverted by a "propos" or a "fil" (of reflection), or even by a program to complete.

While I'm weeding and hoeing, the forces of earth and sky are at work. When evening comes, all you have to do is pick up the ripe fruit, which falls into the open hand that welcomes it. ...

## 18.2.10.6. (f) Passion and hunger - or climbing

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<sup>215</sup>(\*) See note "Le Prestidigitateur" (n° 75").

**Note 146** □ (December 17) It seems to me that with the day before yesterday's reflection, there was a kind of unblockingp

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of an understanding that had remained undecided, a little stunned, in front of a quantity of facts and intuitions piled up before me in a rather amorphous heap - like a puzzle of which I had only succeeded so well in to assemble a few pieces here and there. Now I feel as if I've stumbled across **the** key "piece" of the unknown picture that needs to be reconstituted, around which the others will finally fit together effortlessly. In any case, I have no doubt that I have touched the "nerve" behind the role played by my friend Pierre in the burial of the master and his (more or less) faithful followers, and at the same time, the "nerve" of his relationship with me, the deceased master.

This craving to play with a certain power, discreetly pulling invisible strings with an air of candor - this craving must surely have been present long before I met him, ignored by himself and everyone else. If I didn't see it manifest itself in the first years we knew each other, before the episode of my departure (in 1970), it's undoubtedly because in those years of intense learning and the blossoming of a delicate and powerful thought, my friend's energy was totally absorbed elsewhere. The conditions were ideal, in fact, to serve as a springboard for his exceptional abilities. The episode of my departure, first from the institution of which we were both a part, and then (in the year that followed) from the ma- thematic scene, was a crucial turning point not only in my own spiritual adventure, but surely in his as well. It was this episode that suddenly opened him up to means of power that only the day before he wouldn't have dared to dream of: firstly, the power to "oust" from the scene an ex-master who was taking up a great deal of space there, and from whom he had previously confined himself to discreetly distancing himself<sup>216</sup> (\*); then, when it became clear that the latter was disappearing from the scene, the even more exhilarating power to make a certain School bearing the name of the deceased master vanish without a trace; and, finally, to cut off, in all its main branches (except the one on which he himself was perched), the blossoming of a vast program in the service of a vast Vision, which he himself had long nurtured<sup>217</sup> (\*\*).

The meaning of this great turning point in my friend's life appears to me as a kind of reversal in the mutual relationship of hegemony of the two dominant forces in his person, those which seem to me to take precedence over all others: mathematical passion, and the "craving" for power play ("à patte de velours"). The first of these forces is essentially "pulsional" in nature<sup>218</sup> (\*), the second is egotic, "ac- quised". Before the turning point, it's the drive for knowledge that dominates my friend's life (as far as I know), while the drive for power is more or less dormant, in a state of vacancy. At the end of a vertiginous social ascent in the space of a few years<sup>219</sup> (\*\*), and in a situation that suddenly presented a draconian choice, it was the temptation of power and its secret intoxications that prevailed (with the hand held high, I believe, and without any desire to fight) over the passion for knowledge. The latter does not disappear

<sup>216</sup>(\*) On the subject of this concern to distance oneself, then to evict, see the notes "Eviction" (n° 63) and "Brothers and spouses - or the double signature" (n° 134), as well as the sub-note (n° 134<sub>1</sub>) to the latter, and finally the section "The unfinished harvest" (n° 28).

<sup>217</sup>(\*\*) See, on the subject of the liquidation of a "School" and the "chainsaw" effect, the notes "L'héritier", "Les cohéritiers... ", ". . and la tronçonneuse" (n° 90, 91, 92) and the first four notes of the Cortège "Fourgon Funèbre" (coffins 1 to 4), n° s 93-96. On the subject of the vision that was buried, see the two glimpses (in two different lightings) given in the two notes "My orphans" (n° 46), and the sub-note n° 136<sub>1</sub> to the note "Yin the Servant (2) - or generosity".

Note that in the main text, the expression "et ce faisant. . ." (" . . . to cut short... the blossoming of a vast program. . ") is not adequate. The liquidation of a School was the **first** radical "chainsaw stroke" to "cut clean" a set of master branches, but not the last (as witnessed in particular by the notes-cercueils cited, n° s 93-96).

<sup>218</sup>(\*) The fact that mathematical passion is "pulsional in nature", that it is an expression of the "child" (aka the "worker"), does not prevent it (as is forcefully pointed out in the same paragraph) from also being invested more or less strongly by the "cravings" of the "boss" - and this is part of the common lot (from which I have been no more exempt than anyone else) in the relationship between "the worker" and "the boss".

<sup>219</sup>(\*\*) On this subject, see the note "L'ascension" (no. 63').

not from the stage, but is now a vassal and humble servant of Fringale, an **instrument** in the latter's hands. Passion (aka "the worker") goes about her work under the jealous eye of Fringale, aka "the boss", who never leaves her side. As the worker has good tools (not all of which are forbidden to him), and good hands, even if he's kept short in this way, he continues haphazardly to maintain production and the company's reputation. But it's not as it used to be, of course, when the (sometimes very childish) worker took his

The boss was far away and only came once a season!

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□ The subsequent evolution seems to me to be more quantitative than qualitative. It's the progressive-  
The boss's **tactics**, following a style that remains uniform, while the boss-worker relationship doesn't change a bit. This type of boss is cautious by temperament, and only likes to venture where he's sure of winning. To do this, he needs to be sure of the ground - or, alternatively, to be sure of the tacit approval of the entire "Congrégation", starting with the smaller group of ex-students of the deceased. The evolution of the personal relationship maintained with the latter, against all odds, is a faithful reflection of the evolution of "local knowledge". There is a gradual **escalation** in the boldness of the game of power and contempt, culminating after twelve years (in 1981) with the prowess of the Colloque Pervers, where all restraint (and even, all caution) are cheerfully thrown overboard in the general euphoria<sup>220</sup> (\*). And so it took twelve years for my friend to convince himself that the ground was so fertile that no caution was called for: any shot wins! The time was ripe, decidedly, to finally bring out the secret weapon, the **motives** - exhumed under an alternative paternity the following year<sup>221</sup> (\*\*).

I don't feel motivated to retrace here the successive steps of this twelve-year climb, even though I have everything in hand to do so. That would be the work of a chronicler, as I did enough of it in the unexpected "investigation" pursued in the first part of L'Enterrement (or "La robe de l'Empereur de Chine"). These "steps" of an escalation seem to me like so many **probing shots**, launched by my friend in the direction of a mute Congregation, with the same answer each time: he could go there! For nearly fifteen years, she has been his silent ally and guarantor, while he has been, without realizing it or caring, her docile instrument<sup>222</sup> (\*\*\*).

### 18.2.10.7. (g) Daddy-cake

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□ **Note 147** I don't know if this craving in my friend is exercised against others as well as myself, and younger thematians in whom he smells my "scent". I haven't heard anything to that effect. However, it's clear to me that it was through his relationship with me, and thanks to a situation that is certainly unusual in the scientific world, that this propensity in him, which had been living in the shadows, became, overnight, an all-consuming craving. During the episode of my departure, when he explained to me, with all the appearance of seriousness, that he had given his life, totally, to mathematics<sup>223</sup> (\*), he undoubtedly "believed" what he was saying, and I myself, a little stunned though I was, didn't think of questioning his words. And yet, if I'd had a finer ear, or to put it better, if I'd had the maturity to listen and trust a "finer ear", which does exist in me as it does in everyone, I would have known that what he was telling me about himself might have been true the day before, but that it wasn't true that day. It was a noble reason given for a dubious deed, a deed that neither he nor I had the simplicity to face up to, even though its meaning was obvious. It was **something else** than such a passion, which had in those days seized the hearts of both of us.

<sup>220</sup>(\*) On the subject of the "Perversity Colloquium", see Cortège VII "Le Colloque - ou faisceaux de Mebkhout et Perversité", notes n°s 75-80.

<sup>221</sup>(\*\*) On the subject of the exhumation of motifs, see the notes "Souvenir d'un rêve - ou la naissance des motifs", and "L'Enterrement - ou le Nouveau Père", n°s 51,52.

<sup>222</sup>(\*\*\*) See note "Le Fossoyeur - ou la Congrégation toute entière", n° 97.

<sup>223</sup>(\*) On this episode, see the note "Brothers and spouses - or the double signature" (n° 134).

the reins of his life, never to let go until today.

So it was my person, or rather something in my friend's relationship to my person, that (given the right opportunity) triggered this drastic change in the nature of the force dominating his life, and in the direction of his investment in mathematics. This is the moment to remember the famous "strands" or "aspects" of Burial, highlighted in the reflection of the 13th

November (in the note "Rétrospective (1) - ou les trois volets d'un tableau" n° 127), and in the note that follows it ("Rétrospective (2) - ou le nœud du tableau", n° 127'), which had time to get a little lost along the way.

since then. I pretended to remember it, just a little, in my note of ten days ago, "Patte de velours ou les sourires" (n° 137, December 7). In particular, I reconnected with the intuition of the eternal role of "adopted father" that I had to play with my young friend, and which, it seems to me, has remained active and intact.

to this day. On the occasion of this reflection, I would like to reiterate my unreserved conviction, which must have formed and taken shape gradually over the course of at least the last six or seven years (since p. 685 even longer, perhaps): that it was "around this aspect (the paternal aspect in his apprehension of my person) that the conflict arose - a conflict that already existed in him long before he ever heard my name...". (So that's the famous "Superfather" part, while the "Supermother" part is still in limbo, at least for the moment).

In fact, it's barely a page later that the famous "smiles and velvet paw" style makes its first, quick appearance, as an object of attention. In the days that followed, the associated associations seemed at first to distance me from my friend's person, as well as from the occult "paternal" aspect of the role my friend had assigned me in his life. This aspect has not been mentioned again until today - you can't think about everything at once, let alone talk about everything at once! In terms of thinking, however, it seems to me that somewhere, in the indistinct but nonetheless present and active background, the thought of this paternal aspect must have been present, it must have acted as an effective and discreet stimulus to this long digression on a "claw in velvet" style. After all (I'm making this clear to myself now, after the event, but it must already have been there in the form of a diffuse yet peremptory motivation...), the "father" figure is no stranger to this famous style, quite the contrary. In fact, it's fair to say that the very first person in a little girl's life (or boy's, for that matter) to be led gently and smoothly (though not always tenderly) by this style is none other than Dad!

And as long as the innocent kid (or boy) adopts and makes his (or hers) own this style and know-how - which must become second nature almost at the same time as learning to speak, or almost - the very first guinea pig and beneficiary, no doubt, will be that same big daddy! More often than not, when I've seen this game played, it's been accompanied by the hidden anger of a grudge, as well as a deliberate attempt at derision. And of course, in most families, there's no shortage of reasons to hold a grudge against the father, or even those cleverly suggested (or even created out of thin air) by the tender mother.

wife. In my friend, however, I never at any time sensed such a nuance of rancor or spite. When I saw him injure or harm "for pleasure", it was **really** (so I felt) **for pleasure alone** ;

not (I think) the pleasure of the suffering or humiliation itself that he inflicted, but rather the secret intoxication of exercising, at his own pleasure and in that particular style in which he was a master, a **power** - more exhilarating or even more piquant, no doubt, by this ingredient with a "**perverse**", "**forbidden**" connotation (harming, or causing suffering **for pleasure**), and yet which he could indulge in, delicately and casually and apart from that, to the hilt and gogo. . . <sup>224</sup>(\*)

<sup>224</sup>(\*) For a detailed illustration, see the note "La Perversité", n° 76.

### 18.2.10.8. The nerve within the nerve - or the dwarf and the giant

**Note** 148 (December 18) With last night's reflection, I feel that this "foreground" of the Burial painting, centered on the relationship between my friend Pierre and myself, continues to emerge from the mists of misunderstanding and confusion. For some time now, I've been faced with the task of inserting a certain "Superpère" section into this foreground (among others), and although I hadn't really formulated it clearly, this section didn't really seem to want to fit in willingly. If there's one student I've always felt completely "at ease" with, not tense at all and at no time that I can remember, it's him! Admittedly, I have little recollection of our very first encounters, and I can't claim that there wasn't that tension in him then - often barely perceptible, yet very real - which arises when we first approach someone invested (in one capacity or another) with authority or prestige, and towards whom we have a particular expectation. It is at least probable that such a tension must have been present, and that I paid no more attention to it than to any other young researcher I happened to meet. What is certain is that, if there was any tension at first contact, it quickly vanished without a trace. To use the image that appeared last night, he was as comfortable with me as a kid (or ex-kid) is with a sugar daddy he's never had to fear, and who has rarely refused him anything.

I thought about the situation again last night, after stopping to write. It now appears to me that my friend's relation to me was operating on two quite distinct levels, and (it would seem) without communication mutual. One of these levels, which undoubtedly became established in the weeks and months following our meeting, was that of the personal relationship - that of the "sugar daddy", therefore, kind as can be, not impressional at all, himself a bit of a child on the edges, including in his work, to such an extent that there is a nuance, I would almost say, **maternal** towards him, which I have already had occasion to mention once or twice: that of a child, giddy and a little boisterous, and above all as naive as any. It's also true that, in terms of his work, and objectively speaking, he really had no reason to be impressed. Of course, I knew a lot of maths that he didn't (and that he'd learned in a few years, by playing around), and above all, I had an experience of mathematics that he still lacked. But he had a speed of assimilation, and an acuity of vision to quickly recognize himself in muddled and confusing situations, by which he often amazed me, and which I lack. If I myself sometimes impressed colleagues, it was above all due to the uncommon **slaughter** that I have in my work, due above all, I believe, to a certain approach that I have to mathematical work. But there was certainly no reason for my brilliant young friend to be impressed, when his own slaughter, provided he started writing (which he didn't mind at all), was far more effective than mine.

This level of my friend's relationship with me, the "sugar daddy" level, seems to me to include the totality of his conscious image of me, and a good part of his unconscious image too. It's this image, it seems to me, that elicits in response, following paths no doubt established since childhood, a kind of reflex craving, that of the famous "claw in the velvet" game - a game that requires us to be entirely "at ease" with our partner, entirely "sure of him" and thus also sure of ourselves<sup>225</sup> (\*). This is the level of complete assurance, based on an intimate knowledge of a situation, corroborated again and again

<sup>225</sup>(\*) (December 29) This assertion is only apparently contradicted by cases (which do not include my friend) where the "playmaker" seems (at first sight at least) to be impressed, even subjugated by the person he's leading. This is, however, a **pose** for the sake of it, of which the actor himself is the first dupe (on a conscious level, I mean) - which is essential to give this pose a certain air of "truth" that can't be improvised! The most extreme case of this kind of play that I've ever known is that of my mother in relation to my father. On this subject, see the two notes "Le renversement (1) - ou l'épouse véhémence" and "Le renversement (2) - ou la révolte ambiguë", n°s 126, 132.

still by experience, which is interpreted  $\square$  in a fully concordant way by the faculties of perception and appreciation, both conscious and unconscious. The game itself is occult, unconscious to the player himself (I presume so, at least), but the feeling of assurance and the perception of reality that underpins it are in the conscious, rational, "objective" realm.

The other level, on the other hand, is entirely unconscious (at least that's my impression), uncontrolled and uncontrollable, of an irrational nature that seems to defy and make a mockery of any reasoned or reasonable knowledge of "objective" reality (which I've just recalled). At this level, the personal relationship itself, linked to any realistic perception of the Other, disappears. I myself appear as a giant, powerful and secretly envied, and my friend feels like a dwarf, overwhelmed by the conviction of his irremediable insignificance, and consumed at the same time by the insane desire, not to be a giant himself when he is a dwarf by immutable condition, but somehow to **rise to** his level, to **pass himself off** as a giant at the very least, or, more secretly and insidiously still - the insane desire **to be that giant himself**, or at the very least, to **pass himself off as one**. I think I detect yet another nuance in this desire, which is like the echo, in deeper layers, of the desire present in the layers close to the surface, which finds symbolic satisfaction precisely in this "velvet paw" game, and is its nerve and spring: the desire for **role reversal**. In the upper layers, it's the reversal of yin-yang, dominated-dominant, object-subject roles that's at stake. This relationship is not the case here, however, as the giant has no desire to dominate the dwarf - he's content to be a giant, and thus, without knowing or caring, to be a perpetual, burning challenge to the one who feels overwhelmed by his irremediable dwarf condition... . This superb ignorance in which he feels himself held, he feels as a tacit contempt and as an affront. It's this relationship that he's determined to overturn, himself appearing as the giant, and consigning the latter to insignificance - insignificance through **oblivion**, if not insignificance through **derision**, in return for the ignorance and contempt in which he feels himself held.

I said earlier that the two levels, "papa gâteau" and "géant", "would seem to have no mutual communication". On reflection, it seems to me  $\square$  plutôt now that there is indeed communication between the two,

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if only by this desire for reversal: the desire at one of the two levels now appears as an "echo" of the similar desire already seen at the other. At first glance, it seemed to me that this reversal of roles, at the deeper "dwarf-giant" level, was not a yin-yang reversal of roles. What is true is that this reversal is not of the dominated-dominant type indeed. And yet, on further reflection, there's no doubt that the **values** embodied by the giant are yang and superyang values, while the dwarf appears as the embodiment of yin non-values - in terms, I mean, of my friend's ideological options, not so very different from the options that were still mine in the early years of our relationship<sup>226</sup> (\*).

This statement will become clear, no doubt, once I've established a bridge between the image of "the dwarf and the giant" and reality, or at the very least, explained the origin of this image in the history and prehistory of the relationship between my friend and me. As far as "prehistory" is concerned, it's hardly necessary to point out that a conscious or unconscious image of this kind only comes into being as a result of the deep-seated "self-contempt" I've already mentioned several times in my reflections; or, to put it more accurately, that such an image is nothing other than a tangible, more or less concrete **materialization** of this contempt. Perhaps I could even say that this "secret conviction" is on the lookout for a situation to support it, while at the same time

<sup>226</sup>(\*) This concordance in the choice of "yang" or "superyang" values lasted until I left the company in 1970. In the years that followed, my value system at the conscious level "swung" towards "yin" and "superyin" options - see the note "Yang plays yin - or the role of Master", n° 118.



the scarecrow-image that expresses it. I believe that in everything in the psyche, however deeply buried, there lives a force that prompts it to express itself, often symbolically. This expression may often remain unconscious, but it is no less active, quite the contrary, in the facts and gestures visible in everyday life.

To return, this time, to the **story** of my friend's relationship with me, I'm sure she is too, begins even before we met. He must have heard of me around the time of his first contacts with the world of mathematicians, in Brussels, around 1960 - so four or five years <sup>before</sup> we met, when he was sixteen or seventeen<sup>227</sup> (\*). It's no coincidence that he asked me, and no one else, to teach him mathematics, or at least to teach him what was to become the central theme and tool of his work (namely, algebraic geometry). Before we met, the way I appeared to him (at least as a mathematician) could hardly have been anything other than my own brand image, making me a kind of heroic and prestigious embodiment of the core values of the mathematician world, and this at a time when he himself was a modest student, fresh out of high school. This image he had of me, and which was the very one I liked to portray, was no mere Epinal image, made to make glory-loving high-school students dream. It was based on tangible realities, and he certainly had enough flair to smell them in those years, in contact with mature mathematicians who were well into the game. From 1965 onwards, he was better placed than anyone else to take my measurements himself. I sensed in him a fascination for a vision that was opening up to him, born and matured in me over the past decade, and which continued to unfold and develop before his eyes. There was no doubt in my mind that these visions, which he made his own "as if he'd always known them", would serve him in the full light of day as inspiration and tools to develop even more far-reaching visions and work, within his means. This was not to be - and it is only in the light of this long meditation on a Burial, almost twenty years later, that I can glimpse how the fine, passionate perception of what I had to convey to him, must have served **at the same time** to flesh out and support' by first-hand elements of irrefutable reality, a **scarecrow-image**, aberrant; an image likely to **paralyze**, like the "intimate conviction" of which it is an expression. The very acuity of his perception of a "greatness" and depth in what I was transmitting to him, and which he was the only one to have made his own (and without effort) in its entirety - this acuity and vivacity which were his strength, then turned against him, making the aberrant image even more striking and peremptory.

Three days ago, I thought I'd touched the "nerve" of the role my friend has been playing for nearly fifteen years - and there was no doubt then that I'd just touched a nerve center: this all-consuming **craving** for a certain **game**, a delicate game of power, which was at the same time the symbolic and ephemeral satiation of the desire for a certain role reversal. . . With today's reflection, going down into deeper layers, it seems to me that I'm now touching on the **nerve within the nerve**, the even more secret **sting** that ceaselessly arouses and sustains this craving. For at the level of the "sugar daddy" there is certainly the opportunity and

<sup>227</sup>(\*) (December 29) I found this chronological information in the "Note biographique" (two pages long), by Pierre Deligne, written in 1975 on the occasion of the award of the "Prix Quinquennal" by the (Belgian) "Fonds National de la Recherche Scientifique" (Rue d'Egmont 5, 1050 Brussels). I intend to return to this biographical note in a later post, where I'll talk about Deligne's visit to my home last October. It was during this visit that I learned from him of the existence of this notice, which he was kind enough (at my request) to send me later. It was in this note that I also found the concrete form "the dwarf and the giant" of a certain image in my friend, a diffuse conception of which had gradually emerged in the course of reflection on L'Enterrement. It began to appear in the note "L'enterrement" (n° 61), and became clearer, notably, in the course of the reflection in each of the notes "L'éviction", "Le noeud", "Le renversement", "Le massacre", ". . . and the chainsaw", "L'Eloge Funèbre (2) - ou la force et l'auréole". It is only with the present note that this perception begins to "settle" into a coherent overview of the "foreground" of Burial.

(March 1985) For Deligne's biographical note, see "La profession de foi - ou le vrai dans le faux" (n° 166).

to play this game in complete safety, leading the dance with nonchalant delicacy, and sure to win every time. But no doubt the charm of easy opportunity dulls in the absence of a spur. And as I noticed only yesterday, there's no sting of pent-up grievance, of secret resentment - which is why we call him "cake"! This missing sting, in short, is something I've just touched on, when, in the course of associations, and as if under the dictation of a knowledge that would have been there all ready long ago, I was led to describe this "other level", "uncontrolled and uncontrollable", where a dwarf and a giant live side by side.

And the initial impression of a still confused intuition, that between the two levels there was no mutual communication, suddenly disappears, giving way to an understanding, expressed and aroused at the same time by the double image of the "nerve within the nerve" and the "goad". In terms of "layers", some superficial and others deep, I'd like to use a third image again, saying that these nourish or maintain the movement of the others, that they are the deep foundation, firmly anchored in the structure of the ego. Without this foundation, the surface agitation would quickly dissipate and fade away, giving way at last to something else. ...

## 18.2.11. The other Self- even

### 18.2.11.1. (a) Rancune en sursis - ou le retour des choses (2)

**Note** 149 □ (December 20) Since the reflection of five days ago, and that especially continued in the second p. 692 from that day's notes, "Le nerf secret" (n° 145), I feel that work on the famous "foreground" of the Burial painting has suddenly taken another turn. Before this reflection, I'd felt in the position of a It's a little embarrassing to be faced with a jigsaw puzzle, where you don't seem to understand much at all. Since April, I'd been gathering the pieces one by one, and taking careful inventory. It wasn't that I was short of pieces, no, it was more that I felt I had too many! In any case, there had to be enough to make a picture, partial perhaps, but a picture that would stand up. The last piece of the puzzle I threw on the table was that of "reversal" (of yin and yang), held in reserve from the very beginning of "The key to yin and yang" (as an "association of ideas" to which I promised myself I'd return), and finally bursting out with unforeseen force in "The key to yin and yang".

note "Les obsèques du yin (yang enterre yin (4))", November 10 (n° 124) The thirty-five days that followed, until five days ago, were spent turning over and over the pieces already uncovered, as the most compelling associations demanded my attention<sup>228</sup> (\*). I expected that, in doing so, the pieces would eventually come together, revealing the unknown picture. Nothing of the sort happened. On the contrary, they continued to thumb their noses at each other, as if fragments of ten different newspaper clippings had been thrown in a jumble and I had been left to assemble them! I was beginning to wonder whether I wouldn't be obliged, at the end of the day, to make a final inventory of the pieces, and another of the question marks concerning their assembly, and call it a day. ...

The situation changed five days ago, when, by dint of turning and flipping these famous coins, some palpating them and smelling them, something finally "clicked", when one of them (that of a **craving** behind a certain **style**) was suddenly recognized as "neuralgic". I had the immediate impression of a **qualitative change**, that a **perspective** that had hitherto been lacking was already being organized p. 693

<sup>228</sup>(\*) The "piece" that had been the starting point for all our thinking on yin and yang since the beginning of October only came back to the fore fourteen days later, on November 24, in the note "Le renversement (3) - ou yin enterre yang" (n°

from that particular piece. That's how I put it the following day, taking up my thoughts in the following note ("Passion et fringale - ou l'escalade", n° 146). And my premonition began to be confirmed the very same day, with the appearance of the "**papa-gateau**" piece, which was said to have been called the "neuralgic part" precisely for the purpose of fitting it without burrs!

The "**Superpère**" piece, which had always been there (already inherited from the first part of Récoltes et Semailles, and taken up again in the early days of "La clef du yin et du yang"<sup>229</sup> (\*)), now seems to have been written off, as if it had simply strayed there by accident. Under the fresh impression of the new "cake" piece<sup>230</sup> (\*\*), I tend to forget that this famous Superpère (not "cake" at all, as it happens) did indeed have something to do with the relationship between my friend Pierre and me, even if it didn't take center stage (which was not by a long shot. . . ). I ended up remembering it at the next session, of course - at the very moment, in fact, when I was about to explain to myself why this eternal piece of the puzzle had nothing to do with it! It was, in fact, "just the opposite" of the cake-piece, which had just placed itself with such ease. And then no, on closer inspection, this supposedly foreign piece, whose contours had remained the vaguest, suddenly clarified its shapes, "taking on those of the image-force (conjured up by none other than my friend Pierre himself<sup>231</sup> (\*\*\*)) of the **dwarf and the giant**. At first, when I saw it reappear in such strongly marked features, I expected it to be "uncommunicative" with the double neuralgic piece already in place (made up of daddy-cake, and the imperious urge to "make it work" - a little phone call here, a little phone call there. . . ). And now, on the contrary, it appears as "the nerve within the nerve", as an even more neuralgic piece, fitting together without friction or detachment with the part of the puzzle already in place!

p. 694 This piece, under its former name "Superpère", had already been □ maintes fois frôlée, and even taken in the hand and turned over and over like the others, and even (I remember now) declared to be the centerpiece, the "heart of the picture" and all that; but, perhaps for want of a striking image (provided by the interested party himself), and above all, no doubt, for its absurd, aberrant nature, entirely ludicrous even in terms of the coarse "common sense" of the current and universally accepted consensus, I was embarrassed and ashamed of the damn thing, it burned in my hand: no one (including a certain "myself" who tenaciously continues to live on inside me. . . ) would ever take it seriously! I might as well pack it in and "play" with more manageable pieces!

When I just spoke of "pièce maîtresse", "cœur du tableau" etc., in connection with the play that became "Le nain et le géant", it's the "self-contempt" aspect of course that I'm thinking of, rather than the "Superpère" aspect. For the moment, the latter designation for this piece-aiguillon, or "nerve within a nerve"-is hasty and unjustified. I mean, it doesn't seem, at first sight at least, that this famous faceless giant with oversized hands is anything like a father figure. If he needs a name, it's "Superman" or "Supermale", rather than "Superfather". So all things considered, the latter is still very much on the cards, for the time being at least, as is the "Supermère" piece (or "part"), to which I'll also have to return.

For the moment, the most urgent thing seems to me to be to try and situate the part of the picture already placed, with the "secret nerve" and the even more secret "nerve within a nerve", in terms of a yin-yang dynamic in the person of

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133).

<sup>229</sup>(\*) See sections on "The enemy father (1) (2)" (n° s 29,30) and the note on "The Superfather (yang buries yin (2))", n° 108.

<sup>230</sup>(\*\*) The term "new" piece may not be entirely justified. But it is a piece, at least, that had previously escaped inventorying, so obvious was it!

<sup>231</sup>(\*\*\*) For further details, see the last footnote of the preceding "The nerve within the nerve - or the dwarf and the giant" (n° 148).

my friend. On this subject, I have three hard facts. Two are expressed by the yin-yin "double signature"<sup>232</sup> (\*): my friend Pierre's basic tone is "yin", both in what we might call the "acquired personality", expressed above all in the tone of his relationships with others, and in the "innate personality" or drive, expressed above all (for an outside observer such as myself, at least) in his spontaneous working style, free from the interference of the "boss". The first fact, concerning the acquired personality, or the "structure of the ego" (or in more graphic terms, "the boss's **head**"), seems to indicate that this structuring took place in childhood, and from the very first years of life, by identification with a "yin" model. This does not exclude

a priori, that this model was the father, if he had himself (as seems to me to be the case) p. 695 an "acquired personality" with a basic yin tonality. But on the other hand, my friend's predisposition to a craving for a kind of power game which, in our country if not everywhere and always, is typically (if not exclusively) "feminine", and more precisely, which is **the** game among all others that the wife is wont to play with the husband - this predisposition makes me suppose that the identification was made with the person of the mother, and that it is from her that he has "inherited" this craving (or a propensity for such a craving), and that it is also from her that he has taken on the appropriate "style" (or "tactic"), that of the "claw in the velvet paw".

It's possible that the father was both a husband-cake and a father-cake, and that my friend had long since had ample opportunity to make him his first "guinea pig", and to get his claws (and velvet!) on him. But it's also possible that the propensity or predisposition in question in my friend remained unused until after his meeting with me, because the first designated target, namely his father, had yang aspects strongly enough marked to **"provoke"** this craving, and at the same time **give rise to** the tried-and-tested tactic of "making strong heads work". To tell the truth, none of the impressions I remember from the first years I knew my friend suggest that he was familiar with this game, or even that he had played it before. In any case, even with the benefit of hindsight, I can't detect any trace of it in his relationship with me, or with others, in his "spoiled brat" manner. So I'd be inclined to think that this propensity in him was still latent, and that it only developed and took the hold I know it has on his life and work, after my "death" in 1970 (when he was twenty-six), and thanks to a particularly tempting conjuncture.

The "third fact" to be recalled here is my friend's choice of a value system in line with generally accepted values, his choice of "virile" (or yang) values. Over the past fifteen years, these values seem to have increasingly turned to "superyang". In his case, there's an obvious contradiction in this choice: while adopting "official" **yang** values, he has nonetheless modeled himself, in most essential traits, after a **yin** model<sup>233</sup> (\*). And it's not just that that this choice of values is purely "bogus", that it's nothing but a false flag, flaunted for reasons of circumstance, and which would have course only in □the peripheral layers of the psyche. The image-force of p.

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the dwarf and the giant, acting from deeper layers, would lose its meaning, and also the imperious urge to overthrow that it arouses, if the valorization of yang were not also internalized in these layers. There's no doubt that this contradiction must give added impetus to this "intimate conviction" of crackedness, of insidious powerlessness - when (for want, perhaps, of an adequate "model" in his childhood on which to model himself) he knows (in his innermost being) that he is fundamentally **different** from what he **"should be"**!

If my friend, as seems plausible to me, didn't find in his father the traits that, according to the current consensus around him, should **have** been there, and that he could then have made his own, this must have aroused in him

<sup>232</sup>(\*) The idea of a "double signature" is introduced with the note "Brothers and spouses - or the double signature", n° 134.

<sup>233</sup>(\*) This is a kind of contradiction common especially among women, and one from which my own life has been exempt.

a diffuse **resentment**, a resentment that couldn't cling to any concrete grievance against a father whose only fault was that he was too much of a "cakewalk"! This resentment, lacking a "hook" to hang on to, would then have remained "**vacant**", **waiting for** a suitable target - a target who, first of all, was (by context) a father figure, and moreover, whose **aptitude** for this role was obvious, through the undeniable presence, perhaps even excessive, of those traits that were lacking in his "original" father. It's these traits, too, that make the newcomer "father" the ideal **target**, in the kind of "game" that's already all set in motion here, waiting only for the right partner, aka "the spare father", aka (here we go at last!) "the Superfather"!

All of a sudden, I seem to be back on very familiar ground, which I only now recognize. It's a terrain in which I've been a prisoner for twenty years, during the only marriage of my life (the marriage from which three of my five children were born). In the lines of the preceding paragraph, and without any deliberate intention (but rather as one who, cautiously, groped in the shadows to become aware of his surroundings), I have **also** just described in turn the neuralgic forces in the relationship to her father, and then to me, **of the woman who was my wife**. I can't say when or how the knowledge (or rather, the irrefutable intuition) of the silent, obstinate presence of these two forces in her, and of their mutual relationship, came to me. One day I knew, without ever having given it a moment's thought, that the inexorable force dominating my wife's relationship with me, from the very first days of our marriage, was driven by resentment towards me for not having been there for her, like **another real** father, in the days of a distraught childhood... .

□ It's true and I know, certainly, that there was nothing "clueless" about my friend's childhood, and that the personality

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that he developed and that I've known, from the sixties until now, bears little resemblance to that of my ex-wife. Yet, beyond the obvious dissimilarities, I see in the part of the picture that is emerging from the shadows, a striking similarity with another "picture", which is well known to me. This similarity appears in the nature of the relationship with the father (linked to a temperament of the father where yang traits are deficient), and in the repercussion of this on a relationship of adulthood which, in both of them, dominated his life, as the focus of the forces of conflict in both<sup>234</sup> (\*).

For a moment, I was about to overlook a third "similarity", which is not without consequence in my own life: in both the relationships in question, the **protagonist** was **none other than myself**. And what, in both cases, designated me for the role of "Superpère" that I was called upon to play, was (in addition to immaturity) that which since my childhood had been dearer to me perhaps than anything else in the world - that in which I had also invested myself most inordinately: a "build" that was more virile than nature... .

So once again, in a different and more penetrating light than eight months ago, I find myself with the feeling of a "return of things"<sup>235</sup> (\*\*) - with, today as in the past, a nuance of incredulous astonishment (it seems too "right" to be true!). And also, this time again but in more restrained tones than the sudden burst of laughter of yesteryear, there's the perception of a comic, adding to these inexorable "returns" the gentler note of humor.

<sup>234</sup>(\*) (February 19, 1985) There is a striking kinship between my friend Pierre's relationship with me, and (since the early days of their marriage) that of my former wife. Moreover, this kinship extends beyond the relationship to my person alone, in the sense that both have developed a propensity to make certain beings, to whom bonds of affection bind me (my children in particular in one case, pupils in the other), **instruments** for reaching me through them.

<sup>235</sup>(\*\*) See the note "Le retour des choses - ou un pied dans le plat", n° 73.

### 18.2.11.2. (b) Innocence and conflict - or the stumbling block

**Note** 150 ( December 22) Yesterday again, I didn't find the time to work on my notes, except for the careful rereading and correction of the previous day's notes. Over the last few days, my energy has been diverted by tasks of □correspondence and the like, and I'm gnawing at the bit (this is nothing new!) of myself p. 698

to find myself face to face with myself, to push forward the reflection I had begun. The writing is decidedly slower in this third part of *Récoltes et Semailles*, centred on the present reflection, "The key to yin and yang", where the dynamics of yin and yang are the constant thread for penetrating further into the meaning of Burial. If I didn't take the precaution of setting the alarm clock, to allow for an interruption in the work after about three hours (just to stretch the body, or to warn me that the clock is ticking and it's time to stop), the whole night would pass like an instant! The three hours have gone by each time, and I feel as if I've barely started (or restarted), with two or three unfortunate pages I've just typed, if not just one or two, just long enough to get round to some seemingly innocuous association I thought I'd step over in the process... ..

There's an impression of extreme slowness in progress, counted in pages per hour or per day - and the natural reaction to this impression, with a hot substance right in front of my nose pulling me forward, would be to double and triple my efforts, as I used to do until just a few years ago. But I know that this is the trap to avoid - the trap of this extraordinary "ease" in the work of discovery<sup>236</sup> (\*), when it's enough just to "push" forward, to be sure of moving forward indeed, slowly perhaps but surely ; like a man holding the ploughshare of good, hardened steel, pulled by a pair of powerful, impassive oxen, and slowly and surely making his way, furrow after furrow, through dense, sometimes rough earth, yet at the same time supple and docile to the shiny ploughshare that delicately and unhurriedly opens it up, penetrates it and turns it over in wide, brown, steaming swathes, bringing intense, teeming subterranean life out into the open. The pace may be slow, but the field is vast, and each furrow dug seems to barely make a dent in the expanse that remains uncultivated. Yet at the end of the day, furrow after furrow, the field is ploughed, and the ploughman returns happy: for him, the day has not passed in vain. His toil and his love were his seed, and his joy at work, and his contentment at the end of each furrow and at the end of a long day, are his harvest and his reward.

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□With the reflection of the day before yesterday, and perhaps for the first time in the writing of *Harvest and Sowing*, p. 699

I feel as if I've stepped onto the uncertain terrain of that which is not yet directly perceived or felt, and which remains (and perhaps will remain) **hypothetical**. Lacking eyes that know how to see in what seems to me darkness and night, I groped my way along a hesitant path, with no assurance that it was "the right one". When the path forked, I didn't flip a coin to see which way I'd go; I relied on my intuition and common sense to point me in the most plausible direction to continue, though I had no idea where it would lead. The path I was following, or tracing out for myself, seemed to "stick" to the facts I knew, and that was a good sign. But it wasn't out of the question, especially where these facts were tenuous, that another, quite different path would not have "stuck" just as well, on condition perhaps that I delved a little deeper into this or that raw fact... . Then, at the bend in the road and to my own surprise, I found myself

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<sup>236</sup>(\*) See note "The trap - or ease and exhaustion", n° 99.

suddenly found myself back on "very familiar ground", which I had long and painstakingly traversed, and which I had come to know and leave behind. A situation which, only moments before, had seemed obscure, shrouded in the uncertain mists of "without doubt" and "perhaps", was suddenly illuminated by the light of another situation which, for its part, was understood. As I wondered about the distant origins, in myself and in the other, of the conflict in the relationship between this friend and me, they seemed to be revealed by a deep similarity, suddenly glimpsed, between this relationship and another, which had weighed on my life with a completely different weight, for twenty long years.

The appearance of this similarity was so powerful, I confess, that this feeling of hesitation, uncertainty and trial and error vanished immediately, to be replaced by a feeling of assurance and conviction. When, at the end of the reflection, I speak of the feeling ("of incredulous astonishment") that it "fell too right to be true", this feeling was the response to another, in the background, which said that "it fell too right **not to be true**! And this feeling, surely hasty and unjustified in the present state of the facts at my disposal, has not been readjusted in the meantime; it is still present as a background note, whether I like it or not. Surely, without the help of certain experiences that I've come to understand and accept, and above all the long experience of my married life, the thought could hardly have occurred to me of this "vacant grudge" (of a grudge "on probation", in short); and this very thought was also the "detour in the road" that, in the space of a few moments, brought me once again onto the "very familiar ground" of my married experience.

p. 700 ☐ One could say, certainly, that an unconscious deliberate purpose will have brought me to a place already designated in advance,

which perhaps teaches something about me and this deliberate intention, and nothing about motivations in others. Just as it is possible that an assumed experience will have enabled me to apprehend a reality in others, which would otherwise have remained entirely enigmatic, for lack of my own sensitive "antennae" (and for lack of tangible facts concerning my friend's childhood, and the personalities of each of his parents).

It seems to me that I'm very close to completing my rough sketch (à bâtons rompus!) of the "foreground of the picture" (of the Burial.) To assemble the last pieces of the puzzle that remain in my hand, I'll use as necessary the elements of apprehension (however hypothetical they may be) that appeared in the reflection of the previous note. This will also be a way of testing their coherence with all the other facts known to me.

The day before yesterday's reflection, the "Superpère" piece of the puzzle clarified its shape and contours. I had first identified it, somewhat hastily, with the piece "The Dwarf and the Giant", where the giant nevertheless appears more as a kind of "Superman" in overwhelming format, and not as the "father", or a "Superpère". But this last piece ended up appearing again in the same reflection, this time as the target of a "resentment in abeyance", a resentment in search of a target, as if the aforementioned "Superpère" had been **called** by this very resentment and had appeared in response to this call, in fulfillment of a diffuse expectation. If that's the case, it's fair to say that if the Superpère (borrowing my build and features, which were apparently tailor-made for the occasion) hadn't appeared in my friend's life, he'd have had to be invented! That's what it's all about, with nothing more hypothetical for me, in the case of the woman whose husband I was - and whose target I was, moreover, "expected to be during a young life...".

Thus, the Superpère appears as the "face side" of that "faceless giant with oversized hands" from the play "Le nain et le géant". "The dwarf" must see him mostly from behind, the giant, no doubt doing his famous "demonstrations of strength" (referred to in the October 5 note "Le Superpère" (no.° 108)). So here we have the "Superpère" piece at last, fitting in with the "giant" side of the "Le nain et le géant" piece.

As for the "dwarf" aspect of this one, its outline has also been made clearer by the reflections of the day before yesterday, which join here those of the note of October 17 "La moitié et le tout - ou la fêlure" (n° 112). Again, as

so -often, the endless rejection of "yin", "feminine" traits, in favor of "yang", "masculine" traits'□ which makes p . 701 my friend finds himself "fundamentally different from what he '**should**' be", even though he has modeled himself on a predominantly "yin" model.

It's important to emphasize here that at no time in the past did I think, nor did I want to suggest, that my friend's person was marked by a predominantly yin **imbalance**, i.e. by a deficiency, a "void" on the side of the yang, virile traits in his acquired personality. Let me remind you that the main impression I got of him, at least during the first years I knew him, was that of a **balance**, a harmony, which made him so endearing to me and to all those, it seemed to me, who knew him at the time. This impression is very closely associated with another, which I've mentioned elsewhere<sup>237</sup> (\*) - that he seemed to have retained something of the freshness and innocence of a child, in his approach to things (mathematics in particular) and also, it seemed to me, to people. This balance, and this "freshness" or "innocence", are not subject to the slightest doubt in my mind - they're **facts**, and there's no question of trying to disguise them. They were expressed in my friend by a delicate sensitivity, and, when the occasion presented itself, by the nuanced and unambiguous expression of what was perceived and seen. There was a firmness, as there was a gentleness. The gentleness has faded over the years, leaving only the empty, muffled shell of a vanished gentleness - and the firmness has become closed and hard, behind a façade of precious, borrowed half-tones. A delicate yin-yang balance was transformed over the years (probably without anyone noticing) into the eternal yang imbalance - the same one, but in a different style, that had dominated my own life since childhood. That was his choice, and those choices can change - there's no such thing as a foregone conclusion! The fact remains that I've never known my friend to have gone through a period of yin imbalance, or to have been sluggish, careless or inconsistent, and I don't think he ever was.

All this makes it at least likely that the person who served as his childhood "role model", and who surely had strongly marked yin traits, was not lacking in yang traits to balance them out. If (as I'm inclined to believe) this person was his mother, then I presume she had fairly strong yang traits (in contrast to such traits, which are probably less pronounced in the father) to appear as□ "the best choice", as a "masculine" role model for a boy; and at the same time p . 702 time, to encourage the blossoming of a harmonious temperament.

At this point, all would seem to be well in the best of all possible worlds, in a close-knit family (perhaps) untroubled by any misunderstandings. All would be for the best, were it not for one tiny stumbling block, in the form of a mute and seemingly innocuous consensus: a boy is supposed to look like his father, not his mother...

### 18.2.11.3. (c) Providential circumstance - or Apotheosis

**Note** 151 (December 23) It seems to me that to finish assembling the "puzzle" of the foreground of the Burial painting, I only have to place one last piece. This is the one I called "la Su- permère", in the note "Supermaman ou Superpapa?" of November 11 (n° 125). This "Super" appellation had been inspired, first and foremost, by the "portrait" painted of me, with superlatives, in my Eloge Funèbre<sup>238</sup> (\*). A symmetrical reflex must also have been at play, since there's a

<sup>237</sup>(\*) On this subject, see the note "L'enfant" (n° 60), in Cortège V "Mon ami Pierre".

<sup>238</sup>(\*) See notes "L'Eloge Funèbre (1) (2)" (n° s 104,105), and "Les obsèques du yin (yang enterre yin (4))" (n° 124).



already had "Superpère" in the air, in more ways than one! On reflection, however, the name I gave to the image that had just appeared wasn't quite right. What was evoked by this superyin image had no maternal connotations whatsoever. If it was in symmetrical relation with another image, it was that of "Superman", with muscles of steel and an IBM software brain, rather than that of "Superfather". In this case, it would be "Superwoman" or "Supernana", with heavy tits up to her navel and beyond (not to say, up to her knees. . .), and asses to match, to make Hercules dream - as for the brain, let's not talk about it. ... a bit in those tones. The inadequacy of the language, too, must have forced my hand a little, given that there's no ready-made "female" counterpart to the famous "Superman" (itself a recent invention, incidentally, a modern version of a Hercules decidedly out of his depth). I'll go for "Supernana" anyway, for want of anything better. ...

It has to be said that I've been dragging this misnamed piece around for almost a month and a half, without really doing anything with it, other than recalling it here and there for memory's sake, as a promise that it would be taken care of,

□

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but later. In the end, she wasn't going to inspire me all that much, and it might well be because of that. name that didn't really fit. After all, of all the friends, (ex-)students and other colleagues I've had in the mathematical world up to the present day, I'd be hard-pressed to find a single one with whom I've played even the slightest bit of a "maternal" role, or for whom I've had the impression that they assigned such a role to me. Even those with whom I played a more "yin", receptive role, rather than the predominantly "yang" role of one who teaches, communicates and transmits, must be very rare - as far as I can see (after the years 1952, 53, when I did my thesis), only Serre, and even then. . . . If I try to remember what my current, not to say permanent, arrangements were in relation to other mathematicians, it was above all that I always had brand-new "carpets" to "place" (to use the image that was current in my time), not counting the "carpets" (also of my own making) that were less new but which (to my mind) hadn't really been used, so to speak. To put it another way, in my relationship with my fellow mathematicians, and even though we hardly ever talked about anything but maths (I must have been even worse at it than any of my colleagues and friends!), the yang predominance (or rather, the superyang imbalance) in my acquired temperament was back in full force, as in any other relationship. Perhaps even more so, given my inordinate investment in mathematics, an investment of an egotistical nature (needless to say) and motivated precisely by my long-standing superyang options!

It's these obvious aspects, manifested at every step in my relations with other mathematicians, that must have obliterated, to my colleagues as well as to myself, this **other** fact, in the opposite direction: that my style in mathematical work, and my approach to mathematics, are strongly predominantly **yin**, "feminine". It's this particularity, it seems to me, apparently rather exceptional in the scientific world, which also makes this style so **recognizable**, so **different** from that of any other mathematician. That this style is indeed "unlike any other" has come back to me through countless echoes, ever since I started publishing maths, and at least since my thesis work (in 1953). Moreover, this style has not failed to arouse resistance, which I'd like to call "visceral" - I mean, which didn't seem to me (nor do they seem to me today).

be justified by "reasons" that might be called "objective" or "rational". This reminds me that my thesis work (in which I introduced, among other things, nuclear□spaces), which I had submitted to *Memoirs of* the American Mathematical Society, had been rejected by the first referee, a well-known mathematician who had worked in the same field, and who had considered my work to be more or less muddy. It was thanks to Dieudonné's energetic intervention that my thesis was published, despite the unfavorable opinion.

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of the referred. I learned a few years ago that it is one of the hundred most cited articles in the mathematical literature<sup>239</sup> (\*) over the past two or three decades. I assume that if we have another twenty or thirty years of mathematics ahead of us, the same will apply to SGA 4, as (among other things) a basic reference for the topos point of view in geometrical topology; which SGA 4 has been classified as "unreadable" (among other qualifiers of the same water<sup>240</sup> (\*\*)) by my brilliant friend and ex-student Pierre Deligne. I know (as he knows himself) that this is one of the mathematical texts to which I have devoted the most time and the most extreme care, rewriting and having rewritten from top to bottom, in particular, everything concerning sites and topos and categorical "prerequisites". The reason for this exceptional care was that I felt that this was a real cornerstone for the development of "arithmetic geometry", the foundations of which I had been laying for decades<sup>241</sup> (\*\*\*). I also know that when I did this work, I had long had (without wishing to flatter myself) the master's touch for writing maths in a way that was both **clear**, where the main ideas were constantly put forward like an omnipresent thread, and **convenient to find** one's way around for reference purposes<sup>242</sup> (\*\*\*\*). While I may have been wrong to write (and have written) a detailed reference work forty or fifty years ahead of my time, the fact that times that were ripe (in the sixties) suddenly ceased to be so is not, it seems to me, my fault!

□ These latest associations with Deligne take me back to the period after my departure, when echoes in the same sense came back to me more than once "like puffs of insidious disdain and discreet derision". This nuance of **derision** was absent in the signs of "visceral resistance" to my working style, to which I alluded earlier, taking place before my departure. I don't detect any hostile or malicious intent towards me. I had occasion to evoke such signs even within Bourbaki<sup>243</sup> (\*), at least (if my memory is correct) until around 1957, when my work on the Riemann-Roch-Hirzebruch-Grothendieck formula dispelled any doubts that might have remained about my "solidity" as a mathematician. I don't recall perceiving any resistance to my style of work between 1957 and 1970 (the year of my "departure"), except occasionally at Serre<sup>244</sup> (\*\*), but never with a hint of enmity - it was more an epidermal reaction of annoyance. On the other hand, I had the impression that my friends sometimes felt overwhelmed, because I was moving too fast and they didn't want to spend their time just keeping up to date with my complete works as I sent them my paving stones, or told them (by letter or in person) what I was concocting.

I think I've understood the nature of the "visceral resistance" to my style I alluded to earlier. Its cause seems to me to be independent of the burial that took place later (where this resistance ended up playing an important role). This resistance is nothing other than a ("visceral") **reaction** to a "**feminine**" **style of approach** to a science (mathematics, in this case). Such a reaction is commonplace and "in the nature of things", in a scientific world which, as much and more than any other partial microcosm in today's society, is steeped in **virile values**, and feelings, attitudes, reactions (of apprehension, fear, fear, fear, fear, fear, fear, fear, fear, fear, fear, fear, fear, fear, fear!)

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<sup>239</sup>(\*) perhaps my memory is failing me here, and these are the hundred (or twenty?) most cited articles in functional analysis.

<sup>240</sup>(\*\*) See "The clean slate", n° 67.

<sup>241</sup>(\*\*) which is surely another reason why Deligne is so keen to discredit this text, that he sometimes even forgets his preferred half-tone style, and doesn't mince his words when debunking it! On this subject, see "La table rase", already cited in the previous footnote.

<sup>242</sup>(\*\*\*\*) In 1965, when he had just arrived at my SGA 5 seminar, Deligne familiarized himself with the part of SGA 4 that had already been written on the net, and wrote some of the presentations himself (using my handwritten notes as a starting point). This same Deligne learned from me the art of writing a mathematical text, and in particular the art of clearly presenting a dense and complex substance.

<sup>243</sup>(\*) See note (unnamed) no. ° 5, in the first part of *Récoltes et Semailles*.

<sup>244</sup>(\*\*) On this subject, see the note "Brothers and spouses - or the double signature", n° 134.

and rejection) that go with these values. The reaction of resistance to my particular style of work, the embodiment of a creative approach with a "feminine" underpinning, simply stems from the common conditioning of the scientist in the world of today and of recent decades - the scientific world, at any rate, as I've always known it.

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Like any other reaction resulting from conditioning, there is nothing "rational" about it, and in the one where it manifests itself, there's considerable resistance to even thinking about examining its meaning. It's strongly felt to be **its own justification** - a bit like the aversion to "faggot" in most good-natured circles, or that to "metèque", which is also very local. And yet, in my case, I didn't sense in this reaction a hint of (conscious or unconscious) enmity towards me, but rather an attitude of **reserve**, of unfavorable prejudice, **towards my work alone**. Only when it became clear that I was doing things with my style (or in spite of my style, never mind!) that people hadn't been able to do before (and that they couldn't really do anything else either, after the fact) - only then were these reservations relinquished, as if with regret perhaps... . In any case, if in some people these reserves remained in tacit, unconscious form, I was too locked up in my work and my tasks to perceive them.

To tell the truth, it seems unlikely to me that such a "visceral reaction" could magically disappear, simply because Mr. so-and-so has demonstrated theorems that we hadn't been able to demonstrate before. At the level at which deliberate statements of acceptance and rejection are made and unmade, one thing and the other ("such and such a way of working should not be allowed", and "Mr. so-and-so has demonstrated such and such theorems") are really unrelated!

You might say that it's only natural that things changed after I withdrew from the mathematical scene - once I was no longer there, in short, to "cut a rug" for those who would pretend to be picky about my style, but couldn't do the same with their own. This "explanation" is flawed, however, because it doesn't take into account the nuance of derision, of hushed malice, which didn't exist before. Nor is there anything in what I know of that would lead me to suppose that between 1957 and 1970 I had the time to make myself so disagreeable to the entire congregation of my fellow members that a grudge or revenge motive might have come into play after my departure. With many friends in the world I was leaving, I had maintained warm, sometimes affectionate relations, and (as I have said elsewhere) I cannot recall a single relationship of enmity with a fellow mathematician from before 1970.

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There was, however, a **subsequent** grievance against me on the part of the Congregation, the cause of a kind of collective "rancor", and in any case, a collective act of "reprisal", which, though it remained unspoken, was nonetheless

of "unfailing efficiency". I probed this "reprisals for dissent" aspect in the May 24 note, "The Gravedigger - or the whole Congregation" (n° 97). In this note, I left out a certain tone in these reprisals, with regard to myself and those who had the imprudence to claim to be me.

the tone of derision, which goes beyond the simple "end of refusal". And every time I felt this "puff", **it was a certain style that was the designated target**. To put it another way, it's the particularity that distinguishes this style from any other, its "yin" or "feminine" nature, that has been the providential circumstance, eagerly seized upon by the collective unconscious to wash away the affront of dissent, adding to the reprisal of **exclusion** the extra dimension of **derision** - derision that is supposed to designate, through a certain style, the irrefutable signs of **impotence**.

And now that the word "impotence" has finally named a certain unspoken fact, it becomes apparent how this same "providential circumstance", added to that of my "death", becomes the opportunity to

for my friend and ex-student and ex-heir Pierre Deligne, to make **this** role **reversal** tangible, credible and **raw**, this insane and seemingly hopeless desire of the man who feels like a "**dwarf**" in front of a "**giant**"! "Perched on the shoulders of a giant" (to use the very words that appear as the final word in his curriculum vitae<sup>245</sup> (\*)), from now on he will be the "giant" for all to see, and he will point out to the derision of the entire Congregation, like a "dwarf", a great braggart and a great vacuum-breaker, this giant of pure junk - but yes! - and yet he had been (and remains in spite of everything... ) "a perpetual and burning challenge for those who feel overwhelmed by the irremediable condition of a dwarf... .".

This spectacular reversal in the distribution of the roles "dwarf" and "giant", between himself and the Other (the One who is felt to be a **challenge**, and who must be supplanted at all costs) this reversal is also at the same time **the reversal in the roles "feminine" and "masculine"**. It is indeed as an incarnation (pletho- of the **feminine** (never clearly named and yet ardently repudiated), than that of the feminine (never clearly named and yet ardently repudiated), than that of the feminine (never clearly named and yet ardently repudiated).

who was (and remains despite everything. . . ) a giant, is designated to the □foule (and above all to the Prestidigitateur himself. . . ) p. 708

as a pitiful dwarf and an object of derision; and it's also as a heroic and exemplary embodiment of **virility** that the one who was a dwarf (and who, despite everything and deep down, "knows" that he is and remains one, by immutable condition. . . ) finds himself a giant with hands of steel, acclaimed by the same crowd that has come out to boo the Other.

This reversal, however symbolic it may be, is visibly out of all proportion to the "private" reversal operated by virtue of a tried-and-tested tactic (known as the "velvet paw") in the restricted and inconsequential circle of "between four eyes"; a gentle little merry-go-round where he feels he holds the strings that "make the Other walk" and turn... . The dwarf making the giant walk, all right, but still and irremediably a dwarf! Whereas the apotheosis of the dwarf who finds himself giant and even higher perched, and who designates to the derision of all the very one on whom he is perched - this apotheosis takes place in the middle of a public square, before a large and jubilant crowd, who have come to acclaim the Eulogy of a deceased and buried "dwarf", as the "highlight" of a superb and delectable Funeral Ceremony.

#### 18.2.11.4. (d) Disavowal (1) - or reminder

**Note** 152 (December 24) With yesterday's reflection, I feel I've just about finished "assembling" this first plan of the Burial picture, at least as well as I feel able to do so with the "pieces" of the puzzle I now have. It goes without saying that in this second part of my reflection on the Burial (the third part of Récoltes et Semailles), my aim has been, no longer to gather material facts (I have gathered enough of these in the "investigation" part, in Cortèges I to X), but to arrive at an understanding of the **inner workings** of the Burial, through the secret **motivations** (most often unconscious, no doubt) in each of the many protagonists<sup>246</sup> (\*). These motivations derive, first and foremost, from the nature of the interested party's relationship with my modest person (as the "deceased"); or, more precisely perhaps, with what I represent for him/her for one reason or another, linked or not to my departure from the mathematical scene and the circumstances surrounding it.

□The "foreground" consists, apart from myself, in the one of all who played at my funeral the role of the "priest in chasuble", or of the "Grand Officiant aux Obsèques". He is also, among those who were friends or students in the mathematical world before my departure, the one with whom I was most closely linked,

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<sup>245</sup>(\*) On this subject, see the last footnote in "The nerve within the nerve - or the dwarf and the giant", n° 148.

<sup>246</sup>(\*) (December 31) This "statement", taken literally and given the number of its "many protagonists" (and would there be

only ten!), would of course be entirely out of reach. Apart from my friend Pierre, the best I can do is to get a general idea, by identifying "motivations" and "intentions" as best I can in a "collective unconscious", which at best only approximates those of a particular "protagonist".

by mathematical affinities of exceptional strength; and the only one, too, who continued a personal relationship with me after I left, a relationship that continues to this day. For all these reasons, I have a wealth of "data" about him that is unmatched by anything else known to me among the participants in the funeral. Finally, of all the mathematicians I've known<sup>247</sup> (\*), he is undoubtedly also the one, by far, whose role in his life he assigned to me weighed the heaviest - much heavier, visibly, than that commonly assigned to his teacher, even in the practice of an art to which one would have devoted oneself body and soul (as I myself had devoted myself to it). I've come to realize this over the last ten years or so, and that the role he assigned me also spilled over into his mathematical passion (and into what ended up taking its place). This perception in me, which had remained diffuse for all those years, became considerably clearer and fleshed out in the course of my reflection on L'Enterrement, and right up to yesterday.

It seems to me that with yesterday's reflection, along with this first plane of the picture centered on the relationship between my friend Pierre and myself, the "third plane" has finally been put in place and assembled, consisting of "the whole Congregation", rushing in jubilant to participate in the Funeral and Burial with their eager acquiescence. As I wrote yesterday, what was still missing from the image that had emerged in the course of the reflection of the note (of May 24) "The Gravedigger - or the whole Congregation", was the nuance of **derision** put into the exclusion of the one treated as deceased and as a "stranger", an "outsider". The meaning of this derision, made clear as early as the note (November 10) "Les obsèques du yin (yang enterre yin (4))", was recalled and put into perspective yesterday: it's derision of what is felt (on an informal level) to be "feminine", and which is therefore the object of a "visceral" reaction of rejection, by equating (equally informal) the "feminine" with "impotence" - man alone, in his triumphant virility, being supposed to be the bearer of "power", of creative force. I've also emphasized the entirely refractory nature of the "feminine".

We can't deny common sense and reason to such visceral assimilations, the result of conditioning, when the ideas and images it arouses are felt with such force of conviction and evidence, that they're as their own justification.

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There is one aspect, however, which appeared in a sudden flash with the final word in the note "Les obsèques du yin", which has not yet been taken up. Here are the lines that end the reflection in that note:

"These are no longer the funerals of a person, nor those of a work, nor even those of an inadmissible dissidence, but the funerals of the 'mathematical feminine' - and even more profoundly, perhaps, in each of the many participants applauding the Funeral Eulogy, **the funerals of the disowned woman who lives within himself.**"

It even seems to me, now that I think about it, that this aspect was more or less overlooked in the case of my friend Pierre himself, about whom I have no shortage of first-hand facts! If this aspect was even remotely present, and perhaps felt by an attentive reader, it must have been between the lines rather, when attention was mostly absorbed by the various angles of the "reversal of yin and yang" aspect - (an aspect which, at first glance at least, seems specific to the person and particular role of my friend in the Burial). This omission reminds me that I'll have to talk again (in a few days 2) about my friend's last visit, from October 10 to 22 (reported in the note of October 21, with a promise to come back to it "in a few days". . . ). This will be the most propitious moment, it seems to me, to examine one last (?) angle of the "reversal" - with the reversal of the original yin-yang balance **in the very person of** my friend. This is yet another **burial** of certain original yin traits in him, under the iron rule of yang traits that appeared later and took possession of the place. I find myself here, in a new and more

<sup>247</sup>(\*) And even among all the people I've known, with only two exceptions.

deep, in the face of this startling realization that had already occurred to me more than once<sup>248</sup> (\*): that in believing he was burying the man who had been his master (and who still remained a friend), it was none other than **himself** that he was actually burying with his own hands!

So if I return once again to the "third plane" or "background plane", to this "Congregation" alias "mathematical community", the few lines quoted earlier would suggest that what I felt so strongly in the case of my friend Pierre, could well also be true for "each of the many participants applauding the Funeral Eulogy". It is this aspect, it seems to me, that I still need to examine somewhat, p. 711 before I felt fully satisfied and (provisionally?) considered the "background shot" (as well as the foreground) of the painting of my funeral to be complete.

(December 25) Yesterday, on the pretext that it was Christmas Eve, I treated myself to a real "high", staying on top of my notes until just after 3 a.m. (for once!). It's true that the whole day had been scattered with other tasks, and (having reread the previous day's notes) there were only a few hours of the night left, if I wanted to continue the same day. As is often the case, in the end I didn't even manage to tackle anything I had in mind when I sat down in front of the white paper! Instead, I took stock of where I was in the "picture" of the Burial, and highlighted an aspect, in both the "foreground" and the "background", that was still unclear: that of the "**burial of the disowned woman**" who lives in each of the participants in my funeral.

Clearly, in this quotation, the expression "burial" is used as an image to designate an act of **disavowal** and **repression** (or "repression", to use a received terminology). In order to disavow and repress something (in this case, something that "lives" within oneself), one must first make sure that this "something" is indeed present, "alive" (even if miserably). We're talking here about the "woman" in every being, whether male or female, i.e. the "side" of the person that is made up of traits, qualities, impulses or forces of a "feminine" or "yin" nature. This simple, essential fact - that in every being, woman or man, lives **both** "the woman" **and** "the man" - is an extraordinary fact that is still generally ignored today. I myself only learned it eight years ago, when I was in my forty-seventh year<sup>249</sup> (\*).

Of course, "psychoanalysts" have "known" about it and talked about it for a long time now. There are certainly plenty of books about it, and everyone has heard a little about it, just as I had. In fact, "everyone" is quite willing to admit that there must be some truth to it, as long as it's people with recognized expertise who say so, and there are books written about it, and everything. Yet to have heard of it and be "all ready to admit... ", and even having read a book or even p. 712 ten on this subject, or even (I'd venture to say) to have written one, or even several, does not in itself imply that you "know" the thing; at least, not in a stronger and, above all, less useless sense, than that of a simple memorization of ready-made formulas, like "Freud (or Jung, or Lao-tzu. . . ) said that. . . ". Such formulas constitute a certain cultural baggage, a kind of calling card for a "cultured" person, "in the know" about this or that, or even sometimes (with diplomas to match) an expert in this or that, and as such they can even be admitted to have a certain "usefulness"; what's certain is that everyone is very attached to it, to the baggage they've accumulated left and right, at school and in books, in "interesting conversations" etc., and which they carry around with them, and which they carry with them through thick and thin, like a bulky, flashy trophy, for the rest of their lives. When I irreverently implied earlier that this precious baggage was "useless", I meant: useless for something that, in any case, nobody

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<sup>248</sup>(\*) This "observation" appears for the first time in the reflection in the note "L'Enterrement" (n° 61).

<sup>249</sup>(\*) On this subject, see the note "Acceptance (the awakening of yin (2))", n° 110.

and is even shunned like the plague by everyone, namely, learning about oneself. Or to put it another way: that this baggage is useless if you want to **take charge of your life**, that is, if you want to digest and assimilate the substance of your own experience, and thereby mature and renew yourself... .

If I were to sum up in a few words the essential content of my long reflection on yin and yang, it would be by "recalling" this "simple and essential fact", which I have just recalled. If there's a reader who's followed me this far, and if he hasn't yet sensed, in terms of his own experience, this fact : that there is "woman" in him, even though he is a man, and that there is "man" in him, even though he is a woman - it's because, in making this vain effort to "follow" me, he would have wasted his time overloading a baggage, no doubt already heavy, with yet another weight, labelled "Harvest and Sowing". And if he were a man, and even if he weren't one of the participants in this funeral, of which he would have had no knowledge or suspicion before reading me, it's a safe bet that he too, day after day and without his own knowledge, "buries a disowned woman who lives inside himself" (just as I myself had done in the past and for most of my life).

p. 713 There are a thousand and one ways for a man to "bury" the woman who lives inside him, as also for a woman to "bury" the man who lives inside her<sup>250</sup> (\*), that is-□à-directly : to disown and repress him. One of the most

The most common way of "burying" something that is alive in oneself is through attitudes or acts of rejection of that same thing, when it is apparent in others. This rejection is none other than the "visceral reaction" I mentioned yesterday in a case in point. What gives the reaction of rejection its strength ("visceral") is **not** really (as I seemed to imply yesterday) because the thing rejected in another person simply goes against a set of "values" that would have our full and undivided support. Those who know themselves to be "strong" are not offended by the sight of "weakness". On the contrary, the strength of the reaction comes from the fact that this thing, observed in others and "out of place", **calls us into question ourselves**. It's like an insidious **reminder**, immediately rejected, of something concerning us, which deep down **we know**, even though we'd like to hide it from ourselves and from others; a reminder which then takes on the tones of a silent and fearsome challenge. In such a context, a benevolent attitude of tolerance towards the apparent "flaw" in others would appear to us as a perilous admission of connivance, to be avoided at all costs. On the other hand, by rejecting them, we unequivocally disassociate ourselves from the other person, in short, we give convincing proof (first and foremost, to the inner Censor within ourselves) that we ourselves are free of reproach, that we are and remain conformist and "good-natured". At the same time as **an act of unconditional obedience** to certain value **norms**, distinguishing what is honorable from what is inadmissible, the reaction of rejection is at the same time a **symbolic act of burial**, whereby the thing in ourselves "that doesn't belong" is eagerly "classified" as something that **"isn't"**. **Not in us, anyway!**

In this picture, the infinitely variable form that rejection takes seems to me to be of no consequence. It can be outraged rejection, with all the signs of indignation or disgust, or it can be rejection through irony or "delicately dosed" disdain. It can be expressed in clear, unequivocal words, or it can be merely suggested, with allusive or double-entendre words, or even without words, with the right smile (or lack of smile. . . ), placed where it's most appropriate. Rejection can be fully conscious, or it can be confined to the penumbra of what is barely visible to the eye, or take refuge in the complete shadow where the eye never penetrates.

p. 714 The intensity of the rejection reaction, too, is infinitely variable'□ depending on whether the "questioning" in question

<sup>250</sup>(\*) The same goes for a man who "buries the man living inside him", or for a woman who "buries the woman living inside her", attitudes that are far from as rare as one might think.



is felt to be relatively harmless, or indeed frightening. The ones that provoke perhaps the strongest reactions are those that directly concern **sex**. This extreme susceptibility has diminished somewhat over the last few generations. I have noticed, however, that things as universal in nature as the so-called "homosexual" and "onanistic" (or, to put it more kindly, "narcissistic") aspects of the amorous impulse are as strongly rejected today as they were in the past. This is the case, at least, if one is confronted with it, not in an "interesting conversation" about Roman mores or depth psychology, but in everyday life. Even between the eyes, it's rare for us to talk about the manifestations, in our own person, of these aspects of the sex drive (generally experienced as rather embarrassing "burrs", to say the least).

In the case in point, the rejection I experienced before I left the mathematical scene was certainly not as strong as the one I've just described. It's true that the object of this rejection, namely, "feminine" ways of being and doing things when we're supposed to be "among men", does have a "sexual" connotation, in a broader sense of the term than that linked to the mere evocation of actions and gestures revolving around "the buttocks" and the rest. I have no doubt that this connotation was generally felt, at an unconscious level<sup>251</sup> (\*). It was, however, discreet and indirect enough to exclude any brutal reactions, going beyond a simple "reserve" with regard to my "seriousness", my "solidity" as a mathematician. What's more, the fact that my "cross" was a purely intellectual activity made it seem relatively harmless, far removed (what would you expect? . . .) from any disturbing, scabrous association of a man-woman belly-dancing up her skirt! Nevertheless, after my first contacts with the mathematical world (in 1948), it took almost another ten years for the reservations my style aroused, even within a benevolent microcosm, to finally disappear - from my sight, at least. The situation changed again after my departure, however, as an atmosphere of benevolence, friendship and respect for me was suddenly altered (without my realizing it for the next six years) by what was felt by this same microcosm to be "dissidence", and disavowal.

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□ I'm not sure, to tell the truth, if this change of mood was really as "sudden" as I've just to say. Or to put it another way, I don't have enough facts to give me any idea of **how the** change I was suddenly confronted with after I left in 1970, in 1976<sup>252</sup> (\*). It's true that all that time I'd had little contact with the world I'd left, which might have given me a sense of its "temperature" and evolution. What is clear to me is that in this evolution, the attitude of the group of all those who had been my students, and of their uncontested leader Pierre Deligne, played a decisive role. The Burial could only have taken place, and the atmosphere that gave rise to it, thanks to a "unanimous agreement"<sup>253</sup> (\*\*) and without fail, encompassing the "three planes" of this Burial: "L'héritier" (aka Grand Officiant aux

p. 715

<sup>251</sup>(\*) On this subject, see the note "Les obsèques du yin (yang enterre yin (4))", n° 124.

<sup>252</sup>(\*) It was, I recall, during my unsuccessful efforts to get Yves Ladegaillerie's thesis published. This episode is mentioned in the two notes "On n'arrête pas le Progrès" and "Cercueil 2 - ou les découpes tronçonnées", n°s 50, 94.

<sup>253</sup>(\*\*) For the first appearance in our reflections of this observation of "unanimous agreement", see the note of the same name (capitalized!), n° 74.

Obsèques), the group of "cohéritiers" or "proches", formed by the eleven other "élèves d'avant", and finally "la Congrégation" (perhaps not "toute entière" - we'll have to come back to that. . . ). How this perfect harmony came about remains unknown to me, and perhaps will remain so. At present, I don't feel prompted to probe it, and I doubt that anyone else will do it for me (quite the contrary!).

This reminds me that, when writing the previous note "The providential circumstance - or the apotheosis", the question had occurred to me as to **which** of the two, "The Congregation" or "the priest in the chasuble", ultimately represented **the** master force at work in the Burial, of which the other would have been the "instrument", as it were<sup>254</sup> (\*\*\*)). I didn't dwell on it then, not being sure even if the question made sense - it had

p. 716 It's a bit like the famous chicken-and-egg question! What is certain is that neither of them (the "priest", nor the "Congregation") could do without the other's help in implementing the Funeral.

Another question, however, which seems to me to have a clearer meaning, is which of the two was more heavily involved in this work. It's true that "the Congregation" is not a person, and it's improper to speak of "his" investment in a task. But it's also true that for me, this personified entity takes on a concrete form, through ten or twenty **people** I've known well, with each of whom, for a decade or two or more, I've been in close, friendly relations. So when I speak of the "investment" of the Congregation, I'm thinking in concrete terms of the "sum" of the investments of all those former friends who were involved in my funeral. Thus clarified, it seems to me that the question is no longer rhetorical.

The answer that comes to me, without a hint of hesitation or doubt, is that there is **no common ground** between the investment of the "heir" and that of the Congregation - any more, in fact, than there is in an ordinary funeral. This is all the more true when the inheritance is important to the heir (while no-one in the Congregation has anything to gain for himself), and when the ties (of attraction or conflict) that bind him to the deceased are strong and play a vital role in his life. If there is any doubt in such a situation, it can only stem from the presence of "co-heirs" among those close to the deceased. (We're talking here about the "second plan", rather than the "background" formed by the bulk of the Congregation). In my case, the only one of these "close relations" and co-heirs whose part in my funeral could be of comparable weight to that taken by the principal heir Pierre Deligne, seems to me to be Jean-Louis Verdier, playing the role of Second Officiant aux Obsèques. This appellation is not gratuitous, as on more than one occasion during the funeral, I saw both of them officiate in perfect harmony! But as I've already written elsewhere, apart from some of J.L. Verdier's public acts, I know very little about him since we lost touch; too little, no doubt, to be able to form even the slightest idea of the ins and outs of his relationship with me, or with his prestigious "protector" and friend.

p. 717 **Note 153** (December 26) In yesterday's reflection, I tried to clarify this intuition, which appeared "in flash" on November 10, that in "each of the many <sup>participants</sup>" at my funeral, it represented the burial-the "disowned woman who lives within himself". When I have spoken and spoken again here of "each" of the participants, it's a rather exaggerated expression, which it's perhaps best not to take entirely literally. At the very least, I'm convinced that this intuition is right for everyone.

<sup>254</sup>(\*\*\*) I recall that in the May reflection, in the note "Le Fossoyeur - ou la Congrégation toute entière", I had realized that my friend had been an **"instrument"** of a **collective will** of flawless coherence". The lines that follow do not really contradict this intuition, but rather complement it, leaving open the possibility of a certain symmetry in the relationship between the "Congregation" and "the priest in the chasuble".

of those (and I'm sure there are many) in whom this "visceral reaction of rejection" to my particular style of mathematics has taken place, a reaction that has been the focus of my attention over the past three days.

On the other hand, it's clear that such a reaction is **not** present in my friend Pierre, or at least, that there was no trace of it, quite the contrary, in the five years preceding my departure. It was the deep **kinship** between my style of approach to mathematics and his own that gave rise to such perfect communication during those years, and was also the cause of that uncommon affinity between us mathematically, an affinity that he and many others must have felt, as I myself did. It was this kinship, too, that was surely the cause of the **fascination** that my mathematician persona and my work exerted on him, not only in those years (when it was expressed "positively"), but also in the years that followed and right up to the present day (when it has been expressed mostly "negatively", but just as eloquently<sup>255</sup> (\*)). I have no doubt that if there had been in him the slightest reservation, the slightest unease about my style of working and approach to mathematical things, in those early years, I wouldn't have failed to sense it.

It's true that from those years onwards, my friend did his utmost to erase from the outside world the role I had played for him, if only as the person who had taught and passed on something important to him, and from whom he had drawn important ideas for his work - and a fortiori, to erase this relationship of affinity, even fascination. After my departure, there was a gradual escalation in the disavowal of my person, not only through silence, but also through the affectation of disdain towards my style of working, and vis-à-vis many of the ideas and notions I had introduced. The first trace of such an affection that is  $\square$  connue to me was in 1977, on the occasion of "operation SGA 4"<sup>1256</sup> (\*). p. 718 I haven't tried to follow the progression of this climb step by step, and I don't feel inspired to do so (as I said yesterday, on a related matter).

This disavowal of a style of approach closely related to his own, and of a body of work from which his own emerged, is akin to a **disavowal of himself**. When I first thought about this disavowal of my style and my work (while I'm still mostly under the impression of the five years of close mathematical contact before I left in 1970), I was inclined to play it down, to give it only a kind of **tactical** significance, as a particularly tempting **means** of supplanting and satisfying antagonistic impulses, by seizing the windfall of a certain "providential circumstance". This is indeed the tone of the note from il three days ago, "La circonstance providentielle - ou l'apothéose" (N° 151). And what I just remembered, Knowing that in the years before my departure there was no trace of rejection of her own style or of mine, also points in this direction, and not in the direction of the situation examined yesterday: that of a disavowal of "the woman who lives within oneself" (if only, among other things, through a certain approach to mathematics), a disavowal that would have **pre-existed the implementation of L'Enterrement**.

This does not prevent the person who chooses such means from **paying for them**, whether he likes it or not. To be operational, this "affectation of disdain" for a certain style had to be played out, not only in relation to others, but also and above all, **in relation to oneself**. But one cannot disavow, before others and oneself, a "style" that is also profoundly one's own, **while practicing it** as if nothing had happened. This "tactical disavowal" of others, by the logic of things, involves a disavowal, a **repression of** a part of oneself - in this case, by the repression of the style of approach to mathematics that is one's own, by virtue of one's "own" culture.

<sup>255</sup>(\*) Or at least, this fascination must have been, at the outset, the force in the "positive sense" (that **of identification** with the one who is felt to be similar) among the two forces at play in the establishment of this ambiguous, conflicting relationship of identification with my person.

<sup>256</sup>(\*) On this subject, see "Two turning points" and "The clean slate", n°s 66, 67.

nature of the creative force within him.

p. 719 This observation is not the result of a direct perception of a fact. It is the result of a short reflection, making use of known facts and drawing common-sense "conclusions" from them. I've learned to be careful with such conclusions (and especially, outside mathematics!), and not to rely on them. only if they are confirmed after the fact by other facts. But I remember here, very opportunely, that I had been led, in terms of what is known to me of Deligne's work, to note that there is no trace in this work of certain inclinations (of a "yin" nature) in my friend, which were nonetheless quite apparent in the years before my departure, and which I also recognized in myself. I wrote about this in some detail in my notes of a month ago (November 26 and 28) "Yin the Servant and the new masters", and "Yin the Servant (2) - or generosity"<sup>257</sup> (\*). Perhaps the most important of these things is a certain humility, which allows us to see (and describe, without fear of looking foolish) simple, silly things to which no one has ever deigned to pay attention. The best things I've brought to mathematics myself<sup>258</sup> (\*\*) are just such things. Most of my work, and that of my most brilliant pupil, would never have been written if I had disavowed this inclination of my nature, which was not to everyone's liking. ... This propensity (or "inclination") is intimately linked to another, without which its effect would remain extremely limited. It's an attitude of humility again, and of "service": when it's a question of getting to know and describing with delicacy and under all its facets this new thing scorned by all, of not finding one's time too precious to devote ten pages to it if necessary (instead of being content with two lines: here's the thing - you can do what you like with it!), or even ten thousand; to spend a whole day on it (for a man who has plenty of other things to worry about. . . ), or a whole life, if need be.

When I spoke of "new worlds" to be discovered, in a somewhat haughty tone perhaps, I was talking about nothing other than **this**: seeing and receiving what seems infinitesimal, and carrying and nurturing it for nine months or nine years, the time it takes, in solitude if need be, to see a vigorous, living thing develop and blossom, made itself to engender and conceive.

If this propensity, which could be called "maternal", is today the object of derision, it is to the "benefit" of attitudes felt to be "virile", which tolerate only one possible type of approach to mathematics: that of "muscle", to the exclusion of "guts". Real math", also known as "hard math".

p. 720 (or "**hard** maths"), as opposed to the (unappetizing) "**soft** maths", □c'est les démonstrations en dix ou cinquante pages serrées, de théorèmes-au-concours (of proverbial difficulty, or it's no game at all!), making use of all the "well-known" theories, notions and facts available on the left and right. As for the "wood", it's just there, that's what it's there for! And as for those who have patiently cleared the land\*, who have sown, planted, smoked and pruned throughout the seasons and over the years, to make these spacious, slender-trunked forests grow and spread out, so much in their place (where the bush was thick and impenetrable) that you'd think they'd been there since the creation of the world (as a backdrop, no doubt, and as a reserve of "all wood" . ...) - these people, who are only good for producing fluffy articles (or even fluffy books or series of fluffy books, if they find publishers foolish enough to print them), and unreadable to boot, are "soft math" retards, not to say "flabby" - but no matter how virile we may be, we're no less polite...

With this beautiful flight of fancy, I suddenly feel like I'm back at the starting point of this long meditation on the

<sup>257</sup>(\*) These are notes n° s 135, 136. The sub-note to the second cited note (n° 136 ) should also be added.

<sup>258</sup>(\*\*) See sub-note no. ° 136 cited in the previous footnote.

yin and yang - to the very first note of early October, "Le muscle et la tripe (yang enterre yin (1))" (n° 106). It's the same burial again, at parade pace and to the sound of a bugle, of what is "feminine", buried by the male disdain of Bras-de-Fer aka Cerveau d' Acier aka Superman. This burial doesn't just take place in the small mathematical microcosm, that's for sure, and its scope goes beyond any specific case, which can however be used to smell it a little closer. And that smell is one of the main lessons I've learned from Burial, in which I appear to be dead before my time.

When I narrow the focus of my attention even further, to focus on the particular role played by my friend Pierre, I see in Burial yet another meaning. Once again, I see a **reversal**. As I said yesterday, without thinking I'd come back to it so soon, it's no longer a reversal in a **relationship** (real or fictitious) that links him to another, but a reversal that takes place **in his very person**. He is not sought after for his own merits (as the object, perhaps, of a "foolish desire". . . ), and he is no longer limited to being purely symbolic (while, at the end of a magnificent sleight of hand, the one who felt "dwarfed" does not cease to feel just as dwarfed, as if he hadn't just been "dwarfed").

convinced himself that he had become a "giant". . . ). It's a reversal, I wouldn't say irreversible, but at least perfectly **real**. It starts from a state of harmonious balance of creative□ "feminine" and "masculine" impulses, p . 721 with a predominantly feminine note. The result is a state of war and repression, where **attitudes** and **poses** (egotistical, like all attitudes and poses), flying the "virile" flag, obstinately repress the **creative force**, derided and symbolically "buried", in the form of a grotesque, flabby effigy, with the features of the "Superfemale".

In less nuanced terms, but more vivid and striking perhaps: a "**feminine**" being, slender and vigorous, supple and **alive**, has been metamorphosed, by a permanent trick of prestidigitation, into a "**virile**" being, indomitable- sand, stiff and **dead**.

#### 18.2.11.5. (f) Staging - or "the second nature

**Note** 154 (January 1, 1985) Five days have passed, taken up by various occupations. The end of the year was the perfect opportunity to write letters that had been outstanding for weeks or months, not to mention a few cards of good wishes, in response to those received around Christmas. We also had to build compost heaps with manure that had already been brought in two or three months ago, and plant waste from the garden and the landfill, or brought in from the municipal dump, to have good compost ready for the garden in early spring. As the site is on a slope, an additional terrace had to be built next to the one already provided for the "day-to-day" composting of household waste.

With all this going on, I've hardly found time to work on my notes, except for housekeeping work. I have reread with great care, still making a few alterations here and there, the entire reflection since the "Masters and Servants" section (i.e. since the November 24 note "The reversal (3) - or yin buries yang").

(n° 133)), adding the footnotes already provided for the last fortnight's notes. He The main aim was to have a manuscript ready for typing, but quite apart from any practical issues, this re-reading was useful in regaining an overview of the thinking that had taken place over the past four or five weeks. As is also the case in long-term mathematical reflection, while the particular "moment" of reflection in which I find myself on a day-to-day basis is placed under the strongly focused beam of intense attention, the "thread" of reflection and the sinuous line it has followed in the weeks, or even in the years, that have passed since I first wrote this book, is still visible.

the months gone by, tends to get lost along the way, to drown and dissolve in the vagueness of a penumbra. I cannot say whether this is a general fact in any long □term research work, or whether it is related p . 722 to this systematic mechanism of "burying the past" in my life, to which I've already had occasion to refer.

allusion<sup>259</sup> (\*). In any case, as the days and weeks, even months, of long reflection go by, I lose touch with the earlier stages of my work, resulting in a growing sense of unease. This discomfort is eventually resolved by a more or less thorough retrospection of the work that has just been done, which re-establishes the contact that had gradually been loosened. I've found that these retrospective "halts" play an important role in my work. Each time, I leave with a fresh wind in my sails, relieved of the "malaise" that had signalled a gradual loss of an overall perception of **continuity in the time of** the work I'm pursuing. In my mathematical work, it's not uncommon, not to say the rule, for such a step backwards to lead me to rethink the work already done from top to bottom, and to see in a new perspective both the work done and the work still to be done<sup>260</sup> (\*\*). But whether it's a mathematical task or a meditation on my life, the "malaise" I'm talking about is always the sign of a still imperfect understanding, not only (and for good reason) of the work still to be done, but also of what has been done in the course of the work that has already been done. This imperfection is by no means reduced to a faulty memorization of each of the various stages of reflection, and of their chronological order (aspects that are relatively incidental in the case of mathematical reflection, where the object of attention is a mathematical situation, itself foreign to the psychic particularities of the person examining it, and to the events of that examination). It seems to me to be more a sign of a lack of **unity**, of insufficient **integration** of all the partial understandings that have emerged as a result of the mathematical process.

p. 723 successive stages of reflection. These partial understandings also remain imperfect, even hypothetical, as long as they are not integrated into an overall vision□, where they become clearer. mutually. To use the image of a **jigsaw puzzle** again, investigating an unknown substance is akin to putting together a jigsaw puzzle whose pieces are not given in advance, but have to be discovered in the course of the work. What's more, each piece uncovered appears at first only in a vague and approximate form, even grossly distorted in relation to the "correct", as yet unknown, form. The "local" work of reflection consists in identifying the pieces one by one, and trying as best we can to guess the contours of each one, guided above all by assumptions of internal coherence within the piece examined, or between it and other, presumed neighbouring pieces. But each of these pieces only reveals its true nature and its precise, final form, once they are assembled in the as yet unknown overall picture from which they originate. The "uneasiness" I was talking about is that which, in the presence of a multiplicity of perfectly well-spotted pieces, presented in a more or less shapeless heap, signals to me that it's time to finally assemble them - or also, if (more or less partial) assembly has already taken place, that it's still too fragmentary, or that it's out of kilter and needs to be completely reworked. To find **the** right assembly, the chronological order in which I came across the pieces of the puzzle is no doubt often incidental. But taking the pieces in hand one by one (and in that order, while we're at it), in the attitude of someone who knows they have to fit together and who is waiting for each one to be placed in its proper place, is undoubtedly an essential step in the work, to finally see them fit together. The "final word" in the previous note (from six days ago) tried to capture in words a certain strong impression in me - that of a **metamorphosis** that had taken place in my friend Pierre over the years, in the fifteen years since I left the mathematical scene. I'd seen signs of it here and there over the years, which sometimes left me flabbergasted, but not at any point.

<sup>259</sup>(\*) This mechanism was set in motion at the moment of the "changeover" that took place in my childhood, which I place in the summer of 1936 (when I was in my ninth year). This crucial episode in the structuring of the ego is alluded to in the note "Le Suprême (yang enterre-yin (2))" (n° 108), and in the sub-note n° 108<sub>1</sub>.

<sup>260</sup>(\*\*) For similar thoughts on the role of occasional "retrospectives" in long-term work, see also the second part of the note "Rétrospective (1) - or the three sides of a picture" (n° 127), and in particular the footnote referring to it.

At the time (as far as I can remember), I stopped by to get a **general** idea of what was going on. It has to be said that, while I sensed a certain "wind", and a particular role my friend was playing in it (with the burial of motifs in particular, which I was dimly aware of(\*)), I was very far from suspect the large-scale burial of myself and my entire body of work that my

<sup>261</sup> □ami was deftly orchestrating. It was the gradual discovery of this burial during of the past year, which was finally enough of a **shock** to shake an inertia in me, and to motivate me to finally "put down" on a situation that had seemed drowned in the mists of a distant past. So it was also in a very different frame of mind from the somewhat "routine" frame of mind that had been mine during our past encounters, in a frame of bemused attention, that I received my friend during his recent visit in October. It was during this visit that this impression appeared, or rather, this sudden perception of something that had surely been present for a long time, and which I had until then been happy to ignore: the perception of this "metamorphosis" - the very one I came back to by a different route in the reflection of the previous note. If I have rediscovered this impression, this time through what I know of my friend's mathematical work, it is surely not by the greatest of coincidences, but guided by what direct contact with him has taught me over the last two months. The force of evidence of this impression of a metamorphosis, culminating in a "virile, indemoluble, stiff and **dead** being", could certainly not come as the result of a reflection comparing and assembling facts (or partial impressions of another nature), but only through immediate experience, which remained unspoken. And this experience remains unspoken to this very moment<sup>262</sup> (\*).

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In the previous note, I wrote that this "reversal" (in the very person of my friend), or this "me- tamorphosis" (to use the expression that appeared in □le "mot de la fin" ), was not "sought for its own merits", adding in parentheses: "as the object, perhaps, of a 'foolish desire'. . . " (of this desire for reversal, therefore, mentioned in the note "The nerve within the nerve - or the dwarf and the giant"). Yet, on re-reading the notes the next day, I wasn't so sure, or whether my deliberate pro- posal of **opposing** these two "reversals" I'd discerned in Burial was really well-founded. After all, in this image of the dwarf and the giant, the "giant" embodies (as I've pointed out more than once) "virile" values, and the "dwarf" is overwhelmed by "female" de-values. And even though this image **is located** outside my friend's person, plastered as it is on his relationship with another person (me in this case), this doesn't prevent it from having no "objective" existence outside his person, but rather from being the **projection** onto the outside (onto his relationship with so-and-so) of a conflicting reality played out in **none other than himself**. To put it another way, this image of the dwarf and the giant appears as a symbolic staging of the **real conflict** at play in deeper layers than those in which the image lives, which is none other than the never-ending **conflict between the yin and yang "sides" of his person**.

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<sup>261</sup>(\*) (February 30) For echoes of this feeling, which remained in an informal and diffuse state (until the discovery of "l'enterrement dans toute sa splendeur" from April 19 last year), I point in particular to the occasional allusions, in the first part of Récoltes et Semailles (written in February and March last year), to the fate of the notion of **motif**, notably in Introduction, 4 ("A journey in pursuit of obvious things") and in the section "Le Rêveur" (n° 6). The formulation of this sentiment becomes considerably clearer in the final pages of the final section of this first part, "The Weight of a Past" (n° 50), starting with the passage "I might consider the 'Letter to . . . ' " (read: Daniel Quillen), which represents a sudden turning point in our thinking. The first "notes" to emerge from this final stage of today's reflection, and above all the double note "My orphans" and "Refusal of an inheritance - or the price of a contradiction" (n° s 50,51), written at the end of March, take stock of what was previously felt to be a diffuse state, concerning the fate of my mathematical work and a certain "wind" of fashion towards it and myself.

For a description of a particular form this "diffuse feeling" had taken in relation to the motifs, see the note "Le tombeau" (n° 71) and the following one, "Un pied dans le manège" (n° 72).

<sup>262</sup>(\*) (February 30, 1985) It's still unspoken at this very moment, even though I've just given an account of my friend's visit, in the note "Le devoir accompli - ou l'instant de vérité", n° 163.

Such an **externalization** of an inner conflict, which must remain rigorously concealed, is one of the few all-purpose procedures used by the unconscious to "evacuate" the original real conflict as far as possible, substituting another that seems more "acceptable", or at least less disturbing. In this case, the chosen image-paratone remains itself unconscious (I presume so, at least); and even, I'd tend to believe, it remains confined to relatively deep layers of the unconscious, yet closer to the surface than the knowledge of the real conflict. (The latter, moreover, is none other than the "place" of this "double-sided knowledge" referred to in the note "The two kinds of knowledge".

- or the fear of knowing", n° 144.)

This suggests that the "insane desire" recalled in parenthesis in the previous note, that "**to be that giant himself**, or at least to **pass for him**", - that this desire is merely the "externalized" **transposition**, in terms of the lightning rod-image of the dwarf and the giant, of the desire for a "metamorphosis" in himself; a metamorphosis if not real, at least apparent - that or a predominance in his being felt as unacceptable, the predominance of "yin" tones (felt to be "soft" and contemptible), would find itself

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□ "reversed", metamorphosed into a predominance of "yang" or "virile" tones (felt as "heroic", and as the only ones worthy of envy). Far from being in any way opposed in their intimate nature, these two desires now appear to me as inseparable, one being like the shadow, the **symbolic** and tangible **expression of** the other. As for the "metamorphosis" I came to perceive during my friend's visit (better late than never!), it now appears as the symbolic and tangible expression of the other.), it now appears as the **realization** or fulfillment of this "insane" and imperious desire; fulfillment, not through the intervention of a providential grace, but as the long-term effect of the "boss's" stubborn determination to "rectify the situation", to **remodel** himself according to borrowed traits, and to impose these same traits on the worker-child (who, as you can imagine, is never consulted for this kind of typically "boss" operation).

In the previous note, I emphasized the **reality** of this "reversal" (or "metamorphosis"). I can now see more clearly the nature and limits of this "reality". It's the reality of a **pose**, striving to mold itself according to a model, felt as the ideal to be attained. The choice of model, i.e. the type of pose adopted, undoubtedly predates our meeting. But it seems to me that the energy invested and dispersed in this pose was minimal at the time of our meeting, and in the years that followed. There was, I believe, a sudden and drastic change in the dimensions taken by this investment, by the extraordinary "occasion" created by my departure; the departure first, from my institution (where overnight my friend had to appear to himself as having surreptitiously **substituted himself for his "rival"**), and shortly afterwards, my departure from the mathematical scene. A second, even more important aspect of reality is that, by virtue of an inordinate investment, this pose ended up becoming "**second nature**". And that's exactly what this "second nature" is, as I perceived it during our recent meeting. It's burdened with an immense inertia - just as it had been for myself. In my case, this hasn't prevented a renewal from taking place, and the fact that it has taken place in me doesn't detract from the inertia in my friend, which stands in the way of a renewal in himself.

This "new" reality that has gradually taken root in him has not "resolved" the conflict within him, any more than the occupation of a country by a neighboring country "resolves" a conflict. Rather, the conflict within my friend is "frozen".

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in a certain "balance of power", and chances are it will remain so until □ the end of its days. You could say that the structure of the ego, i.e. the mechanisms of behavior, have indeed changed, sometimes dramatically. Such changes, however, imposed by the will of the "boss", change nothing of the original nature of the worker-child's creative forces. They're simply like shackles imposed on the worker, who has to manage as best he can to work anyway, under the watchful eye of the "boss".



suspicious of the "boss", when the latter doesn't take the tools out of his hands, to show the worker what he has to do!

But that doesn't stop the business from running and making money, and the boss, by and large, is happy. There's a nasty atmosphere, sure, but like most bosses, he's thick-skinned and doesn't let it get to him, as long as the returns stay good.

#### 18.2.11.6. (g)Another self - or identification and conflict

**Note** 155 ( January 2) It's been over a week, since the December 24 note "Le désaveu (1) - ou le rappel" (n° 152), that I've had the impression of being just about finished with the foreground of the Burial painting. And then no - three times in a row already, I've had to come back to one point or another that doesn't seem to be right.

just three words to add, no doubt, to put a final point on a final point.

i. And each time, this "last point" kept me busy for an entire evening, when it turned out that what had seemed "not quite clear" had even remained rather obscure, and that it was by no means a luxury to return to it and find its own light. I suspect it will be no different again today, as I propose to return to a (final?) point, touched on in passing in the note "Disavowal (2)".

- or metamorphosis" (n° 153). This is one of the aspects of a relationship in which I play the role of "father". adopted" the aspect of my friend's ("ambiguous") **identification** with me. This aspect is mentioned in three or four lines, in a footnote to the note quoted. There was no further mention of it that evening, but the very next day, rereading the previous day's notes, I felt I had to come back to it. When I started thinking about it again last night, I thought I'd follow up on it, but in the end it was another of the "last points" left open since the previous reflection, which kept me busy late into the night.

On the many occasions in the course of Harvesting and Sowing when I've been led to note, in relation to such and such a friend or pupil, an aspect of adoptive or ☐adopted father, this was each time on the occasion of the appearance of

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conflictual features of this relationship. So, without any deliberate intention, it was the **conflictual** aspects of such a relationship with a "paternal" connotation that were at the center of my attention and were highlighted. I was well aware that in such a relationship, there is always a more or less strong component of **identification with the father**, with the only reservation that this identification can sometimes take a "negative" form, through identification with the "negative" (or opposite) image of a repudiated father<sup>263</sup> (\*). This knowledge remained in the background, without intervening in a visible way in the reflection, while nevertheless contributing its share to a diffuse apprehension and to the formation of a still blurred, unformed image of this or that relationship. I'm only going to say this once,

I believe, and in general terms, in the sense of identification, at the end of the section "The enemy father (1)" (n° 29):

" . . . it was the reproduction of the same archetypal conflict with the father: the Father both admired and feared, loved and hated - the Man to be confronted, defeated, supplanted, perhaps humiliated . . . but also the One we secretly wish we were, stripping him of a strength to make it our own - another Self, feared, hated and shunned. . . "

It hardly needs saying that in these lines, written on the occasion of a "retrospective on my past as a mathematician", if there was a precise case in point that guided my pen as I wrote, it was that of the relationship with my occult "heir" and ex-student-who-doesn't-say-his-name, Pierre Deligne - at a time, however, when I had no suspicion, on a conscious level at least, of the Big Show Burial orchestrated

## 2. A walk through a work or the Child and Mother

<sup>263</sup>(\*) This was notably the case in the relationship to me of three of my sons, who were by no means "adopted", let alone "adopters". . . .

by him! In reproducing these lines, written over nine months ago, I was struck by the extent to which they seem to prefigure and "call forth" (as it were) the image of the dwarf and the giant, which seems to have been formed and materialized for the sole purpose of giving tangible form to the intuition that has just been expressed. However, I have no doubt that it is not in me, the chronicler-researcher, that the image has been formed, but in my friend himself.

here it is<sup>264</sup> (\*\*)!

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□ Conflicting identification is clearly seen in the words "The one you secretly want too being" and, even more strongly and unequivocally: "another Self". In the image of the dwarf and the giant, as it came to my attention on December 18 (in the note "Le nerf dans le nerf - ou le nain et le géant", n° 148), it's a question of the "insane desire **to be that giant himself**, or at least, to **pass for him**", lines that seem to come in response to the "Celui qui secrètement on voudrait être" quoted just now. But this time I'll stop here (every day has its time!), one step short of the "another Self" that came nine months earlier as a matter of course! It's true that this time, while it's a question of "working on parts", in a very specific case, it's a question of being far more careful and circumspect than in a context where we're pretending (as if nothing had happened!) to make an assertion of a general nature, which wouldn't concern anyone in particular... .

But when you think about it, it's true that it's a very small step indeed, for the unconscious hungry for **symbolic** satisfaction, which it can buy with mental images of its own making, between the "insane desire" (and obviously of considerable strength) to be this or that, and **the act of identification** with the very thing one wants to be. For identification, however unconscious, to be even remotely credible, and for the satisfactions it brings to be savoured with a minimum sense of security, it must undoubtedly be backed by certain "objective" characteristics of resemblance to the person (in this case) with whom one identifies. I presume that, in the case of my friend's relationship with me, the first "objective characteristic" likely to foster a feeling of resemblance, and an act of identification, was the strong affinity between his approach and mine to our common subject, mathematics. This would be the force "in the positive sense", "that of identification with the one who is felt to be **similar**", mentioned in passing in the footnote quoted at the start of today's reflection.

However, as I've already pointed out several times in the course of reflecting on the relationship between my friend and me, from the very first years of this relationship, he didn't fail to perceive aspects of "superyang" imbalance in the character I'd been playing since childhood, which had long since become my "second nature". I can't say whether, at the level of conscious perception, my friend was able to distinguish clearly between these two entirely distinct aspects of my person. □

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to doubt it). In any case, the superyang aspect of the "boss" in my company must have aroused in him two quite distinct types of reaction. One, the only one I perceived until the last few months, and the only one conscious in him (I presume), expressed itself occasionally in a slightly pained attitude of regret, which I've had occasion to evoke, an attitude that never left the friendly or affectionate tones. On closer inspection, the other reaction itself appears "ambiguous", made up of two apparently opposing components. One was "positive", in the sense of an unreserved **valorization of** my person as the embodiment of heroic, "larger-than-life" "values"; generally accepted values, to be sure, which one assimilates in one's early years like the air one breathes, but for which one's immediate childhood environment had probably not provided any inspiring "model". This component-la, like the feeling of **affinity** (of an altogether different nature) mentioned earlier, went hand in hand with a sense of identity.

<sup>264</sup>(\*\*) On this subject, see the final footnote to "The nerve within the nerve - or the dwarf and the giant", n° 148.

the sense of **identification** with myself, without any element of antagonism. On the other hand, this antagonistic element is part of the other component, or rather, the other side (or "**reverse**") of this identification I've just described, and it remains more enigmatic for me. This is surely where the "paternal" role my friend has assigned me, by virtue of my conformity to a certain ideal "profile" supposed to embody such values, plays a crucial role. In groping my way, using the few tenuous elements at my disposal, to fathom the root cause of the strongly antagonistic content of this identification with an "adopted father" (with very "Superpère" features!), I came across (two weeks ago) a plausible, but still hypothetical, "scenario" in the December 30 note "Rancune en sursis - ou le retour des choses (2)".

This is not the place to revisit this scenario. It seems more interesting to revisit the image of "the dwarf and the giant" (which had just appeared in the note of the day before), from the point of view of this conflicting identification of my friend with myself. It then becomes clear that the two protagonists in the image, the dwarf and the giant, are none **other than himself**, or rather, **two distinct aspects of himself**. "The dwarf" represents what my friend feels to be **the original**, "unchanging" **aspect** of his being, rooted in his childhood as far back as he can remember, and no doubt even further... . It's also what is felt as the banal, insignificant, not to say derisory aspect of his person. It's **the aspect that's been disowned**, and therefore also felt as "irremediable", as "overpowering", as the "**pole**" of a person's identity.

**shameful** and contemptible of his being. "The Giant", on the other hand, represents **the vertiginous ideal** we despair of ever attaining' □ **to which** we can, at best, hope to resemble in the slightest, even if it means giving the lie to p. 731

to himself and to others, by every means at his disposal. One of these means has been to supplant the One who appears as the prestigious and envied embodiment of this ideal, and to "prove" his superiority over the Rival by every conceivable means. As for the Giant himself, he now appears as distinct from the Rival and Father, and is **the pinnacle, the ideal, heroic pole of the self**. The supreme gratification of the "boss" is anything that feeds the illusion that you **are** indeed this ideal pole, this projection of a spirit eager to enlarge itself. But the very craving for this gratification reveals an anxiety, "a deeply buried doubt" - it tells us that the person concerned "is not fooled, deep down inside, by these factitious signs of importance, of 'value'... . ."<sup>265</sup> (\*).

At a more superficial level of the psyche, these "factitious signs"<sup>266</sup> (\*\*) are, however, part of those "objective (more or less) characteristics" mentioned earlier, which are supposed to "lend credibility" to an act of identification with an ideal model (whether this remains in the impersonal form of a faceless "Giant" who lives within oneself, or takes on the familiar face of the enemy Father, the Rival).

#### 18.2.11.7. (h) The enemy brother - or the transfer (2)

**Note 156** (January 3) Yesterday afternoon, taking advantage of a little free time while waiting for friends to come by, I leafed through C G Jung's autobiography, which a friend had just brought me by chance. I was hooked by what little I had read. It was the first time I'd held a text by Jung in my hands, and until then I'd had only the vaguest idea of him - a dissident pupil of Freud, who had managed (according to scattered echoes that had come back to me) to reintroduce the shifting chiaroscuro of mystery into the straight alleys of the Master. That was about as far as it went. Now I had the impression of a living person like you and me, who doesn't waste his time bringing it back, and above all: one who will right to the real questions, <sup>those □ which</sup> he feels essential from his own lights, and who is not content p. 732

<sup>265</sup>(\*) Quotations in quotation marks are taken from "Infallibility (of others) and contempt (of self)", n° 4.

<sup>266</sup>(\*\*) These signs may be "factitious", but they often end up forming a "second nature" of unyielding solidity, "indémolissable" (to use the expression of the endnote in "Le désaveu (2) - ou la métamor-

(when the adventurous question is as old as the world) ready-made answers from learned people.

The "biography" aspect (intended for publication) was of course of particular interest to me, since the notes I'm currently writing are indeed somewhat akin to a biography, and in a spirit very close to Jung's: the external event remaining constantly subordinate to the inner adventure, of which it is both a revelation, and the occasional stimulus. It struck me that Jung didn't write an autobiography (or, more accurately, contribute to one) until he was 83, and, more importantly, that at no earlier point in his life did he take the trouble to examine his own childhood in depth. It would seem to me that for Freud's students, it must have been self-evident that one of the first things, if not the very first thing, to familiarize themselves with the ways of the unconscious, would have been to explore those ways in their own person! There's no doubt in my mind that a so-called "knowledge" of the unconscious that is limited to what is learned in a university curriculum (even if taught by a prestigious master like Freud himself), and to the analysis of a certain number of "clinical cases", remains a non-integrated knowledge, a fragmented, "dead" knowledge - a knowledge that by itself does not provide, or even promote, an understanding of oneself, or of others, or of the world.

But it's also true that self-exploration is an undertaking which, by its very nature, cannot be the subject of an institutionalized "program" - any more than the restoration, at its very root, of a disturbed psychic equilibrium (in a "patient", let's say) can be the fruit of the intervention of an "ogue" of any kind, confined to implementing boilerplate techniques. Disturbed equilibrium" is by no means confined to the socially unacceptable stage of a nervous breakdown or neurosis, but can be found in virtually everyone (to a **greater** rather than lesser degree). Psychologists themselves (or ethnologists, sociologists and other "ogues"), of all persuasions, are no exception! And a genuine restoration of disturbed equilibrium is by no means in the nature of a simple "medical act" performed on a third party. It is **an act of the person himself** and of no one else - **an act of love**, which he is free to do or not to do. It is not the result of the inexorable unfolding of psychic mechanisms (with or without the intervention of an expert in psychic mechanics), but an act in the full sense of the word, a **creation**, a **re-birth**.

p. 733 □ Before I finish writing the peremptory sentence above, about "so-called 'knowledge'" of the unconscious", I realized how the context can make her seem overconfident. Without knowing anything about Jung's work (which we've just been discussing), I seem to be dismissing him and his "so-called" knowledge of the unconscious - given that he apparently hadn't bothered (until he was 83) to explore the soil in which his own unconscious had grown. I assume, however, that when you read his biography, it will become clear that, without having devoted himself to such "exploration", Jung must have had **other** ways of making contact with his own unconscious (ways which themselves no doubt remained unconscious for a long time), so surely the premises of the offending assertion don't apply to him.

Something else of a completely different order caught my eye as I leafed through the glossary. Under the term "quater- nité" (NB this is the French edition), Jung insists on the "totalizing" character of the number four. Until about ten years ago, I was very resistant to the idea of a philosophical or "mystical" use of numbers - any speculation or discourse in this direction seemed to me nonsense, childish, "Hokus- pokus" (as we say in German, for fourpenny magic tricks). The little I've learned about the Yi-King (or "Book of Transformations") has made me less peremptory. Yesterday I made the connection between the "cosmic" character attributed to the number four, and the spontaneous grouping that had taken place, when writing "The key to yin and yang", in "packets" generally of four or eight notes, united under a common title. The

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phose", n° 153)!

The first group is reduced to a single note; this is true, but (as I had noted with satisfaction on completing the sixth group, "La mathématique yin et yang", which has seven notes instead of eight) if we combine it with a later group, into which this isolated note seems to fit most naturally, we still find a package of eight notes ( $7+1 = 8$ ), so again a multiple of four. This pattern has continued to the present day, the last group to be completed being Group 10 "Violence - or Games and the Sting" (156<sub>1</sub>). It has to be said that, from group 7 onwards ("The reversal of yin and yang"), I let myself be guided by this "pattern" which had just emerged without my looking for it, and without seeking or assuming any "meaning" other than that of a certain mathematical "regularity" in the form, felt to be harmonious.

This reminds me of the only other text I've written on a theme that could be described as "cosmic", focused again on the dynamics of yin and yang in human life and in the creative act<sup>267</sup> (\*). This text p. 734 grouped together, apparently without initial deliberation and certainly without effort at any point, in a rigorous numerical order. I'd forgotten what it was, but looking at it now (you're either curious or you're not!), it turns out to be seven "stanzas" of four "stanzas" each. So, once again, they were grouped by four. It's true that the number of stanzas is seven, which is not a multiple of four - so according to the Jungian criterion, the character of totality would not be satisfied for the work as a whole<sup>268</sup> (\*\*), but only for each of the seven "stanzas" that make it up. But I've still got enough to get away with, given that the famous "poetic work" was also provided with a providential "epilogue" (not to mention an interminable prologue, which I had the good sense to leave out), so we've still got  $7+1 = 8$ , we're saved!

It's time to return to yesterday's reflection where I left off. I had tried to understand the image of the dwarf and the giant in my friend, in terms of his identification with me. It appeared that "the dwarf" and "the giant" represent (or "**stage**", to use the expression in the note preceding yesterday's) the two extreme "**poles**" in my friend's person (I mean: what the "boss" has **instituted** as "extreme poles"): one "shameful, despicable pole", and another "ideal, heroic pole". To tell you the truth, with a difference of emphasis or lighting, I agree with the interpretation I found the day before for the same image-force of the dwarf and the giant, in the note of the day before yesterday "La mise en scène - ou la "seconde nature"" (n° 154). It was then a question of of the "staging" of the conflict instituted by the boss, the ego, between the two "sides" yin and yang of being. This formulation of the original conflict, in terms of the two "sides", would correspond to an undistorted knowledge of this conflict - and I'm convinced that this knowledge must indeed exist, in deep (but by no means inaccessible) layers of the psyche. The formulation in terms of two "extreme poles", yesterday, represents a **distorted view** of the conflict - distorted by a deliberate statement by the boss, valorizing one of the "sides" to make it an ideal, heroic "pole", and devaluing the other to make it a pole again, p. 735 extreme opposite to the previous one, a shameful, contemptible pole. I presume that this intermediate image lives in shallower, intermediate layers, perhaps partially cohabiting with the externalized image, the "staging" of the dwarf and the giant, even closer to the conscious surface, and partially encroaching on the superficial layers<sup>269</sup> (\*). In these layers, the idyllic image of "daddy" reigns.

<sup>267</sup>(\*) This is the "In Praise of Incest", discussed in note n° 43 (referring to the section "The Guru-not-Guru - or the three-legged horse", n° 45), and especially in the note "The Act" (n° 113), pp. 507 - 509. See also the beginning of the note "The dynamics of things (yin-yang harmony)", n° 111.

<sup>268</sup>(\*\*) The projected work (under the provocative name "Eloge de l'Inceste") was in fact to comprise three parts (L'Innocence, le Conflit (or la Chute), La Délivrance (or l'Enfance retrouvée)), of which only the first was completed. That's what we're talking about here.

<sup>269</sup>(\*) This presumption regarding the image of the dwarf and the giant stems, of course, from the very explicit expression of this image, in the final word of Pierre Deligne's biographical note written by himself (alluded to in the last footnote to the note "Le nerf dans le nerf - ou le nain et le géant", n° 148).

cake" a little soft around the edges, from a respectful and considerate son, with visible velvet and an invisible velvet claw. ...

Compared to the day before yesterday's reflection, yesterday's seems to me to have nuanced it, and thus to have sharpened its contours somewhat, without yet bringing anything essentially new to it. It's true that when I stopped the reflection because of the prohibitive hour, I didn't feel that I'd reached the end of the path I'd embarked upon, that of "ambiguous identification". In retrospect, I realized that, no doubt as a result of my inveterate habit of "seeing myself as a yang", it seemed to go without saying that any identification with myself could only concern my yang traits. In this case, in the stage image of the dwarf and the giant, it was the **giant** that I had recognized myself in, in a distorted but still clearly recognizable form. If, however, I am insistently presented as "**the dwarf**"<sup>270</sup> (\*\*) by the effect of the "reversal" syndrome in my friend, this assimilation (with obviously malicious intent) was immediately rejected by me, by a reflex of universal naturalness and great strength: to be confronted with a desire for derision, targeting traits (yin, in this case) that are perfectly real in me, while passing over in silence the complementary traits that are just as real (which, for their part, benefit from a valorizing consensus) - such a situation elicits in me the never-ending reaction, if not to deny the incriminated traits entirely, at least to tacitly minimize them, by putting forward, as if to **oppose** them, the unjustly retracted traits.

With this "visceral" reaction, I'm well and truly entering the round of conflict, just as I'm supposed to! It alerts me to that eternal □ "hook" where I'm taken for a ride. My

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I also find my own vision of reality distorted, in response to a provocative distortion. So it was in vain that I wrote yesterday, from the tip of my lips (or the keys of my typewriter), that

"the first "objective character" likely to foster a feeling of resemblance and an act of identification, was the strong affinity between his approach and mine to our common teacher, mathematics".

In writing it, I was forgetting that this "strong affinity" consisted of a **yin, feminine** approach to the discovery and knowledge of things - that this was precisely the aspect by which, as "similar" to him, I too appeared as a **dwarf**, just like him: it was the secret, vulnerable, shameful side that he reserved for himself to bring into play, when the right moment appeared, to supplant and "overthrow". This "providential circumstance"<sup>271</sup> (\*), the yin predominance in my drive for knowledge, was **not** only a **weapon in the hands** of a dubious friend - it was also, and first and foremost, a kind of "objective foundation" for his identification with me; not, this time, as identification with the **father**, but as identification with an **older brother**, not to say an "older sister".

When I use the term "objective" here, it's to express that this time it's a question of an "identification" rooted, not in one of the fictions of the "boss" wanting (or fearing. . . ) to be this or that, but in a profound, tangible, indubitable **reality** - that of a **kinship** between the original nature of one and the other. In any case, surely this kinship could not fail to be perceived by him as by me, and I have no doubt that at some deep level, the **meaning of** this kinship was also perceived. At the very least, I presume, without being totally convinced, that this perception must have served as material for his identification with me. This identification would have taken place on **two** distinct **levels**: on the one hand, the "ideal" level, in which I figure as the embodiment of **values** of which he would like himself to be a

<sup>270</sup>(\*\*) This "dwarf" himself-mime being no other than a metaphor for the "Meganana" with the features of a "false" giant, with flabby and ramomo forms. ... (Feb. 85)

<sup>271</sup>(\*) See note of the same name, n° 151.

exemplary incarnation (even if only in appearance, as the model appears out of reach, and is supposed to actually realize the ideal); on the other hand, the "real" level, where identification is established through of a **de facto kinship** correctly perceived, but a kinship in common traits deemed redhibitoires, pitiful<sup>272</sup> (\*).

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This is a good time to remind myself that at the time of our meeting, and for more than ten years afterwards, I had the same repression of my "feminine" traits that I've come to see in my friend. Looking back, it seems to me that, at the time of our meeting, this repression already existed to some degree in my friend, but that it remained mostly latent, and in any case, was much less strong than it was in me. As I've pointed out on more than one occasion, my person had long been marked by a superyang imbalance, whereas his gave off an impression of harmonious balance. Since then, he and I have **evolved in opposite directions**: my friend from a state of yin-yang balance to a strong yang imbalance, and I from a strong yang imbalance to a state of (relative) yin-yang balance.

The idea that immediately arises is that my friend, perhaps by virtue of this double identification with me, has followed (some thirty years later!) the evolution, in the sense of a deterioration of an original balance, that I myself had followed since the age of eight. It's possible that a moderate over-valuation of "virile" values to the detriment of "feminine" values was transformed, through contact with me or with the environment I was part of, into an over-valuation with a touch of zinc. But as I've pointed out elsewhere, the "nerve" (or "living force") in the Burial orchestrated by him, and the nerve also in his own metamorphosis (which is also the burial of the child in him by the boss's care. . . ) - this nerve can scarcely lie in the mere adoption of this or that other, more or less extreme (or even demented!) value system. And the same applies to the "nerve" in my identification with myself, and the disproportionate role this identification has played in my friend's life. There's no doubt that one and the same "force" is at work, and that its roots reach far back into his childhood<sup>273</sup> (\*\*).

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Another strange idea comes to me here. It seems that the heaviest burden that I have carried for forty years of my life, this repression of the "feminine" in me by the "masculine", which was also akin to that of the child in me by "the Big Boss" - that this burden was **"taken up"** by my friend, at precisely the moment when it might have seemed that he himself was free of a similar burden. It was around the time when my value system shifted in a yin direction, a development that foreshadowed the moment of my reunion with the child some fifteen years later, when I suddenly felt relieved of an immense weight<sup>274</sup> (\*). The immediate association here is with the Hindu idea of **karma**. It's clear to me that over the last eight years, I've lightened a substantial part of the karma I've been carrying around with me since childhood. I would have thought (and still tend to think) that this lightening has not been "at the expense" of anyone, that it is beneficial not only for me, but "for the whole world". I can even say that I **know** very well that this is so, even if it turns out that someone else chose (or even had to choose) to take it over. It's also true that I don't consider this karma I've lightened as an "evil". It was for me the nourishing substance of a **maturation** that was ahead of me.

<sup>272</sup>(\*) These two "levels" thus correspond to two distinct "archetypes", and here in opposition to each other, in the identification with my person: that of the father (aka "the giant"), and that of the Brother, or even, that of the Sister (aka "the dwarf"). The latter is also found in the image of the "cake-daddy" - suggested by the father in the flesh "as he is", alas! and not "as he should be". . .

<sup>273</sup>(\*\*) For a more precise intuition along these lines, see especially the note "Rancune en sursis - ou le retour des choses (2)", n° 149.

<sup>274</sup>(\*) This "shift" in value system is discussed in the note "Yang plays the yin - or the role of Master" (n° 118), and the "reunion" in the note of the same name (n° 109).



me. I know that it is good for me and for everyone that I have eaten and been nourished by it, that knowledge has been formed in the nourishing womb of ignorance<sup>275</sup> (\*\*). It seemed to me that this substance or karma, once transformed into knowledge, left no residue, that it disappeared. To tell the truth, I don't know what the Hindu or Buddhist tradition teaches on this subject - if for them there is a law of "karma conversation" (similar to that of the conservation of matter), which would be unaffected by the vital creative processes of ingestion, digestion and assimilation.

For the sake of propriety, I've just omitted **excretion** from these "vital processes". Yet excretion (along with the death of the entire organism) is a key process in the recycling of what has been absorbed, returning to the infinite cycle of transformation of "dead" organic matter into living organic matter, whereby life is eternally reborn from death<sup>276</sup> (\*\*\*)).

p. 739 **Note** 156<sub>1</sub> □(February 30) This "pattern" eventually broke with the ultimate group 12, which, alas!

n°

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**six** notes, bringing the total number of notes making up "The key to yin and yang" to 62. I had anticipated that there would be **eight** notes in this "Conflicts and Discovery" group, which would have been in keeping with the criterion of totality, and would have brought the total number of component notes to  $64 = 8 \times 8 = 4 \times 4 \times 4$ , which is also the number of hexagrams in the I Ching! I was sorry that my expectations were not fulfilled, but I didn't want to "cheat" and include in "The key to yin and yang" the Jeux notes devoted to Pierre Deligne's visit to my home, whose natural place seems to me to be in the continuation of "The Funeral Ceremony", **after** "The key...".

However, I remain dissatisfied with this group n° 12, the only one of the twelve parts. of "La clef. . "which doesn't leave me with an impression of **unity** of inspiration and purpose. This lack of unity seems to me to be due, not to the theme of "Conflict and Discovery" itself, but to the irruption of extraneous (and at times disturbing) events in the course of reflection.

(March 7) Last night, as I reread the thoughts of January 14, which I had grouped together in a note (n° 162) called "conviction et connaissance - ou la passation"<sup>277</sup> (\*), I felt dissatisfied with this name. On the one hand, the "main" title and the subtitle didn't seem, "at a glance", to fit together - in fact, they correspond, one to a first and the other to a third "movement" in the reflection, which by themselves are apparently unrelated: description of the process of the blossoming of knowledge (in the form of a sudden **convic- tion**), and evocation of the endless chain and "passing on" of karma, from one generation to the next, and from one person to the next. What's more, the most intimately personal content, the "neuralgic" content for my own person, which was the substance of the "second movement" of the reflection (and had in fact been the "passerelle", leading from the first movement to the third) - this crucial content did not appear in the chosen name. (There's no doubt in my mind that this surreptitious concealment is not at all the ef-

fect of pure chance. ... ) Since all three themes seemed important in their own right, and I couldn't think of any "appropriate" name or double-name that would evoke all three, I ended up com□prendre

that the best thing would be to split the note into three, with a suggestive name for each one separately: "Conviction and knowledge", "The hottest iron - or turning", "The endless chain - or passing (2)" (n° s 162, 162', 162").

<sup>275</sup>(\*\*) For thoughts along the same lines, see the end of the note "Le cycle" (n° 116'), and in particular the last paragraph.

<sup>276</sup>(\*\*\*) On the cycle of life and death, see also the note "The Act", n° 113.

<sup>277</sup>(\*) This was also the last note in "The key to yin and yang".

.....It was afterwards that I suddenly realized that this operation, dictated (so to speak) by the very substance of the reflection, had at the same time resolved the "aesthetic" dissatisfaction that I had been dragging on for almost two months, while this twelfth and last part of "The key to yin and yang" (which I had called "Conflict and discovery") stubbornly refused to let itself be completed (naturally, that is) in a sequence of eight notes, and only wanted to include the six that were already written. And I received my reward for not giving in to the easy temptation to "cheat" and "stick" two notes at the end of "La clef", "on the spur of the moment" and who se place was elsewhere! This last part of "The Key" (which will eventually be called "The Enigma of Evil - or Conflict and Discovery"), at the same time, takes on a beautiful symmetrical structure, with two packets (of three notes each) on the central theme, clustering around the two "digression-notes" on Fujii Guruji and on my monk friends.

## 18.2.12. Conflict and discovery - or the enigma of Evil

### 18.2.12.1. (a) Without hate or mercy

**Note** 157 (January 4) In yesterday's and the day before's reflections, I tried above all to get in touch with the reality of my friend's identification with me, and in so doing, to discern its scope and implications. It's a job I'm still doing as one groping around in the half-light, not to say, in the dark of night. Or perhaps I should say that my eyes remain closed, and my eyelids opaque to a light that I remain unable to perceive. In any case, I have no recollection of having "felt" or "seen" this identification at any time during my relationship with my friend, any more than I have "felt" or "seen" his antagonistic attitude towards me. Yet I **know**, without any possibility of doubt, from a rich body of concordant facts, that this identification with me, and this antagonism which is like a shadow of it, are **realities**.

- just as someone born blind "knows" the sun, daylight, colors, light and dark, exist, even though he has never seen them. He knows it, without having **knowledge of** these things. Or if he does have a very diffuse knowledge of them, through a more refined tactile sense perhaps (or through a "memory"

which is rooted not in his life alone, but in those of countless generations ☐ of sighted beings that preceded it), this knowledge remains indirect and fallible, like that of a warm, sonorous voice coming to us through a distant, uncertain echo.

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The work we've done over the last two days has again been like a stopgap, like a substitute for an immediate perception that's lacking. This is more or less the case in all "meditative" work, as I understand it. The work constantly **pushes** against the current of **inertia** - the inertia of leaden eyelids! Certainly, in moments when the eyes are fully open and awake, there's no need for meditation or work: all you have to do is look, and see. As these moments are rare, rather than sit back and wait for them, I prefer to take the lead, without worrying that the work is clumsy and "slow". It may be slow, and sometimes even slower than usual - but that doesn't mean it's ever stagnant or going round in circles. When there is work - real work, I mean, driven by a real desire - then there is progress: something is done, takes shape, is transformed, imperceptibly at one moment, visibly at another... . And sometimes, at the end of a clumsy, stubborn progression through a formless, contourless penumbra, continuing for hours or days, even months or perhaps years, the miracle happens: the blind man **sees**! And what is seen is not a fleeting vision that disappears as if it had never been, leaving only the faint trace of a memory. It's a **knowledge** born of these obscure labors, a new knowledge, as intimately ours as the taste of the things we love.

I wrote in the day before yesterday's reflection that if there was a case in point whose thought had "guided my pen" nine months ago, as I wrote the final lines of the note "The Enemy Father (1)" (which I had just quoted), it was that of my friend Pierre in his relationship to me. Yet other "cases in point" even closer to me must have been present in my mind at the time, in the background of my reflection. When I talk about it

of "a father both admired and feared, loved and hated" and then of "another Self, feared, hated and shunned. . . ", the terms "feared", "hated", "hated", and probably even the term "fled", do **not** apply to friend Pierre's relationship to me. Neither by direct perception, fleeting and slight though it may be, nor by cross-checking with the facts known to me, have I ever had the slightest indication of **fear**.

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that my friend would have had of me, or of a hatred or only of an **animosity** that he would have nourished against me. The opposite ☐ is true, as I've had occasion to point out on more than one occasion. And it's this It's precisely this circumstance that has made the seemingly gratuitous, unflinching antagonism that has manifested itself in crescendo over the past fifteen years so disconcerting, under the guise of "thumb!", aka "velvet paw"<sup>278</sup> (\*), finally reaching the pitch of quiet impudence, sure (provided certain forms are respected) of total impunity. . .

This disconcerting, enigmatic progression is immediately associated with the equally "disconcerting" and "enigmatic" (and these are euphemisms, to be sure!) progression in the deterioration that followed, also over a period of some fifteen years, in the couple's relationship with my former wife, and by the same token, in the family we had founded. In the absence of any sign of chronic hatred or animosity towards me on the part of my wife, it took me ten years of inexorable deterioration in the relationship (while most of my energy was taken up by mathematics, playing the role of the famous pile of sand for the ostrich... . ), before finally acknowledging the presence, in the one I continued to love, of a tenacious, mysterious and implacable will to destroy, working against me through those who were dear to me. That was in 1967, five years before I left home, and ten years before the resolution of a conflict that I felt was the heaviest burden of my life. With the hindsight afforded by a long-accepted relationship, I can only note what continues to remain a mystery to me: an insatiable will to destroy, and at the same time an **absence of hatred**, or only animosity, towards those, adults or children, who are mercilessly struck down, whenever the occasion arises.

It's the same mystery, all things considered, as the one I'm now confronted with in my friend's relationship with me, with the difference that this "tenacious will to destroy... exerted against me through those who are dear to me" was rigorously confined to the world of mathematicians, and that its instruments and hostages were not my children "in the flesh", but those who symbolically took their place: the students and assimilated who, if anything, "bore my name". In both cases, not only do I detect no hatred or animosity, but there are also feelings of sympathy, and often even affection, towards me that cannot be doubted.

☐ These aren't the only situations in which I've been confronted with a desire to injure, even even a desire to destroy (in the strongest sense of the word<sup>279</sup> (\*)), without any trace of hatred or animosity. The one that most strongly marked my life was in 1933, in my sixth year, with my mother as the protagonist - the year in which the **family** we formed, my parents, my sister and I, was destroyed forever<sup>280</sup> (\*\*).

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The various situations of this kind that I've experienced up close, from a desire to destroy, or a desire to hurt as deeply as one can, without my detecting any trace of animosity, seem to

<sup>278</sup>(\*) See the two notes "Pouce!" and "Patte de velours - ou les sourires" (n° s77, 137), as well as the notes that follow the latter, forming the "La griffe dans le velours" part of "La clef du yin et du yang".

<sup>279</sup>(\*) By "strongest sense", I mean here a will, not to cause suffering for the sake of suffering, or to destroy some limited thing dear to the other, but the will to psychically (if not physically) destroy the other; the will (when possible) to implant an indelible and devastating despair in the face of "that which is beyond comprehension". Behind the brilliant and affable exterior of "Colloque Pervers", I seemed to find this extreme dimension in two of the most brilliant of its actors... ..

<sup>280</sup>(\*\*) For more on this episode, see "Le Superpère", note no. ° 108.

very different from one another. I doubt I could find a common "explanation" for them, or at least a common trait in the protagonists' distant antecedents, which would suggest a deep causal link<sup>281</sup> (\*\*\*). Perhaps more important than an explanation, and in any case more important than an explanation, is the **realization that** such a thing exists: **the will to destroy in the absence of hatred**. This brings me back to the theme of "gratuitous violence", touched on earlier in a different way<sup>282</sup> (\*\*\*\*). Here, we're talking about gratuitous (and sometimes destructive) violence **against a loved one** or a person considered to be a threat.

"friend". The mere **existence**, in everyday life, of such violence (which rarely says its

□nom), is an important **fact** in everyone's life - one of the important facts of human life. Observing this p. 744

In fact, by going against the inveterate mechanisms that constantly push us to try and hide it, is the first step towards assuming it. No theory, no reasoning, no "approach" can save us from taking this step.

I don't know if I'll ever **understand** this fact, but it seems to me that to understand it is also to "understand conflict". What is clear to me is that such understanding cannot come from "theory", any more than from "experience" (by virtue of experience alone). It is not some "sum total" of an accumulation (of "knowledge", or "experience"), just as it is not of the order of the intellect alone, nor even of the order of the "intelligence" alone<sup>283</sup> (\*). I'm not sure I know anyone, even by name, in whom such an understanding lives. But it seems to me that anyone who, after a hundred and a thousand evasions in the face of an irrefutable reality with a thousand faces, has finally come to the simple **realization** of this fact, humbly, without bitterness or revolt, without resignation or indignation - as the realization of a formidable **mystery perhaps**, the meaning of which escapes him, but the extent and depth of which he senses; a mystery which intrigues or challenges him, without frightening or worrying him any more - that person has not lived in vain.

### 18.2.12.2. (b) Understanding and renewal

**Note 158** (January 5) Although it wasn't premeditated, the final accents of yesterday's reflection were very much in the vein, again, of a Funeral Eulogy - but this time delivered (or sung) by the deceased himself. One is never so well served as by oneself!

Yesterday I was confronted once again with one of the most perplexing aspects of the "mystery of conflict": that of the will to destruction without hatred or apparent motive, exercised in the shadows, obstinately and relentlessly, against a loved one, or such loved ones or friends. Sometimes, such a will gets out of control, leading to an all-out destructive frenzy in which anything vulnerable becomes a welcome target. It's like an irrepressible bulimia for "action" in reverse, whose repetitive nature (like that of clown games), and consummate mastery in the art of pulling the strings,

can have a most comical effect, when the observer (or even the one who has just paid the price) is

gifted with a sense of humor' □ and that the "Actor-Marionettist has only modest powers over others. La p. 745  
situation is more serious, it is of consequence, when there are children among those who bear the brunt of the

<sup>281</sup>(\*\*\*) Yet a deep-seated, virulent self-contempt is surely common to all these situations. Perhaps such virulence (when it is not resolved by an act of grace, by a profound inner transformation, i.e. as long as it is not "assumed") must find an outlet and express itself through destructive acts, through a will to destroy, which turns against its own person when it does not seek and find its target in others. In many a person, and even in close relationships, I have witnessed the simultaneous action of a will to destroy, directed both against oneself and against some external target, chosen from among those close to us (mother, father, spouse or child. . . ). (February 1985) See also the reflection in "The cause of causeless violence" (n° 159), three days after the present note, which obviously prepared it.

<sup>282</sup>(\*\*\*\*) See the note "La violence ingénue", n° 139.

<sup>283</sup>(\*) (March 5) In any case, I know that such an understanding will only come to me through an understanding of this violence. **in myself.**

circus games, even if these are only "bloody" in the figurative sense; and also when those possessed by a thirst for destruction find themselves invested with considerable, even discretionary powers over some of their fellow human beings. History records the names of some despots possessed by such a madness of indiscriminate destruction, turning their fiefdoms into vast mass graves. We think of Ivan the Terrible, or Stalin, or a certain Chinese emperor (whose name and millennium I've forgotten) who was finally slaughtered by his own cornered subjects, armed with sticks and stakes<sup>284</sup> (\*). There's no doubt that there have been similar cases in our own country, perhaps on a smaller scale, and about which "History" has been more discreet... ..

When I wrote yesterday, without any false modesty, that I didn't understand the "fact" I'd just observed, that of the thirst for destruction in the absence of hatred, this in no way meant that I had no ideas on the subject, quite the contrary. On the contrary, I have more than just "ideas", I have some very strong intuitions. They were born and bred in the soil of my life, rich in the conflicts that sometimes seemed to devastate it, like endless storms raging across a still landscape.

p. 746 of winter' □ ruthlessly <sup>ripping out</sup> what needs to be ripped out<sup>285</sup> (\*). But all is belly for the sleeping earth waiting in silence. When spring returns, in the hollows of the great dead trunks lying there inert, there is intense life, and in the following spring (if not the very same year) we can already see grasses and flowers blooming there.

These "strong intuitions" all concern, I believe, the "**ingredients**" of conflict. I've spoken a little, and spoken again, about some of them, first and foremost "**self-contempt**", and its links with the repression of certain aspects and essential forces of our original being, such as the yin or yang "sides", one of which is often denied. I've also often had occasion to talk about **vanity**, which is like a calling card, the most universal of all signs, and the most apparent, of the presence of conflict within us, and which appears to me as the "**front**" of the same medal, the "back" of which is self-contempt. There is **contempt for others**, an outward projection of self-contempt, for which it is at the same time a cover, or better said, a diversion and an exorcism. Basically, contempt for others is nothing more than deliberate ignorance of their existence as sentient beings who share in this world in the same way as ourselves. Gratuitous violence cannot

<sup>284</sup>(\*) This emperor, fearing a popular uprising, had forbidden the people to use any metal objects (such as knives, forks, etc.) that could be used as weapons, with the exception of one knife per village, attached by a strong chain in a public place.

What all three of these characters had in common was that, in addition to their thirst for destruction, they were also possessed by **fear**: fear of being murdered, and beyond that, no doubt, fear of their own inevitable **death** - as they sowed death all around them. This coincidence is surely not fortuitous. I also note that Stalin (the only one of the three about whom I've had any detailed information whatsoever) began his political career as a great master precisely in the art of pulling the wool over people's eyes, of manipulating them by playing on their vanity and greed. His first acquired style was, it seems, that of the "velvet paw", until it became unnecessary for him to bother hiding his claws.

If I haven't included my (ex-) compatriot Hitler among the examples cited, it's not because I have any particular sympathy for him, but because I don't detect in him the mania for "**all-out**" destruction mentioned. The targets of scorn, then destruction, were those designated as "the others", "foreigners": first of all "the Jews" (and the Communists and other "Judeo-Bolsheviks" dear to Nazi jargon), then "Asians" and other non-Arian metatics. The good, non-Jewish German was all very well under Hitler, at least until the first major Allied air raids, when the war really started to go badly for them.

<sup>285</sup>(\*) No sooner had this image been jotted down in the rush of the pen than it occurred to me that it was only partially adequate - it would almost have an aftertaste of "cliché"! As I reflect for a moment on this aftertaste, I rediscover the old deliberate intention within me to "see my life as yang": movement, arrows and storms... .

Without even taking the time to pose, but sensing that the image wasn't right (and yet, that's what had come to me, nothing to do!), I "corrected" it in the text by going on to the "sleeping earth waiting in silence" - and voilà, yin! It was the chord that "resolves" a "false chord" (or "dissonance"). In many ways, a more accurate image than that of the storm, "tearing away what must be torn away", and in more yin tones precisely, would be that of the worm gnawing away "what must be gnawed away" - and which finally collapses - but all goes belly-up for the earth that waits in silence, and when spring returns... . (continued without change!).

to germinate and proliferate on the terrain of such contempt. There's the **fear of knowing**, the fear of reality, a fear whose nerve center, this "Black Point", the epicenter of a vortex of anguish ready to be unleashed at the slightest alarm, is the fear of knowing ourselves: the fear of becoming aware of our own poses and subterfuges, however crude; and the fear also of becoming aware of the creative force within us that day after day we reject and bury, through these same poses and subterfuges.

In my life, fear appeared at the age of six, when there was still (it seems to me) nothing to fear. vanity. This must have only appeared later, at the moment (I presume) of the "changeover" that took place around the age of huit<sup>286</sup> (\*). And it was fear, too, that disappeared first and without trace, as soon as apparition of a curiosity that's both benevolent and irreverent, certainly intrigued but in no way impressed by the abracadabra and macabre spectacles of the "Point Noir" genre. The mechanisms of vanity, on the other hand, have remained in place with no apparent change in the eight years since the fear of knowing disappeared. It's only the hold these mechanisms have on my life that has changed, as they are defused by the presence of an awakening curiosity that doesn't let itself be fooled!

I have in my hands a whole range of conflict ingredients - which I know at first hand, without a shadow of a doubt, are indeed essential ingredients. And for years now, I've also had everything I need to "assemble" these ingredients, carefully explaining, in the light of what I've observed in myself and others, their links of contiguity and dependence. It's a job that will take a few days or a few weeks, not even months, I presume, and will surely be very instructive and useful. If I haven't taken the trouble to do it yet, giving priority to other, more directly personal directions, it's probably because I was well aware that it's not from such an "assembly" of ingredients, in general terms from which my person is absent (if only as one "example" among others), that an "understanding of conflict" could come to me ; nor does the mere fact of placing side by side, "assembling" or even mixing a certain number of simple bodies, "ingredients" in the composition of a compound body, reconstitute the latter. For "reconstitution" to take place, a "chemical reaction" must first take place - something bringing the ingredients into contact and into play in a far more intimate way, and by forces of an altogether different order, than simple "assembling" or mixing could do.

The same is true for an understanding of the things of life. Intelligence alone can, at a pinch, spot the ingredients of something like "conflict", and it can in any case, in the presence of ingredients already known and with the help of facts about them (known first or second hand), put them together in a plausible, even "correct" way. This kind of work can be useful in recognizing a conflict situation from time to time, and in identifying a more or less precise "etiology" - but that's not the whole story.

an "understanding of conflict". I will say, however, that I have moved one step closer to such an understanding, the day when my **relationship to conflict** will have been transformed, When I say "my relationship to conflict", I mean p. 748 in the first place, of course, the conflict within myself, and (from there) the conflict that occasionally pits me against this or that person; and lastly, the conflict I see acting in people close or less close to me in my everyday life, which often expresses itself in conflicts pitting one of them against another.

Over the past eight years, there has been such a progression towards an understanding of conflict, which also means: a transformation, or rather, successive transformations, in my relationship to conflict. I've already mentioned two or three episodes<sup>287</sup> (\*). Perhaps a full understanding of conflict is equivalent to a full acceptance of its existence, wherever and however it arises.

<sup>286</sup>(\*) On this "tipping point", see the note "Le Suprême" (n° 108).

<sup>287</sup>(\*) On this subject, see in particular the two notes "Acceptance (the awakening of yin (2))" and "The slave and the puppet - or the floodgates", n° 110, 140.

manifesto<sup>288</sup> (\*\*). I'm obviously a long way from that! And perhaps a full understanding of conflict also means total resolution of the conflict within oneself. I'm even further from that!

But there's one more thing I think I know, and that's the nature of the force that, from a combination of ingredients, suddenly gives rise to an **understanding** that renews the person. It is precisely this force that is not "of the order of intelligence". I doubt that any intellectual work, the reading of books let's say, no matter how learned, profound or sublime, will in any way stimulate its appearance. When it does emerge, it's only in silence and in contact with that which is most intimately personal to us and our lives; something, therefore, that no book and no person, be it Christ or Buddha, can ever reveal to us.

When I say "that which is most intimately personal", I don't mean that these are things we can't talk about, either to ourselves or to others - and sometimes it's good to talk about them. But even when spoken through the voices of angels and prophets, what is **said** is not the thing itself. This thing-already known, but buried perhaps, whose contact can suddenly give rise to a new knowledge-is **known** neither to angels nor prophets, nor even to the closest and most beloved of beings, but to the most intimate of beings. from **you** alone.

p. 749 □To return to conflict, and "destruction without hatred", which seems to me the hardest "core" of the conflict, the most resistant to understanding, that is, to **acceptance**. I also think I know, in the next step before me to enter further into it, what **is** that "most intimately personal" thing I'll first need to get in touch with again; the thing that would play the role, in this case, of that famous "Black Point" so tenaciously evaded! It's the experience of situations of "gratuitous violence", of contempt for others (and of "destruction without hatred" too, perhaps), in which **I** was the actor - the one who did the violence, the one who found it worthwhile to despise. It's by coming into contact with this reality, or never, that I'll be able to get to the bottom of this famous "self-contempt", and finally **see**, beyond all "no doubt" and all "maybe", if this is indeed the deep root of evil, and not just in "everyone but me"!

### 18.2.12.3. (c) The cause of causeless violence

**Note** 159 (January 7) The reflection in the previous two notes revolved around the mystery of the existence of this strange thing: a will to destroy (or a will to hurt, or humiliate, or harm), in the absence of any hatred or animosity. The impetus for this reflection came from my friend Pierre's relationship with me, which immediately led to an association with my ex-wife's relationship with me. More than once during the reflection on Burial, I was led to realize, or to remember, that in these two cases as in others, it was certain traits in myself, the "super-viril" traits that I had cultivated in myself since the age of eight, that served as stimulators and "attractors" for such antagonistic impulses. If I'm not mistaken, this was first mentioned in the October 5 note "Le Superpère (yang enterre yin (2))" (n° 108). This link is taken up again in the following note of October 9 "Les retrouvailles (le réveil du yin (1))" (n° 109).

In this note, I return to the moment when, for the first time in my life, I perceived this link. It was October 18, 1976, the very day of my reunion with the child in me, and in the final lines of the notes that bear witness to this most important day of my adult life. In these lines (reproduced in the

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<sup>288</sup>(\*\*) The meaning of such "full acceptance" can give rise to innumerable misunderstandings. It is quite different from connivance. It does not exclude **refusal**, clearly and unequivocally - it contains it. On this subject, see the reflection in the note "Spouses - or the enigma of "Evil"" (n° 117).



note quoted), I speak of the "secret hatred and resentment" of three women I had loved, including the one who at the time was still my wife (although I had not lived with her for five years). With Looking back, it seems to me that in each of the three cases I had in mind, this impression of □ "secret hatred" p. 750 did not, strictly speaking, correspond to reality - by which I mean any direct perception I might have had at any time<sup>289</sup> (\*) of such hatred. What I had perceived, and what I had had ample opportunity to experience, was a will to destroy, or a will to cause pain, or to injure, both lasting and apparently inexplicable, gratuitous - something I had **interpreted** as a sign of hatred, "secret", because never expressed. In fact, I think that for two of the women in question, it was in these quoted lines, for the first time since I had □ known them, that I made the observation that appeared to me p. 751 as a "secret hatred". At this point in my life, it was impossible not to make the confusion I've just mentioned. This confusion in no way detracts from the importance of making this observation, involving myself in it just as crucially as these women to whom I was closely linked.

As for the "resentment" referred to in one breath with "secret hatred", I sensed from the outset that if a "certain force" superyang within me had drawn the resentment of each of these three women to my person, it was for grievances for which I was in no way responsible - for wounds and damage suffered "long before they knew of my existence, in the distraught days of a childhood deprived of love". This perception, which had decanted over the years as the fruit of an intense experience, surely had the effect of an invisible guide for my reflection of last December 20, in the note "Rancune en sursis - ou le retour des choses (2)" (n° 149), where the intuition appears that this same process of **displacement** of an initial resentment, or a "rancune en état de vacance", could well have taken place in my friend Pierre, around the time of our meeting or perhaps even earlier. The facts known to me make this intuition at least plausible.

There is, however, an important difference with the case of my ex-wife, and with the other two cases discussed in the post-reunion meditation. I don't have the impression that

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<sup>289</sup>(\*) (March 6) After writing these lines, I remembered that in the course of my married life, there were two episodes, the first lasting a few days, the second a few minutes, when I felt assailed as if by two beams of hatred, shooting from the eyes of my then wife.

The first time, in the fifth year of our marriage (1962), my wife suffered what is euphemistically called a "nervous breakdown". This episode had a profound effect on the couple's life and the family atmosphere. It is also the moment in my life, of all those I have conscious memories of, that was experienced as the most atrocious, and that marked me most deeply (as it was supposed to).

Unless we have an exceptionally stable inner foundation (which, due to lack of maturity, I was far from having at the time), the hatred we are the target of, and even more so when it comes from loved ones close to us, has a devastating effect on our psyche, when it arouses in us a similar and destructive hatred of ourselves. It would seem that something in us must, at all costs, find a "meaning" for "what passes understanding", even if this "meaning" is an outright condemnation and rejection of ourselves by ourselves: since we are hated (and even though the "reason" for this hatred escapes us completely...), it's because we are hateable... .

If I was so affected by this episode, which remained like a sword of Damocles hanging over my life for the next six or seven years, it was surely because it resonated violently with a traumatic childhood experience. This had disappeared from my conscious memory, but it was all the more active whenever I was suddenly confronted with inexplicable malice or hatred - all as sudden and inexplicable as the will to destroy that had assailed me at the age of five, coming then from the person of all people who, as far back as I could remember, had been the peaceful and secure center of the Universe.

It's one of the most important things I've learned in my life about the malevolence or hatred I'm sometimes the target of, that I'm in no way the real and immediate **cause** of it (even if certain aspects of myself, which I neither disavow nor reject, contribute to attracting it to me). For years, however, this knowledge remained too epidermal to defuse this deep-rooted mechanism, which comes into play when I'm confronted with apparently "causeless" malice or violence. To defuse it, I first had to go back to its roots and retrace the steps of those forgotten days and nights, heavy with anguish, when my mother suddenly, mysteriously and inexplicably, became a hostile and fearsome stranger... ..

my friend's childhood was in any way "distraught" or "deprived of love". This difference seems to me to manifest itself in the tone of my friend's antagonism towards me, which at no time reached that pitch of **vehemence** with which I was so familiar in the other three relationships. Equally, in my friend's relationship with me, the appearance of signs of antagonism was at first extremely discreet and sporadic, and even after my departure in 1970, it took another eight years before this antagonism expressed itself directly and unmistakably against my person<sup>290</sup>. This seems to correspond to the existence of an initial "resentment" which remained diffuse, imponderable, without the presence of a hard "core" corresponding to the feeling (even if hidden from the conscious gaze) of an outrage or a wrong suffered, felt as irreparable perhaps. ...

p. 752 □ In evoking, in the penultimate note, the will to destroy, or that to injure or harm, in the **absence of hatred and animosity**, the thought came to me (with some insistence) of an apparent contradiction, which I thought I'd come back to straight away. This is it. In the two cases that were the focus of my attention, involving my pupil (and mathematical "heir apparent") and my wife, there had indeed been an unconscious "grudge" that they had transferred onto me. The very idea of a "grudge" or "resentment" seems linked to that of an "animosity" or "enmity": one would be tempted to say that a grudge (or resentment) is one of the possible ways (and one of the most common) of feeding an animosity. And this assertion is certainly justified, in the case of what we might call a "direct" grudge, a "real" grudge, motivated by a **grievance** (real or imaginary) towards the person concerned, for a **wrong** or **damage** that that person has allegedly inflicted on us. But in the cases I'm dealing with, it's not such a grudge we're talking about, but an indirect grudge, "**by proxy**" so to speak, transferred from an initial potential target, inadequate for one reason or another<sup>291</sup> (\*), to an "**adoption target**" or replacement, which appears to "fit" the needs of the cause. The remarkable thing, is that such "misplaced resentment" (it's a case of saying it!), which acts as **the** stubborn force at work behind attitudes, behaviors and acts of such a nature that they □ **would be said to be** driven by hatred or "causeless" animosity - that such a "grudge" is **nonetheless devoid of any feeling of hatred or animosity!** Indeed, it's the combination of these **two** aspects of "gratuitous violence" in the strongest sense of the term (the one I'm examining here) that makes it so disconcerting, as something truly "beyond comprehension"<sup>292</sup> (\*): the complete absence of any rational, tangible "cause" for this violence, both in the person who bears the brunt of it (without having provoked it by attitudes, behaviors or acts that are hurtful or prejudicial to the other), and in the person who carries it out (without being driven by any "cause").

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<sup>290</sup>See "Two turning points", n° 66.

<sup>291</sup>(\*) There are many such "reasons", which often mean that the person who (voluntarily or not) has caused damage or inflicted The most common, perhaps, especially when it comes to the mother or father, or someone considered beyond the reach of rank or social position, is the barrier of fear of violating a taboo. Perhaps the most common, especially when it's the mother or father, or someone considered out of bounds by rank or social position, is the fear of violating a long-standing, internalized taboo of authority. These are very strong barriers (in my case, they have tended to disappear over the last fifteen years or so, and increasingly so. . .). In the opposite direction, it can happen that the person in question is "not up to the task" of assuaging a grudge commensurate with the wrongs suffered - that he or she appears too insignificant, too evasive or pusillanimous perhaps, to be up to the role that would otherwise be rightfully his or her.

However, I can also imagine that in certain cases, the wrong suffered is too imponderable, too subtle (and even "non-existent", to say the least, according to the prevailing consensus, long internalized by the person concerned), to give rise to anything other than a diffuse resentment, unable to "condense" and take shape and strength in a relationship itself in soft tones, with no apparent angles. In fact, this is no doubt a simple variant of the previous case, which has emerged in the course of reflection.

with the note "Rancune en sursis - ou le retour des choses (2)" (n° 149).

<sup>292</sup>(\*) On the subject of this violence "beyond comprehension" ("unfassbar" in German), see the note "The slave and the puppet - or the floodgates" (n° 140). When I speak here of gratuitous violence "in the strongest sense of the term", without immediately qualifying it as anything other than "beyond comprehension", the precise meaning I have in mind is identified in the following explanation, by clarifying the "two aspects" that combine in it.

feelings of hatred or animosity that he might harbor, "rightly or wrongly", against his target).

Perhaps the question of the presence or absence of hatred or animosity, in the cases I'm dealing with (where we find ourselves confronted with violence that appears "gratuitous", as if unprovoked), is relatively incidental here, surely, as was the case for me, in the experience of the person who suffers this violence, and from the moment the violence suffered becomes conscious, there must appear an impression of "secret hatred" or "animosity" on the part of the person inflicting it. However, this impression is in no way the result of a perception (which would have suddenly appeared, as if by a wave of a magic wand), but rather that of a cookie-cutter **assimilation**: violence = hatred (or animosity)<sup>293</sup> (\*\*).

One thing that seems much more important to me, however, is to note not only **the existence** of something as seemingly aberrant, as demented, as contrary to the most inveterate "common sense" reflexes, as "grudge by proxy", displaced from its "original <sup>target</sup>□ (or targets) to p. 754 a "replacement target" (a target of pure convenience, almost!); but to note, **moreover**, that this is one **of the most common mechanisms**, encountered at every street corner, whether in one's own person (the last one you'd think of going after. . . ), or in that of one's family and friends. I even have the impression that this mechanism is **universal in nature**, that it's part of the basic mechanisms of the human psyche, that it's one of those few all-purpose mechanisms that make up the **syndrome of flight** from reality: the refusal to take cognizance of it, and the fear of assuming it.

More to the point, today I feel as if I've put my finger on the **mainspring common to all situations of "gratuitous violence"**, without exception. This impression emerged, with the force of a sudden conviction, when I began to examine (three paragraphs above) an "apparent contradiction". I then had the feeling that a whole host of fragmentary and heterogeneous impressions stored up over the course of my life, revolving around the "sensitive point" among them all of this violence "that surpasses understanding", were suddenly coming into order, suddenly acquiring a perspective that they still lacked - a perspective that appeared there unexpectedly, at the end of a thought, just as I was about to place a very last dot on a very last i. ...

#### 18.2.12.4. (d) Nichidatsu Fujii Guruji - or the sun and its planets

**Note** 160 (January 8) For the past week, there's been an unusual cold snap - temperatures of -15 and below, and when the wind blows from "Mont Ventoux" (the name says it all!), it must be even colder. It seems that this wave is sweeping the world (according to someone who listens to the news), and that in the south of France it hasn't happened since the famous winter and spring of 1956. When I was growing up in Germany, I experienced cold like that, but there was snow to protect the earth, and put a gentle tone in the air and on things. With this snow-free cold, the earth's surface is frozen like a block of ice. In just a few days, the garden has been raked - I don't know if there will be anything left in the spring from what we sowed and planted. The remaining leaves of leeks, celery, chard, lamb's lettuce, beet and chard are like sheets of ice, frozen vegetables. We hurry to harvest as much as we can

day by day, to eat it as we went along, before it thawed and everything went on the compost. And <sup>yesterday</sup>□the water supply had frozen in the kitchen, luckily there was still running water downstairs in the old p. 755

<sup>293</sup>(\*\*) (March 6) In some cases, however, there may well be a perception of a hatred that is actually present, even though it has in no way been provoked (see today's footnote on this subject). It's a hatred which, except in exceptional circumstances, remains confined to deep layers of the unconscious, and which moreover remains there in a state of "vacancy", without a designated target, even though it is the secret force driving acts of violence (most often in insidious form) which, in turn, do indeed and with unfailing constancy aim at the same chosen target. ...

garage, less exposed to the cold. Today a friend came over with a portable gas flashlight, and managed to get the water going again. I'll have to leave a trickle of water running so it doesn't refreeze so dry. Luckily I've got a good wood-burning stove in the dining room, where I've moved my work. I warm myself with vine stumps, which I break with an axe every day, a good grape crate full overboard in the cold weather. When the wind's been blowing all afternoon, it's enough to give you a cold sore, just standing there for a quarter of an hour, twenty minutes, breaking wood in the wind. Not to mention the fact that the car outside won't start - I've heard that cars don't stand the extreme cold very well, antifreeze or no antifreeze. The same complaisant friend got it running again earlier today, but will it still work tomorrow to proofread the typing of the secretary I gave the job to? In short, all it takes is a cold snap in winter, or a heat wave in summer, or a good little illness at any time, to remind us of some of the realities of life that we tend to forget when everything's humming along just fine...

Over the last three months, my work rhythm has gradually shifted towards the night hours. I work until around two or three in the morning, and sleep until around eleven or twelve. With the weather the way it is, if I listened to myself once in bed, I'd stay up for my easy twelve hours - and conversely, once at work, I'd never go to bed again! Right now, I'm trying to keep a reasonable balance. I don't worry too much about time shifts, as long as I get a good night's sleep, and don't lie in bed for hours on end with my thinking machine still running. Even now, when there's hardly any work to be done in the garden, there's still enough to keep me busy every day, including the firewood, and a little gymnastics here and there. I have the impression of a satisfying balance in my life, where: the work of discovery doesn't seem to devour everything else, but without being on the small side. Since I went back to work on September 22, I must be spending an average of five to six hours a day on it. It's modest, but the "output" seems hardly less than before. The "slaughter" (around a hundred pages a month) is about the same, give or take, as writing the first two parts of *Récoltes et Semailles*. But from a qualitative point of view, there's no doubt in my mind that this third part is the most profound, the one that has taught me the most about myself and others.

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□Na mu myo ho ren ge kyo!

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Just as I was finishing this short retrospective on the rigors of winter and the evolution of my life balance, I received a phone call from one of my Buddhist monk friends from the Nihonzan Myohoji group, announcing the death of their revered "preceptor"<sup>294</sup> (\*), Nichidatsu Fujii, better known as Fujii Guruji, or "Osshosama" to those close to him. My friend in Paris has just received the news in a phone call from Tokyo, and I assume that Fujii Guruji has died today<sup>295</sup> (\*\*). He had just turned one hundred on August 6, physically weakened but in excellent mental condition.

By a strange coincidence, August 6 is the anniversary of two other important events, one of historical significance, the other of a personal nature for me. It's the anniversary of the atomic bombing of Hiroshima (August 6, 1945) - which the Japanese commemorate as "Hiroshima day". (That's why

<sup>294</sup>(\*) "Preceptor" is more or less equivalent to "teacher", meaning "master" or "teacher". Nihonzan Myohoji is the phonetic transcription of the group's Japanese name, which translates as "Japanese Mission". This is a "missionary" Buddhist group, with a primarily pacifist vocation. See below for further details.

<sup>295</sup>(\*\*) As it turned out, he had only been dead for a few hours. The news spread fast!

Fujii Guruji's birthday festivities were held towards the end of July, to keep the days around August 6 available for pacifist and anti-atomic demonstrations). On the other hand, my father was born on August 6, 1890, six years to the day after Fujii Guruji's birth.

After the death of Claude Chevalley, Nichidatsu Fujii's is the second death of a person who played a significant role in my life, and it occurred during the writing of *Récoltes et Semailles*. In view of this passing (which doesn't really come as a surprise), I'm particularly pleased that just last year, there was a warm exchange of letters with him. I had been invited to attend the ceremony for the old Master's hundredth birthday, which was to be held with exceptional pomp in Tokyo (a small book of testimonials about him had even been hastily edited, to be presented to him for the occasion). This had been an opportunity for me to write (as I do almost every year), a few words of early congratulations, apologizing for not being able to attend the ceremony on July 30, as I myself was still more or less bedridden at the time of writing. (It's also true that I'm not

public ceremonies, but I didn't think it was worth mentioning it in this article.

my letter. In any case, I'd have disappointed and pained more than one of my monk friends, by abs- p . 757 stubbornly refused to attend any of the "grand occasions"<sup>296</sup> (\*), to which they never tired of inviting me). I had to add a few words about the beneficial side of illness, which forces us, in spite of ourselves, to "unplug" from our occupations and give the body what it craves. Fujii Guruji himself had been bedridden a lot during the past year, which must have weighed heavily on him, given his action-oriented temperament and uncommon energy. Although it had been over seven years since I had received any personal communication from Fujii Guruji, I was surprised to receive a letter from him, dictated by him while he was still bedridden. The letter (which I've just reread) is dated July 13, 1984. It's a letter full of delicacy, in which he worries about my health, and laments not being able to send someone to take care of me. He also talks about his health, and the mood in which he endures his forced inaction. He ends with these words, in very "Japanese" style, which should be taken with a (large!) grain of salt, and which showed me, perhaps even more than the rest of the letter, that my tone was as good as ever<sup>297</sup> (\*\*):

"Indeed I am a very old decrepit man of no use even if I may get back to normal life. Yet still, I would like to live and see how the world turns."

There he was able to watch the world go round again for almost six months. ...

My links with the Nihonzan Myohoji group go back to 1974. There's no way I can even begin to sketch out these many and varied relationships here - I'd need a whole book to do that.

volume. They are among the richest "spin-offs" from the "Survive and Live" episode<sup>298</sup> (\*\*\*) which followed my p . 758

<sup>296</sup>(\*) Chief among such "grand occasions" was the inauguration of "Shanti stoupas", or "Pagodas of Peace". The construction of these Pagodas, or places of meditation for world peace, goes back to a very ancient tradition in the Buddhist world (initiated by King Ashoka in India), and was one of Fujii Guruji's main preoccupations. It has inspired the construction of a large number of Shanti Stoupas around the world, including three in Europe and one in the USA.

<sup>297</sup>(\*\*) The letter was dictated in Japanese (the only language Guruji spoke) and translated directly into English. French translation of the lines quoted: "Certainly I am a very old and decrepit man and of no use even if I can regain normal health. And yet, I'd like to live and see how the world turns out."

<sup>298</sup>(\*\*\*) This episode is alluded to several times in "Fatuité et Renouveau" (the first part of *Récoltes et Semailles*). *Survivre et Vivre* (first called "Survivre", then "Survival") is the name of a group, initially pacific, then also ecological, which originated in July 1970 (on the bangs of a "Summer School" at the Université de Montréal), in a milieu of scientists (and above all, mathematicians). It rapidly evolved towards a "cultural revolution" direction, while broadening its audience outside scientific circles. Its main means of action was the bulletin (more or less periodical) of the same name, whose successive directors have been Claude Chevalley, myself, Pierre Samuel, Denis Guedj (all four mathematicians) - not to mention an English-language edition, maintained at arm's length by Gordon Edwards (a young Canadian mathematician whom I had met in Montreal and who was among the few initiators of the group and the bulletin).

departure (between 1970 and the end of 1972). There had been mention of this group, and of the (not very periodical!) bulletin of the same name, and also of my "departure from maths" and my "trajectory", in a Japanese newspaper (or newspapers?), in 1972 or 73. The "criticism of science" and denunciation of the military apparatus, and also, perhaps, the "criticism of a civilization" aspect, must have "passed" in some article, attracting the attention of one of the monks at Nihonzan Myohoji. He told others about it, and in particular a younger monk from the same town (Kagoshima), who had become a monk under his influence and was something of a "pupil". This

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was the first missionary monk of the group to land in the "West", in Paris to be precise, in the spring of 1974<sup>299</sup> (\*). He came to find me a few weeks later and unannounced, in the remote village where I was living about fifty kilometers from Montpellier. Since that memorable day in May, when I saw, under the midday sun, a strangely dressed man, singing on the road to the accompaniment of a drum and heading (there was no mistake. . . ) towards the garden where I was working alone - since that day I've had the privilege and pleasure of seeing many Gurujī devotees and<sup>300</sup> (\*) sympathizers pass through my house. Their contact has been of great benefit to me. At the beginning of November 1976, I even had the honour and joy of welcoming Fujii Gurujī himself, then aged 92, to my rustic home, accompanied by a group of seven or eight monks, nuns and disciples. I had already met him the previous year, at the solemn inauguration of the group's temple in the eighteenth arrondissement of Paris. Over and above the *de rigueur* words of courtesy, there was then a strong contact, an immediate sympathy. The more intimate and personal context of a several-day visit to my home gave me, of course, a much richer apprehension both of Fujii Gurujī as a person, and of his relationship to the group of which he was the head, and the soul.

Interestingly, this visit from Fujii Gurujī followed very closely, by just two weeks, the crucial turning point in my life that took place between October 15 and 18 of the same year, mentioned elsewhere<sup>301</sup> (\*\*). The weeks that followed those days of crisis and renewal were among the most intense of my life, with each day bringing its own unexpected harvest of inner events and discoveries. To tell the truth, this visit, planned and prepared for weeks, of a whole group of monks and nuns around their revered master, seemed to come there as a kind of strange interlude, like a diversion in the adventure that was then absorbing the totality of my being. It was respect for my hosts, and in particular for Fujii Gurujī who had come to honour my home, that enabled me to remain there for these few days,

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The first bulletin, written entirely by me (naive and full of conviction!) and printed in a thousand copies, was distributed at the International Congress in Nice (1970), which brought together (as it does every four years) several thousand mathematicians. I was expecting a massive turnout - there were (if I remember correctly) two or three. Most of all, I sensed great embarrassment among my colleagues! When I talked about the collaboration of scientists with the military, which had infiltrated scientific life from all sides, I was really putting my foot in it... It was in the "big scientific world" that I felt the greatest discomfort - the echoes of sympathy coming from there were reduced to those of Chevalley and Samuel. It was in what I have elsewhere called the "swamp" of the scientific world that our action found a certain resonance. The bulletin ended up with a print run of some 15,000 copies - an insane amount of housekeeping, by the way, when distribution was done by hand. Didier Savard's juicy drawings undoubtedly contributed greatly to our canard's relative success.

After my departure and that of Samuel, things turned into a leftist groupuscule, with sharp jargon and unanswerable analyses, and the bulletin died a natural death. What had been to be understood and said, at a certain point close to the effervescence of 1968, had been understood and said. After that, there was little point in spinning a record over and over again. ...

<sup>299</sup>(\*) He did assure me that he was the first Buddhist missionary monk in the West, in the history of Buddhism - but I can't guarantee that this information is reliable! It's not clear that becoming a missionary was really a great "step forward" for Buddhism. Right from the start, this aspect of the Nihonzan Myohoji group aroused reservations in me, which have only grown stronger over the years.

<sup>300</sup>(\*) It was precisely one of these who had the honor, as an "illegal alien", of being the occasion for the first literal application, in French jurisprudence, of a certain rather incredible article of a certain "Ordinance of 1949". I had the honor of finding myself in the Correctionnelle, for having "lodged and housed" such an outlaw free of charge. See about this episode in the section "My farewells - or strangers" (n° 24).

<sup>301</sup>(\*\*) See the section "Desire and meditation" (n° 36) and the note "Les retrouvailles (le réveil du yin (1))" (n° 109).

the availability that the occasion demanded. As has often happened to me, it was only once I got to the heart of the event that I realized that it was in no way an "interlude" or a "diversion", but rather an opportunity for me to take part.

□ that it was part of the adventure I was living. Underneath its very "tales from the Orient" exterior, a perfect delicacy and unusual charm, this so-called "interlude" brought me into the presence of men and women like myself and the men and women I'd always known, in less exotic, less extraordinary contexts. It was because I sensed this kinship that I also felt that my hosts were friends and brothers, and not characters straight out of a tale of a thousand and one nights, as must have been the case for many of the astonished villagers. And Fujii Guruji himself, who spoke so familiarly to me while his "relatives" remained at the proper distance demanded by the respect due to the revered master, I felt very, very distant (from me as well as from his relatives), and yet close at the same time, as if he had been my father, or a benevolent elder brother.

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And as is not uncommon with even the most benevolent of fathers or older brothers, he had an expectation of me, one he made no secret of, an expectation shared by those accompanying him, all of whom were my guests. And I also knew I couldn't meet it. My adventure was linked to Fujii Guruji's, by links I could only dimly discern, perhaps deeper than I could see, and to those of his disciples who followed him with their eyes closed. But it was no more that of my prestigious and benevolent host, than it was that of my father, also prestigious to me and benevolent, very close and yet different: another person, another destiny.

It wasn't easy to "get over" the fact that I wouldn't be one of them in an undertaking that was theirs, and that I didn't feel was mine. According to the picture of me that Fujii Guruji and his followers had been given, this was the last thing they would have expected - and all the more so as the relationship on a personal level, between the group or the various members of the group and myself, was a veritable honeymoon. It was during this visit, too, that some long-standing resistance, due to my upbringing, vanished, and I joined my hosts in chanting their mantra with them, accompanied by drumming:

"Na mu myo ho ren ge kyo!"

This mantra is the foundation, the alpha and omega, of their religious practice. They sing it most often accompanied by the prayer drum, one hour in the morning and one hour in the evening. Following the teachings of the Japanese prophet Nichiren, this drum chanting is in itself the sovereign good, the dispenser of peace in the world.

the one who sings it and around him. This song is therefore for my □ Japanese friends what is commonly called a "prayer". The meaning they give it, in agreement with Nichiren, and with their direct "preceptor" Fujii Guruji, is that of an **act of respect** for the person addressed, and through him, for every living being in the universe - as a being promised (according to the Lotus Flower Sutra) to become Buddha, incarnation of perfect wisdom. These seven syllables also serve as a greeting for any other person, or even for any other being we wish to greet, with this connotation of respect for what is of divine essence in the other. They also serve as a thanksgiving before the meal. To tell the truth, it seems to me that there is hardly an occasion, whether in moments of surprise, emotion or contemplation, that is not conducive for a Nichiren follower to say the sacred words. As for me, without sharing the religious beliefs of my monk friends<sup>302</sup> (\*), it's with joy that I join them, when the occasion arises, to do Odaimoku - to sing on the drum what they call "the Prayer". It's in their memory, and as an act of affectionate respect

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<sup>302</sup>(\*) I don't feel I belong to any particular religious denomination. Through my parents' upbringing, I was an atheist (with an anti-religious undertone) until the age of fourteen. A remarkable lecture by my natural science teacher, on the history of the evolution of life on earth, made me understand, without the slightest possibility of doubt, the presence of a creative intelligence at work in the Universe. This understanding, which at the time remained at the level of the intellect alone, broadened and became clearer in the course of my subsequent maturation, continuing after my departure from the mathematical

## 2. A walk through a work or the Child and Mother

scene in 1970.



to their master, Nichidatsu Fujii Gurugi, that I have also included "the Prayer" in my daily life, chanting it before each of the two main meals of the day, at least when I'm at home, or with friends, or with people I know won't mind<sup>303</sup> (\*\*). This is one of the most valuable things I owe to Fujii Gurugi and to those of his disciples whom I have known and who have given me their affection, without tiring of my reluctance to associate myself in any way with their missionary activities.

There are several million Nichirenite Buddhists in Japan, divided into a number of different sects. very different physiognomies. The Nihonzan Myohoji group is one of the smallest in terms of numbers, comprising just a few hundred active monks, nuns and sympathizers. Yet it is well known in Japan and elsewhere,

distinguishing itself from all traditional religious groups by an unequivocal political commitment, including the main emphasis is on the struggle for peace, anti-militarism and, in particular, anti-nuclear action. At the time of the Vietnam War, it was the only Buddhist group (unless I'm mistaken) to take a clear stand against the Americans, and to fight against the presence of American bases in Japan (which served as logistical support for the continuation of the war in Vietnam). In recent years, Fujii Gurugi has also been in close contact with the leaders of the American Indian Movement (AIM). Monks from Nihonzan Myohoji have taken part in marches organized by American Indians, not to mention other Peace Marches in various parts of the world. Indian leaders were visibly attracted and impressed by Fujii Gurugi's unusual personality. The fact that this man of indomitable energy, approaching a hundred years of age, was a great missionary of a religious faith different from their own, didn't seem to bother them at all. On the contrary, the religious dimension of the venerable Master's zinc-spiked "anti-American" options was surely, in addition to his age, one of the reasons why they welcomed Gurugi as they would have welcomed one of their own, like a highly respected father or grandfather in whom they recognized themselves<sup>304</sup> (\*).

I'm sure that this religious dimension played a similar role for me - it brought Fujii Gurugi closer to me, even though I don't claim to belong to any particular religious faith. If I ask myself what attracted and struck me most about him, I see several things. The most obvious is an inner **joy**. This joy seems to flow spontaneously from a **unity** within him, or rather, perhaps, from a **fidelity** to himself. One senses that this man is happy, because all his life he has done without hesitation what he felt he had to do. He never appears to me not free of contradictions, but devoid of ambiguity. The meaning of some of his acts or omissions escapes me, but at no time has it occurred to me to doubt the man's total integrity. If he This is not the result of an analysis of what I know about him through other people. You only need to have met him once to know that he is a man who knows no ambiguity, a man in deep accord with himself. This is what the Indian chiefs of the AIM must have sensed, to give him the place they have made for him among them. This is surely where his extraordinary ascendancy lies over those who claim to be him, men and women whose ideological and philosophical options cover a spectrum ranging from hard-line Marxism-Leninism to the good-natured conformism of a chain's CEO.

<sup>303</sup>(\*\*) I refrained in particular from singing the prayer at the weekly meal I had at the Faculty, in the company of a few students or colleagues, not being sure that one or other of them would not feel some kind of constraint, which I would impose on him thanks to my position as elder or "boss".

<sup>304</sup>(\*) To give an idea of the bond of trust and respect linking the Indian chiefs to the person of Gurugi, I'd like to point out here that during the great annual initiation festival, held around the "sun dance", Gurugi's monk disciples took part, beating the great prayer drum from sunrise to sunset, to the throbbing rhythm of *Na mu myo ho ren ge kyo!* These large drums, hollowed out of a single trunk and stretched with ox skins, are unusually powerful, and (I presume) hard to bear for twelve hours on end. (I experimented with this for two hours, during the inauguration of the temple in Paris, an experiment that was conclusive. ...). ) In any case, Robert Jaulin (who, along with the monks, was among the few non-Indians invited to take part in the festival) told me that the Indians stoically endured Grandfather Gurugi's sacred drum from the beginning to the end of the initiation, of which the Gurugi tam-tam was one of the many trials. ...

department stores. What unites them is not the veneration of a certain Sutra, which perhaps none of them has had the effrontery to read<sup>305</sup> (\*), nor a certain prayer of Pali origin, restored to Japanese via Chinese translation, and which professes veneration of this Sutra. What brings them together (or should we say, what had brought them together?) is one **man**, exerting over them an ascendancy he no more sought to exert, than the sun sought its planets.

I also saw that this man was **alone**, and that solitude didn't weigh heavily on him. It was his natural condition, perhaps always had been. This solitude, and this integrity, or accord with himself, appear to me as so many different aspects of one and the same thing. Yet another aspect of the same thing is that of **strength** - strength without violence, and which doesn't care about being or appearing "strong". It's the sun's force, again, which suffices to be itself in order to create this field of forces around it, and the orbits that the planets travel in.

Surely, this is also the force I spoke of more than once in Harvest and Sowing, as "**the** force" in us - with the difference that in one man it is fully apparent and sensitive to all who approach him, and in another it is buried more or less deeply, to the point where sometimes we could even "see" it. believe it to be non-existent. But if some of my monk friends seem to deny it in themselves, this Sutra nevertheless they profess to venerate, and the very prayer they chant day after day, clearly proclaim that a p. 764 such a force lives in every living thing in Creation, promised like them, and like their revered master Osshosama himself, to the Buddha's destiny.

### 18.2.12.5. (e) Prayer and conflict

**Note** 161 (January 13)<sup>306</sup> (\*) It's been another four days since I've had the peace and quiet to work.

- to continue the notes, I mean. The main reason lies in the rather incredible difficulties I'm having in getting this third part of Récoltes et Semailles typed out on the net. In the thirty-plus years I've been in the habit of getting typing work done, I've never experienced anything like it. Clearly, having this highly personal, not to say intimate, text in my hands triggered (surely unconscious) reactions of considerable force in the people in charge of typesetting, each time going in the direction of a veritable sabotage of the work entrusted to them. In the space of a few months, the same scenario was repeated three times in a row, with some variations, with three secretaries in a row, all of whom had not given each other the word<sup>307</sup> ! This third time, moreover, a sordid note was added, as the secretary, Mme J., pretended to use the rather unusual manuscript that had been entrusted to her care,

<sup>305</sup>(\*) More than one of Guruji's disciples has made it clear to me that he would consider it an overstretch to pretend to read the Lotus Flower Sutra, even though a Japanese translation exists. Only a man of great depth of spirit, such as his master Fujii Guruji himself, would be able and worthy to read this sacred text, which is infinitely beyond the intelligence of the layman. Clearly, the faith of these men and women is not in some more or less deified historical figure, such as Buddha, or the perfect Bodhisattva and prophet Nichiren, but in Fujii Guruji himself.

<sup>306</sup>(\*) (January 23) The whole first part of this note was written against strong resistance to mentioning the disturbances interfering with my work. These took on a vaguely ridiculous figure, and to even mention them was a bit like graciously providing the rods to get beaten! On the other hand, these disturbances, "which can literally saw you off", had become so grating and invasive in my work, especially during a week or two, that it would have been a kind of cheating, an inauthenticity in the testimony, to pass them over in silence as if nothing had happened. I come back to my setbacks ten days later, in the note "Jung - or the cycle of 'evil' and 'good'".

(March 7) This last note, the first of a whole series of "reading notes" on C.G. Jung's autobiography, was finally discarded in a final section of Harvest and Sowing, made up of the part of the reflection prompted by this autobiography.

<sup>307</sup>(\*\*) Those who wish me well will have no trouble accusing me of delusions of persecution - after the brotherhood of movers, here comes the brotherhood of typists-secretaries who are mobilizing to do me harm! See, for precedents, the note "Le massacre" (the name of the note already says enough about me... ) p. 538, about my friend Ionel Bucur's move... .

as a means of blackmail to extort some sort of ransom. She is a former executive secretary, with a great ☐ habitude of the trade. The first eleven pages of typing were impeccable and without a single mistake and in the next fifteen pages alone, there were eleven lines skipped - I've rarely seen a text crippled so badly! I didn't ask what the ransom was (over and above the price agreed for the text already typed) to get my manuscript and the typing back, as I have no desire to encourage this kind of procedure. This means I'll probably have to resort to legal action.

Fortunately, I still have a draft of the manuscript, which I can use if need be. Don't forget that this kind of circus, especially when it becomes repetitive, can literally "saw you off". When I imagined the difficulties and antagonisms that my modest meditative and autobiographical paving stone would undoubtedly raise, I certainly didn't imagine that it would be from this side, from the brotherhood of secretary-typists (instead of that of my honored mathematician colleagues) that the first trouble would come, and in the nature of a sort of war of attrition! Now I'm not so keen on entrusting this same text (once recovered) to the hands of a fourth secretary, when there's nothing to suggest that she'll have any more commiseration for it than those she'll be taking over from. And doing the secretary's job myself would require a time investment of well over a month, which I'm absolutely not willing to provide.

Perhaps I'll have to forego a typesetting of this third part of *Récoltes et Semailles*, which I'll entrust directly to the publisher in the form of a rough manuscript. (I don't anticipate the same kind of trouble with the protes responsible for typesetting the text for printing!) This would mean, above all, that I would forgo including this third part in the limited pre-edition of *Récoltes et Semailles* to be produced by my university, USTL, for personal distribution among colleagues and friends. Or maybe I'll have it printed later, if I end up finding a secretary who does a decent job. I'll only send out this part (surely the most "difficult" of the three) at the express request of those really interested in receiving it, among those who will have received the first two parts. I'm really looking forward to getting these printed and sent off (although I'm in less of a hurry for the third part). The typing of these two parts was completed months ago, and had been handled (without any problems) by the USTL secretaries. They could have been printed a long time ago, if I hadn't wanted to include

a table of contents of all three parts of *Récoltes et Semailles*, when for more than three months I think ☐ I've been on the verge of finishing this interminable third part. Now I'm going to give myself until the end of this month to finish, or if not, to take care of the printing of the first two parts (Fatuity and Renewal, and Burial I, or the robe of the Emperor of China), without including a complete and definitive table of contents of the third part (Burial II, or the key to yin and yang).

And now, after all these unpleasant incidents, I have to find my way back to a train of thought that was cut short.

The death of Fujii Guruji in his one hundred-and-first year, on January 9th, was an opportunity to evoke, with him, an aspect of my life that I hadn't touched on before. Unable to see Guruji again on his deathbed, and to take part in a wake with his loved ones, I spent the night after his death in a solitary vigil, jotting down until morning some of the reminiscences and thoughts prompted by the event. In retrospect, I thought it would be a good idea for me to try, on this occasion too, to say what the encounter with Fujii Guruji, and with those of his disciples with whom I had a close acquaintance, had brought me.

In the notes from five days ago, I already mentioned the Na mu myo ho ren ge kyo chant, which for many years has entered my life, and which is a blessing. There's also the affection received by Fujii Guruji himself, and

by many of his disciples, young and old. It is this affection, surely, that gives its price and beauty to the song I received from them, which is itself an act of respect and affection for all living things in creation, including them and myself.

Also, my contacts with the monks and nuns of Nihonzan Myohoji were my first and only close contacts with men and women whose main, if not total, investment is in religiously-motivated tasks (just as for a long time my own investment was in mathematical discovery work). This was an opportunity for me to realize that, as elsewhere, beyond a certain affinity with a common (so-called religious) vocation and allegiance to the same strong and engaging personality, differences in temperament, conditioning and even profound **choices** remain just as marked, and just as active in person-to-person relationships. To put it another way,

the efforts of some people to **model** themselves after some religious ideal (in this case, that of the "Boddhisatva", the infati-

gable propagator of the Buddha's teachings) débouchent sur des **attitudes** plus ou moins à fleur de p . 767

skin, not on a process of inner **transformation** or maturation. On the other hand, the adoption of a "creed" (however sublime it may be) and investment in a so-called "religious" activity, seems to have no essential impact on the play of habitual egotic mechanisms. Conflict is no less present in monasteries, convents, temples and other religious communities of all denominations than anywhere else in the world. And often the religious vocation is taken as one means, among others, of evacuating conflict, by convincing oneself that it has disappeared by virtue of the creed.

It's also true that, on different occasions, in one of my monk guests there was an inner peace and joy that radiated from him, sensitive to me as to all who approached them, and beneficial to themselves as to all. Clearly, such a state of harmony and wholeness, of profound accord, is alien to any effort to be this or that - it is an "effortless" state, a state of perfect naturalness.

For four of the monks in whom I sensed such radiance, I have the impression that this has been their customary state for many years, even decades. This is particularly true of Fujii Guruji himself. For two other friends of mine, I've seen them on other occasions as knotted and as torn as anyone else. It was as if that state of harmony in which I had known them, and a certain spontaneous understanding of things that was one of the signs of it, had become null and void - as if they had left no trace of themselves. I'm convinced, however, that there is an indestructible "trace", deeper than a simple mark recorded in memory - a trace in the nature of **knowledge**. Like everyone else, these friends are free at any time to take account of the knowledge deposited in them at the creative moments of their existence, to let it act and bear fruit; just as they are also free to ignore it, to bury it, to "play dumb" in short. This is, after all, the most common thing in the world... .

The thought occurred to me that this state of perfect naturalness, of profound agreement with oneself, and the radiance that accompanies it, are **not** very common things, on the other hand. It's quite remarkable that in the rather small group of monks I've been able to welcome into my home, be it for a few days or a few weeks, there have been so many in whom I've found this state of inner harmony, of strength at the core of their being.

She's the one who unites humility and fortitude, the gentle and the incisive. Wouldn't that be

end of committee, well and truly the action of a creed, or of the Prayer that expresses it? This one' □ if obviously not

p . 768

can on its own create a state of grace, perhaps it tends to **encourage** the emergence of such a state, and its renewal day after day? After all, the very act of singing a beautiful song and putting our whole selves into it is already to some extent a "state of grace" - and the very beauty of a song (or a prayer) already encourages us to "put our whole selves into it".

It's also true that the most beautiful of songs, when we repeat it with our minds elsewhere, remains inactive, for want of

it's up to us to open up to it. Or to put it another way, what we play back **is not** the song we think it is, and our soul doesn't feed on it, any more than a paper or plastic rose is a rose, and a bee would come and pick it.

#### 18.2.12.6. (f) Belief and knowledge

**Note** 162 (January 14) As I concluded the reflection of a week ago, I had the feeling that I had "put the doigt" on something important. That very night, I wanted to lapidarily express this "something" in the name given to this note, "The cause of causeless violence" (note n° 159). I also knew that this sudden flash of understanding was in no way the culmination, or even the final point, of a reflection that for more than a month<sup>308</sup> (\*) had been revolving around the mystery of "causeless violence", or "gratuitous violence". On the contrary, the new "perspective" that suddenly appeared was rather like a new point of departure. The mechanism of the "displacement" of resentment or resentment for wrongs and damages suffered in earlier days, to an **acceptable** "target" in place of the real culprit(s), felt to be out of reach or "taboo" - this mechanism, which I had first recognized sporadically, in this and that isolated case in the course of my life, and tacitly taken for some kind of strange, erratic aberration of the unconscious, is at last recognized as one of the "basic mechanisms of the human psyche". At the same time, it appears to be responsible for the innumerable and disturbing manifestations of "causeless violence", whether between wife and husband, lover and lover, parent and child, or the "anonymous" violence that reaches its climax in times of war or large-scale violence.

social convulsions.

p. 769

□ I don't know whether these links have long since become part of the B.A.BA of psychological science or psy-

chiatric science (assuming such a "science" exists), or whether what I say here will come across as the phantasmogoria of a "psychoanalytic dilettante". As my aim is not to present a doctoral thesis in psychology, nor even to break new ground for some old or new theory, but to understand my life through the situations in which I am involved, I don't care about the "status" of what I happen to put my finger on, or the "perspectives" I suddenly see opening up here and there. I'm well aware that, in any case, if I want to understand anything at all, I can't do without personal reflection, be it in mathematics, or in my life and in those to which my life is linked in one way or another. All the more so, when what we're trying to understand seems to defy reason from the outset, and when I see everyone around me and elsewhere evading it like the plague, with reassuring clichés. (And it seems to me that psychology professionals are no more an exception than anyone else, at least when it comes to themselves).

I was well aware that the "sudden conviction" that came with "the last dot on the last i", namely that "I'd put my finger on the spring common to all situations of 'gratuitous violence'", in no way absolved me from the task of examining on the spot, and from every angle, this new intuition that had arrived in the field of conscious gaze, not yet free of the diffuse halo of what had just emerged from the mists. On the contrary, this was precisely the first job to be done, and I could already see a host of new questions arising, both specific to particular cases and general. If there was any certainty in this cookie-cutter "conviction", or to put it more accurately, a kernel of certain knowledge, it in no way told me that the formulation I'd just given to this conviction was "true", "correct", without any major reservations or alterations perhaps; but rather, that I had indeed put my finger on a **new** (for me) and **essential fact, that a new perspective** on violence had indeed just emerged.

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<sup>308</sup>(\*) Specifically, since the December 7 memo "Velvet paw - or smiles" (n° 137).

<sup>309</sup> (\*). As for the precise and nuanced meaning of this new fact and this new perspective, its exact scope and also, perhaps, its unforeseen extensions and repercussions, they cannot fail to emerge, as soon as I invest the necessary work in them. The "knowledge" that had just appeared told me, in particular, that the time was ripe for such work, for entering further into an understanding of violence, and in any case, into that of "gratuitous violence"; that every hour and every day that I would devote to this task, to follow through on what had just appeared, would take me further into this understanding. I don't recall that such a feeling of the appearance of something new and essential (even though it would still remain diffuse and approximate), and the intimate conviction of being able to penetrate further into the understanding of this thing, ever deceived me. If there has been a sure guide in my research to "place" my investments in this direction or that, it is the feeling of the appearance of the **new**, and this intimate conviction that tells me when the time is ripe to enter further into this "new" glimpsed and to know it<sup>310</sup> (\*).

□ This doesn't mean that, whenever the time is ripe to launch myself in such and such a direction, and to to know such things, I'm going for it! It was impossible even when I was investing all my energy in mathematics, when I gradually found myself with ten irons, then a hundred at a time in the fire!<sup>311</sup> (\*) And it's been the same in meditation, that is, in self-discovery. At the level of conscious work, we can, alas, only do one thing at a time (which isn't bad, however, when you take the trouble to do it right. . .). This work on **one** of the "hundred irons in the fire" can, it's true, in the mysterious ways of the unconscious, also benefit all the others, or at least several of them - it can "warm them up", make them more receptive to hammer blows on the anvil of conscious attention, from the moment we turn our attention to them. But we need to know how to choose "the right" iron from among the hundred - the one whose shaping will also advance the work on others, which are in the process of heating up like it.

p. 771

<sup>309</sup> (\*) As I was writing these lines, the comparison with the "standard conjectures" on algebraic cycles, which I presented at the Bombay symposium in 1968, came to mind. They seemed to me then (and still do today) to be, along with the resolution of singularities, one of the most burning issues in algebraic geometry. As I worked out these conjectures, I sensed that a "new perspective. . . had just been established", this time on algebraic cycles, their relation to Hodge's theory and Weil's conjectures. What struck me most was that I could see the beginnings of an approach to Weil's conjectures that would be "purely geometric", by which I mean, without having (at least in appearance) to go through the medium of a cohomological theory.

As I have already pointed out elsewhere (in sub-note n° 106<sub>1</sub> of the note "Muscle and tripe"), the reality of this "new perspective" and its scope, is entirely independent of the question (which remains in the limbo of the future) whether this conjecture will be

true or false. A conjecture, for me, is not a **bet** (that you win or lose), but a **guess**.

- and whatever the answer, we can only come out "winners", by which I mean: with renewed knowledge. (Compare with the reflection in the section "Error and discovery", n° 2.) Assuming that the conjecture turns out to be wrong, I can already see two or three "less optimistic" variants, which from then on affirm it, the weakest of which is practically equivalent to the existence of a "reasonable" theory of semisimple patterns over a body.

Identifying these variants, for someone with a bit of experience, is an exercise lasting an afternoon or two (and perhaps the starting point for a long journey into the unknown. . .). Unpacking the first statement (inspired, as usual, by an idea of Serre's, set out in his article "Analogues kählériens des conjectures de Weil"), was not an exercise, but indeed a **discovery**; or again (to use the expression from Zoghman Mebkhout's letter, quoted in the note "Echec

d'un enseignement - ou création et fatuité", n° 44') a **creation**. And it was an understatement when Zoghman shyly ventured to say that "my students don't know very well what a creation is" - or rather, I'd say: they did, but have long since forgotten this, busy as they have been pushing the wheels of a funeral wagon... ..

<sup>310</sup> (\*) Compare with the note "The child and the sea - or faith and doubt", n° 103.

<sup>311</sup> (\*) See note "A hundred irons in the fire, or: nothing's worth drying out!", n° 32.

### 18.2.12.7. (g) The hottest iron - or the turning one

**Note** 162 In the course of reflecting on the Burial, I came across many "irons" that asked me to work on them, more or less hot depending on the case. It seems to me that they all warmed up in the course of the work, some more, some less. The very first of these "irons" was the question of **self-contempt** in the case of my own person, first posed as a matter of conscience, on the bangs of the first embryo of Récoltes et Semailles<sup>312</sup> (\*\*). It remained rather tepid, until the reflection of December 13 (a month and a day ago), in the note "La violence du juste - ou le défolement" (n° 141). It was the first time in my life, I believe, that I had devoted a reflection, however brief, to the few cases in my life where I myself have exercised and caused to be exercised "violence without cause", violence "beyond comprehension". I've thought about it from time to time in recent years, but always in passing, without dwelling on it, and above all: without devoting any written thought to it.

p. 772 Yet violence-which-doesn't-say-its-name had profoundly shaped my life - it was one of the crucial things, if not **the** crucial thing <sup>between</sup> ~~all~~ of them, that I needed to understand as deeply as I could. could, to understand my life, and "life" in general, human life. But the fact that this is the case, which is obvious as soon as I take the trouble to think about it, had remained hidden. It finally emerged, as if by chance, on the bangs of the reflexion in the days leading up to that of December 13, continued in the set of four notes brought together under the name "La griffe dans le velours" (n° s 133-136). It is in these notes that, for the first time in Récoltes et Semailles, "**violence**" is named, and becomes the object of attention. It has remained the focus of attention until now, or at least until the note of January 7 (a week ago), "The cause of causeless violence".

This promising title may give the impression that this latest note is a sort of culmination of the re-flection on violence that has been ongoing throughout the past month. And it's true that it is one of the main fruits of that reflection. However, I am well aware that if this new perspective suddenly appeared, and this feeling of intimate conviction concerning a certain link suddenly glimpsed, it was because **my own person** was also directly involved in what had just appeared, among this "crowd of fragmentary and heterogeneous impressions stored up over the course of my life". The last and freshest of these impressions, felt at the time to be very "fragmentary" and indeed insufficient, was precisely that reflection of December 13th on **violence within myself**. This reflection, which to the superficial reader may seem like a digression among many others in the investigation of the Burial, appears to me now, with hindsight, as a pivotal moment and a crucial turning point (in potential at least) in my self-reflection. The very same day, moreover, I felt that I had at last taken the first step in a direction I'd been avoiding until then, and which would lead me straight to the heart of the conflict within myself. This "lukewarm iron", which had been lying there as if for memory for ten months already, was suddenly red-hot - all I had to do was stop and blow and strike, for it to turn red-white and reveal a shape and a message to me. And so it remains today.

Clearly, however, this is not the place to discuss this iron. Of all those appearing in Harvesting and the Semailles is certainly the one that burns the brightest for me, and after it, the one that closely followed "La cause de la violence sans cause", if the child didn't have a terribly adult, stubborn boss on his back. <sup>p. 773</sup> rivé à des tâches de longue haleine et aux "priorités" qu'elles imposent, c'est dans cette direction assurément, leading me to the heart of the conflict within myself and others, that I would now launch into, without having to probe myself! But as the name suggests, it's usually the boss, not the child, who orders and decides on investments. The "enigma of evil" will therefore wait for the more propitious moment when the boss would be able to make the investment.

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<sup>312</sup>(\*\*) See note (n° 2) referring to the June 1983 section "Infallibility (of others) and contempt (of self)" (n° 4).



when he's on vacation (a rare occurrence), or when he's not too busy with other pressing "priorities", such as finally completing the writing of *Récoltes et Semailles*!

### 18.2.12.8. (h) The endless chain - or handover (3)

**Note 162**” But before returning to the Burial, I'd like to note at least one of the associations of ideas aroused by the reflection of a week ago - one, perhaps, less obvious than others, and which for that reason risks vanishing without trace if I don't note it now. It's linked to the Hindu idea of karma, and goes in the same direction as the association that appeared in the note "Le Frère ennemi - ou la passation" (n° 156): in the sense of the tenuous intuition of a kind of "**law of conservation of karma**".

This original, diffuse resentment in a person, which later translates into seemingly "gratuitous" impulses of aggression and violence, does not arise from nothing. It is the response to deep-rooted aggressions that were indeed experienced, especially those suffered in early childhood. It's true that many of these repressive assaults are not "acts of violence" in the strict sense of the term, i.e., they stem from an intention to injure or harm, particularly on the part of parents towards their child. It's also true that such intent (almost always unconscious) is present in many more cases than is generally accepted. But perhaps, from the point of view of the creation or transmission of karma, the question of **intentions** or **motivations** (overt or covert) is secondary, when "violence" is indeed taking place, inflicting "harm", causing "damage". I couldn't say.

Still, in most cases, a superficial look can give the illusion that the "harm" suffered is null and void, that it has been cashed in, and that once cashed in, it has "disappeared" without a trace. And this is a fact that it's not very often that those who have sown their children with anguish and powerlessness to be themselves, end up reaping <sup>directly</sup> ~~indirectly~~ <sup>resentment</sup>, at the hands of these same children, what they once sown; or at least, one has the impression that they're only reaping a tiny fraction of it! Or to put it another way, of the diffuse resentment they have aroused in their children, only a tiny portion condenses into a "hard" resentment, directed at them - and which they complain about loud and clear, as if it were the blackest of ingratisudes, it's a given! But the rest of this resentment or accumulated "karma" is not lost for all that. It finds effective use, in a way that may seem inexplicable, through the mechanism of "displacement" of resentment towards makeshift targets; sometimes erratic targets, sometimes specially matched targets, pampered so to speak, nurtured over a long lifetime!

In ordinary times, this intense work of karma, like an abscess deeply implanted in human life, takes place in the shadows, and everyone makes a point of ignoring it, of agreeing to see it only as an occasional "burr" here and another there, in relation to what is considered normal and proper.

It's in times of exception, when war or misery are raging (or in places of exception, like penitentiaries and asylums), that this underground work erupts and spreads out freely in the full light of day, in a frenzied blaze of contempt and murderous madness, exalted by grandiloquent flags over heroic mass graves and cold, naked cities... .

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2. A walk through a work or the Child and Mother

**Part four.**

**BURIAL (III)  
or the Four Operations**



# The funeral ceremony (continued)

## 18.3. Final homework (or visit)

### 18.3.1. (1) Duty done - or the moment of truth

**Note** 163 □ (February 16) Today is exactly a month since I began the impromptu reflection, triggered by reading C.G. Jung's autobiography. I thought I'd spend a few days there, putting down on paper the first strong impressions I'd gained from reading it - and today I'm still working through them! They were enriched and transformed in the course of reading, by virtue of the work triggered by it and by the writing of my reading notes. I've just had time to go through the first four chapters on Jung's early years - the chapters written in Jung's own hand. I was about to confront these impressions with others, not always concordant at first sight, aroused by later chapters. But just as I was about to do so today, I realized that this digression (which is already approaching a hundred pages. . . ) really doesn't belong in this other "digression", already long enough on its own, which I've called "The key to yin and yang". (A digression which, a month ago, I thought was nearing its end<sup>313</sup> (\*).) It's true that my reading notes on Jung fit in well with the dialectic of yin and yang, and that they have also led me, without having sought to do so, to clarify many things that had been barely touched upon previously, both about my life and about life in general. It doesn't seem enough to me, however, to open a parenthesis of such prohibitive dimensions within another parenthesis, itself situated in the final chapter, "The Funeral Ceremony", of a long reflection on my funeral. It's time to take up this reflection and bring it to a successful conclusion!

Ultimately, therefore, I'm not going to include these reading notes in "The key to yin and yang", or even in Burial, with which they have only a tenuous connection. They can be seen as an illustration of what I've tried to express, in general terms, in the notes on (among others) "Surface and Depth" and "In Praise of Writing" (n°s 101, 102). I hesitate whether to include them in *Récoltes et Semailles*, as a fourth part, or whether I'll make it a separate text in volume 2 of *Reflexions*<sup>314</sup> (\*\*). He

□ is true that this reflection on Jung's life, as it actually unfolded, is indeed a part inseparable from the long reflection I've been pursuing for the past year, which for me has been called Harvesting and

<sup>313</sup>(\*) (March 26) As I wrote this line, I was still under the impression that the note I was starting was going to be part of "The key to yin and yang". It was only over the next few days that I realized that another stage of reflection had already begun. "The key" therefore takes fi n with the previous note "The chain without fi n- or passing (3)" (n° 162").

<sup>314</sup>(\*\*) (March 26) Finally, these reading notes will form (not the fourth, but) a fifth and final part of *Récoltes et Semailles*, which will no doubt form part of volume 3 (not volume 2) of *Réflexions*, along with other texts of a more mathematic nature. The set of notes on Burial that form the "third breath" in the writing of Harvest and Sowing, beginning on September 22 last year, a set of which I was thinking of making a third part of Harvest and Sowing, will be divided into two distinct parts, under the respective names "The key to yin and yang" and "The four operations", forming the third and fourth parts of Harvest and Sowing respectively.

Semailles - and I'm as directly involved in it as I am anywhere else in these notes. It would therefore be artificial to separate this part of the reflection from Harvest and Sowing, for the sole reason that it unexpectedly hatched in the middle of a Funeral, and that it "overflows" a little too much on the latter's central theme.

For the moment, I'm going to take the opportunity of this caesura in my reflection on Jung's autobiography, to get back to my sheep, and to bring this Funeral Ceremony to a successful conclusion, if I can!

It's about time I wrote an account of my friend Pierre's visit to my home last October. I report his arrival in the note of October 21st ("L' Acte", n° 113), having just arrived the evening before, with his daughter Nathalie (two years old). After the departure of my visitors (in the note "Le paradis perdu" of October 25, n° 116), I write: "In a few more days, it will be time to take stock of what this visit has brought me - a visit on which I was no longer counting. . . "Those "few days" have become almost four months - but here I am at last!

I would have liked to give an "on the spot" account of this encounter, which for me represents an important episode in the adventure of discovering the Burial, its reality and its meaning. This time, however, I feel restrained by a concern for discretion, to deliver as is the totality of the multiple and vivid impressions left on me by the passage of my friend. It's true that I had no such hesitation in including one of these impressions in my reflections (in the December 26th note "Le désaveu (2) - ou la métamorphose", n° 153). But mentioning a certain impression one had of such and such a friend at such and such a time, and giving a vivid description of the precise "moment" when such a diffuse impression suddenly became

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manifest, irrefutable - these are two quite different things. The second is a bit like taking a photo of a friend at a time when he doesn't feel observed, and, what's more, circulating it □ without having been assured of his agreement. So I'll confine myself to giving a few impressions left by this visit, and refrain (as elsewhere in *Récoltes et Semailles*<sup>315</sup> (\*) from taking indiscreet photos!

I'd first have to **put** this visit **in context**. I had originally intended to visit Pierre at his home<sup>316</sup> (\*\*) to have him read *Récoltes et Semailles*, including *l'Enterrement*. At the beginning of May, I wrote to him, saying that I'd like to see him soon and have him read a text, intended above all for "my friends of yesteryear and pupils of yesteryear in the mathematical world", into which I had "put my whole self" - "I don't think I've ever cared for a text like that". I thought the typing would be finished by the end of the month, and proposed to come and see him in the first half of June. In the end, because of delays in the typing, not to mention the work involved in putting the finishing touches to *l'Enterrement* (as it was then planned, i.e. essentially what is now part I of *l'Enterrement*), my visit was postponed several times, and in July and August Pierre was not in France. Moreover, he had shown no curiosity about the work I was so anxious to hand over to him and have him read before anything else. Finally, in June I sent him the first part of *Récoltes et Semailles*, "*Fatuité et Renouveau*", thinking it would be a good thing for him to get to know it, before sending him *l'Enterrement* - in case my reflection on myself "clicked" with him and triggered something - you never knew! I'd been ill for ten days or so, and there was no question of me going to Paris any time soon.

I couldn't wait to get him to read *L'Enterrement*, in which Pierre was crucially involved, and

<sup>315</sup>(\*) There is one exception, however - the "photo" I took of J.L. Verdier during a telephone conversation, in the note "*La plaisanterie - ou "les complexes poids"*" (n° 83). I remember that, in order to describe the little scene "on the spot", I had to silence a certain reluctance within myself - I felt a little as if I'd been holding up a sign to my ex-student, which is absolutely not my style. Of course, I was also delighted and pleased with myself that he'd taken the plunge, even though it was one of the biggest and most obvious pitfalls. Serves him right!

<sup>316</sup>(\*\*) I express this intention at the beginning of the note "My friends" (n° 79), and in the first footnote to it.

I would have liked him to come and read it at my home, before he left on vacation. With this in mind, I sent him the complete Introduction towards the end of June, as well as the table of contents of the Funeral - I thought it would come as a shock to him, and that he would be keen to come and see me as soon as before he left p. 778

to find out in detail what I had to say about this famous funeral and his role in it. Instead, I didn't hear from him again until late August - to the point where I wondered whether he'd received my letter at all. That was the great suspense! In his second letter after his return (dated August 25) he finally said a few words about the introduction and table of contents, in terms that seemed to me most evasive. "I got the impression that you didn't know much about the love with which your "orphans" were surrounded. . . ", he wrote, enclosing an annotated bibliography in support, a sign of obvious good will to clear up what he seemed to feel was a sorry misunderstanding. In his next letter (dated September 12), he announced that he would be moving to Princeton on October 7, and said he would try to stop by my place before then. Receiving no further word from him, I thought he'd left for Princeton - but no, when I phoned IHES I learned that his trip had been delayed. And a week later, when I didn't expect to see him for a long time, here he was, in the flesh, in the company of little Nathalie!

(February 17) The meeting took place in an atmosphere that, to all appearances, could not have been more peaceful and friendly. A superficial observer in the vicinity would have sworn that Pierre was poring over a mathematical manuscript, and that from time to time he submitted to me his observations and constructive criticism as a mathematician well "in the know". As far as Pierre himself was concerned, it had to be understood that he had come along (out of consideration for me, who had, after all, been his "master"), sacrificing two precious days of a very busy man's time, to do his best to clear up an unfortunate misunderstanding that had crept up on me, through who knows what unfortunate combination of circumstances. Both his good faith and mine were certainly above suspicion, and there was no need even to mention it, as it was so obvious. His role, on the other hand, was to enlighten me on any points of material detail that didn't seem entirely clear in my notes, or on which I might have made a mistake. He made a list of his observations as his reading progressed, and submitted it to me on the day of his departure - I had the good sense to make a note of it on the spot, using keywords. He did, in fact, manage to read, in two days, the bulk of Burial I, and in any case, all the notes (listed in the table of contents), and by internal references in the text) that directly concerned his person. A fine performance, if p. 779 consider that it took me two months to write these notes full time...

During these two days, little Nathalie has been the wisest of wise little girls. I can hardly say that I heard the sound of her voice - whether she was talking, screaming or crying. She didn't seem to dislike me, but she didn't show much. As for her daddy, he was the real model daddy.

- always available at a moment's notice, to feed, walk or take to bed a little girl who wasn't overly demanding or annoying. He had brought her, he told me, because after the big preparations for the move to Princeton, Mom was too busy cleaning the house to take care of Nathalie. But beyond this practical and force majeure reason, I thought I sensed another, surely unspoken, reason: the little girl's presence added a note of sweetness to the atmosphere of a meeting that my friend, without perhaps wanting to admit it even to himself, was dreading. At the same time, her presence was a living, shining sign of the unspoken willingness with which he had rushed to the United States in the rush of the move - a willingness of obvious good faith and equally obvious goodwill.

For my part, I had not the slightest intention of rushing my friend, to get him to tackle anything.

whatever - I was at his disposal to go into greater depth with him on any question he felt prompted to enter into. As it happened, his main concern was not to go **into the substance of** any of the many situations examined in my notes, where his probity as a mathematician (or his probity at all) was clearly called into question. An observer who overheard our conversation, which sometimes even veered into a ma- thematic discussion (something that hadn't happened between us for more than three years<sup>317</sup> (\*)!), would never have suspected that there was anything in the text my friend was commenting on that would call him into question in any way whatsoever. As for me, I sensed that my friend was firmly clinging to this fiction, painfully maintained, of patently best faith in the best of all worlds. He cautiously avoided anything that might have shattered it, by making it clear that the tacit "consensus" he wanted to establish between us, against all odds, was in no way a reality, but rather a fiction, playing the role of a "straw" to cling to... ..

p. 780 □ During those two days, I could feel just how false the situation was, charged with anguish beneath those outwardly peaceful and good-natured. It was like the rope in the hanged man's house, which nobody talks about, even though it's on everyone's mind! In the end, I made a remark along these lines - I think it was on the day of departure, after lunch. After all, in those notes he was reading, and in the introduction he must have received nearly four months ago, I had expressed myself quite clearly and forcefully on a number of **acts** of his own. Did he really have nothing to say on the subject? He replied, with blurred eyes and a pale, miserable smile, that he was trying his best to "preserve himself" - without specifying (as far as I can remember) what he was trying to "preserve" himself from, surely, my inquiry must have been felt by him as a violent intrusion into a life which had hitherto seemed to him most tranquil and untroubled - where everything must even have seemed to him astonishingly **docile** ; so docile, perhaps, that he had forgotten that it could be otherwise. **To assume** the situation in which he has placed himself, to simply confront it, to examine it as it is - this would represent an upheaval of such magnitude in his vision of himself and the world, such a collapse of the rigid structure of the ego, that most would rather die a thousand deaths and set the world on fire (if they could), than risk such a leap into the unknown. It was from all this, surely, that my friend was (and no doubt still is) keen to "save himself".

I shouldn't be surprised, having seen this kind of scenario repeated hundreds of times, an expression of great fear in the face of the reality of things, and above all, beyond that, in the face of the risk of inner renewal. I certainly shouldn't be surprised, and yet, each time I am again, I am astonished, when I see the most blatantly obvious denied, and suffer and inflict a thousand torments, for the sole purpose of avoiding what I know well, and with certain knowledge, to be the greatest of blessings... .

p. 781 Anyway, after this unsuccessful attempt on my part to "get off the rails", the conversation turned to short. Those minutes were I think the only<sup>317</sup> (\*), during those two days, where our conversation took a person □ nelle turn - or something was said that went beyond the fiction of "consensus", maintained despite evidence to the contrary! I'm afraid that, as is often the case, I didn't have the affectionate yet straightforward "roundness" that could have helped my friend on this occasion, by de-dramatizing an atmosphere which, despite appearances, was extremely tense, and had been for months. As I went about my domestic duties, gardening and writing, leaving my friend to his reading, and also during meals together, there was a silent **expectation** in me towards my young friend - the expectation of an **answer** to what I was saying, through this text in his hands.

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<sup>317</sup>(\*) On the cessation of all mathematical communication between Deligne and myself, see the note "Two turning points" (n° 66).



Surely, it would have been a relief for him if I'd taken the lead in some way, even if it meant starting with a neat argument that he hadn't stolen, no, and finally establishing a **contact where there was** none.

It's true that, over the past fifteen years, whenever I'd tried to raise something personal and close to my heart with him, I'd been met with complete silence, or (when it was in person) with the de rigueur astonished inflections, in the purest "velvet paw" style. It's true that I no longer felt like playing that game, which I'd left with no desire to return to since the 1981 "turning point"<sup>318</sup> (\*). But it's also true that this time there was a visibly unique "moment" in the relationship between us, which might have merited a departure from the rule (or habit, which has become second nature. . . ) of not going against someone else's reluctance to broach such and such a thing. Sometimes it's a good idea (and within certain limits) to "force the hand" a little, a bit like taking a kid to the dentist despite his (irrational) fear of it...

I'm not saying all this just to feel sorry for poor friend Pierre, who didn't get all the kind encouragement from me he might have wished for, and what's more! After all, it's normal for me to have my limits, just like everyone else, and what's more, it's not necessarily my role, and even less my obligation, to cushion the blow for those who have put themselves in situations (even if unwittingly) that were likely to come back to haunt them, one day or another and in one way or another.

□ By the way, after seeing Pierre and Nathalie off at the Orange train station on the evening of October 22, I had only not at all the feeling of a "meeting for nothing", of a "missed opportunity". I hadn't been naïve enough to expect much - it's so rare for two people to get to the heart of an issue that deeply concerns them both! There was no dialogue, that's a given - and yet I felt I'd learned a great deal. There had already been these "material details", more than one of which was very interesting, and which dotted the last i's and crossed the last t's, as regards the question of the "scenario" alone of certain operations that had taken place, and their contexts. I'll come back to this, in continuation of the present note<sup>319</sup> (\*). More importantly, during those two days, I observed my friend with new eyes, in the light of what I had learned of him during my reflection on the Burial. I can say that I "reacquainted" myself with him - in his relationship to me, to things, to his daughter... . This chapter remains a private matter - it's here that the natural reserve that I mentioned at the beginning of today's notes comes into play.

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But from the point of view of understanding the Burial, there was another reason above all, more subtle than the previous two, why it was important for this meeting to take place. I think I had sensed this importance from the moment I had decided to go to Paris to meet my friend, but I couldn't say why then, apart from the fact that it's always important to talk face-to-face with the person concerned, if at all possible when there are things of consequence involving both of us. Here, however, we didn't talk about these very things - and yet I had the impression of having learned, about the **reality of** the Burial, what I still had to learn.

I could put it that way too. Before this encounter, all the circumstances and actions that make up the Burial seemed so **implausible**, so crazy, so delirious, that despite all the tangible, irrefutable material "proof" that had accumulated over the weeks and months, and despite the three hundred or so pages of notes I had already devoted to it - somewhere deep inside me, I

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<sup>318</sup>(\*) See "Two turning points", n° 66.

<sup>319</sup>(\*) See the note "Dotting the I's" (n° 164) which follows this one.

me,

far from it - that a stubborn doubt may linger for some time, a tenacious vestige of resistance against the discarding of an old vision of things, a vision often more comfortable, or more in line with current consensus, than the one that has taken its place. Sometimes, too, this doubt is not simply the expression of inertia against a creative change in vision, but also reflects a healthy, valid element in the old vision, of a **real** aspect of things, which had perhaps been thrown overboard a little too hastily, along with the rest! The fact remains that, as always when a doubt arises, the right thing to do is to become aware of it (which is not always easy, given the inveterate reflex to "silence" unwelcome doubts), and, having done so, to examine it carefully. I can't remember a single time when I've examined a doubt carefully, without having learned something interesting (or even important to me), and of such a nature as to make any doubt disappear<sup>321</sup> (\*). Any doubt is the unmistakable sign of work that needs to be done.

p. 784 In the case in point, namely that of my unexpressed, perfectly irrational doubt about the very reality of a so-called "Funeral", I must confess that before this meeting with my friend, I hadn't even reached this first prerequisite for any work: I hadn't really become aware of it. I hadn't really become aware of it. It remained a simple, diffuse **uneasiness that didn't** say its name - it was up to me to question it! It was only afterwards that I became aware of the malaise and its meaning, just as it had dissipated, precisely by virtue of the encounter with my friend. In fact, I believe that this effect would have occurred regardless of the attitude he adopted - whether it was that of a sort of eager collaboration to provide me with all the missing "material details" (as was the case), or, let's say, on the contrary, that of a vehement denial, furious perhaps, of the most obvious facts. In any case, the **psychic** reality of the Burial could not fail to appear to me, this time by direct perception (and not by "induction" from documents, and by cross-checking from other facts known to me etc.), seeing my opposite number purely and simply **ignore** the ubiquitous absurdities of the "best of all possible worlds" version, absurdities whose very enormity had made me doubt at first, in my innermost being, the reality of the said Burial!

To give just one example: I had to learn from Deligne himself that he had indeed learned the "God's theorem" from Zoghman Mebkhout himself - but that he hadn't wanted to do it.

<sup>320</sup>(\*\*) This **incredulity** in the face of the testimony of our healthy faculties, when these too violently upset the current consensus or the ways of seeing we hold dear, was already evoked in the note "The robe of the Emperor of China" (n° 77). Clearly, writing this note had been a way for me to (at least partially) overcome this incredulity in the face of evidence, by putting my finger on this inveterate reaction. In so doing, however, I **distance** myself from this incredulity, presented as that of ordinary mortals (adults), by identifying myself with the "little child who believes the testimony of his eyes" ("even though what he sees is quite unheard of, never seen before and ignored and denied by all"). This must have been my unconscious intention in writing this note - to distance myself from an attitude of disbelief in my own faculties, and from a gregarious instinct to "do as everyone else does". Such attitudes and instincts do exist in me, as they do in everyone, but (like everyone else) they remain mostly unconscious. So it was like an attempt to exorcise that part of me which was alienating me from myself - and I think the main result was to push that which I wanted to distance myself from **deeper** into the unconscious. The insidious doubt, acting as a secret flaw in my knowledge of things, was not eliminated for all that, nor was the unfortunate incredulity "overcome" ("at least partially", sic)!

I realize once again that, at that point in my thinking, it was still below the level of what I call "meditation". - which is a reflection in which obscure and delicate inner movements (such as that secret disbelief, and the real motivation in me writing the note, which was to "exorcise" that troublesome disbelief) remain constantly the object of vigilant attention.

<sup>321</sup>(\*) It would be more accurate to say that the doubt has been transformed into **knowledge**, which has taken its place. This has nothing in common with what happens when we dispel (or "overcome"! ) a doubt, which has the effect of making it disappear from view, whereas it has taken refuge (or been exiled. . . ) in invisible, deeper layers. It is further than ever from being resolved (and transformed into knowledge), and it continues as ever to act, like a secret **flaw**, a malaise, a sign of a work that remains eluded. Compare this with the comments in the previous footnote.

refer to him in his article with Beilinson and Bernstein<sup>322</sup> (\*), out of **scruples** (!) towards Kashiwara, not being sure (as a non-specialist) what was the share of one and the other in the said theorem<sup>323</sup> (\*\*\*) - he will have

I had to hear Deligne express himself in these terms, to see with my own eyes this strange combination of good faith in detail, and phenomenal and dazzling bad faith in substance and essentials. I didn't think it necessary to draw my friend's attention to the curious way (highlighted in the note "Le Prestidigitateur" (n° 75), which he had read!) in which he had gone about this result "which should have found its place" in his article, to give the appearance that it was none other than himself (or at least, one of the three authors of the prestigious article) who was the brilliant author! Nor did he have any explanation to offer for the strange fact that this Colloquium, which I called the "Pervers Colloquium", was essentially held in the wake of the work and philosophy developed by Mebkhout in previous years (something which Deligne did not even pretend to dispute<sup>324</sup> (\*)), but that his name is rigorously absent from the Colloquium Proceedings published in Asterisk<sup>325</sup> (\*\*). He seemed to regard this as some kind of unfortunate **coincidence**, in which neither he nor anyone else had anything to do with it. All in all, what I've called "l'Enterrement" (the funeral) can be reduced for my friend Pierre to some twenty or thirty such "coincidences".

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I rediscovered a game I knew well from him - and not only from him; a game where you play the fool with the most innocent air in the world, with the certainty of never being stuck. And it's been a while since I've been wasting my time trying to convince anyone (for example) that certain so-called "coincidences" aren't mere coincidences. Pointing out the obvious can be useful at times, but once you've done that, it's a waste of time to try and convince anyone that these are indeed **things**, and not just imaginations - what would you expect? It's a waste of time to try and convince people of bad faith, whether it's conscious or unconscious - it's all the same, and whether it takes the form of idiocy or finesse - it's all the same.

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<sup>322</sup>(\*) See the notes "L'inconnu de service et le théorème du bon Dieu" (n° 48') and "L'Iniquité - ou le sens d'un retour" (n° 75), as well as the notes that follow the latter, forming with it the Cortège "Le Colloque - ou Faisceaux de Mebkhout et Perversité".

<sup>323</sup>(\*\*\*) Of course, there is no more reference to Kashiwara than to Zoghman Mebkhout in the article by Beilinson, Bernstein and Deligne, developing the formalism of so-called "perverse" beams (not to call them "Mebkhout beams"), based on the philosophy of Mebkhout-never-named. Deligne knows better than I do the role of Kashiwara in the theorem of the good God (aka Mebkhout): Kashiwara's constructibility theorem enabled Mebkhout to define the functor from a triangulated category of "continuous" coefficients (complexes of differential operators) to another formed of "discrete" (constructible) coefficients - something nobody in the world had thought of doing before him, let alone suspected that we'd have category equivalence. This was precisely the "missing link" in the duality formalism I had developed over a period of ten years (1956-66), and which my cohomology students, led by Deligne, were quick to bury after I left in 1970.

<sup>324</sup>(\*) Deligne only pretended to qualify my view of things somewhat, saying that in his opinion, the influence of Mac Pherson's ideas in the June 1981 Colloque de Luminy (known as the "Colloque Pervers") was even greater than that of Mebkhout. I wasn't in the know enough to discuss the matter on the spot, and it was obviously a point of detail, which would hardly mitigate the enormity of what happened. Moreover, Deligne did not dispute that neither the Colloquium in question, nor the far-reaching renewal in the theory of the cohomology of algebraic varieties of which it was the sign, would have taken place without Mebkhout's pioneering work in the years leading up to it, and without the philosophy he had developed in complete solitude.

It was my understanding that Mac Pherson's idea of the "intersection cohomology" of varieties, developed by him independently of Mebkhout's ideas, remained somewhat of a dead letter until Mebkhout's "philosophy" illuminated it in a new and unsuspected light (something discovered by Deligne). This was the start-up of Mebkhout's beam theory (wrongly called "perverse", in place of a certain Colloque. . . ). This was the main **event** of the Colloquium, and (it would seem) a turning point in the history of our understanding of the cohomology of algebraic varieties. The keystone of this new understanding seems to me to be the theorem of the good God, which had been "up in the air" since the early sixties and which neither I nor (subsequently) Deligne had managed to clear up.

<sup>325</sup>(\*\*) The term "rigorously absent" is true, to the letter, at least for volume 1 of the Proceedings (consisting of the Introduction and the paper by Beilinson, Bernstein, Deligne), which constitutes the main part of the Colloquium. There are two thumbnail references to Mebkhout in the bibliography to two of the papers in Volume 2 (one by Brylinski, the other by Malgrange), neither of which concerns the authorship of the theorem of the good Lord.

What had changed when we first met, however, and what put a note of anguish in my friend that he was doing his best to control and hide, was that this time the game was no longer confined to a harmless little sport between four eyes, neither seen nor known - and with a **dead person**, too! This time, the cards are open on the table, and **it's a public game**. All bets are off as to what the famous Congregation will endorse and tolerate. It's true that it has already tolerated and endorsed a great deal over the last ten or fifteen years, and it may well continue to do so, who knows? Like my friend Pierre, she may not be more than twenty or thirty "coincidences" away...

(February 18) When I finally drove Pierre and Nathalie back to the Orange train station on the evening of October 22, I felt like an idiot. Pierre looked like someone who had scrupulously and meticulously fulfilled all his duties, according to the timetable he had set himself - and I felt a dull frustration that nothing had been said or discussed at this meeting, which had finally taken place, after months of talk about it.

It was dark, the little one (in the back seat) must have been asleep - it would be about forty minutes in the car to the station, driving dry. We didn't speak for quite a while. It was I who broke the silence, under the impulse of this discontent within me that was looking for some outlet; a discontent with myself surely, rather than with anyone else. That didn't change the fact that I'd gone there to nag...

a bit of a friend. I told him I wasn't quite sure myself, if I wasn't going to sue. □

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legal action against Springer, forcing them to withdraw the pirate volume SGA 4 from circulation<sub>2</sub>,<sup>1</sup> published in Lecture Notes<sup>326</sup> (\*). I couldn't even tell when I'd been touched by this idea, which I'd brought up again at random, as a way of sounding out my friend ("ihm auf den Zahn fühlen", as they say in German). To tell the truth, he didn't react too much; it was more of a monologue that I was doing, picking up on a "thread" that I'd dropped a long time ago, in April or May no doubt. I realized, as I followed it, that a simple judicial showdown didn't make much sense after all - that it would only make sense to take SGA 4<sup>1</sup> out of circulation under its current title and presentation if the initiative came from someone other than me - either Springer or, better still, who knows, Deligne himself. I had to add that I didn't think it would be a luxury for Deligne to make such a public gesture, to make amends for certain things he'd done to me. It would clear up a much-needed atmosphere!

My friend followed my monologue with monosyllables, placed here and there. He implied that Springer might not be so keen on throwing away his entire stock of SGA 4 copies<sup>1</sup> - to which I<sup>2</sup> retorted that all he had to do was change the cover, as he'd already done on another occasion and without any problems<sup>327</sup> (\*\*), so it shouldn't have cost him much. And even supposing he scrapped the stock - one Lecture Notes title out of more than a thousand, you can imagine if that was going to be written off! Not to mention that Deligne, supposing he really wanted it, had the few million old francs he'd need to cover the shortfall...

I didn't have to say, but it was implied (and surely heard), that what was at stake was perhaps more valuable than one or two months' salary for any of us. In the end, I had to say that in this kind of thing, what counts first and foremost is not seeing **how to** do something (or, at least, how to do it right), but how to do it right.

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On the contrary, to list the **obstacles** to doing so), but first of all to be clear about what you **want to do**. □

<sup>326</sup>(\*) On this volume, see in particular the four notes "Le compère", "La table rase", "Le feu vert", "Le renversement", n° 63", 67, 68, 68'.

<sup>327</sup>(\*\*) This was my first misadventure with the Springer publishing house, which had published Hartshorne's notes (on a course in which I had developed the local cohomology formalism) with Hartshorne as author. This was Lecture Notes volume no. ° 41 "Local Cohomology", where the covers had to be changed. Springer had the courtesy then to apologize for the mistake, and to do their utmost to rectify it. Household customs have changed since. . .

Once that's done, the rest becomes a matter of stewardship, and "follows" (when it wants to "follow" indeed).

As my reluctant interlocutor failed to explain his true feelings, I took it for granted that he was well aware that it would be a good thing to "clean up", in short, a much-needed situation - but that he was simply undecided as to what he was going to do about it, "face to keep" no doubt, things like that. I was way off the mark, in fact! It finally dawned on me, when we were already on the station platform waiting for the train. That was when Deligne came back to me, a little sheepishly, to tell me that he'd prefer it if I contacted Springer about SGA 4<sup>1</sup>. Clearly, he didn't want to get involved, or even, at the moment, to offer an opinion on the fate of the book he had authored (admittedly, with my "collaboration"<sup>328</sup> (\*)).

It was only then that I realized that my thoughts along the way had been a monologue - and that it was still not clear to my friend Pierre that there was something not quite "in order" about a certain "SGA 4<sup>1</sup> - SGA 5 operation". It's surely no coincidence, then, that<sub>2</sub> it was on this theme, of all others, that I had branched out, looking for an outlet for my discontent. It was this operation, linked to the massacre of a beautiful work in which I had put the best of myself<sup>329</sup> (\*\*), that had touched me the most - by a breath of violence (in the massacre) and quiet impudence (with regard to what had been massacred). And I was touched again, by this affectation (which I knew all too well in my friend) that, in the end, it didn't concern him at all, the "ideas" I might have had about this and that.

The train was about to arrive, and this was the first time I'd been able to get **to the bottom of** something **that was** close to my heart, in a few words, thanks to an **emotion** that was finally coming to the surface. It didn't take long to say what I felt about it. These were real feelings, of someone wounded in a sense of decency, by someone he cared about who had played him for a fool - this was no longer literature, a little scientific around the edges, dutifully annotated with a pencil in hand.

□ He was taken aback by the blow, still trying as best he could to keep his imperturbable composure. I must have said something like: "And so, you think it was a beautiful thing, this title "SGA 4<sup>1</sup> ", to suggest that it was stuff that came **before** SGA 5 - where you had learned, eleven years before, the maths that has served you every day to this day!". He replied in the tone of someone reciting a lesson, that if he had called it SGA 4<sup>1</sup>, it was only to indicate a relationship of **logical** dependence, not anteriority.

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And so it was that I was given to hear with my own ears, and from the mouth of the person concerned himself, this "farce" so enormous that I could hardly believe the testimony of my eyes, when I had read it in black and white, first from his pen (in "SGA 4<sup>1</sup> "), then from Illusie's (in the volume called SGA 5, which followed, as was "logical", that of my predecessor... ) !

I had to tell him that he knew just as well as I did that SGA 5 "stood" entirely on its own, with no prerequisites or conjectures of any kind, and that it depended neither logically nor in any other way on later contributions. I looked him straight in the eye as I spoke to him, and as he replied. He repeated his lesson in the same atonal voice, that SGA 5 was logically dependent on SGA 4<sup>1</sup> - but I saw in his wavering eyes that he knew as well as I did what was really going on. His eyes were more honest, despite themselves, than his mouth.

So it finally happened between us, the "moment of truth" - but no camera, no tape recorder - and we were left to our own devices.

<sup>328</sup>(\*) On this subject, see the note "Le renversement" already quoted, n° 68'.

<sup>329</sup>(\*\*) See the note "The massacre" (n° 87) and the two notes that follow it.

phone, couldn't have detected it. Only he and I knew what was going on.

The train arrived within minutes, I think. Anyway, that day there was nothing more to say to each other.

## 18.3.2. (2) Dotting the i's

**Note** 164 (February 20-21) To conclude the retrospective of Deligne's last visit (last October) to my home, I'd like to review here the clarifications he kindly provided on a number of points, which remained vague in my reflective notes on Burial I, or even erroneous. This will be an opportunity for me to provide some additional clarifications, prompted by those

p. 790 supplied by Deligne.

## I Motifs ("Lecture Notes 900" volume).

1. Deligne told me that the main purpose of the LN 900 volume<sup>330</sup> (\*) had been to develop a "theory of the **motivic** abelian class field" on a number field  $K \subset \mathbb{C}$ , a finite extension of  $\mathbb{Q}$ . In other words, to determine the "motivic Galois group of  $K$  over  $K$ , made abelian". In this connection, I recalls that I was the first (and with good reason!) to raise this question, towards the end of the sixties. The question has a precise meaning, for a chosen notion of pattern, using the "Betti free functor" on the category of patterns over  $K$ , thanks to the given inclusion of  $K$  in the field of complexes  $\mathbb{C}$ . In fact, I had posed the somewhat more general question of determining the "**metabelian**" **motivic Galois group** of  $K/K$ , deduced from the complete motivic Galois group by making abelian, not all this proalgebraic group, but only its neutral component. We were to obtain a completely canonical extension of the profinite group  $\text{Gal}(K/K)$  by the projective limit pro-tore of the (tores on  $\phi$  associated with the) multiplicative groups  $L^*$  of the finite sub-extensions  $L$  of  $\mathbb{C}/K$ . I remember that Serre was very intrigued by this question, but neither he nor I (nor Deligne, whom of course I had thrown into the mix) could improvise a plausible "candidate". The question then fell into complete oblivion, as did the yoga of motives from which it sprang. This silence was only broken in 1979 by Langlands' article (mentioned to me by Deligne in an annotated bibliography of motives, in his letter of 28.5.1984)<sup>331</sup> (\*\*), in which my idea of the motivic Galois group would be made explicit in the literature for the first time. As I didn't have the honor of receiving a reprint of this article, I don't know whether it refers to my humble self. The next appearance of the motifs in the literature seems to be LN 900, where any allusion to my person, as having anything to do with the theme and main problem of the volume, is absent<sup>332</sup> (\*\*\*)).

p. 791 2. Deligne pointed out that, contrary to what I had thought  $\square$  was true (according to a certain "maison style". . . ), the Deligne-Milne article in LN 900, taking up "ab ovo" the Galois theory of Tannakian categories(\*\*\*) developed by N.R. Saavedra, was written almost entirely by Milne<sup>333</sup> (\*). Deligne also explained to me the error in Saavedra's work, which obliged (if you wanted to have the

<sup>330</sup>(\*) For details of this "memorable volume", see the two notes "Souvenirs d'un rêve - ou la naissance des motifs" and "L'Enterrement - ou le nouveau Père", n° s 51,52.

<sup>331</sup>(\*\*) This is R.P. Langlands' article "Automorphic representations, Shimura varieties and motives. Ein Märchen Corvallis", in Proc. Symp. pure Math. 33 (1979), AMS, vol II P. 205-246.

<sup>332</sup>(\*\*\*) (April 8) I recently learned that the motifs are used in a 1979 article by Deligne (published in the same volume as the Langlands article cited in the previous b. de p. note).

(May 12: this "fi n" has become the sub-note "Pre-exhumation", n° 168(iv))

<sup>333</sup>(\*) On this article by Deligne-Milne, see the note "L'Enterrement - ou le nouveau Père" (n° 52), and also the comments in the later note "La table rase" (n° 67).

formalism of a Galois-Poincaré theory of fiber functors) to reinforce Saavedra's definition of a so-called "Tannakian" category. The work in Deligne-Milne's article did no more than make this adjustment, once the error had been spotted. Incidentally, this raised the very interesting question of a manageable internal characterization of  $\otimes$ -categories that are "true" Tannakian categories (which, more suggestively, could be called  **$\otimes$ -categories of Galois-Poincaré**, since it is for them that we can develop a theory of a Galois-Poincaré groupoid<sup>334</sup> (\*\*)). This question was not addressed in the article in question, nor has it yet been satisfactorily resolved. Clearly, the aim was not to pose or resolve interesting mathematical questions, but to provide a substitute reference for Saavedra's article. (See the end of the note "La table rase" (n° 67).)<sup>335</sup> (\*\*\*)

3. On several occasions in Burial I, I stressed the fact that the Hodge-Deligne theory, developed by Deligne in the late 1960s, was only a first step towards a theory of "Hodge-Deligne coefficients" on a finite-type scheme over  $\mathbb{C}$ , and towards a "six-operation formalism" for such coefficients. I was (and remain) convinced that, were it not for Deligne's deliberate move against some of the key ideas introduced by me (such as the six-operation formalism), Hodge-Deligne theory would have reached "full maturity" by now. Deligne pointed out that the only definition

of a category of Hodge-Deligne coefficients on a finite-type scheme over  $\mathbb{C}$ , ran into serious  $\square$ ses difficulties, which he would not have been able to overcome. (It would have been all the more compelling to clearly **formulate** this p. 792

question from the very beginnings of the theory, as well as the closely related question of the formalism of the six operations for such coefficients, something Deligne has always refrained from doing). In his view, Mebkhout's point of view and Mebkhout's bundles<sup>336</sup> (\*) should provide a way of approaching the right definition. (And if it hadn't been for this deliberate intention, Deligne certainly wouldn't have waited for Mebkhout to develop the philosophy the latter had developed (against the grain of his elders), and to use it for a visibly fundamental work that for fifteen years has remained on the sidelines and still not even reported in the literature, except by myself in Récoltes et Semailles).

4. I mistakenly thought that I had introduced the "filtration by weights" of a pattern, reflected (for any  $l$ ) in the corresponding filtration on the  $l$ -adic realization of this pattern (filtration defined in terms of absolute values of Frobenius eigenvalues). In fact, Deligne reminded me that I had only worked with "virtual" notions of weights (which amounted to working with virtual patterns, elements of a suitable "Grothendieck group". . . ). It was Deligne who discovered this important fact, that the virtual notion I was working with should correspond to a canonical **filtration**, by "increasing weights"<sup>337</sup> (\*\*).

<sup>334</sup>(\*\*) The term "groupoid" (de Galois-Poincaré) has the advantage of suggesting a close kinship with the notion of the fundamental groupoid of a topological space or topos. Technically speaking, however, the term "sheaf" (de Galois-Poincaré) would be more appropriate. This is the sheaf of "fiber functors" defined, not only on the base field  $k$  of the  $\otimes$ -category envisaged, but on any objects of the fpqc site of schemes on  $k$  (with particular attention paid to objects of this site that are of the form  $\text{Spec}(k')$ , where  $k'$  is an extension of  $k$ , or even a **finite** extension of  $k$ ).

<sup>335</sup>(\*\*\*) (May 12) Having recently become acquainted with Saavedra's book, it now appears that it, and the very name ("Tannakian category") of this notion, which I introduced around 1964 and which gives the book its name, is a **mystification**. I dismantle it in detail in the suite of notes entitled "Le sixième clou (au cercueil)" (n° s 176<sub>1</sub> à 176).<sup>7</sup>

<sup>336</sup>(\*) These are the beams that Deligne had introduced under the name of "perverse beams". (On this subject, see the two notes "L'Iniquité - ou le sens d'un retour" and "La Perversité", n° s 75, 76.) He wasn't annoyed and, in our conversations, kindly referred to them as "Mebkhout's beams". . .

<sup>337</sup>(\*\*) The heuristic reason that convinced Deligne of the existence of such a (necessarily unique) filtration of a pattern is that there are non-trivial extensions of abelian varieties by tori (whose motivic  $H^1$  thus provides a non-trivial extension of a pattern of weight 2 by a pattern of weight 1), but not the other way around. This may sound thin - yet I myself was convinced more or less on the spot - it was too seductive to be wrong! A more serious reason, at the level of  $l$ -adic representations from patterns on a **finite  $K$ -field**, would be to prove that any extension of a module

This discovery (just as "conjectural" as the "conjectural theory of patterns") immediately provided the key to a formal definition of **Hodge-Deligne structures** (also known as "mixed Hodge structures") on the field of complexes, as a "Hodge-like" transcription of "already known" structures on the pattern and on its made by Hodge

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□ Technically speaking, the influence of my ideas in the definition of Hodge-Deligne structures is twofold. On the one hand, via the notion of weights of a pattern, suitably clarified by Deligne into a structure of "**filtration** by weights". On the other hand, since the 1950s, I had emphasized the importance of the algebraic **De Rham cohomology** of a smooth algebraic variety  $X$ , not necessarily proper, as a richer invariant than the naive Hodge cohomology (direct sum of  $H^q(X, \underline{\Omega}^p)$ ), which is related to the former by the well-known spectral continuation, associated with a canonical filtration (**the De Rham filtration**) of the De Rham cohomology. I was the first to define the algebraic De Rham cohomology (at a time when nobody would have thought of looking at the global hypercohomology of a complex of differential operators, such as the De Rham complex), and to insist on its **filtered** graded structure, in contrast to the bigraduated structure of the Hodge cohomology, which since Hodge had been on the forefront. In the case of  $X$  proper (i.e., where Hodge theory is available, implying that the preceding spectral sequence degenerates into a zero-square), and on the base field  $\mathbb{C}$ , we recover the bigraduated structure on De Rham's cohomology, from its filtered structure, by taking the "intersection" of this filtration and the conjugate complex filtration (thanks to the "real structure" of De Rham's cohomology, isomorphic to Betti's cohomology  $H^*(X, \mathbb{C})$ ). I subsequently proved (when no one but myself believed in De Rham cohomology in the non-clean case), that for a scheme  $X$  smooth over the field of complexes, De Rham cohomology (which has a "purely algebraic" meaning) is canonically isomorphic to complex Betti cohomology (defined by transcendental means).

That said, once we had postulated the existence of a notion of pattern (not necessarily semisimple) on  $\mathbb{C}$  and of a motivic cohomology of a  $\mathbb{C}$ -schema  $X$  (not necessarily clean, admittedly), and of a notion of "Hodge realization" (suitable and to be found) of a pattern on  $\mathbb{C}$ , which (according to my ideas) was to associate with the motivic cohomology of smooth  $X$  a "generalized Hodge structure" (to be defined), having as its basic set the De Rham cohomology  $H_{RD}(X)$ , the first structures we read about on the latter, namely De Rham filtration (introduced by me as early as the 1950s) and filtration by weights (introduced by Deligne on the basis of my ideas on virtual weights, clarifying Serre's ideas, themselves stemming from Weil's conjectures), we fall very exactly on the notion of "mixed Hodge structure" introduced by Deligne.

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□ Bien entendu, cette filiation d'idées (164) était parfaitement connue de Deligne. It would have been in keeping with the ethics of the trade (which I was unable to pass on to him) that he clearly indicates in his work introducing mixed Hodge structures<sup>338</sup> (\*). He preferred not to mention it in this work, which is also **his thesis**, just as he saw fit, on this particular occasion, not to mention the name of the man who had been his teacher.

5. In the annotated bibliography on the motives (attached to his letter of August 25), Deligne states that "one of the reasons why we [!] hesitated to build on them [on the few "classic texts"]<sup>339</sup> (\*\*)

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Galoisian module of weight  $i$  by another of weight  $j$  is trivial if  $i < j$ . I can't remember whether Deligne or I were able to demonstrate this statement, which would prove the existence of a canonical filtration "by increasing weights" for the  $l$ -adic Galoisian module associated with a pattern (an object already quite close to the pattern itself. . . ).

<sup>338</sup>(\*) This is the article "Hodge Theory II" (Pub. Math. IHES 40 (1971) pp. 5-58). On the other hand, Serre and I are mentioned in the same line, in the "Hodge I" announcement at the Nice Congress (in 1970), as I point out in the note "The victim". (n° 78, on page 308). For comments on this subject, see sub-notes n° 78, 78'.

<sup>339</sup>(\*\*) These are the few sporadic ("classic") texts on motifs by Kleiman, Manin, Demazure, published up until



on motives] is the use made of conjectures about the existence of algebraic cycles - conjectures for which there is no real evidence, whereas motives are, for me, indubitable".

My answer to this explanation is that these "classic texts" are in no way representative of the "state of the art" at the end of the sixties - indeed, they're far from it - and it's **not** from these texts that he, Deligne, learned this "state of the art"! He knows full well that my "standard conjectures" were **one** of many possible approaches to a provisional "construction in form" of a notion of (semi-simple) motif on a body, which in no way limited the scope and internal dynamics of the ideas he got from me. (See sub-note n° 51<sub>1</sub> of the note "Souvenir d'un rêve - ou la naissance des motifs" n° 51.) Killing two birds with one stone, he endeavored after my departure both to discredit the standard conjectures as "unapproachable" and devoid of interest, and to discredit a certain approach to motives which would have been mine and which would have represented a cul de sac, indissolubly linked as it would have been (to hear him tell it) to these hopeless conjectures.

so much so that it was more charitable for me, in the LN 900 volume where at last the work that really needed to be done is done, to pass my name <sup>340</sup> pudiously under silence... (\*)

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6. In the same "annotated bibliography", I read:

"From this "classical" point of view<sup>341</sup> (\*\*) there is a regrettable gap in the literature: your conjectural description of the  $\otimes$ -Tannakian category of motifs on  $F_p$ , with unique equivalence to non-unique isomorphism - with these various fiber functors (crystalline and l-adic), cf. Tate,

isogeny classes of abelian varieties over a finite field, Sémin. Bourbaki 352 (1968)."

These are crocodile tears, over a "regrettable lacuna" which is due to no one (apart from me. . . ) but my friend Pierre Deligne himself, since apart from me, he must have been the only mathematician in the world who knew of the "conjectural description" in question. . . It was up to him to include it in the same LN 900, for good measure! There was nothing conjectural about this description, as far as I recall, except that it was necessary to assume that we had a category of "patterns on  $F_p$ ", satisfying some reasonable conditions, which we have the right to expect from a category of this name. If I remember rightly, the reference to Tate-Honda implied that the category in question was generated multiplicatively by the Tate motif (and its inverse) and by the abelian varieties defined on  $F_p$ . There were some nice things (and many more), which I had entrusted into the hands of my brilliant ex-student and which have remained carefully buried until today. ...

## II Staggered cohomology ("SGA 4 1/2" SGA 5, SGA 7, discrete Riemann-Roch).

1. One of the first comments Deligne made to me about <sup>□</sup> l'Enterrement I concerned the vicissitudes of the conjectural theorem I had worked out in SGA 5, known as the "discrete Riemann-Roch theorem". I write about it in some detail in sub-note n° 87<sub>1</sub> to the note "Le massacre" (n°

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1970. They don't go much beyond the initial idea of a motif, and can't give any idea of the finessse of the "yoga" I had developed, and which I had tried to communicate to anyone who would listen. In particular, there is no mention of the motivic Galois group, which had been an essential initial motivation for developing yoga. In particular, there is no mention of the Galois motivic group, which had been an essential motivation for developing yoga in the first place (see "Souvenir").

of a dream - or the birth of motifs, n° 51.)

<sup>340</sup>(\*) Deligne took the lead on any questions I might have asked him on this subject, from the very first day of his stay with me, in saying to me with his most beautiful smile: "Do you really think that everyone doesn't know about it, even though you're the one who introduced the motifs! The amazing thing was that, despite everything my friend had done to make people forget about it, I could see that it was still generally known. But in the absence of any written references for my ideas, Deligne had every opportunity to create the impression that my contribution had had to confine itself, as usual, to proposing a vague general idea (moreover unusable as it stands, given its dependence on conjectures "as unapproachable today as they ever were". . . ) - so vague, in fact, that it really didn't merit any serious mathematician, doing real work, taking the trouble to make even a token reference to it...

<sup>341</sup>(\*\*) See penultimate note of b. de p.

87). Deligne tells me that when he communicated my conjectural statement to Mac Pherson, he saw himself as having the role of "factor", of intermediary. He did not add a new ingredient to my statement - the idea of translating my statement into homological language, to give it meaning for singular spaces, was Mac Pherson's doing, not Deligne's. He told me he was surprised, on receiving the offprint of Mac Pherson's paper proving my conjecture in the analytic-complex case and in the homological context (by transcendental arguments), to find the conjecture under the name "Deligne-Grothendieck conjecture". He had thought of writing to Mac Pherson to rectify the misunderstanding, but (he wouldn't have known himself why) he didn't in the end. ...

2. Contrary to what I assumed and implied, Deligne had not committed himself at the time of the SGA 5 oral seminar to writing one or more of the seminar papers, for example the paper on the cohomology class associated with an algebraic cycle (which he ended up writing eleven years after the seminar for inclusion in the volume of his composition called "SGA 4<sup>1</sup>", without further ado<sup>342</sup> (\*)).

In this connection, I asked whether he didn't think that the privilege of having been able to learn "on the spot", in SGA 5, the basic techniques that served him in all his subsequent work, didn't impose on him an **obligation** or a responsibility to do his utmost to make these techniques available to the mathematical public, through a rapid publication of SGA 5. Deligne replied that **he didn't think so**. I refrained from asking him the same question about the philosophy of motives, which was his main source of inspiration for the cohomology of algebraic varieties (which constitutes the central theme of his work. . . ).

□

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3. It was Deligne who took the initiative of asking Verdier for his agreement to include in "SGA 4<sub>2</sub><sup>1</sup>" the famous "Etat 0" of Verdier's work on derived categories. Verdier initially objected, deeming it would be pointless (I can't remember the exact expression). It was Illusie who finally convinced Verdier to agree. Verdier's initial reaction seems to me to be the most natural and in line with simple mathematical common sense. What's more, Verdier had decided years ago to bury the derived categories, in the form of a major "work on parts", which was one day supposed to constitute his thesis - so it was going to look a bit goofy to publish a preliminary sketch that had long since been largely covered by the literature. I think I understand why Deligne and Illusie were so keen to publish this Etat 0, in which my name was not mentioned. As for Verdier's reasons for going back on his initial common-sense reaction, I think I can sense them, and I've written about them in the note "Thèse à crédit et assurance tous risques" (n° 81). 4. In the note "La table rase" (n° 67), I pointed out the ambiguity of the expression "ce sémi- naire" in the passage of the Introduction to SGA 4<sup>1</sup> (p. 2) where it says: "For the application to *L-functions*, this seminar contains another demonstration, this one complete, in the particular case of the Frobenius morphism". This ambiguous expression, given the context and its spirit, had every chance of being read as meaning "SGA 4<sup>1</sup>", so as to suggest that the parent seminar SGA 5 did not contain a "complete" demonstration of the rationality of *L-functions*. Deligne clarified to me that in his mind, "this seminar" did indeed mean "SGA 5". To tell the truth, this clarification means nothing to me. I'm well aware that Deligne knows as well as I do that in SGA 5 there's a "complete" demonstration, but yes, of a trace formula, which overflows

<sup>342</sup>(\*) This act of dismantling (among many others) the SGA 5 seminar in favor of the volume called "SGA 4<sup>1</sup>"<sub>2</sub> fulfilled two functions, both in the sense of a "reversal" of roles: to make me a "collaborator" of

Deligne, and support the claim of anteriority (already suggested by the misleading name SGA 4<sup>1</sup>, and spelled out "between the lines" in the introduction to both SGA 4<sup>1</sup> by Deligne, and SGA 5 by Illusie) of "SGA 4<sup>1</sup>" over SGA 5 (where references to SGA 4<sup>1</sup>, via the aforementioned pirated presentation of SGA 5, abound). On this subject, see also the comments in the note "Le renversement" (n° 68'), where I finally discover the meaning of the strange name given to the pirate-volume, and of the presence in this volume of my talk on cycles algebraic.

He also refers to "the special case of the Frobenius morphism" (contrary to what he implies). But it's no coincidence that Deligne's writings abound in inaccuracies and ambiguities, if not blatant untruths, which all point in the same direction: to suggest an impression, concerning my work or that of Mebkhout and others linked to me, likely to discredit it, while enhancing his own credit, or creating some from scratch<sup>343</sup> (\*).

□<sup>5</sup>. I'd like to take this opportunity to add a few comments about SGA 7 II (seminar presented p. 798 as directed by P. Deligne and N. Katz), on which I had already commented in some detail in note (unnamed<sup>344</sup> (\*)) n° 56. A more detailed examination has shown me that, on this occasion, N. Katz did not hesitate to discreetly push the wheels of Deligne's well-mannered Funeral Van, in many ways.

Katz agreed to appear with Deligne as co-author of the volume and the seminar, which in no way corresponds to the reality of what had taken place during the oral seminar, four years before the volume was published. The overall conception of the SGA 7 seminar (which continued over the two years 1967-69) came from me, and the seminar was presented as a seminar directed jointly by Deligne and myself. N. Katz appeared as a collaborator and lecturer, among a number of others. But since N. Katz agreed to sign as co-author of the volume (of which he wrote five papers, but none of the main results), it's only natural to consider him co-responsible, along with Deligne, for the overall structure of the volume, and for the fact that I was not mentioned.

I'm thinking first and foremost of the oversight in the introduction to the volume (signed by Deligne), where nothing suggests that I had anything to do with any of the themes or results presented in the text, even though one of the two "key results" of the seminar featured (namely, Lefschetz's theory of brushes

) had already been developed by me before the SGA 7 seminar, and had in fact been one of my motivations for considering a seminar on the theme of monodromy. In Katz's presentation of this theory (Exp. XVIII), entitled "Etude cohomologique des pinceaux de Lefschetz, par N. Katz", my name does not appear in the title as is customary ("d'après A. Grothendieck"), but appears in a laconic footnote after N. Katz's name, "D'après des notes (succinctes) de GROTHENDIECK". The qualifier "succinct" seems to have been added to minimize the fact that these unfortunate "notes by Grothendieck" played a role here. They may have been "succinct", but they were nonetheless the culmination of several days' work on the task - by no means obvious at first sight - of transcribing into

an entirely different technical context, results stated and demonstrated by transcendental means. As with étale duality or □the Nielsen-Wecken theory<sup>345</sup> (\*), the classical arguments werep

We had to redo the whole thing, taking the classical **results** as a guideline and completely forgetting their traditional "demonstration" (if you can call it that). It's only natural that, even with the help of my detailed notes, Katz had to make an effort to get into the swing of things, just as I had to do before him - but this in no way implies (at least, not according to the generally accepted rules of the game) that he is the author of Lefschetz's brush theory in étale cohomology!

Continuing in the same vein, in the introduction to the same talk (p. 225), Katz pretends to introduce Mrs.

<sup>343</sup>(\*) Suggesting, in particular, his authorship of the motifs' key ideas, that of staggered cohomology, and that of the "theorem of the good Lord" and the Mebkhout philosophy that goes with it.

(March 26) For the case in point and "this seminar", see also the sub-note "Les doubles-sens- ou l'art de l'arnaque" (n° 169 ).<sup>7</sup>

<sup>344</sup>(\*) (March 26) In the meantime, I have filled this gap by including this note in the table of contents under the name "Prélude à un massacre".

<sup>345</sup>(\*) Less restrained than his friend N. Katz, Deligne didn't think it worth mentioning that I had something to do with what he called "the Nielsen-Wecken method" - on this subject, see sub-note no. ° 67<sub>1</sub> to note "La table rase" no. ° 67.

Raynaud as the author of the structure theorem of the "prime to  $p$ " moderated fundamental group of an algebraic curve in  $\text{car. } p$ . If I remember correctly, it is this theorem (demonstrated by me in 1958, before I had even met my future student) which, along with the "Lefschetz cow theorem", constitutes the deep technical ingredient of the theory, and I was quite happy, in the demonstration of the irreducibility theorem, to have to use it in all its force.

In the introduction to Katz's lecture XXI (pp. 364-365), after describing the main theorem of the exposed, concerning complete intersections in projective space, it is stated:

"There are heuristic arguments due to A. Grothendieck and relying on the yoga of crystalline co-homology, which make the general statement plausible for any projective and smooth  $X$ , by essentially the same method."

This comment implies that I was inspired by the method of the text (by an unspecified author, who can hardly be more than one of the two authors of the volume), to embroider on it "heuristic arguments" that allow the proven result to be generalized. I seem to remember that it's just the opposite - that it's my "heuristic arguments" (which I had developed in my corner long before the seminar, in the wake of my thoughts on Griffiths' theorem and Lefschetz's brushes<sup>346</sup> (\*\*)), which happen to "work".

p. 800 □(without conjectural ingredients what's more) in the case where  $X$  is a complete intersection. Moreover, in the previous paper (also by Katz) devoted to said Griffiths theorem, it is stated in the introduction that **"the demonstration given here** (due to GROTHENDIECK) is the translation into purely algebraic terms of the original, more or less transcendental demonstration by GRIFFITHS". This comment may give the impression that there are several demonstrations of Griffiths' theorem to choose from, and that I've been given the honor of choosing mine. In fact, as far as I know, there is no other. Moreover, from the work I was obliged to put into it, I doubt that this demonstration is a simple "translation" of Griffiths', any more than the demonstration of any of the great key-theorems in étale cohomology was a "translation" of an already known demonstration, or (while we're at it) than mastering the étale cohomology of schemes was a matter of "translating into purely algebraic terms" the familiar theory of ordinary cohomology.

I've reviewed **the** three references to me in the texts of N. Katz's talks (there's only one in all eight of Deligne's talks! ). All three seem to me to reflect the same deliberate intent. Finally, I'd like to point out that in the text of the last talk in the volume, by N. Katz, devoted to the "mod.  $p$  congruence formula" for an  $L$  function in  $\text{car. } p$ , my name does not appear<sup>347</sup> (\*) - not even for the ordinary cohomological expression of the  $L$  function. In fact, the analogous expression in terms of crystalline co-homology (which remained conjectural), had led me to conjecture the congruence formula for several years. I had communicated this conjecture to Deligne, who had found a surprisingly simple demonstration, thanks to his symmetrical Kunneth formula (discussed in SGA 4 XVII 5.4.21). I assume that Katz, who was well versed in this sort of thing, was also well aware of the origin of the conjecture, without seeing fit to mention it. (In the text, he presents a different and much less elegant demonstration than Deligne's).

<sup>346</sup>(\*\*) These reflections, along with my thoughts on the theory of evanescent cycles in abstract algebraic geometry (another of my "purely algebraic translations of transcendental theory"! ), were the inspiration for the SGA 7 seminar.

<sup>347</sup>(\*) That's not entirely true - he figures there (so it's a fourth reference to me), in a talk with Deligne, on page 410, to thank us for explaining to the author various equivalent reformulations of the form in which he presents the congruence formula. The funny thing is that, of the three numbered references he gives for these brilliant variants, none exists in the presentation, so these thanks take on the appearance of an amiable hoax! (It's not the first one I've come across in L'Enterrement... )

A funny detail: at the end of the introduction to this ultimate presentation of SGA 7 II, we read that Deligne's demonstration "should appear in the reissue of SGA 5" (which SGA 5 hadn't yet had the chance to publish).

to know its □ première "edition"). This may suggest that five years before the APG 4 operation<sup>1</sup> - APG<sub>2</sub> 5, Deligne still intended (as was normal) to include in the future published version of SGA 5 the additions he had made since 1966 to the theory of staggered cohomology, developed in SGA 4, SG4 5<sup>348</sup> (\*).

p. 801

### III Mebkhout's philosophy (Colloque de Luminy June 1981, paper on the "per-vers beams" of Beilinson, Bernstein, Deligne).

I'll repeat here what I said on this subject in the previous note.

1. Deligne told me that he had learned about the "theorem of the good Lord"<sup>349</sup> (\*\*) in a conversation with Mebkhout at a Bourbaki seminar - at any rate, this was before the summer of 1980. This tallies with what I know from Mebkhout, namely that the theorem in question had been communicated by Deligne to Bernstein and Beilinson in October 1980, to be immediately used by them in their proof of the Kazhdan-Lusztig conjecture<sup>350</sup> (\*\*\*). Deligne adds that he had not cited Mebkhout in his paper with Bernstein and Beilinson, not being sure how much of this theorem was due to Kashiwara<sup>351</sup> (\*\*\*\*).

2. Deligne did not deny that the Colloque de Luminy in June 1981 (where he himself was the star attraction) would not have taken place without Mebkhout's work in the preceding years. He only made a point of adding that the role of Mac-Pherson's ideas seemed to him "even more essential". He did not suggest that there would be anything strange or abnormal about Mebkhout's name not appearing in the Colloquium Proceedings.

### IV Duality formalism in cohomology, derived categories ("The right reference", "State 0" of derived categories).

□<sup>1</sup>. Deligne tells me that he was unaware of Verdier's article<sup>352</sup> (\*), which (between others, without naming myself) the formalism of homology and cohomology classes associated with a cycle (which I had developed in SGA 5 in 1965/66) only **after** the publication of SGA 4<sup>1</sup> in 1977, i.e. a year later. at least after the publication of the article in question. This seems to contradict the impression I had that Verdier's brilliant operation in 1976 was a sort of "trial balloon" for the considerably larger operation by Deligne et al. that followed the year after.

p. 802

Deligne told me that it was clear to him, from reading Verdier's article, that it merely expounded some of the ideas I had developed in SGA 5. He was even pleased that Verdier had finally taken it upon himself to provide a reference. (The idea that the publication of SGA 5 might have provided a more adequate reference must not have occurred to him. . . ) To a question from me along these lines, Deligne replied that he hadn't

<sup>348</sup>(\*) I presume it was the lack of any reaction (from any of the people who were in on it) to the swindles that took place in SGA 7, which must have encouraged Deligne to the next step in his escalation: the large-scale swindle of the SGA 4<sup>1</sup> - SGA 5 operation.  $\tau$

<sup>349</sup>(\*\*) See note "L'inconnu de service et le théorème du bon Dieu", n° 48'.

<sup>350</sup>(\*\*\*) See the May 28 footnote to "Iniquity - or the meaning of a return" (n° 75), and also the note "A feeling of injustice and powerlessness" (n° 44").

<sup>351</sup>(\*\*\*\*) See the comments on this subject in the previous note "Le devoir accompli - ou l'instant de vérité", especially p. 784, and the footnote about "Kashiwara".

<sup>352</sup>(\*) This is the article cited in the note "Les bonnes références" (that was definitely the right name!), n° 82.

(May 12) For comments on this diffi culty believable version by Deligne, see the note "Gloire à gogo - ou l'ambi- guité" (n° 170(ii)), pages 930,931.

noting that my name didn't appear in Verdier's article - adding that he confessed he hadn't even thought to ask himself the question. I had the impression that he was tacitly implying that this sort of thing was the least of his worries and not worth dwelling on. ...

2. In Beilinson, Bernstein, Deligne's paper (often cited in Burial I), written by Deligne and presented by him at the Colloque de Luminy<sup>353</sup> (\*\*), the duality in staggered cohomology (which I had developed in 1963) is called "Verdier duality"<sup>354</sup> (\*\*\*).

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□ I asked Deligne about this strange appellation. He replied (with a touch of embarrassment this time) that it was because "everybody" called him that. I didn't ask him to tell me who "everybody" was, or why, even though he, Deligne, knew perfectly well whose theory it was.

This reminds me of something that had struck me long ago. When talking to me at least, or writing to me, Deligne never used the expression "catégorie dérivée" without adding "de Verdier". It gave me an unpleasant impression every time, without me ever stopping (until I discovered l'Enterrement) to probe the meaning, let alone dot the i's and cross the t's. I would no doubt have stopped there, if I'd taken the trouble to take a slightly curious look at "SGA 4<sup>1</sup>", and at the "Etat 0" of Verdier's<sub>2</sub> "thesis" exhumed there. (For details of the latter, see II 3 above).

## V The Eulogy

1. The IHES jubilee booklet containing my Eloge Funèbre<sup>355</sup> (\*) was not written by its founder and first director, Léon Motchane (as I had thought). What's more, the identity of the booklet's author, which I learned from Deligne, is of little importance here. He confirmed that it was indeed he who had written the passage concerning me, and that this passage, like the one concerning Deligne (due to the author of the booklet), had indeed received his "green light" before being sent to the printer. The text he had dedicated to me was initially longer, and had been (with his agreement) truncated by the author of the booklet. Deligne had also revised and corrected his own text. These texts therefore represent Deligne's point of view, concerning his work and mine.

p. 804

2. I asked Deligne if I'd made a mistake, assuming that in none of his publications did he suggest that he'd learned anything □ from me. He confirmed this, with just one comment reserve. It concerns the biographical note he had written for the Fonds National de la Recherche Scientifique (Brussels), on the occasion of the award of the "Prix quinquennal". This prize had been awarded to him (in 1974, I believe) in recognition of his demonstration of Weil's conjectures. It's true (he added) that

<sup>353</sup>(\*\*) On this "memorable Colloquium" and the article in question, see the note "L'Iniquité - ou le sens d'un retour", n° 75.

<sup>354</sup>(\*\*\*) This operation took place in several stages. After 1963, at my suggestion, Verdier developed a theory of "six operations" duality in the context of ordinary topological spaces, following the masterwork I had developed in the coherent and stale algebraic context. This duality had been christened by my cohomology students, appropriately enough, "Verdier duality" or "Poincaré-Verdier duality", with no mention of my modest self. In the "good reference" of 1976, Verdier takes up again, in the analytic context and without naming me, part of the formalism I had developed in the coherent framework in the fifties (without having to change anything). As a result, this duality, in the analytical context, is still known as the "Verdier duality", or sometimes as the "Serre-Verdier duality", always without any mention of myself - even Mebkhout follows the general trend! But (in a stroke of genius) it's quite clear that algebraic coherent duality is merely a "purely algebraic translation" of transcendental analytic theory, just as étale duality is such a "translation" for transcendental topological theory. It was therefore only natural to call them "Verdier duality" (Serre and Poincaré being left out, as they are far away). According to what Deligne told me, that's what "everyone" did in a hurry. Curtain...

<sup>355</sup>(\*) See the two notes "L'Eloge Funèbre (1) - ou les compliments" and "L'Eloge Funèbre (2) - ou la force et l'auréole", n° s 104, 105.

this biographical note is not part of a mathematical publication, and its distribution has remained more than limited. I myself was unaware of its existence. At my request, he sent me a photocopy within a few days, and I'll come back to it in the following note.

Deligne's systematic disavowal of me didn't seem to bother him. He didn't seem to find anything strange about it, worthy of attention. Given this disposition, I didn't feel prompted to ask him any questions along these lines - I don't think I'd have got anything more out of him.

To conclude this retrospective, I would only add that as far as "material facts" in the strict sense of the term are concerned, I have no doubts whatsoever about Deligne's good faith, which seemed obvious to me<sup>356</sup> (\*\*). The only exception in this respect is his assertion that the SGA 5 seminar (of 1965/66) would logically depend on the results of SGA 4<sup>1357</sup> (\*) (developed from 1973 onwards, alongside Deligne's presentations on his demonstration of Weil's conjectures). It's true that by "capturing" some of the talks given at the SGA 5 mother seminar (especially the one on the cohomology class associated with a cycle), with the connivance of Illusie (who was responsible for editing SGA 5) and many others, he has achieved the brilliant result that SGA 5 is full of references to SGA 4<sup>1</sup>, so as to give the impression (to an inattentive reader) that SGA 5 does indeed depend on SGA 4<sup>1</sup>, which is presented in every respect as an "earlier" text. It's a sleight of hand that's probably unique in the annals of our science, and one that seems to me to distinguish the seventies of our century from all the other eras that mathematics has known.

**Note 164** □ Concerning the "philosophy of weights", stemming from Weil's conjectures, the "filiation" seems to me p. 805 1

can be summed up as follows.

a) As stated in sub-note n° 46<sub>9</sub> of the note "My orphans", Serre had communicated to me, as part of the "philosophy" behind Weil's conjectures, a kind of "yoga of **virtual** weights", at the level of l-adic cohomology of finite-type scheme over a body. He had not attempted to give a precise explicit formulation, and the relationship between what was happening for different *l*'s remained entirely mysterious.

b) One of my two main motivations for developing a "yoga of motifs", from the early sixties onwards, was precisely to link together "virtual weight structures" for different *l*. (See "Souvenir d'un rêve - ou la naissance des motifs" (n° 46), and especially p. 208). From then on, it became clear that this structure had to be found on all possible "realizations" of a pattern, not just l-adic realizations - and in particular (on the base body C) on the De Rham-Hodge realization.

c) Made aware by me of this philosophy of virtual weights, whose ultimate source is the pattern, Deligne brings an important clarification to this yoga, with the presumption that the structure of virtual weights on a pattern is linked to a (necessarily canonical) **filtration by increasing weights**. This filtration should then be found on all realizations of the pattern - both the l-adic realizations and (on the C-body) the De Rham-Hodge one.

This "presumption" of Deligne's was the starting point of his theory of "mixed" Hodge structures (which I call "Hodge-Deligne structures"), and one of the two essential technical ingredients of his definition

<sup>356</sup>(\*\*) (May 12) With hindsight, however, certain reservations about this impression have emerged, such as those referred to in a previous b. de p. note (\*\*) p. 802). It also became apparent that Deligne had omitted to point out to me two gross material errors in my notes, which could hardly have gone unnoticed by him (it had escaped me that he revealed part of the "yoga of the weights" in Hodge I as early as 1970, and that he had spoken of the motives as early as 1979).

<sup>357</sup>(\*) It's true that this confirmation came, not through Deligne's spontaneous initiative in bringing me "material clarifications" to enlighten me and show his complete good faith, but under the unforeseen pressure of the need to "keep face", when I had just expressed my feelings to him verbally about the incredible SGA 4<sup>1</sup> - SGA 5 operation. See this subject the last part (dated February 18) of the previous note "Duty done - or the moment of truth". 2

in the form of these (the other being De Rham's filtration, which I introduced back in the 1950s). It is the success of his attempt to describe a "Hodge cohomology" for separate schemes of any finite type over  $\mathbb{C}$ , which can be regarded as the main (if not the only) "evidence" we now have about the validity of the "presumption" about the existence of a filtration of weights on patterns.

Of course, it was part of my great program of work on motifs, of which Deligne was informed first-hand and on a day-to-day basis, to explain a notion of "Hodge coefficients" on a schematic.

p. 806 of finite type on  $\mathbb{C}$ , such that a pattern on  $X$  corresponds to a "Hodge realization", and that for smooth and pure patterns on  $X$  (e.g. those coming  $\square$  from a clean and smooth scheme on  $X$  taking his "motivic cohomology on  $X$  in dimension  $i$ "), we find the notion (more or less already known) of "families of Hodge structures" (studied in particular by Griffiths in the sixties). Moreover, for variable  $X$ , these categories of "Hodge coefficients" had to satisfy a formalism of six operations, reflecting the same formalism at the level of patterns - Deligne's contribution represents a first step towards the fulfillment of this program - namely (essentially) the description of the category  $Hdg(X)$  for  $X$  reduced to a point<sup>358</sup> (\*), and that of the "realization" functor i.e., essentially, the construction of a cohomological theory on separate  $\mathbb{C}$ -schemas of finite type, with values in this category of Hodge-Deligne structures.

## 18.4. The Dance of Death

### 18.4.1. (1) Requiem for a Vague Skeleton

**Note** 165 (February 22) Since his visit last October, and even since his letters at the end of August<sup>359</sup> (\*\*), my friend Pierre has been with me the cream of ex-students and good boys, visibly filled with a touching goodwill to clear up the unfortunate misunderstandings that have crept in between us, and to make me feel his good disposition and good faith. It was agreed that, until the planned pre-publication of *Récoltes et Semailles* by my university (USTL), he would keep confidential the content of his readings of my notes, and even their existence. I don't know if he was entirely true to his word - but I do have the impression, from various echoes that have come back to me<sup>360</sup> (\*\*\*) that he must have had a word with both of them, to suggest that this might be a good time to give a few signs of consideration to the master (the one we sometimes talk about in small groups, but carefully refrain from naming in public. . . ).

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<sup>358</sup>(\*) To get it right, we'd have to complete Deligne's definition by introducing a suitable **triangulated** category  $Hdg^*$  (Is this also the category derived from  $Hdg$ ?). That he failed to do so seems to me one of the first signs (among others of the disaffection with the yoga of derived categories and the six operations that prevailed until the "turning point of the Pervers Colloquium" in 1981.

<sup>359</sup>(\*\*) See the note "Le devoir accompli - ou l'instant de vérité" (n° 163), where I "situate" this visit, as well as the two letters of late August (received after a silence of almost two months, followed by my sending the introduction and table of contents of *l'Enterrement*).

<sup>360</sup>(\*\*\*) So I received an undated preprint from Illusie (I imagine it must be last-minute) of a talk from an unnamed seminar (a talk which, it says, does not correspond to any oral presentation in the seminar). Incredibly, my name appears in the title, but yes: "Déformations des groupes de Darsotti-Tate, d'après A. Grothendieck", by Luc Illusie! And in the introduction, there's still an arm's length of "Grothendieck" - I thought I was dreaming. Something must have happened...

There was a letter with it, where he asks for my insights on points of Grothendieck-style homotopic algebra, and wonders why "people (i.e. Quillen et al.)" in *K-theory* work with beams rather than with the complexes (pseudocoherent or perfect) of the panoply I had introduced over twenty years ago. Indeed, one wonders why... In my reply, I must have implied that it wasn't for him or any of my ex-students to ask me such questions. I haven't heard from him since.



□ I have the impression, moreover, that deep down, my friend doesn't believe (or doesn't want to believe, at least)

that I'm going to

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publish *L'Enterrement*, along with the first part of *Récoltes et Semailles*. This is very much in keeping with the image of the "sugar daddy", scrupulous about naming anyone who might feel sorry for him, and quite willing to acknowledge in public the various failings of his own making that come to mind. Reading this section on "Fatuity and Renewal", which I read briefly before my friend went on vacation and before I sent him the introduction to *L'Enterrement*, didn't worry him at all - on the contrary, it would have stimulated an air of self-satisfaction that has become quite familiar to me in him.

- that air of condescension, or at least protectiveness, towards the decidedly deceased master. It's not at all the same with *L'Enterrement*, where the cards are suddenly laid squarely on the table! I suspect that reading the introduction must have come as a shock to him - and it's a pity I wasn't there at the time, perhaps something might have happened. In any case, he gave himself time to pull himself together, before coming to see me, out of the blue, five minutes before he was due to move to the States. And he came in such good spirits, and the meeting took place in such a family atmosphere, so "cakey", that it seems to eliminate, so to speak "by the absurd", that the aforementioned sugar daddy could himself take seriously a certain text that hardly resembles him (let's say no more about this text, which is best forgotten. . . ), and even spread it among people who are just as reasonable and "well" under all circumstances.

relationships, that my friend Pierre himself and that the ex-deceased as he always knew him...<sup>361</sup>(\*)).

□ As he had promised, and in the very days following his return to Bures, my friend made me I received this biographical note he'd told me about, which he'd written in 1974 (or 1975) for the Belgian Fonds National de la Recherche Scientifique<sup>362</sup> (\*). It's a fairly short text, two short pages, which I read with interest at the time, and which I've just reread (I think it's the third time I've read it). At first glance, however, I didn't feel that this text offered anything new, and that it deserved a closer look in *L'Enterrement*. It's true that the technique of escamotage, with which I was already sufficiently familiar in my friend's work, is illustrated here in a particularly striking way, in a compact text of around a hundred lines. My name appears four times (as does Serre's, and Weil's three times) - with nothing to suggest that he may have met me other than as an anonymous listener at my seminar (on an unspecified theme) in 1965-66. In three of the four passages in which I'm mentioned, I'm mentioned in one breath with another mathematician (twice Serre, once Rankin), so as to avoid giving the impression that I played any special role with him. This is a technique that has already proved its worth elsewhere<sup>363</sup> (\*\*). As it won't take long, I'll take the liberty of quoting in extenso the three passages in which my modest person appears, to enlighten readers who, like me, don't have access to the text of the biographical note.

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The third paragraph continues with the evocation (just given) of the year 1965-66, spent "in the ideal atmosphere of the Ecole Normale Supérieure as a foreign boarder"<sup>364</sup> (\*\*\*):

□ "In Paris, I attended Grothendieck's seminar and J.P. Serre's course. Three hours of lectures

p. 809

<sup>361</sup>(\*) However, at no time was there any hesitation in my intention to make all my notes on Burial public, in the same way as the first part of *Harvest and Sowing*; and I have, of course, left no ambiguity on this subject.

<sup>362</sup>(\*) This biographical note is mentioned for the first time in the last footnote to "Le nerf dans le nerf - ou le nain et le géant" (n° 148). See also the end of previous note n° 164 (part V 2).

<sup>363</sup>(\*\*) I'm thinking here of the laconic one-line reference, quoting in a breath Serre (without naming him) and "the conjectural theory des motifs de Grothendieck", in Deligne's announcement (at the Nice Congress) of his results in Hodge theory. For further details and comments, see sub-note n° 78' of the note "La victime" (n° 78').

<sup>364</sup>(\*\*\*) For some reason, Henri Cartan is not named here. Perhaps it's because Deligne, encouraged by a certain deliberate intention I had for him (see note "L'être à part", n° 67'), was to carefully avoid any appearance that he might have been anyone's pupil. The situation of "normalien" immediately gives rise to the association of ideas "pupil of Cartan", and a such an association would have been strengthened by mentioning Cartan by name.

per week but, despite working happily and relentlessly, the rest of the week was barely enough for me to assimilate them (165<sub>1</sub>). From Grothendieck I learned the modern techniques of algebraic geometry, from Serre the fascinating beauty of number theory (165<sub>2</sub>). Serre's lectures were devoted to the theory of elliptic curves, where...",

to continue on the charms and variety of these Serre courses. The reader not in the loop will think that it was these courses, at a rate of three per week, that were the object of the "happy and relentless work" of which the author speaks (implying: no need for work to assimilate the "greatest natural generalities" of a Grothendieck seminar. . . 165<sub>1</sub>).

In the fifth paragraph, in connection with his demonstration of Weil's conjectures, we read:

"My most notable achievement is to have proved the "Weil conjectures" (. . .). I undoubtedly achieved this for being familiar both with Grothendieck's work and, in an entirely different field, with Rankin's work on modular forms."

Admire the dubious "sans doute" (masterfully placed there!) and the "dans un tout autre domaine" (sug-  
gérant que mon oeuvre n'aurait rien à voir avec les formes modulaires<sup>365</sup> (\*)), and above all the "tant avec"  
with which I have the honor of being introduced, to equate the vast groundwork I had done<sup>366</sup> (\*\*), with a  
"punctual" technical idea borrowed from Rankin.

Finally, in the next paragraph referring to Deligne's work on Hodge theory, it says:

□ "Inspired by arithmetic, and more particularly by Grothendieck's conception of the deeper meaning of Weil's conjectures, I generalized (non-trivially) his theory to the case of arbitrary varieties and (in collaboration with Sullivan) to other "form" invariants than just cohomology. The root of this theory is already old, with Picard's treatise on "algebraic functions of two independent variables" (circa 1890), but we probably know little more today than a vague skeleton."

p. 810

I had to take the trouble to recopy this passage, only to realize that "Grothendieck's conception of the deeper meaning of Weil's conjectures" was my brilliant ex-student's masterly "thumb" way of not naming the **motives**, though he could not be blamed for passing them over in silence! There's no doubt that "his [hence, **my**] theory", about which I'm only just wondering (this whole passage had escaped my attention in previous readings), can only mean the famous theory of motifs, which there had been no question of mentioning by name for four years already (and which we won't be mentioning for another eight!). The formulation was even so vague and, to put it bluntly, incomprehensible except to a small handful of people in the know (who, like me, will doubtless not have had the opportunity to read this pre-Funeral Eulogy), that it wasn't even worth pointing out here that this "theory" (which he had generalized) was, nonetheless, entirely conjectural! The "generalization" in question can hardly mean anything other than the Hodge-Deligne theory, given the context. It's a little symbolic satisfaction that my friend is giving himself, by asserting here (without fear of ever being contradicted, given the location, and the vague elusiveness of the formulation) that the theory

<sup>365</sup>(\*) It's true that "modular forms" represent an unfortunate hole (among many others) in my mathematical culture, just like analytic number theory, on which I've never yet "latched on". Still, I'm sufficiently well-informed to know that an understanding of modular forms is hardly conceivable without the ideas coming from algebraic geometry, which gives the theory its "geometric" content, and that the deepest questions of modular form theory are intimately linked to the (long unspoken) presence of **motifs**. As we shall see, they also appear, just as tacitly, in the next paragraph of the biographical note (aka Eulogy (3)!).

<sup>366</sup>(\*\*) On the notion of schema and the development of a formalism of staggered cohomology, to which Deligne is careful not to allude, except in the preceding quotation by the kindly and impersonal euphemism "modern techniques of algebraic geometry" .

of Hodge-Deligne (which still remains in its infancy) would "generalize" the vast picture of patterns I had shown him. In the latter, however, a fully matured "Hodge theory" appears as one of the "planes" of the picture among many others<sup>367</sup> (\*) As for "other invariants of form", it was "well known" to me as early as the sixties (as part of my "yoga of patterns") that algebraic varieties

"arbitrary" (as Deligne insists) had a "motivic homotopy type", whose  $\pi_i$  higher ( $i \geq 2$ ) generalize the fundamental "geometric" motivic group, and are explained (for a given fiber functor on a number field  $K$ ) as affine algebraic pro-groups on  $K$ .

□ As for the reference to Picard as "the root of this theory", this is, it seems to me, an entire passage- The term "vague skeleton" was introduced for the double reason of "looking good", and at the same time to introduce the final paragraph, which immediately follows<sup>368</sup> (\*). The term "vague skeleton" also seems to me to be the expression of another "symbolic satisfaction" that my friend is paying himself, by treating inwardly and yet without seeming to do so (always in the same "thumb!" style) this vast vision from which he has secretly drawn inspiration, while keeping it buried<sup>369</sup> (\*\*), as nothing more than a "vague skeleton".

p. 811

In the end, these all-encompassing escamotages turned out to be more interesting than I had anticipated, when I was about to point them out in passing, out of a sense of conscience. What strikes me most now is not (as on my first, quick and superficial readings) the perfection of the "pouce ! It's rather that this text, written nine years before the *Eloge Funèbre*<sup>370</sup> (\*\*\*), foreshadows the latter in a striking way, and this (it seems to me) in two ways. On the one hand, by the vague rigor that must surround every appearance of my modest person (as opposed, here, to the luxury of technical detail that accompanies the evocation of the *Cours de Serre*). On the other hand, and in the same vein, the complete silence surrounding

□ de étale or *l-adic* cohomology, as a new and essential tool that I have developed from nothing, and without which Weil's conjectures would probably not be demonstrated even a hundred years from now! In fact, as in the Eulogy, the word "cohomology" is not mentioned in connection with my name - nor is there any allusion to the fact that Deligne's demonstration of Weil's conjectures was simply **the last step** in a long journey, the longest and most innovative part of which was accomplished by someone other than him, even before my brilliant pupil appeared on the mathematical scene<sup>371</sup> (\*).

p. 812

**Note 165<sub>1</sub>** As I point out a few lines further on, the wording irresistibly suggests that the "three hours a week" refers to the "J.P. Serre lectures" just mentioned, and referred to again two sentences later. In fact, Serre only gave one lecture a year (at the Collège de France), for one hour a week. If we try to resolve the ambiguity by interpreting the text as

<sup>367</sup>(\*) (February 27) For further details, see "La Mélodie au tombeau - ou la suffi sance" (n° 167).

<sup>368</sup>(\*) This final paragraph will be the subject of the note (n° 165) which follows this note.

<sup>369</sup>(\*\*) The vision of patterns remained "buried" in two ways. On the one hand, with regard to the **outside**, the mathematical public, by refraining from any allusion to the notion of pattern (except in Hodge I's half-line "inch!", in 1970, cf. note 78'), until 1982 when the notion was exhumed "with great fanfare" under the tacit paternity of Deligne (see notes n° 51 and following). But on the other hand, even for his own use, I can see that this vision has been stripped by Deligne of its true **breath**, of that which makes it was **more than just** a collection of all-purpose recipes (for getting to grips with the cohomology of algebraic varieties), but a **dream-force** vast and deep enough to serve as an inspiration, a line on the horizon, for perhaps generations of arithmetician geometers.

The term "vague skeleton", by which Deligne refers (always tacitly) to this vision, captures the **gravedigger-like** disposition in which he maintains himself, in his relationship to this dream and to the worker from whom the dream springs. These are not the attitudes in which one can still feel a breath (as he had once felt), nor embody a dream. You don't embody a dream by **using it** for your own ends (and denying it at the same time. . .), but only by **making** yourself its **servant**.

<sup>370</sup>(\*\*\*) See the two notes "L'Eloge Funèbre (1) - ou les compliments" and "L'Eloge Funèbre (2) - ou la force et l'auréole", n° s 104, 105.

<sup>371</sup>(\*) This contribution by another is glossed over by Deligne under impersonal terms such as "modern techniques [or, elsewhere, "powerful tools"] of algebraic geometry".

referring to Serre's "courses" in successive years (contrary to what the context suggests), we come across another inconsistency, since Serre changed his theme every year, and by no means limited himself to that of elliptic curves (as stated two sentences later).

While Serre's persona is used here by my friend to try and give the lie to the role I played in the crucial years of his mathematical training, it's interesting to note that the one and only reference I'm aware of in the literature to the fact that Deligne was my pupil comes from Serre's pen, thus repairing (without noting) the glaring omissions of my brilliant ex-student himself. This is the report Serre wrote in May 1977 on Pierre Deligne's work, for the International Committee responsible for awarding the 1978 Fields Medals. This report was made public after the Fields Medals were awarded at the 1978 Helsinki Congress. The report begins:

"Deligne's first works, directly inspired by Grothendieck, whose pupil he was, concern various technical points of algebraic geometry. I'll just mention them: . . ."

p. 813 □ Further on, Serre also mentions the influence of my ideas and results in the demonstration of conjectures of Weil, and (via motifs) in Deligne's work on modular forms, but not in the Deligne-Mumford work on modular multiplicity of algebraic curves of type  $(g, \nu)$ , nor in the idea of Hodge-Deligne cohomology, whose relation to the yoga of motifs and Weil's conjectures seems to have escaped him. (True, Deligne did his best to hide it.)

The speech on Deligne on the occasion of the award of the Fields Medal would have been another opportunity, in accordance with established practice, to publicly remind people of this link to me, which had been kept quiet until then by the person concerned. For some reason, the mathematician in charge of presenting Deligne's work was not J.P. Serre, but N. Katz, the "co-author" with Deligne of SGA 7 II (see note n° 164 (II 5)). Needless to say, N. Katz makes no mention of the link in question, which was well known to him at first hand. (On the other hand, he does, incidentally, make good a number of the illustrious laureate's rather embarrassing omissions about me... .)

**Note 165<sub>2</sub>** The choice of qualifiers here ("modern techniques" for me, "fascinating beauty" for Serre) is certainly no accident. I clearly perceive in it my friend's intention to evacuate (symmetrically) precisely that **fascination** which, since our meeting (and perhaps even before that) had bound him to my person and my work, which he saw being made and unfolding before his eyes, day by day.

On other occasions, I've noticed a deliberate intention on the part of my friend to view and present my publications (notably the EGA ("Eléments de Géométrie Algébrique") and SGA ("Séminaire de Géométrie Algébrique du Bois-Marie") as a kind of "compilation" of more or less technical results, which "everyone" has always known about, and for which I would make the laudable effort of putting them in black and white, in order to finally provide the missing references so that no one would talk about them any more. He knows, however, what he's getting at: that each volume of the EGA and SGA presents ideas that I introduced and of which I was the sole holder and advocate for years, and techniques that no one had dreamed of (except me), and which I had to develop, test and perfect with tireless patience, before they could be used by the public.

p. 814 perfectly honed, ready to enter the realm of the "well known". He knows this better than anyone, but at the same time, this deliberate purpose he has been displaying □ depuis depuis plus d'une décennie has ended up becoming a "second nature", he himself became the first (if not the only) dupe.

I was reminded of this only a few weeks ago, when my friend, who has been very considerate of me since his visit to my home in October, sent me a copy of an exchange of letters with Dr. Heinze (in charge of "Ergebnisse der Mathematik" at Springer) about a project to reissue the EGA (of which many

With one exception (the second part of EGA III, where the presentation would have been better using derived categories (sic!)), this treatise "has aged very well". Its great merit would be to provide indispensable references: "Thanks to it [EGA], in algebraic geometry (as opposed to analytic geometry, for instance) one can march securely on the ground without having to worry if this or that is indeed in the literature". (He follows this up with a number of constructive suggestions, about possible appendices that could be added to some of the volumes, and mathematicians who would be able to provide them. ... )

It is typical of Springer's relationship with me that this correspondence (about the republication of books I had authored) continued **with Deligne**, and without Springer having deemed it necessary to inform me about the project in the first place. It was more than a month later (in a letter dated 24.1) that Dr. Heinze told me in passing, as a matter of conscience, about the matter - that Mr. Professor Deligne "had been kind enough to give me a copy of his letter of 19.12.84" (it was really kind. . . ), and that "of course, we [Springer] would be interested to know your opinion on this subject [the republication project]" (it's really too much of an honor. . . ). I replied that, in view of Springer's publishing procedures (thinking of publishing SGA 7 and SGA 5 in Lecture Notes, without even informing me, let alone asking for my agreement), it seemed to me perfectly superfluous to inform Springer Verlag of "my opinion", which was obviously irrelevant. That's where things stand...

## 18.4.2. (2) The profession of faith - or the truth within the falsehood

**Note 166** (February 23) In the end, I didn't get to my real point yesterday, talking about my friend Pierre's biographical sketch. The "vague skeleton" encounter (a.k.a., pattern theory) has been a unexpected episode, just as I was about to move on to the final paragraph of the notice, *sui generis* immediately the last passage quoted. So here, at last, is the final word in the "biographical note", which is what I wanted to get to all along:

p. 815

"In conclusion, I would like to emphasize how precious to me is contact with the work of past mathematicians (from 1800 to the present day), whether direct or relayed by scholars more erudite than myself, such as A. Weil and J.P. Serre. We "are dwarfs perched on the shoulders of giants", and the finest modern mathematical theories are motivated by the hope of solving some of the problems they bequeathed us.

Pierre Deligne

As is often the case, my first reaction to these lines, a sort of profession of faith in this case, stopped at the surface, at the literal meaning - but I must have sensed, however, that beyond the literal meaning there was something fishy going on. This quotation (from a famous mathematician, no doubt, whom I was supposed to have read, "like everyone else") wasn't coming back to me. I sensed a deliberate attempt at modesty, even humility, which had all the hallmarks of a pose, and which simply didn't correspond to the simple reality of things. If each generation were "smaller" in size than its predecessors, the human species would have long since died out, reduced to a paltry mass of homunculi! I'm well aware that human creativity is no less today (nor, no doubt, greater) than it was a hundred years ago, or a hundred centuries ago. I'm also well aware, speaking only of maths, that the ideas and work of people I've known well, without excluding myself from their number, would have been to the credit of even the greatest mathematicians of the past. And I'm well aware that **my** motivation in doing math,

and certainly not that of most of my former friends in the mathematical world<sup>372</sup> (\*), lies in the "hope of solving some of the problems" bequeathed by my predecessors! If it were otherwise, our science would be powerless to renew itself - it would have ceased to be creative.

What must have shocked me even more about this borrowed profession of faith, or to put it more accurately, **pained** me, was that I knew above all that the person who made it, more than any other person in the world I had known, had shared "means" that had amazed me, and that I had also known to him.

p. 816 a "freshness" in his approach to mathematical things, whereby he was called upon to do great things, as few mathematicians have had the privilege of doing. There was in me a sorrow, and also like a spite, because behind the pose of one who claims to have found humility in dealing with the great men of the past, I sensed an **abdication**. An abdication of that creative force within him, which he seemed to have forgotten a long time ago, and which made him **something** quite **different** from what was suggested by that derisory image of the dwarf perched on the shoulders of a giant<sup>373</sup> (\*).

This is the first time, since my first reading of the biographical note, that I've tried to pinpoint what feelings this reading first aroused in me. In the days that followed, and without any deliberate intention on my part, it continued to work. It was this last passage in particular that kept running through my mind, like something decidedly unusual, and which hadn't "gone away". Behind the apparent absurdity of the profession of faith that closes this short biographical text, I must have sensed a **meaning**, which was undoubtedly directly perceived at an unconscious level, and which gradually rose to the superficial layers, without there being any reflection as far as I could remember. After all, I knew that my friend Pierre wasn't in the habit of haunting the writings of the past any more than I was. While he certainly read more than I did, it wasn't the old grimoires, but rather the latest reprints and preprints circulating in well-informed circles, of which he was always the first to have access. And I also knew that it wasn't from Picard or other venerable precursors of the last century, or even of this century, that my friend had drawn the inspiration that had nourished his work since (and even before) my departure from the mathematical scene! And if it's true that he had enjoyed "perching on someone's shoulders", not in a public and rhetorical profession of faith, but secretly and **genuinely**, I was after all in a good position, since I'd been reflecting on a certain Burial, to know **who** had been the one to do it, so to speak,

p. 817 the costs! In place of Celui-qu'on-nommais-jamais<sup>374</sup> (\*\*), (and who nevertheless remains ever present. . . ) we verbally substitute "the great men of the past", to whom in the preceding paragraph we come from elsewhere all just to tacitly attribute authorship of the motifs (a.k.a. "what today is little more than a vague skeleton") - thus making the **true** identity behind the surrogate figure all the more striking. ...

I've observed time and again that there's a force within man, apparently universal in nature, that pushes him to express against all odds, often in a roundabout and symbolic way, desires and intentions (both conscious and unconscious) that cannot be manifested openly, thus giving them an outlet and satisfaction that may seem derisory (in "rational" terms and according to current consensus), but are no less substantial. It's a force, in a sense, that pushes us, as if in spite of ourselves, to proclaim the truth of our being to whoever will listen (and there's "someone" in all of us with a keen ear. . . ), **even though** what is thus "proclaimed" would be the greatest secret and would be anathema, before others as well as ourselves. The ideal terrain for the expression of this force is the dream, and this is one of the reasons why the dream is the most powerful key of all for us to enter into the world of our dreams.

<sup>372</sup>(\*) Including, incidentally, Pierre Deligne himself!

<sup>373</sup>(\*) (February 25) This impression of "abdication" is strongly associated with that aroused by a certain "third part" to my Funeral Eulogy. See the reference to it at the end of the note "L'Eloge Funèbre (2) - ou la force et l'auréole" (n° 105), p. 459-461.

<sup>374</sup>(\*\*) Or, if we can't avoid it, we'll call it "by the tape", in the de rigueur "thumb!" style... .

knowledge of ourselves. But because of the intimate, personal nature of dreams, which speak to us about ourselves to no one but ourselves, this means of expression is by no means sufficient for us, as it is unfit to assert the truth of our being **before others**, or even, symbolically, before the whole world. This is why, behind every nonsense that seems to defy reason, a "meaning" is hidden - or to put it better, nonsense is the **privileged means of expression**, chosen by the unconscious with infallible instinct, to **proclaim this meaning**, both hidden and ostentatiously displayed before everyone<sup>375</sup> (\*)!

This is surely what I felt darkly, in the days that followed my reading of this "nonsense": the "dwarf" (born to be a giant) perched on the shoulders of a "giant" (of much more modest means). than those of the so-called "dwarf", perched on top of him while denying him... .). One of the reasons<sup>376</sup> (\*\*) □ for my difficulty in p. 818

to become clearly aware of the meaning revealed by this nonsense, was undoubtedly my reluctance to recognize myself in this cookie-cutter image of the "giant"; or rather, perhaps, to recognize myself in a certain pose or brand image which was indeed mine and which, through the unexpected tricks of this grating nonsense, was suddenly calling out to me! It wasn't until weeks later, in the December 18 note "Le nerf dans le nerf

- ou le nain et le géant" (n° 148), that I finally return to the unusual image of the dwarf and the giant, this time by working on pieces, at a time when the context of reflection on the Burial was all set to welcome it. This image immediately revealed itself (on the very same day) as an "image-force" crucial for understanding my friend's relationship to me, and more profoundly and above all, for the beginning of an understanding (doubtless destined to remain forever fragmentary) of my friend's relationship to himself, i.e. also: of **the** particular form taken by **division in his own person**. And insofar as L'Enterrement was implemented, before any other, by my friend's ex-student and ex-heir<sup>377</sup> (\*), it is this same image that now appears to me as **the** neuralgic force obstinately at work throughout this long Burial, as its true **nerve**. It is at the center of reflection in the fortnight following the crucial moment of its appearance in the notes, throughout the nine notes that follow one another, between December 18 (with the aforementioned note "Le nerf dans le nerf - ou le nain et le géant") and the December 3 note, "Le Frère ennemi - ou the handover" (n° 156).

The "validity" of the role of neuralgic image-force taken on in my thinking by this image of anodyne appearance, that is to say, also, the question of **the** □ **reality**, in the psyche of my friend himself, of such an image- p. 819  
force, the expression of deep-seated conflicts and the driving force behind irrepressible acts of compensation<sup>378</sup> (\*) - this question, it seems to me, cannot be settled by a "demonstration", i.e. by a so-called "demonstration" approach.

<sup>375</sup>(\*) For another particularly ostentatious example of **meaning** proclaimed by apparent nonsense, see the note "La plaisanterie - ou "les complexes poids"" (n° 83). See also the comments in the note "La surface et la profondeur" (n° 101), particularly at the end of the note (p. 440), and in the following one, "Eloge de l'écriture" (n° 102).

<sup>376</sup>(\*\*) Another reason, which seems to me to have been the main obstacle, is a certain **inertia**, or more precisely, a kind of **pusillanimity** in "believing the testimony of one's eyes, even though what one sees is quite unheard of, never seen before and ignored and denied by all". I was confronted with this again recently in the note "Le devoir accompli - ou l'instant de vérité" (n° 163). See in particular the b. de p. note (\*\*) on page 782, where I probe this kind of "incredulity" in the face of the obvious. ...

<sup>377</sup>(\*) It's true that in this "implementation", he acted in close connivance with "The whole Congregation", to whom he served as a kind of instrument for the accomplishment of a collective will. (See note "Le Fossoyeur - ou la Congrégation toute entière", n° 97.) But it's possible that this same image-force that I perceived in my friend, was also present at the level of a "collective unconscious" in the said Congregation, finding its expression in the unconscious of many of its members, including some of my students (and not just Deligne).

(May 12) This intuition has come a long way since these lines were written, and now it imposes itself on me with the force of evidence. On this subject, see the note "Le messager (2)" (n° 181).

<sup>378</sup>(\*) By the term "irrepressible", I in no way mean to suggest that the presence of this force has become a kind of inevitability. that would have escaped my friend's responsibility. The action of such a force within us is "irrepressible" only insofar as we enjoy and persist in evading knowledge of it, in order to cash in on the various benefits and gratifications we "buy" through this deliberate "ignorance". The price is exorbitant, it's true, but ignoring that price too is part of the same deal.

It's an "objective" fact that's bound to win the support of any good-faith and sufficiently informed interlocutor. For me, this reality is beyond doubt, and my firm conviction is not the result of such a "demonstrative" approach. It is true that it has deepened in the course of the fifteen days of reflection I mentioned earlier (a reflection which I won't attempt here to "summarize" or "assess"). But it was there from day one - from the moment I took the trouble, for the first time since my reading, to write down in black and white what it inspired in me, as if under the dictation of a silent voice<sup>379</sup> (\*\*) which then "reminded" me of what, deep down, I already "knew". I had to "know" it, by means of faculties of perception that are by no means extraordinary, but incomparably more unbound than those we commonly allow to come into play at the level of **conscious** awareness of things. These mechanisms of repression of what is perceived "somewhere" within us, and which doesn't "fit" with the routine logic of our received ways of seeing (or rather, **not** seeing) reality around us - these mechanisms, needless to say, are as strong in me as in anyone else. If there's a difference in this respect between me and others, it's that I've come to realize their silent action within me, and especially since I sometimes "meditate": that I sometimes take the trouble, prompted by an indiscreet curiosity, to **put down** on those things I wish to know, which has the effect of **bringing to the surface** of consciousness what was obscurely perceived in deeper layers and giving it form.

p. 820      □ The initial perception, moreover, is transformed in the course of the **work**, which gives it shape while bringing it

out into the open. This work is at the same time a **decantation**, by which little by little the conscious translation of perception (into intelligible words) frees itself from the subjective a-prioris that unknowingly tainted it. In this case, one of these distorting a-priori (detected in the last of the notes quoted earlier) is the inveterate mechanism within me that leads me to "see myself as yang", and this even in situations where, visibly, it's the yin side of my being, "the woman in me", that provides the key to understanding (or at least, one of the keys, or "illuminations", indispensable for a nuanced understanding). Elsewhere, I've talked about the **signs** - all "subjective" but nonetheless unmistakable - that tell me the **progress** of such work<sup>380</sup> (\*), and others that warn me when I'm on the wrong track, or when there's momentary stumbling, which ends as soon as it's detected.

### 18.4.3. (3) La mélodie au tombeau - or sufficiency

**Note** 167 (February 25) Most of yesterday was spent writing a long letter to a young colleague, Norman Walter, who seems motivated to take up pattern theory, unimpressed by a decidedly unpromising economic climate. This time, it was eight tight pages (typewriter), on the "six operations" for pattern categories and for the most important "coefficient categories". It made me realize again, with amazement, that in the twenty years or so that the question has been asked (not in the literature, admittedly. . . ), none of the "good" categories of "usual" coefficients (sic!) for the cohomology of schemes has yet been defined, with the sole exception of the "l-adic coefficients" for *the* first to the basic scheme  $X$ ; and even this work, in the framework of triangulated categories (indispensable for the six-operation formalism), carried out in Jouanolou's thesis, has never been published. I myself have never held a copy of Jouanolou's work in my hands.

<sup>379</sup>(\*\*) This image of "dictation" by a "silent voice" has come to me more than once, I believe, in the writing of *Récoltes et Semailles*, and each time as a matter of course. This is by no means the repetition of some "stylistic effect", but reflects (it seems to me) a common aspect, more or less evident from one situation to another, of the process of discovery.

<sup>380</sup>(\*) On this subject, see the note "L'enfant et la mer - ou foi et doute", n° 103.



thesis by this student<sup>381</sup> (\*\*). These are striking signs of the general disaffection with the program of foundations that I had undertaken in the sixties, and which I certainly would not have suspected would not continue in the same vein, but would be broken off (or "cut up". . . ) as soon as I left the mathematical scene. . .

When the prime number  $l$  is **nilpotent** on the scheme  $X$ , the category of " $l$ -adic coefficients on  $X$ ",  $Z_l^*(X)$  let's say<sup>382</sup> (\*), should be none other than that of "crystalline coefficients", with Frobenius operation  $F$  and **filtration** to boot. The construction in form of this triangulated category, not to mention the

six operations, is still waiting for someone to do it. As for the "recollement" of the "ordinary"  $l$ -adic case (although not found!) and the previous "crystalline" case, via a "mysterious functor" that I foresaw as early as the late 1960s, to arrive at the definition of the unrestricted coefficient category  $Z^*(X)$

on  $l$ , it is still not done even in the simplest non-trivial case of all,  $X = \text{Spec}(Z_l)^{(*)}$  As for the De Rham-Hodge coefficients  $DRHdg^*(X)^{383}$  (\*) for a general scheme, I had little precise idea how to describe them, and Deligne failed to pin them down in a truly satisfactory way. The idea Zoghman Mebkhout is the author of this innovative work - and we know what adversity he had to work under, and what fate befell his person, once the scope of his ideas had been (very partially) re-known. The fact remains that we now have a reliable guideline for approaching a construction in the form of  $DRHdg^*(X)$ , in terms of conditions of finiteness, holonomy and regularity on complexes of "crystals" (absolute - i.e. relative to the absolute base  $\text{Spec}(Z)$ ?), with perhaps the additional data of a "De Rham filtration" and another "filtration by weights" - and with the hope that we may arrive at

<sup>381</sup>(\*\*) Jouanolou's thesis, written without any real conviction (which set it apart from all my other "students before I left"), dragged on and on, and was not defended until after 1970. As with Deligne's thesis, I don't recall being informed of the defense, let alone being asked to sit on the jury. Jouanolou did not see fit to send me a copy of his work. I wrote to him last year to request one. He informed me (without comment) that, to his regret, there were none left. . .

(May 12) My memory misled me here - in fact, Jouanolou's thesis was defended as early as 1969. For details, see the final note (still unwritten at the time of writing) n° 1767, in the suite "Le sixième clou (au cercueil)".

<sup>382</sup>(\*) The sign \* after the indication of the base ring for the chosen theory (here, the ring  $Z_A$ ) indicates that we are working, not with "constructible bundles" without more ( $l$ -adic in this case, in a suitable sense) but with "constructible" **complexes** of bundles, objects of suitable triangulated categories (whose description in form can be tricky, even though the category of constructible bundles, in this case  $Z_A(X)$ , would already be known). When working with patterns (by which, more often than not, we mean "iso-patterns", i.e. "isogenically close patterns", forming a  $Q$ -abelian category), the natural coefficient categories for "realizing" such (iso)patterns must themselves be  $Q$ -abelian.

take  $Q_A(X)$ ,  $Q^*(X)$ . When we want to work with all  $l$  at once, the most natural thing is to work with a category of "adelic" bundles (or complexes of such), whose base ring is the ring of adels  $\hat{Z} \otimes_Z Q$ , obtained by "tensorizing" the product of all categories of coefficients  $Z^*(X)$  by  $Q$ .

Note that when the prime number  $l$  is not prime to the  $X$  scheme, then in the description of the "coefficients  $l$ -adic" elements on  $X$ , the nilpotent elements of  $Q(X)$  cannot be neglected - they intervene in the vicinity of the fibre  $X(l)$  of  $X$  in  $l$ . A fortiori, the same will be true of the adelic coefficients on  $X$ , which brings them closer to the coefficients (just as hypothetical for the moment) of De Rham-Mebkhout, discussed in the next paragraph. In fact, I have the impression that the two main types of coefficient, the adelic coefficients and De Rham-Mebkhout's coefficients (provided the latter are equipped with all the richness of structure alluded to below), are of comparable "fidelity", as (weakened) descriptions, or "realizations", of the same **motif**, very closely circumscribed by one as by the other. In the sixties, I put forward some conjectures about this "fidelity", similar to those of Hodge and Tate (which my friend buried with the rest. . . ). I intend to return to them in the volume of *Réflexions* that will be devoted to the "vast array of motifs". One senses a strong kinship between the two types of coefficient (adelic, De Rham-Mebkhout, the latter taken here "within isogeny"). The advantage of the latter over the former, which makes them appear "more fine" in some respects, is that the natural base ring for them is  $Q$ , whereas it is the (much larger) ring of adels for the adelic theory.

<sup>383</sup>(\*) (May 12) As we'll see below, this "improvised" name and notation prove to be inappropriate. I have finally opted for the notation  $DRM^*(X)$  or  $Meb^*(X)$ , dual to  $DRD^*(X)$  or  $Del^*(X)$ , for the coefficients of De Rham-Mebkhout and De Rham-Deligne respectively. The latter were left behind by their father in 1970, and adopted by in the year of our Lord 1985, as one of the basic ingredients (along with Mebkhout's coefficients) in the Grothendieckian panoply. ...

make something, moreover, that holds up without restricting itself to the null characteristic, and which for a given positive characteristic more or less gives back the "habitual" (sic!) crystal coefficients. The extraordinary thing is that I seem to be the only person in the world to feel the task - Zoghman Mebkhout himself, no doubt instructed by bitter experience, doesn't seem to have the slightest inclination to think for even one more day about questions of the foundations of **his** philosophy! It would be wrong of me to be surprised by this, as I see Deligne preaching by example with Hodge's theory, cutting short his own impetus, which had animated him "in my day" and brought forth an approach rich in promise (unfulfilled. . . ). I suspect that the formalism (not yet even in limbo) of Hodge coefficients (above complex algebraic varieties  $X$ ) should be more or less contained in that of the coefficients I used to call (following my language reflexes of the sixties) "De Rham coefficients", or also "De Rham-Hodge", to recall the link between the **filtered** De Rham object and the associated **graded** object (called "Hodge"). But given the crucial role played by Mebkhout's philosophy in understanding these categories of coefficients (which are still hypothetical, of course), it would probably be better to call them "**De Rham - Mebkhout coefficients**" ( $DRM^*(X)$ ) or, at a pinch, "De Rham-Hodge-Mebkhout coefficients",  $DRHM^*(X)$ . When  $X$  is of finite type over the complex field  $\mathbb{C}$ , we should be able to reconstruct the hypothetical  $Hodge \boxtimes HDG$  coefficient categories<sup>384</sup> ( $X$ ) (which I certainly wouldn't call Hodge-Deligne, whereas Deligne

It seems to me that we've done everything to hide the problem, far from highlighting it!), in a more or less "tautological" way, as well as the six operations on them, based on De Rham-Mebkhout coefficients, to which we simply add an additional structure (of a transcendental nature) called "de Betti". It seems to me, therefore, that the main issues in describing "categories of 'natural' coefficients" for the cohomology of algebraic varieties<sup>384</sup> (\*) are currently as follows:

1. Description of the category of  $l$ -adic coefficients  $Z_l^*(X)$ , for  $l$  given prime number and for **any** scheme  $X$  (not necessarily "prime to  $l$ "), and a formalism of the six operations for these coefficients. (This question appears more or less equivalent to that of the "mysterious functor").
2. Description of the  $DRM$  category<sup>\*</sup> ( $X$ ) of "De Rham-Mebkhout coefficients" for any scheme  $X$ , or possibly, of analogous  $DRM$  categories<sup>\*</sup> ( $X/S$ ) for relative schemes (

$$DRM^*(X) = DRM^*(X/\text{Spec}(\mathbb{Z}))$$

), and a six-operation formalism for these coefficients.

For 2), there may be several possible variants, depending on the richness of structure we decide to introduce into these coefficients. In any case, the "theorem of the good Lord" (aka Mebkhout) shows us a priori (for  $X$  of finite type over the field of complexes, at least) that there must exist a formalism of the six variances for crystalline coefficients à la Mebkhout, without having to introduce "over the top" filtering à la De Rham or/and by weight. A third important type of additional structure, which is bound to exist on the De Rham-Mebkhout crystal complex  $K_{\text{sur}} X$  associated with a pattern (or "absolute coefficient") on a general  $X$ -*scheme*, will be the giving for any prime number  $p$  of a "Frobénius"

$$K(p)^{(p)} \rightarrow K(p)$$

where  $K(p)$  denotes the restriction to the subscheme  $X(p)$  deduced from  $X$  by reduction mod.  $p$ , and where the exponent

$(p)$  denotes the "Frobéniusé" of  $K(p)$ , i.e. its inverse image by Frobénius  $X(p) \rightarrow X(p)$ . Thus, according to

<sup>384</sup>(\*) In a sense, these questions are preliminary (or tacitly assumed to have been resolved) to the development of the yoga of motives with all the precision and generality it deserves, and which I saw as early as the 1960s.

additional structures (among the three we have just named) that we can propose to introduce on a crystalline complex, we can foresee a priori a total of **eight** variants, for a notion of "coefficients of De Rham-Mebkhout". It is a work on coins only that will be able to show us which of these variants p. 824 give rise to a formalism of the six operations. It's also true that, for the purposes of pattern yoga, when the aim is to find simple "algebraic" objects that "stick" as closely as possible to the patterns, in order to describe their structure as faithfully and richly as possible, it's the "richest" coefficients that a priori seem "the best". It was in their richness that the main charm of Hodge's coefficients lay - so much so, in fact, that we could hope to reconstruct from scratch the category of patterns on  $C$  (if Hodge's conjecture were true), and even those of patterns on any  $X$  of finite type on  $C$ .

This reminds me that it's possible for some of the structures to be "superfluous", that they follow from the others (but in a way, it's true, so hidden, that it'll be hard to spell it out in down-to-earth terms)<sup>385</sup> (\*). For example, on the De Rham cohomology (relative on  $S$ ) of a scheme  $X$  smooth on another  $S$ , I demonstrated (towards the end of the sixties)<sup>386</sup> (\*\*) the existence of a canonical (absolute) curvature-free connection, which I called **the Gauss-Manin connection**. As a result, the Hodge-Deligne structure associated by Deligne with a smooth  $X$ -scheme on  $C$  (and surely even that associated with any finite-type scheme  $X$  over  $C$ ) is canonically equipped with such a connection, relative to the prime subbody  $Q$ . If anything, the motivic cohomology itself can already be reconstituted from its "realization p. 825 de Hodge", this means that on any Hodge structure that could be called "motivic" or "algebraic" (i.e. originating from a pattern), there would be such a canonical Gauss-Manin connection. It would not be difficult, then, to describe other, more subtle, canonical structures associated with a Hodge-Deligne structure, whose existence "follows from the pattern": the existence of operations of certain profinite Galois groups, for example.

on  $Bet(K) \otimes_{\mathbb{Z}} \mathbb{Z}_l$  (where  $Bet(K)$  is the "network" underlying the Hodge-Deligne  $K$  structure), and "structure Frobenius" on "reductions mod  $p$ " (for almost any  $p$ ). It is precisely this rich multiplicity of seemingly unconnected structures, whose hidden link is **"the motif" common to** all these structures - it is this richness that for me represented (and still represents) the particular fascination of the theme of the cohomology of algebraic varieties, and the fascination of "motifs", which are like the delicate common melody that gives life and meaning to this theme of innumerable variations<sup>387</sup> (\*).

<sup>385</sup> (\*) In the same vein, I'd like to point out the need to pay attention to possible compatibilities, more or less hidden, to be imposed on the set of structures associated with a given type of "cohomological coefficients". I'm thinking here, above all, of the compatibilities (of a more or less algebraic nature) that are automatically realized in the case of "motivizable" coefficients (i.e., that arise from a pattern). It is plausible that they will have to be imposed in the categories of coefficients envisaged, if we wish to have a formalism of the "six operations" (independently even of the aim of "pinpointing" the motives as closely as possible). I'm thinking in particular of the holonomy and infinitesimal regularity conditions for Mebkhout coefficients, and also (if we put a De Rham filtration as an additional structure) the Griffiths conditions linking De Rham filtration and Gauss-Manin connection. These examples make it quite clear, I suppose, to what extent the fundamental task of describing the "right" categories of cohomological coefficients, with the "six operations" constraint, will oblige us to explore and make full use of all the structures envisaged to date on "the cohomology of algebraic varieties", and the relations that can link these structures. This was, in fact, the main purpose of Yoga of Patterns from the outset - to provide a **unity** behind a disparity, and at the same time, a reliable guiding filer for recognizing oneself in that disparity.

<sup>386</sup> (\*\*) (May 2) In fact, it was as early as 1966.

<sup>387</sup> (\*) (March 26) After my brief reflection on the (intimately related) questions of the various types of "coefficient categories" (for "identifying motives"), and the "algebraic conditions" to be satisfied by an "algebraic" cohomology class (i.e. from an algebraic cycle) discussed at the beginning of yesterday's note (n° 176), I decided to include a reflection on the motives, "coefficients", and standard conjectures, as early as Volume 3 of the *Réflexions* (containing the last part of *Récoltes et Sowing*). I believe I now have the principle of a formal description of "the" triangulated category of patterns on a diagram, at least in the crucial case (to which we should be able to reduce ourselves by passages to the limit) where this is of type finor on the absolute basis  $\mathbb{Z}$ . As the only new ingredient compared with my ideas of the sixties, there is the "Mebkhout philosophy" (expressed by the "good God theorem"). In addition, I'm assuming that the problem (surely affordable now) has been solved.

If there's anyone, apart from me, who has heard and felt this melody and allowed himself to be immersed in it for a long time, as it burst forth and unfolded before him, it's Pierre Deligne. If there's anyone to whom I've entrusted something alive, something delicate and vigorous into which I've poured the best of myself, nourished over the years by my strength and my love - it's him. It was a thing made to unfold in broad daylight, to grow and multiply - a thing that was seed and bosom, ready to transmit the life within. This brief contact between yesterday and today was a little like

**reunion** with something I'd long since lost sight of - reunion with not, words, or concepts, nor inert objects, but with a ☐ chose filled with intense **life**. And this contact

makes me realize once again that this "thing" I'd left behind is vast and deep enough to inspire the entire life of a mathematician who gives his heart and soul to it, and of other mathematicians after him.

- because his life will probably not be enough for the task<sup>388</sup> (\*).

It's a strange and welcome coincidence that this encounter should have taken place just as I've had another, equally unexpected "encounter": the encounter with this text in which my friend expresses himself, while refraining from naming it, on the subject of the thing that was closest to my heart, of all the things I've put into his hands. "We probably know little more about it today than a vague skeleton." . .

These words have continued to haunt me over the past three days. I recognize the smugness - the smugness of someone for whom "nothing is beautiful enough for him to deign to rejoice". And, without looking for it, the memory of the "**tomb**"<sup>389</sup> (\*\*) came back to me. The same impression came back to life in me, expressed by the same silent, insistent image. I had once thought I was entrusting this living thing, which was so dear to me, into loving hands - and it was in a tomb, cut off from the benefits of wind, rain and sun, that it languished for the fifteen years I had lost sight of it. Today I find her bloodless, "a vague skeleton... .", the object of the condescending disdain of the man who was kind enough to **use** her, and who is careful never to **give himself away**.

## 18.5. THE FOUR OPERATIONS (on a body)

### 18.5.1. (0) Le détective - ou la vie en rose

**Note 167** (April 22) The note that was to follow on from this one had a long-anticipated name: "Les quatre

operations" (a name which will be explained in detail at the beginning of the following note<sup>390</sup> (\*)). I

thought I'd devote a note, or two at the very least, to this "tidying-up" (of an investigation which seemed to me to have been completed at the time). It's already been almost two months since then, and given the influx of unforeseen twists and turns, I haven't quite got round to it yet. A year on, it's as if the surprise scenario of

the discovery of L'Enterrement is repeating itself, albeit on a different pitch. Finally, in the table of contents, the famous "Four operations" have come to designate not one note or two, but a whole copious set, a little cluttered I'm afraid, of **thirty** notes and sub-notes<sup>391</sup> (\*\*). They are grouped into eight parts (1) to (8), with (I hope) suggestive names, from (1) "The

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now!) of the "mysterious functor", which plays a crucial role in the complete description I'm now looking at.

<sup>388</sup>(\*) (March 26) It now seems possible that I may have overestimated the scale (though not, admittedly, the scope) of the task. On this subject, see the previous b. de p. note, dated the same day.

<sup>389</sup>(\*\*) On the subject of this strong, long-unspoken impression, which haunted me after the "second turning point" in my relationship with Deligne, see the note "Le tombeau" (n° 71).

<sup>390</sup>(\*) (May 12) After splitting this former note "Silence" (n° 168) into four, the "next note" is "The four operations". (n° 167).

<sup>391</sup>(\*\*) (May 12) Since these peremptory lines were written, this number has increased to fifty-one notes and

magot" to (8) "Le sixième clou (au cercueil)". Along the way, I had to completely rework the four notes<sup>392</sup> (\*\*\*) which had formed the "first draft" of the "Four operations" (between February 26 and March 1). I explained myself at the beginning of the note "Le seuil" (n° 172) of March 22 (exactly one month ago), about this departure from the spirit followed elsewhere in the writing of Récoltes et Semailles.

The four notes in question are: "**Silence**", "**Manoeuvres**", "**Sharing**", "**Apotheosis**" (n° s° 168, 169, 170, 171)<sup>393</sup> (\*\*\*), devoted successively to sketching□d, ensemble each of the four p . 828

"I would advise the reader to read these four notes first, to the exclusion of the footnotes (more copious here than in any other part of Récoltes et Semailles) and the subheadings. I would advise the reader to confine himself first to reading these four notes, to the exclusion of the footnotes (more copious here than in any other part of Récoltes et Semailles), and the sub-notes (also exceptionally numerous and substantial) to which reference is made in the "main" text. He could continue in this vein with the following four main notes: "**Le seuil**", "**L'album de famille**", "**L'escalade(2)**", "**Les Pompes Funèbres**" **im Dienst der Wissenschaft** "" (n° s° 172-175), which are no longer technical in nature.

Readers wishing to take a more detailed look at the tortuous intricacies of these "four operations" can include the footnotes and sub-notes in a second reading, and even (if they have not read the first part of Burial, or feel the need to refresh their reading memories), refer as they go along (as I have often done) to the passages in Burial I (or "The Robe of the Chinese Emperor") to which it refers extensively.

The essential content of each of the thirty notes that make up (or describe and comment on) "The Four Operations" is, each time, non-technical in nature. It seems to me that it can be understood by any interested and intelligent reader, even if he or she is by no means an expert in the cohomology of algebraic varieties, nor even a mathematician or even remotely "scientific". However, for those who are reluctant to get involved and get caught up in all the mysteries of the "art of the con", I would particularly recommend the following sub-notes, whose substance seems to me to be the richest, and whose interest visibly exceeds that of "dismantling" the sometimes abracadabrious and always artfully put together "schemes" (for the use of those who just want to be bamboozled. . . ). These are the sub-notes "**L'éviction**" (n° 169<sub>1</sub> ), then "**Les vraies maths. . .**", ". . . and "**non-sense**", "**Magouilles et création**" (forming the first three of the five sub-notes grouped under the name "La Formule"), and finally the four sub-notes to the note "L' Apotheose" (n° 171), concerning Zoghman Mebkhou's strange adventure: "**Eclosion d'une vision - ou l'intrus**", "**La maffia**", "**Les racines**", "**Carte blanche pour le pillage**" (n° s° 171<sub>1</sub> à 171<sub>4</sub> ). These are eight sub-notes (from a total of twenty-one<sup>394</sup> (\*)) that I particularly recommend to the reader.

□As <sup>for</sup> the other thirteen sub-notes, the reader who won't care about their "documentary interest" for- p . 829 would nevertheless read them, in moments of leisure, in the spirit in which he would read a rocambolesque Roman detective adventure, where the improvised amateur detective (in my modest person) follows the trail and gathers the "clues", some tenuous and elusive and others so enormous that no one could see them anymore ; These clues eventually coalesce into a colorful and indisputable **tableau** (de moeurs), in which a "second Monsieur Verdoux (alias Landru), smiling and affable" proceeds to dismember and calcinate his candid and innocent victims, under the tender (even admiring) eye of all the good people in the neighborhood. Since then, they've been

sub-notes, and nothing proves that (like a sea. . . ) it won't rise again. . .

<sup>392</sup>(\*\*\*) (May 12) These notes, having reached prohibitive dimensions, were finally split into several, into notes n° s° 168 (i) - (iii), 169 (i)-(v), 170(i)-(iii), 171 (i)-(iv).

<sup>393</sup>(\*\*\*) (May 12) These notes, having taken on prohibitive dimensions, were finally split into several, into the following notes n° s° 168 (i) - (iii), 169 (i)-(v), 170(i)-(iii), 171 (i)-(iv).

<sup>394</sup>(\*) (May 12) Twenty-seven in the meantime, not counting the sixth nail in the coffin (which counts seven pleasant notes and delectable).

long accustomed to the somewhat peculiar smell, which obviously no longer bothers anyone. More than a few have even taken a leaf out of the book of their friendly, clever neighbor, and the chimneys are purring and chirping to no end.

The "detective", fully edified, has only to tiptoe away: clearly, the agreement here is unanimous, and all is for the best in the best of worlds... .

#### 18.5.1.1. The four operations - or "tidying up" of an investigation

**Note** 167<sup>395</sup> (February 26) (\*) I seem to have come full circle, more or less, on Burial. An incomplete and provisional tour, to be sure - but for the moment, I don't think I'll go much further. I feel I need to take a step back, and that now is the time to finish. All that's left for me to do is to take stock of what I've learned in the course of this impromptu meditation that was the writing of *Récoltes et Semailles*.

By far the largest part of my work has been the reflection on Burial. This reflection continued on two distinct levels. First, after the much-needed "act of respect" represented by the double note "Mes orphelins" and "Refus d'un héritage - ou le prix d'une contradiction" (n° s 46, 47), there was the gradual discovery of L'Enterrement "in all its splendor". I'd been sniffing it for the last seven or eight years - this "wind of discreet derision" towards a work of art and a certain "art".

p. 830 style, and the equally discreet, unflinching "fin de non recevoir" reserved for those who still pretended to be inspired by it and who, <sup>□</sup>in one way or another, "carried my name". This is the aspect of En- This is examined in the note "Le Fossoyeur - ou la Congrégation toute entière" (The Gravedigger - or the entire Congregation) and in the preceding notes (n° s 93-97), forming the Cortège X alias "Le Fourgon Funèbre" (The Funeral Van). This aspect, which had remained diffuse over the years because I hadn't bothered to think about it in detail, has become considerably clearer in the course of my work, although I haven't found any genuinely new facts.

The new fact, on the other hand, with which I was confronted for the first time on April 19 last year, or the "news item" if you like, is a certain large-scale operation that was carried out around my work, and that of the only mathematician who, after my departure from the mathematical scene, assumed the thankless and perilous role of "Grothendieck's continuator": Zoghman Mebkhout.

The discovery on April 19th (of the 1982 volume Lecture Notes 900, in which the motives were exhumed, after twelve years of deathly silence<sup>396</sup> (\*) and without any mention of myself) was the starting point for what might be called an investigation, in the narrower sense of the term: an investigation into the fate that had been reserved for my work, and first and foremost by those who had been its first and foremost custodians, namely, my students. This investigation brought to light a number of facts, some more unforeseen than others, which over the course of days and weeks, came together to form a picture, somewhat external, of what the Burial had been and who its principal players had been. This picture may not be complete, but it is rich enough in perfectly precise and irrefutable details to satisfy my curiosity in that direction. This is the first of the two "levels" of reflection to which I alluded earlier. It essentially corresponds to the "first breath" of reflection on the Burial, continuing from April 19 until around June 10, and ending with the "illness episode".

This is also, more or less, the "Burial I" part (or "The Chinese Emperor's robe") of my notes. To this should be added the note "L'Eloge Funèbre (1) - ou les compliments" (n° 104), dated 12.

<sup>395</sup>(\*) This note, which was originally intended to be called "Les quatre opérations" and follow on from "La mélodie au tombeau - ou la suffi sance" (note n° 167), predates by almost two months the note (of an introductory nature) that precedes it, "Le détective - ou la vie en rose" (n° 167'). I advise you to read the latter first.

<sup>396</sup>(\*) (April 19) For a correction concerning these "twelve years", see the sub-note "Pre-exhumation", n° 168<sub>1</sub> .

May, but was discarded (somewhat arbitrarily no doubt) in the later and ultimate procession "The Funeral Ceremony", part of "Burial II". I would encourage attach to this "survey", forming the "first p . 831

level" of reflection, the note that follows the one quoted above, namely "L'Eloge Funèbre (2) - ou la force et l'auréole" (n° 105),<sup>397</sup> (\*), continuing moreover in the comments on the following note "Le muscle et la tripe (yang enterre yin (1))" (n° 106). These last two notes are from late September - early October. Also, in the "Funeral Eulogies" tradition, i.e. the (very rare) written documents in which Deligne expresses himself to some extent about me, we can add to this survey the two notes recently prompted by Deligne's biographical note, namely "Requiem pour vague squelette" and "La profession de foi - ou le vrai dans le faux" (n° s 165, 166). Finally, there is the note "Les points sur les i" (n° 164), giving a number of clarifications (mainly material), most of which were provided by Deligne himself during his visit to my home last October<sup>398</sup> (\*\*).

After the illness episode, which put an end to all intellectual activity for more than three months, the "second wind" of reflection (or the "second level" I was talking about earlier) was motivated by an effort to understand the **meaning of** the set of facts, some of them very large, not to say unbelievable, that the investigation of April and May had brought to light. The central part of this reflection is "The key to yin and yang", largely independent of the theme of the Burial itself, which nevertheless reappears periodically, each time re-launching a meditation on myself, my life and existence in general.

It's clear, moreover, that the two levels of reflection, "investigation" and "meditation", are by no means independent or clearly separated, but interpenetrate each other. In concrete terms, this is reflected by the presence, throughout the first part of Burial, of an effort to **understand the** meaning of what I was discovering as the days went by, and also by the appearance, again in the second part, of material facts adding to those already obtained during the preliminary "investigation".

For the time being, my aim is to provide a "summary", or broad outline, of the **facts that** have come to light. day by day throughout the investigation' □ **facts** that I have never yet taken the trouble to order so p . 832 coherent. This will therefore be an **account of** what I now know of this "large-scale operation" targeting my work<sup>399</sup> (\*) and that of Mebkhout. Depending on whether it was the latter or mine that bore the brunt, and on which part of my work was targeted, I can in fact distinguish **four** main operations ("the four operations", in short), which I'd like to review first. As it happens, the order in which they came to my attention in the course of reflection also coincides (apart from a mini-reversal of the last two) with the chronological order in which they were set in motion, after my "departure" in 1970 (and even before).

## 18.5.2. (1) Le magot

### 18.5.2.1. a. Silence ("Motifs")

#### $a_1$ . The "Motifs" context

<sup>397</sup>(\*) This note was actually planned for the day after May 12, when the previous note "L'Eloge Funèbre (1) - ou les compliments" was written. I realized then that the text I'd just looked at a little more closely was a veritable mine, which I was far from having exhausted... (for some details on the Eloge Funèbre, see the beginning of the note "L'Apothéose", n° 171).

<sup>398</sup>(\*\*) For more on this visit, see "Duty done - or the moment of truth" (n° 163).

<sup>399</sup>(\*) As far as I know, this refers exclusively to the part of my work between 1955 and 1970, devoted to developing my ideas on the cohomology of schemes and (co)homological algebra.

**Note 168(i) I "Reasons" operation**

Inspired by some of Serre's ideas, and also by the desire to find a certain common "principle" (or "motif") for the various known (or presumed) purely algebraic "avatars" for the classical Betti cohomology of a complex algebraic variety, I introduced the notion of "motif" in the early sixties. Throughout the sixties, and especially from 1963 onwards<sup>400</sup> (\*\*), I developed a rich and precise "yoga" (or "philosophy") on this theme, alongside my work on the foundations. This vast theory, which remained conjectural and will doubtless remain so for a few generations to come<sup>401</sup> (\*\*\*), nonetheless immediately (and to this day) offers a very sure guide to recognizing oneself in situations where the cohomology of algebraic varieties comes into play, both in terms of guessing "what one is entitled to expect from it" and of "what one can expect from it".

p. 833 wait", than to suggest "the right notions" to introduce and sometimes, to provide approaches towards demonstrations. I say on this subject in the Introduction to *Récoltes et Semailles* ("The End of a Silence", p. xviii):

"Of all the mathematical things I'd been privileged to discover and bring to light, this reality of patterns still strikes me as the most fascinating, the most charged with mystery - at the very heart of the profound identity between "geometry" and "arithmetic". And the "yoga of patterns" to which this long-ignored reality has led me is perhaps the most powerful instrument of discovery I have unleashed in this first period<sup>402</sup> (\*) of my mathematical life."

Apart from tentative sketches of a possible explicit construction (among many others) for the category of semi-simple patterns on a body, the ideas I had developed on this theme in my personal notes remained at the stage of oral communication. I was far too absorbed in the many other tasks of writing basic texts<sup>403</sup> (\*\*) to find the leisure of the few months required to develop my handwritten notes into an overall "masterpiece" of the inner vision that had developed within me, sufficiently "researched" to appear publishable to me. From 1965 until my departure from the mathematical scene in 1970, Pierre Deligne was my privileged interlocutor for my motivic (and other) meditations, and the only one who fully assimilated the yoga of motives and felt its full significance.

Further details on the subject of the "yoga of motives" (more detailed than in the part of the Introduction from which the passage quoted is taken) can be found at the end of the note "My orphans" (n° 46) and especially (concerning the genesis of yoga) in "Souvenir d'un rêve - ou la naissance des motifs" (n° 51). For the insertion of the "yoga of motifs" into the formalism of the six operations (which remains, even today and since my

<sup>400</sup>(\*\*) 1963 was the year of the strong "start-up" of staggered cohomology (developed in the SGA 4 seminar in 1963/64), which in turn brought abundant water to the mill of motivic reflections, which until then had been little more than speculations. The following year, I developed the formalism of the "motivic Galois group", whose detailed conceptual foundation was developed (following the program of theory I had submitted to him) in N. Saavedra's thesis,

published only in 1972 (Springer Verlag, Lecture Notes n° 265).

<sup>401</sup>(\*\*\*) (April 8) It now seems to me that this theory is not as far "over the horizon" as it might have seemed to me - if only we finally get around to it! On this subject, see the comments in the note "L'avare et le croulant" (n° 177) of March 27.

<sup>402</sup>(\*) If I'm restricting myself here to "this first period of my mathematical life", it's because I'm thinking of the "yoga of Anabelian algebraic geometry", which seems to me to be of comparable depth and scope. It's mentioned, to some extent, in "Esquisse d'un Programme", which will be included in the "Réflexions" following "Récoltes et Semailles".

<sup>403</sup>(\*\*) These are primarily the EGA (Eléments de Géométrie Algébrique, in collaboration with Jean Dieudonné) and SGA ("Séminaire de Géométrie Algébrique du Bois Marie) texts, the latter written alone or in collaboration (with students in particular), according to guiding ideas and masterminds of my own devising. During the years 1959 to 1969, the average "output" of these texts, all of which without exception became standard reference texts, was around a thousand pages a year.

This work

of foundations came to a halt overnight, as soon as I left the mathematical scene. On this subject, see the note "Yin the Servant, and the new masters" (n° 135).



initially ignored by my students  $\square$  cohomologists, as a fundamental structure in homological algebra. ... ), p. 834  
 see note "Melody at the grave - or sufficiency" (n° 167). For the thread of ideas (entirely overlooked in the literature) surrounding the yoga of weights (which constitutes one of the essential ingredients of the yoga of motives) and the theory of Hodge-Deligne (directly derived from the latter yoga), see the note "Dotting the i's" n° 164 (part II 4), as well as the sub-note (n° 164<sub>1</sub>) which follows it.

## . Burial. . .

**Note 168(ii)** The "Motifs" operation consisted, firstly and immediately after my departure from the mathematical scene, in the systematic **retraction** of the yoga of motifs and of the very word "motif"; and then, after a twelve-year silence<sup>404</sup> (\*), and with the exhumation (in 1982) of a narrow version of yoga, in the retraction of my modest and defunct person, as having anything to do with the said yoga.

The first obvious evasion of yoga, in the form of the "yoga of weights", took place as early as 1968, i.e. before my departure, in Deligne's article (in Publications Mathématiques) on the degeneration of spectral sequences. It is first mentioned in the note "Poids en conserve et douze ans de secret" (written before the discovery of the "memorable volume" of exhumation), and in detail at the beginning of the note "L'éviction" (notes n° s 49, 63).

This probing retraction, in the absence of any reaction<sup>405</sup> (\*\*), continued and intensified with Deligne's Hodge I, II, III articles, setting out the fine generalization of Hodge's theory developed by him in 1968/69. Although this theory stems directly from the yoga of motives (as mentioned above), Hodge II and Hodge III make no mention of this - a fact made all the more glaring by the fact that Hodge

II constitutes the thesis of Deligne, who had been my pupil during crucial years of his formation<sup>406</sup> (\*). p. 835  
 As for the short Hodge I "announcement" (at the Nice International Congress in 1970), Deligne confines himself to a half-line sibylline reference to "Grothendieck's conjectural theory of motives" (in one breath with a bogus reference to Serre, obviously intended to give the change<sup>407</sup> (\*\*)). The escamotage continues with the presentation of the "yoga of weights" at the International Congress in Vancouver (1974), where neither Serre's nor my name is mentioned. In this paper, as in Hodge I at the International Congress in Nice (1970), he never mentions an important part of the yoga he had learned from me,

<sup>404</sup>(\*) (April 8) For a correction to these "twelve years", see the sub-note "Pre-exhumation" (n° 168(iv)) which follows this "Silence" note.

<sup>405</sup>(\*\*) It was from me in the first place that such a reaction could and should have come. While in retrospect the lack of honesty in the presentation of this article is obvious to me (cf. quoted note, n° 63), I myself did not have the rectitude (or the honesty) to acknowledge it, in the presence of a "slight unease" when I held the article in my hands and skimmed through it. To On the role of a certain complacency or ambiguity in me, which came to the fore in the course of reflection on L'Enterrement, see the note "Ambiguity", n° 63". At the conscious level at least, the thought of the possibility of professional dishonesty, in Deligne or in any other of my students, had never occurred to me; or rather, I had pushed it aside on various occasions when the dishonesty was blatant and signaled to me by this ~~never-identified~~ "malaise".

<sup>406</sup>(\*) There was a kind of connivance between Deligne and me to conceal his relationship as a pupil to me, it being understood that he was far too brilliant for me to claim to have been his "master". I update and examine this connivance in the note "L'être à part" (n° 67').

<sup>407</sup>(\*\*) This refers to Serre's article on the Kählerian analogues of Weil's conjectures, which was the "detonator" that set me off. on 'standard conjectures'. It's a fine article, and there's no question of minimizing it. But I'm well aware that Deligne himself would be hard pressed to explain how this article was "a source" for his generalization of Hodge theory - and no one has ever thought of asking him. Having witnessed the birth of the Hodge theory up close Deligne, I know exactly what his source was (see note no.° 164<sub>1</sub> already cited) - and that he didn't find it in Demazure's exposé on the ABCs of définition des motifs! He cites this article as a reference to "the theory conjectural theory of Grothendieck's motives", so as to give the impression, to any reader who wasn't really well-informed (and there weren't many of them to be well-informed. . . ) that the said "conjectural theory" was reduced to Demazure's exposé in question, thus taking advantage of the absence of any more detailed published trace of the yoga of motives.

in the motivic context (which remains rigorously silent): the behavior of the notion of weight by the "six operations" and, first and foremost, by  $Rf_!$  and  $Rf_*$ . This is just one of many examples of a practice that has become commonplace, and of which Deligne seems to me to have been one of the very first promoters: that of reserving exclusive knowledge of the "big problems" that arise in a given field of mathematics to a restricted group of "people in the know" (or even to him alone), so as to ensure total hegemony, instead of making them available to the scientific community and allowing everyone to draw inspiration from them<sup>408</sup> (\*\*\*). As far as I know, this problem isn't mentioned anywhere before he was solved by Deligne in his 1980 article "Weil II" (in the case of  $Rf_!$ ), without of course mentioning me (who had communicated to him the relevant conjecture in the motivic context, of which the l-adic context he deals with is a reflection, in the same way as the context of De Rham - Hodge coefficients would be. . . ).

To the (very fragmentary) extent that I am familiar with Deligne's work or can form an idea of it, I think I can say that the yoga of motifs that he took from me was the main source of inspiration throughout his work. He kept this source occult, maintaining until 1982<sup>409</sup> (\*) a deathly silence around the notion of motif. The only exception (unless I'm mistaken<sup>409</sup> (\*\*)) is the "half witness line" of 1970, just as incomprehensible<sup>410</sup> (\*\*) to anyone other than him and me (and, at a pinch, to Serre perhaps) as his cryptic reference two years earlier (in the article on the degeneracy of spectral sequences) to "weighty considerations" that had led me to conjecture "a particular case" of his degeneracy result (cf. note on "Eviction", n° 63).

## and exhumation

**Note 168(iii)** A sudden change of scene with the publication of the "memorable volume" Lecture Notes 900<sup>411</sup> (\*\*\*). The motifs are exhumed with great fanfare, and part of the original yoga is finally revealed. In this volume, where my name appears two or three times "in passing" and as if by the greatest of coincidences, nothing could lead the reader to suspect that I had anything to do with the ideas developed here. These ideas are presented in such a way that there can be no doubt in the reader's mind that the volume's brilliant main author, Pierre Deligne, has just discovered them and is presenting them here in their entirety.

warm. It's true that, no more than in Nice or Vancouver, he doesn't claim to be the one who discovered the yoga of the weights, which is the first time it has been explained in the literature, it is nowhere mentioned. It's clear here that he's the one who came up with all these fine ideas, developed (apparently) for the first time in the volume, which is centered, incidentally, around a fine theorem of which he is indeed the author. This is the "inch!" style in which he is a master, on which I comment first in the note "Pouce!" and in "La robe de l' Empereur de Chine" which follows it (n° s 77, 77'); see also the earlier notes, written in the emotion of discovering the "memorable volume": "L' Enterrement - ou le Nouveau père", "La nouvelle éthique - ou la

<sup>408</sup>(\*\*\*) On the subject of this new mentality, of which I never came across any trace until I left in 1970, see the note "Yin the Servant, and the new masters", n° 135, as well as the end (dated February 28) of the note "Les manoeuvres" (n° 169) (x). It's this mentality that I wanted to capture by the name "**The hoard**" given to the set of notes and sub-notes (n° s 168- 169<sub>8</sub>) referring to the first two of the "four operations" around my work.

(x) This fin became the note "Le magot" (n° 169(v)).

<sup>409</sup>(\*) (April 8) For a correction, see the sub-note already quoted "Pre-exhumation" (n° 168 (iv)).

<sup>410</sup>(\*\*) As explained in the previous b. de p. note, the purpose of this inch-reference was not to be "understandable".

or to inform, but to (doubly) mislead. As for Hodge-Deligne's fi liation of ideas from motifs to structures (described in the two notes quoted above), I have every reason to believe that I'm the only person in the world, apart from him, who knows it.

<sup>411</sup>(\*\*\*) Springer Verlag, Lecture Notes in Mathematics, n° 900, Hodge cycles, Motives, and Shimura varieties, by P. Deligne, J.S. Milne, A. Ogus, K.Y. Shih.

foire d'empoigne", as well as "Appropriation and contempt" (n° s 52, 59, 59').

In fact, not only were all the main ideas in volume LN 900 concerning motifs known to me as early as the sixties (where Deligne had every opportunity to learn about them from me from 1965 onwards), but also the central problem of the book had been raised by me (and, of course, communicated to Deligne) as early as the late sixties. For details, see the note "Les points sur les i" (n° 164) (in Part I of this one).

As I point out in the Introduction to *Récoltes et Semailles* (in "La fin d'un secret", p. xviii), Deligne was not the only person to whom I spoke in detail about the yoga of motives, even if he was the only one to make it his own intimately. If, for ten years or so<sup>412</sup> (\*), I completely concealed the very existence of this yoga, and later my role in discovering, developing and deepening it, this concealment could only have taken place with the connivance of many of the mathematicians I counted among my friends, and in particular, with that of each of my (commutative) "cohomology students"<sup>413</sup> (\*\*). This cover-up was carried out for the dubious "benefit" of a single person, but through the acts and omissions of a good number of others.

□ Besides Deligne and my other cohomology students, this is the responsibility of the **co-authors** with Deligne of the "memorable volume" LN 900 which seems to me the most heavily committed, namely that of **T. S. Milne, A. Ogus and K.Y. Shih**. These are mathematicians I don't know personally, and there's no reason for me to prejudge their bad faith. For me, however, this in no way detracts from their full responsibility as co-authors of this unusual volume.

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## . Pre-exhumation

**Note 168(iv)** (April 8) I was recently reminded of Deligne's paper "Values of *L-functions* and periods of integrals", published in 1979 (Proceedings of Symposia in Pure Mathematics, Vol. 33 (1979), part 2, pp. 313- 346), in the same volume as the aforementioned paper by R.P. Langlands "Automorphic representations, Shimura varieties and motives. Ein Märchen Corvallis" (pp. 205-246). The latter article (but not Deligne's) appeared in the annotated bibliography on motives sent to me by Deligne last August, and I had been under the impression that Langlands' article was the first and only mention of motives in the literature after my departure, before the exhumation of 1982 (apart from the papers by Saavedra and Kleiman cited in the penultimate footnote).

In fact, in the article quoted by Deligne, there's a "chapter 0" entitled "Motifs", introduced by : "It recalls **part of the formalism, due to Grothendieck**, of motifs" (emphasis mine). The presentation is such that it becomes clear that the general principle of construction I had given for a category

<sup>412</sup>(\*) According to an "annotated bibliography of motifs" that Deligne was kind enough to send me last August, there were still two sporadic works on motifs in the literature after my departure, one and the other in 1972 (in N. Saavedra's thesis, prepared with me, and in a report by S. Kleiman). The next reference, by Langlands, was in 1979. After that, it's LN 900 in 1982. Unless I'm mistaken, the word "motif" does not appear in any of Deligne's published texts between 1970 and 1982.

nor is there any allusion in any published text (with the exception, at most, of the biographical note examined in notes n° s 165, 166) to the fact that he may have learned something from me. ...

(April 8) Regarding "unless mistaken", see correction in sub-note "Pre-exhumation" (n° 168 (iv)).

<sup>413</sup>(\*\*) I think I can say that all my pre-1970 students, with the sole exception of Mrs. Sinh (who was not on site, but working in Viet-Nam), were aware of (but had not necessarily assimilated) my ideas on motifs, on which I gave a series of detailed talks at the IHES (in 1967). Those of them who have remained connected to the theme of the cohomology of algebraic varieties therefore seem to me to be in solidarity with the burial that has taken place of the yoga of motifs, on the initiative of the main "interested" Deligne. I'm referring here in particular to J.L. Verdier, L. Illusie and P. Berthelot, each of whom was more active than a mere connivance in some of the other three "operations" discussed below.

of (semisimple, it's implied) patterns over a body, was multivalent - indeed, in section 0.6 it says that "**one of Grothendieck's definitions** of patterns is obtained by...". In this respect, then, the presentation is honest. It's true that the part of the "yoga" of motives presented here is the most elementary part, which practically already existed in the literature (in presentations by Manin, Demazure, Kleiman, Saavedra), and where my paternity  $\square$  was therefore particularly notorious. (On the other hand, it would seem that the concealment of my persona

- and Serre's - in weight yoga, and later in the motivic Galois group, passed without a hitch. ... )

As I have already pointed out (in the note "L'escalade (2)", n° 174), it would seem that, after the temporary culmination of "Operation Burial" in 1977 (with the "SGA 4<sup>1</sup> - SGA 5" operation), there was a relative lull until the "apotheosis" of the Colloque Pervers in 1981, which marked the end of any hint of restraint in the butchering of a corpse. (See the note "L' Apotheose", n° 171.) Deligne's article is obviously written under the sign of this lull. I presume that Langlands' interest in motivic yoga had forced his hand in finally "spilling the beans" (already stale) on the motives, at a time when it was not yet psychologically ripe to simply pass over the name of the deceased. In the three years that followed, there was indeed a striking "escalation" (to use the expression in the note "Les manoeuvres" that follows this one), between this timid "pre-exhumation" of the motifs, and the "exhumation with great fanfares" that took place with the "memorable volume" LN 900 in 1982.

(April 22) The (mini)discovery commented on in the preceding page continued and amplified considerably in the days that followed. I read the article by R.P.Langlands, and the very next day, the "sixth nail" in my coffin<sup>414</sup> (\*), in the form of the book by (my ex-student) Neantro Saavedra Rivano, entitled "Tannakian Categories". So there's still a substantial "continuation of the story" (of the "Motifs operation"), which I developed in the series of sub-notes (n° s 175<sup>1</sup> to 175<sup>7</sup>) grouped together under the obvious name, "The sixth nail (in the coffin)". I thought it preferable to return this suite to the end of the "Four Operations" survey, as the new facts that appear throughout it, and especially in the note "L'Apotheose" (n° 171), and its four sub-notes<sup>415</sup> (\*\*), seem to me essential to situate this "suite" properly and give it its full meaning.

### 18.5.2.2. Maneuvers ("Staggered Cohomology")

#### $b_1$ . The "Weil Conjectures" context

p. 840 **Note** 169(i)  $\square$ (February 27) Now for the second of the "big operations":

**II The operation "Cohomologie étale".** As with the motives, it will be useful first to set the scene in a few words.

The idea of the existence of a theory of "cohomology" of an algebraic variety over any field  $k$ , which would associate with such a variety (at least if it is projective and smooth) "cohomology spaces" whose coefficient field would be of zero characteristic (for example, a  $p$ -adic field), and whose properties would model the well-known properties of Betti cohomology (defined by transcendental neighbour

<sup>414</sup>(\*) This is the sixth of the "nails" in the order of their discovery, but the first of the six, seen in the chronological order in which they were deftly "laid" by my friend Pierre, with patented equipment provided (for the service of science) by the well-known Funeral Company Springer Verlag GmbH (Funeral Service "Lecture Notes in Mathematics"). . .

<sup>415</sup>(\*\*) (May 11) Since these lines were written, the quoted note has been split into four separate notes (n° s 171 (i) to (iv)) and expanded by a further eight sub-notes (n° s 171 (v) to (xii)).

when the basic body is the body of complexes) - this idea can be found "between the lines" in the statement of Weil's famous conjectures (1949). It was in cohomological terms, at any rate, that Serre explained Weil's conjectures to me, around 1955 - and it was only in these terms that they were likely to "hook" me indeed.

At the time, no one had the slightest idea how to define such a cohomology, and I'm not sure that anyone other than Serre and myself, not even Weil if that's what it was, had even the slightest conviction that it should exist. We only had a good direct geometric grip on  $H^1$ , via the theory of abelian varieties and their points of finite order (developed by Weil), and via Albanese or Picard varieties associated with a non-singular projective algebraic variety. This construction of  $H^1$  suggested that the "natural" coefficient bodies should be the  $l$ -adic bodies  $Q_l$ , for  $l$  prime number **distinct from** the characteristic.

For  $l$  equal to the characteristic (when the latter is non-zero), Serre's very partial results, which were particularly convincing in the case of algebraic **curves**, suggested that we should be able to take as our base body the body of fractions of the ring of Witt vectors of  $k$  (assumed to be perfect). It was therefore to be hoped that there would be an  $l$ -adic theory (with a grain of salt for  $l = p$ ) for **any** prime number  $l$  - and in a conceivable sense, they should "all give the same result". Finally, when  $k$  is of zero characteristic, so that we have (at least in the non-singular projective case of  $X$ ) Hodge's cohomology spaces (which made sense for any  $k$ , since Serre's introduction of the "coherent" cohomological theory of algebraic varieties) and De Rham's (which I had introduced on the basis of De Rham's cohomology), we are able to obtain the same result.

Rham differentiable), these immediately provided cohomological theories having all the propriétés voulues<sup>416</sup> (\*), and they were still to give "the same result" as the hypothetical cohomologies  $l$ -adic. . 841

These questions were central to my thinking and to my published and unpublished mathematical work between 1955 and 1970 (when I left the mathematical scene). Leaving aside my work in coherent cohomology (the "six operations" formalism, the Riemann-Roch-Grothendieck formula), it can be said that, broadly speaking, most of my cohomological work consisted in finding answers, or broad lines of answers, to these questions. At least from the point of view of Weil's conjectures, acting as my main source of inspiration, my thinking on the cohomological theme has materialized in four main **currents**, or "**threads**", closely interwoven to form a single, vast weave.

**Thread 1-** I have developed (with the assistance of collaborators<sup>417</sup> (\*\*)), a formalism for **cohomology  $l$ -adic** schemes, for **the** first with residual characteristics, having all the known properties (and beyond . . . ) of the familiar "discrete" cohomology of topological spaces. With just three open questions<sup>418</sup> (\*\*\*), of a technical nature, we can say that we had, "in principle" as early as 1963, and "in fact" as early as 1965/66

<sup>416</sup>(\*) Back in the 50s, I developed the formalism of cohomology classes (Hodge and De Rham) associated with an algebraic cycle.

<sup>417</sup>(\*\*) The main collaborator in the development of the stale cohomology formalism was Artin. The  $l$ -adic adaptations are developed in the thesis of my ex-student P. Jouanolou (which he unfortunately didn't bother to publish, which I never held in my hands, and which has become unobtainable). I intend to give more details about the development of stale cohomology, in "historical" comments that I intend to attach to the Thematic Outline (to appear in Reflections following R and S).

<sup>418</sup>(\*\*\*) These three "open questions" are as follows:

- a. The "cohomological purity conjecture" for a regular subscheme  $Y$  of a regular scheme  $X$ . The relevant statement is proved when  $X$  and  $Y$  are both smooth on a regular  $S$ -base scheme (a sufficient case for most applications), and also (by Artin, making full use of singularity resolution) in the case where  $X$  is excellent of characteristic zero.
- b. Even more serious is the question of the validity of the **fi nitude theorem** for  $R^i f_*$ , for  $f$  a separate fi ni morphism of Noetherian schemes (excellent if need be), when  $f$  is **not** assumed to be clean. We need this result to challenge  $Rf_*$  (and two others among the "six operations") in the "constructible"  $l$ -adic frame. I proved the fi nitude result by means of

(with the developments of the SGA 5 seminar, following on from SGA 4 in 1963/64), of a **complete mastery** of this cohomology, within the general framework of so-called "étale cohomology" - in the form of the "six operations" duality formalism. The principle behind the definition of étale cohomology dates back to 1958, and I proved the necessary and sufficient "key results" for the complete formalism (including theorems of the "weak Lefschetz" type and notions of cohomological depth in the étale context) in February and March 1963.

**Thread 2.** With the yoga of **motives**, I discovered the philosophy that makes it possible to link together the different  $l$ -adic (and other) cohomologies of a variety, as being so many different "realizations" of a "motive" that is common to all of them, and which is the "motivic cohomology" of this variety. This philosophy was born in the early 1960s, with a "yoga of weights" directly inspired by Weil's conjectures (and an idea of Serre's inspired by them, concerning a notion of "virtual Betti numbers" associated with an algebraic variety<sup>419</sup> (\*)). The crucial notion of "motivic Galois group" was added in 1964, in the wake of the start of  $l$ -adic cohomology.

**Thread 3.** inspired by the ideas of Monsky-Washnitzer, who had built a cohomological theory (at constant coefficients) " $p$ -adic" for **smooth** and **affine** algebraic varieties in char.  $p > 0$ , in 1968 I came up with a general definition for a " $p$ -adic cohomology", which I also call **cris-cohomology**.

**talline**<sup>420</sup> (\*\*). This theory was supposed to encompass "coefficients" (so-called "crystalline") not necessarily

constant nor locally constant, and give rise to a "six operations" formalism just like  $l$ -adic theory. It was clear from the outset, at least, that for **smooth** varieties, this cohomology has the expected relationship with De Rham's cohomology, and that it generalizes Monsky-Washnitzer's<sup>421</sup> (\*).

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assumptions of singularity resolution and "cohomological purity" (cf.a)), which for the moment do **not** apply to algebraic varieties of char.  $p > 0$ . I would point out, however, that in the context of torsion coefficients (as opposed to  $l$ -adic coefficients), the duality formalism of the six operations (thus including Poincaré duality) had been established by me in 1963 without finiteness conditions. This implied, for example, "finitude" for  $H^i$  with constant or locally constant coefficients (torsion or  $l$ -adic) for a smooth (not necessarily clean) scheme over an algebraically closed body.

c. Validity of the "duality theorem" on an excellent regular pattern. Situation similar to b).

The situation was significantly improved by Deligne's elegant (1973?) proof of the finiteness theorem, for a morphism of finitely type schemes over a regular  $S$ -scheme of dimension  $\leq 1$ . This case covers most applications (algebraic schemes over a body, finitely type schemes over  $\mathbb{Z}$  in particular). In the same situation of a scheme  $X$  of type finitely on a regular 1-dimensional scheme, and using similar simple arguments, Deligne also manages to prove the biduality theorem.

<sup>419</sup>(\*) On this subject, see sub-note no. 46 to the note "My orphans" (no. 46).

<sup>420</sup>(\*\*) This terminology is now (and has been for a long time) established by usage, as is the expression "crystalline site". The two new ideas (compared with those of Monsky and Washnitzer) that led me to this theory are that of **crystals** (of modules etc.), linked to an idea of "growth" over "thickenings" (notably infinitesimal) of a starting scheme, and secondly the introduction of a structure of **divided powers** in the ideals of increase of the envisaged thickenings, so as to ensure the validity of a "formal Poincaré lemma" (with divided powers). Thanks to these two ingredients, the De Rham cohomology of a smooth scheme on  $k$  can be interpreted as the "ordinary" cohomology, with **coefficients in the structural ring bundle**, of a suitable "crystal site".

Strangely enough, the crucial intuition of crystal (as well as the more far-reaching one of topos) seems to have been left behind by my students, along with the guiding thread (omnipresent in my cohomological reflections) of the "six operations". This, it seems to me, is the main reason for the regrettable stagnation in crystalline cohomology after my departure, and also in the (closely related) "Hodge-Deligne" theory, since the first strong start of both.

It seems to me at least plausible, not to say obvious, that in either direction, the philosophy developed (in general indifference. . .) by **Zoghman Mebkhout** would have an essential role to play. But his timid suggestions in this direction (to Berthelot in 1978) obviously fell on deaf ears, coming from such an insignificant character. . .

<sup>421</sup>(\*) P. Berthelot's thesis, taking my ideas as a starting point, provides a further justification, by establishing a duality formalism for clean and smooth varieties, rich enough at least to write an expression

□ **Fil 4.** The unifying geometrical notion, linking by a common "topological" intuition cohomology étale and its immediate variants (linked to Zariski topologies, fpqc, fppf etc.), crystalline cohomology, and finally "Betti" cohomology defined in the transcendental context, and (even more generally) the faisceautic cohomology of any topological spaces, is the notion of "**site**", and, beyond this, more intrinsic and more hidden, that of **topos**. From 1964 onwards, the latter gradually came to the fore. I discuss the significance of this notion, central to my work and now banished from geometry, in the note "Mes orphelins" (n° 46), pp. 180-182, from which I shall confine myself here to extracting the following passage:

"This pair of notions [schemas and topos] potentially contains a vast renewal of both algebraic geometry and arithmetic, as well as topology, through a synthesis of these "worlds", too long separated, in a common geometric intuition."<sup>422</sup> (\*)

The language of topos, and the formalism of étale cohomology, are developed in the two consecutive and inseparable seminars SGA 4 (in 1963/64) and SGA 5 (in 1965/66)<sup>423</sup> (\*\*). The first is in collaboration with □<sup>424</sup> d, autres<sup>424</sup> (\*), and develops, in addition to the language of topos, the key results of cohomology, including key duality start-up statements (six-operation style). The second, in which I practically went it alone<sup>425</sup> (\*\*), develops a complete formalism in much greater detail.

p. 845

**crystalline** cohomology for the ordinary *L-function* of such a variety over a finite body. But, as I pointed out in the previous b. de p. note, we are still a long way from a mastery comparable to that which we have in *l-adic* cohomology, which would be expressed by a "six operations" formalism for general "crystalline coefficients". These (according to what Deligne recently told me) have not yet been **defined**, any more than the right "Hodge coefficients" (above complex algebraic varieties)! For some comments on the "coefficient problem", which I believe is crucial to an understanding of the cohomology of algebraic varieties, see the note "La mélodie

to the grave - or sufficiency" (n° 167). This problem was clearly present for me throughout the sixties, but has been buried (among many others, and by the care of my cohomology students) to this very day. ...

(April 23) See also the note "Le tour des chantiers - ou outils et vision", n° 178.

<sup>422</sup>(\*) I propose elsewhere (in sub-note n° 136<sub>1</sub> to the note "Yin the Servant (2) - or generosity" (n° 136), to call by the name of **arithmetical geometry**, this "new science" still in its infancy, "so vast that until today I've never even not thought of giving it a name", born in the early sixties in the wake of Weil's conjectures, and of which the "yoga of motives" is "like the soul, or at least like a neuralgic part of it". With this name, I would like to suggest

"the image of a "geometry" that would be developed "above the absolute base" SpecZ, and which admits "specializations" both in the traditional "algebraic geometries" of different characteristics, and in "transcendental" geometric notions (above the basic bodies C, R, or  $\mathbb{Q}_A \dots$ ), via the notions of analytic or rigid-analytic "varieties" (or better, **multiplicities**), and their variants.

(loc. cit. p. 637). I write above (same page):

"Beyond the edification of the new algebraic geometry, and through towards the "mastery of étale cohomology" (and that of the *l-adic* cohomology which follows from it), it is the elaboration of a master builder of this new science still in the making, which was in my eyes my main contribution to the mathematics of my time."

<sup>423</sup>(\*\*) A second edition (in three volumes) of SGA 4, completely revised compared to the original edition (especially concerning the language of sites and topos, and categorical complements) has been published in Lecture Notes (Springer Verlag). in 1972-73, n° s 269, 270, 305. For the vicissitudes of SGA 5, see details below. An "illusie edition" of a copiously dismantled version of the original seminar was published in the same Lecture Notes (no. 589) in 1977, **eleven years later**, after the end of the oral seminar.

<sup>424</sup>(\*) The development of the language of sites and topos, based on my initial idea of 1958, was mainly driven by and with the help of M. Artin, J. Giraud, J.L. Verdier. For details, see the promised historical commentary, already quoted in a previous b. de p. note.

<sup>425</sup>(\*\*) The only exception (if my memory serves me correctly) was provided by J.P. Serre, who gave some fine talks on finitely generated groups and the Serre-Swan module associated with the Artin conductor, which I needed for the development of the general fixed point formula I had in mind. It was intended that these lectures should appear in SGA 5, but seeing the turn events were taking, Serre had the good sense to make them available to the mathematical public by publishing them elsewhere.

of duality, including the fixed-point formulas leading to the cohomological theory of *L-functions* (which forms an important part of Weil's set of conjectures). I write about this double seminar in the note "La dépouille... ." (n° 88), in the following terms:

"The set of two consecutive seminars, SGA 4 and SGA 5 (which for me are like **a single** "seminar") develops from nothing, both the powerful instrument of synthesis and discover-verte represented by the **language of** topos, and the perfectly perfected, par- ticularly effective **tool** that is étale cohomology - better understood in its essential formal properties, from that moment on, than even the cohomological theory of ordinary spaces was. This whole represents the most profound and innovative contribution I have made to mathematics, at the level of a fully completed work. At the same time, and without wishing to be, while at every moment everything unfolds with the naturalness of the obvious, this work represents the most far-reaching technical "tour de force" I have accomplished in my work as a mathematician. For me, these two seminars are indissolubly linked. They represent, in their unity, both the **vision** and the **tool** - the topos, and a complete formalism of stale cohomology.

While the vision is still rejected today, for over twenty years the tool has profoundly renewed algebraic geometry in its most fascinating aspect for me - the "arithmetical" aspect, apprehended by an intuition, and by a conceptual and technical baggage, of a "geometrical" nature. "

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## $b_2$ . The four maneuvers

p. 846 **Note 169(ii)** □ **Operation Cohomologie étale" discredited the unifying vision** of topos

(such as "nonsense", bombing etc.), and by the same token, and by assimilation, the role I had played in the discovery and development of the cohomological tool; and secondly, to **appropriate the tool**, i.e. the **authorship of** the ideas, techniques and results I had developed on the theme of staggered cohomology. Here again, the "beneficiary" of the operation is Deligne<sup>426</sup> (\*), and it is his excep- tional ascendancy (due no doubt as much to his exceptional means as to his implicit position as "heir" to my work) that has made an operation of this scale (of debunking and appropriation) "pass", without apparently making a single wrinkle... .

It was in 1965/66, in the SGA 5 oral seminar and through the texts already written in the previous SGA 4 section, that the young newcomer Deligne made his first apprenticeship in scheme theory, homological algebra (Grothendieck style) and the new techniques of stale cohomology (born two years before)<sup>427</sup> (\*\*) - techniques which were to form the basis of all his subsequent work.

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For all other presentations, I was the only speaker, or, if there were others towards the end, they followed the detailed notes I had developed for the seminar. The editors' (sic) task was therefore limited to finalizing the notes I had made available to them.

<sup>426</sup>(\*) There were, however, substantial repercussions for **Verdier**, as we shall see later: firstly in 1976, when he gave the "kick-off" for the dismantling of APG 5 with his "memorable article" (see "episode 3" of an escalation below), and then in 1981 at the "Colloque Pervers" (first mentioned in this connection, in the note "Le partage" (n° 170)). dedicated to "Operation III").

<sup>427</sup>(\*\*) This is what I recall (having somewhat forgotten) in the note (of May 27 last year) "L'être à part" (n° 67'). I would add that it was in this same SGA 5 seminar that the young Deligne also learned from me (but "as a



In the operation (which I have elsewhere called "Operation  $\text{SGA}_2 4^1$  - SGA 5") set up by my brilliant ex. As a student, I see four inseparable "**maneuvers**".

**Manoeuvre 1: Discrediting** the  $\square$   $\text{SGA}_2 4^1$  - SGA 5 mother seminar as a "ganguel of nonsense" and others p. 847  
It's all done on the fly (and "mine de rien") in the various introductory texts to the volume, by the pen of Deligne, called by the strange name " $\text{SGA}_2 4^1$ " (subtitle: Cohomologie étale) published in Lecture Notes of Mathematics n° 569 (Springer Verlag). For details of the shaping of the double seminar SGA 4 - SGA 5, where Deligne learned his trade and found his basic tool for all his later work, see the note "La table rase" (n° 67).

**Maneuver 2. Sabotage** the overall editing of my SGA 5 oral presentations<sup>428</sup> (\*). Normally, this should have been done within a reasonable timeframe (a year or two at most), by my cohomology students (for want of other reliable volunteer editors), who had the privilege of learning a great deal about their profession, as well as ideas and techniques that they and the other seminar participants had been the only ones to know about for many years. It was also the best (and quickest) way for them to familiarize themselves with a substance and with ideas and techniques, which during oral presentations tended to go a little "over their heads" (with the exception of the ever-dashing Deligne, needless to say). In any case, this drafting, or rather **non-editing, dragged on for eleven years** - until, as luck would have it, Deligne gave Illusie the "green light" to edit and publish this unfortunate SGA 5, which had until then been left to its own devices by mutual agreement - the moment when it became clear that it would be published (in 1977) **after** a certain volume, written by Deligne himself, composed (in 1973 and the following years) initially for the purpose (as I first thought) of popularizing the "ingredients" ("inputs") of stellar cohomology essential for his demonstration (of the last part) of Weil's conjectures, is christened for the occasion with the unusual name " $\text{SGA}_2 4^1$ ". (This name, however, does not appear to have been bewildered or surprised, even  $\square$  shocked, no one but me ... .  $(169)_1$ <sup>429</sup> (\*) For details, see notes p. 848  
"Le feu vert" and "Le renversement" (n° s 68, 68'), where the sense of volume calling itself " $\text{SGA}_2 4^1$ " begins at appear to me, as do the notes "Silence" and "Solidarity" (n° s 84, 85).

**Manoeuvre 3. Dismantle the original SGA 5 seminar**, of which the published version (by the "care" of my ex-student Luc Illusie) now represents no more than an outrageously mutilated "corpse". I give an account of this shameless dismantling, or to put it more accurately, the **massacre** of what was a splendid seminar entrusted to the hands of my students, in the note of the same name (n° 87) - one of the longest and most revealing of the reflections on Burial.

**Manoeuvre 4: Break up the unity of my work** on staggered cohomology, represented by the two inseparable shutters SGA 4 and SGA 5, by "cutting it in two", "by the violent insertion, between these

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He had always known it", it has to be said!) the art of putting the description (or "theory") of an interwoven and, at first glance, dense situation down in black and white, in a form that is at once convenient, striking, clear and rigorous. Twelve years later, after he had ransacked the seminar, this did not prevent him from displaying an air of disdainful condescension and contempt towards what remained of it (and the SGA 4 section that formed its basis).

<sup>428</sup>(\*) As I mentioned three notes (de b. de p.) above, there were detailed notes for each of my oral presentations. It would have taken me several months to write them up. If I didn't do it, and as early as the year (1966) of the end of the seminar, it was because, in principle, volunteers (? ? ?) had taken on the task of detailed editing. It dragged on and on until I left in 1970, when I had completely "given up" on questions of this kind.

in favor of tasks that seemed (rightly) more essential and urgent. On this subject, see the note "Le feu vert" (n° 68), in which I ask myself for the first time about the meaning of what happened with "that unfortunate seminar". It was April 27 - and I discover the reality, the "breath" of the "massacre" on May 12, two weeks later. ...

<sup>429</sup>(\*) On this subject, and for clarification of the original and true **meaning** of the acronym APG (from which my name and person were eventually ousted), see the sub-note "L'éviction" (n° 169<sub>1</sub>) which follows this one ("Les manoeuvres", n° 169), and was originally intended as a b. de p. note here.

two-part, foreign and disdainful text"<sup>430</sup> (\*\*), answering to the unusual name "SGA 4<sup>1</sup>"<sup>431</sup> (\*\*\*). This ingenious name says exactly what it's supposed to say - you just had to think of it! With this name alone, the volume presents itself as **the** central and fundamental text on étale cohomology, destined to **replace** the "dense presentations of SGA 4 and SGA 5", "which can be considered as a series of digressions", "some of them very interesting" it's true, but which the central text "should allow the user to forget".

p. 849 There's no need for my brilliant ex-student and friend to compromise himself here in lengthy and pointless discourse: this lapidary name alone, "SGA 4<sup>1</sup>", states and lays down the unanswerable evidence of the **anteriority** of this text in relation to the "digressions" known as SGA 5 (which, as it certainly could not have been otherwise, were published after him. . . ), and at the same time, it takes for granted an (alleged) **logical dependence** of SGA 5 on the "previous" text.

This implausible claim that SGA 5 is logically dependent on the apo-cryphal text is confirmed in the introduction to<sup>432</sup> (\*), where the author announces without batting an eyelid (and apparently without anyone before me - these days - finding anything peculiar in it... ) :

"... its existence [that of "SGA 4<sup>1</sup>"] will make it possible to publish SGA 5 **as is in the** near future" (that's underlining) -

read: in the state of a ransacked and plundered **corpse**. ... Although I had already been aware of my friend's "Motifs" operation for over a week, it took me two days (from April 26, with the note "La table rase", to April 28, with the note "Le renversement" (notes n°s 67, 68)) to grasp the meaning of the "mystery" represented for me by my brilliant pupil's obviously preposterous assertion - and at the same time, to understand the meaning of the seemingly innocuous acronym "SGA 4<sup>1</sup>", which I hadn't even considered the previous two days.

p. 850 The same sham of "logical dependence" is clearly suggested in the introduction to SGA 5 by Illusie (169)<sup>433</sup> (\*\*). It is further rendered plausible, for an uninformed reader, by the innumerable references to "SGA 4<sup>1</sup>" which the late editors of my<sup>434</sup> (\*\*\*) presentations (or of those, **du moins**, that one has well wanted to include in the edition-massacre) are more than happy to stuff their essays. Many of these references are by no means bogus, but refer to two of the original seminar papers (one by Illusie, the other - particularly crucial - by Deligne<sup>435</sup> (\*)), which were incorporated without further ado into the text.

<sup>430</sup>(\*\*) This passage in quotation marks is quoted (from memory) from the note "la dépouille. . ." (n° 88) - the very note in which, for the first time in the reflection on Burial, I "pose" to become aware of the place of the SGA 4 - SGA 5 seminar, in inside "my work fully completed". As for the deeper, "carnal" experience of the "breath of violence" attacking this central, harmonious and living part of my work, it was revealed to me in a dream the very night following this reflection. It found its written expression the next day, in the note ". . . and the body" (n° 89).

<sup>431</sup>(\*\*\*) Subtitle: Cohomologie étale - by Pierre Deligne. ... The subtitle says it all!

<sup>432</sup>(\*) I would remind you that, during his last visit to my home (last October), Deligne gave me an oral confirmation of this same delirious thesis - without any real conviction, it's true, and without even pretending to tell me how my seminar, which formed a harmonious and coherent whole without having waited for him, would depend on Deligne's work, which came out of it seven years later. ... This short scene on a station platform, where we were waiting (with his little daughter Natacha) for the train that was to take them back to Paris, is recounted at the end of the note dedicated to this visit, "Le devoir accompli - ou l'instant de vérité" (n° 163).

<sup>433</sup>(\*\*) For details, see the sub-note "Good Samaritans" (n° 169<sub>2</sub>) to the present note (n° 169), originally intended as a b. de p. note here.

<sup>434</sup>(\*\*\*) (April 9) detailed verification made, the "late editors" in question (and that's an understatement. . . ) are limited to my dear ex-students Luc Illusie and Jean-Pierre Jouanolou. Bucur's and Houzel's drafts were ready before I left, and Illusie didn't go so far as to slip in references to a text called "SGA 4<sup>1</sup>", which didn't see the light of day until some ten years later. He and Jouanolou were content to wait for Deligne's "encouragement" to write what was incumbent on them, eleven years after the seminar's completion, and, for the presentations they had already written "in my time", to stuff them with empty references to the pirate-text of their brilliant friend and protector.

<sup>435</sup>(\*) This is the lecture "The cohomology class associated with a cycle, by A. Grothendieck, edited by P. Deligne". It is stated that this talk was "inspired by Grothendieck's notes, which **formed a state 0** of SGA 5 IV" - by which it is suggested,

in the volume entitled "SGA 4<sup>1</sup>" - not asking me for anything or only informing me of it, but as something that (in the absence of the deceased master) would rightfully belong to them. ...

□ This act of brigandage also allows my ex-student Deligne to achieve this brilliant **reversal of the roles**, to be able to present myself on the cover of the book (and while being just as careful not to consult me. . . ) as his **collaborator** (for the development of staggered cohomology!)<sup>436</sup> (\*) - a collaborator a little "confused" around the edges<sup>437</sup> (\*\*) it is true, but "collaborator" all the same. . .

As for the pirate-text called "SGA 4<sup>1</sup>/<sub>2</sub>", in addition to the two lectures already mentioned, torn from their original SGA 5 context, and in addition to numerous "digests" of some of the results of SGA 4 - SGA 5 particularly important for arithmetic applications, plus an original chapter of applications to trigonometric sums, and apart finally from the "Etat 0" of Verdier's "thesis"-sic (which will be discussed further with "operation III"), it consists of a handful of additions (very useful, admittedly<sup>438</sup> (\*\*\*) to the cohomology formalism □ developed in SGA 4 - SGA 5. There's enough here to make a fine, if somewhat heterogeneous, article, p. 852.

about 30 pages (or 50, if you include the "Trigonometric sums" chapter). In

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no doubt, that it was an act of charity to rid SGA 5 of this sad state (zero), in order to make the beautiful presentation that we have here in a brilliant volume. ...

As for Illusie's presentation (ex-chapter II), which disappeared from SGA 5 only to reappear (in redesigned form) as an appendix to Deligne's presentation on finitude theorems in staggered cohomology, it developed the theorems of finitude relevant for  $R^i f_*$  (under assumptions of "purity" and "resolution", see b. of p.(\*\*\*) on page 841), and theorems of the "generic Künneth" and "generic local acyclicity" type. No one before me had ever thought of **formulating** only such statements in cohomology. Moreover, the so-called "outdated" demonstrations in the oral seminar, in addition to principles of dependence (e.g., making it possible to deduce from a finitude statement for the functor  $Rf_*$  the similar statement for  $Lf^!$  and for  $RHom(., .)$ ), introduced a uniform technique for using the strong form (à la Hironaka) of the resolution of

singularities, which has proved its worth elsewhere - and it was there and nowhere else that Deligne and my other cohomology students learned it. It was subsequently used, in particular, in my proof of the "algebraic De Rham" theorem for smooth varieties over the field of complexes, and in that of Mebkhout-le-nom-nommé's theorem, known as the "Riemann-Hilbert theorem" aka the "theorem of the good Lord" (which Mebkhout didn't have the advantage of learning the method in SGA 5, from which it had disappeared... .).

Seven years later (??) Deligne found an elegant method to prove in a few pages the finitude of  $Rf_*$ , as well as the biduality theorem (very close technically), under (if not optimal, at least) very unrestrictive assumptions (see b. de p. note quoted). Nothing, either in Deligne's presentation or in his friend's appendix, could lead the reader to suspect that I had anything to do with the notions introduced and used (such as local acyclicity and its "generic" variant), or with the statements proved (of finitude, biduality, Künneth and generic acyclicity), and with the links between them. My name is absent from both the text and the bibliography, which consists of four references to Deligne, all of them post-1970, i.e. my "departure".

I find myself once again, at the turn of this explanatory b.p. note, faced with the deliberate intention of wiping the slate clean of the origins and roots of what my brilliant students wield with such mastery (as if they'd always known. . . ) - that is, of **erasing the traces of a past**, the past before my "death".

(March 16) For the special role reserved for Deligne's "finitude" complements, see the sub-note "Le cheval de Troie" (n° 169<sub>3</sub>) to this "Maneuvers" note.

<sup>436</sup>(\*) This staging (in which I appear as the "collaborator" of my pupil Deligne) is all the more shameless, given that it had been seven years since I had clearly and publicly stated my intention to stop publishing maths (and even less, from then on, as a "collaborator", one might think. . . ).

<sup>437</sup>(\*\*) In his summary (a copy of which he sent to me) of "SGA 4<sup>1</sup>/<sub>2</sub>" for the Zentralblatt (September 1977), Deligne makes a point of pleasure to talk about the "**confused** - albeit rigorous - state of SGA 5" (emphasis mine), which (one would have guessed) the new text was supposed to "remedy". . .

<sup>438</sup>(\*\*\*) These are finitude results (already mentioned three b. de p. notes above and in the one quoted there), filling in a few pages two gaps in the SGA 5 mother seminar, plus an exposé on fixed point formulas "modulo"  $l'$  and  $p$ . The problem of explicating such formulas, and the relevant conjecture for a mod  $p$  expression of the Artin-Weil function  $L$  for a finitary type scheme, over a finitary body had been posed by me as early as the SGA 5 seminar, and were surely part of the problems (unworthy of any mention in Illusie's introduction to SGA 5) posed in the closing lecture (a lecture that disappeared body and well, along with many others, in the Illusie edition). Deligne had found an elegant common solution, using the "symmetrical Künneth formula" (which, for the sake of argument, he developed in one of the apocryphal lectures in SGA 4). It was understood (and taken for granted) that these results would be included in the edited version of SGA 5, from which they were directly inspired. Needless to say, my name does not appear in the eight-page discussion of this formula in the volume entitled "SGA 4<sup>1</sup>/<sub>2</sub>". 2

of my brilliant ex-student, it would have been self-evident to include these few additions, each in its own place, in the two or three lectures of SGA 5 from which they were inspired and which they completed. Instead, they serve as a pretext for the outright deletion of Lecture II from SGA 5 (with the blessing of Illusie, who was in charge of writing it and who "supplements" it by turning it into an appendix in "SGA 4<sup>1</sup>" to the chapter on finiteness theorems), and to rename the biduality theorem in étale cohomology (which I had worked out in 1963, on the model of the "coherent" analogue I had discovered in the fifties) "Deligne's theorem"(\*) (which the aforementioned Deligne was to generously "cede" to his friend Verdier, four years later, as part of the "package" christened "Verdier's duality". . . ).<sup>439</sup>

### b<sub>3</sub> . Episodes of escalation

p. 853 **Note 169(iii)** □(169(iii)) The operation "cohomologie étale" continued throughout the eleven years, from 1966 to 1977, between the end of the SGA 5 seminar and the publication, one after the other, of the cut-and-dried volume "SGA 4<sup>1</sup>", followed by the massacre edition (known as the "Illusie edition") of SGA 5<sup>440</sup> (\*). It was achieved, above all, thanks to the joint participation, in deed and in omission, of my five "cohomologist" students:

**P. Deligne, L. Illusie, J-L. Verdier, J.P. Jouanolou, P. Berthelot**<sup>441</sup> (\*\*). Illusie is responsible for

<sup>439</sup>(\*)The **biduality theorem**, or "local duality theorem" (the two names are those I had given it), both in the coherent context and in the "discrete" (étale, in particular) context, is in the nature of a "local" Poincaré duality theorem, valid for "varieties" (algebraic or analytic, or "moderated" spaces etc.) that can have any singularities. It's an entirely new type of theorem in the arsenal of "basic facts" in the cohomology of spaces of all kinds, and it's an important and profound complement to the "six operations" duality formalism I've developed, to express with maximum flexibility and generality all phenomena of the "cohomology-duality" type.

gique" (like Poincaré). Along with the introduction of the  $Lf$  functor<sup>1</sup> (the "unusual" inverse image), it is one of the main ideas of the Poincaré family.

I've introduced the innovative formalism of the duality of varieties and spaces "of all kinds", both of which form the "soul" of the overall yoga of the "six operations".

In the coherent case, the demonstration of the biduality theorem is trivial. This does not prevent it from being what I unhesitatingly call a "profound theorem", because it gives a simple and profound view of things that would not be understood without it. (On this subject, see J.H.C. Whitehead's observation on "the snobbery of young people, who believe that a theorem is trivial because its demonstration is trivial", an observation I take up and embroider on in the note "The snobbery of young people".

young people - or the defenders of purity", n° 27.) In the discrete case, the demonstration is equally profound, using the full force of Hironaka's resolution of singularities.

Attributing the authorship of such a theorem to Mr. X (Verdier first, in this case, for the discrete analytic case, then Deligne for the discrete étale case, until the two friends agree to award the whole to Verdier alone), on the pretext that the aforementioned gentleman has copied an already known demonstration in a neighbouring context, or that he has been able to broaden the conditions of provisional validity (which I had identified in 1963) - and this without even deeming it useful to recall its origin, is what we used to call "in my day" a swindle. In short, I'll just have to wait for the relevant purity and resolution theorems to be proved, so that (in staggered cohomology) I can perhaps once again claim authorship at least of the biduality **theorem** (in the optimum framework, this time, of excellent schemes) - at a time when the great **ideas** that inspire and give meaning to theorems have become the object of general contempt.

(May 11) I should point out that the validity of the biduality formalism in the analytic case was of course known to me as early as 1963, when Verdier learned of it from me. In SGA 5, I always pointed out the validity of the ideas and techniques I was developing. In the mass-murder edition of SGA 5, Illusie took care to remove all trace of such comments.

<sup>440</sup>(\*) (March 12) It now seems inaccurate to me to consider that the "Cohomologie étale" operation ended in 1977 with the double publication "SGA 4 - SGA 5", which would be its "culmination" (as I write two paragraphs below). I've been misled here by the deliberate intention (convenient at times, but artificial) of wanting to "split" the "Burial" operation (of the deceased master and his *fi dèle*) into four separate operations - whereas these are in fact indissolubly linked. The real "culmination", or rather **apotheosis**, of the "Cohomologie étale" operation, and at the same time of the whole Burial, took place four years later at the Colloque (known as the "**Colloque Pervers**") de Luminy in June 1981 (which we'll be talking about in particular with "operation IV"). At this colloquium, where all-round cohomological formalism (coherent and sprawling) was the focus of general attention, **my name was no longer mentioned**... ..

<sup>441</sup>(\*\*) This solidarity was expressed, for each of these five ex-students, first of all by omission, by abstaining from any effort to contribute to making available to all a vast body of new ideas and basic techniques, through which they

(apart from Deligne's) which seems to me the most heavily committed, since it was he who assumed responsibility for the publishing-massacre, thus making himself the docile instrument of Deligne<sup>442</sup> (\*). p. 854

There can be no doubt about Deligne's intention to appropriate the "true" authorship of étale cohomology. It is attested by the very spirit of the whole "staggered cohomology" operation, which is without doubt unique in the annals of our science. It is also expressed, discreetly at first in 1975, in Deligne's biographical note (where any allusion to a cohomological tool I might have placed in his hands, and which might have played a role in his demonstration of the last part of Weil's conjectures<sup>443</sup> (\*\*), is absent), and resoundingly eight years later, in the brief but eloquent set of three texts (from 1983) that I have named "Funeral Eulogy" (in three parts)<sup>444</sup> (\*\*\*). They are examined with the care they deserve in the two notes "L' Eloge Funèbre (1) - ou les compliments" and "L' Eloge Funèbre (2) - ou la force et l'auréole" (n° s 104, 105) (and taken up, in a more penetrating light, in the later note "Les obsèques du yin (yang enterre yin (4))", n° 124). As for Deligne's autobiographical (and by no means funereal) "Eulogy", I review it in the two notes "Requiem pour vague squelette" and "La profession de foi - ou le vrai dans le faux".

(n° s 165, 166)<sup>445</sup> (\*\*\*\*)

□ The operation culminated in 1977<sup>446</sup> (\*), with the publication (in no particular order) of <sub>2</sub> (sic) - SGA p. 855  
"APG 4 <sup>1</sup>

5 ". This is the (provisional) culmination of a long, eleven-year **climb** in the burial of my work and my person, each new step of which is emboldened by the tacit encouragement I have found

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and, after 1976, by their **silence** in the presence of the very large operations of a Verdier (in 1976) and a Deligne (assisted by Illusie, the following year). In addition to Deligne and Illusie, Verdier played an active role in the "Cohomologie étale" operation, giving, with "the right reference" (see "episode 3" below), the "kick-off" to the dismantling of SGA 5, thus showing his friends that the time was definitely ripe for the large-scale operation that followed the year after without a hitch. As for Jouanolou, his active contribution was limited to "**going with the flow**", happily peppering his presentations with the de rigueur references to the pirate-text, and doing his best to gloss over the composer of the themes with variations that he unfolds with mixed conviction... .

<sup>442</sup>(\*) Illusie has also become **Verdier's** accomplice, covering up his deception of the previous year by refraining from alluding, in the introduction to SGA 5 or elsewhere, to my talks on the homological formalism and the homology class associated with a cycle.

<sup>443</sup>(\*\*) (March 12) Nor is there any allusion in this text, or (to my knowledge) in any other by him, to the fact that a substantial part of these conjectures had already been established by someone other than him. On this subject, see the sub-note "'La" Conjecture" (n° 169<sub>4</sub>) to the present note "Les manoeuvres".

<sup>444</sup>(\*\*\*) In my reflection on Burial, the encounter with the Funeral Eulogy, on the very day (May 12 last year) that a certain tableau d'un massacre burst into my investigation, marking an important moment. The long reflection "La clef du yin et du yang" (which gives its name to the second part of L'Enterrement) was triggered five months later by an unusual "association d'idées", which appeared the day after this encounter. It was triggered by a certain deliberate intention (unspoken, admittedly, but nonetheless laid out large. . . ) to "reverse roles" in the two "minute portraits" I'd just looked at a little more closely. . .

<sup>445</sup>(\*\*\*\*) For details of this autobiographical note, see also the last b. de p. note (dated December 29) at the end of the note "Le nerf dans le nerf - ou le nain et le géant" (n° 148). This notice was published by the "Fonds National de la Recherche Scientifique" (Belgian), rue d'Egmont 5, 1050 Brussels, on the occasion of the award of the "Prix Quinquennal" to pierre Deligne, in 1975.

In this two-page autobiographical note, as in the minute portraits that make up the "Funeral Eulogy", the art of thumb-sucking is exercised as much on the theme of "motifs" as on that of *l-adic* cohomology. In both texts, written eight years apart, the neuralgic point around which the reflexes of appropriation are concentrated seems to be Weil's "conjecture".

(March 12) Even more absolutely and defiantly than in the "textes - Eloges" examined in the four notes cited, the intention to appropriate bursts forth and spreads out in the **Colloque de Luminy** of June 1981 (see the b. de p. note of the same day, page 853, above). Or, to put it more accurately, an appropriation that had hitherto been symbolic and by **intention**, and which had previously expressed itself in groping manoeuvres (encouraged by the eager support of some and the indifference of all), became an **accomplished fact** at the brilliant Colloquium (at least in the unanimous consensus of all the brilliant mathematicians assembled on this memorable occasion, and in the general euphoria).

<sup>446</sup>(\*) (March 12) This is a provisional "culmination"! See the first of today's b. de p. notes, in this same note "Les manoeuvres" (p. 853).

in the previous stages, by general indifference and apathy (if not over-enthusiastic acceptance. . . ) towards their dubious nature. I've already mentioned some of these stages, with the "Motifs" operation reviewed earlier. I've identified three more episodes, more directly linked to the "Staggered Cohomology" operation, which I'd now like to review.

**Episode 1.** concerns the fate of a certain conjecture of the "**discrete Riemann-Roch**" type I had introduced in 1966 during the SGA 5 oral seminar, in the final lecture in which I had identified and commented on a number of open problems and unpublished conjectures. This presentation was lost in the Illusie edition, where no allusion is made (and not without reason. . . ) to the conjecture in question, or indeed to any of the many questions raised. Yet, seven years after the seminar, the conjecture reappears in the analytic context under the pen of Mac-Pherson, without any allusion to any seminar whatsoever SGA 5 (or to a schematic context), and under the unusual name of "Deligne-Grothendieck conjecture". This is the well-known article<sup>447</sup> (\*\*) in which Mac-Pherson proves this conjecture in the analytic context.

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□ During his visit last October, Deligne told me that in 1972 he had confined himself to **communicating** such information as

I told Mac-Pherson about my conjecture (which he had learned, along with the other SGA 5 listeners, during the oral seminar). He told me he was surprised by the name Mac-Pherson had given him, but didn't bother to write to him to have it rectified. On this subject, see the note "Dotting the I's" (n° 164, part II 1), and for further details on the conjecture itself, the long sub-note n° 87<sub>1</sub> to the note "The massacre" (n° 87)<sup>448</sup> (\*).

**Episode 2:** The vicissitudes of the SGA 7 seminar, devoted to questions of **monodromy in stellar cohomology**, which took place between 1967 and 1969 under the joint initiative and direction of Deligne and myself. Deligne made several contributions, the most important being his demonstration of the Picard-Lefschetz formula in the étale context. As with SGA 5, the writing of the oral presentations dragged on for several years - a bit like repeating the (beginning of the) scenario of the (non-)writing of its unfortunate predecessor! Publication finally took place in 1972 and 1973 (in Lecture Notes n° s 288, 340), thanks to Deligne, at a time when I had disappeared from the mathematical scene for three years. On his initiative, the seminar was **split into two parts**, the first presented as directed by me, the second as directed by him and N. Katz (who had simply been one of several lecturers during the second year of the seminar)<sup>449</sup> (\*\*).

In the first volume, SGA 7 I, published under my name, the detailed theory of evanescent cycles, which I had presented in a series of talks opening the seminar, is "slashed" to a twenty-page summary by Deligne (the other talks had been written within a reasonable timeframe, by myself and other seminar participants). As for Volume II, which appeared under the joint Deligne-Katz signature, and in which the part that

I had taken in the development of the main themes and results is no less than in Volume I' □ <sup>this</sup> part, is systematically retracted. I give more details on this subject in the note "Prélude à un massacre" (where I try to pinpoint the meaning of the APG 7 mini-operation) and especially in the note "Dotting the I's" (part II 5), n° s 56, 164.

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I'll confine myself here to recalling the biggest oversight. It concerns my transposition of the cohomological theory of "Lefschetz brushes" and of the "theorem" into the context of stale cohomology.

<sup>447</sup>(\*\*) Mac Pherson, Chern classes for singular algebraic varieties, Annals of Math. (2) 100, 1974, pp. 423-432.

<sup>448</sup>(\*) This conjecture will thus appear for the first time, in its original and complete form, only in Harvest and Sowing, and this almost **twenty years** after I recommended it to my students... .

<sup>449</sup>(\*\*) For the meaning I discern in this **cut**, which no mathematical reason justifies, see the note "Prélude à un massacre" (n° 56) quoted below, and also the sub-note "L'éviction (2)" (n° 169<sub>1</sub>) to the present note "Les manoeuvres".

irreducibility". This transposition of classical results, proven (when indeed they are proven. . . ) by transcendental means, was (as is often the case) not at all automatic. I remember spending days if not a whole week on it. To my knowledge, there is no other known demonstration of the main facts than the one I came up with at the time, using spectral sequences and the "well-known" structure (which I had determined in 1958) of the "moderated" fundamental group of an algebraic curve<sup>450</sup> (\*). This theory is reproduced in SGA 7 II, in a presentation by Katz (exp. XVIII) and according to the notes I had given him. In the introduction to the volume, Lefschetz's theory of brushes is presented (along with the Picard-Lefschetz formula proved by Deligne) as one of the two "key results" of the seminar, without any hint of a role for me in any of the themes developed in the volume. The only reference I know of in the literature to any such role for Lefschetz's theory is a laconic and ambiguous footnote<sup>451</sup> (\*\*) (after the title ("Pinceaux de Lefschetz") of Katz's talk, and the name of its author) "D'après des notes (succincts) de Grothendieck".

In Deligne's article "La Conjecture de Weil I" (169)<sup>452</sup> (\*\*\*) published in the same year (1973) in "Publications Mathématiques", Lefschetz's brush theory is an important technical ingredient in his demonstration of Weil's conjectures. In this article, Deligne doesn't even pretend to disregard my role in the l-adic trace formula (which is another crucial ingredient of his demonstration, the paternity of which was still all too notorious in well-informed circles)<sup>453</sup> (\*); on the other hand, when he takes care to formulate the results of the Lefschetz theory he is about to use, no allusion is made to my person. He merely refers to the relevant lectures in SGA 7, and it's unlikely that any unfortunate reader will ever unearth there the elusive footnote by his friend Katz. ...

**Episode 3.** The last episode I know of in the "escalation" took place in 1976, a year before the "culmination" of the "SGA 4<sup>1</sup> - SGA 5" operation. It was published in Asterisk (n° 36 (SMF), p. 101-151) of an article by J.L. Verdier entitled "Homology class associated with a cycle". Verdier was one of my five cohomology students, and (like his buddies) he had attended the SGA 5 seminar, wisely taking notes without really knowing what he had gotten himself into there. In the ten years since then, he (like his buddies) has finally figured it all out. The fact remains that in this article he takes up a number of ideas I had developed in the seminar in question, at length and "in front of listeners who begged for mercy", around the biduality theorem and, above all, around the formalism of homology and cohomology classes associated with a cycle<sup>454</sup> (\*\*). In this article, my name is not mentioned (except once,

<sup>450</sup>(\*) In the introduction to Katz's presentation, which will be quoted here, he generously attributes this theorem to my former student Michèle Raynaud, who presented it in the SGA 1 seminar in 1950/61.

<sup>451</sup>(\*\*) This note is ambiguous, in that it is careful not to assert authorship, which could just as well be due (unless otherwise stated) either to the author of this XVIII exposé, or to the other co-author of the volume (as the introduction to the volume implies by omission). Following Grothendieck's ("succinct"! ) notes in no way implies that there aren't several demonstrations (some of them earlier) from which he would have done me the honor of choosing my own. This (as elsewhere in the same volume) is a typical example of the "inch!" style so dear to my friend Deligne, who has obviously set an example... ...

<sup>452</sup>(\*\*\*) see sub-note ""The" "Conjecture"" (n° 169<sub>4</sub>), from a b. note here.

<sup>453</sup>(\*) The following year, however, in his autobiographical note (discussed in the two notes already cited, n° s 165,166) Deligne cannot deny itself the satisfaction, however symbolic, of skirting this role. It's true that this was a text for circulation very limited, which perhaps no mathematician "in the know" has ever held in his hands except me. But three years later, in the volume entitled "APG 4<sup>1</sup>", destined to become a standard reference text, the same trickery (albeit implemented with an even greater dexterity, given the circumstances. . . ) is used, this time for a wide audience of non-specialist "users". ) is set up, this time for a wide audience of "users", non-specialists in stellar cohomology. For a dismantling of this masterfully executed deception, see the sub-notes group

"La Formule" (n° s 169 -169<sub>58</sub>) to the present note, as well as the two sub-notes that precede it, "Le cheval de Troie" and ""La" Conjecture" (n° s 169<sub>3</sub>, 169)<sub>4</sub>.

<sup>454</sup>(\*\*) The idea of defining the **homology** of a scheme (or "space" . . . ) as its hypercohomology with values in a "complex

p. 859 by way of a joke of a very particular kind. . . ), and no allusion □ is made to any SGA 5 seminar the author may have heard of. Details can be found in the two notes "The right references" and "The joke - or 'complex weights'" (always the same weights, no mistake. . . ) n° s 82, 83.

It was from this "memorable article" that the duality formalism on analytic complex spaces, for analytically constructible discrete coefficients, reproducing the one I had developed (as early as 1963 and especially, in SGA 5 in 1965/66) in the étale schematic context, became succinctly the "Verdier duality" - until five years later (in the euphoria of the June 1981 Colloque de Luminy) the same sleight of hand was performed for étale duality too. But here I'm anticipating (as I already did with the episode of the "memorable article" itself) the **third** major operation, this time with Verdier as the main (if not the only) "beneficiary" - an operation that will be discussed below<sup>455</sup> (\*).

## b<sub>4</sub> . Impudence

**Note** 169(iv) Verdier's article shed an unexpected light on the fate of SGA 5 in the hands of some of my former students. It showed me what kind of "benefit" they could find in their exclusive knowledge of the ideas and techniques I had developed in SGA 5, for their benefit above all others. It also showed me, without doubt, the connivance and solidarity of all my cohomology students with this kind of operation. By calling this article

"the right reference", I hadn't thought to name it so well - it did become (as confirmed to me from various quarters) a standard reference text, which none □ of them could certainly ignore. This is what ends up to me in the notes "Silence" and "Solidarity" (n° s 84,85). I knew I shouldn't be surprised that in the Illusie edition of what was once the SGA 5 seminar, no allusion is made, at any point, to a formalism of homology (and homology classes associated with cycles) that I would have developed in that seminar - and indeed there was no need to mention it, since (ten years later) his buddy Verdier had already taken on the task of providing the missing reference to general satisfaction<sup>456</sup> (\*).

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In the course of the SGA 5 seminar, I had taken up the theme of the "dualistic" cycle class in the 1950s (in the coherent framework), in great detail in the staggered framework. The methods I had developed on the theme of the cohomology (first) and homology (second) class associated with a cycle, starting in the second half of the fifties (in the coherent framework), and of which I presented a synthesis (staggered version) in SGA 5, were "all-purpose techniques", applicable to both continuous (De Rham, or Hodge style) and discrete "coefficients", and in the schematic as well as the analytic or differentiable framework (among others). The need for such a theory had, moreover, been one of my main motivations for developing (as early as the 1950s) a formalism of cohomology "with supports" in a closed space (with the very useful spectral sequence "from local to global"), intended to provide an "algebraic" equivalent for the classical (and elusive) "tubular neighborhood" of a closed subspace. It was also on this occasion that I first developed (in both coherent and discrete contexts) cohomological "purity" and "semi-purity" statements.

<sup>455</sup>(\*) See "Sharing" notes, n° s 170 (i) - (iii).

<sup>456</sup>(\*) As for the **cohomology** variant (just touched on in Verdier's article, which Deligne refrains from quoting), it is awarded (as we have seen) to Deligne. As I am duly presented as the author of the presentation hacked by Deligne, there was no major reason to conceal the disappearance of SGA 5 from my presentations on this theme. Illusie mentions it "in passing" in the introduction to his pen, without the matter being deemed worthy of explanation (and nobody before me seems to have been surprised, indeed. . . ). On the contrary, right from the second sentence of this introduction, it is clearly stated that

"the **only significant changes** from the original version concern Lecture II [finitude theorems]), which is not reproduced, and Lecture III [Lefschetz formula]. ... "(emphasis added).

Given the little and given the context, I shouldn't be surprised if my ex-pupil affects not to see any **other** "important changes" in the living, harmonious body that I had once entrusted to his and my other pupils' hands, a body reduced in



The "good reference" provided by Verdier, like the "memorable volume" devoted to Deligne's partial exhumation of the motifs, is for me pure plagiarism. The same cannot be said of the text known as "SGA 4<sup>1</sup>"<sup>457</sup> (\*\*). Certain shapes are still preserved, in the de rigueur "pouce! excels at constantly **suggesting** the false, without ever (or almost. . . (169)<sub>3</sub><sup>458</sup> (\*\*\*) goes so far as to suggest en clair. My first confrontation with "SGA 4<sup>1</sup>" and with the particular form that this style takes there p. 861 (that of disdainful depreciation<sup>459</sup> (\*) is in the note "La table rase" (n° 67).

But the operation in question strikes me above all, more than a banal plagiarism ever could, by a certain dimension of **impudence**. To my mind, none of the other three operations reaches this extreme dimension<sup>460</sup> (\*\*). And it affects me more strongly than any of the other three, perhaps, because even more than that, it affects me like an act of violence, like a massacre "for the pleasure of it" of a fine work that I had brought to completion and into which I had put my whole self - for the sake, before all others, of those who went on to destroy it, to make it the fodder for their own self-importance, and (under the guise of people of high standing and exquisite company) to come and display their discreet insolence and airs of complacent contempt<sup>461</sup> (\*\*\*)).

## . The hoard

**Note** (169(v)) □ (February 28) The two "operations" I have just reviewed, like the fourth p. 862 (known as the "Perverse Colloquium") were carried out with the participation or connivance of many, for the "benefit" (it would seem) of one. This is a striking feature common to all three.

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the Illusia edition to the state of a deformed corpse! And it's just one "change" among many, not an "important" one, that two inseparable friends have **shared** one of the "packages" of presentations I had developed with infinite care: the part awarded to Verdier having become, already a year since the publication of SGA 5, "the" good reference that everyone was waiting for (Deligne dixit), and the part awarded to Deligne becoming "the" good reason to duly quote the indispensable basic text "SGA 4<sup>1</sup>" at every turn of the page, and moreover, to present their late master as the humble (and confused) collaborator of his most brilliant pupil. . .

<sup>457</sup>(\*\*) (March 21) Further reflection in the series of sub-notes grouped under the name "The Formula" (n° s 169<sub>5</sub> to 169<sub>8</sub>) has shown me that this impression was wrong, despite "certain forms" that are still retained. ...

<sup>458</sup>(\*\*\*) On this subject, see the sub-note "Le cheval de Troie" (n° 169<sub>3</sub>), taken from a b. de p. note here, which was supposed to explain this "or almost. . .".

<sup>459</sup>(\*) It's the "depreciation" that affects to make a clean sweep of the "gangue of nonsense" amassed by a "confused" ("though rigorous". . .) and wishful thinking predecessor. . .

<sup>460</sup>(\*\*) (March 11) This assessment is, of course, entirely subjective. As I wrote this line, I hesitated a little, thinking of the unimaginable "operation" of the Colloque Pervers (or "operation IV", which will be discussed later). This memorable Colloquium was indeed a collective **apotheosis** of the Burial of my person, by that of a reckless continuator (Zoghman Mebkhout) interposed. It was on this occasion that I realized that this apotheosis is at the same time a natural **extension** and ultimate **culmination of the** "Cohomologie étale" operation, of which the "SGA 4<sup>1</sup> - SGA 5<sub>2</sub>" episode was, in fact, only a provisional "culmination". In the latter, my ex-student Deligne can't help referring here and there to my modest person and my work, albeit reluctantly, and to distance himself from it with dismissive epithets. At the Colloque de Luminy in June 1981, on the other hand, where cohomologie étale was the focus of general attention, my name (as well as that of the unknown Zoghman Mebkhout) was never mentioned... ..

<sup>461</sup>(\*\*\*) This sufficiency and contempt can be seen quite clearly in and between the lines of the volume entitled "APG 4<sup>1</sup>" (probably the only one of its kind in the history of our science). They also made their appearance, in the very year of publication

of this volume (albeit in more subdued tones), in Pierre Deligne's personal relationship with me. (See the note "Les deux tournants", n° 66.) I found them in the casualness of this and that other of my students, refraining from answering letters about things that were close to my heart or that had pained me. I found them, in touches

between the lines in the introduction to the "Illusie edition" (or massacre edition) of a work done with love, and also last year, in the paternally condescending airs of yet another student (referred to in the note "The joke - or 'weight complexes'", n° 83).

operations, confirming the thinking behind the note "The Gravedigger - or the whole Congregation" (n° 97).

But I see a more insidious common thread in the first two operations, based on motifs and staggered cohomology, concerning a certain **spirit that** animated them. What we're talking about here is a certain inner attitude towards the **possession of high-level scientific information with** limited circulation, or at the very least, information confined to a group of a few people linked by alliances of interest (or even to a single person), who use their power to **block its circulation for** as long as it seems advantageous to them to reserve the exclusive "benefit" of it for themselves.

Thus, after my "departure" in 1970, Deligne was **the only one** (apart from myself) to have intimately assimilated the "yoga of motives" and to have felt its full significance - to make the use of it that we know. My five cohomology students (including Deligne), and perhaps another two or three ex-SGA 5 listeners who had the perseverance to really assimilate its substance, were **the only ones to** have at their exclusive disposal the ideas and techniques I had developed in that seminar.

In both cases, whether I was speaking to Deligne in countless one-to-ones between 1965 and 1969, or to the select group of SGA 5 listeners in 1965/66, if it is true that it was "for their benefit above all others" that I was explaining and developing at length before them a certain inner vision, it was **not** as representatives of some "interest group" that I was placing in their hands those things which were of value to me. For me, it was self-evident that I was addressing them as people who, like me, were driven by a natural desire to prove themselves and to make a contribution to **knowledge**.

of mathematical things, through a **spirit of service**, towards a "mathematical community" with no boundaries in space or time<sup>462</sup> (\*).  And what I put into their hands, I knew well

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that these were not "curiosities", museum pieces, but living, burning things, made to grow and swarm - and this was indeed what was immediately sensed by those to whom I was addressing<sup>463</sup> (\*). If I addressed them, it wasn't as a kind of **shareholders** to whom I'd entrusted shares, in the name of some common "interest", but as **people** to whom I was linked by a **common adventure** - people, therefore, who would be keen to act as **relays for** the "information" I was communicating to them (even if it meant putting their own spin on it, passing it on to those around them...), just as I myself would relay it on their behalf<sup>464</sup> (\*\*).

With the benefit of almost twenty years' hindsight, I realize that there was a fundamental misunderstanding between them and me.

- we weren't on the same wavelength. What I had entrusted like living things into hands that I believed to be loving, was hoarded like some kind of **hoard** that we would hasten to bury. Possession of the hoard represented a certain **power** (admittedly derisory, given the price. . .) - if only the power to hold back, to prevent (if only for a while) a living thing, made to blossom and flourish, from being buried.

<sup>462</sup>(\*) On the subject of such a "spirit of service", see in particular the note (also quoted below) "Yin the Servant, and the new masters" (n° 135).

<sup>463</sup>(\*) (April 10) That didn't stop some of them from doing their utmost, after the fact, to debunk what they had hoarded. at length, after having struggled at first (apart from Deligne) to grasp its meaning and scope and to assimilate it. I see in this tone of debunking (which goes hand in hand with the "magot" attitude mentioned below) a double **compensation**. On the one hand, it evacuates a sense of unease (created within them by the misappropriation of something that is not theirs, but **everyone else's**), by pretending to **devalue** what has been misappropriated in their own eyes. On the other hand, there's the compensation for the "father", seen as the embodiment of a creative force that would surpass them (whereas they are unable to assume the same force, which rests in them as in the one they secretly blame... ). My "deceased" state, and the example set by the direct heir, created a favorable conjuncture for "venting" a secret antagonism, the "father" now being felt to be in a **position of weakness, of inferiority**.

<sup>464</sup>(\*\*) So it was to this "mathematical community without frontiers" that I was addressing, at the same time as to them and through them. I've explained elsewhere (see b. de p. (\*) on page 847) why I didn't take it upon myself, at least in the year following this seminar, to rewrite it on line and make it available to everyone.

to blossom and spread.

I've tried to grasp the two attitudes, of different essence, that confront each other in this "misunderstanding"<sup>465</sup> (\*\*\*), in the two notes "Yin the Servant, □ and the new masters", and "Yin the Servant (2) - or generosity" p. 864 (n° s 135, 136). I don't want to seem to be posing here as the exemplary embodiment of the "attitude of service", as opposed to the "attitude of caste": one in which "knowledge" becomes the distinguishing mark of an elite and (at a more advanced stage in the degradation of morals) the means of arbitrary power over others. As the reflections in *Fatuité et Renouvellement* (the first part of *Récoltes et Semailles*) reveal, the reality is more complex. I saw in myself, and in some of my actions in my past as a mathematician, the seeds of the general degradation I see today. And it's just as true that this "**service impulse**" within me has been a powerful driving force in the development of my written mathematical work, and more particularly, in the tireless pursuit of the two series of EGA and SGA foundation texts<sup>466</sup> (\*).

I don't seem to have been able to communicate anything to my students about this impulse, or the attitude that drives it.

reflected. The work undertaken, insofar as it embodied a "service" attitude and disposition of a community, came to a screeching halt after I left<sup>467</sup> (\*\*) - as if by a sudden stroke of □scie (or p. 865 chainsaw...<sup>468</sup>(\*)).

From the echoes that still reach me here and there from the world I left behind, I can see that this spontaneous attitude, which I had in common with the benevolent elders who welcomed me in my early days, has become (like this very benevolence) a **stranger** in the world that had once been mine.

## . Eviction

**Note** 169<sub>1</sub> (March 9)<sup>469</sup> (\*\*) SGA stands for "Séminaire de Géométrie Algébrique du Bois Marie". It designates (or at least, in the sixties, it designated) the seminars in which I developed, between 1950 and 1969 (and in collaboration with students and others, from 1962 onwards) my program of the foundations of new algebraic geometry, in parallel with the (less "advanced") texts,

<sup>465</sup>(\*\*\*) In writing these lines, and this word "misunderstanding", the association came to me with Zoghman Mebkhout's letter (quoted in the note "Echec d'un enseignement - ou création et fatuité", n° 44'), which spoke of a "sort of misunderstanding" between my students and myself (putting aside Deligne. . . ). At the time, I wasn't sure I'd understood what "sort of misunderstanding" I was talking about.

hension" he meant. Could it be the same as this "misunderstanding" I'm talking about here - and that he would have excluded Deligne from it, by his deliberate intention (which surprised me more than once in my friend) to see him only "in pink"?

<sup>466</sup>(\*) This "relentless pursuit" often went against another equally strong impulse in me, that of letting go of all the "tasks" that were holding me back, and launching myself ever further into the unknown before me, which was constantly calling me (and still calling me. . . ).

<sup>467</sup>(\*\*) (April 10) In retyping these lines, I'm struck by a singular irony of the situation, the meaning of which (like that of the Burial as a whole) is not yet fully grasped. It is the man who has invested himself entirely in tasks of "service" for the benefit of a certain "mathematical community", who finds himself ousted from his very work, and with the tacit and unreserved approval of said "community", by the very people who have made the **refusal of service** a caste imperative and a second nature.

The apparent paradox seems to me to be resolved to a large extent, however, by remembering that the "community" to which this "service impulse" in me was addressed was by no means the sociological entity (with its "caste" of notables etc.) that was an unreserved stakeholder in my Burial; but it was that "mathematical community without boundaries in space or time" referred to above. (For comments on the distinction and confusion between those

two "communities", see the first b. de p. note to the subsequent "Respect" note (n° 179).

<sup>468</sup>(\*) On the subject of the "chainsaw" effect, cutting short (especially in almost all of my students) the lively, vigorous impetus of a work that was just beginning, see the two notes "Les cohéritiers... .", ". . and the chainsaw" (n° s 91, 92).

<sup>469</sup>(\*\*) This sub-note is derived from a b. de p. note to the main note "Maneuvers" (see b. de p. note(\*) page 848 )

and in more canonical style) of the EGA series ("Eléments de Géométrie Algébrique")<sup>470</sup> (\*\*\*). These seminars were held at "Bois Marie", the site (in Bures sur Yvette) where the IHES has been based since 1962. In fact, the first two seminars (between 1950 and 1962) were held in a makeshift room in Paris (at the Institut Thiers), in front of an audience of no more than a dozen people, and in front of whom I strictly "went it alone". The acronym SGA dates from those years, when there was no question of "Bois Marie". I later added this pretty name to the original "Séminaire de Géométrie Algébrique", to make it less austere.

It goes without saying that these seminars, from SGA 1 to SGA 7, are numbered in chronological order. It goes without saying that the overall conception of each of these seminars came from me. It was inspired by my overall, long-term goal of laying a broad foundation for algebraic geometry, and increasingly, for a broader "geometry", which I felt very strongly from the outset.

from 1963 onwards, which remained unnamed. (Today, I would call it "geometry".

p. 866 arithmetic", a synthesis of algebraic geometry, topology  $\square$  and arithmetic<sup>471</sup> (\*).). The last of one of these seminars was SGA 7, which (unlike its predecessors) ran for two consecutive years, 1967-69, and was run in collaboration with Deligne.

The volume with the misleading name "SGA 4<sup>1</sup>" is (as explained above, pages 847 and 851) made up of texts dating from after 1973, i.e. after the last of the SGA seminars, apart from those plundered from SGA 5, and the famous "Etat 0" of a "thesis" by Verdier (to be dealt with in Operation III). All questions of dates aside, the heterogeneous nature of the texts making up this volume is in no way in keeping with the spirit in which I had pursued the SGA series, in which each volume presented a **large-scale groundwork** on a part of my program that had not yet been developed elsewhere - to the exclusion, therefore, of volumes of "**digests**", or **compilations** of results already known and well-developed, or even new results of a sporadic nature. At the very least, giving Deligne's volume the name SGA 8 (assuming I agree to this) would have been inappropriate, as it would have suggested the (unfounded) idea of a **continuation** of the work I had pursued in the previous seminars SGA 1 to SGA 7. As for the acronym "SGA 4<sup>1</sup>" chosen by Deligne, it is not only "inappropriate", but in itself constitutes a **deception and a sham**. This is something that should be **obvious** to every one of the many mathematicians who, since 1977, have had occasion to acquaint themselves with this volume, and who, moreover, know the meaning of the acronym SGA, inseparable from my person and my work, and thus also from a certain **spirit**. This does not alter the fact that this imposture, in the very name of a standard reference text, has been tolerated by the "mathematical community" for eight years, without apparently "making any wrinkles". Along with the Colloque Pervers of 1981, which is a natural extension of it, I see in it **the great disgrace of** the mathematical world of the 70s and 80s, a disgrace that seems to me unprecedented in the history of our science.

There was a precursor episode to this **operation-eviction**, designed to give the impression that my person would play only an occasional, scrappy and incidental role in the development of fundamental APG texts. This is the "SGA 7 mini-operation". This operation is mentioned in "episode 3" (of a

p. 867 escalade) in the note  $\square$  "Les manoeuvres" (n° 169), and above all (from the point of view that interests me here) in the note "Prélude à un massacre" (n° 56). This is the publication, in a separate volume SGA 7 II, of part of the original seminar, under the names of Deligne and Katz and to the exclusion of myself (and disregarding the role I played in the development of its main themes and certain key results). I write to

<sup>470</sup>(\*\*\*) Written in collaboration with J. Dieudonné.

<sup>471</sup>(\*) See b. de p. note (\*) on p. 844.

(n° 56):

"This "SGA 7" operation is by no means a **continuation** of the work pursued in the SGAs, but I feel it as a kind of brutal "saw blow" (or chainsaw. . . ), **putting an end to** the SGA series, with a volume that ostentatiously stands apart from my person, even though it is linked to my work and bears its mark just as much as the others."

These volumes, SGA 7 I and SGA 7 II, do not yet display an air of condescension and thinly veiled contempt for the work from which they derive. If this step in the escalation could nevertheless be taken four years later, it's because the previous steps (including this seemingly innocuous SGA 7 mini-operation) had "passed", without ever (to my knowledge at least) eliciting the slightest reaction in the mathematical world. I'd like to end with an edifying (no doubt provisional) epilogue to the operation to evict me from the SGA, an eviction implemented by Deligne with the tacit approval of "the entire Congregation". This is the very "cool" reply I recently received from Mrs. Byrnes, in charge of "Lecture Notes" at Springer Verlag, to whom I had written to ask for clarification concerning a volume entitled SGA 5, published under my name in 1977 in the "Lecture Notes", without Springer having seen fit to ask for my agreement, or even to inform me of this publication carried out by them. I learned from his letter (received a month later) that it was all the more pointless to bother with such a formality, **since I wrongly claim to be listed as the author of the said volume SGA 5, edited by L. Illusie**, given that I **only** appear on the cover **as the director of this seminar!** (And one wonders, in retrospect, what the late director was doing at the seminar... . ) I wrote, just to see, to Mr. K.F. Springer himself, about various strange experiences I've had with Springer Verlag since 1972 (the year SGA 7 I was published under my name in the same way - admittedly, I'm no more an "author" than I am a "publisher").

don't follow SGA 5... . ). I'm still waiting for his reply... <sup>472</sup>(\*).

□(March 16) This sub-note has been given the appropriate name, "Eviction (2)". The (2) sign is a reminder that there are

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already had another note by the name of "L'éviction" (n° 63), to which I had occasion to refer recently (with the "Motifs" operation). The "eviction" referred to (very discreetly. . . ) in that note was the one that took place in 1970, when I left the IHES, a departure that obviously suited my brilliant young friend, who had recently moved to <sup>473</sup>(\*). The connection between these two "evictions", one from the IHES and the other from the SGA series, seems obvious to me. I note a striking progression, in the nature of yet another "escalation": the first time, it was simply a matter of me being ousted from an **institution**, to which I certainly felt very strongly attached (I could see myself finishing my days there, really!), but from which I very quickly detached myself, without any residue of regret. The second time was when I was ousted from the SGA, which itself represents (symbolically certainly, and even more than symbolically) my work as a mathematician - a work to which I remain attached to this day. It's true that my "eviction" from the IHES has been over for fifteen years now - but I doubt, despite everything, that the same will be true of my eviction from a work to which I had devoted fifteen good, hard years of my life.

I've been thinking about the fact that I once made it easy to oust myself from the SGA, by following my spontaneous impulse to present those of my students and collaborators who had invested full-time, at certain times, in the development of one of its seminars, as "leading" the seminar in the same way as I did. It wasn't customary in my day, and it certainly isn't today. I don't know whether

<sup>472</sup>(\*) (April 9) For the rest of the story, see the note "Les Pompes Funèbres - - im Dienst der Wissenschaft" (n° 175).

<sup>473</sup>(\*) The episode of my departure from the IHES (in 1970) is mentioned in the section "La récolte inachevée" (n° 28) and in the notes "L'arrachement salutaire", "L'éviction", "Frères et époux" (n° s 42, 63, 134), and finally in the sub-note (n° 134<sub>1</sub>) to the last-mentioned note.

I did the right thing. On the one hand, it didn't entirely correspond to reality, in the sense that there was no symmetry in the role I played there, and in that of my collaborators, even if they were brilliant and as committed as I was. This presentation of things is in line with the "ambiguity" (or "complacency" towards brilliant young mathematicians) that I examine in the notes "L'ascension" and especially "L'ambiguïté" (n° s 63', 63"). If this ambiguity introduced by me has encouraged some of those who have intensely collaborated with SGA at one time or another, to "oust" me (more or less partially or more or less completely), I would be wrong to hold it against them! I reap  $\square$  simply what I sow.

But that in no way prevents me from making a public statement about what happened.

On the other hand, it's also true that the relationship I was establishing with certain colleagues could be perceived by them as a mark of esteem and trust (which it was), and at the same time encourage them to invest themselves fully in the task, just as I was investing myself in it. But now I'm thinking that such esteem and trust can be expressed in an equally clear and encouraging way, without being tainted by ambiguity. It was a bit as if I were "**buying**" an investment commensurate with the task, by granting an "**advantage**", an "advantage" moreover which (with hindsight) seems dubious to me. For it's a false advantage to appear to be something you're not. And it's quite clear that in creating an appearance that was (if not entirely false, at least) a little false around the edges, it was **my** responsibility before that of anyone else, of me as the elder, that was engaged.

Decidedly, the reflection is increasingly similar to that of the note "Ambiguity", in the unforeseen light of a "species situation" that I hadn't even thought of when I wrote it. I realize that, just as my relationship with the (by no means unrecognized) "young genius" Deligne was false, because out of false modesty I refrained from assuming the role of elder and "master" that was indeed mine with him, so too was my relationship with other brilliant young people, investing themselves wholeheartedly in a task that seemed "common" to me at the time<sup>474</sup> (\*).

The reflections in l'Enterrement made it quite clear that, if there was a "common" task, it was for the space of a year or two, the time it took for the young man to complete (say) a thesis (which is not bad at all). The very year of my departure in 1970 signaled my immediate and almost total abandonment of this vast and visibly burning set of "tasks", which I was well and truly committed to.

"burned in my hands" the day before still<sup>475</sup> (\*\*). Apart from Deligne's work on Weil's conjecture, this  $\square$  was at the same time the beginning of a long stagnation in each of the major themes that had most interested me.

a stagnation which (apart from the partial "revival" triggered by the philosophy of Mebkhout- the unnamed) continues to this day<sup>476</sup> (\*).

## b<sub>7</sub> . Good Samaritans

<sup>474</sup>(\*) I begin to realize that this was an illusion, at the end of the note "Le feu vert" (n° 68), p. 260.

<sup>475</sup>(\*\*) This immediate abandonment of a program and burning tasks, on the very day after my departure, is evoked in the note "Instinct and fashion - or the law of the strongest" (n° 48), and especially in the double note "Les cohéritiers. ...", ". . . and the chainsaw" (n° s 91, 92), where I try to review (according to the echoes that have come back to me) what has become of the themes that had been taken up by my various "pre-1970" students.

<sup>476</sup>(\*) (March 17) This impression of "stagnation" will perhaps take on a more concrete meaning in a later note, where I intend to make a short annotated enumeration of the most "burning" themes that were on my agenda, and which were left behind, as soon as I left and with perfect ensemble, by those who had been my students.

(April 9) On this subject, see the note "Le tour des chantiers - ou outils et vision", n° 178.

**Note** 169<sub>2</sub> (March 13)<sup>477</sup> (\*\*) In this introduction to SGA, Illusie warmly thanks Deligne for, among other things

"convinced to write... ... a demonstration of the Lefschetz-Verdier formula, **thus removing one of the obstacles to the publication of this seminar.**"

(emphasis mine), in other words: the obstacle of **Illusie's lack of "conviction"** to write what he had been committed to writing for **eleven years** - which lack suddenly ends, as mentioned above, at the precise moment when the good Samaritan Deligne gives the "green light" to the good Samaritan Illusie that he "could go"... .

This is the "**true** within the false". As for the **falsehood** that this passage is obviously trying to suggest, without having to say so clearly (in a tried-and-tested style that has become a textbook . . . ), it's that the SGA 5 seminar **would depend on** the formula in question (which was established at the time of the seminar only on the basis of hypotheses for resolving singularities, since lifted, in the most common cases, by Deligne's finiteness results presented in the "earlier" volume entitled "SGA 4<sup>1</sup>"<sup>478</sup> (\*\*\*)). In fact, as the two friends know just as well

well as I do, the role of this Lefschetz-Verdier formula in SGA 5 (as in my demonstration of the cohomological formula  $\square$   $\ell$ -adic of  $L$  functions) had been purely **heuristic**, providing the **motivation** to look for and prove "explicit" fixed-point formulas (i.e. where the "local terms" could be calculated explicitly). In this way, Illusie joins forces with his friend to create the impression that SGA 5 is indeed (and in a sense that is not clearly explained by him or his friend) **subordinate to** the text, which can therefore only be called "SGA 4<sup>1</sup>".

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For details, see the note "Le massacre" and its sub-note n° 87<sub>2</sub><sup>2</sup>. In this note and all its sub-notes, I eventually discovered (better late than never) that this entire introduction written by Illusie, and in general, the overall presentation of the edition-Illusie (or edition-massacre), is a model of bad faith, served up casually and with those airs of candor that make his person so charming.

This touching impression that Illusie is striving to create, that it was indeed **thanks to** the good Samaritan Deligne (and the second good Samaritan Illusie, needless to say) that the unfortunate SGA 5 seminar ended up being published (eleven years later, and in the state I know), apparently "passed" without any problems. I found this version in Serre's report on Deligne's work, written in 1977 for the International Committee for the Award of the Fields Medal. I have no doubts about Serre's complete good faith, as he had only followed the intricacies of the oral seminar from a distance - not to mention that a lot of water had passed under the bridge since then... . He surely took at face value (like everyone else, and without question) what was said or suggested in the introduction to Illusie, which he must have read one day, to see (and he saw nothing!) . .

Interestingly, this same de Serre report is also the only place in literature, to my knowledge, where it is stated (in this case, in the very first sentence of the report) that Deligne was my pupil. No publication by Deligne, on the other hand, could lead any reader to suppose that the author might have learned anything from me.

## . The Trojan horse

<sup>477</sup>(\*\*) This sub-note is taken from a b. de p. note to the note "Les manoeuvres" (n° 169) (see note (\*\*) on page 849). For a more detailed dismantling of the "inch!" technique for making a "user" in a hurry believe a lie, see the sub-notes "The Trojan Horse" and "The Formula", n° s 169<sub>3</sub> and 169-169<sub>58</sub>.

<sup>478</sup>(\*\*\*) See b. de p. (\*\*\*) page 841 and (\*) page 850.

**Note** 169<sub>3</sub> (March 10)<sup>479</sup> (\*) In the sub-note (n° 67<sub>1</sub>) to the note "La table rase", I point out two examples where Deligne has disregarded his usual caution, and has indeed "advanced to say in plain language" the wrong thing. For the

p. 872 curious and sufficiently well-informed reader, and who would not have at hand the said note and sub-note, I point out that, apart from the "kindnesses" towards SGA 4 and SGA 5' and the somewhat blatant "omissions" of my humble

The blatant swindles I've identified are concentrated in paragraphs 3 and 4 on page 2 (in "Fil d'Ariane pour SGA 4, SGA 4<sup>1</sup>, SGA 5" - admire the beautiful procession here. . . ). These seventeen lines are a model of the art of "fishing in troubled waters", and would be well worth a detailed analysis<sup>480</sup> (\*).

Suffice it to note here that in the first of the paragraphs quoted, we read that, to establish "in stellar cohomology a duality formalism analogous to that of coherent duality", "Grothendieck used the resolution of singularities and the purity conjecture"<sup>481</sup> (\*\*). We then add that in the present volume (thanks be to Heaven and the brilliant author), these "**key points**" are established by another method" (emphasis mine), valid "for finite-type schemes on a regular scheme of dimension 0 or 1", i.e. in virtually all cases encountered by the user.

□ And so, Deligne strives to create the impression, and even clearly states, that all the formalism of dualité étale that I had developed remained conjectural (at least in non-zero characteristic), and that "these key points" were ultimately established only by him, Deligne, and in the present volume, i.e. by his finitude results (those already mentioned in previous b. de p. notes, results to which, incidentally, he immediately refers). This would indeed, as if by magic, lend credence to the fiction of the famous "**logical dependence**" of SGA 5 on the text entitled "SGA 4<sup>1</sup>" (a dependence<sub>2</sub> posited by this very name, and by the beautiful procession "SGA 4 - SGA 4<sup>1</sup> - SGA 5"), and thereby justify the incredible assertion (already quoted and commented on) in his introduction:

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"Its existence [of "SGA 4<sup>1</sup>"]<sub>2</sub> will soon make it possible to publish SGA 5 as is.

So here's the **Deligne version**, slipped in here and there in the saw-cut text called "SGA 4<sup>1</sup>", and

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<sup>479</sup>(\*) This sub-note to the "Maneuvers" note is taken from a b. de p. note to that note, see b. de p. note (\*\*\*) on page 860 .

<sup>480</sup>(\*) For further details and comments on the second of these two paragraphs, see the sub-note "Double entendre - or the art of the con" (n° 169 ).<sub>7</sub>

<sup>481</sup>(\*\*) The text follows on from "conjecture of purity", with: "established in a relative framework [? ?] in SGA 4 XVI, and - modulates the

resolution - in equals characteristic in SGA 4 XIX". The "in a relative setting" (incomprehensible to any reader who isn't already in the know) is a way of hiding the fact that this theorem was acquired for smooth algebraic varieties in any characteristic.

(March 17) I'm only just noticing the charm of the end of the paragraph quoted, which had "fallen by the wayside" in the first readings:

"**Various developments** are given in SGA 5 I. In SGA 5 III, we show how this formalism [??] implies the very general Lefschetz Verdier trace formula." (emphasis added.)

We admire the "various developments" without any further precision, whereupon the author (who on other occasions knows how to be precise) follows up with "this formalism" (= various developments?), which "implies the very general formula of traces"; only to point out immediately, in the very next sentence (in the following paragraph), that the said formula, "in the original version of SGA 5", was "established only conjecturally".

I've just checked in SGA 5 what these "various developments" are in SGA 5 lecture I. The title tells me: "Dualizing complexes", so also biduality theorem. Why "various developments" instead of "theory of dualizing complexes" or "biduality theorem"? It wasn't any longer, and it still sounded less muddy! This reminds me that in the famous "Finitude" lecture, i.e. in the "Trojan Horse", the brilliant author demonstrates a "biduality theorem", without any allusion to my modest person - which theorem is also christened "Deligne's theorem" (in the introduction to the lecture I in question in SGA 5, written by Illusie). It all adds up...

NB. For comments on this biduality theorem (treated with such false nonchalance. . . ), see the long b. de p. note (\*) on page 852.



The reality is that as early as March 1963, I had established the **complete formalism of the six operations** in the étale framework (thus going far beyond the usual "Poincaré duality"), with no restrictive hypothesis other than the (obviously indispensable) one of working with torsion coefficients "prime" to the residual characteristics of the schemes envisaged<sup>482</sup> (\*). It was only for the **biduality theorem** in staggered cohomology that my demonstration made use of the assumptions mentioned by Deligne. The latter theorem, which was of a type unknown in cohomology (of "spaces" of all kinds) before I discovered it, only played in the SGA 5 seminar only an episodic role, for the demonstration of the  $\square$  formula of Lefschetz-Verdier<sup>483</sup> (\*), p. 874 which itself played a purely heuristic role<sup>484</sup> (\*\*). In Deligne's apocryphal text, the role of the aforementioned biduality theorem is **nil** (apart from being demonstrated under helpful hypotheses, and - under Illusie's obsequious pen and with the encouragement of his friend - becoming "Deligne's theorem"). . ).

This is not to minimize the interest of Deligne's finiteness results, which do indeed fill a gap (among many others) in SGA 5, as is the nature of things. No living mathematical theory is complete! But it has to be said that Deligne has exploited this contribution, as useful as it is modest (he's done deeper and more difficult things, and with no trouble yet... . ), by **inflating** it excessively, turning it into the "Trojan horse" of a monumental swindle: the "Stale Cohomology" operation.

This same "Trojan horse" reappears, moreover, in the aforementioned "review" of the volume called "APG 4<sup>1</sup> ", presented by Deligne for the Zentralblatt (see b. de p.(\*\*) page 851.). In the last paragraph of the latter, I read:

"We prove that for schemes of finite type on a regular scheme  $S$  of dimension one, **the usual cohomological operations** [not to say the "six operations", which must definitely not be named!] transform any constructible bundle into a constructible bundle." (Emphasis added.)

$\square$  This is phrased in such a way as to suggest that, prior to the brilliant volume presented by the author, there was no p. 875

a finiteness theorem for any of the famous "usual operations" in scalar cohomology<sup>485</sup> (\*). However, I had the pleasure of proving the first such finiteness theorem, and the most crucial of all, for the functor

<sup>482</sup>(\*) Thus, the "six functors" and the essential formulas concerning them, the most crucial of which is the "duality formula" for a separate morphism of finite type (which can be considered the most general version imaginable to date, of Poincaré's classic duality theorem), were established by me, without at any time having to impose finiteness hypotheses on the coefficients. As a matter of fact, Deligne knows this better than anyone, since it was **none other than he** who made a detailed redaction (based on my 1963 notes) of the SGA 4 lecture in which this duality formalism is developed (centered around the duality formula in question)!

<sup>483</sup>(\*) (March 17) Nevertheless, in the second paragraph quoted, Deligne goes on to point out that this formula "was established only conjecturally", and that "moreover, the local terms were not calculated" ("affirmation" which makes no mathematical sense, but which helps to create the impression of a "gangue of nonsense" about SGA 5, destined to be charitably forgotten. . . ).

I confess that when I first read these passages, a year ago, I was dumbfounded - the meaning of these strangely "off-the-wall" comments, concerning a text that was otherwise recommended to be forgotten, completely escaped me. With hindsight, and the benefit of a careful "work on documents", an **intention of appropriation** finally appears, served by a meticulous and perfectly perfected **method** of concealment ("à l'embrouille"), behind what at first sight had given me the impression of a simple epidermal malice, expressed with the good fortune of a complacent pen. For

For a more detailed examination of the method, see the sub-notes "La Formule" (nos 169<sub>5</sub> - 169<sub>9</sub>) to the note "Les manoeuvres".

<sup>484</sup>(\*\*) As I point out below (in the sub-note "Real maths. . . ." (no 169<sub>5</sub>), this formula has been psychologically important, providing **motivation** for the development of "explicit" fixed point formulae.

<sup>485</sup>(\*) This is very much in line with the "confused state of SGA 5" which (as stated earlier in the same review) the present volume was intended to "remedy".

$Rf_!$  (cohomology with proper support), and this in fact in the very days (if I remember correctly) that followed my discovery of the **definition** of such a functor in stable cohomology (coinciding! with the "banal"  $Rf^*$  when  $f$  is assumed proper). This was in February 1963, before I'd had the honor of meeting my future student, and at a time when nobody except me (and Artin, at a pinch) was yet too sure whether étale cohomology really "existed". It really began to **exist** in those days.

There remained the analogous question for  $Rf_*$ , which proved to be more resistant, and has still not been resolved with all the generality it (doubtless) deserves. As early as that same year (if not the very same month), I had already carried out the necessary "unscrewings" (which today's first-timers can do in a jiffy. . . ) showing that, starting from the finiteness for  $Rf_*$ , we could prove that of  $Lf^!$  and  $RHom(., .)RHom(., .)$ <sup>486</sup> (\*\*). Admittedly, this has since become "basic folklore" in staggered cohomology, and is surely part of the "technical digressions" that my brilliant precursor "SGA 4<sup>1</sup>" is intended to "make people forget". . .

## b<sub>9</sub> . "The conjecture

**Note** 169<sub>4</sub> (March 12)<sup>487</sup> (\*\*\*) More than once since the publication of Deligne's article "La conjecture de Weil I" (in which he establishes the "last part" of the conjectures, which I had left in abeyance), I had noticed as a strange thing, but without dwelling on it until these very last days, that Deligne speaks of Weil's conjecture, where the custom until then had been to say Weil's conjectures. It is indeed in this form, of a series of

p. 876

The conjectures in question in Weil's article (Number of solutions of equations in finite fields, Bull. Amer. Math. Soc. 55 (1949), p. 497-).

508), which is also how I learned them from Serre in the mid-fifties. It's true that in this set of conjectures, heterogeneous at first glance, there is an obvious **unity of** inspiration, stemming in the first place from intuitions linked to cohomological formalism (via Lefschetz's formula), and also (I presume at least) from Hodge's theory.

By creating and developing such a **cohomological tool** for varieties over any basic body, I was able to demonstrate many of these conjectures. I did so, assisted by Artin, Verdier and others, devoting three well-packed years of my life to meticulous piecework, materializing in two thousand "unreadable" pages of "gavage of nonsense" and "technical digressions", which allowed a Deligne to "slash" the last step in twenty tight pages... . Moreover, inspired by a remarkable "kahle-nothing analogue" to Weil's conjectures, discovered by Serre, I was able to derive (along with what I called the "**standard conjectures**" on algebraic cycles) the principle of at least one **transposition of Hodge's theory** onto an arbitrary basic body (or more precisely, a transposition of what, in Hodge theory, is really relevant, from an "algebraic" point of view, to the theory of algebraic cycles on complex algebraic varieties). Even if I were to slightly (and obviously) reformulate these conjectures in their initial (perhaps over-optimistic) form, they are valid at least in characteristic zero, and are "surely true" also in characteristic  $p > 0$  (as long as Weil's conjectures are. . . ).

It's surely no coincidence that the same Deligne who insists on "singling out" Weil's conjectures has also endeavoured to conceal the role played in their demonstration by the man who was his teacher, and that it is he too who has endeavoured (successfully, given the general apathy) to discredit the "conjectures".

<sup>486</sup>(\*\*) As for the remaining two of the six operations, namely  $Lf^*$  and  $\otimes^L$  it is trivial that they transform constructible in constructible coefficients.

<sup>487</sup>(\*\*\*) This sub-note is taken from a b. de p. note to the note "Les manoeuvres" (n° 169); see b. de p. note (\*\*\*) on page 857.

standard" as a dead end, out of reach what's more, and as an **obstacle**, to say the least, now overcome, thanks to God (and his modest self), on the way to proving Weil's conjecture<sup>488</sup> (\*).

## . The Formula

### (a) Real math. ...

**Note 169** □(March 17) The famous "Weil conjectures", for an algebraic variety  $X$  defined over <sup>877</sup> finite field  $k$ , concern the "*L-function*" (known as the "Artin-Weil function") associated with  $X$ . This is defined as a certain formal series with rational coefficients, the knowledge of which is equivalent to that of the number of points of  $X$  rational on the  $k$ -field and all its finite extensions. The first assertion among these conjectures is that this formal series (with constant term 1) is the serial development of a rational **function** on  $\mathbb{Q}$ . All the other assertions concern the particular form and properties of this rational function, in the special case where  $X$  is projective connected and non-singular. At the heart of these conjectures is a certain formula, presumed to be canonical, presenting this rational function in the form

$$L(t) = \frac{P_0(t) P_2(t) \dots P_{2n}(t)}{P_1(t) \dots P_{2n-1}(t)}$$

where the  $P_i$  ( $0 \leq i \leq 2n$ , with  $n = \dim X$ ) are polynomials with integer coefficients and constant term 1. The degree  $b_i$ , of  $P_i$  is supposed to play the role of an "i.th Betti number" for  $X$  (or more precisely, for the variety

corresponding  $\bar{X}$  on the algebraic closure  $\bar{k}$  of the field  $k$ ). Thus, when  $X$  comes from a non-singular projective variety  $X_K$  defined on a body  $K$  of zero characteristic, by "reduction to car.  $p > 0$ ", then  $b_i$  must be equal to the i.th Betti number (defined by transcendental means) of the **complex** algebraic variety, obtained by

from  $X_K$  by any folding of  $K$  into  $\mathbb{C}$ <sup>489</sup> (\*). The rational function must satisfy a **functional equation**, which is equivalent to saying that the roots  $\xi_\alpha$  are exactly  $q^n$ , where  $q = p^f$  is of  $P$

the cardinal of the base field  $k$ , and where  $\xi_\alpha$  traverses the roots of  $P_i$ . (Morally, this had to "come from" the existence of a "Poincaré duality" for the unnamed and undefined "cohomology" of the variety  $\bar{X}$ .) I believe Weil was also to conjecture that for  $i \leq n$ , the zeros of  $P_{2n-i}$  were exactly the  $q^{n-i} \xi_\alpha$ , where  $\xi_\alpha$  still traverses the zeros of  $P_i$  (or, which amounts to the same thing in view of the duality condition, that the zeros of  $P_i$  are grouped in pairs, each with a product equal to  $q^i$ ). The heuristic "reason" here is a another important property of the cohomology of complex □ non-singular projective varieties, expressed p. 878 this time by the "Lefschetz theorem" (the so-called "cow" version). Finally, the last of Weil's conjectures, a "geometric" analogue of Riemann's conjecture, is that the absolute values of the inverses of the zeros of  $P_i$  are all equal to  $q^{-i/2}$  (an assertion that leads to highly accurate estimates on numbers of

<sup>488</sup>(\*) (March 16) For details of this double escamotage-débinage, see the Funeral Eulogy (notes n° s 104,105), and the few words on this Eulogy at the beginning of note n° 171 (x). For a more detailed examination of the art of escamotage, see the set of sub-notes "La Formule" (n° s 169<sub>5</sub> - 169 ).<sub>9</sub>

(x) (May 11) This beginning of the former note "L'apothéose" has been separated from it, to become a separate note "Les joyaux" (n° 170(iii)).

<sup>489</sup>(\*) At the time of Weil's conjecture, it was not even known that the  $b_i$  defined in this way were independent of the plunge of  $K$  in  $\mathbb{C}$ . A few years later, this would result in Serre's theory of the cohomology of coherent bundles, which gave a "purely algebraic" meaning to the more finis  $h^{i,j}$  invariants of Hodge theory.

points of  $X^{490} (*)$ ).

The rationality of the function  $L$  of a general variety  $X$  had been established by Dwork in 1950, using non-cohomological " $p$ -adic" methods. The disadvantage of this method was that it did not provide a cohomological interpretation of the function  $L$ , and consequently does not lend itself to an approach to the other conjectures, for nonsingular projective  $X$ . In the latter case, the existence of a cohomology formalism (on a "coefficient field"  $R$  of zero characteristic), including Poincaré duality for non-singular projective varieties, and a formalism of cohomology classes associated with cycles (transforming intersections into cup-products), makes it possible in an essentially "formal" way to transcribe the classic "Lefschetz fixed-point formula". By applying this formula to the Frobenius endomorphism of  $X$  and its iterates, we would obtain an expression (1) as required by Weil, where the  $P_i$  are polynomials with coefficients in  $R$ . This must have been clear to Weil from the moment he set out these conjectures (1949), and it was certainly clear to Serre and me in the 1950s - hence the initial motivation to develop such a formalism. This was done as early as March 1963, with  $R = \mathbb{Q}_l$ ,  $l \neq p$ . There was just two grains of salt:

a) It was not clear a priori (although we were convinced that it must be true) that the polynomials  $P_i(t)$ , which a priori had coefficients in the ring  $\mathbb{Z}_l$  of  $l$ -adic integers, were in fact **ordinary integers**, and moreover, independent of the considered prime number  $l$  ( $l \neq p = \text{car. } k$ ).

b) From the rationality of the function  $L$  for a non-singular projective  $X$ , we could only deduce that for a general  $X$ , if we had the resolution of singularities.

The problems raised by a) played a crucial role, of course, in the emergence and development of the yoga of **patterns**, and in the subsequent formulation of **standard conjectures**, closely related to this yoga.

p. 879 They have also stimulated thinking to also find a  $\square$   **$p$ -adic cohomological** theory (realized by the The "**crystalline**" theory then offers a possible approach to proving the completeness of the coefficients of  $P_i$ , once we know (e.g. via an affirmative solution to the standard conjectures) that they are rational and independent of  $l$  (**including** for  $l = p$ )).

In any case, by 1963 we already had the expression ( $L$ ) of the function  $L$  (but which a priori depended on the choice of  $l$ ), the functional equation, and the correct behavior of Betti numbers by specialization. All that remained was to solve question a), prove the assertion for the absolute values of the roots of  $P_i$ , and finally (for good measure) the "Lefschetz-like" relation on the zeros of  $P_i$ . This was done ten years later in Deligne's article "La conjecture de Weil I", Pub. Math, de l' IHES n° 43 (1973) p. 273-308.

As ingredients of this Deligne demonstration, there was therefore no need for a fixed-point formula more sophisticated than the "ordinary" one, which was available (without anything "conjectural") as early as 1963. The only other cohomological ingredient in Deligne's article, if I'm not mistaken, is the cohomological theory of Lefschetz brushes (étale version) that I had developed around 1967 or 68, supplemented by the Picard-Lefschetz formula (proved in the étale framework by Deligne), both of which were set out in the aforementioned APG 7 II volume (from which my name, as luck would have it, has all but disappeared. . . ).

On the other hand, the "more sophisticated" fixed-point formula, known as the "**Leschetz-Verdier**" **formula**, played an important **psychological role** in encouraging me to develop the cohomological interpretation ( $L$ ) of  $L$ -functions, valid for any variety  $X$  (not necessarily non-singular projective). Verdier's formula reminded me that there must be fixed-point formulas without non-singularity conditions on  $X$  (as was already well known in the case of the ordinary Lefschetz formula), but above all, it drew my attention to the following

<sup>490</sup>(\*) From this last of Weil's conjectures, it follows at the same time that the writing ( $L$ ) of the function  $L$  is **unique**.

on the fact that there are fixed-point formulas concerning cohomology with **coefficients in a bundle** ("constructible"), interpreting an alternating sum of traces (in spaces of cohomology with coefficients in such a bundle) as a sum of "local terms" corresponding to the fixed points of an endomorphism  $f : X \rightarrow X$  (when these are isolated). In this heuristic motivation, the fact that this Lefschetz-Verdier formula "remained conjectural" in that  $p > 0$  (in the absence of the resolution of singularities,  $\square$  and hence the "biduality theorem"), **was entirely irrelevant**<sup>491</sup> (\*).

p. 880

As so often, the essential step here was to find **"the" right formulation** (in this case for a "cohomological formula of *L-functions*"). Verdier's formula suggested using an arbitrary (constructible) **l-adic bundle**, instead of the usual bundle of coefficients (which until then had remained implicit), namely the constant bundle  $Q_l$ . By copying Weil's definition of the "ordinary" *L-function*, we had to define one "with coefficients in  $F$ ". Once you've thought about it, the definition is self-evident: it's the one given in my Bourbaki lecture of December 1964 (Formule de Lefschetz et rationalité des fonctions  $L$ , Sémin. Bourbaki 279), which need not be repeated here. In addition, the plausible "local terms" of the Lefschetz-Verdier formula (in terms of the given bundle of coefficients, and the Frobenius correspondence) were also essential. Finally (you either have the nerve or you don't!), why not write the formula, here, abandoning even the cleanliness assumption of the "orthodox" Lefschetz-Verdier formula, but working with **clean-support** cohomology?!

So, once again, the essential step was to find the "right statement" (in this case, **the** "right formula"), **sufficiently general** and, by the same token, sufficiently **flexible** to lend itself to a demonstration, "passing" without problems through recurrences and "unscrewings". I wouldn't have known (and no one to this day would) how to **directly** demonstrate "the" formula for "ordinary" *L-functions*, for any  $X$  (or even smooth, but not clean, or vice versa), in terms of l-adic cohomology (with proper supports) with coefficients in the **constant** l-adic bundle  $Q_l$ , without going through the faisceautic generalization. (No more than I would have been able to prove the **ordinary** Riemann-Roch-Hirzebruch formula, in car.  $p > 0$ , if I hadn't first generalized it as a faisceautic formula for a proper **application** of smooth algebraic varieties - and no one, as far as I know, can do this even today. ... )

$\square$  In the Bourbaki paper in question, I confine myself to giving the general statement of the formula for functions  $L$  "with coefficients in an ordinary l-adic bundle, and I show how, by some very simple unscrewings, we're reduced to the case where  $X$  is a smooth projective curve. I knew that once I'd reached this point, **it was a foregone conclusion** - because dimension one is sufficiently "in hand" that proving the formula in question becomes a matter of routine<sup>492</sup> (\*). At this point, I didn't worry about finding a good fixed-point formula in dimension one and proving it. He gave a fixed-point formula, known as "Woodshole's formula", the following year, which was enough to top Frobenius and the application to *L-functions*. I took con birth of his statement, which didn't really satisfy me, as it seemed to me that the conditions he imposed on his cohomological correspondence (for the purposes of a demonstration of which I'm unaware) were a little artificial - I would have liked

p. 881

<sup>491</sup>(\*) (March 20) So much so, in fact, that last year I had long since forgotten this fact entirely, and was stunned to read (in Deligne's column) that the Lefschetz-Verdier formula "was only conjecturally established in the original version of SGA 5". I come back to this point in the reflections of the following day and the day after (March 18 and 19). (In the sub-notes n° 169<sub>6</sub> and 169<sub>7</sub>.)

<sup>492</sup>(\*) When I say "routine work", I don't mean it in a pejorative sense. Nine-tenths, if not many more, of mathematical work is of this type, as much for me as for any other mathematician who happens to have moments that are precisely **something else**, creative moments. After Verdier, I myself spent some time cranking up the delicate, well-oiled techniques available to find and prove a formula for fixed points in dimension one that satisfied me (provisionally at least). This was "routine" work, just as Verdier's had been.

a formula that applies to any endomorphism of an algebraic curve. The SGA 5 seminar was the first good opportunity I had to develop such a formula to my liking (unless I'm mistaken, it's the one that appears in Lecture XII of the Allusie edition, having miraculously survived the vicissitudes that befell that unfortunate seminar). Weil's conjectures had been an initial motivation, and an invaluable thread, to "launch" myself on the development of a complete formalism of étale cohomology (and others). But I sensed that the cohomological theme, which had been at the center of my efforts for eight or nine years and would remain so for the years to come until I left in 1970, had an even wider scope than the Weil conjectures that had led me to it. For me, the Frobenius endomorphism was not an "alpha and omega" for cohomological formalism, but an endomorphism among many others...

p. 882

It seems to me that Deligne's initial motivation for his "SGA 4 operation<sub>2</sub> - SGA 5" was the desire to appropriate the trace formula alone, and thus, as a "corollary", that of  $L$  functions.

In short. I believe that both "pieces" were too big, and that even today and notwithstanding "SGA 4<sup>1</sup>" and Colloque Pervers et tutti quanti, "people" (even those who are not so well informed) "know" that it was not he who created the  $l$ -adic cohomological tool, nor did he single-handedly prove "the" Weil conjecture. Nevertheless, to conclude with the "Cohomologique étale" operation, I'd like to follow the twists and turns of my friend and ex-student Deligne in his presentation of the central theme<sup>493</sup> (\*) of the volume entitled "SGA 4<sup>1</sup>", namely, "la" formule des traces, leading to the cohomological formula of  $L$ -functions. This is the subject of the "Rapport sur la formule des traces" (quoted [rapport] in his book, loc. cit. p. 76-109).

In four places in the volume, Deligne makes comments of a somewhat "historical" nature on the trace formula. Readers of this volume who are not already in the know, and whether or not they read the four passages (which we shall review), will get the impression that a certain Grothendieck (author or director of a rather vague seminar subsequent to the volume "SGA 4<sup>1</sup>", This seminar should not be read) seems to have had some idea about  $L$ -functions, albeit a rather muddled one, before the author of this brilliant volume finally came up with understandable statements and demonstrations. In the whole volume, the only precise reference to this quidam is to a certain Bourbaki exposé (from 1964), in the course of a "Remark 3.7." (loc. cit. p. 88), which comes as the last in a string of three remarks, some more technical than others<sup>494</sup> (\*\*). It reads:

"If we admit the formalism of  $Q_l$ -sheaves. . . **it is easy to reduce the proof** of 3.1, 3.2 to the case where  $X_0$  is a smooth curve and  $F_0$  is smooth. This is clearly explained in [2] §5 (for 3.1; 3.2 is treated similarly)."

p. 883

(emphasis added). In short, this unnamed quidam (except under the flattering sign [2]<sup>495</sup> (\*) has (non

<sup>493</sup>(\*) In fact, nowhere in "SGA 4<sup>1</sup>" is it stated that the "Rapport" forms the "central theme", nor is it stated that the main purpose is to provide the main ingredients of étale cohomology for "the" Weil conjecture. At the time of writing the double introduction to the volume, a purpose of appropriation to the dimensions of all étale and  $l$ -adic cohomology must already have been present.

<sup>494</sup>(\*\*) As I was writing these lines, I was struck by the striking sense of **identity** between the style I'm probing here, and the one deployed four years later, for the appropriation "by contempt" of the "theorem of the good God" (aka Mebkhout). I discover the twirls in question in the note "Le prestidigitateur" (it's worth the capitalization. . . ), n° 75". There the "sore point" was hidden in an even messier note 4.1.9 (instead of 3.7). You can't stop Progress...

(March 22) It had escaped me that there is in fact a second reference in "SGA 4<sup>1</sup>" to the same Bourbaki lecture of 1974, a reference served up with consummate art in "Fil d'Ariane", as we'll see in the sub-note "Les double-sens - ou l'art de l'arnaque" (n° 169).

not **done**, of course, but) **explained the trivial job** - so trivial, in fact, that it's hardly worth mentioning in this closing remark, and still having the kindness to suggest that, trivial for trivial's sake, it is at least clearly explained. (We already know from other comments by the brilliant author that clarity isn't exactly the forte of the confused quidam in question... .) To put it another way: this "Report on the Formula of Traces" chapter is about **doing the real work**, leaving the trivialities to those who are there to do it. ...

While I'm on the subject, I might as well say right away that on this same page is one of the four passages I was alluding to, containing historical comments on "the" trace formula. It's section 3.8 (following, appropriately enough, on from the previous comment 3.7). It explains that there are "two methods" for proving 3.2 (i.e., the trace formula in the only explicit case where it is mentioned in this volume, namely the special case of the Frobenius correspondence). Needless to say, the quidam's name does not appear in either of them. A distinction is made between the "Lefschetz-Verdier" A-method, and the "Nielsen-Wecken" B-method (that name sounds familiar too...). Let's see what he has to say:

B.Nielsen-Wecken. A method inspired by Nielsen-Wecken's work can be used to bring 3-2 [the for-trace mule for Frobenius] to a particular case proved by Weil; this will be explained in the following paragraphs."

□

In fact, par. 5 (pp. 100-106) is appropriately entitled "**The Nielsen-Wecken method**". We have p. 884 said earlier that the method was **inspired** by the work of Nielsen-Wecken - so it's surely out of sheer modesty that the author of the volume calls the method "de Nielsen-Wecken". It's all the more clear that these aren't the guys from now. If the reader happens to look at the bibliography at a certain XII lecture to which he's never referred (and in a seminar, incidentally, that he's advised to forget), he'll know that these are guys who published in the early forties. If he even reads their fine work (which I bet the brilliant author has never held in his hands), they'll know that their methods are triangulation techniques. It's apparently not the one in the text. In the absence of any mention to the contrary, it is indeed the modest author of the volume who is also the author of the method. No date is given for this one, no doubt out of modesty again, so as not to say that it was really he who first did the work to demonstrate this famous trace formula.

Let's take a look at the "Lefschetz-Verdier" A-method and see what they have to say about it. It's not exactly encouraging:

"If  $X_0$  is clean. ... the general Lefschetz-Verdier trace formula allows us to express the second member of 3.2 as a sum of local terms, one for each point of  $X^{p^n}$ . In the **original version of SGA 5**, this formula was only proved modulo the resolution of singularities [we knew we'd only encounter glitches!] Readers will find an unconditional proof in the **final version** [still too modest to recall that it was thanks to him that the bet was saved - in any case we'll be careful not to read that damn SGA 5]. In the case of curves, to which we can reduce (3.7), the ingredients [ ? ? ? - we give up. . . ] were moreover all available."

But then, if they were (a more alert reader, if there is one, may well ask), why all this chatter about a Lefschetz-Verdier formula that had only been proven et patati et patata? Hadn't we just said that the **real work** was done in dimension **one**? Answer: it's the method

<sup>495</sup>(\*) Each to his own - in 1970 (at the International Congress in Nice), it was Serre (in Deligne's paper "La théorie de Hodge I") who, instead of being named, was entitled to the acronym [3], in the cryptic line which alludes for the first and last time) to "sources" for the theory presented. ...

the "cuttlefish method": ejecting ink to fish in troubled waters! At this point, the reader is already fully convinced that this is surely not the right method. It's with an extinguished eye that he scans the next paragraph, which will give him the rest:

p. 885 "To deduce 3.2 from the Lefschetz-Verdier formula, you have to be able to compute its local terms [pity, in what a galley.....!]. For a curve and the Frobenius endomorphism [ah! they are deflate!], this had been done by Artin and Verdier [and they went at it with two again!] (see J.L. Verdier'□the Lefschetz fixed point theorem in étale cohomology, Proc. of a conf. on Local Fields, Driebergen, Springer Verlag 1967) and the **definitive version of SGA 5**) [one wonders what the original version might have looked like, poor us!]" (Here and above, my emphasis, purely out of malice!).

Clearly, it's out of charity that the brilliant author refrains from referring to the relevant lecture from the seminar doomed to oblivion, or from even hinting that "the" formula is indeed to be found there! The inquisitive reader, however, would have found an exposé XII with the unusual name "Formules de Lefschetz et de **Nielsen Wecken** en géométrie algébrique, par A. Grothendieck [toujours le même quidam, ma parole!] rédigé par I. Bucur [connais pas]". Surely the quidam and his acolyte will have copied their brilliant predecessor's presentation, overloading it with superfluous details to their heart's content? .

In this famous "report", there's nothing to make the reader suspect that there exists (apart from the Lefschetz-Verdier formula or rather, should we say, the Lefschetz-Verdier-Deligne formula, in any case uninspiring, as is clear from the author's own disillusioned comments) an explicit trace formula and all and all, for anything **other** than the Frobenius endomorphism alone. Both in the passage quoted, referring to Artin-Verdier, and in another (quoted below) referring to SGA 5 (so as not to name the quidam), it is suggested that the work was done **only** in the case of the Frobenius endomorphism. We're buddies with Verdier (and we're proving it to him), but as for the trace formula, it's a foregone conclusion: thumb-reference to Verdier all right (in a breath with Artin<sup>496</sup> (\*), and drowned in the middle of a technical and uninspiring text, as soon forgotten as read) - but it's well understood and there's no mistake: the trace formula, that's **him, Deligne!**

p. 886 It's true that the aforementioned Deligne has more than one string to his bow, and that it's not for nothing that he's scattered these comments with a historical allure (sic) in□four different places, just to make up in one for what one could reproach him for having omitted (or overdone) in the other. In that case, he can fall back on the introduction to the same chapter - everything has been taken care of! It's a seven-line introduction, worth quoting in full<sup>497</sup> (\*).

## (b). . and nonsense. .

**Note** 169<sub>6</sub> (March 18) I had to stop in mid-stream yesterday, as it was getting prohibitively late, and it had become clear that I wouldn't be finishing with "La Formule" overnight! Before I get into some of the twists and turns surrounding the aforementioned formula, I'd like to take the opportunity to first, in this case

<sup>496</sup>(\*) I'd already come across Deligne's tried-and-tested technique of drowning a fish in order to get rid of so-and-so (in this case, Verdier, who is a good friend of mine and who will be given substantial compensation elsewhere), by naming him in a breath with another - so you can't blame him for not being generous! This is the "**dilution** by assimilation" method. The art in this method is to find a gentleman who is a "pair" with the person you're trying to swindle. As far as I'm concerned, my friend uses Serre every time...

<sup>497</sup>(\*) (March 20) I'll come back to this introduction in yesterday's reflection (Cf. "Les double-sens - ou l'art de l'arnaque", sub-note n° 169<sub>7</sub>.)



of the fine "de Lefschetz-Verdier" formula, to put one's foot in it. This formula perfectly illustrates something that seems essential to me, and to which I have returned insistently on more than one occasion in the course of *Récoltes et Semailles* and as early as the Introduction<sup>498</sup> (\*\*), but in terms that remained perhaps a little too "general".

This formula is a striking example of a statement that is **profound**, and whose demonstration is "trivial" (169<sub>6</sub> bis). When Verdier told me that he had derived and proved a Lefschetz formula for "cohomological correspondences" (which hadn't even been defined yet) on any algebraic varieties ("clean" ones, anyway) and for any constructible "coefficients", I was incredulous at first. Perhaps the idea had occurred to me of a Lefschetz formula with more or less general "coefficients" - I must have written one at least a long time ago for "locally constant" coefficients, i.e. in a local system. But **I didn't believe in it** for general coefficients - it sounded too good to be true! It didn't take Verdier long to convince me. It must have taken him a quarter of an hour to write down the formula and demonstrate it to me - and that's because I'm slow, especially when it comes to making sure of something so unexpected! It's what you might call a "**trivial** demonstration", in terms of what's "well known", I mean. And following the wind that's blowing these days (and of which J.H.C Whitehead has already perceived the first whiffs<sup>499</sup> (\*\*\*)), there is only one step (blithely taken by the many) to classify the theorem itself as □ "trivial" - one formula among ten or . 887 hundred, which "fall" all by themselves from the cohomological formalism - in this case, from the **complete** formalism I had just developed in the étale framework the previous year (1963): the six operations, **and** the biduality theorem.

If I say that the theorem discovered by Verdier (following the path traced by Lefschetz) is "profound", it's not for the reason (however pertinent) that the formalism from which its demonstration derives is itself "profound". In fact, the same fashionable wind has long since (and with the unconditional support of Verdier himself, no less!) classified formalism as "Grothendieck's big toast", which is swept aside with one hand, while tacitly using the aforementioned "toast" at every step (without naming it). The question of whether this theorem "remained conjectural" (as Doe points out with an air of commiseration), or was fully established in every characteristic (as it is now, thanks to the "biduality theorem" bearing Doe's name) is for me just as incidental, when I say that it's a profound theorem, and one that substantially enriches our understanding of the "cohomological theme" of all kinds (discrete or continuous coefficients, and "varieties" or "spaces" of all kinds . . . ). The same could be said of the ordinary Lefschetz formula, in the case, say, of a compact differentiable (or other) variety, and of an endomorphism of it with isolated fixed points: the "formal" demonstration, based on a formalism of duality in cohomology, takes up a page, if not a few lines. In both cases, however, there has been a **creation** - something new and substantial, which had eluded everyone until then, which "didn't exist" (yet), has suddenly appeared... .

Where exactly is "creation" in this case? I believe that more than one mathematician, and more than one of my former students, who once knew what a creation is and have long since forgotten, would do well to meditate on this case, or on any similar one closer to it. I am well aware that if I had proposed to myself, or to one of my students or other colleagues among those who were then well "in the loop" of cohomological formalism<sup>500</sup> (\*), to explain a general formula of Lefschetz's, for the purposes of

<sup>498</sup>(\*\*) See Introduction 4, "A journey in pursuit of the obvious".

<sup>499</sup>(\*\*\*) On this subject, see the note "Youth snobbery - or the defenders of purity" (n° 27).

<sup>500</sup>(\*) There weren't many of them around then to "get in on the act" (nor are there now, given the way things have turned out). events... ) - but there must have been three or four of them, apart from Verdier and myself. As for Deligne, he hadn't yet appeared in the area...

any coefficients and any "cohomological correspondences" (it's up to them to define them ad hoc!) on a compact (sorry, clean)  $\square$ quelconque variety, everyone would have arrived there infallibly, by taking a few hours or days, or if need be a few weeks<sup>501</sup> (\*). Once the problem has been posed (albeit in a vague way, while the main terms are still waiting to be defined. . . ) and seen, "solving" it (in this case, finding the right formulation, suggested by the existing cohomological formalism) becomes a simple task. ) and seen, "**solving**" it (in this case, finding **the** right formulation, suggested by the existing cohomological formalism) becomes a matter of "**routine**" (what Weil calls, in the same sense I believe, an "exercise"). This "routine work" requires flair, a modicum of intelligence and imagination, to be sure, but (as I've written on more than one occasion) it's then "the things themselves that dictate" how we should approach them, provided only that we know how to listen to them. (And if we don't know how to listen to mathematical things, we'd be better off choosing another profession. . . ) It's not in this way that we'll be able to find the right formulation. ) It's not in this kind of work that we find the **spark** I'm talking about, which brings out the new<sup>502</sup> (\*\*).

The creative moment, the spark that triggers a process of discovery, was here when the **problem was seen**, and moreover, "**assumed**" - when the intention was born to really **look, to go all the way to the end** to get to the bottom of it, to "see" **what** exactly is **the** "true" domain of validity of Lefschetz's formula, which everyone claimed was "understood". What ignited the spark was not "virtuosity" or "power" (in the usual sense of brain power, to master difficult techniques or memorize interwoven situations... .). It's an **innocence**: everyone thinks they've understood Lefschetz's formula, but I, poor me, don't feel I've understood it yet, and I'd like to know for sure! In a case like this, once you've got going, you've **got it made**: things tell you what to do, and you do it. Going "all the way" can mean, in one case, proving "the" right theorem (in terms, in this case, of an already existing formalism - that In this case, it's irrelevant whether the formalism itself is "established" or "remains conjectural".) In another case, this

can  $\square$  signify: releasing "the" right conjecture<sup>503</sup> (\*); and that this is often itself provisional, that it is also incidental. This conjecture is one of the steps on the road to a deeper collective knowledge of things (in this case, mathematical things), a step that could not be avoided<sup>504</sup> (\*\*).

Profoundness and fruitfulness are closely linked qualities - the former seems to me to be the tangible sign of the latter. The very first sign of the fruitfulness of the formula discovered by Verdier came in the very same year (if not in the days or weeks that followed, I can no longer say): this formula was **the** main motivation, leading me to write a cohomological formula for *L-functions* "**with coefficients**" in any l-adic bundle. The fact that, **technically**, I didn't have to make any use of the Lefschetz-Verdier formula is irrelevant here. What is certain, however, is that without this formula as a thread, or rather: which

<sup>501</sup>(\*) Of course, what I'm assuming here is that the person in question has "latched on" to the problem posed, so that the "feeling" I would have had (otherwise I wouldn't have suggested it!) has "passed", and that the student or colleague is indeed "triggered". It's by no means a given that "it'll pass" - far from it!

<sup>502</sup>(\*\*) And even less does the "spark" come from some supporting work, done perhaps ten years later, which establishes that the hypotheses that make such and such a demonstration "work" are indeed verified where we expected them to be. ...

<sup>503</sup>(\*) The two cases, where the "spark" (followed "to the end") leads us to a theorem, or on the other hand to a conjecture, are not different in nature. "To the end" means: to make a still diffuse intuition fully concrete, by probing it in all its aspects and by all the means at our disposal. A theorem is by nature no more "finished" than a conjecture. There are theorems that are visibly provisional (even limping and awkward), just as there are conjectures (such as Weil's set of conjectures) that give the impression of an entirely completed, perfect whole. This does not prevent Weil's conjectures from being a point of departure for other, broader developments (conjectural at first, like them) that encompass them. In this sense, it can be said that nothing in mathematics, as long as it is alive and well, is "finished" or "definitive".

<sup>504</sup>(\*\*) On the dynamics of discovery, and the crucial role of "error" in it, see (in the first part of R & S) the section "Error and discovery" (n° 2).

was whispering to me that "there must be something", such a thing as an *L-function* "with coefficients" in a bundle - without this insistent voice, I wouldn't even have thought of finding **the** right notion, and the relevant formula that goes with it; where I would have succeeded, no doubt, in the following years, but **first** having to discover by my own means that other formula of more general scope, which was "on the way", that **had to be** discovered.

Psychologically, the two situations are very similar. Just as Verdier first had to clear the notion of "cohomological correspondence", to clarify the "Lefschetz formula problem" (at beyond the "ordinary" formula), so I had to release the notion of an *L* function "with coefficients", to  $\square$ precise the "*L-function* formula problem" (implied: beyond the case of the "ordinary" *L-function*, associated with a smooth, clean  $X$ . ... ). The "creative moment", the one when a spark flew, was when I **saw this problem**: defining such generalized *L-functions* - and I **took it on**, going right to the end of that problem. Once I'd seen the problem for myself, and assuming I'd managed to "pass it on" to any of the people around me who were "in the know", it was clear that they wouldn't have been able to resist solving it, in "**the** only" natural and reasonable way, by putting in a few days no doubt (as must have been the case for me), definitions, statement, demonstration and all<sup>505</sup> (\*).

It's true, of course, that the "unscrewings" that lead back to dimension one are "easy", and even "trivial" if you insist on it. It's not in this kind of unscrewing, which anyone can do as well as me (or won't deign to do), that there is **discovery**. The discovery lies in a **notion** that no one had thought of, even though it's **obvious**: that of an *L* function "with coefficients". In this notion and in the formula that is inseparable from it, there is the possibility (in the context of finite-type schemes on the prime field  $F_p$ , or more generally, on the absolute base ring  $Z$ ) of interpreting the "six operations" in cohomology, starting with the functor  $Rf_!$ , (operations therefore of a "**geometric**" nature) in terms of operations on "fields of *L-functions*", i.e. in "**arithmetical**" terms. This was a further step in the direction inaugurated by Weil's conjectures in 1949, towards a marriage between geometry and arithmetic, through the cohomological theme.

What becomes of these two discoveries, in this text that presents itself as **the** standard reference book for staggered and l-adic cohomology - this text due to the most gifted and prestigious among those who were my students?

The Lefschetz-Verdier formula, which had inspired me without my ever having to "use" it, became the **scarecrow** wielded aptly, to let the reader (who only wants to believe!) know what a tenuous and uninvolved thread (and "conjectural", what's more, not to mention that the local terms "were not calculated") was suspended a certain seminar  $\square$  auquel to which ("in keeping with the spirit of this volume") one abstains

I'd like to remind you that if the aforementioned formula has ceased to be "conjectural", it's thanks to the modest author of this brilliant volume.

As for the notion of an *L-function* with coefficients, which is the central notion of this Report and constitutes the very heart of the book, it appears without fanfare in par. 1.6 of the Report (loc. cit. p. 80), without the slightest comment to indicate a motivation or provenance. A definition is, after all, a definition; you don't have to justify it. Readers who wonder about the origin of this notion, which is admittedly a bit abracadabra (especially when it's thrown at you like that on an empty stomach. . . ), have a choice between Artin-Weil (but there were no l-adic bundles in their time, obviously introduced

<sup>505</sup>(\*) I'm leaving out the last step of the demonstration, which I had left in abeyance (as not posing any real problem), and which was likely to take longer.

by the author in this same volume. ... ), and (more likely) this same brilliant author, who is leading him swiftly towards a certain formula known as "traces".

This is introduced in par. 3 (loc. cit., p. 86), which begins as follows:

"Grothendieck's **cohomological interpretation** of *L-functions* is the following theorem:..."  
(follows the formula in question 3.1 - NB my emphasis).

Apart from the introduction to the chapter (to which we shall return), this is the only occasion in the entire chapter when a certain name is pronounced<sup>506</sup> (\*). So it's this same quidam again, referred to two pages later by the acronym [2] (as one who knew how to "clearly explain" some "easy reductions") who also gave this abracadabra "interpretation" 3.1, thrown in there without warning. It had no merit whatsoever, as the reader will immediately (and unsurprisingly) realize, for the demonstration takes up barely half a page (on the same page 86) and was, moreover, "classic": it's a simple corollary of the famous "**trace formula**" which gives the Report its name, and which is the subject of what is obviously the "true theorem."

(3.2). No name is put forward to indicate the paternity of the latter - i.e. of "**the**" Formula - still this □

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modesty, precisely among the most brilliant people! Two pages later (as we saw yesterday) the names of Lefschetz, Verdier, Artin, Nielsen and Wecken are mentioned, a veritable debauchery of modesty for the occasion - all to avoid saying that it's him!

The point I'd like to emphasize here, and which seems to me to go far beyond the case in point and these hints of fraud, is this. Whether it's the so-called (and rightly so) "Lefschetz-Verdier formula", or the "cohomological interpretation" of *L-functions* ("with coefficients"), it's precisely **this** that makes their de-coverage **acts of creation**, which is also, these days, the object of general disdain (if not casual derision), commonly expressed by epithets with pejorative connotations such as: "**trivial**", "**childish**", "**obvious**", "**easy**", "**conjectural**", when it's not "soft math", "dream", "nonsense" and other niceties, left to the improvisation gifts of each individual. This is the part of the work, on the other hand, that I've always known (and above all, it seems to me, never **forgotten**) comes "on top of" and by force of circumstance, like the "housekeeping" that's sure to follow (provided only that you stick with it), the **technical** part therefore, the one that's often reputed to be "**difficult**", I've also referred to it as "routine work" (without attaching any pejorative meaning to it) - it's this part of the job that is valued by today's consensus, and singled out to the exclusion of all others.

For me, the notion of "difficulty" is relative: something seems "difficult" as long as I don't understand it. My job then is not to "overcome" the difficulty by force of will, but to enter into my incomprehension sufficiently to come to understand something, and make it "easy".

what had seemed "difficult"<sup>507</sup> (\*). For example, the unscrewing I did for the "function formula

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*L*" as in other circumstances, unscrewings which today pass for "trivial", □ have not been more. For me, it was easier to deal with the "easy" cases than with the so-called "intractable", supposedly "difficult" ones. They were different stages of the work, that's all<sup>508</sup> (\*). It's not because one stage comes after another, or because it happens to be

<sup>506</sup>(\*) (April 9) There is one exception (which had initially escaped my attention), with a thumbnail reference (on p. 90) to "one of Grothendieck's essential uses of the theory of derived categories" (to define traces in "unorthodox" cases).

<sup>507</sup>(\*) Readers will note that this is a description of the "yin", "feminine" approach to a difficulty - that of the "rising sea". I don't mean to imply that this is the only possible creative approach - there's also the "hammer and chisel" approach, the "manly" approach - the only one in vogue (not to say, today, the only one tolerated. . . ). See about these two possible approaches the note "The rising sea. . ." (n° 122), and on common attitudes to either approach, the notes "The muscle and the gut (yang buries yin (1))" and "The providential circumstance - or apotheosis" (n° s 106, 151), as well as "The disavowal (1) - or reminder" (n° 152) which follows the latter.

<sup>508</sup>(\*) The cases I'm thinking of, where I've "unscrewed" to bring myself back to situations of dimension (or relative dimension) **one**, apart from the general formula for *L-functions* with coefficients, are above all the two change theorems

be longer, that it's more "difficult". In both cases, we needed **an idea**: in one case, the idea of "unscrewing" (something we'd never thought of doing in this kind of situation, and with good reason when it comes to fixed-point formulas for any correspondence other than Frobenius!); in the other case, an idea that was no doubt trickier to formulate, inspired by a fixed-point formula (due to Nielsen-Wecken<sup>509</sup> (\*\*)).

more sophisticated than Lefschetz's original formula, and implemented by introducing a careful splitting of the bundle of coefficients, expressed in terms of suitable derived categories<sup>510</sup> (\*\*\*). □Lap

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The second stage took longer, as it turned out: when it came to working it out with all its generality<sup>511</sup> (\*) (given that there are other endomorphisms of a curve than the Frobenius one), there was a whole "carpet" of non-commutative traces "à la Stallings" that finally stuck after it, and which I had to develop carefully. It was long and it was "easy" - and it was also something that **had to** be done, that much was clear. But even coming up with the kind of ideas that make a job "easy" (or simply, possible. . . ), is for me part of "routine work". It contributes to the charm of the job, which makes it something more than a simple crank turn.

The **creative** part of the work, on the other hand, is the **child's** idea: the one that everyone should have seen years ago, if not centuries or millennia ago - and yet no one saw it, even though it was staring us in the face the whole time and we had to make a big detour around it, every time, to avoid bumping into it!

When you come across an idea like this, whether you've "stumbled across" it on your own, or someone else has explained it to you (as Verdier once explained to me), you feel like an idiot: it's unbelievable that you hadn't seen it before, when it was the most natural thing in the world!

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in étale cohomology (for a proper morphism, and by a smooth morphism), which constitute the two key statements that make said cohomology "livable" (as Deligne writes), and the "comparison theorem" for  $Rf_!$ , between étale cohomology and transcendental cohomology (for finite type schemes over the field of complexes). (There's also Lefschetz's (so-called "weak") theorem for affine morphisms.) Psychologically speaking, it was once I'd managed to reduce myself to such "irreducible" situations that I had the impression that it was (more or less) "won", that the expected theorem would indeed "come out", and experience has confirmed on each of these occasions that this feeling hadn't fooled me. Technically speaking, however, it's the unscrewing that represents the "easy" stage. It so happens that, by a kind of "providence" which struck me at the time, the ingredients needed to deal with the two "irreducible" cases, in one and the other, were available.

the other base change theorem, had been developed by me (without suspecting anything), in SGA 1 for the first, in SGA 2 for the second, three and two years before... .

<sup>509</sup>(\*\*) (April 10) It was from me, along with the other SGA 5 listeners, that Deligne learned this "Nielsen-Wecken" formula and its transposition into étale cohomology, which dispensed him from ever having to look at the three fine articles (in German) by these authors (published between 1941 and 1943), and served him in the rather peculiar way that we know

(see sub-note "Real maths..." , n° 169 ).<sup>5</sup>

<sup>510</sup>(\*\*\*) The language of derived categories is indispensable in this demonstration. After my departure, and until about the year with the publication of the volume entitled SGA 4<sup>1</sup> , my cohomological students instituted a tacit and effective boycott against derived categories, which had been the key conceptual tool for developing the duality formalism ("six operations" and biduality), in the context of "coherent" and then "discrete" coefficients. Despite its crucial role in proving the Lefschetz-Verdier formula, as well as the "classical" duality formulas in the étale context, this formalism itself, as a mathematical structure and coherent conceptual whole, was the object of the same boycott, which continues to this day (starting with the very **name** "six operations", which is still anathema).

It is possible that it was the need to demonstrate the trace formula that prompted Deligne, in 1977, to take the first step towards lifting the boycott on derived categories, by exhuming in the pirate-volume a skeletal "Etat zéro" of Verdier's "thesis" (a text in which my name is not mentioned). On this subject, see the note "Le partage" (n° 170) devoted to "Operation III", and for more details on the funny "thesis" affair, the notes "Le compère" and "Thèse à crédit and comprehensive insurance" (n° 63", 81).

<sup>511</sup>(\*) (April 23) A generality rightly described as "superfluous" by Illusie in his Introduction to the SGA murder-edition. 5 (second paragraph), obsequiously echoing his prestigious friend Deligne, who refers (without further clarification) to the "useless details" he would have "pruned". At the same time, this debunking absolves him once and for all from letting the reader suspect that there exists in dimension one an explicit trace formula more general than the one he sets out for Frobenius, where

he repeats step by step the steps of my demonstration, while giving the impression that it is his own. See the following sub-note "Les double-sens - ou l'art de l'arnaque", n° 169<sub>7</sub> .

all of them, the most obvious, the most "stupid", to put it mildly... . We **should have** stumbled upon it long ago, of course, but we didn't. ...

p. 895 It would seem that these days, and increasingly so, in such a situation (and when you're in a position of strength, especially . . . ) you compensate in flexibility'□when it's someone else (an illustrious stranger perhaps, or such a "deceased" long since buried... ) who has the misfortune to stoop (or to have once stooped. . . ) to an idea like that. But my poor fellow, what you're telling me is **trivial**! And to prove to the poor guy just how trivial it is (and to put him in his place . . . ) we're going to spit it back at him in no time - you'll see what it's like to do math! We've got something else up our sleeves than these first-timers (or this left-behind... )! Just pull on it a little, blow, pull again and abracadabra hopplà! And **here's** a statement I'm pulling out of the hat that's got some guts, and here's a whole theory even, and it's no joke, it's hard work, yes! Young man, get dressed, you'll be back when you know how to do the same!

Without even thinking about it, I made a shortcut to the misadventure of my "posthumous pupil" Zoghman Meb- khout, a modest assistant in Lille or God knows where, at the hands of my "occult pupil" Pierre Deligne, the jewel among all of a selective institution (and so on...); a misadventure that occurred in the year of our Lord 1981, and which continues to this very day.... This is "Operation IV", known as the "Service Unknown" (or "Perverse Colloquium", to put it mildly) - the most incredible of the four operations. (See note on "Apotheosis", n° 171.)

But at the same time, as I was writing the previous paragraph, it felt like I was more or less rewriting something I'd already written on another occasion. ...

It didn't take me long to remember - it was in the first part of Récoltes et Semailles, written a year ago now, in the section "**La mathématique sportive**" (the name says it all), n° 40 (p. 105). The difference between the episode I mention and that of the Colloque Pervers is that this time the role of "token stranger" is played by "that young white boy who was stepping on my toes", and that the haughty, "sporty" big boss wasn't a naughty ex-student of mine, but none other than myself. It's true that I don't think I've gone so far as to appropriate (symbolically, in this case) someone else's idea. But I can't swear to it in good faith, and I'd have to ask the person concerned (twenty years later, but better late than never) to let me know how **he** remembers the episode, which is a bit hazy in my memory. He had the misfortune of redoing things I'd known about for ages (among other things, building the Picard diagram of an unreduced diagram by "unscrewing" from the reduced case... . ), and it didn't go down well

p. 896 - that's what en est□resté me; but I wouldn't swear that his approach (in a less general framework than the mine, of course) was really entirely covered by mine<sup>512</sup> (\*).

The fact remains that I must once again point out a **kinship** between an attitude that was mine at certain times at least, in the sixties, and that which I encounter in some of my former students. They reflect back to me an undoubtedly disfigured image of the man I was - an image that

<sup>512</sup>(\*) The opportunity never arose for me to net and publish Picard's "relative" construction in question by "unscrewing" on nilideals, a construction planned for a later chapter of EGA (which never saw the light of day).

In any case, when I speak of the "appropriation" of someone else's idea (big or small), I'm not necessarily talking about plagiarism in the usual sense, when you present this idea (even in a modified and perfected form) without indicating its origin. - which seems to me to be becoming more and more common. But appropriation can be that of casual disdain, whose breath fades the joy of a discovery, as if for the sole pleasure of frustrating it, to the tune of a disillusioned "oh! it's only that ... . "disillusioned. This air implies, without our having to say it, that what we've just been told we've known for as long as we can remember, and if perhaps we hadn't bothered to spell it out again, then it really wasn't worth the trouble... . For these tunes There (or for its ancestor), see (in the first part of R et S,) the section "Le pouvoir de décourager" (n° 31) (repeated in the already quoted note "La mathématique sportive", n° 40); and (in the harsher atmosphere of the 70s and 80s) Burial I, "Appropriation et mépris" (note n° 59).

for years I wanted to deny. But if Harvest and Sowing, which was above all a reflection on my past as a mathematician, had **a meaning**, it was to make me understand, among other things, that even though some of my students have disowned me, it's not up to me to disown any of them. What comes back to me through them is part of the harvest of what I helped sow, just as they themselves contributed. And this observation, which I have been making with an uncompromising pen for nearly three weeks now, is not an indictment of anyone, but an **observation** that involves me as much as any of them.

### (c) Heritage - or trickery and creation

**Note 169<sub>6</sub> bis** (April 10)<sup>513</sup> (\*\*) As everyone knows, the meaning of the word "trivial" in mathematics is highly relative. Here, by "trivial" I mean: in terms of what was supposed to be "known", i.e. (in this case): the formalism of the six operations, and the biduality theorem (the latter remaining conjectural because  $p > 0$  in the discrete context, before Deligne found a demonstration... .). In terms of this formalism, the principle of demonstration is explained ☐ completely convincingly in a few minutes (at the same time time than the statement). It's true that this doesn't dispense with the need for a formal demonstration, which meant checking a few tedious compatibilities.

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It was customary in such cases for the author of a theorem (especially an important one) to take the trouble to write a proof. In Verdier's case, there's no doubt in my mind that this is the most profound and far-reaching result of all those whose names he has the honor (and rightly so) to bear (in Weil's words). However, he did the same for this theorem as he did for the theory of derived categories: as long as he had the credit for it anyway, he didn't think it worth doing the work, and making it available to everyone with a complete demonstration.

This is an eloquent sign of a certain state of mind, which I've had occasion to mention here and there, most recently at the end (dated February 28) of the note "Les manoeuvres" (n° 169). I've been able to see that it has set a trend. While the "Lefschetz-Verdier" formula (with the above reservation) was indeed an act of creation by Verdier, at a time when he was still working with me and was passionate about his work, I see a direct relationship between the fact that he never had the respect to demonstrate "his" theorem, and the fact that **his life as a mathematician never saw another similar act of creation**. Creative moments come to us only when "we are worthy of them", i.e.: in a state to welcome them. ...

This beautiful formula, left behind by a father on the run, has had some strange vicissitudes. First, it was the theme of one of my first lectures (exp. III) at SGA 5, in 1965. Illusie took on the task of writing it, without bothering to do so for twelve years. Then, in perfect connivance with him and Deligne (and I imagine, with the at least tacit agreement of Verdier, to whom Deligne would grant substantial compensation), she became the head of the "Trojan horse" (or "scarecrow", as I write below), deftly maneuvered to lend credibility to the incredible imposture called "SGA 4<sup>1</sup>". This was set up from scratch to bury<sub>2</sub> the master common to all three of us, i.e. also, in short, the "**grandfather**" of the aforementioned formula (which, were it not for my modest self and the six operations buried with me, would probably still not be written for another hundred years... .). For a picture of morals, here's a picture of morals!

If my dear ex-student cohomologists, instead of wasting themselves in such shenanigans playing the dwarf (which they are not) perching on the ☐shoulders of a giant (which I am no more. . . ), had during thesep

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<sup>513</sup>This sub-note is taken from a b. de p. note to the previous sub-note ". . . and 'non-sense'" (n° 169<sub>6</sub>); see reference page 886.

I'm sure that De Rham-Mebkhout's and Hodge-Deligne's theories of crystalline coefficients, with the "mysterious functor" at their heart, have long since reached the "fully mature" stage of the formalism of the six operations. And even (as I've suspected for the last week or two. . . ), the great dream of their teacher, that "**motif**" made to be melody and which (in these same hands) has become a fiefdom, a hoard and a "vague skeleton", has already been embodied in a vast symphony (by no means "conjectural", but "fully mature" too), and is now **everyone's heritage**.

#### (d) Double entendres - or the art of the con

**Note** 169<sub>7</sub> (March 19) But I must return to the "twists and turns" of my friend Pierre Deligne, in his presentation of the famous "Formule des traces". Remarkably, nowhere does he specify that, for the application to Weil's conjectures proper (which were undoubtedly aimed primarily, if not exclusively, from a practical point of view), there is no need for a formula and a sophisticated demonstration - Lefschetz's "ordinary" formula (étale version) suffices<sup>514</sup> (\*). And it's no coincidence, of course, that it's precisely the lecture on the cohomology class associated with a cycle that he chose to "borrow" from SGA 5, and incorporate into his digest without further ado - the very lecture that contains the key ingredient (apart from the "ordinary" Poincaré duality, étale version) for establishing the "ordinary" Lefschetz formula in four spoonfuls. One wonders, then, if he hadn't included this "Report", which establishes a trace formula for the Frobenius endomorphism alone (while stubbornly concealing from the reader that he could find much more general and equally "explicit" ones elsewhere (!)). There are two related reasons why he took the trouble to write this "Report". On the one hand, it was quite clear by the 1960s that Weil's conjectures, suitably reformulated in terms of "weights", still made sense for singular varieties and for non-constant "coefficients".

It is true that we can then formulate them in entirely geometrical terms, without explicit reference to the formalism of the *L-functions*.

p. 899

de Weil II" (in which, of course, there is no allusion to any role I might have played in deriving the main statement he proves). But nevertheless, the arithmetical interpretation (in terms of *L-functions* "with coefficients") of geometrico-cohomological operations was bound to have a role to play, in which the formula of **general *L-functions***, in the form in which I had developed it, would take a crucial place. With a view to the long term, it was therefore necessary to provide a reference in the volume entitled "APG 4<sup>1</sup>". At the same time, while it had become clear that general trace formulas (Lefschetz-Verdier style) form an important ingredient of the cohomological panoply, this contributed to the illusion that this volume (as it advertised) did indeed present an essentially complete cohomological arsenal, for the needs of the "non-expert user" of *l-adic* cohomology.

It remains for me to review the three remaining passages, among the four in "SGA 4<sup>1</sup>", which pretend to give historical details about the trace formula. I'll quote them in the order in which they appear in the volume. The first two are at the very beginning of the volume (page 1 of the Introduction, and page 2 of the "Breadcrumb trail"), and are obviously intended to "announce the color". They're probably the most widely read, too. The third is the short introduction to the "Report on the Trace Formula" chapter. (The fourth

<sup>514</sup>(\*) (April 25) It's possible that I'm making a mistake here, as I haven't really taken note yet of Deligne's demonstration of the last part of Weil's conjectures, concerning the absolute values of Frobenius eigenvalues. It would seem that the use of Lefschetz brushes leads him to introduce more general *L-functions* than the  $\zeta$ -function (i.e. the "ordinary" *L-function*).



passage, mentioned the day before yesterday, is part of the body of this same report, and is surely the least read of all).

In the bibliography after the "Breadcrumb trail for SGA 4, SGA 4<sup>1</sup>, SGA 5", the acronym SGA is explained as "Séminaire de géométrie algébrique du Bois-Marie", with no reference (needless to say) to me personally. However, I am one of the directors of SGA 4 and SGA 5. This function of director must have been quite platonic: reviewing the main presentations of SGA 4 and SGA 5 (and let there be no more talk about it. . . ), there is mention of presentations by Artin, Jouanolou, Houzel, Bucur, but none by me. In the reference to SGA 4 and SGA 5, there's no indication of a date - and I've found no hint in the entire volume that would lead the uninformed reader to doubt that SGA 5 ("to be published in Lecture Notes") is not a publication of my own.

which, as its name suggests, is well **after** the volume known as "SGA 4<sup>1</sup>"<sup>515</sup> (\*). When an allusion is made to a presentation in SGA 5 (generally unspecified), it is clearly specified that it is a "zero state" or the "original version" (implied: thick and unbuildable, one suspects. . . ). These references to SGA 5 (for the uninformed reader, who is advised not to consult SGA 4 nor especially SGA 5) are therefore (in the mind of this same reader) references to a text **subsequent to** the one he is currently reading. I suspect, moreover, that these uninformed readers are by far the vast majority, and (as I've written elsewhere) the others are getting old and are going to die their own deaths... ..

I quote from the first page of the Introduction, paragraph 3:

"The "Report on the Trace Formula" contains a complete demonstration of the trace formula for the Frobenius endomorphism. The demonstration is that given by Grothendieck in SGA 5, pruned of all unnecessary detail. This report should enable the user to forget SGA 5, which can be considered as a series of digressions, some of them very interesting. **Its existence will make it possible to publish SGA 5 as is in the near future.**" (Emphasis added.)

This text has two **opposite** meanings, served up **simultaneously** with consummate artistry. For the informed reader of the history of the formula in question for Frobenius, he may be surprised by the flippancy of the presentation (and all the more so, if he is well informed of the ins and outs of the SGA 5 seminar and the role it played in the formation of the brilliant and flippant author); but he will think that the author at least has indicated the source of his demonstration. As for the uninformed reader, he'll learn that the demonstration in the volume he's holding in his hands is also to be found in a certain later SGA 5 text, a text due to Grothendieck, and cluttered with useless details, which this quidam must have added for fun to the original demonstration. The passage quoted remains vague as to the latter. As we saw the day before yesterday, reading the demonstration itself, in the "Rapport" in question, leaves little doubt that it is indeed the brilliant author of the volume "SGA 4<sup>1</sup>" who is its father. Of course, nowhere does it deign to specify whose idea it was **to write** the trace formula; after all, it costs nothing to write something, as long as you don't bother to demonstrate it! Nor is there any mention of Verdier (who was the first to demonstrate the "crucial case" I'd left hanging). It's no coincidence, surely, that it was at the very moment when he is the question of the trace formula, at the heart of **"the"** Conjecture, that the author uses "kindnesses" to assail like "useless details", "digressions" (very interesting indeed, one is either a good sport or one isn't!) that one recommends forgetting(\*), and finally this discreet yet peremptory reminder that "its existence will enable SGA 5 to be published in the near future as it stands", as if SGA 5 is only "standing" and publishable thanks to the "existence" of the text called "SGA 4<sup>1</sup>" - which surely provided the quidam in question with what he needed.

<sup>515</sup>(\*) Nor the slightest hint that might lead the reader to guess what this unread seminar was about, whose very title ("L-adic cohomology and L functions") remains unknown!

need to present in a complicated way what is simply done in the original text here.

In the Ariadne thread, I've already mentioned (in the sub-note "Le cheval de Troie" (n° 169<sub>3</sub>) to the note "Les manoeuvres") the seventeen lines of the two consecutive paragraphs 2 and 3 on page 2, as "models in the art of 'fishing in troubled waters'". The second concerns the famous trace formula. Both paragraphs deserve to be reproduced here in extenso:

"A duality formalism analogous to that of cohesive duality exists in stellar cohomology. To establish this, Grothendieck used the resolution of singularities and the purity conjecture (for the statement, see [Cycle] 2.1.4), established in a relative framework in SGA 4 XVI, and - modulo the resolution - in equal characteristic in SGA 4 XIX. Key-points are established by another method in [Th. finitude], for finite-type schemes over a regular scheme of dimension 0 or 1. Various developments are given in SGA 5 I. In SGA 5 III, we show how this formalism implies the very general Lefschetz-Verdier formula.

As we can see, in the original version of SGA 5, the Lefschetz-Verdier formula was established only conjecturally. What's more, the local terms were not calculated. For the application to *L-functions*, this seminar contains **another** complete proof, in the particular case of the Frobenius morphism. It appears in [Rapport]. Other references: for the statement and the unscrewing scheme: Grothendieck's Bourbaki exposé [5]; for a brief description of the reduction (due to Grothendieck) of the crucial case to a case already treated by Weil, [2] par. 10; for a *l*-adic treatment of the latter case, [Cycle] par. 3."

I have already commented on the first paragraph in the note quoted (see also the b. de p.(\*\*) note on page 872 to this one, on the unpayable "various developments are given in SGA I"). It remains for me to follow the twists and turns

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p. 902 (or at least some of them - there are too many!) in the second paragraph. The first two sentences, The same is true of the Lefschetz-Verdier formula, as if the whole of SGA 5 (and a certain never-before-clearly-named demonstration of a certain trace formula... ) depended on it for life and death, are clearly part of the "cuttlefish method": confusing what is clear, to fish in troubled waters<sup>517</sup> (\*).

The key double-entendre phrase, however, is the one that immediately follows the drowning of the fish:

". . . this seminar contains **another** demonstration, this one complete, in the particular case of the Frobenius morphism".

The informed but hurried reader (and what reader isn't in a hurry. . . ) is taken aback for a second by the ambiguity of the expression "this seminar" - is it SGA 5, is it "SGA 4<sup>1</sup>"? - and since he knows that SGA 5 contained a complete demonstration, it's awarded once again: the author has indeed referred (somewhat vaguely,

<sup>516</sup>(\*) More precisely, it clearly implies that this 34-page "Report" alone contains (for the better) everything that could be useful in SGA 5 (which, even in the massacre edition, is still almost 500 pages long). That's a lot of "digressions" for nothing!

<sup>517</sup>(\*) It's a misnomer to say that the Lefschetz-Verdier formula was "conjectural" - it was established on the assumption that we have a duality formalism ("six operations" and "biduality theorem"), and it was indeed proved in this form in 1964 by Verdier. This demonstration had of course been given in the oral seminar, and it's complete. It was the validity of the biduality theorem for  $p > 0$  that remained "conjectural", and this is established (as we said) in the "Finitude" chapter of "SGA 4<sup>1</sup>".

As for the local terms in the Lefschetz-Verdier formula, they were "calculated" no more, no less, than in the ordinary Lefschetz formula (with isolated fixed points not necessarily "transverse"), and generalized the classical "intersection multiplicities" that figure in the latter. To say that these terms "were not computed" makes no more or less sense than saying that the dimension of an **unspecified** vector space, or the roots of a polynomial with indeterminate coefficients, are "not computed". To "compute", in these cases as elsewhere, means: to establish in a specified **"case in point"** (e.g., in dimension 1, for the Lefschetz-Verdier formula) an **equality** between two terms, neither of which is any longer "computed" or known true the other (e.g., between the local terms defined by Verdier, and certain local invariants related to the Artin conductor. . . ).

certainly... ) where one would expect it to refer. I came close to doing this on the first reading, in April last year (see the note "La table rase", n° 67), but it didn't fit. I was well aware that my demonstration of an explicit trace formula was by no means restricted to the "special case of the Frobenius morphism". What struck me, moreover, was the fact that someone had just insisted heavily (with "arguments") on the very fact that a certain SGA 5 presentation (in its "original version", my goodness!) was **not** "complete": conjectural here, non-calculated terms there... . With this "it completes" well framed by two commas, this categorical opposition irresistibly suggests to the uninformed reader, without he even has to question, that "this seminar" is obviously  $\square$  the volume "SGA 4<sup>1</sup>"<sub>2</sub> that he holds in hands - and in the next sentence, the reader is immediately told where to find it: "It's the one in [Rapport]". And it's certainly not the reading of the aforementioned demonstration in the chapter quoted, which could afterwards arouse in this same reader the slightest doubt<sup>518</sup> (\*)!

p. 903

The word "**other**" in the crucial sentence is **underlined**, **which is** not at all my friend's style. It's the only word underlined in the two introductory texts, and unless I'm mistaken, the only one in the entire volume (apart from the titles, statements and new terms introduced). If he's so keen to highlight this word, it must be for a good reason. (It's only just caught my attention.) The effect of this "**other**" term, and even more so when it's featured in this way, is to emphasize that there were **two** demonstrations of "the" Formula: one **incomplete**, and we've just said a few words about the less-than-engaging situation, with this "Lefschetz-Verdier" formula that's decidedly not sortable! (And in the more technical text of the famous Report, viewed the day before yesterday, we duly come back to this distressing subject... .). As for guessing whether or not, thanks to the brilliant author's finitude results, this lame method ended up working after all, well, who'll ever know. But after this push-back effect (the same, after all, as the one examined the day before yesterday), the psychological reflex in the docile reader is all the more peremptory: instead of the **incomplete** method of a certain muddy SGA 5 seminar (so incomplete that there's no question of even giving a precise reference to it<sup>519</sup> (\*\*)), a method we'll certainly never have to bother with, we'll be entitled, in this **seminar** of good, solid stuff, to the good, **complete** demonstration, which **is** already holding out its arms to us in the presentation specially designed for this purpose, the "Rapport sur la formule des traces", no mistake we'll have no trouble finding it there. . . .<sup>520</sup> (\*\*\*)).

The "**this seminar**" is simply brilliant - my thumb-friend is incoincible on that term there. Still, both in the paragraph quoted, and  $\square$  in the more technical context of the "Report" extending on the method (doomed to oblivion) known as "Lefschetz-Verdier" (p. 88), he has once again ventured<sup>521</sup> (\*) to say "in plain English" (or at least, in chiaroscuro) **the wrong thing**. In both passages, in fact, he stresses (it's a case of saying it) that there is a method (which we can guess is the one misguidedly followed in SGA 5, God knows in which of his "bushy" exposés . . .) for **demonstrating** the trace formula for Frobenius, which would consist in **using the Lefschetz-Verdier formula**. However, only two demonstrations of the "crucial" case existed (before Alibert's 1982 thesis, giving the calculation of local terms in dimension 1 for any cohomological correspondence with isolated fixed points), Verdier's and mine, neither of which (any more than Alibert's) makes use of the Lefschetz-Verdier formula! It was a

p. 904

<sup>518</sup>(\*) See the sub-note from the day before yesterday "Real maths. . . " (n° 168 ).<sub>5</sub>

<sup>519</sup>(\*\*) Nowhere in the volume entitled "SGA 4<sup>1</sup>" did I find a reference to one of the SGA 5 presentations containing either the demonstration of a fixed point formula, i.e. the famous "cohomological theory of *L-functions*". In fact, it has been clearly stated (see below) that "in keeping with the spirit of this volume, no use will be made of SGA 5 ... . " !

<sup>520</sup>(\*\*\*) The best part is that, in reality, Deligne's demonstration is a faithful reproduction of the one he and his fellow listeners learned at the SGA 5 seminar in 1966.

<sup>521</sup>(\*) "Again", since he had already (even more clearly) advanced to "tell the false" in the previous paragraph, as we saw in the sub-note "The Trojan Horse" (n° 169 ).<sub>3</sub>

a delicate and long-unresolved issue (and one that seemed somewhat incidental), of proving that the local terms in the explicit formula in SGA 5 (for correspondences far more general than Frobenius') are indeed those in the Lefschetz-Verdier formula. Illusie eventually verified this, as he announces in the introduction to the massacre edition of SGA 5 (p. VI), and also in the introduction to his paper III<sub>B</sub> "Calculations of local terms" (p. 139)<sup>522</sup> (\*\*).

If Deligne nevertheless goes to such lengths to create this false impression, it's not without reason. In fact, he creates the impression that SGA 5 (the seminar of "technical digressions" "to which no reference will be made, in the spirit of this volume", intended to make it "forgotten") depended on this "conjectural" formula, moreover unusable as it stands (local terms not calculated sic. . .), which was finally established only thanks to Deligne in the eloquently-named volume "SGA 4<sup>1</sup>" that the reader holds in his hands, and on which (if only because of this fact) the later and "confused" seminar SGA 5 depends. . .

p. 905 As for the last sentence of the passage quoted, beginning with "Other references" (sic), it too is a model of its kind, to avoid saying that the vague quidam Grothendieck had given a complete demonstration eleven years earlier (in the "later" seminar doomed to oblivion. . .), and that this is faithfully reproduced

in "Rapport". The impression that had to be created was that the quidam had made some preliminary reductions, whereas the difficult case is due to Weil, and brilliantly taken up (by a "l-adic treatment") by the author. The reference to a prestigious book by Weil that the reader will have heard of, in addition to an internal reference, really gets the juices flowing - either you're serious and know your classics, or you don't! As luck would have it, there's no indication of date in the reference to Weil's book, nor of chapter or page - it doesn't seem that the brilliant author wants to encourage the reader to look elsewhere than in the brilliant volume itself, where the reference suddenly becomes quite precise (chapter, paragraph).

The famous "result already treated by Weil" is, in fact, nothing other than the **ordinary** Lefschetz formula in the case of an algebraic **curve** (projective smooth connected over a closed algebraic field), which Weil managed to formulate and prove by means of the edge in the 1940s, without yet having the cohomological tool (but using the Jacobian to define the missing  $H^1$  l-adic). It was an important new idea to derive this formula in the case of "abstract" algebraic geometry, and must have set Weil on the path to his famous conjectures. Once we have the cohomological formalism, the Lefschetz formula in question becomes essentially trivial. But if we had said in plain English that the quidam's reduction was a reduction to the ordinary Lefschetz formula (for which we proudly refer, without naming it, to the "Cycle" chapter of the brilliant volume - the chapter pirated from SGA 5 precisely. . .)

- it could have given the impression that the said "reduction" was even a **demonstration of** the sacrosanct Formula. You wouldn't!(\*)

I can't wait to get this over with! There remains this introduction to the chapter "Rapport sur la formule des traces", loc. cit. p. 76, which is as follows (amputated from these last two lines, referring to an expository article by the volume's author):

"In this text, I have tried to set out as directly as possible Grothendieck's cohomological theory of *L-functions*. I follow very closely some of the talks given by Grothendieck at the IHES in the spring of 1966. In the spirit of this volume, I won't be using SGA 5.

- except for two references to passages in Lecture XV, independent of the rest of this seminar."

p. 906 At first glance, it seems as if the author is not being secretive about his sources, referring to a "coho theory". mology of **Grothendieck** of *L* functions", and even adding that he "follows very closely" some of my

<sup>522</sup>(\*\*) For the motivation behind Illusie's sudden efforts, see the sub-note "Les félicitations - ou le nouveau style" (n° 169), especially pages 916-918.

<sup>523</sup>(\*) (May 11) Thus, the whole art-"thumb!" here was to refer in two places far from each other (p. 2 and p. 88) to **two** "re-

exposed. In a **normal** volume, there would be nothing to say. But it's also true that **context** is part of the meaning of any text. The context of the unusual volume entitled "SGA 4<sup>1</sup>" profoundly alters the meaning of this passage, for a naïve reader already warned by what he has read before, and who will be edified a little more, moreover, in the course of reading the "Report" itself. Afterwards, he'll have the impression that it's really a kindness of the generous author towards the confused quidam named Grothendieck, to credit him with a "cohomological theory of *L-functions*", which in the end seems to be reduced to a somewhat abracadabrat, but after all **trivial**, cohomological "interpretation". It is demonstrated in just half a page, as an immediate **corollary** of a "trace formula", which is not pricked by worms, and is of course due to none other than the all-too-modest author of the volume.

It is true that in his "report", the author "closely follows" some of the lectures given by this quidam at the IHES in the spring of 1966. Nothing more is said about these undoubtedly lengthy talks, which must have been lost in the shuffle, except what the author of the volume was willing to retain for his report. Is it sorites about Frobenius (for which we will generously refer to SGA 5 "directed" by the same quidam), or generalities about  $\ell$ -adic bundles, or certain "easy reductions" that will be discussed elsewhere - we're in complete vagueness. In any case, these must have been mostly "useless details", which, thank God, we'll be spared by reading the Report - that's all we ask. So, let's put a veil over the quidam and get down to **business**!

While my friend likes to remain vague when it comes to references to a certain person (when he doesn't pass them over in silence), this time we get the impression that he can't be blamed for not being precise: lectures given at the IHES, spring 1966. If he had been just a hair more precise, he would have added: lectures **at the SGA 5 seminar**.

SGA 5? Isn't this precisely the seminar that appears (**undated**) in the bibliography at "Fil d'Ariane", with the mention "to be published in Lecture Notes"? The seminar which consisted (as we understand it) in adding "digressions" (some of them very interesting, all right) and "useless details".  
to the SGA 4 seminar<sup>1</sup> (a really good one) that preceded it? Don't kid yourself, SGA 5 wasn't in the spring!

1966, you want to laugh! And the best proof is right there in front of you, in black and white in the introduction just quoted in the "Rapport sur la formule des traces" (by Pierre Deligne):

"In the spirit of this volume, SGA 5 **will not be used**".

Then it's clear, isn't it? !

## (e) Les prestidigitateurs - or the soaring formula

**Note** 169<sub>8</sub> (March 20) I'm beginning to feel a little tired, not to say exhausted, by the work I've been doing, for more than three weeks and especially (in detail) over the last few days, to patiently "dismantle", in the "little things" that make **everything**, the brilliant scam set up by my most brilliant pupil, emberlificotating in the public square those who only want to be emberlificotated (and there are legions of them, are there not. . . ). I can't wait to get it over with, yes, and yet I don't regret the time I've spent on it, even though I'm about to turn fifty-seven and there's no shortage of more interesting (or more "enjoyable", at least) things to do. It's a bit like the maths work I called (three days ago) "routine work" - you eat your heart out doing it, you know it's all just routine, and yet you know it's routine too.

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ductions" (!) (easy, it's understood) made by this quidam (named once, and not the second. . . ), without a candid reader ever suspecting that this same quidam had **found** and **proved** a formula for the traces; and that his demonstration (doomed to oblivion) is faithfully reproduced in the brilliant "Rapport". . . .

that it **has to be** done! Not out of some austere "obligation" or self-imposed duty, but because you can't (or at least, I can't) do without it, if I want to establish an intimate contact with the thing being probed, to "penetrate" it. It's through this work, by "rubbing shoulders" with the things we want to know, over a period of days, weeks or even years, that we actually "know" them - and it's from this knowledge alone, the fruit of often arduous and unassuming **work**, that **something else** sometimes springs forth, that "spark" I was talking about the day before yesterday, which suddenly renews our apprehension of things and the very work that leads us into them.

It's through this fatigue (which is not yet weariness), a sign of energy that has been expended, that I can I can also fully appreciate the prodigious energy my friend Pierre must have expended in this delicate staging operation called "SGA 4<sup>1</sup>", or "SGA 4<sup>1</sup> - SGA 5". I can't say to what extent this artist's work, oh so much more subtle than that of a mathematician and involving faculties of an entirely different order, is conscious, or the work of entirely unconscious forces. And that's an incidental point, which concerns him alone. In any case, the diversion of energy, and the intensity of investment in a task at the antipodes of the discovery drive - the task of gravedigger-prestidigitator - must have been outrageous,

p. 908 □ and (there's no doubt in my mind) still is today <sup>524</sup>(\*). Appropriation-escamotage reflexes,  
p. 909 in his relationship to my work at least, and to any other work that openly bears its mark, ended up (in the course of the long "escalation" that was the Burial of the late Master) acquiring such an empire over his being, that they became like a second nature, invading and covering over his original nature, that of the "child" in him, setting out to discover the world. ... More than once, I've been able to see at close quarters, in

<sup>524</sup>(\*) This obsession with appropriation that has focused on "the formula" is truly insane, in simple rational terms. On the one hand, this appropriation, by necessity, must remain to a large extent, if not totally, symbolic: a satisfaction that we grant ourselves, by playing as if we **were** indeed "the father", or as if we **could** indeed make the whole world believe it. The fictive, symbolic character is already apparent, if we recall that Deligne himself, in the article "La Conjecture de Weil I", published four years before the "SGA 4<sup>1</sup> - SGA 5" montage, writes (p. 278) "Grothendieck has demonstrated Lefschetz's formula" (for Frobenius's correspondence). It's true that just a few months later, in

In the February 1974 Bourbaki exposé (no. 446), in which Serre discusses this article by Deligne, the author is astonished (and rightly so) at the absence of any published demonstration of Lefschetz's formula ("we have been waiting since 1966 for the definitive version of SGA 5, which should

be more convincing than existing mimeographed presentations"), and he takes this opportunity to ironize on the 1583 pages of SGA 4 that set out ("with all the necessary details, as well as many others") the formalism of staggered cohomology. Surely Serre had no idea that these sarcasms directed at an absentee would fall on deaf ears. I'm convinced that they must have played their part in germinating the brilliant idea of "making people forget" this "ganguel of nonsense" etc. SGA 4 and SGA 5, as the public voice seemed to be demanding through Serre's own mouth... But apart from even Weil I, in terms of published texts (including the murder-edition of SGA 5, which remains a convincing if mutilated testimony. . . ) the claim of authorship simply doesn't hold water, in terms of the most elementary mathematical common sense.

Added to this, as I've already pointed out, is the fact that perfecting the famous formula is **purely routine** work, once you know what you want to achieve. It took me a few days to work out the essential features - which led me to some precise questions of divisibility linked to the Artin conductor, for which Serre had the answers ready, elegantly expressed in terms of the Serre-Swan modulus. The slightly time-consuming (but also routine) work was the careful fine-tuning of the non-commutative trace formalism inspired by Stallings' work (which, as luck would have it, had just reached me). All this is the sort of thing that someone with the felling of a Deligne (or only the more modest felling that is mine) deals with by the dozen in the course of a single year!

It's true that in Deligne's words, "trace formula" means trace formula **in any dimension** for the **Frobenius** correspondence, a formula that he takes care (in "SGA 4<sup>1</sup>") to distinguish from what he calls the "cohomological interpretation" ("de Grothendieck", thank you!) of L functions. He presents the latter as a simple **corollary** of the trace formula. (In fact, in the spirit of my talk at the 1964 Bourbaki seminar, the two formulas were for me **synonymous**, as equivalent expressions, one additive the other multiplicative, of the same relationship between "the arithmetic" and "the geometric").

So the real motivation (albeit superficial) behind this obsession with "the formula" is not at all to do with the cohomological arsenal, but to minimize as much as possible, if not entirely erase, the fact that I played a role in demonstrating "the" Conjecture. It is Ellefi nally, who appears to me (up to the moment of the Colloque Pervers in June 1981) as **the** great point of fixation of the conflict that has been woven in my ex-student around the disavowed master.

seemingly innocuous situations (no match for the scale of an "operation" like the "Spread Cohomology" operation I've just looked at a little more closely), the silent effectiveness of these reflexes, working with perfect ease beneath that air of affable candor. Before you've even realized what's happened (if you ever do. . . ), he's already appropriated what you've joyfully created, first by withering it with the breath of discreet, insidious disdain. (It's also true that he's far from the only one in whom I've perceived this breath, which today seems to be part of the zeitgeist. . . ). )

But this breath that fades the beauty of what someone else has created and fades his joy, also fades the beauty of **everything** and the very creative power that is in him, as in each of us, to commune with the thing and know it deeply. Of course, this doesn't prevent him from doing "difficult" things and being admired, envied and feared. But the work he carried within him, of which I was able to see the first signs, is still waiting to be born. It will be born on the day (if ever) when something will have collapsed, and the master-slave will have become, as his disowned master was, a **servant**.

That's sixty well-packed pages now (not to mention a proud bunch of footnotes!), and nearly three weeks' work, that I've just devoted to the single "Cohomologie étale" operation. It's the most voluminous of them all, if not the "biggest" (this one will be reviewed at the end of last year, in the note with the well-deserved name "L'Apothéose"). . . ). I realize that with all this, I haven't even quite finished going through it all. One thing leading to another, this planned "tidying up" of the "facts uncovered" in a certain "investigation" has set the investigation in motion again, making me take a closer look at the rather ordinary volume called "SGA 4<sup>1</sup>", which I had previously only looked at on the run.

It was also an opportunity to revisit, with a more informed eye, the Allusie edition of SGA 5, from sad memory. I'm now aware of a meticulous agreement between the two thieves, Illusie putting herself at Deligne's entire disposal to present an edition of SGA 5 entirely ☐ment to the wishes of his prestigious protector and friend. This presentation of SGA 5 echoes, in a muted way, the spirit of debasement and contempt that runs through the coup-de-scie text, and lends discreet, effective support to the imposture set up in it.

p. 910

The introduction to the edition-massacre is written from beginning to end in such a way as to create in the uninformed reader the impression of a volume of "technical digressions", on the "SGA 4<sup>1</sup>" text which presents itself as central and prior (!). This impression is further reinforced, in Illusie's presentations, by the abundance of references to the pirate text, to which he generously refers every time he uses a result that his friend had seen fit to include in his digest, even when there are "tailor-made" references in the same SGA 5 volume, or even already in SGA 4<sup>525</sup> (\*).

I discovered the reality of a massacre in the course of reflection in the note of the same name (n° 87), dated May 12 last year, and in the sub-notes to it. In this set of notes, I finally give a detailed (if not yet exhaustive) description of the dismantling that had gradually appeared to me over the past two weeks. Having failed to dismantle in detail, as I have been doing for nearly three weeks now, the meticulous scam set up in the so-called "SGA 4<sup>1</sup>" around "la Formule", I still failed to grasp this aspect of meticulous concertation in last year's presentation of the Allusie edition of SGA 5. To conclude with the "Cohomologie étale" operation aka "SGA 4<sup>1</sup>

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<sup>525</sup>(\*) Thus, Künneth's formula with proper supports (over any basis scheme) is an immediate corollary of the basis change theorem for a proper morphism (derived categories version), which was the first great "break through" in stale cohomology, in February 1963. As such, it figures in the "nonsense gangue" of SGA 4 - we wouldn't want Illusie to refer to it, when there's the central text (intended to make us forget, precisely, those confusing predecessors) holding out its arms... ..

- SGA 5", it remains for me to give a few details on how this consultation manifested itself, in the presentation of "the formula" (the fixed points) in the Illusie edition.

I've already noted (in the sub-note "Les bons samaritains", n° 169<sub>2</sub> ) how Illusie, in his introduction, chimes in with his friend to give the impression that the publication of SGA 5 was dependent on the demonstration of the age-old Lefschetz-Verdier formula. (This demonstration had been available since 1964, and I had of course developed it in the oral seminar, without Illusie, who had taken on the task of drafted in 1965, found it useful to keep his promise for twelve years. ... ).

p. 911

□ I also recall that last year (in the note quoted "The Massacre", n° 87) I had already discovered cer-  
I'd like to point out the vicissitudes of Lecture XI of the original seminar. This lecture, inseparable from the following lecture XII which developed my version (the best known until 1981) of Lefschetz's formula in dimension 1, had completely disappeared from the Illusie edition. According to Illusie's introduction, this paper consisted of "Grothendieck's theory of commutative traces" (a providential slip of the tongue for "non-commutative"! ) "generating Stallings' theory" (of non-commutative traces), and disappeared (just as providentially) in a move (!). In reality, this talk developed the algebraic preliminaries essential for the description of local terms in the following talk, in which I developed a general method for calculating (or better, **defining**) local terms (via a "Nielsen-Wecken"-type formula<sup>526</sup> (\*) ) and its explicit application in dimension one (using Serre-Swan modules, if I remember correctly). In any case, Illusie "replaces" the original "disappeared" paper XI with a "new" paper III<sub>B</sub> , called "Calculations of local terms" (which, unless I'm mistaken and as if by chance, was also the title of the retracted paper!), of **which he presents himself as the author**. In this way, he kills two birds with one stone. On the one hand, it's an act of **mutilation, which** may seem gratuitous at first glance, making a mess of<sup>527</sup> (\*\*) by this brutal **cut**, snatching a presentation from its natural context, leaving a gaping hole in its place, for the pleasure of stuffing it somewhere else. Of all the mutilations that the delicate and meticulous Illusie has inflicted on what was once a splendid seminar (of which he suddenly saw himself as absolute master. . . ), this is perhaps the one that in retrospect strikes me as the most violent, the most brutally ostentatious: I can slaughter for free, and I do slaughter - with all the delicacy befitting my good breeding. Congratulations, Illusie, on this kind of work, which you didn't learn from me, but from someone else, whom you've taken as your model and master... .

p. 912

□ And one. And as a second blow by the same stone, masterfully struck, Illusie manages to **retract the paternity of** this formula of fixed points that I had worked out in 1965, at the same time (and above all) as he succeeded in **concealing this formula itself**. Since 1965/66, this had been "**the**" **correct formula for fixed points in dimension one**, much more general than the one developed by Verdier at Woodshole the previous year (otherwise there was no point in tiring me) and a fortiori, than that of Deligne's famous "Rapport" (which confines itself to the Frobenius correspondence alone, while following step by step the demonstration I had worked out in the general case). It was improved only a few years ago (almost twenty years later) in Alibert's thesis<sup>528</sup> (\*), which for the first time dealt with the case of a cohomological correspondence.

<sup>526</sup>(\*) This formula was appropriated by Deligne (without mentioning myself), with the method of passing from the Nielsen-Wecken formula with constant coefficients (therefore "ordinary"), to a formula of fixed points with quel-conque constructible coefficients. On this subject, see the sub-note "Les vraies maths. . ." (n° 169<sub>5</sub> , page 883-884). As a result (noblesse oblige. . . ) this same Deligne carefully avoids any mention of Lecture XII of the "later" SGA 5 seminar, where the name "Nielsen-Wecken" Nielsen-Wecken and Lefschetz formulas in algebraic geometry").

<sup>527</sup>(\*\*) This mutilation and this mess, among many others sown by the care of my ex-student Illusie at the orders of my ex-student Deligne, allows the latter to express himself condescendingly on the "confused state" ("albeit rigorous", because we're good players. . . ) of SGA 5, to which "SGA<sub>5</sub>4<sup>1</sup>" (however earlier it may be) is supposed to "remedy". . . All this under the watchful eye of the Congrégation des fidèles. Congratulations!

<sup>528</sup>(\*) This thesis was prepared under the supervision of Verdier (no mistake, always the same Verdier), who wrote it in Montpellier in 1981 or 1982 (I don't have the reference to hand). It represents the culmination of ten years of visibly gloomy work... ..



whatever. Illusie has managed to present the text in such a way that **the formula in question is practically impossible to find**: in the technical magma of the lectures (torn from each other) III<sub>B</sub> (sic) and XII, there is nothing (in the introductions to either of them, or elsewhere) to draw the reader's attention to this central result of the two lectures as a whole, and one of the most important of the entire seminar<sup>529</sup> (\*\*)! I confess that I have been unable to ascertain with absolute certainty whether this formula is to be found in SGA 5. Given the deliberate confusion of the text, and my remoteness from the subject, it would take me hours, even days, to find my way around. My problem is the absence of any reference to the Serre-Swan modules, which (if I remember correctly) gave the formula I had devised its elegance and conceptual simplicity<sup>530</sup> (\*\*\*) . It was precisely for the purposes of this formula that Serre had made some beautiful presentations on the Galoisian modules associated with the Artin conductor, which were of course to be included in the the published seminar, and which ended up □passed off (along with five or six other packets of expo- sés du séminaire originel - qu'à cela n'tienne pour les Illusie, Deligne et consorts... ). It's possible that the fixed-point formula in question is formula (6.3.1) in Lecture XII (p. 431). At a glance, there's nothing to distinguish it from the dozens of other copiously numbered formulas, among which this one is drowned. Clearly, the editor (Bucur) was overwhelmed by the task - and it wasn't the brilliant editor-sic Illusie, with fifteen years' experience in the limpid and impeccable tasks of editing, who would have lifted a finger to repair the blunders of his friend Bucur<sup>531</sup> (\*), which suited him perfectly. On the contrary, he manages to increase the confusion, by making the key formula, already untraceable, **indistinguishable from that of Lefschetz-Verdier**, or his particular case in "Rapport". In the introduction to the famous exposé III<sub>B</sub> -sic, by the improvised "father" Illusie, we read:

p. 913

"The second part of this talk (III<sub>B</sub> ), which is **much more technical in nature** [so don't go looking for it!], is **inspired** [!] by the method [!] used by Grothendieck to establish the Lefschetz formula for **certain cohomological correspondences** on curves [so don't go looking for which ones!] (see XII [but it's a fine thing to know where to find "the" formula!] and (SGA<sub>2</sub> 4<sup>1</sup> Rapport) [where the reader will have no trouble finding the formula, and being informed of the identity of its **real father**. . . (emphasis added).

Later in the same introduction, it is said that we (i.e. Illusie, of course) apply the techniques of n° 5<sup>532</sup> (\*\*)

□ "to define, at n° 6, **local Lefschetz-Verdier terms** for coho- correspondences. mological complexes of modules on rings that are not necessarily commutative."

p. 914

The name surreptitiously given to these "local terms" that I had introduced in 1965 for the purpose of writing the formula

<sup>529</sup>(\*\*) Technically, it's **the** crucial formula ("irreducible case") that enables us to prove the famous "*L-function* formula", equivalent to the trace formula (in any dimension) for the Frobenius correspondence. The crucial role of this formula is already attested by the very name of the SGA 5 seminar (a name that is never mentioned in the "previous" text "SGA 4<sup>1</sup> ": "*L-adic cohomology and L-functions*".

<sup>530</sup>(\*\*\*) It's possible that here, and in the following sentence, I'm confusing the structure of the Euler-Poincaré formula (in Lecture X) with that of Lefschetz (in Lecture XII). In the Euler-Poincaré formula, in the form in which it is figured in Bucur's presentation (based on my oral presentation), the Serre-Swan modules are indeed explicitly involved.

<sup>531</sup>(\*) The last lines of the Introduction (by Illusie) to the murder-edition of SGA 5, pretend to "pay tribute to the memory of I. Bucur, who died of cancer in 1976". - a year before the edition-massacre. I don't know if there's a cause-and-effect relationship - I have no doubts about Bucur's fundamental honesty and loyalty, who wouldn't have let an enormity like this go through without at least informing me. Still, the spirit of the operation in which the posthumous tribute is inserted gives it a suspicious flavour. In my opinion, "this was just paying lip service, when there was a better way of honouring Ionel Bucur's memory, by mitigating his blunders rather than shamelessly exploiting them.

<sup>532</sup>(\*\*) In non-commutative footsteps this time - lapsus-persiflage is strictly reserved for the deceased, at least as long as he or she isn't there to respond. ...

formula ("de Lefschetz-Grothendieck"), without having to refer to the local terms of the general Lefschetz-Verdier formula - this name is obviously chosen to maintain the confusion intended and maintained by Deligne - that the explicit formula in question **is** technically **dependent** on the Lefschetz-Verdier formula. A few lines further on, to add to the joy, we learn that "the local terms defined by Grothendieck in Lefschetz's formula of (XII 4.5)"<sup>533</sup> (\*) (which we don't mention are the very ones we've just generously christened "local Lefschetz-Verdier terms") "are indeed local Lefschetz-Verdier terms" (but this time in **another sense**, of course: those of the **general**, "non-explicit" Lefschetz-Verdier formula).

For the art of fishing in muddy water, in a style that I recognize all too well, it's good stuff! The same confusionist technique is used in the introduction to the volume, which reads (page VI, line 5):

"Applications to Lefschetz formulas are given in lectures XII and III<sub>B</sub> ." (emphasis added),

history, especially since the reader is hopelessly lost and has no chance of finding, or even trying to find, **the** only explicit Lefschetz formula known in dimension 1 (until 1981 at least), due (not to Illusie, nor even to his boss Deligne, but) to the late ex-"director" (sic), unnamed as de just<sup>534</sup> (\*\*), from the seminary gaily massacred by his "publisher"-fossilizer Illusie.

p. 915 In the original seminar, the retracted exposé XI, renamed III<sub>B</sub> (with □ a brand-new father), was inserted in a series of six lectures VIII a XIII, centered around the two closely related themes of Euler-Poincaré's and Lefschetz's ex- plicite formulas, treated in the same spirit, following common methods that I had identified during the seminar. In this part of the seminar, as in the others, there was an obvious unity of purpose and vision. This was meticulously massacred by my ex-student, taking advantage of his role as "editor"-sic-of a seminar wrecked by him and my other cohomology students (as a posthumous thank-you to their teacher). With a regularity worthy of the meticulous Illusie, every other lecture of the six, namely lectures IX, XI and XIII, disappeared from the massacre edition. Lecture IX was by Serre and presented the Serre-Swan theory of modules - seeing the turn of events, Serre preferred to withdraw his marbles and see to it himself that his beautiful lecture was made available to all. Lecture XIII was, as the "editor" explains in the introduction to the volume, overstocked - apparently the unnamed "director" couldn't count to thirteen - so it went down the trapdoor! As we've seen, by some brilliant sleight of hand, Expository XI ends up as Expository III<sub>B</sub> , in the appendix to Expository III (as luck would have it), which was originally entitled "Formule de Lefschetz-Verdier" (Lefschetz-Verdier Formula) and has now been renamed, for the sake of confusion, "Formule de Lefschetz" (Lefschetz Formula). In any case, this "move" was not made at random - it always goes in the same direction, that of the confusion tirelessly maintained by the perfect Deligne-Illusie tandem between the Lefschetz-Verdier formula (the one that is "conjectural", "local terms not calculated", but finally proved anyway by the combined efforts of Deligne and Illusie. . . ), and another, explicit formula that must remain rigorously hidden, carefully embedded in a magma of formulas numbered with

<sup>533</sup>(\*) (May 12) Puzzled by this unusual clarification (XII 4-5) concerning "my" formula, I've just looked at the cited reference. I find a "**Conjecture 4.5**" (p. 415), which seems to concern the possibility of defining local terms. We had a feeling that this impayable quidam was going to come up with another one of his conjectures, instead of a real défi nition... ..

<sup>534</sup>(\*\*) While all the essential results of the SGA 5 seminar, with the exception of the Lefschetz-Verdier formula and the Serre-Swan theory of modules (which does not appear in the massacre edition), are due to me, Illusie presents the texts in such a way that for **none of** these results (not only the so-called "Lefschetz formula" lost somewhere in an exposé XII. . . ) does it appear that my modest person had anything to do with it. ) it appears that my modest self had nothing to do with it. As a result, he played a leading role in the operation

**the eviction** of my person from the SGA, prepared for a long time by his friend Deligne, an eviction which finds its epilogue in the note "Les Pompes Funèbres - "im Dienst der wissenschaft"" (n° 175). (See also the sub-note "L'éviction (2)", n° 169<sub>1</sub> .)

four decimal places, insinuations that never said anything, carefully calculated ambiguities. Congratulations again, dear ex-student! As a result, paper X, entitled "Euler-Poincaré formula in staggered cohomology"<sup>535</sup> (\*), deprived of the one that preceded it and the one that followed it, hangs pitifully in the void. Good work, you haven't wasted your time... .

## (f) Congratulations or the new style

**Note** 169<sub>9</sub> (March 22 and April 29) I would like to come back to the confusion between the formula de Lefschetz-Verdier and the **occult** formula,  $\square$ . I have just discovered a rather copious "Terminology index" in SGA 5 - either you're careful, or you're not! Out of curiosity, I looked under "Lefschetz", in case "my" formula was there... . The only reference is to a "Lefschetz-Verdier formula (exposé III)". - which, as we've seen, has been renamed "Lefschetz formula". So the reader is well warned that there is no other "Lefschetz" formula (at least not in this volume) than the so-called "Lefschetz-Verdier formula" (the very one that he has learned is conjectural etc., that SGA 5 depended on it for life and death, and that "SGA 4<sup>1</sup>" as its name suggests saves the day here. . . ) Beautiful work, yes!

p. 916

I'm continuing my tour of my ex-pupil Illusie's prowess, under the tutelage of my other ex-pupil Deligne. I take up again the quotation from the introduction to the volume-massacre<sup>536</sup> (\*), where "the" Lefschetz-Verdier formula, always the same, had suddenly multiplied (by virtue of the art of mathematical prestidigitation) into "Lefschetz formulas", but nobody had ever been able to say which ones. He continues (page VI, line 6):

**"The trace formula in Lecture XII [which we hope no reader will ever think of unearthing. . . ] is demonstrated independently of the general formula of Lecture III, but it is shown in (III B 6) that the local terms that appear there are indeed those of the general formula, and that the latter implies it."** (Emphasis added.)

Nothing in her hands, nothing in her pockets - untraceable Illusie, just as untraceable as her brilliant prestidigitator-in-chief! Having tracked down one ambiguous trompe-oel after another, all pointing in the same direction, I've only just noticed that here, in an innocuous turn of phrase that had escaped me until now (as it will have escaped any other reader of this introduction more than four pages<sup>537</sup> (\*\*)),

$\square$  it is said in chiaroscuro that a certain formula in the traces of Lecture XII (which the reader must work out for himself

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<sup>535</sup>(\*) Unless otherwise stated, the reader will guess that this famous "Euler-Poincaré" formula is due to the two illustrious geometers whose name it bears. Compare with previous b. de p. note.

<sup>536</sup>(\*) See the beginning of the quotation in the previous sub-note "Les prestidigitateurs - ou la formule envolée" (n° 169<sub>8</sub>), page.

<sup>537</sup>(\*\*) Zoghman Mebkhout, who is an attentive reader but who arrived a little late, tells me that he himself has been deceived, convinced that he's not the only one.

that the explicit fixed-point formula (for Frobenius in any dimension, or for general correspondences in dimension one) did indeed depend on the general (non-explicit) Lefschetz-Verdier formula. So Illusie's thumb-affirmation had escaped his attention as well as mine - which was indeed the intended effect... ..

The confusion is reinforced by the fact that my 1974 Bourbaki lecture, presenting the formula for *L-functions* "with coefficients" in a constructible *l*-adic bundle (or, what amounts to the same thing, the explicit fixed-point formula for the Frobenius correspondence in such a bundle), was written **before** an **explicit** formula in dimension one had been made explicit. At the time, I assumed that proving the explicit formula for Frobenius, in dimension one, would appear as a corollary of the general Lefschetz-Verdier formula - that "all we had to do was make the local terms explicit". So, anticipating work that remained to be done, by Verdier in this case, I named this **explicit** formula the "Lefschetz-Verdier theorem" in this Bourbaki presentation. In what follows, both Verdier's "woodshole" demonstration and my own, covering a much more general case, do not make use of the general Lefschetz-Verdier formula. The situation was perfectly clear to all SGA 5 listeners, at least. But for those who only knew about my Bourbaki presentation to the exclusion of SGA 5

as he can to find out which one!) is demonstrated independently of "the general formula of exposé III" (which, for the occasion, is also not entitled to a name, in accordance with the method known as "deliberate vagueness". . . ) - only to follow up in the same breath and in the same sentence (as if to "**make up for**", as it were, a statement that was not in line with the rules of prudence. . . ) with a "but one shows... ". This "but" refers to that "platonic" complement that no one, starting with Illusie and Verdier, had bothered with for twelve years, namely that "my" local terms - sorry, I meant "the ones that appear in them" - were "my" local terms.

(in this formula, traces of exposé XII, the author of which will never be clearly named<sup>538</sup> (\*))

□ - that these terms are those of the endless "general formula" - and the vagueness about the names given to formulas and where to find them, suddenly gives way to exemplary precision, worthy of the meticulous Illusie: this demonstration of a "rabit" can be found in III B 6 - if a reader wants to make sure it's there, he'll have no trouble finding it!

And why this sudden interest in this identity, when the fate of the SGA 5 seminar in its entirety had left Illusie (like my other cohomology students) perfectly indifferent for eleven years? It's so that I can brilliantly follow up, in the same sentence again (it's from the envoy or I don't know my stuff!) that "the general formula" (by Lefschetz-Verdier, not to name it) **implies** "that of exposé XII" (by an equally unnamed defunct).

It's a truly brilliant trick! My brilliant ex-student sweated blood and water, including mathematical piecework, but yes, to arrive at the brilliant result of this seemingly innocuous - and yet, in the eyes of a Deligne and those of his servant, momentous - end of sentence: the Lefschetz-Verdier formula "implies" that of "exposé XII" (which we've just said was demonstrated independently, but never mind for the sake of the all-symbolic satisfactions of the unconscious!).

This "**implication**" is of a very particular nature, mathematically speaking - and I bet I'm the only mathematician in the world, apart from the brilliant inventor of the gag (and perhaps his master Deligne), who could appreciate its flavor. To understand it, however, you don't need to be a specialist, or even a mathematician. The two formulas, the "general" one (a.k.a. Lefschetz-Verdier) and "that of exposé XII" (a.k.a. the unnamed deceased), are expressed respectively as follows

$$T = L, \quad T = L',$$

where the term  $T$  (alternating sum of traces) is the same in both formulas, while the terms  $L, L'$  (sums of local terms) have been defined ad-hoc (one by Verdier in the spirit of Lefschetz, the other by the deceased in the spirit of Nielsen-Wecken-Grothendieck). Eleven years later, Illusie (whose editorial zeal was suddenly awakened at a sign from the chief) makes a sudden effort, worthy of a better cause, to prove

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(remaining sequestered until 1977), there was a misunderstanding, which was exploited to the full by Deligne and Illusie, in mutual agreement, to set up the deception (sewn in thick white thread) "SGA 4<sup>1</sup> - SGA 5".

From the point of view of the imposture of SGA 5's "logical dependence" on the misleadingly-named pirate-text, this doesn't hold water anyway, even if the explicit formula did indeed depend on Lefschetz-Verdier's "conjectural" formula. Indeed, as Deligne himself notes in passing in the famous "Méthode A" (for a reader who asks grâce - see "Les vraies maths. . . " n° 169, page 884), the "easy reductions" of the unnamed quidam brought us back to the case of dimension one, where "the ingredients of the demonstration were moreover all available".

All these deceptions work, as long as they're served up to a reader who's either asleep, in a hurry, or who wants nothing more than to be emberlifi coté. To an attentive and critical reader, the whole clever set-up appears for what it is: a shameless swindle. But I seem to be the first attentive and critical reader, in the eight years since this scam appeared on the mathematical market. ...

<sup>538</sup>(\*) For the reader of SGA 5, it's Illusie, author of the brilliant exposé III<sub>B</sub> on "local terms", who must appear as the modest father of the never-named formula. For a reader of the volume entitled "SGA 4<sup>1</sup>", who hasn't heard of another formula than "Rapport", the father is obviously the brilliant author of the volume, for a reader of the two (if there is one), he'll just have to flip a coin, or give his tongue to the cat... .

directly (?)

$L = L'$  (and the same applies to local one-to-one terms),

so that we can say that the formula  $T = L$  "implies"  $T = L'$  (and thus, implicitly, that the formula  $T = L'$  of the seminar to be massacred, crucial for the theory of  $L$  functions, "depends" on the formula  $T = L$ , which remained "conjectural" before the appearance of Deligne and his providential "SGA 4<sup>1</sup>" - sic. . . ).

□ The situation becomes even more grotesque for those in the know, who realize that p. 919 nobody in the world would have had the idea of the abracadabra definition of the local terms that enter into  $L'$  (those of the unnamed deceased), if this definition hadn't been directly "blown" by the very process of demonstrating the formula  $T = L'$ . To tell the truth, I can say that I found a "demonstration" of the formula  $T = L'$  **even before** I had defined the second member  $L'$  and its local terms: the latter "came out" of the demonstration, no more and no less<sup>539</sup> (\*).

Congratulations, a third time, Illusie, and to you just as much, Deligne, who served as her model. Together, you have pioneered a **new style** in mathematics. A style that has already set an example. It has already become known as the "1980 style", with a visibly bright future<sup>540</sup> (\*\*). It's a style of prestidigitation, aka "the gravedigger's style", where the art lies in constantly **deceiving the reader**; not only on the **authorship** of the main ideas, but also (in the process) on their filiations and mutual relationships, on the significance of each, on what is essential and what is accessory - and all this for the laudable purpose of magnifying that which is to be magnified, of debunking (or burying with a nonchalant gesture and the bend of an anodyne sentence . . . ) those who are to be debased (or buried. . . ); and **above all**, to have the sensation that

**power**: to lead the reader around by the nose, to make and unmake the history of the company.

his science **according to his good** □ **pleasure**, and decide what "are" the mathematical things he claims to expose,

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and what they are not. It's the art of always "**ruling**" by delicately pulling invisible (?) threads, without ever, ever stooping to serve. And all this, so as to be always and totally "inch!" So that if, by any chance, a cleverer-than-thou reader were to go and have a look for himself, and have the unusual idea of using (you never know...) his own lights and faculties (it's rare, but after all, it could happen...), he'd never be able to catch you in the act of saying something which, **taken literally** and with no escape from ambiguity or double entendre, is well and truly and irremediably **false**.

The art of art lies in this style clause, which may seem a challenge, and yet... . With the Colloque pervers d'étrange mémoire, barely four years after the virtuoso displays of prestidigitation of the dazzling "SGA 4<sup>1</sup> - SGA 5" operation, we have seen just how far this new, innocent technique can go.

<sup>539</sup>(\*) I should point out, as is self-evident, that in all conceivable applications (not just to the  $L$ -function formula, concerning the Frobenius correspondence alone), it is the **explicit formula**  $T = L'$  that is **the** relevant formula. From a practical point of view, and as far as one-dimensional phenomena are concerned, the Lefschetz-Verdier formula  $T = L$  is only of historical (or heuristic) interest, and the same applies a fortiori (at least until further notice) to Illusie's result  $L = L'$

(or, more precisely, that the two types of local terms, those figuring in  $L$  and those figuring in  $L'$ , are the same).

These are all very obvious things, but they're the kind of things that these two guys manage to do (and succeed at, these days) to blur. It's a sobering thought as to what sense the unbridled scientific production we're witnessing can have, when such crude breaches of simple mathematical common sense (and this on issues that closely touch on crucial progress made over the last twenty-five years in our knowledge of the relationship between geometry and arithmetic) go unnoticed by one and all. . .

<sup>540</sup>(\*\*) For eloquent examples in this vein, see the few samples of the "1980" style that appear in the note "La maffi a" (n° 171<sub>2</sub>), written by our great authors Brylinski, Kashiwara, Beilinson and Bernstein. Clearly, we have every reason to hope!

(May 12) As other occasional adepts of the "new style", who have distinguished themselves in the wake of the work of an obscure posthumous pupil never named, I can now add Malgrange, Laumon, Katz. (See note "Carte blanche pour le pillage", n° 171<sub>4</sub>.)

to go, in the concealment of an innovative work, and in the shameless despoiling of the man who had long carried this work and matured it in solitude... .

Hats off to the master and the pupil, to Deligne and Illusie! An artist's work! You both deserve the unanimous recognition of the entire Congregation.

## 18.5.3. (2) Sharing ("Duality - Crystals")

### 18.5.3.1. a. The last man in - or deaf ears

**Note** 170(i) (February 28) I've come to the third of the "four operations" around my mathematical work (pending the fourth in the next note, skipping Zoghman Mebkhout's work).

III The "Duality - Crystals" operation (or: "Les Beaux Restes. . .").

As I see it now, it's roughly a question of **sharing** the part of my work concerning cohomology that hadn't yet been appropriated (de facto, or symbolically) by **P. Deligne**<sup>541</sup> (\*). The latter has obviously reserved the lion's share for himself, with the motifs and staggered cohomology, and more specifically- the  $\ell$ -adic cohomological tool. The remainder(\*) is shared between two other of my co-students.

p. 921 homo □ logistes, **J-L. Verdier and P. Berthelot**<sup>542</sup> (\*). The consensus that has emerged, I cannot say when and how, seems to be as follows: to Berthelot all crystalline cohomology, and the rest to Verdier, who essentially annexes everything that revolves around the yoga of duality<sup>543</sup> (\*\*), and the yoga of derived and triangulated categories that constitutes its algebraic prerequisite.

Concerning Berthelot's participation in the sharing of my remains, I have only one fact, albeit a small one. I came across it by chance last year, in the course of reflection in the note "Les co- héritiers... . "(n° 91), and I devoted a small sub-note to it (n° 91<sub>1</sub> ). This is Berthelot's article-survey, which I quote there<sup>544</sup> (\*\*\*), presenting the main ideas for a "synthesis" (he says) of Dwork-Monsky-Washnitzer cohomology and crystalline cohomology, at the September 1982 Colloque de Luminy entitled "Analyse *p*-adique et ses applications". In the introduction, part b), he gives a short history of crystalline co- homology, in a narrow-minded way that in no way corresponds to the much broader vision I had of crystalline yoga<sup>545</sup> (\*\*\*\*).

My name is omitted from both the text of the article and the bibliography. I refer to the sub-note quoted for a few comments and clarifications, which need not be repeated here. I'd just like to add that, once I'm out of the picture, it's none other than Berthelot who is considered to be the father of crystalline cohomology, without him even bothering to say so in plain English - a certain style of appropriation has obviously become the norm... . In fact, it was his thesis, which he prepared with me based on my initial ideas, that was the first published work on the subject of crystalline cohomology (apart from the very brief sketch that I myself had prepared).

<sup>541</sup>(\*) (May 1) It is nevertheless worth setting aside the formalism of duality in **coherent** context, which (contrary to an impression that has turned out to be hasty) has apparently not yet been appropriated by any of my cohomology students, nor by anyone else to my knowledge. It's true that the only reference text, setting out the bulk of my ideas and results on this theme, is R. Hartshorne's "Residues and Duality", which makes it possible to refer to it without at any time having to pronounce an undesirable name. ...

<sup>542</sup>(\*) (May 1) It has since become clear that we need to add a "fourth thief" in the person of Neantro Saavedra Rivano, who appropriates the philosophy of the Galois motivic group, via the categories christened "Tannakian" for the occasion. But he simply acted as a "straw father" on behalf of Deligne, who "recovered" the paternity ten years later.

For a detailed history, see "The sixth nail in the coffin", n° s 176<sub>1</sub> to 176<sub>7</sub> .

<sup>543</sup>(\*\*) See footnote on previous page.

<sup>544</sup>(\*\*\*) Géométrie rigide et cohomologie des variétés algébriques de caractéristique  $p$ , Pierre Berthelot, in Colloque de Luminy 6-10 septembre (CIRM) "Analyse *p*-adique et ses applications".

<sup>545</sup>(\*\*\*\*) on this subject, see the sub-note "Deaf ears" (n° 170(i)bis ) which follows this note.

made of some of the ideas of  $\square$ départ<sup>546</sup> (\*)). His thesis presents a large-scale groundwork for a p . 922 first part (170(i)bis) at least to the program I had proposed.

This memorable "survey" took place in 1982, a year after the "Colloque Pervers" (Luminy June 1982), which we'll be talking about with "Operation IV". I haven't bothered to go back through the Berthelot prints in my possession, to find out whether this participation in my Burial represents a late turning point in his relationship to me and my work, or whether it's the continuation of an earlier attitude. If the former, it's a safe bet that this turnaround comes in response, as it were, to the sudden and unbridled self-escalation in the general degradation of scientific ethics, accomplished the previous year with the Colloquium. Let me remind you that 1982 also saw the publication of the "memorable volume" LN 900, exhuming the motifs<sup>547</sup> (\*\*), in which the person who bore the brunt of the operation was no longer a vague "service unknown" (as at the brilliant Colloquium), but a "deceased" whose name, in spite of everything, is still remembered (albeit reluctantly. . . ). The previous year's operation had shown clearly enough that no restraint was to be expected - and "operation Motifs" did indeed pass, just like "operation Cristaux" and all those that had preceded it, without the slightest wrinkle. ...

**Note** 170(i)bis (170(i)bis) (February 28 and April 30)<sup>548</sup> (\*\*\*) here I mean by the "first part" of crystal theory (in car.  $p > 0$ ) that which concerns the crystal cohomology, with constant coefficients (or "twisted constants"), of **clean and smooth** schemes on a basic scheme of car.  $p$ . It is then sufficient to work with the "ordinary" or "infinitesimal" crystal site, which I had introduced (provisionally) towards the end of the years sixty<sup>549</sup> (\*\*\*\*). In fact, contrary to the restricted meaning Berthelot likes to give to the term "cohomologie cristalline", this one had for me from the beginning a much  $\square$  broader meaning, which I did not hide from p . 923 him or anyone else, and which my students apparently forgot - only to "reinvent" a little piece of it ten or fifteen years later. ...

On the one hand, from the outset, my crystal ideas were by no means confined to the case of schemes of a given characteristic  $p > 0$ . My first crystal reflections, before I came up with the new idea of introducing "power-divided thickenings", focused on schemes of **zero characteristic**, where the divided powers are automatically present (and therefore tend to go unnoticed. . . ). The natural outcome of this direction of research, renewed thanks to the ideas of Zoghman Mebkhout, would be the formalism of the six operations for "De Rham-Mebkhout crystalline coefficients" on zero characteristic schemes (to begin with), a formalism to which I had already alluded in the note "Melody at the tomb - or sufficiency" (n° 167). As early as the 1960s, I foresaw a cris- talline cohomology without characteristic distinctions, in the form of a crystalline "six operations" formalism in the context of (for example) finite-type schemes on the absolute basis  $Z$ . It was to encompass the "ordinary" crystalline theory (which was still being sought - and is still being sought) for finite-type schemes on the  $p$ -element  $F$ -body $_p$ . I'm convinced that forgetting and burying this vision of the late master (however simple and inspiring it may have been) is the cause of the sorry stagnation of crystalline theory, almost twenty years after its vigorous beginnings.

<sup>546</sup>(\*) The only published sketch of these ideas, based on five lectures I gave at IHES in November and December 1966, written by I. Coates and O. Jussila, is "Crystals and the De Rham Cohomology of Schemes", in Dix exposés sur la Cohomologie des Schémas (North Holland, Amsterdam 1968) pp. 306-358. All the essential starting ideas are outlined, including the need to introduce local thickenings à la Monsky-washnitzer (pp. 355-356).

<sup>547</sup>(\*\*) See "Silence" (n° 168), especially ". . . and exhumation" (n° 168(iii)).

<sup>548</sup>(\*\*\*) This sub-note is taken from a footnote to the previous note "La part du dernier". (\*\*\*\*) (May 12) In fact, already in 1966, see b. de p. (\*) above.

<sup>549</sup>(\*\*\*\*) (May 12) In fact, this was already in 1966, see b. de p. note (\*) above.

On the other hand, and to return to the Monsky-Washnitzer approach, which had helped "trigger" my interest in crystal cohomology, I had in mind from the outset the need to introduce (for the purposes of a theory that would not apply only to clean, smooth schemes) a crystal site larger than the "infinitesimal" site, where the "thickenings" envisaged would be spectra of **topological** algebras. (with power-divided ideal), perhaps those used by Monsky-Washnitzer (freed of

unnecessary assumptions such as smoothness)(\*). Identifying "the right site" and "the right coefficients" is part of the program that I had bequeathed (to no avail, it now appears) to my cohomology students, starting with Berthelot. Having thought about the matter recently "in passing" (while writing *Récoltes et Semailles*), and remembering the imperative of a crystalline theory encompassing all features at once, I've come to wonder whether these topological algebras (à la Monsky-Washnitzer, or any other reasonable variant) aren't too "coarse" (in the same way as restricted formal series), because they're too "far removed from the algebraic", and if they shouldn't be replaced by "thickenings" that are (in a proper sense) "étale neighborhoods". I plan to return to these questions in the part of *Reflections* following on from *Harvest and Sowing* (volume 3, I presume), with the exposition of the yoga of the six operations and the "problem of coefficients", and in particular crystalline coefficients of the "De Rham-Mebkhout" type.

Mebkhout had already sensed that his D-Module philosophy would provide a new point of view for crystalline theory. But his suggestions in this direction, notably to Berthelot in 1978, coming from a vague unknown and unrepentant Grothendieckian, fell on deaf ears<sup>551</sup> (\*). . .

<sup>550</sup>(\*) As I pointed out in a previous b. de p. note (see page 922), such Monsky-Washnitzer thickenings are mentioned in my first and only published talk on crystalline yoga, from fi n 1966. From that moment on, it was clear to me that crystalline cohomology of characteristic  $p > 0$  was going to be played out for the most part on rigid-analytic spaces of zero characteristic. Of course, I didn't fail to make this clear to anyone who might be interested, and certainly first and foremost to my pupil Berthelot, once he had chosen to take up the crystalline theme. In the article quoted, in a style that I recognize well and that Berthelot did not invent, it seems as if he had just discovered (fifteen years later) the unsuspected link with rigid-analytic(x) geometry. Here, he poses as the brilliant inventor of a "common generalization" (of Monsky-Washnitzer and crystalline theory), which he pompously christens "rigid cohomology" (and which will soon be called, appropriately enough, "Berthelot cohomology"). I should also point out that Berthelot's work is "the continuation of a reflection carried out with Ogus" - the same Ogus who distinguished himself the same year (1982) by his participation in the "Motifs" scam, as co-author of the LN 900 volume.

The systematic burial continues in a later article by Berthelot (of which I have a preprint) "Rigid cohomology and Dwork theory: the case of exponential sums" (undated). No reference to the deceased for the crucial notion of  $F$ -crystal, or that of cohomology with proper support (which I have the honor of introducing into algebraic geometry in February 1963, twenty years before. . . ). These notions are so natural that there's really no need to bother. . . . The notion of a generic fi bre of a formal scheme (above a discrete valuation ring), as a rigid-analytic space, is generously attributed to my ex-student Raynaud. This notion was known to me before Berthelot, Raynaud or anyone else had even heard the word "rigid-analytic space", since it was the need to be able to define such a generic fi ber that was one of my two motivations for foreseeing the existence of a "rigid-analytic geometry", and it was also he who was subsequently one of the two driving fi les for Tate, setting up a formal construction of such a geometry: its defi nition had to be such that the notion of "generic fi bre" became tautological. . .

(x) (September 1985) In fact, the first to foresee the existence of such a theory was J. Tate, in August 1959. On this subject, see note n° 173 d) ("L'Enterrement - ou la pente naturelle"), and more particularly the footnote on page 1132.

<sup>551</sup>(\*) Having deaf ears doesn't stop the same Berthelot, in the article I quoted in the previous b. de p. note, to refer nonchalantly (at the end of par. 3 A) to "an analogue of the theory of  $D_X$ -Modules on a complex variety", which "for the moment" is not yet available in the rigid-analytic framework. There's no question, of course, of mentioning the name of a certain vague stranger who had come to him with outlandish suggestions four or five years earlier, and all the more so as a certain Colloquium the previous year (discussed in the following note "The Apotheosis", n° 171) had clearly set the tone with regard to the vague unknown in question, surely, within a few years, and with the blessing of the true father of the well-known "Riemann-Hilbert-Deligne" philosophy, Berthelot was to become the brilliant inventor of  $D$ -Module philosophy in the context of "rigid-analytic cohomology", also known as (although he himself refrained from calling it this) "Berthelot cohomology". Which just goes to show that, these days, you don't need a sharp ear to go that far... ..



### 18.5.3.2. b. Glory galore - or ambiguity

**Note** 170(ii) □ (February 28)<sup>552</sup> (\*) To situate "Operation Duality", to the dubious benefit of J.L. Verdier, there are p. 925

should first say a few words about the yoga of duality (called "of the six operations" - but the name sank without trace) that I had developed from the second half of the fifties onwards, and that of derived categories, which is in truth inseparable from it. I expressed myself in some detail on this subject in the note "Mes orphelins" (n° 46, in particular pages 177-178) and in the sub-note n° 46<sub>2</sub> to this one (pages 186-187), and finally (in a beginning of reflection on the role of Verdier in the burial of my point of view in homological algebra) in the note "L'instinct et la mode - ou la loi du plus fort" (n° 48). I don't think it's necessary to return to this, and suggest that readers refer to it if necessary, before continuing with the account of the "Duality" operation<sup>553</sup> (\*\*).

Verdier's attitude to the sharing operation appears more ambiguous than that of his two friends, in that **he played**, sometimes simultaneously, **on two** seemingly contradictory **fronts**. At first, it was hard for me to identify with them, as the situation seemed so confusing. On the one hand, after he defended his thesis in 1967 and especially after I left in 1970, he tried (for reasons that escape me) **to bury and discredit** the yoga of cohomological algebra and duality that he had inherited from me, even though he had devoted most of his energy, throughout the sixties and up to the defense of his thesis, to developing these ideas and enriching them with his own contributions. On the other hand, from at least 1976 onwards (nine years after he had defended his thesis-sic), and with the encouragement and effective support of Deligne, he pretended to claim authorship of both the original ideas (insofar as they were not boycotted), as well as all the methods and results I had developed □ around the theme, methods that apply mutatis mutandis to all kinds of other contexts<sup>554</sup> (\*), such as topological spaces, or complex analytic spaces. p. 926

Regarding Verdier's attitude towards derived categories alone, I have tried to put my finger on the meaning of this ambiguity in the note "Thèse à crédit et assurance tous risques" (n° 81)<sup>555</sup> (\*\*). It also contains a number of material facts, notably about the strange circumstances surrounding his thesis work (still unpublished today) and defense. With the benefit of a year's hindsight, the vision of things that emerges in the course of this reflection seems to me probably correct (perhaps with a few tweaks), but superficial nonetheless. It's quite clear to me that Verdier's **real** motivations lie not at the level of some paltry "calculation of returns", but are of an entirely different nature, and essentially involve his ambivalent relationship with me. Even to a superficial observer, it seems to me, it's particularly obvious in his case that, in believing he was burying the man who was his master, it was none other than **himself** and the creative force within him that he was burying, day after day and right up to the present day.

<sup>552</sup>(\*) The text of this note was edited and corrected on certain points on May 1 (Lily of the Valley Day).

<sup>553</sup>(\*\*) (May 12) See also the note "L'ancêtre" (n° 171(i)) and "Le tour des chantiers - ou outils et vision" (n° 178), in particular the "Six opérations" and "Coefficients" chantiers (n° s 3,4).

<sup>554</sup>(\*) Of course, in the "other contexts" in question, the original difficulty of the slab context, i.e. the need for a "breakthrough", is still present. which gives a minimum grip on the stale cohomology (in the absence of the well-known transcendental constructions using singular simplexes, retraction methods etc.) don't arise. My students have all found situations where the big preliminary "breakthrough" work had already been done by someone else - all they had to do was bring in their furniture, which the "other" often provided on top of everything else. As soon as the opportunity arose, they hurried to bury it, to take advantage of what they saw fit to appropriate, and to make fun of the rest... ...

<sup>555</sup>(\*\*) When writing this note, I was not yet aware of how Verdier had distinguished himself, with the "good reference" he provided in 1976 - see "step 2" below.

To round off the "Dualité" operation, I'm now going to give a brief retrospective of the various stages of this operation that I know of, and more generally, of Verdier's participation in the Enter- rement.

**Stage 1** (1966-1976). It was after I left in 1970 - I can't say exactly when - that Verdier informed me that he no longer intended to publish his thesis. The thesis was supposed to present the new foundations of homological algebra, from the point of view of derived categories. In my view, the *raison d'être* of his thesis work was to be made available to all, to provide a reference text. comparable in scope to the Cartan-Eilenberg book, directly adapted to the new needs that have arisen in the past few years.

p. 927 □courses <sup>of</sup> the fifties and sixties in the wake of my work and that of my students. Looking back,

I realized that this new cohomological language had only been fully assimilated by my cohomology students, and that Verdier's decision was tantamount to drawing a line under this new vision of homological algebra. As a result, his twenty-five-page "thesis", which merely presented a convincing sketch of ideas that he himself said were not his own, lost its meaning and became, strictly speaking, a "thesis-bidon". But in the early 1970s, when I learned (with surprise) of Verdier's decision, I was so intensely absorbed in tasks that were the antithesis of my former mathematical interests, that these questions were infinitely remote to me. It never occurred to me to write about the subject, learned in a draught (I can imagine) between a public discussion on the scandal of the cracked drums of atomic waste at Saclay, and a work session for the *Survivre et Vivre* newsletter! And even less would I have thought of reacting. The first time I finally "posed" on the meaning of Verdier's act, and its nature as deliberate sabotage timidly began to emerge, was in the aforementioned note "L'instinct et la mode - ou la loi du plus fort" (n° 48), taken up a few weeks later, after the discovery of l'Enterrement "dans toute sa splendeur", in the much more detailed and in-depth note "Thèse à crédit et assurances tous risques" (n° 81).

In retrospect, it becomes clear that Verdier's division in the work he had assigned himself, and which formed part of the "contract of good faith" he had entered into with his thesis jury (see note cited n° 81), must date back at least to 1968 or 1969; otherwise the writing and publication of his "thesis" would have been a done deal long before I left in 1970. I would remind you that I had already submitted the work program for his thesis to him in 1950, and that for a gifted and motivated researcher such as he was at the time, this program, with its extensive drafting of new foundations, could hardly have represented more than three or four years' work at the most, updating and all. It's also true that a certain mentality, which consists in arranging to withdraw credit in advance for a planned "job", which one then has no reason to bother doing, has become the norm.

- such a mentality is now becoming apparent to me as early as 1964, with the vicissitudes of the formula Lefschetz-Verdier" duality, and later, with the "Verdier" duality of

p. 928 locally compact spaces, in the spirit of the six operations □ (which always remain unnamed)<sup>556</sup> (\*). But

Throughout the sixties, locked up as I was in my tasks and in the vision I tirelessly pursued through them, like Ahab's elusive and omnipresent white whale, I had no idea that something was "wrong" with the man who was for me like a close companion in tasks I believed to be "common" - any more than I would have suspected it for any other of my cohomological students. And with twenty years' hindsight, I am now struck by the extent to which, for ten years of my life (if not fifteen or twenty), I lived completely **out of step** with the reality around me, not only in my family life (where I came to realize this a long time ago), but also in my professional life.

<sup>556</sup>(\*) On the subject of this rather unusual spirit, see the sub-note "Le patrimoine - ou magouilles et création" (n° 169<sub>6</sub> bis), and also last year's sub-notes (n° s 81<sub>2</sub>, 81<sub>3</sub>) to the aforementioned note "Thèse à crédit et assurance tous risques".

in my professional life, in which I invested myself with passion... .

But I return to "stage 1". In any case, Verdier's ambiguous relationship with me and my work became apparent as soon as the SGA 5 seminar was completed in 1966: he, like none of my other cohomologist students, was not concerned with the editing of this seminar<sup>557</sup> (\*\*), which remained in the hands of "volunteers" - sic - who were overwhelmed by the task, or who had little concern for keeping their commitments. Clearly, even then, the situation among my cohomology students was rotten, although I didn't notice anything, preferring to live in a world where everything is order and beauty. . . It's eighteen years later that I'm beginning to take a first, tentative look at what really happened, in times that (just a year ago) seemed idyllic<sup>558</sup> (\*\*\*)).

After I left in 1970, and even before he announced his "official" decision to scuttle his work Verdier's ambiguity in the sixties was confirmed by his complicity with various mini-escroqueries of his friend Deligne's vintage, which he couldn't fail to notice :□|' escamotage de my person in the Hodge I, II, III articles<sup>559</sup> (\*), then in the published version of the SGA 7 II monodromy seminar (presented under the names of Deligne and Katz, the latter unexpectedly taking the still-warm place of a deceased... .). In the same year (1973), he also came across Mac Pherson's paper, which solved a "Deligne-Grothendieck conjecture" for which he knew Deligne had nothing to do with it.

Until 1976, Verdier's role in L'Enterrement seems to have been mainly passive, at least as far as tacit annexation operations are concerned. On the other hand, by refraining from publishing what was supposed to be his thesis (which had been granted to him "on credit"<sup>560</sup> (\*\*)), he played a crucial role in the enterrement of my point of view in commutative homological algebra (which he had made his own for a while), and of its use as an "everyday" technique in algebraic geometry, topology and algebra. Like his friends Illusie and Deligne, by thus scuttling the work of his own hands, for the pleasure of burying the one who had inspired him, he has well deserved the unreserved recognition of the unanimous Congregation... ..

This deliberate intention to bury was also clearly expressed in his discouraging attitude towards Zoghman Mebkhout, after 1975, when he pretended to be inspired by my yoga of duality, and that of derived categories. On this subject again, I refer the reader to the more detailed notes already quoted, "My orphans", "Instinct and fashion - or the law of the strongest", "Thesis on credit and all-risk insurance" (n° 46, 48, 81), as well as the note "L'inconnu de service et le théorème du bon Dieu" (n° 48)<sup>561</sup> (\*\*\*)).

**Stage 2** (1976). 1976 saw the publication of the "memorable article" □de Verdier in Asterisk<sup>562</sup> (\*), already referred to as "episode 3 of an escalation" with the operation "Cohomologie étale" (see note "Les manoeuvres", n° 169). Let me remind you that this fifty-page article consists (apart from a few pages of its own) in repeating verbatim a certain number of notions and techniques I had developed.

<sup>557</sup>(\*\*) In retrospect, I wonder what Verdier could have been doing with his time between 1964 (when, thanks to my contact, he had managed to get to grips with the new cohomological techniques) and 1970, when he didn't deign to take on and complete any editorial tasks, not even the theories he was to present himself as the author of. For a list of his contributions, valid but none of which were completed, see sub-note n° 81<sub>1</sub> to the much-quoted note.

<sup>558</sup>(\*\*\*) see in particular, in "Fatuity and Renewal", the section "A world without conflict?" (n° 20), where only the question mark is used. gation in the section name may suggest some doubt about the "idyll".

<sup>559</sup>(\*) In the joke about "weight complexes" (see note of the same name, n° 83), I thought I discerned an allusion, in a defiant tone, to the oldest patent fraud of which I am aware in one of my cohomology students, namely that of Deligne in his 1968 article on the degeneracy of spectral suites. Although I didn't see the light at the time, the example set by my most brilliant pupil was not lost on everyone!

<sup>560</sup>(\*\*) See note n° 81.

<sup>561</sup>(\*\*\*) (May 1) See also the sub-note "Ecllosion d'une vision - ou l'intrus" (n° 171<sub>1</sub>) to the note "L'Apothéose".

<sup>562</sup>(\*) J.L. Verdier, "Classe d'homologie associée à un cycle", Astérisque n° 36 (SMF) p. 101-151 (1976).

ten years earlier in SGA 5, without any reference to myself or to a seminar on the subject. This publication, which I discovered a year ago in the wake of the Colloque Pervers (in the note "La bonne référence", n° 82), shed a whole new light on why he and my other cohomology students were so reluctant to make the SGA 5 seminar (under this name, and with his authorship) available to the mathematical public.

There's no need to go back over the comments I made on this article in yesterday's note (n° 169). As an amusing detail, I'll just add that it was the manuscript of this "work" (sic) by Verdier, which the latter had been kind enough to send to Zoghman Mebkhout the previous year (1975), that was for the latter the Sesame-Ouvre-Toi of the cohomology of varieties, and the foundation of an unreserved admiration for the man who, from then on, appeared to be his "benefactor". This admiration was, moreover, long-lasting, and only disintegrated completely, I believe, following Zoghman's misadventures at the Colloque Pervers.

Deligne tells me<sup>563</sup> (\*\*) that he only became aware of Verdier's article after the publication of "SGA 4<sup>1</sup>/<sub>2</sub>" (sic) and SGA 5, the following year (1977) - which would run counter to my conviction that the publication of Verdier's "good reference" marked an essential last step in the "escalation" of scams, which eventually culminated in the totally different "SGA 4<sup>1</sup>/<sub>2</sub> - SGA 5" operation the following year. On reflection, I find Deligne's version hard to believe. As one of the best-informed mathematicians I know, and one who has remained in close contact with Verdier throughout his life, it's hardly possible that he wasn't already aware of Verdier's project, that he didn't receive a preprint of it (even before Mebkhout), and that he wasn't one of the very first to be served for the separate printings, in 1976. This article (as confirmed by Deligne himself) a gaping hole in the literature (failing publication of the SGA 5 seminar after 1966), and it's hardly possible either that Deligne didn't take the peine at least to go through it - question of a quarter hour at the most for someone "in the know" like him<sup>564</sup> (\*). In any case, the fact that this blatant plagiarism elicited no reaction from any of the other six or seven ex-SGA 5 auditors who were "in the loop", was a sure sign of the smooth connivance between all concerned. The time was ripe for a massacre of the SGA 5 mother seminar, and for shattering my work on staggered cohomology... .

p. 931

**Stage 3** (1977). In this "SGA 4<sup>1</sup>/<sub>2</sub> - SGA 5" operation which took place in 1977, on Deligne's initiative and with Illusie's eager participation, Verdier this time played a supporting role, contributing to the meagre fascicule with the misleading name "SGA 4<sup>1</sup>/<sub>2</sub>", a certain "Etat 0" of his thesis-sic (which had disappeared, body and all. . . ), exhumed especially for the occasion after a fourteen-year slumber! Nowhere in the volume, whether in the introduction where this text-rabiot (' "no longer available" - and for good reason!) is duly highlighted, or in the text itself, is there any allusion to any role I might have played in the ideas developed therein; nor, for that matter, to the fact that this text was one day destined to become a thesis. Neither Verdier nor Deligne saw fit to inform me of this publication (and with good reason, too), nor to send me a copy of the trompe-oeil volume. For details, I refer you to the note "Le compère" (n° 63)", written under the emotion of discovering this exhumation on the sly), and to the more in-depth reflection in the already oft-quoted note, "Thèse à crédit et assurance tous risques" (n° 81).

So, ten years after his unusual thesis defense, Verdier seized the opportunity offered by Deligne

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<sup>563</sup>(\*\*) See "Dotting the i's" (n° 164), part IV 1.

<sup>564</sup>(\*) I can imagine, moreover, that much stronger than the mathematical interest (although this article had nothing to teach to Deligne, whom he did not already know as a listener of SGA 5), must have been that of being able to take note first hand and in black and white, of the deceased master's smooth escamotage, following the tradition he had himself inaugurated eight years ago!

to take, in short, an "**option**" on an undisputed and undivided paternity of the "derived categories" point of view in homological algebra, with the full backing of his prestigious friend; and this at a time when both were still maintaining a de facto **boycott** on the use of this same point of view<sup>565</sup> (\*\*). This boycott, which weighed heavily on Zoghman Mebkhout's work, condemning him to complete solitude, remained in force until the "Colloque Pervers" in 1981.

□ And so, in 1977 Verdier emerged as the father-in-residence of a cohomology yoga that, for the time being, remained the object of a good-natured tacit disdain - but you never knew. ... Moreover, since the previous year, with the publication of "the right reference", he had been the father of part of the duality formalism developed by me (on the "discrete" homology and cohomology classes associated with cycles, the biduality formalism, constructibility version finiteness theorems etc.) - not to mention the duality of locally compact spaces, which also remained in an ambiguous, waiting status - just like the yoga of derived categories that gives it its meaning. p. 932

**Stage 4 (Colloque Pervers, June 1981).** This is by far the culmination of Verdier's participation in l'Enterrement. This Colloquium consecrates the shameless spoliation of Zoghman Mebkhout, pioneer of the unifying and fertile point of view of D-Modules in the cohomology of algebraic varieties. As official organizer of the Colloquium, along with B. Teissier, Verdier plays a leading role. I'll come back to this in the following note, with "Operation IV" (known as the "Pervers Colloquium" or "the unknown on duty"). Here, I shall confine myself to the direct repercussions for Verdier, in terms of the "sharing" of an inheritance (where the deceased bequeather remains carefully ignored. . . ).

This Colloquium marks the triumphant "re-entry" of derived and triangulated categories into the mathematical arena. As the "father" of these categories (which he had done everything in his power for fifteen years to bury), it is Verdier, after Deligne, who emerges as the main hero of the happening. This, at least, is the impression one gets from the Colloquium's main article, written by Deligne, which alone constitutes Volume I and the centerpiece of the Colloquium Proceedings<sup>566</sup> (\*). As luck would have it, it's the skeletal and providential

"Etat 0" of a thesis (which I would never have dreamed of accepting as a doctoral thesis, and which had come to bail out

the pirate text "SGA 4<sup>1</sup>" □ un peu maigre aux entournures) - here it becomes the brilliant piece à conviction, allowing the father-to-the-lark Verdier, in a cloud of references to "SGA 4<sup>1</sup>", to modestly swagger as the far-sighted precursor of the great rush known as "perverse beams" (which have nothing to do with it, though) and of a new and belated re-start of the cohomology of algebraic varieties (on the shores of a vague unknown whose name nobody dares to pronounce. . . ). p. 933

This same article (signed Beilinson-Bernstein-Deligne) also marks the return in force of the six-operations formalism (never named, of course) in the spread context, with the now concerned notations I had introduced in the fifties. As I wrote elsewhere<sup>567</sup> (\*) "there's not a page in the article quoted. . . that is not deeply rooted in my work and bears its mark, right down to the notations I had introduced, and the names used for the notions that come into play at every step - which are the names I had given them when I got to know them before they were named".

<sup>565</sup>(\*\*) As I explained in a previous b. de p. note (note on page ), in the text-compendium entitled "SGA 4<sup>1</sup>" Deligne was unable to avoid recourse to derived categories in the demonstration of "the" formula. This is undoubtedly what suggested to him the idea of expanding his volume with the "state 0" of a wrecked thesis. In fact, this did not alter the boycott on derived categories until 1981.

<sup>566</sup>(\*) Proceedings published in Astérisque n° 100 (1982) - under the title "Analyse et topologie sur les espaces singuliers". In fact, the Proceedings in question, dated 1982, were only completed in December 1983, and Mebkhout read them in January 1984.

<sup>567</sup>(\*) See the note "L'Iniquité" (n° 75), p. 288.

The formalism of étale duality, which I had developed eighteen years earlier, when my pupil Verdier was still learning the ABCs of cohomological language, has been renamed "Verdier duality" in the general euphorie<sup>568</sup> (\*\*). His prestigious patron was not going to skimp on the little, on such jubilant days! The name of the deceased does not appear in the article<sup>569</sup> (\*\*\*), nor in the introduction to the volume, signed by Teissier-Verdier. Nor that of the vague unknown (Zoghman Mebkhout, not to name him), without whom the article, and the whole brilliant Colloquium, would never have seen the light of day... ..

For the slaughter, it was slaughter! Apart from the motives, which would soon follow (from the following year), and perhaps the crystalline yoga, the uneventful sharing of the cohomological legacy of an unnamed deceased was now a done deal, and this **to** unanimous agreement and **general satisfaction**.

### 18.5.3.3. c.Jewels

p. 934

**Note 170(iii)** □(March 1) The three "operations" I reviewed in the previous notes concern the "sharing" of the "legacy" I left behind, in the form of my written and unwritten work on schematic cohomology. The direct "beneficiaries" of this sharing were three of my five cohomology students, namely Pierre Deligne, Jean-Louis Verdier, and Pierre Berthelot<sup>570</sup> (\*). But each of these three operations (like the one that follows) could only be carried out with the connivance (and sometimes the active support) of a large number of colleagues more or less "plugged in" to schema cohomology, among whom figure in first place my five cohomology students, including, in addition to those I have just named, Luc Illusie and Jean-Pierre Jouanolou(\*).

These three operations, and the fourth to be discussed, seem to me to be indissolubly linked, both in their deepest motivations and in their most tangible events. The first discrete signs date back to the years 1966 to 1968, but the most flagrant manifestations came after my "departure" in 1970. This departure, and a certain general state of morality in the mathematical "big world"<sup>571</sup> (\*\*), created the right external conditions for such a large-scale operation, undoubtedly the only one of its kind in the annals of our science.

This operation was aimed firstly at **discrediting** most of the **key ideas** I had introduced into mathematics<sup>572</sup> (\*\*\*), and burying the unifying **vision** in which they were embedded; then, to discredit or obscure the **role of the worker** in the creation of those of the tools I had fashioned under the dictation of these ideas and inspired by the overall vision, which served as the basic tools in the work of Deligne and my other cohomology students; and finally, in a final stage, to appropriate the authorship of these tools for myself.

<sup>568</sup>(\*\*) In the notation index, the dualizing functor (which I introduced in the stellar context in 1963, and which is the subject of Lecture I of the Allusie edition of SGA 5, where it has managed to survive) is called "Verdier duality". This name reappears throughout the text (e.g. on pages 62, 103 - looking at happiness-luck. . . ). I swear I'm not making this up!

<sup>569</sup>(\*\*\*) My name does appear in the bibliography, along with the acronym EGA (which will have to be replaced by an ad hoc text one of these days. . . ). Mebkhout's name is absent from both the text and the bibliography. There is no trace of it in the entire volume.

<sup>570</sup>(\*) (May 2) In fact, a fourth "beneficiary" should be added, whom I discovered only recently, namely Neantro Saavedra, mentioned in a previous b. de p. note (note (\*) page 921).

<sup>571</sup>(\*\*) (May 2) There must have been a two-way street: a certain state of degradation of mentalities (in which I myself had participated before my departure) encouraged the escalation of the plundering and debunking of my work by a group of my former students, whose growing cynicism surely contributed in turn to creating the more or less generalized state of corruption I see today.

<sup>572</sup>(\*\*\*) (May 2) for further details, see the note "My orphans" (n° 45) and above all "The building site tour - or tools and vision" (n° 178).

ideas and tools that have been successfully adopted by my students, or have come to the fore despite the □boycott p.

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that they had to bear<sup>573</sup> (\*).

This operation came to an end in 1982, with the publication of the volume Lecture Notes 900, consecrating the re-ap- parition of motifs in the mathematical public arena, in a narrowed form (compared to the vision that had emerged for me during the sixties) and under the paternity (implicit and obvious) of Deligne. It finally found its epilogue the following year, in the three-part "Funeral Eulogy" served up in the IHES jubilee booklet, published to mark the twenty-fifth anniversary of its existence.

The "mine" of these texts was first discovered on May 12 last year<sup>574</sup> (\*\*), in the note "L' Eloge Funèbre (1) - ou les compliments" (n° 104). It continues almost five months later in the note (n° 105) that follows it, "L' Eloge Funèbre (2) - ou la force et l'auréole"<sup>575</sup> (\*\*\*). I'll confine myself here to recalling in a few words the spirit and salt of this unusual "Eulogy".

The brochure presents (among other things) a "portrait gallery" of short topos on the various past and present professors of the institution celebrating its jubilee. In the text (by Deligne) dedicated to me, which is supposed to evoke a work of art, the word "cohomology" or "motif" is not mentioned. Nor is the word "schema", or any other that might suggest a theory I've developed or a theorem I've demonstrated that could perhaps have been useful. On the other hand, I'm generously saddled with superlatives and other niceties: "gigantic work... .", "twenty volumes... .", "greatest natural generality. ... "<sup>576</sup> (\*\*\*\*) "great □attention terminology. . .", "problems. ... in the line he tra- p .<sup>936</sup> . . became too difficult... ". It's burial with great fanfare and in the limelight, with a well-sent "compliment", enormous and plethoric like the deceased whose memory is being "honored", and at the same time with a finesse in comical insinuation, which was decidedly lacking in the clumsy ancestor... .

There's nothing to suggest that I had anything to do with "demonstrating" Weil's conjectures ("of proverbial difficulty"), duly highlighted in Deligne's topo. On the contrary, it is stressed that "this result seemed all the more surprising" as it had to be demonstrated, so to speak, against a "series of conjectures" of my own making (Grothendieck never makes any others!), which (he adds, to leave no doubt as to what is to be thought of them) "are as unapproachable today as they were then" (read: when I had the unfortunate idea of stating them...).

These two minute portraits, and a third part which completes them remarkably well (in a single lapidary sentence of three lines<sup>577</sup> (\*)), are real gems, no doubt unique in their genre too, among the eulogies deftly served in honor of a "deceased" (still not deceased in this case!). They are explored, with all the care they deserve, in the three consecutive notes already cited (n° s 104-106), and,

<sup>573</sup>(\*) (May 2) Among the ideas and tools that I had introduced, which were buried and which have come to the fore despite the boycott instituted by Deligne and my other cohomology students, I'd like to mention the following: derived categories, motives (admittedly a narrow version) and the yoga of Galois-Poincaré-Grothendieck categories (renamed "Tannakian" for the purposes of the Burial), the formalism of non-commutative cohomology around the notions of fields, sheaves and links (developed by Giraud after the initial ideas introduced by me from 1955 onwards).

<sup>574</sup>(\*\*) It was on the very same day that the shameless massacre of the original SGA 5 seminary had already been revealed to me, at the hands of Illusie and with the active support or eager connivance of all my cohomology students, under the tenderized eye of the "entire Congregation"... .

<sup>575</sup>(\*\*\*) For an unexpected extension of the Funeral Eulogy, see also the following note "The muscle and the gut (yang buries yin (1))" (n° 106), which at the same time opens the long reflection "The key to yin and yang".

<sup>576</sup>(\*\*\*\*) This Frenchman-petit-nègre is a truly impayable find, to evoke in a comical way (and mine de rien. . . ) the plethoric and gratuitous bombardment of a gigantic chatterbox. ...

<sup>577</sup>(\*) I discovered this third part in the course of reflection in the aforementioned note "L'Eloge Funèbre (2) - ou la force et l'auréole". - and it immediately strikes me as more significant than the other two combined! It's the one that inspired the name "La force et l'auréole" given to this note.

under the more penetrating light of the dynamics of the "reversal of yin and yang", in the note (a few weeks later) "Les obsèques du yin (yang enterre yin (4))" (n° 124).

## 18.5.4. (3) APOTHEOSIS ("Coefficients de De Rham et $D$ -Modules")

### 18.5.4.1. a. The ancestor

**Note** 171(i) (March 1 and May 2-8<sup>578</sup> (\*\*)) In each of these four partial "operations" that I have distinguished- my early burial, it's Deligne who visibly plays the role of conductor (or rather, Grand Officiant at the  $\square$ Obsèques), with the more or less active participation of my other four coho- students.

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mologists, and with the connivance of a considerably larger group of mathematicians, all of whom are well aware of the situation (which is obviously not to their displeasure. . . ). This "group of connivance" takes on impressive and almost unbelievable proportions in the fourth of partial operations, which I shall now review.

IV Operation "L'inconnu de service" (or "du Colloque Pervers").

It's the operation of **appropriating the work of Zoghman Mebkhout** - the only mathematician (to my knowledge) who took the risk, after my departure from the mathematical scene, of appearing as "Grothendieck's continuator".

This operation continued over a period of ten years, from 1975 to the present day. At the risk of repeating myself, I'll start by recalling the historical context.

In the second half of the 1950s, I had developed a form of "coherent duality" in the context of diagrams. These reflections, motivated by the desire to understand the meaning and exact scope of Serre's duality theorem in analytic geometry and especially in algebraic geometry<sup>579</sup> (\*), had a major impact on my work.

were pursued in near-complete solitude, having failed to interest anyone but myself<sup>580</sup> (\*\*). It was these reflections that  $\square$  lead me to gradually draw out the notion of the derived category,

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<sup>578</sup>(\*\*) (May 13) This and the following four notes originally formed a single note, "L'Apothéose" (n° 171), dated March 1. It also included the previous note "Les joyaux" (n° 170(iii)). It was taken up again and considerably expanded between May 2 and May 8, especially the mathematical part, and split into the four separate notes "L'ancêtre", "L'oeuvre. . . ", ". . . et l'aubaine", "Le jour de gloire" (n° s 171 (i) to (iv)), in addition to the note "Les joyaux" already mentioned. Added to this are the eight sub-notes (n° 171 (v) to (xii)) relating to the four notes in question, and the four sub-notes (n° 171<sub>1</sub> to 171<sub>4</sub>) from the month of April, recounting my friend Zoghman's strange misadventures with the "law of the middle", as he himself told me. It is these sixteen notes (n° s 171 (i) to (xii) and 171<sub>1</sub> to 171<sub>4</sub>) which now form the part "L'Apothéose" in "Les Quatre Opérations" (of which the aforementioned Apothéose is the fourth and - until further notice - the last).

<sup>579</sup>(\*) My first thoughts on duality were in the context of analytic spaces, and predate those of Serre. Using "evetisque" duality techniques and the Poincaré-Grothendieck lemma on the  $\delta^-$ -operation (which I had just proved), I proved that if  $X$  is a Stein variety, the  $H^i(X, \underline{Q}_X)$  (resp.  $H^i(X, \underline{\omega}_X)$ ) are Fréchet spaces.

nuclear, in perfect duality with the  $H^{n-i}_c(X, \omega)$  (resp. the  $H^{n-i}_c(X, \underline{\omega})$ ). At the time, I didn't think of applying the same method to the case of vector fibres (not having realized the very simple algebraic fact that the operator  $\delta^-$  being  $\underline{Q}_X$ -linear, extends to differentiable differential forms with values in a holomorphic vector fibre), nor to complex varieties other than Stein's (the only ones I was familiar with at the time). Serre's proof of his analytic duality theorem in the general case is practically the same as the one I had found in a particular case.

<sup>580</sup>(\*\*) Of course, the mathematician of all people in whom I would have expected an interest in my thoughts on coherent duality was Serre. He was interested, I seem to recall, in the generalization of his duality result to a coherent bundle  $F$  (not necessarily locally free) on  $X$  projective and smooth over a  $k$ -field, identifying the dual of  $H^i(X, F)$  with  $E_X^{n-i}(X; F, \underline{\omega})$ .

This gave intrinsic geometrical meaning to a "calculatory" FCC result (which had intrigued and inspired me, of course), in the case where  $X$  is projective space. But apart from this result, one of the first in my journey to discover duality, and still close to what was familiar to him, Serre always refused to listen, when I felt like talking to him about duality. I don't think I ever tried to talk to anyone else about it, apart from (much later) Hartshorne, who made a





whose objects were presented as natural "coefficients" in the homological and cohomological formalism of spaces and varieties of all kinds, forming part of a first embryo of a formalism of "six operations" on ringed spaces (while waiting for ringed topos). Four of these operations had already been more or less familiar to me since my 1955 work "Sur quelques points d'algèbres homologiques"<sup>581</sup> (\*), albeit in the language of derived categories.

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vantes (along with the point of view of derived categories), these are the "internal" operations  $\otimes$  and  $RHom$  ("total derived functor" version of the bundle formalism  $For_i$  and  $Ext^i$  introduced in "Tohoku"), and "external"  $Lf^*$  and  $Rf_*$  (inverse images, and direct "à la Leray"), forming two pairs of adjoint functors (or bifunctors). In the case where  $f$  is an "immersion" morphism  $i : X \rightarrow Y$ , we can add the pair of adjoint functors  $Ri_!$ ,  $Ri^!$ , embodying respectively the "extension by zero" and "bifunction" operations.

"local cohomology with supports in  $X$ ". The common thread in my reflections is to arrive at a **duality theorem** (global, at a time when there was no question of a local version. . . ), generalizing that proved by Serre for a locally free coherent bundle on a smooth projective variety over a body. ), generalizing the one proved by Serre for a locally free coherent bundle on a smooth projective variety over a body. The aim was to give a formulation that would apply to any coherent bundle (or complex of such), or even a quasi-coherent bundle, without any smoothness or projectivity assumption on  $X$  (keeping only cleanliness, which then seemed essential<sup>582</sup> (\*\*)). What's more, in analogy with my reflections on Theorem of Riemann-Roch, I felt that the right statement had to concern, not a variety over a body, but a proper morphism

$f : X \rightarrow Y$  of otherwise arbitrary schemes. It was by means of  $\square$  successful approximations, on p. 939 in the course of several years' work<sup>583</sup> (\*), that the global duality theorem is gradually being decanted from

At the same time, the notion of derived category also emerges from the limbo of the prescient to take concrete form, and give the formalism and the statements an **intrinsic meaning**, without which I would have felt incapable of working! It was first of all to arrive at a fully satisfying statement of global duality that I introduced the formalism of **dualistic complexes** and derived the **biduality theorem**, and that I discovered (under suitable Noetherian hypotheses) the existence of an injective, essentially canonical dualistic complex, which I call the "**residual complex**", and a theory of variance for it. An early formulation of the global duality theorem, which at one time seemed to me to be "the right one", was that the functor  $Rf_*$  commutated to dualistic functors on  $X$  and  $Y$  (for two dualistic complexes that "correspond" to each other). It was only later that I discovered that the theory of variance for dualistic complexes alone (via residual complexes) generalizes to a functor of an entirely new nature, the  $Rf$  functor<sup>1</sup> or "unusual inverse image", of local nature on  $X$ . From this point on, the duality theorem for the proper morphism  $f$  is definitively formulated: this new functor is a **right adjoint** of  $Rf_*$ , and thus forms part of a sequence of three adjoint functors

$$Lf^*, Rf_*, Rf^!.$$

To have a fully completed formalism, all that was missing was the description of an  $Rf$  functor<sup>1</sup>,

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Harvard seminar, published in 1966 ("Residues and duality" by R. Hartshorne, Lecture Notes in Mathematics, n° 20, Springer Verlag).

<sup>581</sup>(\*) In Tohoku Mathematical Journal, 9 (1957), p. 119-221.

<sup>582</sup>(\*\*) See b. de p. (\*) page 940, below.

<sup>583</sup>(\*) Needless to say, during these "several years of work", I had many more irons in the fire than just questions of coherent duality! I familiarized myself with the then-known foundations of algebraic geometry (with FAC de Serre's point of view as my main reference), with the problematic of Weil's conjectures, and with the formalism of intersection multiplicities learned in one of Serre's lectures, where he developed his idea of "alternating tor sums"). This was to trigger my interest in the formalism of  $K$ -theory and the Riemann-Roch-Grothendieck theorem in 1957, which was very close (in spirit) to my thoughts on duality.



functor already known when  $f$  is an immersion, reducing to  $Rf_*$  for  $f$  proper, and forming with  $Rf^!$  a pair of adjoint functors  $Rf_! , Rf^!$ . I don't remember being distressed in the 1950s by this imperfection of a formalism whose general scope, beyond schematic coherent duality or analytical' still eluded me<sup>584</sup> (\*).

This shortcoming only became fully apparent to me in 1963, when I discovered that, in the context of the just-arrived co-equal homology (with "discrete" coefficients), there existed a formalism analogous in every respect to the coherent formalism, with the addition, precisely, of a functor  $Rf_!$  ((of direct image with proper supports) defined for **any** separate morphism of finite type. In fact, I was guided step by step by the work I'd done in the coherent case years before (with no one else interested but myself), and in the space of a week or two, at the very least, I was able to establish the complete "six operations" formalism, based on the two key theorems of base change. This duality formalism is incomparably more sophisticated and powerful than the one previously available in the transcendental context, for topological varieties only (and local systems on them), and even more satisfactory than the formalism I had arrived at in coherent duality.

My work on coherent duality is set out in R. Hartshorne's well-known seminar "Residues and Duality" (published only in 1966)<sup>585</sup> (\*\*), those sur la dualité étale in one or two chapters of SGA

4, and especially in the SGA 5 seminar, which was entirely devoted to it. And it's only as I write these lines that I suddenly realize that, apart from a few sporadic precursor-texts (in the Cartan and Bourbaki seminars of the 1950s), there is no systematic **published** text: and from my pen, expounding the formalism and yoga of duality, either in the coherent context, or in the slack context. The SGA 4 lectures devoted to this theme, centered around the only "global duality theorem" for a separated morphism of finite type (establishing that  $Rf_! , Rf^!$  are adjoint), were written

<sup>584</sup>(\*) Of course, I had realized that already in the case of an open immersion  $f: X \hookrightarrow Y$ , where the functor  $Rf^!$  coincides with the "restriction to  $X$ " functor  $Lf^*$ , which (in the context of quasi-coherent bundles) admits **no** left adjoint. The usual left adjoint  $Rf_!$  ("extension by zero outside  $X$ ") does not preserve quasi-coherence. On the other hand, I had also verified that, apart from quasi-coherence hypotheses and even for a proper one-point base morphism, there is no "duality theorem". Thus, the impossibility of defining an  $Rf_!$  under general hypotheses seemed to me to be a given and in the nature of things.

It was Deligne who realized in 1965 or '66 (as soon as he arrived!) that it was possible to make sense of  $Rf_!$  and to recover the coherent duality theorem for a separate morphism of type fini not proper, provided we worked with coefficients that are (complexes of) quasicoherent **pro-beams**. However, this beautiful idea did not have the fortune one might have expected - nor did the initial formalism of coherent duality, which it allowed to perfect.

Deligne successfully took up this idea in his attempt to construct "De Rham coefficients" on algebraic schemes of zero characteristic, a promising attempt that he nonetheless jettisoned with profit and loss as soon as I left in 1970. Six years later, it was left to Mebkhout to find "the" right category of (crystalline) "De Rham coefficients" that I had been anticipating for ten years. ...

<sup>585</sup>(\*\*) The seminar in question (published in Lecture Notes in Mathematics, n° 20, Springer Verlag) sets out the essence of my ideas on coherent duality formalism, centred on the six-operation formalism, biduality, and a theory of "residual complexes" (which are canonical injective representatives of dualistic complexes). These ideas were taken up in the analytical framework by Verdier and, above all, by Ramis and Ruguet. The Hartshorne seminar does not, however, contain a number of more fine developments intimately linked to this formalism: a residue theory (for finite and flat schemes on any basis), and a cohomological theory of difference, which have never been published (as far as I know). In the '50s, I had also developed the formalism of the "determinant module" of perfect complexes, which was finally to be included in SGA 7 and whose editor (following the example already well established by certain "editors" of SGA 5) withdrew after two years.

Finally, I'd like to point out that in the wake of my reflections on coherent duality in the 1950s, I was led to introduce and develop the purely algebraic version of **Hodge's** and **De Rham's cohomology**, and in particular the formalism of cohomology classes associated with an algebraic cycle (initially assumed to be smooth), and a theory of Chern classes, modelled on the one I had developed in Chow theory.

by Deligne two or three years after the seminar, according to my handwritten notes<sup>586</sup> (\*). As for the SGA 5 seminar, it was practically sequestered for eleven years by my cohomology students, only to be published (after Deligne's 1977 saw-cut text), copiously plundered and unrecognizable, ransacked by the care of the "publisher"-sic Illusie, to the complete devotion of his prestigious friend<sup>587</sup> (\*\*). It is here, in this ruin of what was one of the most beautiful seminars I've developed and, along with SGA 4, the most crucial of all p.

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in my work as a geometer - this is the only trace written by my hand, or at least from notes written by my hand, that evokes in any way formalism and the yoga of spread duality, and, beyond this still partial yoga, and irresistibly suggested by it, that of the six operations. My students were careful to erase all traces of this last yoga<sup>588</sup> (\*), of exceptional suggestive force, which had inspired my work on cohomology throughout the sixties. It was really the "nerve" in the idea-force of the "coefficient types"<sup>589</sup> (\*\*), of which the yoga of patterns is the soul. ...

Such an aberrant situation, in which an important advance in a science, embodied in a new vision, was eradicated by the very people who had been its first beneficiaries and repositories, could not have arisen without this other situation, also highly exceptional, created by my sudden departure and the conditions surrounding it. Moreover, the turn events were to take had already been prepared before my departure and throughout the sixties by the divided situation in which I found myself, preoccupied on the one hand by interminable fundamental tasks that only I was able or willing to take on<sup>590</sup> (\*), and on the other hand constantly solicited by questions on themes often far removed from my own.

<sup>586</sup>(\*) Deligne's paper was written **after** the SGA 5 seminar. In fact, Deligne did not follow my notes to the letter, but a variant of my method, which Verdier had introduced in the context of locally compact spaces in 1965 (essentially using the étale model). At that time, there was no ambiguity in anyone's mind about the authorship of all the main ideas in duality, and a fortiori, about the authorship of the étale duality; it wouldn't have occurred to anyone (surely not even to Deligne!) that the fact of following a variance of my initial method could, over the following two decades, be used to fish in troubled waters, and attribute staggered duality to Verdier (while Deligne pockets the rest of the staggered cohomology "package"... ).

<sup>587</sup>(\*\*) On this subject, see the note "The four maneuvers" (n° 169 (ii)), and the sub-notes that follow.

<sup>588</sup>(\*) (May 8) I've just gone through my handwritten notes for the first three presentations of SGA 5, notes that Illusie has last year at my request. (He was the only one of the former editors who took the trouble to return the notes I had entrusted to them. . . ) The first talk consisted of a wide-ranging "tour d'horizon" of what had been accomplished in the previous SGA 4 seminar, with regard to stale cohomological formalism and its relations to various other contexts. The second presentation develops at length the "abstract" formalism of the six variances. There is an essentially complete form, but no effort yet to pin down compatibilities between canonical isomorphisms. (This was a task of a more technical nature, unnecessary at a time when my main concern was to "get across" this yoga of duality, the strength of which I could feel). Needless to say, there is no trace of either presentation in the Illusie edition. I'd come to believe that (preoccupied with the more technical aspects of the seminar) I'd probably omitted the unifying vision. In retrospect, and almost a year to the day after the discovery of the SGA 5 seminar "massacre", I seem to have put my finger on what was at the very **heart of** this operation-massacre. It's not the disappearance of one presentation or another, annexed by a Deligne, plundered by a Verdier, saved from disaster by Serre or torn from a harmonious "whole", for the sheer pleasure of it, as one might say, by an Illusie. But it is the very soul and nerve of this seminar, the constant and omnipresent guiding thread throughout this vast work done by one - it is this that Illusie set about eradicating from SGA 5 without leaving (almost) a trace. The very name "six operations" is absent from this seminar, just as it is absent from the work of my students, who have had to make a tacit pact not to utter these words except on the very rare occasions when one or other is still confronted with the worker.

declared deceased, to whom (however deceased he may be) it is nevertheless advisable to give the change. ...

<sup>589</sup>(\*\*) This key idea, too, was eradicated, then forgotten, by my cohomology students. It was one of the first ideas to come back to me, when I did my first retrospective on my work and its vicissitudes "fifteen years on", in the note "Mes orphelins" (n° 45). This note, whose name is more apt and profound than I would have dreamed at the time, was written even before the discovery of "L'Enterrement" (in the literal and strong sense of the word). The same key idea of the six

operations and "cohomological coefficients" recur here and there, almost as a leitmotif, when the reflections in Récoltes et semailles bring us back into contact with the fate of my work by those who were my students. See, in particular, the notes "La mélodie au tombeau - ou la suffisance" (developing the "melody", or the theme with variations, types of coefficients), and "Le tour des chantiers - ou outils et vision" (notes n° s 167, 178).

<sup>590</sup>(\*) I would remind you that this far-reaching groundwork began abruptly and continues to this day, from the very day of my departure. This is an eloquent sign of the "misunderstanding" I referred to in the note "Le magot" (n° 169 (v)). All

of the primary bases that absorbed me in the moment, and thus, very often, more intensely and directly fascinating<sup>591</sup> (\*\*). Rarely, among the very themes I had given myself the leisure to explore and develop (such as duality), did I also find the leisure to write up the results of my work in a form suitable for publication (in accordance with my own exacting criteria). This is how I often came to leave it to others (in whom I had complete confidence, of course) to write (as was the case for the "duality" theme, in both the coherent and discrete frameworks), or to develop certain initial ideas that I knew to be fruitful (such as the derived category, or crystalline cohomology, to name but a few of many). In a "normal" situation of a good faith responding to the confidence I had in addressing motivated students, learning from me their trade and a broad basis for their future work, everything was for the best'  $\square$  and for the most to the great benefit of all concerned, including the scientific community. But it's true that this unusual situation put considerable **power** in their hands (the idea never having occurred to me before last year . . . ), especially after my departure. From the moment I left (or even before . . . ), some of them were quick to abuse this power, to obscure the work and the vision, to undermine the craftsman, and to take advantage of the tools he had fashioned, which they thought they could use.

My coherent duality works have never been very popular, it seems to me<sup>592</sup> (\*). On the other hand, my work on flat duality attracts immediate attention. But I think it would be more accurate to say that what attracted attention was the fact that someone had "managed", however, to demonstrate in the stale context the analogue of Poincaré's duality, the one that had been well known to everyone for nearly a hundred years, in the familiar context of oriented topological varieties. This was therefore "a good point" for stale cohomology (there was little doubt that it was "the right one" for Weil's conjectures ("of proverbial difficulty". . . ). In other words, the mathematical public, on the lookout for the famous conjectures, reacted like a "consumer", reluctant to recognize and assimilate a new and profound vision of things, and retaining only a familiar-looking "result". More than twenty years on, I note that this powerful vision of the six operations and types of coefficients, expressed in a disconcertingly simple formalism, remains ignored by all (with the sole exception of the solitary worker), when it is not the subject (when someone dares to allude to it) of wry or ironic comments<sup>593</sup> (\*\*). Such scattered ingredients of my panoply are used here and there without reference to myself (and with ready-made spare fathers), and especially the biduality formalism, since the great rush on intersection cohomology, after the memorable Colloquium (in 1981) about to be discussed. But the  $\square$  vision, childlike in its simplicity and perfect elegance, which has nonetheless given eloquent proof of its power<sup>594</sup> (\*), remains ignored, the object of the

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the world was ready to bring in its furniture and settle down permanently in the houses I'd built - but there was no one left to stir and wield trowel and plumb bob to build and fit out, even if only under the peremptory pressure of need... ..

<sup>591</sup>(\*\*) If I'd listened to myself, how many times would I have left the interminable groundwork I had to do in the service of all, and embarked on the unknown adventure that was constantly calling me, the real one - instead of leaving to others the pleasure of surveying the new lands I'd discovered. Today, I see that these lands are still virgin, or very nearly so, and that those in whom I thought I saw pioneers, had already chosen to be comfortable rentiers before I left... ..

<sup>592</sup>(\*) As I pointed out in a previous note by b, de p., these works inspired those of Verdier, Ramis and Ruguet in the coherent theory of analytic spaces. It has always been clear (to me, at least) that the same formalism can only be found in the rigid-analytic context (which, too, is still in its infancy, from the echoes that come back to me). On the other hand, Mebkhout tells me that the Japanese school of analysis drew a great deal of inspiration from "Residues and Duality", refraining, incidentally, from ever naming the worker. These days, the opposite would have been surprising... ..

<sup>593</sup>(\*\*) For further details and comments, see the sub-note "Unnecessary details", n° 171 (v): in particular part (a), "Packages of a thousand pages. . . ".

<sup>594</sup>(\*) For details of these "eloquent proofs", see the sub-note "Useless details" (n° 171 (v)), part (b) "Machines for doing nothing. . . ".

disdain of those who prefer to scorn (and plunder. . . ), rather than understand.

If what I've done with my hands and my heart has been twenty or maybe fifty years ahead of its time, it's not because of the immaturity of the **mathematics** I found when I put my hand to the dough thirty years ago. It's the immaturity of men<sup>595</sup> (\*\*). And it was this same immaturity that confronted my posthumous pupil and sole continuator, Zoghman Mebkhout. I had had the great good fortune, before I left in 1970, to be confronted with it only in the form of incomprehension, which never departed from a disposition that remained friendly. Zoghman Mebkhout, who arrived on the mathematical scene at a different time from the one whose work he was recklessly continuing, was entitled, after the incomprehension and disdain, and when the tool value of **one** of his results was finally recognized, to the malice of his elders and to the full weight of the iniquity of an era - but I anticipate... .

One of the most important discoveries I've made in mathematics," and one that remains virtually unknown to everyone, was that of the **ubiquity** of the duality formalism I'd begun to develop in the 1950s: the "formalism of six variances and biduality" applies both to the "continuous" coefficients initially envisaged ("coherent" theory), and to the "discrete" coefficients. This ubiquity appeared, as a scarcely believable surprise, in the spring of 1963 - it was thanks to it, and to nothing else, that I was able to develop a formalism of staggered duality and achieve what I call the "mastery" of staggered cohomology. Even then, I was intrigued by the question of a theory that would be "common", whether in the schematic, complex analytic or even topological framework - a theory that would "cap" both types of coefficients. De Rham's cohomology (an old friend of mine. . . ) gave a first indication in this direction, suggesting to look for a "common principle" in the direction of "integrable connection modules" (or "stratified modules", perhaps. . . ). These give

to give rise to a "De Rham cohomology" (with discrete coefficients, morally speaking), which is then put into practice.

□ in connection with coherent cohomology. This approach later suggested to me the idea of "crystal" and of "crystalline cohomology", without yet (it seemed) being sufficient to provide the key to the description of a complete formalism of the six variances for types of "coefficients" which, in a suitable sense, would encompass both discrete ("constructible") coefficients, and continuous coefficients<sup>596</sup> (\*). It doesn't seem not that any of my students could sense this problem<sup>597</sup> (\*\*)' □ with the sole exception of Deligne. He devotes a

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<sup>595</sup>(\*\*) For some initial thoughts on this subject, see the sub-note "Freedom. . ." (n° 171(vii)).

<sup>596</sup>(\*) At the time of writing, my memory on this subject was still hazy. It has since been revived, and I come back to it in more detail in the sub-note "Wacky questions" (n° 171 (vi)).

<sup>597</sup>(\*\*) I had mentioned this problem to Verdier, after he had developed (as I had suggested) the theory of duality of the topological spaces (or at least, an embryonic theory), along the lines of the one I had developed in the étale context (see sub-notes n° s 81, 81 ). This must have been around the mid-sixties. Obviously it didn't "click" then - the very meaning of the question (a little vague perhaps, it's true) seems to have escaped him. Yet, surely I must have mention De Rham's cohomology, both differentiable and complex analytic, which brings together Serre's duality and Poincaré's duality, concerning both types of coefficient.

(May 14) In fact, as early as the 1950s, I knew that Serre's duality theorem could be generalized to the case of a complex of differential operators between locally free bundles on a clean and smooth relative scheme, so as to also encompass De Rham cohomology (i.e., morally, a cohomology with discrete coefficients). This is a duality result very close to Mebkhout's in the analytic framework, which will be discussed in the following note. I didn't pursue this line of thought at the time, mainly, I think, because I couldn't see how to make a suitable "derived category" with complexes of differential operators, in the absence of a good notion of "quasi-isomorphism". It's also true that the isolation in which I was working, on questions (coherent cohomology) that obviously didn't interest anyone else in the world but me, was hardly stimulating to pile a further generalization (with differential operators replacing linear morphisms) on top of those I'd already worked out in my own corner, over the previous years. I was, however, very close to Mebkhout's point of view, where the passage to the corresponding  $D$ -Modules (to the components of a complex of differential operators) gives a perfectly simple key to constructing the derived category we need. As early as 1966 (but without realizing it at the time), I had a dual point of view, which would have enabled me to make

a category derived from "stratified pro-Modules" (an idea later developed by Deligne, in his sketch of a theory of De Rham coefficients, which will be discussed shortly). Indeed, by associating with any coherent Module the pro-Module of

He spent a whole year at a seminar (at the IHES, in 1969/70, as I recall) developing a formalism that enabled him, at least for a finite-type scheme  $X$  over a field of zero characteristic  $k$ , to describe cohomology spaces (known as "de Rham" spaces) which, in the case where  $k = \mathbb{C}$ , give back the ordinary complex "Betti cohomology" (defined by transcendental means). The coefficients he worked with were "stratified promodules" and complexes of such promodules. It wasn't clear, however, whether these coefficients would fit into a formalism of the six operations<sup>598</sup> (\*), and Deligne gave up pursuing this path. As I recall, what was lacking above all(\*) to give confidence was a description in purely algebraic terms (using coherent or procoherent Modules and stratifications), valid therefore on any base field of zero characteristic, of the category of "algebraically constructible"  $\mathbb{C}$ -vector bundles on  $X$ <sup>599</sup> (\*\*), which is defined by transcendental means when the base field is the  $\mathbb{C}$ -field of complexes.

#### 18.5.4.2. b. The work...

**Note** 171(ii) Mebkhout's work, which began in 1972, is set in the transcendental (and technically more arduous) context of analytic spaces. It is in almost complete isolation that he

over the next few years becomes familiar with my work on cohomology and with the formalism of derived categories<sup>600</sup> (\*\*\*), left behind by those who were my students. □ A common thread, which

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The striking parallelism between continuous duality and discrete duality was gradually taking on a prominent role in his thinking. The latter had in the meantime taken on the name of "Poincaré-Verdier duality", without anyone in the wider world (and especially not the new "father" Verdier) even pretending to question the deeper reason for this parallelism. It's the reign of the "utilitarian", short-sighted point of view, content to use the ready-made tools I'd created, without asking any questions - and especially not such vague, not to say preposterous questions. The question isn't mentioned in any published text, not even (and I realize I'm to blame here. . . ) in those from my pen<sup>601</sup> (\*).

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a complex of such stratified promodules, whose crystalline hypercohomology is identified with the Zariskian hypercohomology of the differential operator complex under consideration. (See my lectures "Crystals and the De Rham Cohomology of schemes" (notes by I. Coates and O. Jussila, in Dix exposés sur la cohomologie des schémas (p. 306- 358), North Holland - especially par. 6). We can then define the notion of "quasi-isomorphism" for a (differential) morphism between complexes of differential operators, in the usual way, in terms of the associated complexes of stratified promodules.

<sup>598</sup>(\*) Here again, my memory was hazy, and there's an error - it was clear a priori here, for heuristic reasons of a transcendental nature, that there **must** be a formalism of the six operations. (For further details, see the sub-note ". . . and hindrance", n° 171(viii).) My error is obviously due to a deliberate (conscientious) attempt to rationalize, to make intelligible something that might have seemed inexplicable, namely Deligne's abandonment of a "safe" research direction rich in promise. The reason, after all, is by no means mathematical!

<sup>599</sup>(\*\*) I would remind you that this notion of constructibility was introduced by me, among many variants (algebraic, real analytic, etc.) as early as the 1950s, at a time when I was strictly alone in my interest in these matters. (See my comments of last year, in sub-note n° 46<sub>3</sub> ).

<sup>600</sup>(\*\*\*) (May 14) Mebkhout has since told me that those first readings of mathematical literature, around 1972, were works by Japanese authors of the Sato school. He had great difficulty, he tells me, getting his head around it; it all seemed terribly complicated. That's when he came across a reference to Hartshorne's book Residues and Duality, which was a real delight to read. It's true that this book is superbly written! The few introductory words I had written for this book, evoking the ubiquity of the formalism it develops, inspired him greatly. It was then that he began to familiarize himself with my work, which subsequently became his main source of inspiration. In all his works and presentations, he takes care to clearly indicate this source.

<sup>601</sup>(\*) (May 14) I remember, however, that during the SGA 5 seminar, I was constantly reminded of the ubiquity of the formalism I was developing, and I never missed an opportunity to point out possible variants in such and such other contexts,



The very formulation of common formalism makes essential use of derived categories. Mebkhout makes them his constant working tool, against the winds of fashion and the disdain of his elders, starting with the one who (we don't know whether willingly or reluctantly. . . ) is now the "father" of the said categories, namely Verdier. Compared to the arsenal I had introduced, Mebkhout's essential new ingredient is the microlocal analysis of Sato and his school. More precisely, Mebkhout borrows from them the notion of D-module on a smooth complex analytic variety (equivalent to the notion of "crystal of modules" that I had introduced around 1965-66, which retains a meaning in broader contexts, and in particular on singular varieties), and above all the notion of D-coherence and the delicate condition of holonomy on a coherent D-Module. In addition, he makes essential use of a 1975 theorem of Kashiwara, according to which the cohomology bundles of the complex of differential operators associated with a D-Module  $\square$  holonomous are p . 949 analytically constructible. This was a point of view and results that I was totally unaware of until Mebkhout told me about them two years ago, and Deligne must have been equally unaware of them in 1969/70, when he was thinking about a formalism for De Rham coefficients, which he never followed up on. It was **by putting the two currents of ideas together** that Mebkhout arrived at a common apprehension of the two types of coefficients on a smooth complex analytic variety  $X$ , in terms of complexes of differential operators, or (better and more precisely, in the more flexible language of D-Modules) in terms of complexes of D-Modules with coherent cohomology<sup>602</sup> (\*). This is his great contribution to contemporary mathematics.

More precisely, if  $X$  is a smooth complex analytic space, let us denote by  $\underline{Cris}^*_{coh}(X)$  the sub full category of the derived category  $D^*(X, D_X)$  formed by the complexes of  $D_X$ -Modules with D-cohesive cohomology, by  $\underline{Cons}^*(X, C)$  the full subcategory of the derived category  $D^*(X, C_X)$  formed by the complexes of  $C$ -vector bundles on  $X$  with analytically constructible cohomology, and finally by  $\underline{Coh}^*(X) = D^*_{coh}(X, \underline{O}_X)$  the full subcategory of the derived category  $D^*(X, \underline{O}_X)$ , formed by the complexes of  $\underline{O}_X$ -Modules with coherent cohomology. Mebkhout highlights fundamental functions

$$\begin{array}{ccc} \underline{Cons}^*_{coh}(X, \underline{O}_X) & & \underline{Coh}^*(X) \\ \downarrow M & \searrow N & \downarrow \\ & \underline{Cris}^*(X) & \end{array} \quad (Meb)$$

where the right functor  $N$  is the "tautological" functor, totally derived from the scalar extension functor by the obvious inclusion  $\underline{O}_X \rightarrow D_X$ . The left functor  $M$ , or "**Mebkhout functor**", is much deeper in nature<sup>603</sup> (\*\*). It is **fully faithful**, and its essential image is the full subcategory

by  $\underline{Cris}^*_{coh}$  complexes of  $D_X$ -Modules with bundles of not only coherent cohomology, but also "holonomic" and "regular". These are subtle local conditions, the first introduced

by the Sato school, the second defined ad-hoc  $\square$  by Mebkhout<sup>604</sup> (\*), drawing inspiration above all (he tells me) from my p . 950

comparison theorem between algebraic De Rham cohomology and analytic De Rham cohomology

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for the ideas and techniques I was developing within the framework of discrete cohomology. I find it hard to believe that I didn't mention the problem of synthesizing the two types of coefficients during the oral seminar, if only in the final presentation on open problems, which also disappeared from the massacre edition. Needless to say, there is no hint of such a problem in this edition, which has been carefully purged of anything that wouldn't fit in with the de rigueur label: "volume of technical digressions" . .

(May 19) See also the sub-note "Dead pages" (n° 171(xii)).

<sup>602</sup>(\*) For details of the language of  $D$ -Modules, its relationship to that of differential operator complexes and that of crystals, see sub-note "Five pictures ( $D$ -Modules and crystals)", n° 171 (ix), part (a).

<sup>603</sup>(\*\*) For an "explicit" description of a closely related functor  $M_\infty$ , in the context of  $D^\infty$ -Modules, see sub-note already quoted n° 171 (ix), part (b); "La formule du bon Dieu".

<sup>604</sup>(\*) The name "regular" is taken, of course, from the classical terminology for "regular critical points" of differential equations

of functions of a complex variable. If  $i : U \rightarrow X$  is the inclusion of the complementary  $U = X - Y$  of a

interest) are in fact "purely algebraic", making sense especially in the case where  $X$  is replaced by a finite-type scheme (smooth if you like, but it's not necessary) over a body of any zero characteristic.

The Mebkhout functor  $M$  (or "God's functor"<sup>605</sup> (\*\*)) is described as a quasi-inverse functor of the functor

$$m : \underline{Cris}^*(X)_{\text{hol.rég.}} \dashrightarrow \underline{Cons}^*(X, C),$$

defined by

$$m : F \dashrightarrow DR(F) = \overset{\text{dfn}}{RHom_D}(\underline{O}_X, F),$$

p. 951  $\square$  restriction of the functor (defined on  $\underline{Cris}_{coh}^*(X)$  as a whole) associating to each complex of  $D_X$ -Modules (with coherent cohomology) the associated complex of differential operators (or "De Rham complex")<sup>606</sup> (\*). Kashiwara's constructibility theorem implies that when  $F$  is holonomous (and a fortiori, when it is regular holonomous),  $DR(F)$  is indeed in  $\underline{Cons}^*(X, C)$ , which makes it possible to define the functor  $m$  - an obvious, childish definition, but one that nobody except Mebkhout (and up until the "big rush" five years later. . . ) had even thought of<sup>607</sup> (\*\*)! (To do so, we would have had to remember a

p. 952 certain yoga, that of the derived categories, which everyone by common consent had decided to bury, alongside the deceased who had introduced it among other bombast of the same style... .<sup>608</sup> (\*\*\*)'  $\square$  Moreover, the

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divisor  $Y$  in  $X$ , regularity in Mebkhout's sense (for a complex of  $D$ -Modules  $C$  on  $X$ ), "along  $Y$ " can be written as the canonical morphism

$$Ri^{\text{mér}}(C_U) \rightarrow Ri_*(C_U)$$

of the "meromorphic direct image" of the restriction  $C_U$  from  $C$  to  $U$ , to the ordinary direct image, induces a quasi-isomorphism for the associated De Rham complexes.

In the case where  $F_U$  can be reduced to a "local system", i.e. to an  $\underline{O}_U$ -cohesive bundle with integrable connection, this notion is equivalent to Deligne's notion. It too is obviously inspired by my comparison theorem (with the difference that Deligne is careful not to point this out, whereas Mebkhout is constantly careful to clearly indicate his sources). Mebkhout only became aware of Deligne's notion after introducing his own transcendental challenge. He had not previously sought a purely algebraic description of his condition. Deligne's work showed that in the particular case under consideration, Deligne's algebraic condition implied Mebkhout's, and Mebkhout verified that the converse is also true. This provides the key to a purely algebraic description of Mebkhout's regularity condition, for any complex of  $D$ -Modules with coherent and holonomic cohomology.

Mebkhout told me that the Japanese had a notion of "micro-differential system with regular singularities", which they used in a completely different spirit (for analytical, not geometrical, purposes). After the rush on the "God's Theorem", this was just one of many ways to muddy the waters and obscure Mebkhout's pioneering work. It would seem that the two notions are equivalent - and chances are, given the deliberate messiness of the subject, nobody has ever bothered to check. Mebkhout only ever worked with the notion of regularity as he introduced it in 1976 (and as it appears in his thesis, submitted two years later).

<sup>605</sup>(\*\*) For the origin and meaning of the name "théorème (ou foncteur) du bon Dieu", see the note "L'inconnu de service et le théorème du bon Dieu" (n° 68'), written before I knew of the mystification of the Colloque Pervers, or even of "L'Enterrement dans toute sa splendeur".

<sup>606</sup>(\*) On this subject, see the aforementioned note "Les cinq photos (cristaux et  $D$ -Modules)" n° 171 (ix), part (a), "L'album "coefficients de De Rham"".

<sup>607</sup>(\*\*) (May 7) The **two** functors  $m, M$ , establishing the equivalence of crucial categories in one direction and in the other, must be called **Mebkhout functors**, and similarly for the functors  $m_\infty, M_\infty$  relating to  $D^\infty$ -Modules. (For these, see the cited note "The five pictures" (n° 171 (ix), part (b).) By composing these functors with the natural dualizing functors, we can find two other pairs of functors quasi-inverses of each other,  $(\delta, \Delta)$  and  $(\delta_\infty, \Delta_\infty)$ , countervariant themselves, and more convenient in certain respects (cf. note cited above). These are the four "**Meckbhout contractors**".

<sup>608</sup>(\*\*\*) (May 7) More than once Mebkhout has been treated like a joker, who thinks that writing arrows between derived categories (we're asking you for a bit!) and  $RHom$  is doing maths... . He didn't let it shake him, any more than I did when I introduced (in 1955) the  $Ext^i$  global and local bundles of Modules (while waiting for  $RHom$  with or without underlining), which made everyone seasick and justified the most express reservations about me (at least

condition of **regularity**, beyond that of holonomy, was established by Mebkhout "to measure", precisely in such a way that it becomes reasonable to expect that the functor  $m$ , thus restricted, is fully faithful and even, an **equivalence of categories**. He arrived at this conviction as early as 1976. He eventually proved it, under a very similar form, at least<sup>609</sup> (\*), in his thesis in early 1978.

□ This is above all **the** great new theorem contributed by Mebkhout, representing the crowning of eight years of stubborn work, pursued in complete solitude. It contains, in a single lapidary statement, a whole range of profound results of increasing generality, patiently worked out and proved one by one, between 1972 and 1980. For some of the major milestones in this solitary voyage of discovery of a new "philosophy" in the cohomology of varieties, I refer to the sub-note "The three milestones - or innocence" (n° 171 (x)). In the present note, my main aim will be to describe in a few words the new panorama that presents itself, at the end of this first long stage in the labours of the solitary worker, Zoghman Mebkhout.

The crucial fact (clearly recognized by Mebkhout as early as 1976) is that the Cons category\*  $(X, C)$  (of "topological" nature) can be interpreted, thanks to the Mebkhout functor  $M$ , as a subcategory full Cris category\*  $_{coh}(X, C)$ , which makes sense in the context of "abs-" algebraic geometry. treats"; it can also be interpreted, "morally", as a kind of "derived category" formed with complexes of differential operators in the ordinary sense<sup>610</sup> (\*) The full sub-category in question, defined by

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until 1957, the year of Riemann-Roch-Grothendieck. ... ).

All this didn't stop Mebkhout from trusting his own flair, and following it wherever it led him. He set to work with his bare hands, no experience, no help from anyone. He was **sure** that the theorem he sensed had to be true - all the indications he had in his hands were consistent. With a little experience, it would even have been obvious that he already had everything in hand to prove it, with the now-standard means that the first of my students would apply in a jiffy. But reduced to his own resources, the theorem seemed vertiginously remote and inaccessible - he hardly dared hope that he'd ever prove it!

If he struggled to prove it, for almost two years, it was because he hadn't had the advantage, as my students had, of being supported by a benevolent elder, and of learning from me a certain standard technique for unscrewing constructible beams, combined with the resolution of singularities à la Hironaka. The statement he came up with is certainly a profound one, and the demonstration is also profound, but today of a standard nature. In retrospect, it appears that the difficulty he had to overcome was above all psychological, rather than technical: working against the grain, and entirely reduced to his lights alone... ..

<sup>609</sup>(\*) (May 5) In his thesis, Mebkhout states and proves the corresponding equivalence theorem for  $D^\infty$ -Modules, and gives a remarkable explicit expression of the quasi-inverse functor  $M$ . On this subject, see sub-note 171(ix) (part (b)), and also the sub-note "Ecllosion d'une vision - ou l'intrus" (n° 171<sub>1</sub>). By 1976, Mebkhout had come to the conviction that the two functors  $m$ ,  $m_\infty$  (thus also the scalar extension function  $i$ , discussed in the last quoted sub-note) are equivalences, and to the explicit form of the quasi-inverse functor of  $m_\infty$ . The result that figures in his thesis, concerning  $m_\infty$ , is from 1978. By this time, he had all the ingredients for the demonstration (analogous, but with difficulty additional techniques) in the case of  $m$ .

Given the general indifference that greeted his thesis, passed in February 1979, he made no effort to write a formal demonstration for the case of  $m$  as well. The ingredients are the same as for  $m_\infty$ , and are inspired by the proof of my comparison theorem for the De Rham cohomology of complex algebraic varieties (of which he had taken knowledge in 1975), and SGA 5's unscrewing techniques (which he learned from Verdier's "good reference", while the SGA 5 seminar continued to be carefully sequestered in the care of my dear cohomology students). It wasn't until the end of 1980, given the importance of his ideas for proving the Kazhdan-Lusztig conjecture, that he took the trouble to write a circumstantial demonstration in the case of  $m$  (where a quasi-inverse functor was not available in advance). This demonstration is published in "Une autre équivalence de catégories", *Compositio Mathematica* 51 (1984), pp. 63-88 (manuscript received 10.6.81).

I would like to point out that between 1975 and 1980 (apart from a few lines by Kashiwara in 1980, which will be discussed in the sub-note "La maffia" n° 171<sub>2</sub>), nowhere in the literature, apart from Zoghman Mebkhout's work alone, is there any mention of the  $m$  or  $m_\infty$  functor or of a duality "philosophy", relating discrete coefficients precisely.

analytically constructible, and regular holonomous  $D$ -complexes, or holonomous  $D$ -complexes<sup>∞</sup>.

As we shall see, when the importance of this relationship is recognized, with "Kazhdan-Lusztig" and the rush to cohomology d'intersection (under Deligne's leadership), Zoghman Mebkhout's name is eliminated without fanfare, by a hushed, smiling and discreet agreement, with implacable efficiency. ...

<sup>610</sup>(\*) For the precise relationship between the two points of view, I refer you to the much-quoted sub-note "The five photos" (no.° 171(ix)), part (a).



This is the category of "De Rham coefficients" that I had already envisaged in the sixties, and which was still missing from my panoply of zero characteristic coefficients, to complete and link together, as if in a single large fan, the "*l*-adic coefficients" that I had identified in 1963; it is also the category that Deligne had tried to grasp at the end of the sixties, without succeeding (it seemed) in a way that satisfied him. This category will obviously have an essential role to play in algebraic geometry (and in particular in the description of the category of patterns on a basic scheme  $X$ . . .). The obvious name for this category, for me at least, is

p. 954 the "**De Rham - Mebkhout coefficient category**"<sup>611</sup> (\*\*), denoted by  $DRM^*(X)$  (or  $Meb^*(X)$ ), or  $DRM^*(X/k)$  (or  $Meb^*(X/k)$ )  $\square$  in the schematic framework, when  $X$  is a finite-type scheme over a body  $k$  with zero characteristic<sup>612</sup> (\*).

It is via the functor diagram (Meb) above, which summarizes Mebkhout's philosophy (dating back to 1976, and established by him over the following years), that the **coherent crystalline coefficients** (i.e. the ob-  
Cris-jets\*<sub>coh</sub>( $X$ )) can be viewed as a "common generalization" of the "discrete" coefficients (constructible) and "continuous" (coherent). The category formed by the former is in any case identified, by the Mebkhout functor  $M$  (a functor of deep nature), with the **full subcategory** of the coherent crystal-line category formed by the De Rham-Mebkhout coefficients. The situation is not so good for the tautological functor  $N$ , which has nothing fully faithful about it. But to console us and to complete the picture, we can add that in each of the categories in question, we have a natural **dualistic functor**, giving rise to a biduality theorem ("trivial" for  $Q_X$ -Modules and  $D_X$ -Modules, and using all the force of resolution of Hironaka singularities in the case of constructible  $C$ -vector bundles), on the model that I had identified in the coherent (commutative) framework first, then in the discrete-spread framework (in

p. 955 1963)<sup>613</sup> (\*\*). That said, the two functors  $\square M$  and  $N$  are compatible with natural dualistic functors<sup>614</sup> (\*).

<sup>611</sup>(\*\*) The general lack of understanding of the crucial role and significance of this category is already evident in the fact that it has still not been given a name or a lapidary notation. Instead (in the texts I've looked at), the authors confine themselves to vague references to "regular holonomic differential systems" (well fi n who's going to get it right!), of "construction" or "correspondence" or "relation" (supposedly well known) between these and (E-constructible) beams - and always, needless to say, rigorously ignoring the one who was the lone craftsman, setting in motion all this hype around the new cream pie of the beau monde: " $D$ -Modules".

<sup>612</sup>(\*) In the algebraic case, in addition to the local "regularity" condition, an "infi ni" regularity condition must be imposed. coefficients (in the case of a non-clean variety) to find the "right" De Rham - Mebkhout coefficients, which will correspond, in the case where the base field is the complex field, to  $C$ -vector complexes on  $X_{an}$  with **algebraically** (and not only analytically) constructible cohomology bundles It's for these coefficients too that we have a "theorem of comparison", generalizing my result on De Rham's cohomology, namely that the "total crystalline cohomology"  $RG_{cris}$ , taken from the algebraic (Zariskierian) point of view or in the transcendental sense, is "the same". This statement in turn must be seen as a special case of a more complete statement, namely that the "six operations" from the algebraic point of view are "compatible" with the six operations from the transcendental point of view.

If my students hadn't been so busy burying the master's work, it would have been in the very early seventies (if not the sixties. . .) that they would have come up with the coefficient theory that was needed, in all its simplicity and power. . .

<sup>613</sup>(\*\*) (May 5) The extension of my results on biduality, and on the stability of construc- tibility by the RHom operation, from the étale to the analytic context, is automatic and was known to me as early as 1963. Verdier had been working with me for three years at the time, immersing himself in the yoga of derived categories (whose systematic theory he had taken on) and coherent duality. It was from me that he learned the techniques for extending the coherent duality formalism to the case of discrete coefficients. As we have seen, he appropriated the yoga of duality and biduality, in the complex analytic context, in "the right reference" thirteen years later (in 1976), with the connivance of Deligne and my other cohomology students, all well aware of the situation.

In the mass-murder edition of SGA 5 the following year (1977), Illusie retained (in Lecture I) the biduality theorem, so that for a reader of both texts, Verdier's deception is obvious - but apparently it was taken for granted by everyone (given the times. . .). On the other hand, Illusie has refrained from including the RHom stability result for constructability, which I had of course given even **before** stating and proving the biduality theorem, on which my demonstration (copied by Verdier) in no way depends. So (it has to be done!) Illusie merely establishes the stability in question when the second argument is the dualising complex ! ! This was a way of covering up for his friend

Moreover, if  $F, F'$  are crystalline coefficients in duality on  $X$ , Mebkhout proves that the complexes of C-vector "crystalline cohomology" of  $F$  and  $F'$  on  $X$ <sup>615</sup> (\*\*)

□

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$$R\Gamma_{cris}(F)!, R\Gamma_{cris}(F)'$$

as complexes of topological vector spaces, are "in duality" by a natural coupling, in other words we have a coupling **that is a duality** (of EVT)

$$H_{cris}^i(X, F) \times H_{screams}^{-i}(X, F') \rightarrow \mathbb{C}$$

(for any integer  $i$ ). This duality theorem "caps" the ("absolute") duality known in the case of discrete coefficients (which Mebkhout calls "Poincaré-Verdier duality"), and in the case of coherent coefficients (which Mebkhout calls "Serre duality"), into a duality which I would call "Mebkhout duality", and which he called "Poincaré-Serre-Verdier duality"<sup>616</sup> (\*).

Verdier's article is copied from my SGA 5 lectures from beginning to end (with the exception of the three pages mentioned above). The best part is that the stability in question is already an immediate corollary of the biduality formalism (which does not prevent it from being mathematically zany to pretend to establish the stability of constructibility by  $RHom(F, G)$  only when  $G$  is the dualizing complex). But the complacent Illusie refrains from mentioning this corollary in his presentation, so as to keep up the appearance that the stability result that appears in his friend's "La bonne référence" is indeed of his own making.

One wonders why, under these conditions, Illusie kept the biduality theorem - butchering for the sake of butchering, he wasn't quite there yet! But if he had emptied it, he would have been obliged to empty Lefschetz-Verdier's eternal formula (which makes essential use of it) - that is, the "head of the Trojan horse": the formula whose supposedly crucial role in SGA 5 was to justify his other friend's impudent "coup de scie" operation, shattering the unity of my work on étale cohomology.

Congratulations to my ex-student Illusie, the clever "editor"-fossoyeur... .

<sup>614</sup>(\*) For the tautological functor  $N$ , this compatibility is itself tautological. On the other hand, for the Mebkhout functor  $M$  (or, what amounts to the same thing, for its quasi-inverse  $m = (G \mapsto DR(G) = RHom_D(Q_X, G))$ ), this is a profound result, proved by Mebkhout in 1976 (under the name "local duality theorem"), together with the global duality theorem for the  $D$ -Modules, to be discussed shortly. Nevertheless, "everyone" now takes this result for granted, and above all (even more self-evidently) without ever hinting at some vague unknown. ...

<sup>615</sup>(\*\*) I remind you (cf. "The five photos", n° 171(ix)) that the crystalline ("absolute") cohomology of  $F$  on  $X$  is defined as follows

$$R\Gamma_{cris}(F) \stackrel{\text{dfn}}{=} RHom_D(Q_X, F)' \quad R\Gamma(RHom_D(Q_X, F)) = R\Gamma(DR(F)).$$

On the other hand, the index ! designates the cohomology (crystalline in this case) with its own supports, i.e..

$$R\Gamma_!(F) \stackrel{\text{dfn}}{=} R\Gamma_!(RHom_D(Q_X, F)).$$

<sup>616</sup>(\*) As I have already said elsewhere (in the note "Le compère", n° 63"), Mebkhout "could do no less" than tip his hat to his "benefactor" Verdier (since the latter had communicated to him the providential "good reference"), everywhere.

when he had the chance. Yet **none** of the essential ideas for either duality (and even less, if you like, for the one that caps them all) are due to Verdier. In fact, apart from Poincaré's and Serre's duality theorems in their original form, which of course served as my starting points, all the essential ideas are contained in the formalism of the six variances and biduality that I introduced and developed at length in both contexts, coherently and discretely, in solitude.

It was with this in mind that I wrote last year, in the note "La victime - ou les deux silences" (n° 78') that Mebkhout's "protectors" "had kindly allowed him to carry with his hands a small corner of the coffin bearing my remains". It would have been right for

I'd also like to point out at this point that Zoghman had the courage, even though he could feel the wind blowing in the beautiful world, to state clearly in each of his articles that he was inspired by my ideas, instead of doing as everyone else did and plundering the deceased while passing over him in silence (in writing), and displaying an air of condescension (in words).

As for the name "Serre duality", which has come to be given to the theory of coherent duality I had been developing for years in total solitude, it has all the more salt (and Serre, who wasn't asking for so much, will appreciate it better than anyone!), as Serre had shown a total lack of interest in my duality work, thus depriving me of the only interlocutor I could have hoped to have for my cogitations! I think I can safely say that this disinterest has remained intact to this very day, including with regard to the notion of derived category (and other useless details. . . ).

As I see it, these are the first steps in a vast duality program, including (among other things (171(xi))) the development of a six-operation (and biduality) formalism for De Rham - Mebkhout coefficients on finite-type schemes over a characteristic body.

p. 957 null (while waiting for better). Given the conditions of isolation and the atmosphere of indifference in which Mebkhout had to work, it was out of the question for him to develop a complete formalism, such as the one I had developed.

in the two contexts from which he drew his inspiration (171(xii)). Among the main results he produced and proved over the eight years 1972-1980 (171(x)), the one that strikes me as the most important from the point of view of my program of the sixties is, of course, the one that highlights **the** correct category of crystalline coefficients, known as "de Rham - Mebkhout". As it happens, it was this result, too, that, from October 1980 onwards, enjoyed the most brilliant, even astounding, fortune, even though it was appropriated (like *l*-adic cohomology, or the crystalline cream pie of car. *p*) as a **tool** only, torn from a vision that gave it all its meaning and strength.

p. 958 Even more than for Mebkhout's other results, and just as in my work developing the biduality and six operations formalism, the language of derived categories is essential here to tease out the simple yet profound relationship between discrete coefficients and coherent coefficients<sup>617</sup> (\*), described in the *théorème du bon Dieu* (aka Mebkhout the never-named. . . ). Thus, it was almost twenty years after the creation of the cohomologie étale tool (which everyone today uses as a matter of course, while treating by the the vision that gave rise to it. . . ), and thanks to this (now "pie-in-the-sky") result by an obscure posthumous pupil, that the language of derived categories will suddenly find itself rehabilitated (as if it had never been buried. . . ), in the limelight and to the ovations of the crowd, who have come to acclaim yesterday's buriers playing (modestly) the new fathers. But then again, I anticipate...

### 18.5.4.3. c.... and the windfall

**Note** 171(iii) Verdier was more or less the "thesis boss" of Mebkhout, whose work over the past seven years had been carried out in complete solitude. At no time did he show any interest in the work of this young man, who was clearly as stubborn as he was stubborn - a vague, retarded Grothendieckian who is treated with the height of his greatness. In the four years since our first meeting in 1975, he has granted a total of three "interviews" to this out-of-nowhere fellow. None of my other cohomology students

<sup>617</sup>(\*) (May 7) Precisely, to a holonomic  $D$ -Module (complex reduced to degree zero) the good-god functor associated in general a constructible complex of  $\mathbb{C}$ -vectorials which will have more than one non-zero cohomology bundle, and vice versa. The simplest and most striking example is where we take a divisor  $Y$  on  $X$ , hence an inclusion  $i := X \setminus Y \hookrightarrow X$ , and the sub-bundle of  $i^*(Q_U)$  formed of the meromorphic functions along  $Y$ . It is a profound result of Mebkhout, obtained as early as 1976 (and later absorbed into the Good God Theorem) that this is a regular holonomic  $D$ -Module (nobody before Mebkhout had ever even considered looking at this bundle as a  $D$ -Module, and suspecting that it was even coherent. ). His transformed by the Good Lord functor is  $Ri^*(C_U)$ , which has non-zero cohomology bundles in dimension 0 and 1 at least.

This is an aspect of Mebkhout's philosophy that was absent from Deligne's approach, who obtained a dictionary between constructible  $\mathbb{C}$ -vector bundles and certain stratified  $Coh(Q_X)$  proobjects (the category of coherent Modules on  $Q_X$ ), without having to move on to complexes and derived categories. (He did, however, take care to intervene in these, at a time when I was still around and it never occurred to anyone that we'd one day bury the said categories. ). This (at least at first glance) is an advantage of Deligne's approach, which is closer to intuition. geometric directness of discrete coefficients - but it's also a sign, no doubt, that his approach is less profound. I tend to believe that it will still have a role to play, though, but no doubt in "tandem" with Mebkhout's point of view, which (I presume) is somewhat dual.

(May 24) For details, see the sub-note "The five photos (crystals and  $D$ -Modules)" (n° 171(ix)), part (c), in particular pp. 1009 ff.



nor do they deign to take an interest in the work of the aforementioned quidam. Its significance for their own research escapes them completely (although it's obvious, even to a old-timer like me who's been "out of it" for fifteen years... .). They're far too entrenched in their trip-burial, and in a dull, crank-driven routine, to be able to apprehend anything new that presents itself without a calling card and without pretense, with the sheer force of things that are all too simple and all too obvious. They long ago buried their own creative faculties, confining themselves to being consumers of fashionable brand-name products. Later, however, they will largely take their revenge on the intruder who took the liberty of seeing what had eluded them and everyone else (even though they had everything they needed, like him and beyond, to see and do. . .). But then again, I'm anticipating... .

The defense took place on February 15, 1979, to general indifference. Mebkhout sent his thesis to all the mathematicians he could think of, rightly or wrongly, who were interested in the cohomology of analytic or algebraic varieties - starting, of course, with all my students. Of all those who received a copy of his thesis, **not a single one** even acknowledged receipt, or sent a word of thanks. It's true that Mebkhout's thesis, even more so (it seems to me) than some of his articles, feels the conditions of adversity that had surrounded it - it seemed to me to be thick and not easy to access, to say the least, and those who weren't in the loop had excuses for not hooking up right away. <sup>By□contrary</sup>, I found p. 959

Mebkhout's oral explanations of his philosophy were perfectly clear and immediately convincing, and there's no reason why those he gave to Verdier (1976), Berthelot (1978), Illusie (1978) and Deligne (1979) should be any less so than those I received.

It was at the Bourbaki seminar in June 1979 that Deligne learned from Mebkhout of the "**Riemann-Hilbert correspondence**" that appeared in the unread thesis (this was the name given by Mebkhout to the category equivalence (or "dictionaries") referred to earlier). Apparently, over the past four years, Verdier had never even thought of saying a word to Deligne about his obscure pupil's work, which clearly escaped his notice until around the time of the Colloque Pervers in 1981 (when Deligne had to take it upon himself to explain what it was all about. . .). For Deligne, on the other hand, it was bound to "click" immediately - it was **the** solution, complete and lapidary, to the problem he himself had left to fend for himself ten years earlier!

The reflex that would seem to go without saying in such a situation (so much so, in fact, that I'm still struggling to imagine how anyone could act differently. . .), is to immediately congratulate the young stranger on having finally found the answer to a question that, I'm sure, is quite profound, that we'd been working on for a whole year, and which we'd finally written off. Times have changed... Deligne, always affable of course, confined himself to a vague compliment (and yet, it warmed the heart of the candid Zoghman, who was not spoiled and had no idea of what awaited him): yes, he had received his thesis and had even read the introduction, and he had found it to be "beautiful mathematics". For Zoghman, it was a great day! It was surely the first time (and the last too...) that he had received a compliment from such a great man, whom everyone knows and quotes.... <sup>618(\*)</sup> I can't tell you what's going on in the mind of Deligne, at that time and in the year that followed, concerning this remarkable theorem he had just learned from the mouth of a stranger. I presume he must talk about it around □de him <sup>619(\*)</sup> - still he com- p. 960

<sup>618(\*)</sup> (May 14) This was the first and only time Mebkhout had the honor of a conversation with Deligne.

(June 7) For another compliment, from the previous year (June 1978) and from Illusie's mouth this time, see the note "Carte blanche pour le pillage - ou les Hautes Oeuvres" (n° 171<sub>4</sub>), especially page 1091.

<sup>619(\*)</sup> (May 14) On reflection, and from what I know about Deligne, I doubt he really "talked about it", before doing so with a clear idea and a well-defined plan. See the note "La valse des pères" (n° 1764) about Deligne's very special play, and the role he had the two straw-fathers Beilinson and Bernstein play (see also "Marché de dupes - ou le théâtre de marionnettes", note n° 172<sub>2</sub> (e)).

munique in October the following year<sup>620</sup> (\*\*) to the Soviet mathematicians Beilinson and Bernstein, who surely guessed that they would have use for it. That same year, in fact, it was this "correspondence" (always referred to as the "Riemann-Hilbert" correspondence when one deigns to name it, and without Mebkhout's name ever being mentioned) that was the essential ingredient, the **new fact** that had been missing until then, for the demonstration of a famous conjecture<sup>621</sup> (\*\*\*) of which I know little more than the name, the "Kazhdan-Lusztig conjecture". At the same time, this was the kick-off to a sudden and spectacular revival in the cohomology of algebraic varieties, finally emerging from a long stagnation of more than ten years (if we set aside Deligne's work on Weil's conjectures). This unexpected revival took shape the following year, with the "happening" of the Colloque de Luminy in June 1981, on the theme "Analysis and topology on singular spaces"<sup>622</sup> (\*\*\*\*).

#### 18.5.4.4. d. The day of glory

**Note** 171(iv) On the subject of this "memorable Colloquium", I refer the reader to the note "L' Iniquité - ou le sens d'un retour" (n° 75), and to the following notes, still written in the heat and amazement (the word is not too strong) of the discovery. These notes form Cortège VII de l'Enterrement, which I have named "Le Colloque" ("The Colloquy").

- or Mebkhout and Perversity bundles".

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Suffice it to say that in the Introduction to the Colloquium Proceedings, signed by Bernard **Teissier** and Jean-Louis **Verdier**, the famous "Riemann-Hilbert correspondence" is presented as the "Deus  $\square$  ex machina" of the Colloquium. The same is true of the main paper, which (along with the Introduction) forms Volume I of the Proceedings, signed by **A.A. Beilinson, J. Bernstein and P. Deligne** (and in fact written and presented at the Colloquium by the latter, in the absence of the other two co-authors). Moreover, the first two authors named had been informed directly by Mebkhout (and independently of Deligne) of the ins and outs of his theorem, as early as the previous year (November 1980) - Mebkhout had even travelled to Moscow on purpose for this purpose<sup>623</sup> (\*). Teissier had also known first-hand for a long time - not to mention Verdier, who had chaired Mebkhout's thesis jury... . Finally, I'd like to add that it had been decided "in extremis" to ask Mebkhout to give a talk on D-Module theory (which, apart from himself, none of the people there knew much about), so Mebkhout had the opportunity to inform the entire Colloquium<sup>624</sup> (\*\*) about the theorem he had modestly called Riemann's and

<sup>620</sup>(\*\*) (May 14) This appears from a letter from Deligne to Mebkhout (received October 10, 1980). For details of the Kazhdan-Lusztig episode, see sub-note "La maffi a" (n° 171<sub>2</sub>), part (d), "La Répétition Générale".

<sup>621</sup>(\*\*\*) The same conjecture is demonstrated, independently and nevertheless with a remarkable set, at the same time (at For further details, see the sub-note already quoted "La maffi a" (n° 171<sub>2</sub>) parts (c) and (d).

<sup>622</sup>(\*\*\*\*) The Colloquium Proceedings were published in Asterisk n° 100 (1982). These proceedings were not printed until December 1983, and appeared in January 1984, almost two years after the date marked on the volume.

<sup>623</sup>(\*) On this instructive episode, see the sub-note "La maffi a" (n° 171<sub>2</sub>), part (d) "La Répétition Générale (avant Apothéose)".

<sup>624</sup>(\*\*) (May 14) About the participants in this strange Colloquium, very much a "festival of Grothendieckian maths", but with absolute silence on the late ancestor himself, as well as on the obscure posthumous pupil "who had the gift. . . of bringing all these fine people together". . . Deligne and Verdier were the only "pre-1970" students taking part in the Colloque, but they were enough to take center stage. Strangely enough, Berthelot and Illusie (whose work was particularly marked, I might add, by the absence of Mebkhout's point of view, exhumed there with great fanfare) were not part of the festivities. On the other hand, Contou-Carrère ("later" pupil) has wandered in, quite happy to have been invited to recount his method of solving Schubert cycles.

I remember that he came back euphoric, fully identified with all those brilliant and famous people with whom he felt at home, and who had come to listen to him, obviously interested, but yes! He put on a contrite face to tell me about Mebkhout,

Hilbert, without leaving the slightest ambiguity (as one might imagine) about the authorship of this result, which had the gift (unexpected for him as for everyone else) of bringing everyone together.

□ In fact, the reader would be hard-pressed to find any trace of Mebkhout's presentation in the Colloquium Proceedings.

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Verdier kindly explained to him afterwards that only articles presenting **new** results would be included in the Proceedings, whereas those in his thesis were already two years old and more. Readers will also be hard-pressed to find a single bibliographical reference in the Proceedings, or the slightest indication of the origin of this famous theorem, which is not due to Riemann or Hilbert. It's also hard to find the name of Zoghman Mebkhout. This name does not appear in the first volume, either in the text or in the bibliography. In the second, it appears twice in the bibliography, in references-"thumb!" (we can't say we didn't quote him!) from the pens of Brylinski and Malgrange - references that have nothing to do with the theorem of the good God - alias Riemann- Hilbert - alias Deligne (and especially not of Mebkhout)<sup>625</sup> (\*).

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who had opened up to him with bitterness, but he couldn't really say why - for Contou, at any rate, life was obviously good!

That was in June 1981. Four months later, (in response to his single candidacy for a post in Perpignan) he received a slap in the face, which he took as a humiliation and an affront. (For this episode, see the note "Cercueil 3

- ou les jacobiniennes un peu trop relatives" n° 95, especially pp. 404-406. This note was written without my having yet made the connection with the episode of Contou-Carrère's participation in the brilliant Colloque).

<sup>625</sup>(\*) (May 14 and 26) Apart from the participants already named, I was informed by name of the participation of **Brylinski, Malgrange** and **Laumon**. All three were fully aware of Mebkhout's work, and he had had the opportunity to inform each of them in detail, even outside the lecture he had given at the Colloquium. This did not prevent Brylinski and Malgrange, in their article published in the Proceedings, which makes essential use of Mebkhout's ideas and the bon Dieu theorem, from glossing over both the crucial role played by the emergence of these new ideas and new tools, and the name of their author.

As for Laumon, he made up for it later, in an article in collaboration with Katz. The same N. Katz had already distinguished himself in 1973 with "Operation APG 7", mentioned in the note "Episodes of an escalation" (n° 169 (iii), episode 2).

Mebkhout had already informed him of the results of this operation in 1979 (see the note "Carte blanche pour le pillage", n° 171<sub>4</sub>). The article in question is entitled "Fourier transforms and exponential sum augmentations" (also Laumon's doctoral thesis), which has been circulating in preprint form for the past two years (I even received a

copy by Laumon). These authors developed a Fourier transformation for *l-adic* coefficients, along the lines of that introduced by Malgrange in 1982 in the case of *D*-Modules (in the wake of the work of the vaguely unknown, and without mentioning his name, of course). Mebkhout's work represents the heuristic foundation of the theory developed by Malgrange as well as that of Laumon-Katz, in the same way as it did for the aforementioned article by Beilinson - Bernstein - Deligne (on what they called, That said, Laumon and Katz are also following the general trend (no mention of the service unknown in either the article or the bibliography - nor, of course, any mention of the ancestor. . . ), following in the footsteps of Deligne, Verdier, Berthelot, Illusie, Teissier, Malgrange, Brylinski, Kashiwara, Beilinson, Bernstein - I apologize for the alphabetical order, but that's already twelve people directly and actively involved in the brilliant mystification-escroquerie of the Colloque Pervers - not to mention Hotta's thirteen!

Malgrange is not quoted in the article in question either - apparently there are coteries of allied authors who quote each other in turn, avoiding quoting those next door even when they're pumping on them as best they can. In any case, when it comes to the ancestor or the vague unknown, they all agree. It's often brilliant math, surely.

- but as an old-fashioned person, I'm not indifferent to the mentality and it takes away my appetite for reading, and ultimately, even for making them. Not the ones they make, anyway. The smell is too distressing...

I also took a look at J.L. Verdier's article, "Spécialisation et faisceaux de monodromie modérée", published in the same Actes. Unsurprisingly, I saw "Riemann-Hilbert correspondence", with no allusion (in the text or bibliography) to the vague stranger whose thesis he had chaired. He must have forgotten. ... There's also mention of a Riemann-Roch étale theorem (that name rings a bell. . . ) - and I'd seen that too in the Laumon-Katz article. As neither of them mentions a certain deceased person, I'm thinking that this "theorem" must surely be due to

Messrs. Riemann and Roch, as well as the special case found among the "technical digressions" and "nonsense" of SGA 5 (not to mention the exposition of conjectures, providentially emptied by the far-sighted and astute "editor" Illusie. . . ).

As early as 1977, Mebkhout had already sensed a link between his philosophy and the Fourier transform, at a time when he was rigorously alone in his interest in a yoga of duality, linking *D*-Modules and discrete coefficients (as I once was, for the formalism of coherent, then staggered duality). This "Fourier transform" intuition remained vague

- the context was no more encouraging for him to continue down this path than it was for me, around 1950, to broaden my horizons.

□ To return to the Colloquium in the flesh, we have to believe that none of the brilliant mathematicians assembled in these parts, deigning to listen to the talk given by a vague stranger on duty, didn't realize that the "Riemann-Hilbert correspondence" presented to them as his own, was in fact the very one so brilliantly introduced by the most brilliant among them, as the heuristic keystone of his brilliant talk, which was (in the opinion of the organizers, Teissier and Verdier<sup>626</sup> (\*)) the "highlight" of the whole brilliant Colloquium on so-called (one wonders why) "perverse" beams. And yet it seems that none of them was surprised that the name of the vague unknown was not mentioned in this talk, which was certainly flying so high that there was no need to bother; nor, two and a half years later, with the publication of the Proceedings (early 1984), that the name of the said unknown did not appear either,

neither in the introduction (already mentioned) nor in the article in question by Deligne et al. This article left little room for doubt as to the true authorship of this <sup>correspondance</sup>, which the lead author and presenter-prestidigitator<sup>627</sup> (\*), with his customary modesty, refrained from naming, not even the names of his two illustrious precursors. If there are any who were surprised, they haven't made themselves known to this day - not to me, in any case, nor especially to the main person involved in providing the sauce for the farce, namely the posthumous and rigorously unknown pupil, as it should be, today as before - Zoghman Mebkhout<sup>628</sup> (\*\*).

## a1. Unnecessary details

### Note 171(v)<sup>629</sup>

**(a) Packages of a thousand pages...** . (May 4) Even Serre is no exception to the rule, having long (like André Weil) developed an annoying tendency to declare maths that doesn't interest him to be "bullshit". Yet he and Weil are of a calibre that (one might think) should put them above such childishness. In the event (and Deligne's "last twenty pages" aside), it was through two or three thousand pages of Grothendieckian "bullshit" that Weil's conjectures ended up being demonstrated (and quite a few other things too that neither Weil nor Serre had ever dreamed of). This did not encourage Serre to be more modest, since in the very text in which he presents Deligne's demonstration of the last step in these conjectures (in the Bourbaki seminar of February 1974, presentation no. 446), he takes this opportunity of all to ironize (in polite terms, of course) about the useless details with which the "1583 pages" of SGA 4 must be crammed. In this easy irony, I don't detect malice or bad faith, but rather thoughtlessness and thoughtlessness. He will have taken the trouble to note the number of pages in three volumes (which he avoided reading and whose substance escapes him) and to add them up - just to mock them with "elegance".

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theory of coherent duality to a theory encompassing complexes of differential operators (see b. de p. (\*\*) page 946). There is an allusion to the Fourier transform on p. 2 of the introduction to the paper "Dualité de Poincaré" by Z.Mebkhout, in Séminaire sur les Singularités, Université Paris VII (1977-79).

<sup>626</sup>(\*) This is the implicit "opinion" clearly expressed in the above-mentioned Introduction to the Colloquium, signed by Teissier and Verdier.

<sup>627</sup>(\*) For details of my friend Pierre's prestidigitation-around-the-corner tricks to claim authorship of the never-named theorem, see last year's note "The prestidigitator" (n° 75").

<sup>628</sup>(\*\*) (May 19) For details about the misadventures of my friend Zoghman, candidly lost in a milieu of "tough guys" at see the series of sub-notes "Ecllosion d'une vision - ou l'intrus", "La maffi a", "Les racines", "Carte blanche pour le pillage" (n° 171(i) to 171(iv))

<sup>629</sup>(\*\*\*) This note (in three parts (a) (b) (c)) is derived from two b. de p. notes to the note "L'ancêtre" (n° 171 (i)) - see the b. de p. notes (\*\*) p. 944 and (\*) p. 945.

□ But it all adds up, both my former complacency towards such brilliant students, and this Serre's "elegance" (at a time when l'Enterrement had already been going strong for four years. . . )<sup>630</sup> (\*), and all that followed. Barely three years later, it's as if my non-pupil Deligne, with added malice and impudence, has written again about Serre's own terms or their undertones, with those "useless details" that are pruned away, the "confused state" and the "gangue of nonsense" (where this same Deligne learned his trade and found his main source of inspiration), which a pale digest of his pen is charitably intended "to make us forget". Thus, from complacency to ease and impudence, we have arrived in the mathematical world, in barely ten years, at a state of morals where the simple feeling of decency seems to have disappeared.

It wasn't Weil or Serre, still less Deligne, who created the new tools that were lacking for "**La Conjecture**", but rather the one they take pleasure in ironizing - through deliberate ignorance or calculated malice, the effect is not very different. But I who, with infinite care, have written and rewritten, and had written and rewritten, tirelessly, throughout the months and years, a text that sets out with all the breadth it deserves, the language and certain basic tools for a vast, unifying, new and fruitful vision - I know me, and with full knowledge of the facts, that there is not **a single page** among the 1583 left behind by Serre, by my students and by unanimous fashion, that has not been weighed and reweighed by the workman and that is not in its place and fulfilling its function, which no other page written to date could fill. These pages are neither the product of fashion nor of vanity, which takes pleasure in setting itself above others. They are the fruits of my love and of the long, obscure labor that prepares a birth.

For this part of my work, as well as for all my major contributions to mathematics which have now become part of the common heritage, no **one** has yet been able to do what I did (with "bullshit", "useless details" and "nonsense"), except by copying me (with insignificant variations)<sup>631</sup> (\*\*). Some recopy (as is or in related or even new contexts) by saying so (this is becoming more than a rarity. . . ), others by playing the new fathers, and taking

of disdainful condescension towards the work they shamelessly plunder, and towards the worker who taught them their □ **metier**.

. This indecency has only been able to flourish and flourish because it has p. 966 found a consensus ready to welcome it, first and foremost among those who (often through their exceptional stature) set the tone.

**(b) Machines for doing nothing... .** The yoga of the six operations is an integral part of this vast unifying vision" developed in the SGA 4 and SGA 5 seminars. I'd even go so far as to say that this yoga is the central theme of the SGA 5 oral seminar, or to put it another way, that it is its "nerve" and soul. Illusie has therefore taken care to remove it from the massacre edition (destined to become a volume of "technical digressions"). . . ).

In the note "L'ancêtre" (n° 171 (i), p. 945) I write (without further clarification) that the vision-force of the six operations "has given eloquent proof of its power". For me, perhaps the most striking concrete sign of this power is to be found in our mastery of étale cohomology. To achieve this mastery, in 1963, the "six operations" vision that came to me from coherent duality was my constant guiding principle. I believe I'm the only person in the world qualified to comment on what was decisive in the development of this tool.

It's understood here that in the process of discovery, the so-called "heuristic" elements are almost always decisive. If I'm talking about the "power" of a point of view or a vision (something of a completely different nature), I'm talking about the "power" of a point of view or a vision (something of a completely different nature).

<sup>630</sup>(\*) (May 27) For a further reflection on the evocation of Serre, see part (c) of this note.

<sup>631</sup>(\*\*) (June 7) I recently read Fulton's fine book "Intersection Theory" ("Ergebnisse", Springer Verlag, 1984), and find that an exception should be made for the Riemann-Roch-Grothendieck theorem.

This cannot be measured in strictly technical terms. It is above all its "suggestive" power, as a discreet and sure guide in the voyage of discovery, whispering to us at sensitive moments "the" right notion to introduce, "the" right statement to identify and prove, "the" theory that remains to be developed. It's because we forgot such a guiding vision (after burying it) that, in the cohomological theory of algebraic varieties, the powerful impetus of the sixties ended up, in the years following my departure, in a state of confusion and stagnation. Apart from the great "prestige question" (i.e., that of the absolute values of Frobenius eigenvalues), all the essential questions were stubbornly avoided... ..

Another sign of the power of the vision (or, in this case, the formalism) of the six operations, I see the Lefschetz-Verdier formula of fixed points, both in the context of discrete coefficients and coherent coefficients. Here, the role of the "six operations" formalism has been both **heuristic** (in the sense that the formula

p. 967 is irresistibly suggested by <sup>this formalism</sup>) and **technical** (in the sense that formalism also gives the necessary and sufficient means for the proof of the formula). It's true, given the Burial, that only a tiny portion of the cohomological formalism I had developed was used, up until at least the "rush" on intersection cohomology and on so-called "perverse" beams (where part of the formalism is exhumed without mention of the worker. . . ). But I am well aware that, along with Weil's conjectures and the omnipresent intuition of topos, the vision of the six operations was my main source of inspiration in my cohomological reflections throughout the years 1955- 1970<sup>632</sup> (\*). In other words, the "power" of this vision is for me a self-evident fact, or rather, a reality that I have experienced almost daily for fifteen years of my life as a mathematician. This experience has been strikingly reconfirmed over the past few weeks, as I've resumed contact with the "abandoned sites" of De Rham's crystalline coefficients and<sup>633</sup> (\*\*) patterns.

p. 968 □ This very "subjective" experience I have of the power of a certain vision-force, also has a "objective" sense, difficult to dismiss out of hand. This sense emerges when one remembers that (with a few rare exceptions) the main ideas and notions concerning the cohomology of "abstract" algebraic varieties and schemes (which everyone today uses as if they dated back to Adam and Eve<sup>634</sup> (\*\*)) were developed by none other than myself, during the same period 1955- 1970. (It goes without saying that I'm setting aside here my starting point FAC, and Weil's conjectures).

<sup>632</sup>(\*) (May 15) It is understood that the vision itself took shape gradually over this period, from the first seeds contained in my 1955 article "On some points of homological algebra" (in Tohoku Math. Journal). It reached full maturity in 1963, with the sudden onset of étale cohomology. This occurred (as if by chance) in the same days, more or less, that I introduced the "missing functor"  $Rf_!$  (direct image with eigenstands). But the role of the six operations, as "vision-force" and as omnipresent fi l conductor, only became fully conscious, I believe, with the SGA 5 seminar. As early as 1966, with the start of crystalline cohomology, it was clear to me that the first objective (beyond the limited "running-in" program, which would be accomplished in Berthelot's thesis work) was to arrive at a formalism of six operations (and biduality) for "the right" crystalline coefficients. It took a (deceased) crumbler to emerge from the coffin prepared for him, so that (almost twenty years later, and inspired by the ideas of a vague stranger in the service and co-buried) these "good coefficients" finally ended up just being **defi ned**! A description can be found in fi ni type diagrams on  $Z$  in particular, in volume 3 of Réflexions (with the fifth and final part of Récoltes et Semailles).

<sup>633</sup>(\*\*) (May 15) For the image of "abandoned building sites" (or "desolate" building sites), see part 6 of the Funeral Ceremony (notes 176', 177, 178), and in particular the last of the three notes quoted. While writing Récoltes et Semailles, I devoted a few hours here and there to De Rham's problem of crystalline coefficients and that of motifs, and a convincing defi nition for the former appeared, and a principle at least for the construction of the latter, in the (Compare with the comments in the previous b. de p.).

<sup>634</sup>(\*) About this mentality of the "user" (or "consumer") of fi nished mathematical products, who has forgotten (if he has ever known... ) what a creation is, and also on the subject of Adam and Eve and the good Lord, I refer the reader to last year's notes "A sense of injustice and powerlessness... . " (n° 44') and "L'inconnu de service et le théorème du bon Dieu" (n° 48'). See also "Teaching failure (2) - or creation and fatuity" (note n° 44'),

Mathematically speaking (and from what I've seen so far), this great era has led to a morose mediocrity, the root cause of which is in no way technical. It's one of the signs of this mediocrity that a powerful vision designed to inspire and nurture grand designs has been buried or made a mockery of, by the very people who were its custodians and primary beneficiaries. And another sign that neither Deligne, Verdier, Berthelot nor Illusie, overwhelmed as they were by all the facilities conferred by position and prestige, brilliant gifts and consummate experience, were able to do the work that was required on the basis of De Rham's coefficients, in line with their own research (and the rejected vision. . . ); nor even to recognize the innovative and fruitful work, when confronted with it. And it's in this **same** spirit (for it all ties together, once again...) that, once they'd finally recognized the significance of one of the tools derived from the new work, they hastened to seize it without even understanding it, and to bury, alongside the ancestor, the unknown worker who had fashioned it...

**(c) Things that look like nothing. ... - ou le dessèchement** (May 27)<sup>635</sup> (\*\*) The way I express myself about Serre came about spontaneously, and stems from a perception of things, the concerning, which must have formed in me over the last few weeks or months. There has been <sup>□</sup>pourtant, as I wrote these lines, a residue of uncertainty or perplexity, or reserve, with regard to what I had just written. In short, I was suggesting that Serre, on this occasion, lacked "elegance"!

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The fact is, in the nearly thirty years I've known Serre, he's been the very embodiment of "**elegance**" for me. I'm sure I'm not the only one to feel this way. It's an elegance, both in his work and in his relationships with others, that is by no means purely formal. It also implies scrupulous probity in one's work, and an equal demand for probity in one's dealings with others. On more than one occasion, I've noted his sharpness of judgment in the face of any inclination to "muddle through" on the part of a less scrupulous colleague, trying to gloss over an embarrassing difficulty (so as not to have to admit that he didn't know how to overcome it), or some error of his own making... . This elegance also implied a **rigorous approach**, both to himself and to others.

It's all these things, which for me remain inseparable from the person of Serre, that must have inter- come into this "residual reserve" in me that I've just mentioned, in the face of the spontaneous expression of another perception of things, unexpectedly taking the lead over the familiar perception. There's no question of my wanting to dismiss one of the two for the "benefit" of the other. Both have something to teach me, different aspects of a complex reality that is by no means static. It's up to me to situate one in relation to the other, to arrive at a nuanced apprehension of a person to whom I'm linked by a past, and feelings of sympathy and respect.

This "rigor" I've just mentioned did not, however, extend to everything to do with Serre's relationship with mathematics and mathematicians. Earlier, I invoked an "unconsciousness" or a "lightness", which I could just as easily have called a "**closure**". This contrasts with the attitude of "prudence and modesty" that I encountered in most of my elders who, like Serre himself, welcomed me with kindness in my early days, and sometimes (as was his case) with warmth. I'll say more about this later (in the note "Liberté. . . "). . . , n° 171 (vii)), where I note that this attitude had been part of "the atmosphere of respect. . . that permeated the milieu that welcomed me".

The "closure" I've noticed at Serre, on certain occasions, isn't new. I can see the the second half of the fifties. I believe that it has greatly limited the depth and the scope□ of his work from the sixties onwards. I feel a link between this aspect of "closure", vis-à-vis

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<sup>635</sup>(\*\*) This third part of the note "Unnecessary details" is taken from a footnote to the first part. See the reference in the footnote (\*) on page 965.

different approaches to mathematics than his own, and a deliberate intention, which gradually developed within him, to confine his apprehension of mathematical things and mathematics to a purely technical or technicist view (or "blindness", I would have liked to write), closing himself off to anything resembling a **vision** ; to something, therefore, that goes beyond the tangible, immediate, **provable** statement (or set of statements), or (at the very least) takes the form of a "pure and hard" conjecture, with its contours entirely transgressed, "closed" in short (except that it has yet to be proven. . . ). With hindsight, it seems to me that he ended up pushing this aspect of his creative abilities to the extreme limit, the exclusively "yang" and "super-yang" aspect, the "**macho**" aspect. Given his exceptional ascendancy over the mathematicians of his generation, and of two or three others that followed, it seems to me that Serre contributed a great deal to the advent of the excessive technicist spirit that I see rampant in the seventies and eighties, the only one nowadays that is still tolerated, while any other approach to mathematics has become the object of general derision.

To use C.L. Siegel's expression, what we're witnessing today is an extraordinary "Verflachung"<sup>636</sup> (\*), a "flattening", a "shrinking" of mathematical thought, deprived of a dimension - the visionary dimension, that of dreams and mystery, that of depths - with which it never had before (I'd like to think of it as a "flattening", a "shrinking" of mathematical thought).

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seems) lost all contact. I feel it as a **drying out**, a **hardening of** thought, losing its living suppleness, its nourishing quality - becoming pure **tool**, <sup>□raide</sup> and cold, for impeccable execution of "snatch-and-grab" tasks, tasks at public auction<sup>637</sup> (\*) - when the sense of purpose and direction, and the sense of these tasks themselves as parts of a vast Whole, are forgotten by all. There's a deep sclerosis, hidden by feverish hypertrophy.

This imbalance in thinking is just one sign of a more essential imbalance, a deeper emptiness and deficiency. It's no coincidence that, over the past two decades, this dryness of thought has spread and taken root at the same time as the customary forms of delicacy and respect in relationships between people have been eroded. And it's no coincidence either that this wind of contempt, whose breath I've finally felt, has been accompanied by a more or less generalized corruption, which I've been working on for over a year now.

To this day, Serre has never been aware of this corruption, which surrounds him on all sides. I knew he had a fine nose, though. But it's not just a matter of having a fine nose, it's also important to use it, to take note of what it has to tell us, even when the smells he's talking about are apt to incommode us; indeed, to worry us, when they call us into question ourselves. I know that Serre, no more than

<sup>636</sup>(\*) I have taken this expression (in German) from a letter I recently received from Serre. The phrase is taken from C.L. Siegel's preface to Hecke's works. Serre quotes this impression of C.L. Siegel at the very end of his letter, adding: "It was unfair, and it would be even more unfair now, it seems to me". It struck a chord with me, though, and kept on working. My brief reflection on the relationship between Serre and me probably grew out of it.

In fact, I think that if Serre quoted Siegel, it's because in some way this impression, coming from one of the great mathematicians of our time, must already have been working in him; it was like a blip, no doubt, in the mathematical "vie en rose". A blip, no doubt, among others, but less easy to get rid of, apparently... ..

"Flach" in German means "flat", "devoid of depth"; "Verflachung" designates the process leading to such a state of "flatness", or the culmination of such a process which has just taken place. In the main text, I've endeavored to follow the associations aroused in me by this very telling term, untranslatable as it is, unfortunately. Of course, I have no idea whether my perception of the matter overlaps in any way with that of Siegel, whose text Serre quotes I have not read.

<sup>637</sup>(\*) This image of "public auctions" must have been suggested to me by the announcements of "invitations to tender" (sic) that litter the "CNRS newsletters" and other papers I periodically receive, as a fresh-faced research associate in this esteemed Institution. This jargon, among many other signs, shows the extent to which this "flattening" of the work of discovery is by no means limited to the milieu I had known well, nor to mathematical science. I've yet to find a call for tenders in pure mathematics, but it won't be long - and I can easily imagine some of my friends or students of yesteryear, sitting gravely behind padded doors, on some committee with a boring acronym, to decide which "lines of research" to declare a priority, which "approach strategies" to promote, and which "bids" from "winning" teams to "retain" for "preselection", or even, to honor with the jackpot, the official subsidy by the supervisory Ministry, renewable every two years after a favorable opinion from the competent committee. . .



I wouldn't dream of howling with the wolves, looting, scheming and debasing, where "everyone else" is looting, scheming and debasing. He doesn't do any of that, of course - he just plugs his nose (and too bad if he loses a hand as a result. . . ), and pretends he hasn't smelled anything.

And he's here in good company - not one of my friends in the world we once knew

and whose scent reaches me even in my retreat - not a single one has yet spoken to me, even by hint, of an odor□  
that he smelled and that bothered him. Many more, surely, remainp . 972  
among my colleagues who continue to practice the profession of mathematician with probity, which deserves this respect. But among those sitting in the front seats, I don't know of **one** who has had the simplicity to believe the testimony of his healthy faculties (olfactory, in this case), rather than plugging his nose so as not to have to say to himself: something smells bad here - perhaps we should go and have a look... .

But I'd like to come back to Serre and myself, and to this "closure" I sensed in him, which appeared I don't know when and became more pronounced as the years went by. I believe that the most fruitful part of his work, the one that most profoundly influenced the mathematics of his time, took place in the early years, before this closure appeared, or at least, before it took a decisive hold on his relationship with mathematics and mathematicians. It was also in those years, in the 1950s, that contact with him was most fruitful for me, and it was in those years that Serre played the role of "detonator" for me, giving my work some of its most decisive impulses. It was in those years, too, that a vast vision was born and grew within me, inspiring and fertilizing my work in those years and right up to the present day. I can say, with full knowledge of the facts, that if there was anyone besides me who had a part in the blossoming of the vision, it was him, Serre, and in those years. And it could only have been so because, in those fertile and decisive years, he was open to mathematical things for what they are, including those that still elude immediate grasp; those that seem reluctant at first to let themselves be encircled by the meshes of language already formed - those that may require years of obscure and patient labor, if not a lifetime, before condensing into tangible substance and revealing the limbs and shapes and contours of a **body**, alive and vigorous, attesting to the unexpected appearance, in the familiar context of the known, of a **new being**.

I believe that in the early years of my acquaintance with Serre, and right up to the late 1950s, he retained a sensitivity for the delicate, impalpable thing that is "creation", and for the humble labors that prepare a birth. I think there was a moment when he sensed the blossoming of a vision, and the language that gave it form, like the soul or the spirit, and the body... . There was then a warmth without speech, an availability... discreetly and effectively, where he could support a laborious and intense work that was not his own, and yet in which, through sympathy and expectation, he participated.

□I cannot say when or how this vivacity in him, on the level of our common passion, became p . 973  
I've tried to define it. Already in the early sixties, if not before, he stopped perceiving the forest, agreeing to see only such and such a tree as he found to his liking. The rest was irrelevant. It simply annoyed him, I think, to see me so absorbed in tirelessly clearing vast, seemingly empty expanses and patiently planting all those things that didn't yet look like anything, with the air of one who would already see a flourishing forest<sup>638</sup> (\*).

But that hasn't stopped me from clearing, planting and replanting, pruning and replanting and replanting and replanting.

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<sup>638</sup>(\*) (June 17) Of the six "worksites" I reviewed in the note "Le tour des chantiers - ou outils et vision" (n° 178), only **one** (the "motifs" worksite) had ever been of any interest to Serre - and even then. . . When

I wrote to him recently without comment, in a PS, that I thought I had the principle of a construction in the form of the category of patterns on a fi type scheme nor on Z, he didn't allude to it in his reply. Clearly, these "Grothendieckian maths" no longer make him hot or cold... ..

replanting - nor that we were buddies as always, spending hours and hours discussing maths (usually on the phone). When I had a clear-cut question, and on a question that wasn't on the index, it was to him above all that I used to turn, in case he had any insights - and often, indeed, he did. I continued to learn many things from him, and I'm sure he was learning things from me that would interest him. It was better than an exchange of goodwill or services - there was always a common passion linking us, there was fire and spark.

But he had already ceased to be a source of inspiration for me. That source now lay within myself alone<sup>639</sup> (\*\*).

## a2. Crazy questions

**Note** 171(vi) (May 5)<sup>640</sup> (\*\*\*) My recollection here was a little hazy, and became clearer over the next few weeks, when I had the opportunity to reconnect with these issues to some extent. There were in fact two distinct questions in my mind, one perfectly precise, the other rather vague.

The first question concerned the need for a complete theory of the six variances, for "De Rham coefficients" that had yet to be precisely defined. My crystal-clear ideas, both in characteristic  $p > 0$  only in zero characteristic, provided a very precise primer - we already knew, in advance, what was to replace the "local systems" (or "twisted  $\square$  constant bundles")  $l$ -adic (or Betti, in the transcendental framework), and it was necessary to define "coefficients with singularities", in the spirit of derived categories of course<sup>641</sup> (\*). What was missing, then, was a good "finiteness" condition for crystalline complexes. In zero characteristic, it's "*D-coherence*" (which neither I nor any of my students have thought of, even though it's such a simple and natural idea!), combined with the more delicate conditions of holonomy and regularity, that provides the answer, as we learned (twelve years after starting crystalline yoga) from the philosophy of the good God alias Mebkhout. I'm curious to see whether any of my ex-students will end up making a move (without naming the stranger on duty, or the ancestor, it's a foregone conclusion... .) to find out the conditions

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<sup>639</sup>(\*\*) (June 12) For a continuation of this reflection on the relationship between Serre and me, see the note "L'album de famille" (n° 173), part c. ("The one among all - or acquiescence"), June 11, and parts d. and e.

<sup>640</sup>(\*\*\*) This note is taken from a note by b. de p. to the note "L'ancêtre" (n° 171 (i)) - see note (\*) on page 946.

<sup>641</sup>(\*) It's also clear, when the base body was  $\mathbb{C}$ , that we wanted a category equivalent to that of  $\mathbb{C}$ -vector bundle complexes with algebraically constructible cohomology bundles. This very precise indication suggested that, by unscrewing, the neuralgic question was that of associating, with any crystalline local system on a subscheme (not necessarily a  $\mathbb{C}$ -vector system), a category equivalent to that of  $\mathbb{C}$ -vector complexes with algebraically constructible cohomology bundles.

This is essentially what Deligne did in 1969. This is essentially what Deligne did in 1969, except that it turned out that instead of a crystalline beam, there was a crystalline **pro-beam**, which was an important new idea at the time (and "obvious", as soon as you take the trouble to look. . .). But systematic work with pro-objects would have required considerable groundwork, of which Jouanolou's thesis (on *l*-adic coefficients) gave a foretaste. We'd have had to roll up our sleeves again. ...

Mebkhout's new approach using  $D$ -Modules therefore amounts (from Deligne's and my point of view) to replacing a crystalline pro-bundle by a crystalline ind-bundle (thanks to the **ordinary** coherent dualizing functor  $RHom_{O_X}(-, O_X)$ ), and **going to the inductive limit** to find an ordinary crystalline bundle, i.e. (assuming now  $X$  smooth on a body of zero characteristic) a  $D_X$ -Modules. The unexpected "miracle" then, established by Mebkhout between 1972 and 1976 (starting from an opposite "end", cf. the note "The three milestones" n° 171 (x)), is that this  $D$ -Module is **coherent** (more precisely, with coherent cohomology bundles). Another equally unexpected miracle is that  $D$ -Modules can be characterized as complexes of  $D$ -Modules, which we obtain by means of simple conditions of an entirely new nature compared to Grothendieckian crystal optics (namely, the "microlocal" holonomy condition, in addition to a "regularity" condition introduced by Mebkhout and which has become familiar in the meantime).

(May 26) For details of the duality relationship between De Rham - Mebkhout coefficients and De Rham - Deligne coefficients, see the note "The five photos (crystals and  $D$ -Modules)" (n° 171 (ix)), part (c). For the need to replace Deligne's view of procoherent modules with that of crystals in coherent promodules, and on the possibility (non proved again) to replace the cumbersome point of view of pro-objects (crystalline or stratified) by crystalline beams without more (by passage to the projective limit), see the same note, parts (c) and (d).

corresponding because.  $p > 0$ , or rather no doubt, in the rigid-analytical context of caractéristique  $p$ .<sup>975</sup>  
 . Better late than never...<sup>642</sup>(\*)).

I didn't pursue this question myself in the sixties, as I had enough other things to do and thought that, with Berthelot and Deligne on the job, it was in good hands (which proves that you can be wrong. . . ). Deligne's work in 1969/70 did, however, in principle provide a null-characteristic answer, which would undoubtedly have satisfied me, had Deligne completed his work.

But in my mind, such a conjectural theory of De Rham coefficients, even if it had to relate "discrete" cohomology (in the form of a crystalline cohomology) and "coherent" cohomology, did not "cap" the theory of coherent duality. Thus, I didn't see that a coherent bundle

zariskian defined an "enveloping crystal"<sup>643</sup>(\*\*) (NB in the language of D-Modules, this is the extension of the Ring of scalars  $\underline{Q}_X \rightarrow D_X$ , for smooth  $X$  at least. . . ) - and even if I had seen it, the crystal obtained (already for  $F = \underline{Q}_X$ , which gives the crystal  $D_X$ ) is **not** of De Rham's type. However, I was wondering whether on a complex analytic space  $X$ , consistent duality (e.g. in Serre's form, if  $X$  is smooth and for locally free coefficients) could not be obtained as a "special case" of discrete duality, developed by Verdier on the model of the étale theory. As it stood, this sounded a bit zany and immediately raised a host of questions: how to explain "in discrete terms" the role of the modulus dualising (differential forms of maximum degree)  $\underline{\omega}_X$ , and how can we take into account evanescent pathologies, which had no analogue in "discrete" duality?

□ It was Mebkhout who was the first (and the only one to this day apart from me, it seems) to understand that there is indeed a deep connection between the two dualities, but that this is expressed **not** by saying that one "caps" the other, but by finding a third theory of duality<sup>644</sup>(\*) that of  $D_X$ -Modules (or "crystals" on  $X$ ), which "caps" both the one and the other, and by limiting itself, moreover, on the "discrete" side, to C-vector complexes which have **analytically constructible** cohomology bundles. There's no doubt in my mind that this is "the correct answer" to that "vague question" (and a bit off the mark. . . ) that I never had the chance to ask my posthumous pupil. . .

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(May 15) The writing of "L' Apothéose" became at the same time an unforeseen opportunity to familiarize myself with Mebkhout's work, and with the yoga of D-Modules that he introduced into the cohomological study of varieties. Along the way, it also brought back memories that had sunk. In particular, I realized that as early as the late fifties, or the early sixties, I had been closer to "Mebkhout's philosophy" than I realized only ten days ago, when I wrote the beginning of this note ("Les questions saugrenues"). Within the framework of clean and smooth schemes on an arbitrary basis, I had in my hands a duality statement (in terms of a complex of relative differential operators and the "adjoint" complex), "capping" coherent duality and duality for De Rham's cohomology. Technically speaking, this was pretty much the equivalent of the algebraic version of the

<sup>642</sup>(\*) (May 26) Since these lines were written, and as an unexpected fruit of my efforts to write an account of the Apotheosis that is worthy of passing to posterity, I have been led to draw out (almost unintentionally) what now seems to me to be **the most important aspect of the Apotheosis**.

good definition of De Rham coefficients, at least for a finite type scheme on  $\text{Spec } \mathbb{Z}$ , (which appears to me as the most crucial case of all). Of course, the essential new ingredient, compared to my 1966 ideas, is the philosophy of the vague unknown, whom I will refrain (like everyone else) from naming here.

The approach I foresee for finite type schemes over  $\text{Spec}(\mathbb{Z})$ , must also give the right De Rham coefficients (Mebkhout or Deligne style, your choice) for finite type schemes over any body (of zero characteristic, or no). I intend to outline this approach in the "Coefficients de De Rham" section of volume 3 of *Réflexions*, among other "technical digressions" that my students can come and copy at their leisure. . .

<sup>643</sup>(\*\*) (May 26) It may be better to take the enveloping "co-crystal" (see note 171 (ix) part B, for allusions to the notion of co-crystal). I'll no doubt come back to this question in the presentation promised in the previous b. de p. note.

<sup>644</sup>(\*) For details of this "third theory of duality . . . which overshadows the other two", see note. "The work... ." (n° 171 (ii)).

Mebkhout's duality theorem (discussed, in the complex analytical context, in the note "L'oeuvre. . . .", n° 171 (ii)). However, my duality statement did not satisfy me, and I did not think of publishing it or even advertising it, because it seemed to me, in the said form, too close to Serre's duality theorem (relativized on some basis, it is a given), of which it is a more or less immediate corollary. To arrive at a statement that satisfied me, I would have had to know how to make a "derived category" out of complexes of differential operators, so as to be able to formulate a statement of intrinsic duality in terms of the objects of these categories, along the lines of the theory of coherent duality worked out in previous years.

p. 977 What was missing, then, was a good notion of "quasi-isomorphism" for a (differential) morphism between complexes of differential operators,  $\square$  so as to form a derived category (formally inverting- These quasi-isomorphisms). It was clear that the usual definition (via the associated cohomology bundles) was not usable in the algebraic setting (and probably no more so in the transcendental setting<sup>645</sup> (\*)). The transition to the corresponding D-module complexes now provides a wonderfully simple answer to my former perplexity!

Seeing no ready-made definition for the notion of quasi-isomorphism, I didn't try to find out whether it existed or not, and whether it would indeed be a remarkable derived category. This was at a time when I was the only one interested in the (far less sophisticated) derived categories formed from coherent modules and the **linear** morphisms between them. . . . It wasn't clear to me that this question of a notion of quasi-isomorphism (also a little vague, not to say far-fetched) touched on a fertile mystery, which admitted a "key" of childlike simplicity! And that there was a category of remarkable "coefficients" just waiting to be defined. For this to happen, my reflections would undoubtedly have needed to be pursued in an atmosphere where they met with a modicum of interest and resonance, if only from **an** interlocutor who was a stakeholder!

It was De Rham's cohomology that drew my attention to the obvious fact that the global cohomology spaces of coherent bundles, on an algebraic variety  $X$  over a field  $k$  let's say, are "functors" not only with respect to  $\mathcal{O}_X$ -linear homomorphisms, but even with respect to **all** homomorphisms of  $k$ -vector bundles, and in particular, for differential operators. It was this observation that prompted an embryonic reflection on a "coherent" (or "quasi-coherent") theory of duality, in which the "morphisms" between bundles would be differential operators, instead of being linear.

p. 978 As I said, this reflection turned out to be so short-sighted that it didn't even stick in the back of my mind, as one thing (among a number of others) that should  $\square$  well be cleared up one day - it sank (I think) into total oblivion until just a few days ago. Even my sporadic reflection on crystals, around 1966, didn't bring it up in my memory, as far as I can remember. And yet, without my even suspecting it at the time (because I couldn't remember the question at the time!), this crystalline reflection was to provide me, as early as 1966, with **another** key, "dual" in a way to Mebkhout's, for my perplexities of yesteryear, via the complex of principal parts of infinite order associated with a complex of differential operators. I allude to this in a b. de p. note written yesterday (note(\*\*) page 946), and I intend to return to it in detail in the part of volume 3 of Reflections, developing the yoga of "types of coefficients" and giving, in particular, a formal definition of what I presume to be "the"

<sup>645</sup>(\*) I'm mistaken here. Mebkhout assures me that for a (differential) homomorphism between complexes of differential operators, this is a quasi-isomorphism (in the naive sense of complexes of associated C-vector bundles) if and only if the corresponding homomorphism for complexes of associated D-Modules is a quasi-isomorphism. This is equivalent to (by means of the mapping-cylinder) to say that a complex of differential operators is quasi-zero in the naive sense, if and only if the associated complex of D-Modules is quasi-zero, something apparently well known (at least to Mebkhout, who demonstrates it in his inexhaustible thesis. . . ).

good De Rham coefficients (Mebkhout or Deligne style, your choice) on a finite-type scheme over  $\mathbb{Z}$  (for example).

Technically, and even "psychologically" (in terms of the problems already posed at the time, and the overall vision that gave them strength and life) everything was ready, by the second half of the sixties, to release this definition of De Rham's coefficients. Deligne, after me, came very close to the right notion, and he couldn't have avoided it, had it not been for a force to which he gave omnipotence over his life and his work, which put a premature and peremptory end to his reflections along this path. . . <sup>646</sup>(\*)

Discovering doesn't mean hitting a nail, a chisel or a steel wedge with a hammer or sledgehammer. Above all, discovering is knowing how to listen, with respect and intense attention, to the voice of things. New things don't spring ready-made from diamonds, like a sparkling jet of light, any more than they do from a machine tool, however sophisticated and powerful it may be. It doesn't announce itself with a bang, boasting its letters of nobility: I am this and I am that... . It's a humble and fragile thing, a delicate and living thing, a humble acorn perhaps from which an oak tree will grow (if the seasons are right. . . ), or a seed that will give birth to a stem and this to a flower. It's not born in the limelight, or even in the sunlight. It is not the fruit of the known. Its mother is Night and

penumbra, elusive mists without contours - the presentiment that eludes the words that would capture it, the saucy question that still seeks itself, or  $\square$  telle dissatisfaction so vague and so elusive and very real p. <sup>979</sup>

yet, with that indefinable (and indisputable... ) feeling that something is amiss or amiss and that something is fishy... ..

When we know how to listen humbly to these voices that speak to us in hushed tones, and to follow their elusive message obstinately, passionately, then - at the end of obscure and groping labors, muddy perhaps and without appearance - suddenly the mists become incarnate and condensed, in **substance**, firm and tangible, and in **form**, visible and clear. In this solitary moment of intense attention and silence, the new thing, daughter of night and mists, appears... ..

## Freedom . . .

**Note** 171(vii) (May 4) <sup>647</sup> (\*) I don't pretend to be the "mature" or "wise" man, surrounded by the immaturity and irresponsibility of his fellows - I don't imagine that's the image that emerges of me in the pages of *Récoltes et semailles* <sup>648</sup> (\*\*). And yet, in my relationship with mathematics at least, I think I can say that throughout my life I have maintained a good-natured simplicity <sup>649</sup> (\*\*\*), while at the same time

<sup>646</sup>(\*) On this subject, see the reflection in the sub-note ". . and hindrance" (n° 171 (viii)).

<sup>647</sup>(\*) This sub-note is taken from a b. de p. note to the note "L'ancêtre" (n° 171 (i)) - see note (\*\*) on page 945.

<sup>648</sup>(\*\*) (May 26) I can even say that if the writing of *Récoltes et Semailles* revealed anything to me on this subject, it's a state of being. of "immaturity" indeed, a lack of "wisdom", and by no means the opposite. Perhaps the most unexpected discovery of all, and the most crucial in its immediate implications, was the strength of my attachment to a certain past and to my work as a mathematician. This attachment, still in relatively discreet form, first revealed itself to me at the end of March last year.

last, in the course of reflection in the final note "The weight of a past" (n° 50) of *Fatuité et Renouveau*. It was being confronted with the brutal reality of the Burial, in its aspects above all of deliberate contempt and violence, that set in motion the "weight of a past".

powerful egotistic defense reflexes. At the same time, they reveal to me the strength of the ties that bind me to a past that I may have once believed had detached itself from me. Over the past year, these ties seem to have taken on a new vigour, and very often (especially lately) I feel them as a **weight** indeed, an exhausting weight indeed - like other weights that once weighed on me, and which have now been resolved. . .

<sup>649</sup>(\*\*\*) (May 16) I'd have to make an exception here for a certain possessive attitude towards my "chasses gardées", which I put my finger on in *Fatuité et Renouveau*, in the section "La mathématique sportive" (n° 40). These "sporting" dispositions were to lead me to minimize the ideas of others, whenever these were already known to me on my side. We can therefore say (contrary to what I assert in the main text) that in these cases, my vanity did indeed interfere with "my sound judgment", and tended in such cases to incite me to a discouraging attitude, where a benevolent encouragement

p. 980 than a fidelity to my original nature□. Vanity, which has been as pervasive in my life as in that of any other of my colleagues, hardly interfered (as far as I can remember) with my sound judgment and mathematical flair<sup>650</sup> (\*).

In fact, it was only after I left in 1970 that I began to realize, little by little and each time with amazement, how common it is, even among men of exceptional abilities, that these are sometimes annihilated, hopelessly blocked, it would seem, by prejudices of an "irrational" nature - and all the more tenacious for it! My first experiences in this direction date back to 1976<sup>651</sup> (\*\*), and are mentioned in the note "On n'arrête pas le progrès" (n° 50), and a first written reflection on this subject is continued in the note "Le Fossoyeur - ou la Congrégation toute entière" (n° 97)<sup>652</sup> (\*\*\*), in the particular context of□l'Enterrement. It is also only gradually, and against forces of inertia I've come to realize that these "irrational" causes are nonetheless perfectly intelligible, provided we take the trouble to stop and look into them. It's thanks to this that I've come to "accept" them too, as best I can... ..

Getting back to myself and my relationship with mathematics. Because of my working style, I tend to work on the basis of often hasty presumptions, without worrying about "prudence"<sup>653</sup> (\*); but I follow each of the intuitions (or "presumptions") that emerge right to the end, so that the numerous errors that litter the early stages of the work are eventually eliminated, giving way to a solid understanding that (more often than not) gets to the heart of things. My spontaneous way of proceeding is quite different when it comes to passing judgement on someone else's work, and especially when it's on a subject or in registers with which I'm not familiar. It seems to me that I've always tended to be cautious and modest. In fact, this was the example set to me by most of the elders who welcomed me into their midst, such as Cartan, Dieudonné, Chevalley, Schwartz, Leray - to name but a few. I don't recall any of them expressing themselves peremptorily, either for good or ill, on a work whose substance eluded them. This caution, I now realize, was part of the atmosphere of **respect** I've spoken of elsewhere, which permeated the environment that welcomed me<sup>654</sup> (\*\*). It seems to me that it was this prudence, a sign of respect, that first deteriorated in the environment with which I identified myself for over twenty years of my life. Perhaps my memory betrays me and I'm deluding myself, but it seems to me that I was relatively

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would have been appropriate. It seems to me, however, that such situations have been exceptional in my life as a mathematician, and that they have not hindered my mathematical creativity.

<sup>650</sup>(\*) See previous b. de p. note for reservations on this subject.

<sup>651</sup>(\*\*) (May 16) These aren't exactly my first experiments in this direction - I'd had others in previous years, with Deligne in particular, and also in my past before I left. But these experiences had remained sporadic, whereas the episode surrounding Ladegaillerie's thesis was impressive for the perfect concordance in the acts and omissions of five mathematicians (all of the highest calibre), who surely hadn't consulted each other. This was my first contact with l'Enterrement, over and above the vicissitudes of my relationship with my friend Pierre.

But this extraordinary weight of "irrational" factors in so-called "scientific" thought goes far beyond the context of the Burial, and even beyond that of an era. You don't need to be an expert in the history of science (and I'm not) to realize that it is marked at every step by the effects of an immense inertia, opposing the emergence of any innovative idea, and its blossoming when the idea has nevertheless appeared. For thoughts along these lines, see in particular the first two parts of Fatuité et Renouveau ("Work and Discovery" and "The Dream and the Dreamer"), sections 1 to 8.

<sup>652</sup>(\*\*\*) This reflection is considerably deepened in "The key to yin and yang", in particular, in the two notes (concerning this same "Congregation") "La circonstance providentielle - ou l'Apothéose" and "Le désaveu (1) - ou le rappel" (n° s 151, 152). See also the note "Le muscle et la tripe (yang enterre yin (1))" (n° 106) which opens the long reflection on yin and yang.

<sup>653</sup>(\*) On this style of work, see in particular the note "Brothers and spouses - or the double signature" (n° 134), and also the section (in Fatuity and Renewal) "Error and discovery" (n° 2).

<sup>654</sup>(\*\*) See "The welcome stranger" section, n° 9.

little affected by this aspect of the degradation of an atmosphere of respect. I've always been conscious, I think, of the extent of my ignorance of mathematics in general, and of my limitations in apprehending the work of others, as soon as it fell outside my own, usually strongly focused, sphere of interest.

□ When it came to other people's work, which I was in a position to understand and thereby appreciate or judge p. 982

(Nor do I recall any gross error of judgment, either for good or ill, that I had to note after the fact. The same is true of the feeling I had about my own ideas and intuitions, whether this feeling concerned the presence (or absence) of a "good question", or that of a rich substance to be probed, or the scope of such and such an idea, or the more or less complete and more or less profound understanding I had of a situation or a thing. In all these cases, if there was an error, it was always in the sense of a "minus". Yes - more often than not, the richness of a new theme or idea, its true scope in depth and extension, is only fully revealed little by little, over weeks and months, if not years. This gradual confirmation of an initial feeling that is right (more often than not), but remains vague and diffuse at first, through more or less thorough and meticulous "work on parts", then comes to us as a surprise and as a wonder, constantly renewing itself as the hours and days go by. This is surely the reason for the extraordinary fascination of research work (whether mathematical or otherwise): at every step, the reality that unfolds before our eyes surpasses even our most reckless dreams, in richness, delicacy and depth. ...

But I'd like to come back to my apprehension of other people's work, when it concerned subjects that were familiar to me, or even "hot" subjects for me. I think I can say that my ability to sense the true significance of an idea (which often eludes the author himself) has played a key role in my work. I'm thinking here first and foremost of the exceptional role played by Serre, and of the fact that during those fifteen exceptionally rich years in my work, between 1955 and 1970, most of my ideas, and most of my major investments too, had their starting point in some of Serre's ideas or approaches, some of them seemingly innocuous. I intend to discuss them in more detail in the "Historical Comments" to the Thematic Outline<sup>655</sup> (\*). But this does not mean that I am particularly open to Serre alone. The same thing has happened with other mathematicians, both in my past as a functional analyst and as a geometer<sup>656</sup> (\*\*).

□ I can say that, throughout my life as a mathematician, I have been superabundantly "rewarded" for this simplicity of approach to mathematics, which I've just tried to define. This simplicity, p. 983

<sup>655</sup> (\*) These "Comments" are announced in "Compass and Luggage" (Intr. 3).

<sup>656</sup> (\*\*) By way of example (among many others), I'd like to point out the principle of reducing statements about relative schematic situations "of finite presentation" on any basis, to the case where the latter is the spectrum of a **fi ni** local ring (or even, of a **corps fi ni**), a far-reaching principle which I have extracted from a striking demonstration idea of a remarkable (and very special) result by D. Lazare. On this subject, see the note "Pouce!" (n° 77) and the b. de p. note (\*\*\*) p. 297 to this one.

(May 16) I'm not sure that every time I've taken inspiration from someone else's idea, I've taken care to point it out. For example, I don't remember, in the relevant paragraph of EGA IV, taking the trouble to cite Lazarus as the source of the general reduction method developed there. It was an oversight that, in those days, didn't seem to draw any consequences. I believe that people like Dieudonné (who co-edited the EGA with me) or Serre, who must have known Lazare's result as I did, as being (undoubtedly) the first of its kind, wouldn't have considered it imperative (or even appropriate) to cite it either - it wasn't, in any case, in the canon of Bourbaki style! It's true that Bourbaki made up for it in historical notes, which are lacking in EGA and elsewhere in my work. Today, instructed by the frightening degradation of scientific ethics in mathematical circles during the 70s and 80s, I would be much more meticulous than I have been, in carefully indicating my sources, not only in the technical sense, but also in the heuristic sense, which is often far more crucial still. In the historical "Commentaries" already quoted, I intend to make good at least some of my omissions in this respect.

which I have often lacked in other areas of my life, is a blessing in itself. In fact, the fruitfulness and power of my work are due to this simplicity, which is also that of the **child**. ...

## a4. and hindrance

**Note** 171(viii) (May 4)<sup>657</sup> (\*) I'm mistaken here, and my memories have been clarified (and rectified) over the past two months, as I've got back in touch with the subject a little better. In fact, Deligne's main point had been precisely to give this "purely algebraic description" of discrete bundles (of C-vectorials) and the corresponding derived category<sup>658</sup> (\*\*). The coefficients it introduces (via a condition of  $\square$

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ad hoc "constructability" on a pro-crystalline beam, a condition defined by the existence of an "unscrewing". (modelled on the one I'd introduced in the stellar or complex analytic context) are "tailor-made" to meet these desiderata. From then on, it became (heuristically) "obvious" that a formalism of the six operations **should** exist for these coefficients (in zero characteristic), and it should even be possible to describe, "brutally and stupidly", by judicious application of the "Weyl principle" of reduction to the (known) case where the base body is C.

It may seem a mystery, then, if we stop to think about it, that a Deligne should have abandoned an approach that was visibly full of promise, in favor of the description of "categories of coefficients" which (it was already clear in the mid-sixties) were to play a crucial role in the cohomology of algebraic varieties. Eight years later, he left it to someone else to come up with a somewhat dual and more penetrating approach<sup>659</sup> (\*), which would immediately<sup>660</sup> (\*\*) renew the cohomological theme in geometry. It hadn't struck me that much before, given that Deligne's theory was launched shortly before my departure, and that nothing at that time could have foreshadowed the fate that would be reserved for it. After my departure, on the other hand, and practically up until the last few months, I had completely lost touch with the cohomological theme.

It occurred to me recently, rather hastily and without giving it much thought, that the reason for Deligne's disaffection with a theory in which he had invested a whole year, might lie in the fact that he was not satisfied with his criterion-definition of "constructability" by unscrewing. It may have seemed too simplistic, and it's a fact that it's certainly less profound than the local algebraic condition of holonomy and regularity, which Mebkhout identified in 1976 in his "dual" point of view. But on reflection, this

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"explanation" simply doesn't hold water! Surely it's not because an approach to a neuralgic question would be "too simple",  $\square$  that a mathematician in full possession of his means would jettison and the approach, and the question! At the very most, he would abandon his initial approach the day he found another that would enable him to achieve a deeper and more complete vision of this same question<sup>661</sup> (\*)!

<sup>657</sup>(\*) This note is taken from a b. de p. note to the note "The ancestor" (n° 171 (i)), see note (\*) on page 947.

<sup>658</sup>(\*\*) This is the category (noted as  $\text{Cons}^*(X, C)$  in the note "L'oeuvre. . . .", n° 171 (ii)) formed by complexes of C-vector bundles on x, with analytically constructible cohomology bundles, seen as a full subcategory of  $D^*(X, C_X)$ .

<sup>659</sup>(\*) I have no doubt that if Deligne hadn't dropped De Rham's coefficient theme (which he inherited from me), he couldn't help but discover (eight years before the service stranger) the "dual" yoga of the  $D$ -Modules, and thus become familiar with the ideas of the Sato school.

<sup>660</sup>(\*\*) The term "immediately" does not quite correspond to the reality as it was (but rather to the reality "that should have been", if. . . ). In fact, three years elapsed between the moment when the new philosophy and the new tool were ready, and the moment when the people who set the tone realized that there was something there that could be used (and pocketed. . . ).

<sup>661</sup>(\*) In fact, in this particular case, it seems to me that there is no reason to "jettison" Deligne's approach in favor of that of Le Bon Dieu (not to mention Mebkhout). The two approaches complement each other, with Deligne's approach having the advantage of



As soon as I reflect a little on this strange situation, it becomes clear that in this case too, as in many others, my friend Pierre's motivations were not mathematical, nor even "rational". Thinking about it again, I realized to what extent the problematic around De Rham's coefficients, which only made sense from the point of view of the six operations and crystal yoga<sup>662</sup> (\*\*) (yoga that I had introduced a few years before with crystal topos, and precisely in the spirit of the six operations. . . ) - to what extent this whole problematic around De Rham's coefficients made sense from the point of view of the six operations and crystal yoga (\*\*) (yoga that I had introduced a few years before with crystal topos, and precisely in the spirit of the six operations. . . ). ) - the extent to which all these issues were rooted in my work and my person, and **clearly apparent to all**.

It's true that Hodge's problematic of coefficients also came from the same master, from whom the pupil was already, inwardly (and perhaps unwittingly) distancing himself. But the filiation was much less obvious to the outside world (and no one, including Serre, seems to have perceived it<sup>663</sup> (\*\*\*)), and above all: a first tranche of the far-reaching work to be done was not part of an ostentatiously Grothendieckian vision ("six operations" or whatever. . . ), not in a way clearly apparent to all, at least.

□ But it's no accident, as I've pointed out more than once, that Hodge's cohomological theory-Deligne, after a spectacular start at the end of the sixties, is still in his infancy, where the only tolerated coefficients are constant (or, in a pinch, "smooth", i.e. equivalents in the "Hodge-Deligne" sense of local systems), and where such crucial operations as Leray's higher direct images  $Rf_*^i$  (to mention only those) are not used! The question of defining the right notion of "Hodge coefficients" and the relevant operations on them, is not only **mentioned** in Deligne's work (as far as I know), whereas it was already familiar to me, unless I'm mistaken, even before I had the pleasure of making his acquaintance. When, after I'd left and over the years, I'd sometimes ask (I eventually got bored, of course. . . ) what he was waiting for to develop the theory that was needed at the end of the ends, he'd invariably reply: "it's too difficult. . . "<sup>664</sup> (\*). I wasn't convinced, that's for sure - if I hadn't embarked on a completely different adventure, I'd have gone right ahead and developed this "too difficult" theory, and De Rham's coefficients as well... ..

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of being closer to geometric intuition, and Mebkhout's being technically simpler (avoiding recourse to pro-objects), and in various respects deeper.

<sup>662</sup>(\*\*) Incidentally, I remember that in Deligne's presentation of his theory, he systematically avoided recourse to crystalline language, which nevertheless gave his theory a deeper dimension, by inserting it into an already existing toposic cohomological formalism. I also realize that, like Berthelot and my other cohomology students, he had lost sight of the profound **uniqueness** between  $p$ -characteristic crystal cohomology and zero-characteristic crystal phenomena (which were the subject of his seminar). These are signs of a deliberate intention to ignore a fundamental unity, which is arbitrarily fragmented and thereby destroyed. This deliberate intention is in the nature of a "blockage", through the intervention of egotistical forces alien to the drive for knowledge. For an illustration of this blockage in another of my works

cohomology students, whom I have known to be gifted with fine intuition, see sub-note n° 91<sub>2</sub> to the note "Les cohéritiers. . .

". <sup>663</sup>(\*\*) This seems to be clear from Serre's report on Deligne's work, quoted in sub-note n° 165<sub>1</sub> on the note "Requiem pour vague squelette" (notably p. 813). For an explanation of this filiation, see "Les points sur les i" (note

n° 164), I 4 (in particular p. 793), and its sub-note n° 164<sub>1</sub>.

<sup>664</sup>(\*) This answer has recently been combined with "L'Eloge Funèbre" (or burial by compliment), from the pen of Deligne, mentioned again recently (see the note "Les joyaux" n° 170 (iii)). This "Eloge" ends with this question (worth its weight in Pierre. . . ):

He left the IHES in 1970 at a time when his passion for mathematics was waning. Are we to believe that the problems he was tackling along the lines he had set himself **had become too difficult?**" (Emphasis added).

This kind suggestion is taken up again in part 2 of the Eloge, dedicated to Pierre Deligne, where we learn that certain conjectures of the deceased, "today still as unapproachable as then", had undoubtedly been (or so it is clearly suggested) the main obstacle that the aforementioned Deligne had to overcome, to prove a certain conjecture "of proverbial difficulty". These connections make me realize that in my friend Pierre's stereotypical "c'est trop difficile... .", there was an undertone of derision, which must have given him all the more piquant satisfaction, since it was obvious that this big dodo of the deceased was nowhere near suspecting the aforementioned innuendo (any more than he knew he was dead. . . ).

With hindsight, I'm struck by the parallelism between the stagnation in Hodge-Deligne theory on the one hand, and on the other, Deligne's aberrant attitude towards the theme of De Rham coefficients (attitude culminating in the "perverse" iniquity that will remain attached to the memorable Colloque de Luminy of June 1981. . . ).

These two aberrations now appear to me to be intimately linked, and this at a completely different level from the mathematical one. It's true that, visibly, the development of a formalism for Hodge coefficients is **subordinate** to that for De Rham coefficients (something that was obvious to me as early as 1966, and which people seem to have been discovering over the last year or two, on the heels of the work of the student-posthumous-never-named... ). This mathematical fact makes both the link between the two sets of facts, and the aberrant nature of both, all the more striking: for this "objective" link was a powerful additional incentive (for someone at least "in full possession of his faculties") to develop both the one and the other theory, which could then only become clearer and mutually reinforcing.

The stagnation in both theories (until the 1981 Pervers Colloquium in the case of De Rham, and up to the present day in the case of Hodge) is largely responsible for the general stagnation of the cohomological theme, a stagnation to which I have alluded on more than one occasion<sup>665</sup> (\*). Even disregarding the spiritual dimension of the human being, and taking into account only the factors of "profitability" through "cutting-edge" scientific production, this stagnation illustrates for me in a striking way both the unsuspected empire that occult egotistic forces can take over a being, and this even in the exercise of a supposedly "disinterested" science, and the (apparently) aberrant nature of this empire, which here (at first sight at least) seems to constantly run counter to the aim pursued<sup>666</sup> (\*\*).

## b1. The five photos (crystals and D-

Modules) **Note** 171(ix) <sup>667</sup>

<sup>665</sup>(\*) On the subject of this slump, see in particular "Les chantiers désolés" (La Cérémonie Funèbre, 6.), and more particularly the note "Le tour des chantiers - ou outils et vision" (n° 178).

<sup>666</sup>(\*\*) This is the case, at least, if we take as our "goal" the one we have set before the world ("the advancement of Science", let's say), or even the by no means bogus one that would consist in the enlargement of one's prestige, through the accumulation of works forcing esteem and admiration. Yet it seems to me that even this "benefit" is secondary to the satisfactions pursued by the most powerful occult forces, those to which my friend has chosen to give empire over his being.

<sup>667</sup>(\*) This sub-note to the note "L'oeuvre. . . ." (n° 171 (ii)) is of an exclusively mathematical nature. It may be omitted by readers who do not feel inclined to understand Zoghman Mebkhout's work in mathematical terms. "the yoga of  $D$ -Modules", as a new "theory of coefficients" in the cohomological theory of varieties. The following pages can be seen as a short introduction to this yoga, or to Mebkhout's "philosophy", situated in terms of conceptual baggage and a crystalline overall vision. This was already clear to me in 1966.

This vision was systematically, and virtually completely, obscured by my cohomology students Deligne, Berthe-lot, Illusie and Verdier, who had been its principal repositories. The only surviving written trace is the text of my 1966 IHES lectures "**Crystals and the De Rham cohomology of schemes**", notes by I. Coates and O. Jussila, in Dix exposés sur la cohomologie étale des schémas, North Holland Pub. Cie (1968). However, from a technical point of view, this paper contains all the basic ideas of crystal cohomology. Apart from Mebkhout's work, it doesn't seem that any really crucial progress has been made conceptually (or otherwise) - on the contrary, I see a staggering regression from my ideas of the sixties. Unfortunately, these appear only in very fragmentary form, or between the lines, in the cited exposé - the most important gap, here as elsewhere, being the absence of any explicit mention of De Rham's coefficient problems, and of a formalism of the six operations (and biduality) to be established for such coefficients(x). I was able to observe that Mebkhout, although more familiar than anyone else with my written work on cohomology (and that of my students), was entirely unaware of this original problem (until two years ago) - and it seems to me that, from the point of view of the mathematical "substratum" (and disregarding non-intellectual psychological factors), this has been his main handicap to this day.

Hereafter, I will refer to the presentation cited from 1966 by [Crystals].

(x) (June 16) for a correction, see p. b. (\*\*) page 990.

In complex smooth dynamics,  $D_X$  (or simply  $D$ ) refers to the ring bundle (or, more precisely,  $\mathbb{C}$ -algebras) of complex analytic differential operators on  $X$ . A first crucial fact, highlighted by Sato, is that this is a **coherent** ring bundle. A second fact, tautological in nature but also crucial, is that the category of locally free  $\mathcal{O}_X$ -Modules, where we take as

morphisms, not just  $\mathcal{O}_X$ -linear morphisms, but differential operators between such Modules, plunges like a **full subcategory** (but by an a priori **contravariant** functor) into that of the D-Locally free *modules*, using the contrafonctor<sup>668</sup> (\*\*)

$$F' \rightarrow \underline{Hom}_{\mathcal{O}_X} (F, D_d \xrightarrow{\sim} \underline{Op\,diff}(F, \mathcal{O})_X) \quad (1)$$

where  $D_d$  denotes  $D$ , provided with its D-Module structure induced by its canonical D-Module structure **on the right**, which commutes with the operations  $\square$  of  $D$  on the left on itself (which make the second memberp . 989 of (1) a D-Module). This fully faithful functor induces an (anti-) **equivalence** between the full subcategories formed by the free Modules. This does not admit a canonical quasi-inverse functor, "commuting to the restriction to an open" - which is why the first contrafunctor considered is probably not (in general) an equivalence. If  $C$  ( $C$  as "crystal", see below) denotes a locally free D-Module (or even a free D-Module, for that matter), we can certainly associate with it a functorially dependent bundle of  $C$  :

$$C \rightarrow \underline{Hom}_D (C, \mathcal{O})_X \quad (2)$$

This is a contravariant functor, which might seem to provide "the" natural candidate for a quasi-inverse functor of (1). The trouble is that this bundle (2) is not naturally provided with an  $\mathcal{O}$ -structure $_X$  - Module, but only with a  $\mathbb{C}$ -structure $_X$ -Module (where  $\mathbb{C}_X$  is the constant bundle on  $X$  defined by the field of complexes  $\mathbb{C}$ ). When  $C$  comes from a locally free  $\mathcal{O}_X$ -Module  $F$  by the contrafonctor (1), then (2) is canonically isomorphic to the  $\mathbb{C}$ -vector bundle underlying  $F$ .

The functor (1) extends (like any additive functor) to categories of complexes: it transforms a complex of differential operators on  $X$  (in the ordinary sense) into a complex of locally free  $D_X$ -Modules, and the (contra-)  $X$  functor thus obtained is of course fully faithful (for differential morphisms between complexes of differential operators, in the first category of complexes). It is in this sense that D-Module complexes (with locally free components) can be said to **"generalize"** differential operator complexes on  $X$ .

The point of view of complexes of D-Modules has the decisive advantage, over that of complexes of differential operators, of fitting directly into the yoga (first developed in my 1955 article "On some points of homological algebra"<sup>669</sup> (\*\*)) of complexes of Modules on an annulated space, and hence, and above all, into that of **derived categories** (which I had cleared up in the years following the cited article). The crucial notion of **"quasi-isomorphism"** does not appear to the naked eye, when we adopt the point of view of differential morphisms between differential complexes, whereas it becomes manifest when we pass to the complex of D-Modules.

associated. So, more than a **generalization from the** point of view of complexes of differential operators, the point of view introduced by Mebkhout<sup>670</sup> (\*\*) represents a **crucial relaxation**:  $\square$  it's thanks to this point of view p.

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<sup>668</sup>(\*\*) The isomorphism written here is  $u' \rightarrow \varepsilon \circ u$ , where  $\varepsilon : D \rightarrow \mathcal{O}_X$  is the "augmentation"  $\theta \rightarrow \theta(1)$ .

<sup>669</sup>(\*) In Tohoku Mathematical Journal, 9 (1957) p. 121-138.

<sup>670</sup>(\*\*) (June 8) It should read here: introduced by Mebkhout into the Grothendieckian panoply, for the purposes of a new theory of coefficients. It goes without saying that "the  $D$ -Module point of view" is due to Sato, but used in a completely different light.

Thanks to him alone, complexes of differential operators can now be used as "coefficients" for a new cohomological theory, with all the wealth of intuitions that goes with it. If I were to draw a parallel between De Rham's theory of coefficients and that of l-adic coefficients (which, incidentally, was one of Mebkhout's main sources of inspiration in the development of his philosophy), I'd say that this first step of a **conceptual nature**, a "childlike" step, is akin to the one I took (in 1958) in introducing the notion of étale bundle (containing in germ the crucial unifying notion of **topos**). In the same analogy, the "God's theorem" (to be recalled below) is akin to the base change theorem for a proper morphism in étale cohomology, which was (in 1963) **the** first major theorem to kick-start étale cohomology, leading in the space of a few weeks to a situation of almost complete "mastery" of the étale cohomological tool. The analogous work in the D-Modules framework (or more generally in the crystalline framework), to arrive at a mastery of "crystalline cohomology" (or "De Rham", in a broad sense that I saw to such a theory as early as the sixties) - this work still remains to be done, in the seven years since the first major breakthrough was finally achieved by Zoghman Mebkhout.

The new category of coefficients introduced by Mebkhout, which "contains" (in the explicit sense in the note "L'oeuvre. . . .", n° 171 (ii)) both the "analytically constructible discrete coefficients", and the coherent coefficients introduced by Serre (systematized by me into a cohomological theory of "coherent coefficients"<sup>671</sup> (\*)), is that formed by the complexes of D-Modules with **coherent** cohomology bundles (as D-Modules), seen as a full subcategory.

$$D_{coh}^*(X, \quad ) \text{ or } \underline{Cris}^*_{coh}(X) \quad (3)$$

of the usual derived category  $D^*(X, D_X)$ . If we restrict ourselves to complexes with bounded cohomology (for-mating the full subcategory  $\underline{Cris}^b_{coh}(X)$ ), such a "coefficient" is represented **locally** by a complex of D-Modules free of finite type in any degree, and with bounded degrees; or also, which essentially amounts to the same thing, by a complex of differential operators with bounded degrees.<sup>672</sup>

p. 991 □ When working with derived categories, it is of course necessary to replace the fundamental functors (1) and (2) by the total derived functors

$$F \mapsto \underline{RHom}_{\mathcal{O}_X}(F, D_X), \quad C \mapsto \underline{RHom}_D(C, \mathcal{O}_X). \quad (4)$$

If we look for **covariant** functors of a similar nature to these two functors, we first come across the "scalar extension" functor (denoted by  $N$  in the cited note) :

$$F \mapsto D \otimes_{\mathcal{O}_X} F, \quad (5)$$

(total tensor product), where in the tensor product we still use the  $\mathcal{O}_{\text{structure}_X}$ -Module to the right of  $D$  i.e.  $D, {}_d^{673} (*)$  This functor in  $F$  has the disadvantage, with respect to (1), of not extending to morphisms

<sup>671</sup>(\*) This is the formalism of the six operations and biduality, which I developed in the coherent framework in the second half of the 1950s.

<sup>672</sup>(\*\*) (June 16 - see end of note (\*) on page 988). Mebkhout has just pointed out to me that this is not quite correct - this problem is discussed in loc. cit. 1.5 d) (p. 312). Mebkhout refers to it explicitly in his work "Dualité de Poincaré" (Séminaire "Singularités" de Paris VII, 1977-79), in the last three lines of §4.4 (relative duality theorem for -modules).

<sup>673</sup>(\*) It is known that  $D$  is flat as an  $\mathcal{O}_X$ -Module on the right or left (can be seen immediately on the canonical filtration of  $D$ , and the known shape of the associated scale. . . ). As a result, the "total" tensor product in (5) is in fact a tensor product

between arguments  $F \rightarrow F'$  which are only differential operators (instead of linear). The second functor (4), which should be viewed as a contrafunctor

$$\underline{Cris}_{coh}^*(X) \dashrightarrow D^*(X, C)_X,$$

also admits an important covariant counterpart, given by

$$C \dashrightarrow \underline{RHom}_D(\underline{Q}_X, C) \stackrel{\text{dfn}}{=} \underline{DR}(C) \text{ ("De Rham complex" associated with } C), \quad (6)$$

where the second member is indeed explained by a De Rham complex, thanks to the canonical Spencer resolution of  $\underline{Q}_X$  by locally free D-Modules of finite type (this resolution is deduced from the ordinary De Rham complex, taking the associated D-Module complex by functor (1)).

In crystalline terms (to be explained below), the DR functor is expressed as the total derivative functor of the function  $C \mapsto \underline{Hom}_D(\underline{Q}_X, C)$ , associating with each D-Module (or "crystal") the bundle of C-vectors formed by its "horizontal" sections (on variable openings). This is a **local** operation. The good (global) notion of "**integration**" (or **global cohomology object**) for a "coefficient"  $C$  (i.e. a D-Module or complex of such) is not here the usual functor

$$R\Gamma_X(C) = \underline{RHom}_D(X; D, C)$$

but the functor (familiar to me as a **total crystal cohomology** functor) derived totally from the func-

horizontal sections (global)"  $\text{teur } C \mapsto \underline{Hom}_D(\underline{Q}_X, C)$ ; I denote this total derivative  $R^{\Gamma_{cris}}(C)$ , so that by  $\square$  we have tautological isomorphisms p. 992

$$R\Gamma_{cris}(C) \stackrel{\text{dfn}}{=} \underline{RHom}_D(\underline{Q}_X, C) = R\Gamma_X(\underline{DR}(C)), \quad (7)$$

i.e. the crystalline cohomology of  $C$  on  $X$  is obtained by taking the ordinary (global) cohomology of the associated De Rham complex.

In  $\underline{Cris}_{coh}^*(X)$  a **dualizing functor**, giving rise to a biduality theorem, on the model of those I've identified in the coherent (commutative) context first, then discrete (spread out). I'll denote it  $D$  (as in the contexts cited):

$$D: \underline{Cries}_{coh}^*(X) \xrightarrow{\sim} \underline{Cris}_{coh}^*(X). \quad (8)$$

It is an anti-equivalence, essentially involutive (i.e. we have a biduality isomorphism, functorial in  $C$ ):

$$C \xrightarrow{\sim} D(D(X)). \quad (9)$$

(9). This functor transforms (by composition) the contrafunctors (1) and (2) into covariant functors. The simple fact to remember is that if  $C$  and  $C'$  are "duals" of each other, then De Rham's complex (6) of one identifies with "co-De Rham" (2) of the other: (10)

$$\underline{RHom}_D(\underline{Q}_X, C) \xrightarrow{\sim} \underline{RHom}_D(C', \underline{Q}_X), \text{ and vice versa.} \quad (10)$$

On complexes of differential operators, this operation  $D$  is expressed (with a shift of  $n$  on the degrees) as follows

ordinary.

by passing to the "adjoint" differential operator complex, with components  $\underline{Hom}_{\underline{O}_X}(F, \underline{\omega}_X)$ , obtained by taking the adjoint operators term by term. In this way, the dualizing functor for D-Modules is compatible with the dualizing functor familiar from Serre duality,

$$F \mapsto \underline{Hom}_{\underline{O}_X}(F, \underline{\omega}_X) \xrightarrow{\sim} F \otimes_{\underline{O}_X} \omega_X \quad (F \text{ an } \underline{O}_X\text{-Module loc. lib. of finite type}), \quad (11)$$

where  $\underline{\omega}_X$  denotes the "dualizing module" of differential forms of maximum degree on  $X$ . Note that De Rham's functor

$$DR : D_{\text{coh}}^*(X, D) \mapsto D^*(X, C),$$

does not in general commute to dualizing functors (taking in the second category the  $\underline{RHom}$  functor  ${}_C(-, C_X)$ ). But it is a profound theorem of Mebkhout (which everyone uses without quoting anyone, of course, and as if it were a simple sorite) that for **holonomic** arguments, i.e. for the functor induced

$$\underline{Cris}^*(X)_{\text{hol}} \mapsto \underline{Cons}^*(X, C) (\mapsto D^*(X, C))$$

p. 993 there is commutation to dualizing functors. I do not "recall" here the condition of **holonomy**, and confine myself to

point out that a D-Module complex is holonomic if its cohomology bundles are holonomic D-Modules, and that this is a condition of **local** nature on  $X$ , and moreover, "**algebraic**". On the other hand, Kashiwara's constructibility theorem (which he stated for a holonomic **Module**, at a time when neither he nor anyone else - except Mebkhout - was working with derived categories. . . ) implies that the restriction of De Rham's functor to holonomic complexes does indeed lead to  $\underline{Cons}^*(X, C)$ . Introducing the notion of Mebkhout **regularity**, also local and "algebraic" in nature<sup>674</sup> (\*), we find the "God's functor" (aka Mebkhout)

$$m : \underline{Cris}^*(X) \xrightarrow{\text{hol rég}} \underline{Cons}^*(X, C) \quad (12)$$

which, this time, is an **equivalence** (as we saw in the note "L'oeuvre. . . ", n° 171 (ii)), which is therefore compatible with natural dualizing functors. This is the quasi-inverse functor (13)

$$M : \underline{Cons}^*(X, C) \xrightarrow{\sim} \underline{Cris}^*(X, C) \quad (13)$$

which allows us to consider the category of "constructible discrete coefficients" (of  $C$ -vectorials) on  $X$ , as a full subcategory of  $D^*(X, D)$  and more precisely of  $D_{\text{cons}}^*(X, D) = \underline{Cris}_{\text{coh}}^*(X)$ , which we will inter-

sometimes as a category of "crystalline" coefficients.

(May 19) For the time being, we can say that we have described in three different "languages" or "points of view", as if by as many different "photos", the same reality, or (essentially) the "same" type of coefficients", known as "De Rham coefficients": there is the point of view of bundles of  $C$ -vectors and complexes of such (the "topological" point of view), with an "analytic constructibility" condition<sup>675</sup> (\*\*), playing the role of "topological" point of view.

p. 994 a finiteness condition (essential, in particular, to be able to write theorems of the Riemann- Roch type, involving suitable "Euler-Poincaré characteristics" and "de Grothendieck groups"). He

<sup>674</sup>(\*) Please note that Mebkhout's original definition of regularity was transcendental in nature. For a "purely algebraic" translation, I refer you to the planned discussion of De Rham's coefficients ("Mebkhout" style or "Deligne" style), in volume 3 of *Réflexions*.

<sup>675</sup>(\*\*) Recall that a  $C$ -vector bundle on an analytic space  $X$  is said to be "analytically constructible" if, in the vicinity of each point, it admits a composition sequence whose successive factors are of the form  $i_!(F)$ , where  $i : Y \rightarrow X$  is the inclusion of an analytic subspace  $Y = Z \setminus T$  of  $X$  (with  $T \subset Z$  two analytically closed subspaces of  $X$ ), and  $F$  a locally free  $C$ -bundle

of type  $\text{fini}$  (or "local system of  $\mathbb{C}$ -vectorials") on  $Y$ .





there's the "complex of differential operators" point of view, with holonomy and regularity conditions taking the place of constructibility conditions. And there's the "complex of D-Modules" point of view, with coherence, holonomy and regularity conditions. The second "picture" (taken from the "analytic" angle) is seductive, in that it is intelligible to us in "classical" terms, and the objects it shows us, namely complexes of differential operators, appear to us to be of reasonable "dimensions", whereas D-Modules, even coherent ones (starting with  $D$ ) itself!), appear disproportionate when viewed through " $\mathcal{O}_X$ -Modules" glasses. Technically speaking, however, these provide a more complex. Indeed, while it is "clear" that locally, every complex of D-Modules with cohomology can be represented by a complex of differential operators via (1), it's unlikely that this will also be the case globally, unless we make draconian assumptions about  $X$  (such as a "Stein variety" or, in the algebraic framework, a quasi-projectivity assumption)<sup>676</sup> (\*).

Photo 1 has the advantage of making sense when  $X$  is no longer assumed to be smooth, but is a complex analytic space of some kind. On the other hand, as they stand, photos 2 and 3 are reasonable only under the assumption of smoothness. It's true that we can still define a  $D$ -ring bundle $_X$  without the smoothness assumption on  $X$ , and we can still find a tautological dictionary between complexes of differential operators (with components of locally free  $\mathcal{O}_X$ -Modules) and complexes of D-Modules (with locally free components), but  $D_X$  (it seems) ceases to be coherent, too bad! It's unlikely that a "theorem from the good Lord" will ever be able to make sense of the world.

can emerge in the singular case, on the model of that known in the smooth case. On the other hand, it's obvious that we need pictures of genus 2 or 3 in the singular case too, since the picture n° 1 is **transcendental in nature**: naively modeling it, in terms of Zariski or étale topology for a variety algebraic, we would find "coefficients" that are far too particular to be usable (because these topologies are too coarse, compared to the transcendent topology). Photos 2 and 3, on the other hand, restricted for To begin with, these "smooth" fields of view still make sense in "abstract" algebraic geometry (on a zero-square body, let's say, to begin with), which is (for me) their main charm. In other words, it's essential to enlarge them in such a way that the singular varieties are included in the field of vision.

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This didn't seem to bother Mebkhout, who had other things to worry about- When I asked him the question, his immediate idea was as follows. Suppose  $X$  is immersed in a smooth variety  $X'$ , as a sub closed analytic space. Then the category  $\text{Cons}^*(X, C)$  can be interpreted as the full subcategory of  $\text{Cons}^*(X', C)$  formed by objects whose restriction to  $U = X' - X$  is zero (i.e. objects with support in  $X$ ). But this can also be interpreted, by the Good Lord's theorem, in terms of pictures 2 or 3, as the category of "De Rham - Mebkhout coefficients" on  $X'$  whose restriction to  $U$  is zero. It should be easy to check a priori (remaining in the context of "De Rham - Mebkhout coefficients", i.e. that of photos 2,3), that this category, with equivalence itself defined to within a single isomorphism, is independent of the chosen "lissification"  $X'$  of  $X$ . I've done plenty of this myself, and I'm willing to believe it works. If, on the other hand,  $X$  is not "lissifiable", never mind (says Mebkhout), we'll "do cohomological descent" to reconstitute a global category from these local pieces, or else introduce the "site of lissifications" of opens of  $X$ , and work on that. Chances are, we can manage, but instead of a "lissifying site" (improvised by Mebkhout for the purposes of

<sup>676</sup>(\*) Of course, nothing prevents us from constructing a "derived category" from the category of complexes of differential operators on  $X$  and differential morphisms between such complexes, by formally "inverting" the "quasi-isomorphisms" (defined by transition to the corresponding  $D$ -Module complexes). We'll find (I presume) a **full** subcategory of  $\text{Cris}^*(X)$ ,<sup>coh</sup> but probably not this whole category, in the absence of assumptions like "Stein" or "projective  $X$ " (or only, quasi-projective, in the algebraic case).

the retort, in a conversation that remained platonic), a site that seems to me to be highly redundant, why not work with the crystalline site, which has proved its worth (even if it has been forgotten, it would seem, with a touching set, by those who were my students... ) ? And all the more so as it was quite clear to me, as early as 1966, when I came up with the initial ideas for crystalline yoga, that the future "De Rham coefficients" were to be expressed precisely in crystalline terms!

This leads me to pull out of my drawer a photo that has had time to gather dust, poor thing - and yet, once I've blown on it, it looks as good as new, and in perfect focus. In fact, it was one of the first things I thought of when I wrote the note "Mes orphelins" (n° 46) last year (before I'd even met the Burial. . . ), obscurely sensing that it was time...

p. 996 that someone expresses themselves with respect about things that deserve respect. ... Incidentally, ever since Meb-khout<sup>□</sup>m told me about D-Modules (in 1980 - God knows I wasn't "hip" then!), I haven't been able to to think of them as "crystals" instead, and to use the words "*D-Modules*" and "crystals" (from  $\underline{O}_X$ -Modules) as synonyms, with (of course) a marked preference for the latter.

This brings me to the promised fourth photo, the "crystalline" photo. First, let's assume  $X$  is smooth. Giving yourself a D-Module  $F$  on  $X$  is the same as giving yourself an  $\underline{O}_X$ -Module, with an additional structure, which can be expressed in various equivalent ways. One, the tautological one, consists in saying that we

"extends" the operations of  $\underline{O}_X$  on the abelian bundle  $F$ , into an  $X$ -operation of the Ring  $D_X$  (which contains  $\underline{O}_X$ ). Since  $D_X$  is generated by  $\underline{O}_X$  and the additive sub-bundle of derivations, we can see that it's the same thing to give ourselves what's called an "**integrable connection**" on  $F$ , i.e. a law which, at each derivation  $\xi$  on an open  $U$  of  $X$ , associates a " $\xi$ -derivation"  $\theta_\xi$  of  $F$ , linearly in  $\xi$ , and compatible with the "hook" operation of derivations<sup>677</sup> (\*). We can say that this is a structure of a "differential" nature on  $F$ , of order 1.

Since we're in characteristic zero<sup>678</sup> (\*\*), this structure can also be interpreted as a richer structure, a differential structure of infinite order, which I've called a "**stratification**" on  $F$  (which  $F$  then takes the name of "**Stratified Module**"). One way of expressing a stratification is as a "**given**".

p. 997 **infinitesimal descent of infinite order**" on  $F$  (with respect to the morphism  $X \rightarrow$  a point), or more precisely, as the given  $\square$  of an isomorphism, above the formal completion of  $X \times X$  along the diagonal, between the two inverse images of  $F$  (by the two canonical projections  $pr_1$  and  $pr_2$ ), an isomorphism that extends the identity on the diagonal, and moreover satisfies a suitable "transitivity condition".

The transition from an integrable connection to an "infinitesimal descent data" (or stratified structure) represents a new idea - and a "trivial" one, like all the new ideas I've had the honor of discovering! However, it only takes on its full force when reinterpreted in terms of the notion of **module crystals**. We show that the structure in question on  $F$  also comes down to the fact that, for any "neighborhood infinitesimal"  $U'$  of an open  $U$  of  $X$ , an **extension**  $F_{U'}$ , from  $F|_U$  to  $U'$  (in sum,  $F$  "grows" above infinitesimal neighborhoods, such as a "crystal" - a crystal of modules, in this case, but there are crystals of all kinds. . . ) - this extension behaving in the way we can guess, for the notion of restriction to

<sup>677</sup>(\*) Of course, a compatibility condition is also required for the restriction to an open.

<sup>678</sup>(\*\*) In what follows, we can dispense with any characteristic assumption (in the context of a smooth relative scheme, say), by replacing the formal completion of  $X \times_S X$  along the diagonal, by the formal completion "with powers  $S$  divided". This also leads/ for an  $\underline{O}$ -bundle  $\underline{O}_X$ -Module  $F$  on  $X$ , to replace the pro-bundle  $P(F)$  of its "principal parts of order  $\infty$ ", by "principal parts with divided powers (of order  $\infty$ )". On the dual side, this amounts to replacing the  $D$ -ring bundle  $\text{bundle}_{X/S}$  of relative differential operators (which is not coherent even if  $S$  is noetherian), by the "enveloping" ring bundle of relative derivations of  $\underline{O}_X$  on  $\underline{O}_S$  (which, as Mebkhout assures me, would be quite coherent!). This is, in fact, the conceptual context for De Rham's coefficients, which will extend Mebkhout's from the coefficients to the coefficients.  $D$ -Modules, for the development in particular of a theory of De Rham coefficients for finite type schemes over  $\mathbb{Z}$ .

an open  $V$  of  $U$ , and for morphisms between infinitesimal neighborhoods (or "thickenings")  $U'$ ,  $U''$  of the same  $U$  (identity-inducing morphisms on  $U$ , of course).

What's interesting from the crystalline point of view is that the objects to be studied (the D-Modules) can be interpreted as bundles of "ordinary" Modules on a suitable site<sup>679</sup>  $(*)$ , ringed in **commutative local rings**, namely the "crystalline site" formed by the thickenings  $U'$  of the various  $U$ -openings of  $X$  (the bundle

structural crystalline being simply  $U' \rightarrow \Gamma(U', \underline{O}_{U'})$ ). From then on, we have the whole arsenal of intuitions at our disposal

associated with such a situation. A remarkable relationship that I discovered in 1966, and which stunned me at the time, is that the cohomology of the crystal site (or of the crystal topos that corresponds to it), with coefficients in the structural bundle (or more generally, with coefficients in  $F$ , at least when  $F$  is coherent on  $\underline{O}_X$ ), is identified with the **De Rham cohomology** of  $X$  (with coefficients in  $F$ , in this case,

i.e. the ordinary hypercohomology of  $X$  with coefficients in  $DR(F)$ ). This was the start of cohomology crystalline<sup>680</sup>  $(**)$ .

□ Thus we have a perfect dictionary, explained at length in my 1966 talks already cited<sup>681</sup>  $(*)$ , between four object types on  $X$ , or four structure types on an  $\underline{O}_X$ -Module:

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- □ D-Modules
- $\underline{O}_X$ -Integratable connection modules
- Stratified modules (infinitesimal descent data of infinite order) (Cr)
- □  $\underline{O}$  crystals $_X$ -Modules

This dictionary is valid without any coherence or quasicoherence restrictions on  $F$ . Note, however, that if we compare the extreme terms

$$\text{D-Modules} \Leftrightarrow \underline{O}\text{-crystals}_X\text{-Modules}$$

the natural notions of "coherence" in either context **do not correspond**. The crystalline structural bundle is coherent, but the coherent Modules on the crystalline ring topos correspond exactly

to D-modules that are consistent as  $\underline{O}_X$ -modules, in which case they are even free of finite type. The category they form is canonically equivalent, through the "scalar extension" functor relating  $C_X \rightarrow \underline{O}_X$ , to the category of locally free bundles of  $C_X$ -modules, i.e. to that of "**local systems** of C-vectors" on  $X$ . So, for this kind of object, there are five possible descriptions (or five "pictures").

counting the four in table (Cr) above)! But these are "coefficients" of an excessively special nature<sup>682</sup>  $(**)$ , among those (de Rham - Mebkhout) that interest us.

Instead, let's return to the four photos in table (Cr) above, and see what happens when we no longer assume  $X$  is smooth. All four object types remain meaningful. It would seem, moreover, that the two pre-rather, that all  $D_X$ -Modules and all  $\underline{O}_X$ -Modules with integrable connections, which we naturally encounter as "having a geometrical meaning", "originate" (in an obvious sense) from stratified Modules, which moreover can still be interpreted as crystalline rate of  $\underline{O}_X$ -Modules, as in the smooth case<sup>683</sup>  $(***)$ .

<sup>679</sup>  $(*)$  Note that not **all** modulus bundles are found at the crystal site, but only those that satisfy a simple additional condition (called "special" bundles in [Crystals]).

<sup>680</sup>  $(**)$  Here again, start-up ideas are so "trivial" that it's really not worth bothering with the little stuff, when you've spent fifteen years of your life, afterwards, developing a little bit of it (and forgetting the rest. . .).

<sup>681</sup>  $(*)$  See the presentation [Crystals], cited in the first footnote to this sub-note (note  $(*)$  on page 988).

<sup>682</sup>  $(**)$  In fact, it's the  $D$ -coherence, of course (which had escaped me in the sixties) that is the important notion of finitude here.

<sup>683</sup>  $(***)$  this assertion was made hastily, and is false as it stands. For it to be true, the "site" must be replaced by "site".



confess that, because I haven't given it much thought, I can't quite visualize the exact relationship yet, for  $X$

plunged

in  $X'$  smooth (let's say), between crystals on  $X$  and crystals on  $X'$  (and this even when  $X$  itself is smooth)<sup>684</sup> (\*). What is certain is that the crystal site, or better, the crystal topos  $X_{cris}$ , with its ring structure, depends on the analytic space  $X$  in a covariant way, i.e. if  $f: X \dashrightarrow X'$  is a morphism between analytic spaces- we deduce

$$f_{cris}: X_{cris} \dashrightarrow X'_{cris};$$

hence, in particular, a "direct image" functor for bundles of Modules on these ringed topos. We'd like to understand this operation (in the case of a closed immersion  $X' \hookrightarrow X$ , in particular), and understand under what condition a crystal is transformed into a crystal. In the case of a closed immersion,

that this functor is exact. The idea here is this: if  $F$  is an object of the derived category  $D^*(X_{cris}, \mathcal{O})$  and  $F'$  its image by the total derivative functor of  $f_{cris}^*$ , and further assuming  $X'$  smooth, the condition that  $F'$  is

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regular holonomy **should not depend on the chosen immersion of  $X$  in a (smooth) space  $X$** . If this is indeed the case, then we define the category of crystalline De Rham - Mebkhout coefficients on  $X$  as the full subcategory (of the derived category) defined by the previous condition (obviously local on  $X$ ).

Thus, modulus of a fundamental work which should have been done twenty years ago and which apparently still remains to be done (concerning fundamental operations on crystalline modules), we can say that in the case where  $X$  is any analytic space (not necessarily smooth), there remain **two** pictures (instead of four) to describe for us the "De Rham coefficients" to which we have some: there's  $\underline{Cons}^*(X, \mathbb{C})$  ne varietur, and there's the category (which for the moment remains hypothetical, and which as it stands I still can't see<sup>685</sup> (\*\*)) of "De Rham - Mebkhout" coefficients  $DRM^*(X)$ , for which I've just hazarded a principle of definition. The  $\underline{Cons}$  category<sup>\*</sup>  $(X, \mathbb{C})$ , whose description offers no problem from the transcendental point of view, **disappears** as soon as we move on to the algebraic context. This makes it clear that we need a good definition of  $DRM^*(X)$  that makes sense in this context. And it's clear to me, too, that the right "frame" for this picture, which at first sight seems to be the only one left, is the one formed by the crystalline modules<sup>686</sup> (\*\*).

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crystalline", formed by all infinitesimal thickenings of openings in  $X$ , by the subsite (called a "stratified site") formed by those that locally admit a retraction on  $X$  (a condition automatically satisfied when  $X$  is smooth). When we give ourselves a stratified module  $F$  on  $X$ , its inverse image by such a retraction does **not depend**, apart from a single isomorphism, on the chosen retraction, hence a "canonical extension" of  $F$  above the thickening envisaged.

So, when  $X$  is not smooth, a crystalline structure on  $F$  is "richer" than a simple stratification, since it allows  $F$  to be extended (i.e., to "grow") over **any** infinitesimal neighborhoods of openings of  $X$ , and in particular (and this is of particular importance), over infinitesimal neighborhoods of any order of  $X$ , immersed in a **smooth** ambient space. In fact, it turns out that the most crucial and fruitful new notion, between that of the stratified Module and that of the crystal of Modules, is the latter. It is the one that is destined to dominate De Rham's theory of coefficients. Let me "remind" you that, for a clean and smooth relative scheme  $Z$  on  $X$ , the relative De Rham cohomology of  $Z$  on  $X$  (both in the transcendental and algebraic context. . .) is "not only" provided with a stratification, but indeed with a crystal structure, making it "grow" on any infinitesimal neighborhood.

This is a crucial mathematical **fact**, which Deligne had already forgotten before I left in 1969, when he described De Rham-type coefficients in terms of **stratified** procohesive Modules, instead of the stronger crystalline version, i.e. in terms of procohesive Module **crystals**. It has to be said that my name was less notoriously attached to the notion of stratified Modules (so natural you'd swear it must date back to the last century), than to the far less "traditional" looking notion of Module crystals. On this subject, see the reflections in ". . . et entrave" (sub-note n° 171 (viii)).

<sup>684</sup>(\*) (May 26) The situation has become considerably clearer for me with the introduction of the notion of co-crystal, to which it is alluded to in D) below.

<sup>685</sup>(\*) I allude below to a "fifth photo", which is much clearer for me right now, to capture De Rham's "good" coefficients in purely algebraic language in crystalline terms, retaining a sense without smoothness assumptions. This photo is taken from an angle that is somewhat "dual" to that of the De Rham-Mebkhout photo.

<sup>686</sup>(\*\*) I call a "**crystalline Module**" on  $X$  a bundle of Modules on the crystalline ring topos  $X_{cris}$ . We can therefore consider

I must admit that even in the case where  $X$  is smooth, I don't really understand Mebkhout's description of the "de Rham" coefficients, in terms of the good-god functor, which doesn't respect natural multiplicative structures: it's Mebkhout's contra-functor, which we'll be talking about in (b) which (it seems) is compatible with<sup>687</sup> (\*\*\*). A fortiori, this functor does not commute "to the six operations". The in-

tuition which  $\square$  (attaches to Mebkhout coefficients therefore seems very different in nature, at first sight, from p. 1001 discrete coefficients. From a certain point of view, this is an advantage - you have two photos taken from radically different angles! It simply makes it more difficult for those accustomed to looking from one of these angles to recognize themselves in the photo taken from the other.

In fact, in addition to the four photos already reviewed (for "De Rham coefficients", I mean), there's a fifth<sup>688</sup> (\*) that I've been keeping in reserve: it's Deligne's, with stratified pro-modules<sup>689</sup> (\*\*). It has the advantage of "sticking" very closely to the intuition of constructible discrete bundles: an object "of degree zero" corresponds to an object of the same type, the notions of tensor product and inverse image correspond to each other through Deligne's equivalence; so it will be the same for all six operations (which can indeed be described in terms of these two). On the other hand, the operation of passing from the "De Rham - Deligne coefficients"  $DRD^*(X)$ , to the De Rham - Mebkhout coefficients  $DRM^*(X)$ , seems to me to be in line with the "De Rham - Mebkhout coefficients".

principle particularly well understood, in terms of operations (" $\mathcal{O}_X$  -duality") on  $\mathcal{O}_X$  -Modules (at least, initially, for smooth  $X$ ) - I've already alluded to this in a previous footnote<sup>690</sup> (\*\*\*). I have so I have the impression of being on solid, familiar ground, where I can recognize myself,

as soon as I get the chance. I even thought of sketching  $\square$  (in this note the point of view of Deligne, and making p. 1002 the link with Mebkhout's and with the formalism sketched out in my aforementioned 1966 presentations. But this sub-note is getting rather long, and is becoming more and more of a digression! So I prefer to refer the matter to volume 3 of *Réflexions*, where I think I'll also give a description of "good" De Rham coefficients (Deligne style, or Mebkhout, as the case may be) on finite-type schemes over  $\mathbb{Z}$ .

**(b) La formule du bon Dieu** (May 5 and May 21) I'd like to come back to the description of the Mebkhout functor (also called "du bon Dieu").

$$M: \underline{Cons}^*(X, C) \rightarrow \underline{Cris}^*_{coh}(X) \stackrel{\text{dfn}}{=} D^*_{coh}(X, D_X) \quad (1)$$

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module crystals as special cases of crystalline modules.

<sup>687</sup>(\*\*\*) This "so it seems" is a rather flippant way (almost like a "new style". . . ) of glossing over a beautiful theorem, still due to the same stranger on duty (but of more recent vintage, I understand, than the good Lord's). It implies, for example, for two analytically closed subspaces  $Y$  and  $Z$  of  $K$ , the following formula on local cohomology, obviously too beautiful even to be true (and yet. . . ) :

$$R\Gamma^{alg}_{Y \cap Z}(Q_X) \cong R\Gamma^{alg}_Y(Q_X) \otimes^L R\Gamma_Z(Q_X),$$

which I'm sure some handsome gentlemen will pocket one of these mornings, as if "they'd always known" - while waiting to award it to the most handsome among them... .

<sup>688</sup>(\*) So I've done better than live up to the promise of this note's title "The Five Photos": I've actually highlighted **two sets of** five photos, the first describing "De Rham coefficients" alone, and the second crystalline coefficients in general.

<sup>689</sup>(\*\*) As mentioned in a previous b. de p. (note (\*\*\*) page 998 ), this De Rham - Deligne photo was taken with a slightly distorted "lens" (for reasons beyond the manufacturer's control). It needs to be retouched, and also enlarged, by taking it out of the null feature frame. This will be done in volume 3 of *Réflexions*, where my dear ex-students will be able to come and pump out all the "useless details" and other "technical digressions" they haven't had the leisure to find for themselves, in the nearly twenty years since I left them to fend for themselves with a splendid subject in their hands... ..

<sup>690</sup>(\*\*\*) This "previous b. de p. note" has since been transformed into part (c) of the present "Five photos" note.

where  $X$  is a smooth complex analytic space. As we said in the note "The work. . ." (n° 171 (ii)), this is a functor of deep nature, which is defined as quasi-inverse of the restriction functor of the De Rham  $DR$  functor to the full  $DRM$  subcategory\* ( $X$ ) (of "De Rham-Mebkhout coefficients" on  $X$ ) of

$\underline{Cris}^*_{coh}(X)$ ,

$$m = DR/DRM^*(X) : DRM^*(X) \xrightarrow{\text{dfn}} \underline{Cris}^*(X)_{\text{holrég}} \rightarrow \underline{Cons}^*(X, C) \quad (2)$$

which turns out to be an equivalence ("God's theorem"). In fact, Mebkhout obtains a remarkable direct description of the function  $M_\infty$ , deduced from the functor  $M$  by the "scalar extension" functor  $i$  through the Ring homomorphism

$$D \rightarrow D^\infty \quad (3)$$

where  $D^\infty$  (or  $D^\infty_X$ ) denotes the Ring of "infinite-order differential operators on  $X$ ", i.e. (by definition) the ring of "infinite-order differential operators on  $X$ ".

of the (C-endomorphisms of the bundle  $\mathcal{O}_X$ , seen as a bundle of complex topological vector spaces. It is known that  $D^\infty$  is faithfully flat on the left and right on  $D$ , so that the total derived functor of the Ring extension functor

$$i : \underline{Cris}^*(X) = D(X, D) \rightarrow D(X, D) \rightarrow D(X, D^\infty) \xrightarrow{\text{dfn}} \underline{Cris}^\infty(X) \quad (4)$$

is explained by an ordinary tensor product. Note that we don't know whether the  $D$ -ring $^\infty$  is coherent, but apparently we don't need to. We define the full subcategory

$$\underline{Cris}^*(X)_{HO} \xrightarrow{L} \underline{Cris}^*(X)$$

complexes of D-Modules which are "holonomic", by the condition of being locally deduced (by the functor (i) of a D-module complex  $C$  which is holonomic. (It will follow from the double God's theorem, recalled below, that we can then take even  $C$  to be both holonomic and regular, i.e. a "coefficient of  $\square$  (De Rham - Mebkhout)", and this determines  $C$  on all  $X$  to within a single isomorphism. . . ) Consider the functor  $M_\infty = iM$ , fitting into the commutative diagram

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$$\begin{array}{ccc} & \underline{Cons}^*(X, C) & \\ \uparrow M & & \uparrow M_\infty \\ DRM^*(X) & \xrightarrow{i} & \underline{Cris}^*(X)_{\text{hol}} \end{array} \quad (5)$$

$$= \underline{Cris}^*(X)_{\text{holrég}}$$

It turns out (or rather, the unknown worker proves. . . ) that the functor  $M_\infty$  is also a category equivalence (so  $i$  is too). It can also be obtained as a quasi-inverse of the functor  $m_\infty$ , of the "De Rham" type analogous to  $m$ , defined on  $\underline{Cris}^*(X)_{\text{hol}}$ . To describe the functor  $M_\infty$ , it is more convenient to describe the contractor

$$\Delta_\infty \stackrel{\text{dfn}}{=} M_\infty D = D M_\infty = i(MD) = i(DM), \quad (6)$$

where  $D$  denotes the dualizing functor already mentioned, in  $\underline{Cons}^*$  or  $DRM^*$ , and  $D_\infty$ , the dualizing functor similar

that exists in  $\underline{Cris}^*(X)_{HO}$  (and even in  $\underline{Cris}^{\infty coh}(X)$ ). (NB The three functors involved

in (5) commute to dualistic functors). The quasi-inverse  $\delta_\infty$  of  $\Delta_\infty$  is therefore given by the formula

similar to (6)

$$\delta_{\infty} \stackrel{\text{dfn}}{=} Dm_{\infty} = m D_{\infty} \quad (7)$$

We then find the Mebkhout expression of  $\Delta_{\infty}$ ,  $\delta_{\infty}$  by the following two remarkably symmetrical formulas:

$$\begin{aligned} \Delta_{\infty}(F) &= \underline{RHom}_{\mathcal{C}}(F, \underline{Q}) \\ \delta_{\infty}(C) &= \underline{RHom}_D(C, \underline{Q}) \end{aligned} \quad (8)$$

Note that in the first of these formulas, the second member inherits a  $D^{\infty}$ -structure, thanks to the operations of  $D^{\infty}$ ; on the second argument  $\underline{Q}$ , while in the second formula, the second member is interpreted simply as a complex of  $\mathcal{C}$ -vector bundles. The second of these formulas, put there "for the record", is essentially tautological, and simply states that the functor  $\delta_{\infty}$  associates with the  $D$  complex  $^{\infty}\text{-Modules } C$  the complex of differential operators (of infinite order) "adjoint" to that associated with  $C$  (by De Rham's  $DR$  functor  $_{\infty}$ ) - this complex being interpreted as a complex of  $\mathcal{C}$ -vector bundles. (That a complex with constructible cohomology bundles can be found in this way is equivalent to Kashiwara's constructibility theorem).

□ (It is a profound theorem, on the other hand, that the first functor  $\Delta_{\infty}$  transforms constructible bundles into (complexes of)  $D^{\infty}$ -Modules that are holonomic. The finiteness theorem alone implied by this result<sup>691</sup> (\*) (without even mentioning holonomy) is in itself a remarkable new result. Even more extraordinary, however, is **the fact that the two functors are quasi-inverses of each other**. Formally, this fact resembles biduality relations, which can be expressed either in the category  $\underline{Cons}^*$ , or in

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the  $\underline{Cris}$  category  $^*(X)_{HO}$  - except that the "dualizing" contrafunctors (expressed in both cases as

as a  $\underline{RHom}_{\infty}(-, \underline{Q}_X)$ ) connect two **different** categories. It was this formal analogy that led Mebkhout to call the theorem that affirms isomorphism

$$\Delta_{\infty} \delta_{\infty} \stackrel{\sim}{=} id \text{ in } \underline{Cris}^*(X)_{coh} \quad (9)$$

the "**biduality theorem**" for  $D$ -complexes  $^{\infty}\text{-Modules}$  (a potentially confusing terminology). This relation, plus the fact that the functor  $\delta_{\infty}$  is fully faithful (or, more precisely, that  $\Delta_{\infty}$  is an adjoint of it, something he includes in the statement of his biduality theorem) had been obtained by Mebkhout as early as 1977, before the complete God theorem. The so-called "biduality theorem" thus essentially means (as does "my" biduality theorem, from which it is inspired) that a complex of **holonomic**  $D^{\infty}$ -Modules can be **reconstituted**, as an object of a derived category, by knowledge of the associated complex of (infinite-order) differential operators, seen as simply a complex of  $\mathcal{C}$ -vector bundles (in the appropriate derived category); and more precisely, that it can be reconstituted by the explicit inversion formula

(8) (first formula). A fortiori, a morphism between complexes of holonomic  $D^{\infty}$ -Modules is a quasi-isomorphism if and only if the corresponding morphism for complexes of differential operators (of infinite order) is so in the naive sense (i.e. induces an isomorphism on cohomology bundles)<sup>692</sup> (\*\*).

<sup>691</sup>(\*) This finiteness result implies, for example, that locally on  $X$ , the complex  $\underline{RHom}_{\infty}(F, \underline{Q}_X)$  is isomorphic (in the derived category) to a complex of  $D^{\infty}$ -Modules that is locally free of type fini in each degree, and that its Cohomology Modules arise (locally), by scalar extension, from coherent  $D$ -Modules. In fact, we can even assume regular holonomy.

<sup>692</sup>(\*\*) (May 26) In fact (as I point out below, beginning of (c)) Mebkhout proves this last result, even outside any holonomy condition, in the equivalent form: if the complex of differential operators associated with a complex of  $D^{\infty}$ -Modules is quasi-zero, so is the latter (and so are the  $D$ -Modules).

□(Mebkhout's biduality theorem is in some ways "half" of God's theorem

(for  $D^\infty$ -Modules), when the latter is taken in its strongest form, that asserting that functors

(8) are almost inverses of each other. This is the central result of Mebkhout's thesis, submitted in January 1980. But this "half" alone is already a new and (as far as I know) entirely unexpected result. It constitutes a typical result, bridging the gap between Sato's ideas and my own, but in the optic of my long-standing program: to formulate "discrete coefficients" by "continuous" or "differential" means (and from the point of view of derived categories). In this respect, it seems to me that the spirit and inspiration of this result completely escape the problematic of the Japanese school of analysis. Kashiwara's constructibility theorem seems to have represented an "aside", and by no means the starting point for a new theory of coefficients. As the publications for the period between 1976 and 1980 show beyond doubt, Mebkhout was the only one to develop such a philosophy.

Mebkhout had told Kashiwara, who was visiting Paris in January 1978 and had just finished writing his thesis, about his results. At Kashiwara's request, the candid Mebkhout, happy to have finally found someone who seemed interested in what he had to say, sent him to Princeton the hot Chapter III - the one containing, among other things, the so-called "biduality theorem". That was in February 1978. Three years later, this same result appeared (with a pretence of demonstration) in a famous article by Kashiwara-Kawai<sup>693</sup> (\*). It was renamed "reconstruction theorem" for the occasion, and without the slightest allusion to a certain Zoghman Mebkhout. Incidentally, it was also the memorable year of the Colloque Pervers - the glorious year when a certain "new style"<sup>694</sup> (\*\*) conquered with aplomb (and without encountering the slightest resistance. . . ), that part of mathematics, of all places, where I used to feel at home.

. . .

### (c) The fifth photo (in "pro mode")

□(May 21) The "biduality theorem" (9) is from 1977. To prou-

ver the other half of the "God's theorem" for  $D^\infty$ -Modules, which therefore amounted to proving that the functor  $\delta_\infty$  is essentially surjective, a first difficulty was to prove that for  $F$  in  $\underline{Cons}^*$ , and by defining the  $D$  complex $^\infty$ -Modules  $C = \Delta_\infty(F)$  by the first formula (8), that this could be obtained via the functor  $i$ , at least locally on  $X$ , using a (holonomic, regular) D-Modules complex. A priori, according to Mebkhout's ideas (i.e. following the double God's theorem, implying that the functor  $i$  in (5) is an equivalence), the latter should be unique to within a single quasi-isomorphism.

I haven't tried to understand how Mebkhout finally managed to construct this D-Module in his thesis. It seems to me that the situation should be clarified, here, by using Deligne's idea of the procoherent bundle associated with a constructible C-vector bundle  $F$ <sup>695</sup> (\*). This idea had been developed by him in the context of **algebraic** varieties over  $X$ , but should be adaptable mutatis mutandis to the analytic case, provided perhaps that we work "locally" on  $X$ , or on each compact of  $X$ . The procoherent bundle associated with  $F$ , which is therefore (at least on each compact  $K$  of  $X$ ) a projective system  $(F_i)$  of coherent bundles (defined in the vicinity of  $K$ ), can be defined very simply as the bundle that

<sup>693</sup>(\*) M. Kashiwara, T. Kawai, On holonomic Systems of micro-differential equations, III Systems with regular singularities, Publ. RIMS 17, 813-979 (1981). The "reconstruction theorem" plundered from Mebkhout can be found in par. 4 of this long work (received November 1980). The main result of the work is a weakened variant of the fact that the functor  $i$  in (5) is a category equivalence. This is therefore an immediate corollary of Mebkhout's (geometric) theory, a consequence that these authors

are obtained analytically (independently of Mebkhout). For further details, see the sub-note "La maffi a" n° 171 (ii), part (b): "Premiers ennuis - ou les caïds d'outre-Pacifique".

<sup>694</sup>(\*\*) On the subject of this "new style" (of which Kashiwara and Hotta are eminent Pacific emulators), see the note "Les félicitations - ou: le nouveau style" (n° 169 ).

<sup>695</sup>(\*) This is the idea he developed in his seminar at IHES in 1969-70, but then abandoned. On this subject, see sub-note ". . . and hinders" (n° 171 (viii)).



pro-represents the functor

$$J \rightarrow \text{Hom}_{\mathbb{C}}(F, J)$$

on the category of  $\underline{O}_X$ -Module coherent  $J$  on  $X$  (in the neighborhood of  $K \dots$ ), which functor, being exact on the left, is well pro-representable. For example, if  $F$  is the constant bundle  $\mathbb{C}_Y$  on an analytic subspace closed  $Y$  of  $X$ , "extended by zero" over all  $X$ , we find the profaisceau formed by the  $\underline{O}_{X_n}$  where the  $X$  are the infinitesimal neighbors of  $Y$  in  $X$ . (NB The projective limit of this projective system is the completed  $\underline{O}_X$  along  $Y$ ).<sup>696</sup> We see (returning to the general case) that the pro-beam  $(F_i)$  is equipped with a canonical stratification<sup>696</sup> (\*\*). Deligne's idea is that the "functor of  $\square$  (Deligne)" from category  $\text{yp}$  of constructible  $\mathbb{C}$ -vector bundles on  $X$ , to the category of stratified pro-coherent bundles, is **fully faithful**, and thus allows us to interpret the first category (which is transcendental in nature) in terms of a full subcategory of the category of stratified pro-coherent bundles. The latter has a purely algebraic meaning, and the full sub-category in question can also be defined (more or less tautologically<sup>697</sup> (\*)), in purely algebraic terms too. This is the category I'll call

$$\text{DRD}^*(X) \text{ or } \text{Del}^*(X), \quad (10)$$

which constitutes the "**fifth photo**", which I didn't want to explain yesterday<sup>698</sup> (\*\*). I seem to remember that Deligne had taken the trouble to develop his interpretation (and the previous full-fidelity statement) in such a way as to move on to derived categories (at a time when it had not yet been decided by my unanimous cohomology students, led by Deligne, to scrap the latter), and it is indeed the "derived category" version that I designate by the notation (10), of course.

That said, the "algebraic part" in  $\underline{RHom}_{\mathbb{C}}(F, \underline{O}_X)$  should be very naturally definable as an inductive limit (in a suitable sense) of  $\underline{RHom}_{\underline{O}_X}(F_i, \underline{O}_X)$  -and in particular (passing to cohomology bundles), we describe canonical arrows

$$\lim_{\substack{\rightarrow \\ i}} \underline{Ext}_{\underline{O}_X}^d(F_i, \underline{O}_X) \rightarrow \underline{Ext}_{\mathbb{C} \times X}^d(F, \underline{O}_X) \quad (\forall d \in \mathbb{Z}) \quad (11)$$

Using the stratification on the pro-object  $(F_i)$  and the tautological stratification of the second argument  $\underline{O}_X$ , we should be able to define a stratification on the first member of (11), i.e. a D-Module structure, such that (11) is compatible with the operator ring homomorphism  $\square$  (corresponding p. 1008

$D \rightarrow D^\infty$  That said, Mebkhout's Good God Theorem should be able to be made more precise, by saying that (11) identifies the second member with the  $D^\infty$ -Module deduced from the first by extension of the scalars<sup>699</sup> (\*) - which implies in particular that the arrow is an **inclusion**. In this way, the left-hand member must be visualized as a kind of "**algebraic**" (or "**meromorphic**") **part** of the right-hand member (which, for its part, is of the same nature as the right-hand member).

<sup>696</sup>(\*\*) The notion of stratification for a pro-Module is defined in the same way as for a Module - the description given in the previous day's notes (part (a)) applies in principle whenever we have a "relative" notion (such as Modules, pro-Module, relative scheme etc.) admitting a notion of inverse "image", i.e. giving rise to a "fibred category" on the category of "varieties" we're working on. ... Please note that if  $(F_i)$  is a pro-Module, a stratification of it cannot in general be described in terms of a "compatible" stratifying system of  $F_i$  - the objects considered are of a much more general nature than the pro-objects of the stratified Modules category.

<sup>697</sup>(\*) "Tautological" at least in terms of the already familiar dictionary (first developed by Deligne) between bundles of locally constant  $\mathbb{C}$ -vectorials (or "local systems") on the  $Y - Z$  complement of a divisor  $Z$  in an analytic space  $Y$ , and stratified coherent modules on  $Y - Z$  that are "regular" (in Deligne's sense) along  $Z$ .

<sup>698</sup>(\*\*) Finally, this explanation (described as "tautological"! ) is not given here either, at least not immediately. It is given below (page 1011). Note that notation (10) refers to the "derived categories" variant.

<sup>699</sup>(\*) In addition, of course, the first member of (11) (in accordance with Mebkhout's philosophy) must be a **coherent, holonomic** and **regular**  $D$ -Module.

"transcendent").

The general situation becomes considerably clearer on the previous particular example,  $F = i_* (C_Y)$ , where  $i : Y \rightarrow X$  is the inclusion of a closed analytic subspace of  $X$ . Then the second member of (11) is a local cohomology bundle with supports in  $y$  - a **transcendental** invariant, while the first member

$$\lim_{\rightarrow n} \underline{Ext}_{\mathcal{O}_X}^d(\mathcal{O}_{X_n}, \mathcal{O}_X),$$

is the well-known expression I introduced for local cohomology, in the schematic framework. The fiber of this bundle at a point  $x \in Y$  is *nothing* other than the local cohomology, on the spectrum  $X_x$  of  $\mathcal{O}_{X,x}$ , of the structural bundle with supports in the "trace"  $Y_x$  of  $Y$  on  $X_x$ .

This example shows just how close Deligne's idea is to those I developed on the subject of local cohomology in the early sixties<sup>700</sup> (\*\*). In any case, the main theme of Mebkhout's work between 1972 and 1976 was to study arrow (11) in this crucial case.

$$\lim_{\rightarrow n} \underline{Ext}_{\mathcal{O}_X}^d(\mathcal{O}_{X_n}, X^{\text{dfn}}) = \underline{H}_Y^d(\mathcal{O}_{X \text{ alg}}) \rightarrow \underline{H}_Y^d(\mathcal{O}_X). \quad (12)$$

It proves in this case the relationship announced above, and furthermore (something I had omitted earlier from the statement) that the first member of (12) D-Module is **coherent**, and indeed, holonomic and regular. From this point on,

the analogous statement for (11) must be an immediate consequence by unscrewing<sup>701</sup> (\*\*)' □ (including in the case where  $F$ , instead of being a constructible  $C$ -vector bundle, is a complex in  $\underline{Cons}^*(X, C)$ ). The only grain of salt, apart from the Deligne functor construction, is in the definition of the  $\underline{RHom}_{\mathcal{O}_X}$  of a complex of stratified promodules, with values in a complex of stratified Modules i.e. in a complex of D-Modules (in this case,  $\mathcal{O}_X$ ), as a complex of D-Modules (and as the object of a derived category).

Modulo this grain of salt, we thus find a simple and conceptual description of the "algebraic" good-god functor  $M$  (as opposed to the "transcendental" good-god functor  $M_\infty$ ), or rather of the associated contrafunctor  $\Delta$  and its quasi-inverse  $\delta$

$$\Delta = MD = DM, \quad \delta = mD = Dm, \quad (13)$$

by a double-formula that paraphrases (8). But to write it, using Deligne's equivalence

$$Del: \underline{Cons}^*(X, C) \xrightarrow{\sim} DRD^*(X) \quad (14)$$

we'll look instead at the corresponding functors  $\Delta^\wedge, \delta^\wedge$  between  $DRD^*(X)$  and  $DRM^*(X)$ , where the  $^\wedge$  signs are meant to remind us that we'll be working (on the "constructible" side) with **pro-objects**. We then find the remarkable formulas (morally contained in (8), but this time linking coefficients "of an algebraic nature"

<sup>700</sup>(\*\*) It will become clear below that Deligne's idea is also intimately linked to the one I introduced in 1966 (in [Crys- tals]): for any complex of differential operators, I consider its "formalized"  $P^\infty(L)$  as a complex of stratified pro-modules or, better still, as defining a **crystalline complex**, whose (global) crystalline cohomology is identified with the (global) cohomology of  $L$ .

<sup>701</sup>(\*\*\*) (May 22) I'm a bit lively here! The "elementary types" of  $C$ -constructible bundles are of a more general nature than just  $C_Y$ , (But it's true that the proof of the general theorem uses the same technique as the 1976 special case.)

both, and this by formulas "of an algebraic nature" as well):

$$\begin{aligned} \Delta^{\wedge}(C') &= \underline{RHom}_{\mathcal{O}_X}(C', \underline{Q}_X) \\ \delta^{\wedge}(C) &= \underline{RHom}_{\mathcal{O}_X}(C, \underline{Q}_X). \end{aligned} \quad (15)$$

So here we have twice the "same" formula, with the only difference that  $C'$  is here a complex of stratified pro-coherent beams (or what amounts to the same thing<sup>702</sup> (\*), a complex of pro-coherent Module crystals), whereas  $C$  is a complex of D-Modules (which can be seen, morally, as a complex of D-Modules).

of  $\underline{Q}_X$ -Layered Indo-Coherent Modules, or as a crystal of Indo-Coherent Modules). It's essentially the "same" functor that passes from one to the other, namely, the "ordinary dualistic functor" (coherent), my old friend from the fifties. ... It's "obvious", of course, that it must exchange pro-objects and ind-objects (even if it means going to the inductive limit in the latter. . . ).

□(Of course, there's some groundwork to be done, to give precise meaning to these formulas - a work of the p. 1010 of the kind made by Deligne in his famous scuttled seminar, or by Jouanolou in his equally famous scuttled thesis (which everyone has been quoting since the Colloque Pervers, and which no one has actually held in his hands... .). I'm sure this work will be a little long, but essentially "sorital". The "hard" part is contained in Mebkhout's theorem of the good God, completed by Mebkhout's formulas

(8) known (perhaps improperly) as "biduality" formulas. Their algebraic translation, on the other hand, asserting that the two functors (15) are quasi-inverses of each other, is indeed (morally) "the" theorem of ordinary biduality for  $\underline{Q}$ -coefficients $_X$ -cohesive, with ind-pro sauce and stratifications (which must "pass" without problems into the dualistic functor).

The correspondence between the two types of dual objects can be visualized perfectly (without any groundwork involved!) in terms of complexes of differential operators. (In this duality, by the way, the condition of holonomy (and a fortiori, that of regularity) plays no role). At such a complex  $L$ , the functor  $F \mapsto \underline{Hom}_{\mathcal{O}_X}(F, D_d)$  (contravariant) considered yesterday (in (a),(1)), associates a complex of D-Modules with locally free components of finite type, i.e.  $C$ . On the other hand, the "formalization" of this complex  $L$ , passing to the principal parts of infinite order  $P^{\infty}(L^i)$  (regarded as stratified promodules) provides a complex  $C' = P^{\infty}(L)$  of stratified pro-modules. Having said this, we can see that these two complexes correspond to each other by the formulas (15), in which here, obviously, the  $\underline{RHom}$  reduces to  $\underline{Hom}$ . (It's enough to check that

term-to-term duality for the components  $L^i$ , and it then reduces to the more or less tautological fact that the "continuous" linear homomorphisms  $P^{\infty}(L^i) \rightarrow \underline{Q}$  correspond exactly, just like the linear homomorphisms  $L^i \rightarrow D$ , to the differential operators  $L^i \rightarrow \underline{Q}_X$ , using respectively

the "universal" differential operator (of infinite order)  $L^i \rightarrow P^{\infty}(L^i)$ , and the "augmentation"  $D_X \text{ data} \rightarrow \underline{Q}$

by  $\theta \mapsto \theta(1)$ ). Since at least locally on  $X$ , any object of  $\underline{Cris}^*_{coh}(X)$ , (i.e. any complex of D-Modules with coherent cohomology) is described using a complex of differential operators  $L$ , we can consider that, for all practical purposes, this particular case gives a perfect grip on the duality (15) between the two types of coefficients, provided we make D-coherence and "D-pro-coherence" assumptions on  $C$  and on  $C'$ , "dual" to each other. It would then suffice to develop the "sorite" to which I have alludes, limiting itself, on the  $C'$  or "pro" side, to complexes of procoherent (stratified bundles that, p. 1011 can be described (with near-isomorphism) as a  $P^{\infty}(L)$ ).

Compared to Deligne's original approach, the fact that the pro-coherent and complex Modules of such that he introduces, can be realized locally by a complex of differential operators, is moreover an **entirely unexpected phenomenon**, brought about by Mebkhout's theory. It seems to me essentially equi-

<sup>702</sup>(\*) See b. de p. note (\*\*) on page 1006, about this translation.

are worth<sup>703</sup> (\*) to Mebkhout's theorem mentioned above (dating back to 1976, even before the Good God theorem was demonstrated), concerning the D-coherence of  $\underline{H}^d_Y(\underline{O})_{Xalg}$  (shown in (12) above). This is a profound theorem, the culmination of four years' work, using the full force of the resolution of Hironaka's singularities (not to mention the courage of the worker who found and proved it, against general indifference). The consequence<sup>703</sup> (\*) that I have just pointed out is a profound relationship between De Rham coefficients (as I saw them from 1966 onwards) and complexes of differential operators, a relationship that I had in no way foreseen (nor had Deligne, when he developed his first approach to De Rham coefficients). As for the condition of holonomy and regularity on the complex of differential operators under consideration, it must be equivalent (a posteriori, thanks to the providential theorem of the good Lord) to Deligne's condition of "finiteness" (plus "regularity") (which I failed to make explicit earlier, when introducing the *DRD* category\*  $(X) = Del^*(X)$ ). It's as follows: the cohomology pro-beams of  $P^\infty(L)$  can be "unscrewed" locally by composition sequences, in such a way that the successive factors can be "unbundled".

can be described (via the Deligne functor) by local systems of C-vectorials on  $Y - Z$  subspaces of  $X$  (where  $Z \subset Y \subset X$  are closed analytic subspaces of  $X$ ). To complete this criterion algebraic" aspect, it suffices to replace the local system of C-vectorials by a **coherent** bundle stratified on  $Y - Z$ , subject to the condition that the connection expressing the stratification (NB we can assume  $Y - Z$  smooth) is "regular" in the vicinity of  $Z$ , in the sense of Deligne<sup>704</sup> (\*\*). (NB. The associated pro-beam is obtained by growing the  $\square$  (crystal we have on  $Y - Z = F$  over the infinitesimal neighborhoods of  $F$ , and "crushing" along  $Z$ , to have coherent beams everywhere, not just in the complementary of  $Z$ . ... )

p. 1012

**(d) Crystals and co-crystals - fully faithful?** When  $X$  is no longer assumed to be smooth, what remains to describe "De Rham coefficients" on  $X$ , in addition to the transcendental "photo"  $\underline{Cons}^*(X, C)$ , are the two "photos" (both crystalline in nature)  $DRM^*(X)$  or  $Del^*(X)$ , which have a purely algebraic meaning. Yesterday (in (a)), I outlined a principle of definition for  $DRM^*(X)$ , and today for the *DRD* category\*  $(X)$ . It's the latter that I now find perfectly intelligible. As I pointed out yesterday (see (a), b. de p.(\*\*\*) page 998), the point of view of stratified pro-Modules needs to be refined by that of crystals in (pro-coherent) pro-Modules<sup>705</sup> (\*). The only remaining problem with this point of view is the "pro" sorite it will force us to develop, a sorite which (in my modest experience in such matters) is likely to take on prohibitive dimensions! These promodule crystals, which associate, to each infinitesimal thickening  $U'$  of an open  $U$  of  $X$ , a pro-coherent Module on  $U'$ , "in a way compatible with inverse images" for morphisms  $U'' \rightarrow U'$  of thickenings, cannot even be interpreted as pro-beams on the crystal site (or what amounts to the same thing, on the topos

<sup>703</sup>(\*) (May 26) Here again, I'm "a bit lively", the 1976 result is not enough. Compare with commentary on b. de p. note (\*\*\*) page 1008.

<sup>704</sup>(\*\*) This condition of regularity is introduced here in a natural way, given the equivalence of categories identified by Deligne, between local systems of C-vectorials on  $Y - Z$ , and  $\mathbb{C}$ -bundles with integrable connection on  $Y - Z$ , provided with a "meromorphic structure" along  $Z$ , and with regular connection along  $Z$ . This meromorphic structure (implying the possibility of extending the coherent Module on  $Y - Z$  into a coherent Module on  $Y$ , at least locally in the vicinity of each point of  $Z$ ) was implied in the description given earlier.

Unless I'm mistaken, when we drop the regularity condition into the previous condition (simply assuming a given meromorphic structure of  $E$  in the neighborhood of  $Z$ , so as to be able to associate a pro-coherent Module over  $X$  as a whole, by Deligne's procedure), we find a "cohomological" description of the holonomy condition. Sato's definition is "microlocal" - I've never really got to grips with it yet, I confess. ...

<sup>705</sup>(\*) (May 27) On reflection, I even find it hard to believe that Deligne's theorem  $\underline{Cons}^*(X, C) \cong \underline{Del}^*(X)$  is true for  $X$  not smooth,  $\underline{Del}^*(X)$  is a challenge, as Deligne does, without recourse to the crystalline site. It is perhaps even for this reason when that he finally preferred to scuttle the whole theory, rather than agree to reintroduce the taboo site. ... (Compare note ". . and hindrance", n° 171 (viii).)

crystalline  $X_{cris}$  )! So we cannot a priori apply to them the known cohomological formalism of bundles of Modules on (commutatively) annelated topos, such as  $X_{cris}$  .

The temptation here is to move to the projective limit of the profied beam on each thickening. We thus find crystalline Modules (if not crystals in Modules), whose "value" on each  $U'$  has not nothing coherent or quasi-coherent. The hope is that, at least for  $\square$  (the type of crystal pro-Modules we are interested in (in particular, those obtained by Deligne's functor) such a pro-module crystal can be **reconstituted** from the crystalline Module  $C$  deduced by boundary crossing, by taking on each thickening  $U'$  "the pro-coherent envelope" of the Zariskian bundle  $C_U$  , (restriction of  $C$  to the Zariskian openings of  $U$  )<sup>706</sup> (\*). This seems to me to be the case, at least for the associated pro-module crystals to a coherent Module stratified on a  $Y - Z$  as above, for example in the typical case where we take the formal completion of  $\underline{O}_X$  along  $Y - Z$  and extend it by zero elsewhere (and so on the thickenings). If my "hope" is justified, then the  $DRD$  category\* ( $X$ ) of De Rham - Deligne coefficients on  $X$  could be interpreted as a full subcategory of the ordinary derived category  $D^*(X_{cris}, \underline{O})$ , defined by conditions of the "finiteness" and "regularity" type (themselves described in terms of unscrewing, as above) on cohomology bundles. This would be a disconcertingly simple description, which I could just as easily have given as early as 1966, had I then had the leisure to continue my crystalline reflection... ..

p. 1013

This "foundation" question (whether it's permissible to go to the limit) obviously doesn't depend on whether  $X$  is smooth or not - if it isn't, we plunge it into a smooth  $X'$  and reduce ourselves to the smooth case. If this point of view (almost too good to be true!) did indeed work, then (in the smooth case now) there would be reason (I think) to interpret the "biduality" formulas (algebraic version) (15) as follows being **ordinary**  $RHom_{\underline{O}_X}$  , without bothering with pro-questions (but simply taking care to transport stratifications. . . ). A first test along these lines would be as follows: if  $u : C_1 \dashrightarrow C_2$  is a morphism of D-Module complexes with coherent cohomology, such that its image by the naive dualizing functor  $RHom_{\underline{O}_X}(-, \underline{O}_X)$  is a quasi-isomorphism, is the same true for  $u$ ? But this amounts (by a mapping-cylinder argument) to asking whether a complex of D-Modules with coherent cohomology, such that its "naive dual" is zero (in the sense of derivative cat., i.e. with zero cohomology bundles), is itself zero (in the same sense). Or, if we have the complex of differential operators  $L$  , is it the same to say that the associated complex of D-Modules has zero cohomology bundles, or is it the same for the "formalized" complex  $P^\infty(L)$  , seen this time not as a complex of pro-bundles, but as a complex of ordinary bundles (passing to  $\lim_{\leftarrow}$  ). Mebkhout will surely be able to tell me... .

$\square$ ((May 23) I phoned Mebkhout again last night - in fact, it's been a week or two since I've I phone him almost every night, for mathematical or historical questions - and all in all, that's going to add up to an astronomical phone bill! But the Apotheosis, which I've been working on and polishing for the last three weeks, is well worth it...

p. 1014

In any case, Zoghman has guaranteed me a result that seems to be close to the "test question" on which the I finished last night: if  $C$  in  $\underline{Cris}^*_{coh}$  is such that the associated  $L$  operator complex  $= DR(C)$  is quasi-zero, then  $C$  is itself quasi-zero (analytic case). We have a homomorphism of bundle complexes (of  $C$ -vectors), given by the "principal parts of infinite order".

$$L \dashrightarrow P^\infty(L),$$

<sup>706</sup>(\*) In speaking here of a "Zariskian" beam (as opposed to a "crystalline" one), I've surreptitiously slipped back into the schematic context. Readers who prefer the analytical context will have rectified this on their own.

hence homomorphisms

$$\underline{H}^i(L) \rightarrow \underline{H}^i(P^\infty(L)) \quad (i \in \mathbb{Z}) \quad (16)$$

on cohomology bundles. We're tempted to say that this homomorphism (16) is always injective, and identifies the first member with the sub-bundle of "horizontal" sections of the second (which would be a kind of exactness property of the functor "bundle of horizontal sections" on a suitable category of stratified pro-Modules... ). Injectivity would already imply that if the second member is zero, so is the first, so if this is true for all  $i$  (and according to what Mebkhout assures me) the D-Module complex associated with  $L$  is quasi-zero - which is what I wanted.

Injectivity in (16) also means that for a differential operator  $E \xrightarrow{d} F$ , and a section  $f$  of  $F$  which at each point  $x \in X$  is "formally" in the image (by passing to the completed local ring of the point), and such that moreover, that the "formal solution" (of the equation  $d(g) = f$  in  $g$ ) can be taken, for  $x$  variable, analytically dependent on  $x$  - the equation then locally admits a solution. Mebkhout tells me that he knows of no such result; yet the question is so natural that the answer should well be known!

To conclude with the "five photos", I'd like to return to the two "crystalline photos", one corresponding to Mebkhout's view of the D-Modules, the other to the dual view. It goes without saying that we have to work in the spirit of the derived categories - i.e., a "crystalline" interpretation worthy of the name.

of this name must take this into account. So the two crystalline photos are "fully faithful" only if the corresponding functor,  $\square$  (going from category  $D_{coh}^b(X, D)$  (say), to a crystalline idon category, such that  $D^b(X_{cris} \rightarrow \underline{O}_{X_{cris}})$ , is itself fully faithful. I am hopeful that this is indeed the case, **without even bothering with holonomy and regularity conditions** on the D-Module complexes under consideration.

The simplest case is undoubtedly that of photo n° 4, which consists in interpreting the category of D-Modules as that of Module crystals, hence a total derived functor (known as "Grothendieck's" - to take the lead over fans of "useless details" and "technical digressions" . . . ) :

$$J : D_{coh}^*(X, D) \rightarrow D^*(X_{cris}, \underline{O}_{X_{cris}}) \quad (17)$$

The crucial question here is whether this functor is fully faithful. Only then is the notation  $\underline{Cris}_{coh}^*(X)$  for the first member is fully justified - and with it, also, the crystalline point of view in De Rham cohomology (at least, in this case, in the complex analytic framework, or the framework of algebraic schemes over a zero-square body). To prove full fidelity, in algebraic geometry let's say, we are reduced by standard arguments to the case where  $X$  is affine (or, in the analytic case, to the case of a polydisc), and to the case where the two objects  $C, C'$  considered in the first member (whose  $Hom$  are to be compared in either direction) are both equal to  $D$  itself, with simply a shift of degrees. (This reduction is straightforward, at least if we assume that  $C, C'$  have bounded degrees, i.e. that we are bounded at  $D_{coh}^b(X, D)$ , which seems to be quite sufficient for applications,) We are therefore led to check finally the formulas

$$\Gamma(X, D_X) \xrightarrow{\sim} Hom(J(D), J(D)), Ext_{\underline{O}_{X_{cris}}}^i(X_{cris}; J(D), J(D)) = 0 \text{ for } i > 0. \quad (18)$$

(for affine  $X$ , resp. Stein). I haven't taken the time to check it<sup>707</sup> (\*), but have little doubt that it's true. I demonstrated something very similar, it seems to me, in [Crystals] (in 1966)<sup>708</sup> (\*\*).

<sup>707</sup>(\*) I apologize, as most of my time over the last year or so has been taken up with tracking the prowess of some of my former pupils... .

<sup>708</sup>(\*\*) This is the result I've already alluded to elsewhere, that for a complex of differential operators  $L$  on a scheme

□(As for photo five, there are several different prints of it. Deligne's original print is in terms of stratified pro-coherent modules. The first important change, with a view to generalization to the non-smooth case  $X$ , is to interpret the animals in question as pro-module **crystals**. But this leads us into the (rather unpleasant!) spiral of endless pro-foundations of pro-cohomological algebra - and we lose the benefit of the direct topological intuition attached to  $X_{cris}$ . So I prefer (if at all possible) to take another photo, from more or less the same angle, using a **contravariant** functor (also known as a "Grothendieck functor", mind you... ).

$$J^o: D_{coh}^*(X, D)^{opp} \rightarrow D^*(X_{cris}, \underline{O}_{X_{cris}}). \quad (19)$$

This can be said to be the one deduced from Deligne's photo by passing abruptly to projective limit bundles on each infinitesimal thickening of an open  $U$  of  $X$ . If  $C$  in the first member is associated (countervariantly, as in formula (1) of (a)) with a complex of differential operators  $L$ , its image by (19) is obtained by looking at  $P^\infty(L)$  (the "formalization" of the complex  $L$ ) as a complex of stratified promodules (an idea introduced in [Crystal]), or as a complex of pro-module crystals, and passing to the projective limit on any thickening. Another way of saying this is that any locally free  $\underline{O}_X$ -Module (for example)  $L$  on  $X$ , is associated with a crystalline module (which is not **not** a crystal of modules, unless I'm mistaken), which I note  $P^\infty(L)_{cris}$ , in an "obvious" way (and which my students have long since forgotten), which module depends functorially on  $L$  with respect to differential operators, and thus passes to complexes of differential operators.

Either of the previous descriptions of the functor (19) remains incomplete, not least because an object of the first member does not necessarily originate, on all  $X$ , from a complex of differential operators. I assume that an intrinsic interpretation of this heuristic description can be given by the formula

$$J^o(C) \rightarrow \widetilde{RHom}_{\underline{O}_{X_{cris}}}(J(C), \underline{O}_{X_{cris}}) \quad (\text{where } J \text{ is defined in (17)}) \quad (20)$$

but haven't checked that it's correct. By the standard arguments, we still come back here (to prove that the natural arrow (20), when  $C$  is associated as above with  $L$ , is indeed an iso) to the case where  $C = D$ , and then (20) reduces to the formulas □ (

$$\underline{Ext}_{\underline{O}_{X_{cris}}}^i(J(D), \underline{O}_{X_{cris}}) = 0 \text{ for } i > 0, \quad (21)$$

which are quite similar to (18).

The meaning of the full fidelity of (19) is in any case quite clear, and once again reduces, by unscrewing (and as for (17)) to the case where  $C = D$ ,  $C' = D[i]$  (shift of degrees by  $i$ ), and then reduces to the formulas

$$\Gamma(X, D) \simeq Hom(P, P), \quad \underline{Ext}_{\underline{O}_{X_{cris}}}^i(X_{cris}; P, P) = 0 \text{ for } i > 0, \quad (18.1)$$

where

$$P = P^\infty(\underline{O})_{X_{cris}},$$

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relatively smooth (or in the analytic framework, surely), the "Zariskian" hypercohomology of  $L$  is identified with the crystalline hypercohomology of its formalized  $P^\infty(L)$ . In fact, this statement relates more directly to the "dual" arrow (19) of (17), and can also be expressed by saying that for  $C, C'$  cohomology-coherent complexes of  $D$ -Modules, the arrow

$$Hom(C, C') \rightarrow Hom(G^o(C), G^o(C'))$$

is bijective, in the case where  $C = \underline{O}_X$  (which is not bad at all, and allows every hope... ).

which is a remarkable Crystalline Algebra on  $X$ . We assume here (for the nullity of  $Ext^i$  crystalline) that  $X$  is affine (resp. Stein).

Finally, what seemed to me only yesterday "almost too good to be true", when I was still seeing things through Deligne's photo, is suddenly looking quite reasonable - once things are written without being encumbered by conditions of holonomy (and even less, regularity). God willing, and if no one else does the job for me before then, I hope to get to the bottom of this (and the validity of (21) and (18)) before the end of the year, with the part of Volume 3 of Reflections devoted to De Rham's coefficients.

As I said, I prefer photo five, the one that "sticks" most closely to the topological intuition associated with discrete coefficients. It's with a heavy heart that I'd learn that formulas (22) are false (whereas I'd be less annoyed if this were true of formulas (18), which, however, look technically less screwed up). This would show that we'd have to go back to the pro-point of view (of Deligne's retouched photo) - not such a cheerful perspective! In any case, there's no doubt in my mind that, apart from a few technical adjustments, this is an excellent photo, particularly valid in algebraic geometry (and even on something other than bodies of zero characteristic), and without any assumption of smoothness.

As for photo four, whose fidelity depends on the validity of (18), I confess once again that I still "don't see it right" outside the smooth case (and even in the smooth case), and I'm not sure that for  $X$  not smooth, the crystalline interpretation I've proposed really works as it is. However, it seems to me that that my endemic perplexities of variance, concerning Mebkhout's view of D-Modules (and more importantly, my crystalline interpretation of this view), are about to be resolved, by the introduction of from a dual notion to that of crystal, which I call **co-crystal**. It was only yesterday that this diffuse feeling of unease that I had (for the "variance" of D-Modules by closed immersions) finally gave birth to a "good notion" (as far as I can tell, without having really written anything yet). It seems to fit on the "ind" side, as well as the notion of crystal (which is familiar to me) on the "pro" side. On a smooth variety, the two categories (crystals and co-crystals) are canonically equivalent (and that's why I inevitably tended to confuse them - it's excusable...), but the same is no longer true for any  $X$ . The situation is quite analogous to what happens with the cohomology ring  $H^*(X)$  and the cohomology group  $H_*(X)$ , or the Chow  $Ch$  ring  $Ch^*(X)$  and the Chow  $Ch$  group  $Ch_*(X)$ , or the Grothendieck ring  $K^*(X)$  and the Grothendieck group  $K_*(X)$  (re-excuses). Here too, for a long time, the two types of objects were confused when  $X$  is a smooth variety (topological, or algebraic etc. - depending on the case). This is "explained" after the fact, by the fact that the second term is in any case provided with a modulus structure on the first (the "cap"-product - in the last two cases this was introduced by an ancestor I dare not name here. ...), and that in the smooth case, we find that this Module is free of rank 1 and provided with a canonical basis, which has led to its unfortunate confusion with the ring (much more beautiful, of course). Well, it's the same for the categories  $\underline{Cris}^*(X)$  of the crystals of Modules on  $X$ , provided with a structure

"by the tensor product, and the  $\underline{Cris}_*(X)$  of module co-crystals, on which the former "operates" by a cap-product, perfectly!

But it's time to stop this long mathematical digression, entirely out of place (I admit) in the ordering of a fine Funeral Ceremony. Readers interested in the rest (which, it goes without saying, is quite lengthy) will be reduced to buying volume 3 of Réflexions (if they don't pity their pennies), where an unrepentant defunct intends to continue his confusing "technical digressions"<sup>709</sup> (\*).

<sup>709</sup>(\*) This time, needless to say, as the "collaborator" of another of my students, who has long since been promoted to "father" of the



**(e) L'ubiquité du bon Dieu** (May 27) A "final" footnote, added to the "Five photos" at the last minute yesterday (before I typed the first twelve notes of the Apotheosis), has taken on yet "more". prohibitive dimensions", and I will  $\square$  (finally continue "this long mathematical digression" with a last (and short) section. Thus, "The Five Photos" will consist of the **five** sections (a) to (e) - just as everything gets rounded off and perfected. ...

p. 1019

This is a commentary on the true (presumed) domain of validity of Mebkhout's "theorem of the good God", which goes far beyond (in my opinion) the initial framework of complex analytic spaces - not only in terms of the new **philosophy** it brings (and which has already renewed the cohomological theme), but also in a technical sense.

Once we interpret constructible C-vector bundles on (smooth)  $X$ , either in terms of stratified procoherent Modules (à la Deligne), or (by passing to the projective limit on infinitesimal thickenings of openings of  $X$ ) in terms of crystalline bundles (à la Grothendieck), the "theorem of the good God" alias Mebkhout affirms the equivalence of two categories, **both of** which are "purely algebraic" in nature. In other words, this theorem now takes on a precise meaning, in contexts other than the complex analytic context: both the context of smooth schemes over a body (which need not even be assumed to have zero characteristic - see on this subject the note by b. de p.(\*\*) page 996 above; because  $p > 0$ , the "crystalline with divided powers" point of view is essential here), or rigid-analytic varieties of any characteristic, or smooth schemes of finite type over  $\mathbb{Z}$  (and so on. . . ).

The "formal" part of the Good God Theorem concerns **all** coherent D-Module complexes, not just holonomous ones, and states that the Good God functor, revised by the ancestor (i.e. the duality with respect to the structural beam  $\mathcal{O}_X$  essentially) is **fully faithful** to the category  $D_{coh}(X, \mathcal{O}_X) = \underline{Cris}^*_{coh}(X)$ , to the desired coefficient category  $\underline{Coeff}^*$  selected by the

taste). When you get it right, it should be more or less "sorital".

But in the arrival category, we define, "by unscrewing", two remarkable full subcategories, that of "holonomic.coefficients" resp. that of "regular holonomic coefficients" (as at the end of (c)) and in the note of b. de p.(\*\*) page 1011). That said, the "generalized Mebkhout theorem" (in the context envisaged), which will certainly have nothing sorital about it but is surely profound, will say two things:

1.  $\square$  (The  $\underline{Coeff}^*_{HOL}$  of holonomic "coefficients" is in the  $\underline{Cris}$  category image\*  $\underline{Cris}^*_{coh}(X)$  by the (fully faithful) "Mebkhout-Grothendieck" functor. (NB. Morally, this functor is the of Mebkhout, but watched on  $\underline{Cris}^*_{coh}(X)$  in its entirety, and "revised and corrected by the ancestor", so that the goal is in  $\underline{Coeff}^*$  which has a purely algebraic meaning. ... ).
2. Characterize the inverse image of  $\underline{Coeff}^*_{HOL}$  and  $\underline{Coeff}^*_{HOL reg}$  by conditions of "holonomy" and "regularity" "microlocal", in terms of complexes of differential operators.

For this last point (which for my sixties program is perhaps relatively incidental), we have a ready-made holonomy condition in characteristic null. As for the regularity condition, it's time to see if the Japanese haven't got just the right notion up their sleeves - but Mebkhout won't tell me, as he's seen too much to want to hear about it.

As for me, who hasn't seen any like him, it seems to me that there are **three** different **aspects** of regularity, which complement each other:

1. Geometric" aspect revealed by Deligne by unscrewing in  $\underline{Coeff}^*_{HOL}$  back to the condition of regularity for a "local system" (e.g. fiber with integrable connection) in the vicinity of a divisor

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crystals...

singular.

2. Microlocal" or "Japanese" aspect, expressed directly in terms of differential operator complexes (?)
3. The "cohomological" aspect introduced by Mebkhout, an aspect that for the moment is only well understood (it seems to me) in the complex analytic case. I have no idea whether it has any chance of generalizing to the rigid-analytic case.

Aspect 3°) will of course be crucial whenever we need to establish a **comparison theorem** between "Zariskian" cohomology and "rigid" cohomology, for an algebraic variety defined over a complete value field, and holonomic coefficients.

For my great "variance program" of the sixties, it's of course the "geometric" aspect that's the most important of all. What's important is to define a formalism of the six operations for

$\underline{Coeff}_{hol\ rég}$ . If we can even find one for  $\underline{Coeff}_{hol}$ , as Mebkhout seems to believe, so much the better. But (if I'm not mistaken) the reasons (to which I have some before any □ (other thing) will only give rise to coefficients that are both holonomic and regular.

p. 1021

I return to question 1, which admits as an obvious variant a (more modest) "question 1'", with  $\underline{Coeff}_{hol}$  replaced by  $\underline{Coeff}_{hol\ rég}$ . Once we've proved the full fidelity of the Mebkhout-Grothendieck functor, we're obviously reduced to the following: we give ourselves, on a smooth subvariety (not necessarily closed)  $Y$  of  $X$ , a fiber with integrable connection (or a coherent  $F$ -crystal  $C$ -, depending on the chosen context. . .), with, if necessary, an additional Deligne regularity condition for the latter (at the points of  $Y - Y$ ). The Deligne procedure (possibly revised by the ancestor to move to the crystalline context) allows us to

associate it with a  $\underline{Coeff}$  object\* (which by definition will even be "holonomic", or even "regular holonomic"). Is this object in the image of the Mebkhout-Grothendieck functor? Or, which amounts to the same thing, can the  $\underline{Coeff}$  object in question\* be described locally on  $X$  by a complex of differential operators on  $X$ , using the ancestor's patented process of "formalizing" the complex, interpreted either as a Deligne complex or as a crystalline complex?

The answer to this question is in any case affirmative (unless I'm mistaken) in the complex analytic case, as well as in the case of smooth relative schemes over a body of zero characteristic, without even having to introduce the regularity condition. This is the "entirely unexpected phenomenon brought about by Mebkhout's theory" that I was careful to point out earlier (in (c), page 1011)<sup>710</sup> (\*). In the regular case (including "at infinity"), it's essentially the good Lord's theorem. In the general case, if I'm not mistaken, this must result without tears from what I've called the "cohomological criterion of holonomy" (or "reciprocal: to Kashiwara's constructibility theorem"), due to Mebkhout, referred to in the following note "Three milestones - or innocence" (n° 171 (x), see page 1028).

## b2. Three milestones - or innocence

p. 1022 **Note** 171(x) □ (May 5 and May <sup>711</sup>(\*) Mebkhout's philosophy, developed between 1972 and 1980, can be summarized as follows  
23)

<sup>710</sup>(\*) Nowadays, at least in the field of mathematics we're talking about here, pointing out such facts has become a veritable **work of public health**, at a time when almost all publications on the subject of cohomology, and all (I'm afraid) of those appearing under prestigious names today, are written in such a way **as to obscure** the key ideas that give life to all these texts, and to **blur** or **eradicate** the role and origin of a crucial tool (old or new), a neuralgic notion or a fertile idea. There's an intellectual **corruption** (a sign of a deeper corruption. . .) that's spreading across our science today, in plain sight, that I've never seen in any other science at any other time in history.

can be summed up in **three major theorems**, all three intimately linked to ideas I had developed in the fifties and sixties, but which I (or no-one else) had been unable to foresee<sup>712</sup> (\*\*).

The first major theorem is the main fruit of Mebkhout's work between 1972 and 1976. It concerns **local cohomology** bundles  $\underline{H}^i(\underline{O}_X)$  (a notion introduced independently by Sato and myself) of the structural bundle of a smooth complex analytic variety  $X$ , with supports in a closed analytic subspace  $Y$ .

The essential observation here, which no one had thought to make before Mebkhout, is that the  $D$ -ring operations<sup>713</sup> of infinite-order differential operators on  $X$  (\*\*), due to the fact that they  $\square$  (operate on the argument p. 1023  $\underline{O}_X$ ), also operate on these cohomology bundles. On the other hand, in the "Zariskian" framework of algebraic geometry, I had described these bundles (towards the end of the fifties?) as inductive limits of  $\underline{Ext}$  bundles<sup>i</sup>. This led Mebkhout, by analogy, to introduce an "algebraic part" of cohomology and a canonical arrow

$$\frac{\underline{H}^i(\underline{O})}{Y \quad X \text{ a.l.g.}} \stackrel{\text{dfn}}{=} \varinjlim_n \frac{\underline{Ext}^i(\underline{O}_{O_X} \quad \underline{O}_{X_n})}{X} \rightarrow \frac{\underline{H}^i(\underline{O})}{Y \quad X} \stackrel{\text{dfn}}{=} \frac{\underline{Ext}^i(C, \underline{O})}{C \quad Y_X \quad X}, \quad (1)$$

where  $X_n$  denotes the  $n$ th infinitesimal neighborhood of  $Y$  in  $X$ , and  $C_X, C_Y$  the constant bundle  $C$  on  $X$  resp.  $Y$  (the latter extended by zero on  $X - Y$ ). The second essential observation is that this time the ring  $D$  of ordinary differential operators on  $X$  operates on the first member. It was well known that the kind of The bundles we obtained, both the right-hand member of a transcendental nature, and the left-hand member of an "algebraic" nature, were of rather prohibitive dimensions, as  $\underline{O}_X$ -Modules - nothing coherent, that's for sure. It's also true that we had the feeling (at least on the algebraic side) that there was still a certain type of "finitude" or "cofinitude", in a sense that no one before Mebkhout had thought to specify. Mebkhout's remarkable theorem is that the first member is a  $D$ -Module, and that furthermore, the second member (which looked even more intractable) is simply deduced from the first by the change of Rings

$$D \rightarrow D^\infty$$

Since the second Ring is known to be flat on the first, this implies that (1) is injective. At the same time, given the coherence result/ this can be seen as a finiteness theorem

<sup>711</sup>(\*) This sub-note "The three milestones" is taken from a footnote to the note "The work... ." (n° 171 (ii)). See the cross-reference at the end of this note.

<sup>712</sup>(\*\*) As I pointed out in the note "Les questions saugrenues" (n° 171(vi)), I had long been aware of a variant of Mebkhout's global duality theorem, for a clean and smooth relative scheme  $X/S$ , in terms of complexes of relative differential operators. Specifically, if  $L$  and  $L'$  are such complexes, "adjoint" to each other, then  $Rf_*(L)$

and  $Rf_*(L')$ , as objects of the derived category  $D(S, \underline{O})$ , are "perfect" complexes (locally representable by complexes of free Modules of type fini with bounded degrees), and dual to each other in the usual sense for perfect complexes. In the case where  $S = \text{Spec}(C)$ , this theorem is more or less equivalent to Mebkhout's (restricted to the case of an analytic variety that is algebraic and proper), with the important difference that I lacked a point of view.

"derived categories", to deal with complexes of differential operators. On the other hand, and above all, I had no suspicion that these complexes (subject to the suitable conditions outlined by Mebkhout) formed a perfect substitute for "discrete coefficients" (or De Rham coefficients). On the other hand, it was clear to me, at least as early as 1966, that there had to be such a thing as "discrete coefficients".

substitute for algebraically constructible  $C$ -vector coefficients, making sense for relative characteristic schemes and my crystalline ideas were precisely a first approach in this direction. As we shall see in [Crystals] (these are the talks cited in the previous note "The five pictures (crystals and  $D$ -Modules", n° 171(ix)), the internal logic of my crystalline reflections had, however, brought me back into contact with complexes of differential operators. I was then very close to Mebkhout's philosophy. My cohomology students (especially Deligne, Berthelot and Illusie) must have been blocked by burial syndrome, not to have cleared up this philosophy in the years that followed. (I myself was then fully occupied with other fundamental tasks, and had left the crystalline theme to the care of my students).

<sup>713</sup>(\*\*\*) For a definition of these operators, whose name is frightening at first glance, but which give rise to a formalism in every respect parallel to that of ordinary differential operators, see part (b) of the previous note "The five photos (crystals and  $D$ -Modules)" (n° 171 (ix)).

very strong regarding the second member (which nobody before Mebkhout understood anything about) - this one is notably of finished presentation as a  $D^\infty$ -Module (but perhaps not coherent, since we don't know if  $D^\infty$  is itself coherent).

Mebkhout's first case, that of a divider with normal crossings, was the subject of his post-graduate thesis in 1974. Already this case is not trivial, and of course entirely new - the very question Mebkhout solved had never been seen before. This, moreover, turns out to be the crucial case, to which Mebkhout manages (by successive approximations of increasing generality) to reduce himself to<sup>714</sup> (\*), with strokes resolution of singularities.

p. 1024

□(The result I have just stated, on its own, appears to me to be of such significance that, under certain conditions

If they had been even slightly normal, they would have earned their author an international reputation. Also, the first crucial case he dealt with already showed an originality of vision which, "normally", would have earned him the warm praise of those among his elders (such as each of my ex-students, without exception) who were in a position to appreciate its flavor. Moving on...

In fact, in these four years, Mebkhout arrived at an even more detailed result than the one I've just described. He proves that the D-Module he studies is not only coherent, but also **holonomic** (a notion he found in the Japanese school), and moreover **regular**<sup>715</sup> (\*) (in a sense he defines ad hoc, drawing on my comparison theorem for algebraic-analytic De Rham cohomology). Better still, he proves that the constructible initial C-vector bundle  $C_Y$  (which enters into the definition of the second member of (1)) can be **reconstituted** from the  $D$ -complex <sup>$\infty$</sup>  -Modules  $RHom_D(C_Y, \underline{O}) = C$ , by the extraordinary inversion formula:

$$C_Y = RHom_D(C, \underline{O}_X). \quad (2)$$

No one had ever dreamed of such a formula - and no one would dream of it until D-Day, five years later, when the power of the philosophy was revealed and, at the same time, the signal was given for the burial, alongside the ancestor, of the one who had brought it. ... To dream of it, you'd have to have buried the ancestor's philosophy (with derived categories,  $RHom$  with or without underlining and other "useless details" . . .); and, what's more, to be able to appreciate a geometrical situation that's so trivial yet so full of mystery (local cohomology with supports in a divisor with normal crossings), and to get to the **bottom** of the mystery. This "end" is not yet to be found in the splendid 1976 theorem I've just described - but from that moment on, Mebkhout had a clear vision of it: it's the double "God's theorem", one for regular holonomous D-Modules, the other for holonomous  $D^\infty$ -Modules, and the double inversion (or "biduality") formula mentioned earlier<sup>716</sup> (\*\*). This is also the wonderfully simple solution to the problem of the relationship between discrete (analytically constructible) coefficients and "continuous" coefficients.

p. 1025

□(But I anticipate. When he proved the theorem that constitutes the first great milestone of his work and of his philosophy, the "end", clearly perceived, still seems vertiginously far away. If he'd found a competent, kind-hearted elder with a modicum of experience and mathematical flair, he'd have been disabused of the notion: clearly, he was already very close, and the difficulty to be overcome, as so often in the work of discovery (not to say, always. . .), was more psychological than technical. But before

<sup>714</sup>(\*) For Mebkhout's theorem on local cohomology, see in particular: La cohomologie locale d'une hypersurface, in Fonctions de plusieurs variables complexes III, Lecture Notes in Mathematics n° 670, pp. 89-119, Springer-Verlag (1977), and Local Cohomology of analytic spaces, Publ. R.I.M.S. Kyoto Univers. 12, p. 247-256 (1977).

<sup>715</sup>(\*) Mebkhout's original (transcendental) definition of regularity is recalled in the note "L'oeuvre. . . ." (n° 171 (ii)), b. de p.(\*) page 950.

<sup>716</sup>(\*\*) In previous note "The five photos (crystals and D-Modules)" (n° 171 (ix)), part (b).

In his pursuit of the infinitely distant, he tackled the global duality theorem - the one that was to "cap" the known duality theorem for both coherent and discrete coefficients. The deep motivation, omnipresent in Mebkhout's work, which links the two problems, that of local cohomology and that of global duality, is the presentiment of **an essential unity** between discrete coefficients and continuous coefficients. This was also the thread running through my 1966 crystalline approach, which endeavored to apprehend "De Rham coefficients" (essentially discrete in nature) in "continuous" terms. ...

This is not the place to go back over Mebkhout's duality theorem<sup>717</sup> (\*). His proof ran up against serious technical difficulties, due to the transcendental context, which he overcame using cohomological descent and nuclear EVT techniques (techniques to which I was no stranger either, even if Mebkhout is the only one who still insists on quoting the ancestor... .). From the point of view of his philosophy of duality, this theorem is an essential milestone. If we bear in mind, along with Mebkhout, that applied to holonomous D-Module complexes, it contains global duality for analytically constructible discrete coefficients<sup>718</sup> (\*\*), in addition to coherent duality, we can say that it too already contains the seeds of the whole Mebkhout philosophy of D-Modules. When he first told me about it in 1980 (the year after he defended his thesis<sup>719</sup> (\*\*\*)), its significance became clear to me.

obvious. I don't think I've had the honor of inspiring a work of comparable scope, to any student work<sup>720</sup> (\*).  
p. 1026

Mebkhout had great difficulty getting this theorem published, as it smacked of "grothendieckeries". (The Annals of Mathematics sent it back to him, telling him that this kind of thing wasn't up to scratch. It ended up appearing anyway, in *Mathematica Scandinavica*, in 1982<sup>721</sup> (\*\*).) I think that was his favorite theme, when he was giving lectures on D-Modules philosophy, but in a very different spirit from the Japanese. He told me that this theorem had a way of astonishing listeners, or the occasional interlocutor, with the exception of those who were part of the establishment<sup>722</sup> (\*\*\*). That's a comforting thought. It

<sup>717</sup>(\*) This statement is reiterated in the note "L'oeuvre. . ." (n° 171 (ii)).

<sup>718</sup>(\*\*) At the time Mebkhout established his global duality theorem (1976), he had not yet proved that every analytically constructible C-vector bundle comes from a complex of D-Modules. But he had no doubts about it.

<sup>719</sup>(\*\*\*) See the note "Rencontres d'outre-tombe" (n° 78).

<sup>720</sup>(\*) I'm thinking here mainly of students who have prepared a thesis with me. Deligne is a special case, since he's doing his thesis with me. The inspiration for his work (on Hodge-Deligne cohomology) came from my problematic of "coefficients" of all kinds, which also included a formalism of "Hodge coefficients". Deligne's work is a first step in this direction, much more fragmentary than Mebkhout's, in the direction (intimately linked to Hodge's) of "De Rham coefficients". It's true that Mebkhout, who was severely handicapped by the indifference and disdain of his elders, was not afflicted by the burial syndrome that paralyzed my students. (On this subject, see the note ". . . and hindrance", n° 171 (viii).)

<sup>721</sup>(\*\*) Global duality theorems for coherent D-Modules, *Mathematica Scandinavica* 50 (1982) pp. 25-53. See also "Dualité de Poincaré" in *Séminaire sur les Singularités de Paris VII* (Pub. n° 7), 1977-1979, and especially "The Poincaré-Serre-Verdier duality" in *Proceedings of the Conf. of Algebraic Geometry, Copenhagen* (1978), *Lecture Notes in Mathematics* n° 732, pp. 398-418, Springer Verlag (1979). The introduction to each of these papers, especially the second, represent a sketch of the philosophy brought by Mebkhout, at a time when he was the only one to be its depositary and advocate.

<sup>722</sup>(\*\*\*) (May 24) This ties in well with my own observations. It would seem that the position of a man in the public eye predisposes one to such sufficiency, for whom "nothing is beautiful enough for her to deign to rejoice". I don't know if these dispositions are the rule throughout the scientific world, these days, or even since time immemorial. I was very lucky to be welcomed into an environment where such a spirit of sufficiency did not exist - yet.

It must have crept up on us over the years, settling down in all of us, little by little, without any of us (except Chevalley. . .) noticing. Everything seemed the same as before - and yet it was already different. It was already like a fine layer of dust inside us, covering the original freshness of things. I was touched by this dust, as were others. And today, when I find myself once again confronted with one of those who were once my pupils, I'm touched by it,

shows that this spirit of smugness, which tarnishes the beauty of anything, no matter how beautiful, has not become general in the mathematical community. It prevails mainly (if not exclusively) in the upper echelons, where I've had ample opportunity to become acquainted with it over the last ten years or so... ..

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□(This global duality theorem needs to be supplemented by the result already mentioned of a local nature, also deep, saying that the natural dualizing functor for complexes of D-Modules, with coherent cohomology bundles, which transforms holonomous complexes into holonomous complexes (and the same for regular holonomous complexes), is moreover compatible on these with the De Rham functor  $DR$  ("associated complex of differential operators", viewed as a complex of C-vector bundles with constructible cohomology), for the natural dualistic functor I had introduced on these<sup>723</sup> (\*). This compatibility is obviously an essential ingredient of Mebkhout's duality formalism, for an understanding of the meaning of his global duality theorem. For some reason, he calls it the "local duality theorem"<sup>724</sup> (\*\*). This profound theorem, just like the famous "correspondence" (known as the "Riemann-Hilbert correspondence", when one deigns to name it), is treated by "everyone" (Verdier and Deligne in the lead) as something "well known" which would go without saying, and above all without ever naming a certain unknown (which "everyone" knows is not to be named). ... .

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□(At last, I come to the third major milestone in Mebkhout's work. Technically speaking that it consists of three (or at least two) distinct theorems, but so intimately linked that in Mebkhout's mind, they appear indissociable. As early as January 1978, he proved the " $D^\infty$ -Modules" aspect: the fact that the restriction  $m_\infty$  (where "Mebkhout functor") of the "associated De Rham complex" functor to holonomic  $D^\infty$ -Modules complexes is a category equivalence (with complexes of C-vector bundles with constructible cohomology). Knowing already that this functor commutes to dualistic functors, it's natural to reformulate this theorem by passing to the associated contravariant functor  $\delta_\infty$ , given by

$$C \dashrightarrow \underline{RHom}_D(C, \underline{O})_X \quad (3)$$

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or friends, I often have the impression that this dust has accumulated in thick, dense layers, and that it has formed a kind of impenetrable, watertight armor that calls out to me through them. ...

<sup>723</sup>(\*) This is the duality that, by the general consensus of my students and former friends, has come to be known as the "Verdier duality" (both in the complex analytic case, and in the spread case). . . (See, for example, the note "La bonne référence", n° 82.)

<sup>724</sup>(\*\*) It is under this name that the result appears in Chapter III of Mebkhout's thesis. Mebkhout told me that he had been inspired, for this name (as for the "biduality theorem") from the terminology I had introduced - yet, for me, the "local duality theorem" was just another name for the "biduality theorem" I had identified, of which it represents an important aspect, the "geometrical" aspect.

This compatibility result (Mebkhout explains) was an important step in his demonstration of what he calls, in this same chapter, the "biduality theorem". (For the latter, see the previous note "The five photos", part (b)).

Demonstration aside, and from a "philosophy" or "yoga" point of view, it was certainly an "obvious" thing that the good God functor should commute to dualistic functors (since there is a good God!). Funnily enough, Kashiwara (to whom Mebkhout had spoken in January 1978) didn't **believe** this theorem to be true! That's how out of his depth he was, and how he lacked geometrical vision ("six operations" style). This didn't stop him, however, after Mebkhout communicated his Chapter III to him (in February 1978), from appropriating this result (without mentioning its author, of course) in his big article with Kawai already quoted (see b. de p.(\*) note on page 1005) (prop. 1.4.6 of par. 4 of loc. cit.). This is the work in which the "biduality theorem" (loc. cit. 1.4-9 of par. 4) is also appropriated without further ado (under the name "reconstruction theorem"). This just goes to show the extent to which the emulators of the great masters of the "new style" born in Paris (in place of a "Grothendieck school" which had vanished without a trace. . . ), are not to be outdone by their French colleagues.

My biduality theorem (for discrete coefficients) also figures in the same inexhaustible par. 4 of the same work by Kashiwara-Kawai (prop. 1.4.2) But while we shamelessly plunder the posthumous and unknown pupil, notoriously left out by the bosses, without thinking twice, we give the de rigueur tip of the hat to the illustrious colleague opposite, quoting as we should "the good reference" provided by Verdier (himself plundering a deceased never-named. . . ).

These deceptions are notorious among the well-informed, and Mebkhout has heard several echoes of them. But obviously, they are considered appropriate and welcome in the circumstances, as long as the aim is to eliminate the unfit ancestor and his unfortunate successor.

and it's the same thing to say that this functor is an (anti)equivalence. This theorem can be clarified, then, by Mebkhout's magnificent **inversion** (or "reconstitution", or "biduality") **formula**, giving the expression of the quasi-inverse functor as

$$F \mapsto \underline{RHom}_{C_X}(F, \underline{O})_X \quad (4)$$

Following on from this, Mebkhout also proves a **reciprocal of Kashiwara's constructibility theorem**: if a complex of  $D^\infty$ -Modules (or D-Modules) with coherent cohomology is such that the associated De Rham complex (as a bundle complex of C-vectorials) has constructible cohomology, then it is holonomic (**cohomological criterion of holonomy**). In the case of  $D\text{-complexes}^\infty$ -Modules, where there is no question of regularity, so this implies that in the  $\square$ (derived category (in which p. 1029 nobody had been working for a long time, in 1978 and up to 1981. . . ), the complex (or rather, its dual) can be "reconstituted", to within a single isomorphism, by the inversion formula.

As I've explained elsewhere<sup>725</sup> (\*), Mebkhout now has everything he needs to prove God's theorem for D-modules too: the fact that the functor  $m$ , a restriction of De Rham's functor to regular holonomic D-module complexes, is a category equivalence. The result inspires him less, as there is, to all appearances, no inversion formula to the key<sup>726</sup> (\*\*). In any case, even his magnificent inversion formula doesn't make anyone hot or cold - starting with his quasi-thesis supervisor Verdier (who will nevertheless do him the honour of acting as jury president). It's not exactly an encouraging atmosphere for making the technical effort to prove something he feels sure of anyway, and feels he has everything he needs to prove it. He won't worry about it until the rush to prove the conjecture (not Weil's this time, but Kazhdan-Lusztig's) has started.

It was, as if by design, just the other side that people suddenly needed urgently. In any case, "everyone" was in such a hurry to use the brand-new "fracturing iron" that had just appeared on the market, and it was such a common understanding that the question of a demonstration was not to be raised - in case it appeared that the work had already been done by someone unqualified - that no one, it seems, had the idea, apart from the person himself, to copy and paste the pieces of  $D^\infty$ -theory already written, in order to demonstrate the theorem required in D-theory. It seems that the one and only demonstration published to date<sup>727</sup> (\*\*\*) is that of Mebkhout, published last year (and received in June 1981, the very month of the memorable Colloque Pervers. . . ).

In the previous note (part (b)), I explained a simple principle, inspired by Deligne's approach towards De Rham coefficients, to recover an "inversion formula" (or "biduality", to use the expression  $\square$ (de Mebkhout) in the context of D-Modules (regular holonomies). I don't know, since we p. 1030 seminars all over the world on the new "cream pie" of D-Modules, if this very natural approach has been uncovered - Mebkhout was not aware of it in any case. What is certain is that, if Deligne had had reflexes that "in my day" were taken for granted, it would have been he himself, as soon as he became aware of the beautiful ideas of an unknown man, in June 1979, who would have encouraged him to also write the demonstration of the D-Modules side (closer to the algebraic) of his crucial result, and would have suggested to him this "pro" variant, quite obvious all in all, of his beautiful inversion formula. It was also clear to Deligne, who had paid for the knowledge, that Mebkhout's ideas were going to have a major impact on his work.

<sup>725</sup>(\*) See b. de p. (\*) p. 952 at "L'oeuvre. . ." (n° 171 (ii))

<sup>726</sup>(\*\*) As we saw earlier, there is one - and I'll come back to this point a little further down.

<sup>727</sup>(\*\*\*) Reference: Une autre équivalence de catégories, Compositio Mathematicae 51 (1984), 63-88.

give the De Rham coefficients that were missing, at least in algebraic geometry over a body of zero characteristic; the obvious thing was to encourage him to make the necessary adjustments, to state a theorem of the good Lord (or rather, of Mebkhout in this case) for complex algebraic varieties<sup>728</sup> (\*).

But other times, other customs. It will not be said that a new departure in the cohomology of algebraic varieties has been accomplished by the solitary and obstinate efforts of a vague stranger, claiming to be the son of a dead man whose name no one in the "beau monde" has dared to mention for a long time<sup>729</sup> (\*\*). He will not be

says that renewal will come through the kind of mathematics, precisely, that for ten years the heirs of the deceased have buried, while sharing the oripals. Mebkhout the innocent, if he wanted to "survive" (and "break through", had only to follow the ready-made path of the "new style"<sup>730</sup> (\*), as other brilliant young people (and even some not so young) have hastened to do. What a way to quote the (unspeakable) source of one's ideas, when it's so easy to drown a fish and only quote those who **need to** be quoted. Mebkhout, I think your account is good!

You've landed in a world you're not cut out for - and I'm happy for you that you're not cut out for **it**. You did the work you felt you had to do, without worrying about fashion, without calculating returns, simply trusting your own instincts - even if it meant making your way in solitude. You did **your** job, rather than watching for the discreet (and not so discreet) signs of those who decide what is good and decent and what is not. You didn't waver to please, you didn't say "white" when you saw, black, or vice versa - and it's with **your** eyes that you look. I don't have to congratulate you for that - you didn't look for congratulations, mine or anyone else's. And for all that, I'm happy. And for all that, I'm happy, for you and for everyone.

### b3. The master role (2) - or the gravediggers

**Note** 171(xi) (May 5)<sup>731</sup> (\*\*) The natural question here, of course, is whether there exists in algebraic geometry a "six operations" formalism for D-modules (or "crystals") not necessarily of the DRM type, which would "cap" those I had introduced in the coherent and discrete cases - assuming first, to fix ideas, that we are on the C-body. A first difficulty arises from the fact that the notion of D-coherence is not stable by the natural notion of tensor product of crystals, nor by the ana- logic inverse image operation<sup>732</sup> (\*\*\*). To hope for a six-operation formalism, we must (therefore work with a category

<sup>728</sup>(\*) As I've already pointed out, in the algebraic context, when we wish to paraphrase discrete algebraically constructible coefficients, we must impose on the  $D$ -module complexes under consideration, in addition to the condition of local holonomy and regularity, a condition of regularity "à la Deligne - Mebkhout" in the infinitesimal.

<sup>729</sup>(\*\*) It's true that we haven't yet found a way to find substitute references for EGA and SGA. But these providential acronyms contain no hint of a name that must remain silent. As we all know, the acronym SGA refers to an algebraic geometry seminar run by Bois Marie, under the impetus of a number of excellent mathematicians such as M. Artin, J.L. Verdier, P. Deligne, L. Illusie, P. Berthelot and N. Katz,

P. Jouanolou, and even others less well known but just as quotable. Clearly, there was a flourishing school of algebraic geometry here, known as "du Bois Marie", whose heart and soul was the brightest among the names mentioned. For further details on this "**Bois-Marie school**" and on the APG acronym that expresses it, see in particular the notes "L'éviction (2)".

and "Les pompes Funèbres - "Im Dienste der Wissenschaft"" (nos 169<sub>1</sub> and 175). (See also p. 899, paragraph 3, in the note "Les double-sens - ou l'art de l'arnaque", n° 169<sub>7</sub>.)

<sup>730</sup>(\*) On the subject of this style (which took the place of a "Grothendieck school" that disappeared without a trace. . . ), end of note "Les félicitations - ou le nouveau style", n° 169<sub>9</sub>.

<sup>731</sup>(\*\*) This sub-note is taken from a footnote to the note "L'oeuvre. . ." (n° 171 (ii)). See the reference to this sub-note, placed towards the end of the note quoted (p. 956).

<sup>732</sup>(\*\*\*) (May 22) Mebkhout reported to me that he has proved that the holonomy and regularity condition is stable by total tensor product operations (on  $\underline{Q}_X$ ) and by the notion of inverse image, and that the **countervariant** good-god functor  $\delta$  commutes to it.



even larger than  $\text{Cris}^*_{\text{con}}(X)$ , perhaps that of "quasi-coherent" crystals (in an obvious sense) - but so there's little hope of recovering a biduality theorem! What's more, the natural functor of ex-tension of scalars by  $\underline{O}_X \rightarrow D_X$  obviously doesn't commute to the tensor product - so, even though there would be a theory of six operations for crystals, which would extend the one (morally known from At present, thanks to Mebkhout) of De Rham - Mebkhout crystals (obtained by "structure transport" from "discrete" theory, via God's functors), it would not extend that of  $\underline{O}_X$ -coherent moduli<sup>733</sup> (\*). However, this may not rule out the existence of a "global duality theorem", version quasi-coherent crystals, for a proper morphism (let's say) of finite-type schemes over a body of zero characteristic, which "caps" (in an obvious sense) the "known" duality theorem (morally, by structure transport again) for De Rham - Mebkhout crystals, and the known (without quotation marks) analogous duality theorem in the coherent case<sup>734</sup> (\*\*).

□(I was quite flabbergasted that Mebkhout himself hadn't asked himself at least this last question, as early as p. 1033 At the very moment when he had arrived at the formulation of his "absolute" duality theorem (corresponding to the case where the goal variety would be reduced to a point) - only recently he didn't seem to "feel" it so much<sup>735</sup> (\*\*\*)). For me, this makes it all the more striking to what extent a certain "philosophy", which by the first half of the sixties had become second nature for me, and (it seemed to me. . .) for my students too - to what extent this philosophy has been forgotten by everyone, starting with those who took it upon themselves to be its gravediggers, rather than to pass it on. And I see that this is also the main cause of the stupefying stagnation of a theory (that of the cohomology of patterns) that I had left in full bloom after my departure.

It has to be said that Mebkhout placed himself in the transcendental analytic complex context, instead of the schematic one. This introduced considerable technical difficulties, in a way "parasitic", when it came to achieving an understanding of essential variance phenomena. Here again, his elders failed in their task of making their experience, gained through my contact, available to the newcomer.

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(On the other hand, the good-god covariant functor  $m$  does not commute there, and it transforms ordinary inverse image into extraordinary inverse image). Using this result, we can show that there is no six-operation formalism for De Rham - Mebkhout coefficients that "extends" the two fundamental operations of tensor product and inverse image already known.

In particular, the DRM category<sup>b</sup>  $(X)$  does not admit an "internal Hom" operation (playing the role of  $\underline{RHom}$ ), and for  $f: X \rightarrow Y$ , the functor  $f^*$  does not in general admit a right adjoint  $Rf_*$ . The functor  $Rf_!$  already introduced by Mebkhout (for  $X, Y$  smooth and for  $f$  proper) is a **left** adjoint of  $f^*$ . (NB The operation  $Rf_!$  on the coefficients of De Rham - Mebkhout has been defined in such a way that the **covariant** good-god functor commutes to it, and likewise for  $Rf^*$  - wrongly, or rightly. ...)

So, in terms of the "natural" operations available in the De Rham-Mebkhout context, these are not The question, then, is to what extent this theory extends to  $D$ -modules (let's say quasi-coherent) that are **no longer assumed to be holonomic and regular (e.g. holonomic and regular)**. The question, then, is to what extent this extends to  $D$ -Modules (let's say quasi-cohesive) that are no longer assumed to be holonomic and regular (for example, holonomic without more - a condition that is preserved by tensor product and inverse image). In particular, it would appear that the global duality formula can be written for complexes of  $D$ -Modules with cohomology

coherent (or even just quasi-coherent), and any morphism  $f: X \rightarrow Y$  of separate schemes of type fini on a body  $K$  of zero (let's say) char., so as to cover both the coherent duality theorem and the discrete duality theorem, at least in the following form: the dualising functor "exchanges" the functors  $Rf_*$  and  $Rf_!$ .

<sup>733</sup>(\*) This assertion should be rephrased in terms of a "dual theory with six operations", see b. de p. note. above.

<sup>734</sup>(\*\*) Such a duality theorem can be considered in three different forms. Either by saying that the dualistic functors at the top and bottom "exchange" the functors  $Rf_!$  and  $Rf_*$ , or by saying that two suitably defined functors  $Rf_!$  and  $Rf^!$  are adjoint to each other, or by writing a "projection formula" (which caps both statements):

$$Rf_*(\underline{RHom}(F, Rf^!(G))) \approx \underline{RHom}(Rf_!(F), G)$$

<sup>735</sup>(\*\*\*) (June 8) Mebkhout assures me that he had indeed been asking himself the question for a long time. If I got the impression that he hadn't, it must be because the question had remained entirely platonic for him.

(just as I'd got into theirs . . . ), and thus guide (or at least enlighten) him in his choice of investments, in particular.

But to enlighten and guide is also to **serve**, even though they had long since opted for the role of master.

## b4. Dead pages

**Note** 171(xii) (May 5)<sup>736</sup> (\*) Mebkhout told me that until I mentioned it to him when we met two years ago<sup>737</sup> (\*\*), he'd never even heard the word "six operations" - he wondered what "operations" I meant! Clearly, it had never occurred to him (or anyone else, it seems, apart from me) to go through the main ingredients of a certain simple cohomological formalism, and find that there were six fundamental functors or bifunctors, grouped into three.

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pairs of adjoint functors, with such arrows and compatibilities and so on. These were things that seemed so obvious to me, that I imagined that any reader, □ (either of "Residues and Duality" exposing the elements of coherent duality, or of SGA 4 or SGA 5 exposing the elements of discrete duality, with essentially the same form, will have had fun (as I did as early as the fifties, without going all the way, I admit. . . ) to put together for his own use a more or less systematic and more or less complete form of the main isomorphisms and compatibilities - for it is only in this way, and in no other way, that one manages to penetrate the spirit of a new language, to assimilate it intimately, to make it "one's own". This is surely how, and no other way, the pioneers of infinitesimal calculus arrived at a delicate yet sure intuition of the infinitesimally small at a time when they lacked the conceptual tools to apprehend them according to the canons of rigor that subsequently appeared (or reappeared). ...

With the benefit of twenty years' hindsight, I realize that in the "reference texts" cited, done with the utmost care, even brilliance - while all the "real work" (according to current desiderata) is done, culminating in "the" main duality formula, the adjunct formula between  $Rf_!$  and  $Rf^!$  (the only one practically deemed worthy of attention and effort, even if it means forgetting it the next day, as one forgets trees when one hasn't seen the forest. . . ) - that yet in all these texts the main thing is not said and has not passed from the author to the reader (assuming it was seen and felt by the author himself. ) - and yet, in all these texts, the **main thing** has not been said, and has not passed from the author to the reader (assuming it is seen and felt by the author himself). "The principal" is a "yoga", a "philosophy", a foolproof guiding thread through (in this case) the cohomological jungle in algebraic geometry (and elsewhere). It can be developed at length over fifty pages, or over a hundred, once "everything is done" (so they say); or it can simply be evoked in a few pages, and left to the reader to develop it for his own guidance as far as he sees fit for his own needs, or for his own satisfaction.

It's these few pages, whether on the "six operations", or on motives, or on many other things<sup>738</sup> (\*), pages that I felt strongly about but for which I didn't know how much he was

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important that I write them down - it's they that have been missing, above all, from my written work. Absorbed as I was by the meticulous, never-ending tasks, at the □ (service of all, of the big "piecework", the only one that was

that was supposed to be published - I didn't feel that there were more essential pages, that **only** I could write.

**The essential** things I had to say didn't come across in the written pages, but only by word of mouth.

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<sup>736</sup>(\*) (May 22) This sub-note, like the previous one, is taken from a b. de p. note on the page "L'oeuvre. . . ." (n° 171 (ii)). See the cross-reference sign at the end of this note, p. 957.

<sup>737</sup>(\*\*) This meeting is mentioned in the note "Rencontres d'outre-tombe" (n° 78).

<sup>738</sup>(\*) After these lines were written, I was able to see that I was mistaken about the six operations - in fact, I'm not really sure.

I was fooled by the massacre edition of SGA 5, in which Illusie took care to eradicate all trace of a "yoga of the six operations", which I had developed at length in the oral seminar, with a complete form copiously commented on.

- when it was convenient! Or, in a pinch, it was in between the lines, perhaps, of interminable volumes of foundations - but is there anyone these days who can read between the lines?

The essential thing, then, was what was entrusted on a day-to-day basis to those who, in my life as a mathematician, were "close to me", and first and foremost to my students. It was a matter of course, nothing deliberate. It never occurred to me that I was in some way investing them with considerable **power**. It's not that I didn't feel the force of what I was conceiving and transmitting, but that force, too, was self-evident. For me, surely, in mathematics at least, "strength" and "beauty" were and remain one and the same thing. It would never have occurred to me that these things could be abused, things filled for me with peaceful, intense life, made to live and to engender. When I left, in a way that could not have been more unexpected, I had no worries about them. These pages that I had never thought of writing - there was no doubt in my mind that their message had long been accepted and written down, and that these "loved ones" were going to be so many living pages, telling the message and enriching it with the best they had to contribute.

Those to whom I had addressed myself with trust and respect, as to younger brothers in whom I recognized myself, chose to bury and remain silent. And when the one, true to himself, in whom they recognized me came, they, filled with everything, chose to leave him outside their closed doors - a stranger and an intruder. I don't know you! And these unwritten pages, these pages said in vain, now dead pages in these posh homes with their haughty, closed doors, the rejected brother had to find them within himself, in long, groping and groping efforts. Alone, he had to make his way through the inextricable jungle of a thousand and a hundred thousand volumes. Anyone who has been through this, even if, like me, he was fortunate enough to have the fraternal help of experienced and benevolent guides, knows what I'm talking about. ...

He made his way, painstakingly, over the days and years - a bumpy road, without a compass. sometimes seemed to me after the fact, or at least without any compass other than a flair that was still searching for itself, to

through a□(painfully and hard-won experience. He did not rewrite these ready-made pages for himself, p. 1036 these pages-boussoles, now dead pages in haughty houses - if only in scattered snatches. He wrote **other** pages, **his** pages, painfully his own. He wrote them haphazardly, stubbornly, indifferently. And yet, these pages, often clumsy and worthy of a cad, which my brilliant and wealthy students of yesteryear (if they had bothered to read them) would certainly have looked at with commiseration and without seeing anything in them - these are pages that **had to** be written, like a natural, "obvious" continuation of those pages that I had never even thought of writing, so much did they seem to me to go without saying. ...

## Hatching a vision or the intruder

**Note** 171<sub>1</sub> (April 15)<sup>739</sup> (\*) Taking advantage of the recent visit to my home of my co-buried Zoghman Meb- khout in person, I'd like to give a few warm details of his strange misadventures, as he told me himself, in bits and pieces here and there, in the course of our conversations.

Zoghman has had the honor of an "interview" with his "boss"<sup>740</sup> (\*\*) J.L. Verdier on three occasions. The

<sup>739</sup>(\*) (May 30) The three notes that follow (n° s 171<sub>1</sub> to 171<sub>3</sub> ) were written between April 15 and 18 (1985), at a time when "L'Apothéose" was still reduced to a note of around ten pages. These expanded considerably over the course of the of May, following the re-launch of the Four Operations reflection, prompted by Zoghman Mebkhout's visit to my home. The ten pages have grown to over a hundred, almost all of which are of a later vintage than the three notes that follow. This has led to some partial repetition, as certain facts or episodes are mentioned or described, in different lights, in the earlier notes and those that follow. In order to preserve the spontaneity of the writing, I have not made any adjustments to eliminate these repetitions.

<sup>740</sup>(\*\*) (May 24) Mebkhout insists that the term "boss" (even with quotation marks) is misplaced here. From its beginnings in 1972 to

The first is set in 1975 - he needed a technical result, which was contained (as it later turned out) in the biduality theorem for analytically constructible discrete coefficients - at a time when Zoghman didn't even know the notion of constructibility. (This was a notion I had introduced

as early as the 1950s, and which had been taken up again, in the context of étale topology, in SGA 4.) At that time, this notion was by no means "well known" in analysis, as it is (is today). As it happens

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exactly the notion he needed for his work. Houzel (who had followed SGA 5 at the same time as Verdier, but who must have forgotten a little about what I'd told him), advised him to go and see Verdier. This was his first "interview" with the great man. Verdier taught him that what he was asking (that two discrete complexes with isomorphic "duals" were isomorphic) was true under certain technical conditions ("constructibility"), which he would find set out in the manuscript he was going to give him. This was the "good reference"<sup>741</sup> (\*), where (among other similar feats) he pretended to invent constructible beams and discover the biduality theorem (and its proof), things he had learned from me twelve years earlier (in 1963)<sup>742</sup> (\*\*). He doesn't breathe a word about me, either in this interview or in the manuscript that was to appear the following year. In any case, Zoghman went home fulfilled, and full of gratitude for the great man who had provided him with exactly what he needed at that time, and in the years that followed, when the notion of constructibility was to play a crucial role in all his work.

It was in early 1976 that he began to take an interest in duality, and to be intrigued by the analogy of duality formalisms that I had developed in the coherent case and the discrete "spread" case, and which had been taken up by

Verdier in the discrete topological case. It was at a time when, for years, this formalism had fallen into disuse, and my students had instituted a boycott (tacit and rigorous on derived categories, which

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are its natural language. The notion and the very word "six-operation formalism", which had been one of my main ideas since the fifties and throughout the sixties, was (and still is) strictly taboo after I left. (When Zoghman came to see me two years ago<sup>743</sup> (\*), he hadn't yet heard the word "six operations", and didn't even know what "operations" I meant by that - whereas I thought it had been a familiar notion to everyone for twenty years!) This meant that conditions were adverse for him to embark in this direction, where he was condemned to work in complete solitude. But this didn't stop him from developing a duality theorem in 1976, on non-singular complex varieties, which "covers" both the duality theorem

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Today, he's done his job without a boss, on his own. Verdier was simply president of his thesis jury. Apart from that, his role was limited to communicating to Mebkhout "the good reference", which was very useful, at a time when SGA 5 was still being sequestered by the combined efforts of my cohomology students (and precisely for the purposes of operations such as the "good reference" one. . . ).

<sup>741</sup>(\*) This is the article by J.L. Verdier, *Classe d'homologie associée à un cycle*, *Astérisque* n° 36 (SMF), pp. 101-151 (1976). It is The question is addressed in detail in the two consecutive notes "The right reference" and "The joke - or 'weight complexes'" (n° 82, 83), and more briefly, in the note "Episodes of an escalation" (n° 169 (iii)), with episode 3.

<sup>742</sup>(\*\*) As early as the second half of the fifties, I had been interested in all kinds of "constructibility" notions for discrete bundles (in the algebraic sense, complex analytic, real analytic, piecewise linear - pending the context of moderate topology. . . ), in addition to notions of coherence, as the natural notions for expressing finitude conditions in the beamtic framework, and I had raised the question of the stability of these notions through the "six operations". It was the subsequent development (in 1963 and the years that followed) of stellar cohomology that led me to return to these questions in the stellar framework, and to develop the techniques (unscrewing and resolution) that enable them to be treated by a uniform method, equally applicable to the transcendental context of complex algebraic and complex analytic varieties. The biduality theorem, valid (and with the same proof) in the stale setting (subject to purity and resolution) and in the transcendental context, had already been identified by me in 1963. It also appeared in the very first presentation of SGA 5 (in 1965), where it survived the massacre of the 1977 Allusie edition.

<sup>743</sup>(\*) This visit is discussed in the note "Rencontres d'outre-tombe", n° 78. For comments on the boycott instituted on the "six operations", see also the note "Les pages mortes", n° 171 (xii).

of Serre, and discrete duality (which he calls "Poincaré-Verdier duality"), in terms of a duality statement for complexes of  $D$ -Modules (which also contains a global duality statement for complexes of differential operators). The "coefficients" he takes are, moreover, of a generality that went far beyond the cases of Serre (limiting himself to locally free bundles) and Poincaré (limiting himself to locally constant discrete bundles), faithful in this to the spirit I had introduced into these themes with the then generally repudiated formalism of the "six operations".

When Zoghman explained this theorem to me two years ago, I felt both its interest, which was obvious to me, and its limitation, because in the spirit of the "six operations" it was also obvious to me that "the good" statement had to be a statement about a morphism of analytic spaces  $f: X \rightarrow Y$ , in the form (for example) of an adjunction statement between two functors  $Rf_!$  and  $Rf^!$ . It's true that placing oneself in a transcendental context introduces considerable additional difficulties, which acted strongly (it seems to me) to obscure for Mebkhout the simplicity of the algebraic mechanisms essential in duality - whereas no one around him, and especially not among those who were my students, would have known (or deigned . . . ) to make him feel it. Nevertheless, he had put his finger on an important "principle" - that the

$D$ -Module theory (which I prefer to call "crystalline modules"<sup>744</sup> (\*\*)) provides a "common denominator" to "cap" the  $\square$ (phenomena (of duality, in particular) in discrete cohomology, and in cohomologyp<sup>1039</sup> coherent. With this momentum, encouraged by someone "in the know" and equipped with a modicum of mathematical instinct<sup>745</sup> (\*) and benevolence, there is no doubt that in the space of the next three or four years he would have developed a complete formalism of the six operations within the framework of algebraic geometry of zero characteristic (at least), providing a faithful purely algebraic "paradigm" of the same (admittedly repudiated) formalism in the transcendental framework, for algebraically constructible  $C$ -vector bundles.

Sensing that he had just discovered something important, Zoghman happily asked for and obtained an interview with his benefactor, to explain his findings. It was **the** exact answer to the question I'd put to Verdier ten or twelve years earlier, but he didn't seem to take any notice of it<sup>746</sup> (\*\*) - chances are he'd forgotten it entirely. In any case, his benevolence towards this young man who had come from nowhere and was doing things that he, Verdier, had drawn a big line on long ago, was exhausted. He didn't even want to listen to Zoghman's explanations of the ins and outs and demonstration of the theorem. He basically (and politely) told him that he, Verdier, didn't believe in Santa Claus anymore and that the young man had better pack it in.

Extraordinarily, **no one** around Zoghman is "hooked" on this result<sup>747</sup> (\*\*\*) - no doubt that was the reason

<sup>744</sup>(\*\*) For the (obvious) reason for this "crystalline" terminology, reflecting a more intrinsic vision of the  $D$ -Modules (which my students learned from me and have long since forgotten), see the comments in the note "My orphans" (n° 46) (especially p. 179) and in sub-note n° 46<sub>4</sub> (p. 188) (x). On the subject of "blocking healthy faculties" against links  
See the note "La mystification" (n° 85', p. 350-351).

(x) (May 24) See also note "The five photos (crystals and  $D$ -Modules)" (n° 171 (ix)).  
<sup>745</sup>(\*) It's not that my former cohomology students lack a "minimum of mathematical instinct" - otherwise none at all.  
of them could not have done with me the good work he did. But this instinct is derailed or blocked by the master's burial syndrome.

<sup>746</sup>(\*\*) (June 5) On this subject, see the note "The ancestor" (n° 171 (i)), particularly the b. de p. note (\*) on page 946.

<sup>747</sup>(\*\*\*) (June 3) There has been a misunderstanding here. As stated in the note "Three milestones - or innocence" (n° 171 (x), page 1026), this theorem often amazed the casual listener. But until now, it seems, it's remained a secret.  
platonian - the theorem has not become a tool, something we know and use without even thinking about it. This surely has something to do with the fact that the person who rejoiced in the obvious beauty of the result was never one of those who "set the tone" and decide what is "important", and what is "bombast". (And it's not uncommon, these days, for yesterday's "bombast" to become today's "cream pie". . . ) - In his comments of April 22, Zoghman writes to me: ". . . there was an embarrassment in the face of this theorem. Some people secretly envied it. But very few encouraged it,

too much "grothendieckerie" of the □ (sixties, we're past that nowadays, thankfully! Perhaps I've was, two years ago, the first person he met who sensed the importance of the result and the new "philosophy" it bears - that of a vast synthesis between "discrete" and "differential" (or "analytic") aspects in the cohomology of varieties of all kinds (algebraic and analytic to start with). This theorem, one of the chapters of his thesis, was eventually published in *Mathématica Scandinavica* in 1982 (t. 50, pp. 25-43). The same article had been submitted to the *Annals of Mathematics*, which made the presumptuous young man realize that it was not of the level required for publication in this standing periodical.

Even today, this theorem is generally ignored or scorned in the "beau monde", even though it already contains the seeds of that new philosophy which, via the theorem of the good God (alias Mebkhout), provided the means for a spectacular renewal in the cohomology of algebraic varieties. But "everyone", including my ex-students in cohomology (whom I once knew to have a healthy mathematical instinct), rushed en masse to the new "cream pie", namely a certain powerful tool (which "everyone", however, is fond of naming only by allusion or periphrasis, such as "the relation between constructible bundles and holonomic differential systems", or as "what would normally have found its place in these notes"<sup>748</sup> (\*). . . ), and on the "latest cry" (intersection cohomology), then □ that the **vision**

The innovative work that led to the tool's release remains just as ignored as before, and the fathers of both are treated as stooges.

The situation here is the same as it was for my vast unifying vision of topos, derived categories, six operations, cohomological coefficients and, beyond that, motifs. It was from this vision that tools such as étale cohomology and crystalline cohomology emerged, which the same "everybody" uses today like turning a crank, whereas the vision itself, powerfully alive on the day I left, was buried the very next day. And I can see clearly that the stupefying stagnation I'm seeing in a splendid subject<sup>749</sup> (\*), fifteen years after I left it in full bloom, is not due to a lack of intellectual means or gifts (which are brilliant in more than one of those I've known so well and so poorly), but to gravedigger's dispositions, or unscrupulous nepotism, or both. - dispositions that are the antithesis of the innocence that makes people recognize and find simple, essential things.

To develop his new philosophy, Mebkhout drew on the spirit of derived categories and the six operations, at a time when derived categories were treated as Grothendieckian smoke and mirrors, and when he hadn't even heard the name "six operations" uttered. Today, with the rush on

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quite the contrary."

<sup>748</sup>(\*) This is a quotation (from memory) from the "memorable article" by Beilinson-Bernstein-Deligne (written by Deligne) referred to in the note "Le jour de gloire" (n° 171 (iv)). For details of this periphrase, worthy of posterity (as a reminder and as a warning. . . ), and for the ins and outs of the context, see the note "Le prestidigitateur" (n° 75"). The preceding quotation ("the relation between constructible bundles and holonomic differential systems") is taken from the Beilinson-Bernstein article (from the same year, 1981), which will be referred to in the following sub-note ("La mafia", n° 171<sub>2</sub>), where we will also have the advantage of learning about Brylinski-Kashiwara's contribution to the flowering of this kind of style, in the service of the same swindle.

<sup>749</sup>(\*) I first spoke of this impression of "morose stagnation" at the end of the note "Refus d'un héritage - ou le prix d'une contradiction" (following on from "Mes orphelins") n° 47 (p. 195). This impression has only been confirmed in the year since I wrote that note, with essentially the same restriction as I expressed in sub-note n° 47<sub>3</sub> to the cited note: Deligne's work on Weil's conjectures (Weil I and II), and the fresh start that followed the "rush" on the good God theorem (eliminating both the good God and his servant Zoghman), and on the intersection cohomology. But these localized successes seem to me to be out of all proportion to the brilliant, even exceptional means of those I know who have since "settled" in this "splendid subject" - even though fifteen years have passed since I left; and out of all proportion, too, to the richness and vigor of the key ideas I had bequeathed to them, and which I now find exsanguinated... ..

The new tool appeared, inseparable from the derived categories, and the latter were exhumed with great fanfare, while the name of both the person who had rescued them from nothingness during years of solitary work, and the person who had been inspired by them, also solitary, was hushed up to finally give birth to a new theory of coefficients linking topology, complex analysis and algebraic geometry.

□ Les Deligne, Verdier et consorts rush to the brand-new novelties shouting (with the discretion of

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It goes without saying that this is a rigorous and well-intentioned approach: "It's me, it's me! None of them has yet found within themselves the courage and loyalty to themselves, to mature a vision in solitude, to bear it heavily for months and years on end, far from the applause, when they would be alone in seeing and unable to share what they see with anyone else in the world.

But I digress, it's time to return to my account of the **blossoming of a vision**. It was in 1976, when Mebkhout demonstrated the duality theorem that "caps" Poincaré's duality and Serre's duality, that he arrived at the idea of the equivalence of three categories, embodying respectively the "topological", the "algebraic" and the "analytic" (transcendental) aspects of the same reality, of the same type of objects. From the point of view of a general theory of "cohomological coefficients"<sup>750</sup> (\*), I'll call these objects "De Rham coefficients" - □ Mebkhout<sup>751</sup> (\*). If  $X$  is a smooth analytic space<sup>752</sup> (\*\*), on the one hand there are

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<sup>750</sup>(\*) This idea of various "**types of coefficients**", each of which presented itself to me as a particular incarnation of the formalism of the six operations (and biduality), more or less encircling the finest "type of coefficients" of all, the "absolute", or "universal", or "**motif**" type - this idea was perhaps the main force guiding me throughout the sixties, and especially from 1963 onwards, in the development of my cohomological vision of algebraic and other varieties. The force of this idea in me is clearly visible from the very first note I dedicate to a retrospective on my work, and on these vicissitudes at the hands of fashion: "Les orphelins" (n° 46). I return to it insistently at various points in the reflection on Burial, and more particularly in "La mélodie au tombeau - ou la suffisance" and "Le tour des chantiers - ou outils and vision" (n° s 167, 178). It's also the very first mathematical theme, among those buried by the care of my former cohomology students and by those of a fashion, that I'm thinking of developing following Harvest and Sowing, to give it its rightful place. that it deserves in my mathematical thinking.

Strangely enough, this central idea-force of my cohomological work, and the (basically very simple) algebraic-categorical structure that expresses it, has never been made explicit in literature, not even by myself in the sixties (x). It appears between the lines in my written work, and was conveyed above all in oral communication. In my mind, it went without saying that one of my students would not fail to devote the few days or weeks it took to present this set of ideas in systematic form, while I myself was fully occupied with the basic tasks of EGA and SGA.

With hindsight, I'm more aware of the importance of non-formal texts (even if only a few pages long, in this case, and without any effort at exact, systematic formulations), which give a sense of those rarely-named "key ideas" that lie hidden behind texts that often appear to be technical - how important such texts are in guiding researchers, and in occasionally bringing a breath of air into a literature that tends to suffocate in its technicality. On this subject, Zoghman told me that the few passages of this kind he found in my texts were a great help to him. Among these, he recently pointed out to me the few words of introduction I had attached to Hartshorne's volume "Resigns and duality" (a volume essentially expounding the formalism of the six operations I had developed in the second half of the fifties, within the coherent framework). I now realize how much more useful this introduction would have been, had I taken the trouble to include even a non-formal page or two explaining the "yoga of the six operations" and underlining its importance as an omnipresent conductive file in the edification of cohomological theories that were still waiting to be born... ..

(x) (May 24 and June 1) After these lines had been written, it became clear that from the very start of the SGA 5 oral seminar (in my second presentation), I had taken great care to develop at length the "abstract" form of the six operations, which

would dominate the entire seminar to come. (On this subject, see the b. de p. note (\*) of May 8 to the note "L'Ancêtre" n° 171 (i), page 942.) Moreover, throughout the oral seminar, I constantly referred to the ubiquity of formalism the cohomological approach I was developing, valid in principle for all kinds of "coefficients" other than "*l-adic* coefficients". Illusie was careful to remove from the massacre edition both the detailed presentation of the formalism of the six operations, and any hint of a vision of "cohomological coefficients" that went beyond the particular context of the seminar's main subject.

See also the note "Dead pages" (n° 171 (xii)), and also "Useless details" (n° 171 (v)), part b) ("Machines for doing nothing. .").

p. 1044  $\square$   $\text{Cons}^*(X)$  ("topological" aspect), that of complexes of with coherent cohomology bundles<sup>753</sup> (\*\*\*), generalizing complexes of infinite-order differential operators, which I denote  $\text{DRM}_\infty^*(X)$  (transcendental "analytic" aspect), and finally the category of  $D^\infty$ -complexes with co-herent cohomology bundles, generalizing complexes of ordinary (finite-order) differential operators, which I denote  $\text{DRM}^*(X)$  ("algebraic" aspect). There is a tautological extension functor for scalars of the coherent Ring  $D_X$  to Ring  $D^\infty_X$

$$i : \text{DRM}^*(X) \rightarrow \text{DRM}_\infty^*(X)$$

inserted in a functor diagram (essentially commutative) :

$$\begin{array}{ccc} \text{DRM}^*(X) & \xrightarrow{i} & \text{DRM}_\infty^*(X) \\ & \searrow \text{associated De Rham complex} & \swarrow \text{associated De Rham complex} \\ & \text{Cons}^*(X) & \end{array} \quad (1)$$

where the oblique arrows are the "associated De Rham complex" arrows<sup>754</sup> (\*), which is none other than  $R\text{Hom}_D(Sp_*, \cdot)$  where  $D = D_X$  or  $D_X^\infty$ , and where  $Sp_*$  is the "Spencer resolution" of  $\underline{Q}_X$  by locally free D-Modules<sup>754</sup> (\*).

The existence of vertical arrows derives from Kashiwara's "constructibility theorem", which implies that the De Rham complex associated with a holonomic D-module complex has analytically constructible cohomology bundles. Kashiwara had proved this important theorem in 1975<sup>755</sup> (\*\*), albeit from a completely different angle. He worked with a single holonomic D-module, of which he took the De Rham complex and proved that its cohomology is constructible. Until September 1979 and the subsequent "rush" triggered by the Good God Theorem, he nor anyone else in the beautiful world was working in the spirit of derived categories, and the very idea of writing the vertical arrows in (1) hadn't occurred to anyone!

Once the three arrows (1) have been written, as arrows between derived categories<sup>756</sup> (\*\*\*), the question arises if they are indeed category equivalents. Mebkhout was convinced of this as early as 1976. The conviction had come to him by  $\square$  dressing a table of a dozen typical examples (reproduced in his expository article with The Dung Trang<sup>757</sup> (\*)) of constructible C-vector bundles that can be called "elementary", which

<sup>751</sup>(\*) (May 30) In the note (written later) "The five photos (crystals and  $D$ -Modules)" (n° 171 (ix)), I use a slightly different terminology, referring to "De Rham coefficients" (for short) as "the same type of objects", three of which are given here.

**descriptions** (or three **different** "photos"). Two of these will be called "De Rham - Mebkhout coefficients" (or simply, "de Mebkhout"), "of infinite order" and "of finite order" respectively.

<sup>752</sup>(\*\*) (May 30) In the initial version of these notes, getting carried away by my predilection for the "algebraic geometry" point of view, I had assumed that  $X$  is an **algebraic** variety over  $\mathbb{C}$ . This did not correspond to the framework in which Mebkhout had initially placed himself, not to mention that it made me state a variant of the "good God theorem", for  $D$ -complexes $^\infty$ -Modules, which is true as it stands only when  $X$  is assumed to be proper. So there were some misunderstandings in my mind, and Mebkhout had to kindly call me to order. In retyping these few pages, I have made the necessary corrections.

<sup>753</sup>(\*\*\*) On the subject of definition and first sorital facts concerning the theory of Modules and  $D$ -Modules, the reader is referred to the note "Les cinq photos (cristaux et  $D$ -Modules)" (n° 171 (ix)), and more particularly parts (a) and (b) ("L'album "coefficients de De Rham"", and "La formule du bon Dieu").

<sup>754</sup>(\*) (May 24) See the note already quoted "The five photos. . ." (n° 171 (ix)), part (a).

<sup>755</sup>(\*\*) Masaki Kashiwara, On the maximally overdetermined System of linear differential equations, I Publ. RIMS, Kyoto university 10 (1975), 563-579.

<sup>756</sup>(\*\*\*) Strictly speaking, it would probably be more correct to say that these are full sub-categories (defined by conditions of "constructibility", or coherence, holonomy and regularity) of derived categories in the ordinary sense.

<sup>757</sup>(\*) Lê Dung Trang and Zoghman Mebkhout, Introduction to linear differential Systems, Proc. of Symposia in Pure Mathematics, Vol. 40 (1983), part 2, p.31-63. Zoghman recommended this short article to me as the best introduction in the field.



are also of the type constantly involved in the "unscrewing" of bundles, familiar from the theory of stellar cohomology. From that crucial year 1976, for each of these bundles, he succeeded in constructing a remarkable holonomic complex, both on  $D_X$  ("**algebra**") and on  $D^\infty$  ("**analysis**"), having (from the point of view of the six operations) a very simple algebraic or analytic cohomological meaning, and whose De Rham complex is the bundle in question. Remarkably, while he started from a constructible bundle and not a complex of bundles, in a number of cases the holonomic complex that gives rise to it is in no way reduced to a single cohomology bundle. This showed him that, in keeping with the spirit of the "six operations" (whose name he didn't know. . . ), if there was any equivalence, it could not be deduced from an equivalence between the categories of moduli bundles (on  $C$ , or on  $D$ ) themselves, but only made sense by passing on to derived categories.

For me, it's quite clear that **the act of creation**, in this case, consisted in seeing and writing down the two **obvious** arrows  $m$  and  $m_\infty$  that nobody had deigned to write down - in asking the "very simple" question of whether they might not be category equivalences, thus providing a differential algebraic interpretation, and another differential analytic one, of the topological notion of constructible  $C$ -vector bundle (or complex of bundles). There was the **question, and a clear awareness of the crucial nature of this question**, of its scope - and with it, and as a matter of course, an inner attitude that **assumed** this question, that would see it through to its conclusion. The preliminary "experimentation" with "typical" or "elementary" examples was a first step in this direction.

That was the essential, childlike step, the one that can only be taken by those who know how to be alone. Once this pas-là accompli, le premier de mes élèves cohomologistes venu, utilisant les techniques de dévissage et de résolution apprises à mon contact dans SGA 4 et SGA 5, était capable de le prouver en quelques jours, ou en p. 1046

But not a single one of them, not even Deligne. But there wasn't a single one of them, not even Deligne, who had given up trying to find the unifying vision that would go **beyond** the key idea of the "six operations"<sup>758</sup> (\*), and who was still lacking in linking continuous coefficients and discrete coefficients - not a single one of them was able to see the obvious scope of Mebkhout's ideas, of this vague stranger who still came off as the spitting image of Grothendieck... .

As for the "vague unknown", reduced to his own means and reading, asking himself the question of category equivalences must have seemed to him (and rightly so) the most obvious and childish thing in the world, or to come to the conviction that these were indeed equivalences. On the other hand, in the absence of experience and encouragement from more experienced elders, he developed a world of demonstration that for a long time seemed entirely out of his reach.

And yet, after a year and a half already, he managed to find a demonstration, first for the arrow  $m_\infty$ , in March 1978. He told me that psychologically, my comparison theorem for co

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literature to the philosophy he has been developing since 1976. The bibliography also includes a (complete?) list of Mebkhout's publications on this theme, at least up to 1983.

<sup>758</sup>(\*) (June 5) On rereading, this formulation seems hasty and a little "out of touch" with reality. In fact, my "idea-force of the six operations" was inseparable from a "philosophy of coefficients", which foresaw (and in a very clear way at least since 1966) a "theory of De Rham coefficients" (intimately linked to my crystalline ideas), having the same essential formal properties as the theory of  $l$ -adic coefficients, and forming with them (for *the* variable) as many different "realizations" of the same type of ultimate object, the "motif". Mebkhout's work, carried out between 1972 and 1980, appears to me as a first major step towards the realization of this intuition - a step for which everything was ripe, practically speaking, at least as early as 1966 with the start of crystalline yoga, when the problem of a theory of De Rham's coefficients was clearly posed, in my mind at least. If this step has not been taken by any of my cohomology students since the sixties, it seems to me that due above all to mechanisms blocking spontaneous creativity, which was not lacking in any of them. On this subject, see the note ". . . and hindrance" (n° 171 (viii)).

p. 1047 of the demonstration. For some reason that I haven't quite grasped, he considers his theorem (namely that the functor  $m$  called "of the good Lord", so as not to say Mebkhout. . . is an equivalence), as a "generalization" of my comparison theorem. From this moment on, he also knows  $\square$  that he has the tools that it takes (with Hironaka's solving technique) to also deal with the case of  $m$ , by far the most interesting for an algebraic geometer like me. As an analyst, he had first focused on the case of the functor  $m_\infty$ , which was his favorite<sup>759</sup> (\*). He didn't return to the question, which seemed to him to be a little incidental, until after the defense of his thesis, and demonstrated the following month (March 1979) that the functor  $m$  (the one that everyone today uses in periphrasis without ever writing it down, so as not to have to name an unnameable author. . . ) is indeed an equivalence of categories. ) is indeed a category equivalence<sup>760</sup> (\*\*). As a result, it follows that the "ring change" functor  $i$ , going from the "algebraic" (in which he was still only remotely interested) to the "analytic" (transcendental), was also an equivalence.

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p. 1048  $\square$  In March 1978, Mebkhout had his third meeting with his "benefactor" Verdier, whom he hadn't seen for two years. He explained to him the ins and outs of the (future) "God's theorem", which he modestly called the "Riemann-Hilbert equivalence". With hindsight, Mebkhout is convinced that his explanations must have gone over Verdier's head. What's certain is that Verdier was completely unaware that his "protégé" had just presented him with ideas that deserved attention. He didn't mention it to anyone around him, not even to Deligne, who learned the

<sup>759</sup>(\*) (May 24) Another, perhaps stronger, reason is that in the case of the  $D^\infty$ -Modules he had a magnificent inversion formula at his disposal - see on this subject the note "The five photos" (n° 171 (ix)), part (b), "La formule du bon Dieu".

<sup>760</sup>(\*\*) Mebkhout only wrote the formal demonstration of the fact that  $m$  is an equivalence (demonstration on the same principle as the one for the "analytic" God functor  $m_\infty$ ) only two years later, in 1980. This demonstration is set out in the second of two consecutive articles (the first of which deals with the analytic God functor  $m_\infty$  and takes up his thesis), "Une équivalence de catégories" and "Une autre équivalence de catégories", in *Compositio Mathematica* 51 (1984), pp. 51-62. and 63-88. (Manuscripts received on 10.6.1981.) But from March 1969 and over the following years, he communicated this result (along with the one concerning the functor  $m_\infty$ ) wherever the opportunity arose, notably to Deligne in June of the same year.

I think that because of his extreme isolation, and his analyst's "glasses", he didn't realize that it was above all the functor of the good algebraic God that was going to interest people like Deligne and others, because it forms a "bridge" between topology and algebraic geometry (while waiting for arithmetic, which I seem to be the first and only one to glimpse... ), comparable in scope to that provided by the cohomological étale tool. Otherwise, he would have taken care to edit it into immediate form and publish it illico-presto - especially given the mores (of which he was still ignorant. . . ) of the strange milieu into which he had strayed. Yet his first misadventure (with Kashiwara), in March 1980, should have tipped him off (x).

It was in this same month of March that a note appeared in Mebkhout's CRAS "sur le problème de Riemann-Hilbert" (t. 290, March 3, 1980, Series A - 415), in which he states the equivalence theorem of his thesis (for  $m_\infty$ ), and cautiously affirms that "we hope to show, using the method of cohomological descent as for the duality theorem [7] that the functors  $S$  [which I have called  $m$ ] and therefore  $T$  [which I have called  $i$ ] are also category equivalences". In fact, his demonstrations showed that these are equivalences "locally on  $X$ ", which already implied, in particular, the famous Kawai-Kashiwara theorem (discussed in the next sub-note), namely that the functor  $i$  (scalar extension) induces an equivalence between the category of regular holonomic  $D_X$ -Modules, and that of holonomic  $D^\infty$ -Modules. Incidentally, Mebkhout's final result is considerably stronger, even when applied to **modules** (instead of complex of modules), as it simultaneously displays the canonical arrows

$$Ext^n_{D_X}(M, N) \rightarrow Ext^n_{D_X^\infty}(M_\infty, N)_\infty$$

from the "scalar extension" functor, are also isomorphisms (and not just for  $n = 0$ ).

(x) (May 25) In a letter dated April 24, Mebkhout tells me: "I have to tell you that after my thesis I took a breather. I'd been under a lot of stress for four years."

(at the same time as the "Poincaré-Serre-Verdier" duality, which the same Verdier absolutely refused to believe in three years earlier. . . ), from Mebkhout's mouth more than a year later, at the Bourbaki seminar in June 1979 (four months after the defense). Nevertheless, Verdier gave the go-ahead for Mebkhout to present his results as a state doctorate thesis, for which he agreed to form and chair the jury. The fact that the thesis was not defended until a year later was due to the administrative delays imposed by the notorious "Commission des thèses des Universités de la région parisienne" (an institution that Verdier holds dear as the apple of his eye... ).

As I said in a previous note<sup>761</sup> (\*), the defense took place in an atmosphere of general indifference. Mebkhout may have sent his thesis out left and right, but it continued to go unnoticed - nobody even deigned to acknowledge receipt of the pamphlet.

Mebkhout, however, remains undaunted. Despite evidence to the contrary, he feels he is part of a "family" - people, after all, who do the same kind of math - the kind he learned, for the most part, by frequenting my writings, and even more, by putting oneself in a position of openness, <sup>□</sup> of listening in relation to a p. 1049

certain **spirit** in his writings<sup>762</sup> (\*). He apparently doesn't yet realize, at least not on a conscious level, that this spirit has long since been repudiated by the very people who make up the "family" he believes he has entered, and that for these fine gentlemen who entered mathematics on high-wool carpets, he is a laggard and an intruder.

## The mafia

**Note** 171<sub>2</sub> (April 15-17)

**(a)** But our unsuspecting friend Zoghman, isolated as he is, is not unhappy. Since 1973, he's been lucky enough to have an assistant's post in Orléans, which gives him the freedom to do the maths that interests him, and too bad if for the moment it only interests him. He continues to live in the Paris region, attending seminars and keeping abreast of the literature... .

Had he stopped to think about it, he would have realized that all was not for the best in this "family" that pretended to ignore him, even though he felt part of it. He had come to realize, by frequenting my writings, that at least a good part of the "good reference" that had been like manna from heaven for him, was by no means the work of his "benefactor" Verdier. The notion of constructibility was developed at length in SGA 4 as early as 1963, twelve years before Verdier pretended to invent it in this article. With the publication of SGA 5 in 1977, even in the form of Illusie's edition-massacre, he

<sup>761</sup>(\*) See note ". . and the bargain" (n° 171 (iii)).

<sup>762</sup>(\*) One may wonder (or ask me) what is this famous "spirit" so particular to my writings, which would have inspired my This is the spirit of the "posthumous pupil" Zoghman Mebkhout, who was "repudiated" by all my other pupils, led by Deligne, and by a fashion that followed in his footsteps. If I try to find a filiation for this spirit (insofar as my more than fragmentary knowledge of the history of mathematics allows me to do so), I'd say it's in the tradition of **Galois, Riemann and Hilbert**. If I try to define it in terms of a dynamic of forces at work in the psyche, I'd say it's a mind that manifests itself through a harmonious balance of "yin" and "yang" creative forces, with a "base note" or "dominant" that is **yin**, "feminine". A more detailed description of this approach to mathematics, and to the discovery of the world in general, can be found at In the course of our reflections, we have included notes on "The rising sea", "The nine months and five minutes", "The funeral of yin (yang buries yin (4))" (no.° 122, 123, 124), which are taken up again in notes on "Brothers and spouses - or the double signature", "Yin the Servant, and the new masters", "Yin the Servant (2) - or generosity" (no.° s 134, 135, 136). For a reflection on some

See the two notes "La circonstance providentielle - ou l'Apothéose" and "Le désaveu (1) - ou le rappel" (n° s 151, 152).

it appeared that this famous "Verdier biduality" for complexes of analytically or algebraically constructible C-vector bundles, had been copied purely and simply from SGA's first paper.

5 (the same one referred to in a strangely-named volume "SGA 4<sup>1</sup>" by : "various supplements are given in SGA 5 I"<sup>763</sup> (\*)!). In this same strange volume, whose author likes to express himself with superb disdain about the satellite-volumes SGA 4 and SGA 5 that surround it, he was able to see an exposé on the cohomology class associated with a cycle, from which the volume of "technical digressions" SGA 5 (supposedly subsequent . . .); at the same time, he realized that the cohomological aspect (dual of the homological aspect) of the theme which gave its name to his benefactor's article, had also been copied from SGA 5. However, none of the three themes<sup>764</sup> (\*\*) in "the right reference" referred to me or SGA 5. ...

Of course, he couldn't yet know that what remained of Verdier's article (apart from three pages out of the fifty) had been "pumped" from my lectures on the formalism of stale homology and homology classes associated with algebraic cycles, But the few facts at his disposal were certainly more than enough to alert a well-informed and alert man. In short, it was a situation very similar to the one I had found myself in ten years earlier, leafing through Deligne's article on the degeneracy of spectral suites, in which he glossed over both the initial motivation and the whole yoga of weights (as well as the role of my modest self), and the contribution of Blanchard's ideas, using precisely Lefschetz's "cow" theorem for fibers<sup>765</sup> (\*\*\*). Like me once, Zoghman then had to silence his lucid perception of an unpleasant reality, telling himself (in this case) that this must be a customary "connivance" between master and pupil, that the master closes one eye when his pupils present as

their ideas, techniques and results directly from him<sup>766</sup> (\*\*\*\*). As is often the case in de□tels cases, this interpretation (which suited Zoghman well) was not lacking an element of reality, which

more. On more than one occasion, I had indeed been a party to such ambiguous situations (but it's also true that before I left, things had never yet reached this point, where the master's work becomes a corpse whose pieces are shamelessly shared...). )

Moreover, in the wider family of all those interested in the cohomology of varieties, including the Japanese of the Sato school, all was not so much for the best either. This same Kashiwara, whose 1975 constructibility theorem had been providential in defining the "God's functor", had also pretended to take credit for these unfortunate constructible bundles, which suddenly had everyone in a frenzy! He had renamed them "finitistic sheaves" for the purpose, in par. 2 of his quoted article, where he repeats more or less verbatim SGA 4's developments on the subject. From what I've heard from various quarters, the Sato school is familiar with my cohomological work, even though they quote me only sparingly<sup>767</sup> (\*), and it's hard to believe that Kashiwara was unaware of the notion of constructibility at least in the étale context, where it is the notion of finiteness central to the whole theory. It goes without saying that Verdier the following year no more cites Kashiwara for the "finitist" notion (sic), than he breathes a word about a certain deceased or a certain seminar<sup>768</sup> (\*\*). We may be of the

<sup>763</sup>(\*) For this priceless euphemism, aimed at the appropriation (by him, Deligne, this time) of the same unfortunate biduality theorem, see the b. de p. note (\*\*) on page 872 to the sub-note "Le cheval de Troie" (n° 169 ).<sub>3</sub>

<sup>764</sup>(\*\*) These are the "three themes": constructibility, biduality for constructible bundles, cohomology (and homology) class. associated with a cycle.

<sup>765</sup>(\*\*\*) see for details the beginning of the note "L'éviction" (n° 63), and the b. de p. note (\*\*) on page 233 of this note.

<sup>766</sup>(\*\*\*\*) (May 30) And all the while kindly calling him a smoker to boot. ...

<sup>767</sup>(\*) Mebkhout writes to me on this subject (April 24, '85): "The only references to you that I've seen in the Japanese Sato school are in Chapter 0 of EGA III/ even though they were shamelessly inspired by your work." "

<sup>768</sup>(\*\*) As chance would have it, this seminar (SGA 5) was the very one (along with SGA 4) which, by mutual agreement between my students

nice people both, and from the same "family" maybe - but when it comes to the steak of authorial vanity, everyone grabs for himself. ... <sup>769</sup>(\*\*\*) □ I think it was easier for Zoghman to p. 1052 saying that a Japanese he'd never seen <sup>770</sup>(\*) was definitely a "swindler", than having to say the same about prestigious elders, one of whom was for him like a powerful and distant father and benefactor, elders he had the opportunity to rub shoulders with in seminars, and with whom he even had the honor of being yours and yours again (as has been the custom in French mathematical circles since the days of Bourbaki).

**(b)** Paradoxically, Zoghman's troubles began the day a certain world began to realize the power of one of the tools he had brought to bear in the wake of a whole philosophy (of a kind that was, however, passing as decidedly outdated. . . ). He had mentioned this to Deligne in June 1979, who had listened attentively to his explanations of the duality theorem, and even more so (as one might imagine) of the God theorem. He even very kindly told him that he had read the introduction to the thesis, and that he thought there must be a lot of beautiful mathematics <sup>771</sup>(\*\*). Life was good for Zoghman that day - but not for long.

□ The same year, in September 1979, he took part in the Colloque des Houches <sup>772</sup>(\*), where he gave a talk p. 1053

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cohomologists and, in the words of their chief fi le Deligne, was destined to be "forgotten" (thanks to the publication of the digest-coup-de-scie from his pen. . . ).

<sup>769</sup>(\*\*\*) (May 24) Mebkhout points out that I'm painting the picture a little black here. Verdier was completely unaware of Kashiwara's article and of the notion of holonomy, which Mebkhout taught him during his "interview" with Verdier in 1976. (This was before the publication of the correct reference (published fi n 1976 it seems), but logically one cannot expect him to cite Ka- shiwara, when he knows that both he and his colleague are "pumping" from the same unnamed source. ... ) Conversely, Kashiwara was unaware of the "correct reference" and my biduality theorem (which appears in it under Verdier's authorship), and it was Mebkhout who made them known to him in January 1978, along with the results of chapter III of his thesis. These were subsequently shamelessly appropriated (and virtually unproven) in the aforementioned article by Kashiwara-Kawai - see at

On this subject, see the note "The five pictures (crystals and  $D$ -Modules)" (n° 171 (ix)), in particular page 1005. The fact that Kashiwara was unaware of the bidualite theorem for discrete coeffi cients shows, among many other signs here and there, how much he was

from Mebkhout's philosophy of duality, directly inspired by my work

<sup>770</sup>(\*) (May 24) He did catch a glimpse of the famous Japanese once! Mebkhout writes to me on this subject (April 22, 85):

"The Sato school had come in full force in 1972 for a conference on hyperfunctions. They hid their methods well. For a long time, their results remained unaffordable. There was a certain mythology around this school, which means that now Kashiwara can afford what he does."

(June 4) It has to be said that if it's true (as Mebkhout seems to be suggesting here) that the Sato school initiated the method of surrounding oneself with obscurity in order to dominate, this method has found emulators on this side of the Pacifi c, who are now not outdone by their masters! And it was they, not Kashiwara et al., who masterminded the incredible mystifi cation of the Colloque Pervers, in which Kashiwara was used as a convenient "pawn" to prepare the ground - and then be dropped... .

<sup>771</sup>(\*\*) (June 3) Mebkhout had already received an equally gratuitous compliment the previous year from Illusie at the Colloque d'Analyse  $p$ -adique in Rennes. On this subject, see the note "Carte blanche pour le pillage" (n° 174<sub>4</sub>), page 1091 (and in particular the b. de p. note (\*\*)) on the same page).

<sup>772</sup>(\*) The proceedings of the Colloque des Houches (September 1-13, 1979) are published in Lecture Notes in Physics n° 126 (1980), Springer Verlag. These Proceedings include Mebkhout's paper "Sur le problème de Hilbert-Riemann" (On the Hilbert-Riemann problem), setting out the whole of the his philosophy (which I'd call "De Rham's coefficients") in a perfectly clear manner, with references for the demonstrations, and the presentation by Kashiwara and Kawai. Any reader of good faith will be able to verify, by comparing the two articles, that there is not the slightest hint of a philosophy of this kind, nor the slightest allusion to something like the "God's theorem", in the article by these two authors.

(June 4) In his letter of comment dated April 22, Mebkhout expresses the same view of the International Congress of Mathematicians held in Helsinki the previous year (August 1978):

"I must say that I attended Kashiwara's lecture as keynote speaker at the Helsinki congress (August 1978). There was no philosophy either remotely or closely related to the comparison between discrete and continuous coeffi cients. I wrote up my Copenhagen lecture, which had taken place a week before, and made it available to the mathematical community, which is supposed to be the judge. The same Kashiwara's lecture is published in the Proceedings of the [Helsinki] Congress."

"On the Hilbert-Riemann problem", presenting his equivalence theorem. His talk seems to have gone completely unnoticed. One of the "highlights" of the Colloquium, on the other hand, was a lecture by Kawai a few days before, announcing a remarkable and unexpected result obtained in collaboration with M. Kashiwara. In a somewhat convoluted and incomprehensible form (in keeping with the particular style developed by the Sato school<sup>773</sup> (\*\*)), this theorem asserted that on a complex (smooth) analytic variety, the "change of scalars" functor from  $D$  to  $D^\infty$  induces an **equivalence** between the category of holo-nomic D-Modules "with regular singularities", and that of holonomic D-Modules. Their proof was to be the subject of a very long article of over one hundred and fifty pages, published since<sup>774</sup> (\*\*\*)).

Mebkhout, like all the other listeners, was a bit overwhelmed. This theorem, presented as

p. 1054 sensational and where nobody quite understood what it was all about, yet had a familiar "je ne sais  $\square$ quoi," to him. In the days that followed, he ruminated on this, slowly but surely, according to his habit. I can imagine that in the hustle and bustle of the Colloquium, it must have taken him a day or two just to put the theorem into a form a non-Japanese could understand. From then on, it was a done deal! I bet none of the Westerners present had the slightest idea what these "regular singularities" were. But Mebkhou, for his part, had well and truly defined a few years earlier, for the needs of a "philosophy of coefficients" that was still in its infancy, a notion of **regular** holonomic D-Module<sup>775</sup> (\*). This one, at least, had a precise meaning for him - and, taking the appropriate **derived category** and going "through the looking glass", he knew how to interpret this category in terms of the corresponding derived category of "constructible discrete coefficients". At least, he had demonstrated at length in his thesis the analogous interpretation, in terms of this same category of discrete coefficients "on the other side", of the category of  $D^\infty$ -holonomous moduli - and he was well aware that he had in hand everything he needed to prove the analogous also in the "regular holonomous  $D$ -module" case. This he had done in his thesis, practically, in the form of a **local** result on  $X$ , which was already sufficient to imply Kashiwara-Kawai's "sensational result". Thus, the point of view of derived categories, and that of the interplay between continuous coefficients, discrete coefficients, gave a result of the Kashiwara-Kawai type, but in principle much stronger still, since it gave at the same time an isomorphism between higher  $Ext^i$ , and not only at the  $Hom$  level (which was all that was obtained, working with D-Modules without more, instead of derived categories formed with such Modules). This being the case, it was a devil of a thing if this Japanese notion of "regular singularities" wasn't equivalent to his own - so that the prestigious result would in fact be a pure and simple corollary of his philosophy of coefficients, to which nobody had deigned until then.

interest.

When the entire Colloquium honors the presentation of a vague unknown with its presence, it's expected in the program for some reason, and that at the end of the conference<sup>776</sup> (\*\*) with arrows and diagrams (the kind of  $\square$ trucs that were made in the sixties and which had long since been dropped) of serious people), this quidam announced without laughing that the famous "highlight" of the Colloquium (which nobody would have been able to repeat, which only made it all the more impressive... ) - that this "highlight" was

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<sup>773</sup>(\*\*) (June 4) On this subject, see a previous footnote (note(\*) page 1052). It is especially in the wake of the Colloque Pervers, it seems to me, that the style of deliberate obscurity has been perfected, on this side of the Pacific, into a method of systematic mystification and appropriation to befuddlement.

<sup>774</sup>(\*\*\*) M. Kashiwara, T. Kawai, On holonomic Systems of microdifferential equations III, System with regular singularities, Pub. RIMS 15, 813-979 (1981).

<sup>775</sup>(\*) For Mebkhou's definition of the regularity of a holonomic complex of  $D$ -Modules (along a divisor  $Y$ ), see the note "L'oeuvre. . ." (n° 171 (ii)), p. b. note (\*) page 950. "Regular" in short means: regular along **any** divisor (on any open).

<sup>776</sup>(\*\*) (June 4) In fact, Mebkhou had taken care to allude to this at the start of his talk, naively thinking that it would have the gift of hooking his listeners.

an immediate corollary of a category equivalence theorem (we're asking you!) he'd obtained between the corresponding **derived categories** (what's with these animals?), and another that didn't seem to have much to do with them.), and another one that didn't seem to have much to do with them, a theorem that would appear in a **thesis** (that's the last straw!) that he swears he sent to Mr. Kashiwara and many other eminent colleagues in the large audience some time ago, sounds like a bad joke. There's an awkward silence, a few knowing smiles. It is (no doubt) to dispel the embarrassment caused by the young rascal that Mr. Kashiwara himself asks the customary question. He looks a little stunned, though, and must be wondering if he's dreaming<sup>777</sup> (\*). . . As for the quidam, he doesn't let it faze him. It's just that he's not going to start a second lecture over the first one - that'll be the day!

The next minute, our quidam Zoghman found himself all alone in front of the blackboard, with his beautiful diagrams in front of a deserted room... . No one that day, or in the days that followed, deigned to inquire about the ins and outs of the so-called "results" of the lout, whom we had been so wrong to invite to such a distinguished Colloquium.

It must have been going through Mr. Kashiwara's mind, once the buzz of the occasion. Just a few months later, at the Goulaouic-Schwartz seminar in 1979-...  
80, in an oral presentation on April 22<sup>778</sup> (\*\*), he announces **as his own** this  $\square$  same theorem, which had p. 1056 was enough to send a chill down the spine of a certain Colloque! Yet he is "kind enough" to add, on page 2:

"Note that the Theorem is **also demonstrated** by Mebkhout **by a different route**" (emphasis mine)<sup>779</sup> (\*).

This "also demonstrates" is worth its weight in Kashiwara, even though it's a theorem that neither he nor anyone else suspected, and which he had just learned (a few months before) from the person concerned himself, having not bothered to read the thesis the latter had sent him nearly a year ago! If he'd known about this theorem beforehand, he certainly wouldn't have bothered to give a 167-page demonstration to prove a "cow" analysis result that was an immediate corollary, and even the corollary of a corollary.

The phrase "by a different route" is also priceless. Zoghman assures me that there is no demonstration of his theorem in the literature other than his own, and I doubt very much (given the kind of demonstration, with which I am quite familiar, and for good reason) that any will ever be found. It's a demonstration that corresponds to a geometrical approach to things, using Hironaka-style singularity resolution - a tool that has become second nature to me (and my students), and which analysts (and especially those of the Sato school) ignore. So much so, in fact, that Kashiwara obviously didn't feel capable of simply **copying** Mebkhout's demonstration... ..

<sup>777</sup>(\*) (June 4) Mebkhout writes to me along these lines (April 22):

"After the Les Houches conference someone told me that the same Kashiwara thought his article with Kawai was empty. But he spared no effort to dishonestly catch up. It had been five years [since his 1975 paper proving his constructibility theorem] since he had touched discrete coefficients. His sudden celebrity [with this article] due to a whole other problem allowed him to get down to more "serious" things - especially not bombing! Between 1975 and 1980 I was the **only one**, in the midst of general hostility (something I understood afterwards) to develop that childish philosophy I learned from your writings."

<sup>778</sup>(\*\*) (June 4) Séminaire Goulaouic-Schwartz 1979-80, presentation by M. Kashiwara on April 22, 1980, "Constructible bundles and holonomic systems of linear partial differential equations with regular singular points". For details of this memorable seminar session, where **Mebkhout was present**, see the note "Carte blanche pour le pillage", n° 171<sub>4</sub>.

<sup>779</sup>(\*) I quote here the text of the written presentation, which was written by Kashiwara a year after the oral presentation. For details, see note quoted in the previous b. de p. note.

This kind of whitewash scam can work, **as long as there's a general consensus behind it**, at the expense (in this case) of a vague unknown. The whole<sup>780</sup> (\*\*) world would be wrong to shy away, while the aforementioned  $\square$ inconnu is left out in the cold by the very people who know best. first-hand facts, and who have a direct personal responsibility towards the person concerned: J.L. Verdier (chairman of the thesis jury) and P. Deligne (the first to feel the significance of the result he had learned from Mebkhout the previous year).

While I'm on the subject of Kashiwara, I might as well end this chapter with the epilogue to the total elimination of the service unknown, following on from the dazzling example given three years earlier at the Colloque Pervers in June 1981. This is an article by R. Hotta and M. Kashiwara "The invariant holonomic System on a semi-simple Lie algebra" (*Inventiones Mathematicae* 75, 327-358), published in 1984 (received 2.3.1983). This article, as is clear from line 6 of the introduction, is one of the many applications of the age-old "Riemann-Hilbert correspondence" known as the good Lord's (or service's) stranger's). In this article, **the name of the said unknown is no longer mentioned**, nor does it appear in the bibliography. Already aware of the mentality of the second author, but unable to prejudge the bad faith of the first, Zoghman wrote to him to inform him that he was the author of the theorem crucially used there, and to object to the fact that he was not cited as such. Instead, reference was made to Kawai-Kashiwara's paper (167 pages long), in which the theorem was not mentioned at all<sup>781</sup> (\*). Hotta replied that it hadn't seemed necessary to quote it, since **it was well known that the correspondence in question was due to Kashiwara and Mebkhout**. Curtain...

**(c)** But Japan is far away, and if my friend Zoghman has been toiling for years breaking spears in the center of distant Japan, it's probably because he's

it was far more painful for him to face up to the reality of a mafia that is by no means confined to continents on the other side of the world, but is just as much at the top of its game  $\square$  in the posh seminaries of Paris, as in Moscow or the

Tokyo. It's time to return to the sweet land of France, and to the "little family" formed by my dear cohomological ex-students, and (the slightly larger one) that has formed around them since the distant days of my "death".

News travels fast sometimes. During 1979 and 1980, with the help of Deligne and the Colloque des Houches, "they" must have come to realize that a promising theorem had just appeared on the mathematical market, due, alas, to a vague, retarded Grothendieckian; but that there was a ready-made substitute for this less-than-enthusiastic paternity, in the person of the well-known Japanese analyst Kashiwara, who was only too happy to play the father of the famous "Riemann-Hilbert correspondence".

In January 1980, Mebkhout gave a talk on his unfortunate theorem at Le Dung Trang's "Séminaire des Singularités" at Paris VII. Jean-Louis Brylinski did not attend the talk, but Lê Dung Trang spoke to him about it and had him read his notes. From what he himself told Mebkhout, as soon as Brylinski learned of Mebkhout's theorem, he exclaimed: "But with this, we'll prove the Kazhdan-Lusztig conjecture! (A conjecture that the augurs rightly considered "unapproachable").

<sup>780</sup>(\*\*) (June 4) For a "défi lé" of the actors who participated directly and actively in the mystification-scam surrounding Zoghman Mebkhout's work (or at least, those of whom I was aware), see the note "La maffia" (n° 171), part (f) "Le défi lé des acteurs - ou la maffia". This challenge is not complete - for a more complete list (including the names of thirteen internationally renowned mathematicians), see the note "Le jour de gloire" (n° 171 (iv)), note de b. de p. (\*) page 962. Still missing is the name of R. Remmert, who appeared in the meantime (see the note already quoted "La maffia", part (c1) "Les mémoires défaillantes - ou la Nouvelle Histoire") - and fourteen! (Not counting an anonymous referee - and fifteen. . .)

<sup>781</sup>(\*) (May 25) As has already been explained elsewhere (in "The five pictures (crystals and  $D$ -Modules)" note n° 171 (ix), see in particular page 1005), the work in question contains only "half" of God's theorem, half plundered from the chap. III of Mebkhout's thesis.



You'd think Brylinski would go to him, to have him explain in more detail the mysteries of the holonomy and regularity conditions, giving a precise meaning to the theorem he needed. But according to what he himself candidly explained to Mebkhout, he was "advised" not to approach him, but the eminent Kashiwara. He did not specify who this "one" was. But he obviously had a keen ear (as well as a sharp mind), and was as unknown at the time as Mebkhout still is today. He wasn't told twice, and went to ask Kashiwara, who must still be around. This was his strict right. The result was a joint article with Kashiwara, published in *Inventiones Mathematicae* "64, 387-410) in 1981 (received December 19, 1980), with the title "Kazhdan-Lusztig conjecture and holonomic Systems". Brylinski found himself an overnight star Kashiwara added another jewel to an already impressive list of achievements<sup>782</sup> (\*).

□ Everything would be for the best in the best of worlds, but... I guess the same "we" must have to suggest that the less said about a certain vague unknown, the better. In any case, in the manuscript sent to *Inventiones*, **Mebkhout's name did not appear**, either in the text or in the bibliography.

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Mebkhout was aware of the article's preprint, and complained to Brylinski about the procedure, writing to R. Remmert, editor at *Les Inventiones*. Brylinski reacted "flexibly" (in a style with which I'm now quite familiar. . . ), by adding on proofs at the end of the bibliography (out of alphabetical order) three thumbnail references to Mebkhout (while we're at it!), without making the slightest allusion in the text to the so-called Mebkhout<sup>783</sup> (\*). A reader of this article, if by chance he sees the name of an illustrious unknown added to the end of the bibliography for God knows what reason, will say to himself that it must have been put there to please a friend... ..

**Brylinski's entry into fame was a scam.** The truth is that the conjecture he demonstrates was unaffordable until a new tool appeared. Irrespective of the **authorship** of this tool, nothing in this article highlights this new tool, whose role is concealed from the outset (lines 6 to 8) by the "explanation" (sic) neither flesh nor fish :

"The method employed here is to associate holonomic Systems of linear differential equations with R.S. on the flag manifold with Verma modules, and **to use the correspondence of holonomic Systems and constructible sheaves.**"

(emphasis added). There is not the slightest reference or explanation about this famous "correspondance" unspecified. "It" must have been made clear to the young premier that this "correspondence" was now supposed to be one of those things well known to all, for which it was by no means <sup>necessary</sup> □ d, invoquerp . 1060 a particular theorem, thereby raising incidental and (above all) premature questions of authorship. And Brylinski, who is a young man with a future, hasn't been told this twice. ...

As for Remmert, he forwarded the unknown complainant's letter to the referee of the Brylinski-Kashiwara article. The referee rejected the complaint, expressing the opinion that "the result **was independently known, and pro-**

<sup>782</sup>(\*) To associate the Kashiwara celebrity with the demonstration he had just found, and in which Kashiwara had had no part, while passing over in silence the crucial role played by his unknown young colleague, was the "entry price" Brylinski paid, without being asked, for his entry into a certain "milieu" of famous people - the milieu which gives its name to the present note "La maffi a" . . .

<sup>783</sup>(\*) The introduction to Brylinski-Kashiwara's article ends with thanks to various authors, including Jean-Louis Verdier (and, needless to say, with no mention of the service unknown). She continues with par. 1, devoted to a summary of "holonomic differential systems with regular singularities" (that's the name in Japanese, for  $D$ -regular holonomic moduli). In the opening lines of this paragraph, we read: "For the details and proofs, we refer the reader to [6, 15-17]." Reference [6] is Kashiwara's 1975 paper establishing his constructibility theorem, while [15- 17] (added on proofs) is the "thumb-reference" to Mebkhout. Whatever happens, honor is safe for the "young man of the future" Jean-Louis Brylinski... .

bably earlier, by Kawai and Kashiwara", referring to the "Reconstruction theorem" he attributes to these authors (referring to p. 116 in the article by the authors cited, in the "Seminar on Micro-local Analysis" Guillemin, Annals of Math: Etudies, n° 93).

This assessment by the respondent, who is supposed to know what he is talking about, is scandalous on two counts, and shows that

p. 1061 that he is part of the same swindle, in collusion with (for the moment) Kashiwara and Brylinski. It would already be scandalous, on a mere **presumption**<sup>784</sup> (\*) of anteriority of results obtained independently- Such practices obviously open the door (and have long opened the door . . . ) to the most serious abuses<sup>785</sup> (\*). But there's more. The "theorem

<sup>784</sup>(\*) (June 4) I'm even ignoring the fact that this presumption was unfounded. Remmert's letter (dated 26.1.1981) transmitting the referent's reply does not, moreover, mention the date of the Guillemin seminar (quoted in the letter) and of Kashiwara's talk. In extremis, I have just contacted Mebkhout in Italy (by telephone. . . ) to ask for details of this reference and its date. I learn that Kashiwara's presentation takes place in 1978, a few months after Mebkhout had sent him Chap. III of his thesis (in January 1978) - Mr. Kashiwara didn't waste any time! As the thesis was not defended until February 1979 (due to the slowness of the apparatus represented by the Commission des Thèses des Universités Parisiennes, so dear to J.L. Verdier... ), this could give a plausible basis to the "presumption" of anteriority of the referent, at least as far as the "Reconstruction Theorem" is concerned. But if the referee (in addition to being in good faith, which he obviously isn't) had done his job conscientiously, he would have noticed that there is nothing resembling a **demonstration** of the "Reconstruction Theorem" in Kashiwara's exposé.

Mebkhout returned to the subject in a letter dated 3.25.1981, stressing (1°) that the theorem invoked by the referred was "one of the most important results of his doctoral thesis" and that he had communicated this result, with its proof, to Kashiwara (but he forgets to say **when** - Zoghman never does others!), and 2°) that this theorem was "largely insufficient to establish the equivalence of the categories in question". R. Remmert did not deign to reply to this letter, from a nameless, unsupported complainant.

Zoghman told me earlier (and I'm sure I'll find out everything I need to know by insisting...) that he learned about the Kashiwara swindle at the Guillemin seminar the following year, in 1979, the year he defended his thesis. This was his very first encounter with the kind of procedures used in "la maffia". By the time of the Colloque des Houches in September of that year, he already knew what to expect from the great Kashiwara star. But as his philosophy and results were written down in black and white and published, demonstration and all, he never imagined that there could ever be any question of simply dismissing his work, once its importance was recognized. And the first sign of the power of his approach came precisely at the Colloque des Houches, in connection with the Kashiwara-Kawai theorem.

Of course, in January 1978, Mebkhout (who still had no reason to be suspicious) had told Kashiwara not only about what he called the "biduality theorem" (later renamed the "reconstruction theorem" for the purposes of a scam), but also about the complete God theorem, of which it was basically one "half" (the shallower "half" of the two). He told me that, for the biduality theorem, Kashiwara had "got the hang of it" - it looked as if he'd already asked himself questions like that - but obviously he hadn't the faintest idea how to demonstrate it (Mebkhout's demonstration, however, doesn't use singularity resolution). As for the God Theorem, it went completely over his head - so much so that he'd forgotten all about it by the time of the Colloque des Houches. And yet Mebkhout had sent him, and everyone else, his complete thesis at the beginning of the same year (1979) (at a time when he hadn't yet realized what a fraud the Guillemin Seminar had been the year before). Another thing that shows that the good God's theorem had completely escaped the kingpin's notice is that he didn't even think of pocketing it, so to speak (even if he didn't understand what it was all about. . . ), in the same presentation at the Guillemin Seminar.

As I haven't yet had the benefit of holding Kashiwara's paper (\*) in my hands, I wondered whether it might not give an uninformed reader the impression that the philosophy developed by Mebkhout would have been known to Kashiwara (and by his own means, as he says) at least as early as 1978. Zoghman has promised to send me a copy of the presentation in question, which, he assures me, will enable me to disabuse myself. There is (he says) an accumulation of technical statements, more or less (in)comprehensible (Kashiwara could do no less. . . ), without demonstration and without any apparent constructive file, nor anything (any more than in his Helsinki lecture of the same year, or in that of the Colloque des Houches the following year) resembling a "philosophy of coefficients" linking continuous coefficients and discrete coefficients.

(x) (June 16) Mebkhout tells me that the presentation was in fact given by **Kawai**, as a joint effort with Kashiwara.

<sup>785</sup>(\*) This is exactly the same attitude as that, expressed three years later with the same cynicism, by R. Hotta (in the reply to Mebkhout quoted above): the new "rule", or better said "the law of the middle", is to quote people in positions of power (even out of place) and not to quote the unknown (even though their contributions are decisive and attested by irrefutable publications).

I do not question R. Remmert's good faith on this occasion. However, as publisher of Les Inventiones, he is directly responsible for this swindle, regardless of the fact (of which he could not have been aware) that he was involved.

de reconstruction" he cites (and <sup>which</sup> is also plundered in Mebkhout's thesis<sup>786</sup> (\*), where it appears under the (improper) name of "biduality theorem") is still far from the category equivalence (known as "Riemann-Hilbert") used in the proof of the offending Brylinski-Kashiwara article, an equivalence due to Mebkhout alone, and which he in no way implies<sup>787</sup> (\*\*). As far as I'm concerned, the referee's bad faith in relying on the connivence of the cohomological establishment to boycott the name and work of a vague unknown for the "benefit" of famous people, cannot be doubted. Anyone with a minimum of cohomological-analytical culture, and a minimum of interest in a fascinating theme, can convince themselves of the reality of the facts, and see the crude deception to which the anonymous referent contributes<sup>788</sup> (\*\*\*). The situation is all the more unambiguous in that neither Kashiwara nor any other Japanese or Japanese specialist other differential systems, the word "derived category" was not uttered until 1981<sup>789</sup> (\*\*\*\*), and then only for the first time.

less is there the slightest thought in the direction of a "philosophy" linking discrete and continuous coefficients -p. 1063 which philosophy is equally absent, to tell the truth, from the vague, muddled references to a certain "correspondence (sic) between holonomic systems (resic) and constructible bundles (reresic)" - none of these fine gentlemen has had the honesty to this day **to even spell out in black and white** (as I did earlier) **the categories involved**, and the arrows from one to the other that establish their equivalence. On the other hand, a whole series of Mebkhout's seminar papers, notes and articles since 1977 attest to his pioneering work, carried out since 1972 in complete solitude<sup>790</sup> (\*).

I have to confess that, until I came face to face with the thing, and looked at it and examined it at length and from every angle<sup>791</sup> (\*\*), I would never have suspected, even in a dream, that such shameless collective spoliation could ever take place in the world of scientists. And it's a strange thing to have to tell myself that this iniquitous mystification was staged above all by the combined efforts of two of my closest pupils of yesteryear; and moreover, that the signal was given by **the appearance of a continuator of my work** - a work in which I had invested myself with passion, putting my very best into it. to give<sup>792</sup> (\*\*\*). After my departure, this work became the target and prey of the covetousness of those

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doubt) that he had been misled by a dishonest referee. The referee had expressed "the hope" (cynical, given the circumstances) **"that, as a courtesy**, Brylinski and Kashiwara would mention Mebkhout's result". It was R. Remmert's role as editor to ensure that Mebkhout's result was duly mentioned in the text, not as a "courtesy", but **out of respect for the elementary rules of ethics of the mathematical profession**.

(May 30) Since these lines were written, I've learned of a new fact that sheds unexpected light on R. Remmert's role in the Zoghman Mebkhout scam, by showing his active participation in the scam surrounding mine. As a result, the presumption of good faith that I had been keeping in his regard (out of old habit, and in the absence of irrefutable signs to the contrary) vanished for me. Interested readers will find details of this "new fact" in the following section ( $c_1$ ) (of the note "La maffia"), entitled "Les mémoires défaillantes - ou la Nouvelle Histoire".

<sup>786</sup>(\*) On the subject of this pillage, see the note "The five photos (crystals and  $D$ -Modules)" (n° 171 (ix)), end of part (b) ("La formule du bon Dieu"), p. 1005.

<sup>787</sup>(\*\*) See the note already quoted (also part (b)) for the relationship between Mebkhout's "biduality theorem", and the "God's theorem", of which it constitutes one half - the shallower of the two. It makes no use of resolution, whereas the complete theorem uses the full force of Hironaka's resolution of singularities (a typically "geometric" tool, which was ignored by the Japanese school at least until the early '80s).

<sup>788</sup>(\*\*\*) (May 30) And to which R. Remmert, as publisher of Les Inventiones, gives his unreserved support. ...

<sup>789</sup>(\*\*\*\*) (May 25) Mebkhout points out that this sweeping statement needs to be qualified. While derived categories were practically taboo in France after my departure, the Japanese school continued to use them sparingly. This was a convenient technical means (to avoid recourse to spectral sequences, in particular), but by no means the "tailor-made" language for an intrinsic geometric vision of "coefficients", in cohomology of varieties and spaces of all kinds.

<sup>790</sup>(\*) For a list of these articles, which I won't go into here or even enumerate, I refer you to the aforementioned article by Mebkhout and Le Dung Trang (in Proceedings of Symposia in Pure Mathematics, 40 (1983) part 2). (May 25) See also the bibliographical references given at the end of the pages in the note "Three milestones - or innocence" (n° 171 (x)).

<sup>791</sup>(\*\*) (June 1) I first did this last year, in the week from May 2 to 9 (writing "Cortège VII", called "Le Colloque - ou faisceaux de Mebkhout et Perversité"), and again almost two months ago, writing "L'Apothéose",

<sup>792</sup>(\*\*\*) While retyping this (rather heavily crossed-out) page, the thought occurred to me that if my investment in this

even those who were closest to me, and of a secret violence which, beyond my person and my work, comes to strike even those who openly drew inspiration from it. ...

**(c1) Les mémoires défaillantes - ou la Nouvelle Histoire** (May 30) Six weeks after writing the preceding pages, I'd like to take a break from the story of my friend Zoghman's misadventures, to dwell a little on the "new development" alluded to in a previous footnote (note<sup>793</sup> (\*) page 1061). The pages that follow can be read as an interesting complement to the flowering of the "new style" referred to elsewhere(\*), which excels in the art of writing (to everyone's satisfaction. . . ) a "New History" (of a certain theme in contemporary mathematics, in this case. . . ). Readers eager to know more about the misadventures of my friend Zoghman (lost in a circus he couldn't have foreseen) can continue directly with "La Répétition Générale (avant Apothéose)" (part (d) below, dated April 16).

I have read the introduction and bibliography of the book "Non Archimedean Analysis" by S. Bosch, U. Guntzer and R. Remmert<sup>794</sup> (\*\*). This book sets out the theory of rigid-analytic spaces, rightly presenting J. Tate's 1962 ("private") notes, "Rigid-analytic spaces", as the starting point for the theory. The introduction states that R. Remmert "was able to obtain a copy" of this rare document, which had represented a sort of Birth Certificate for a newcomer to the notions of "varieties" (analytic, in this case).

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Remmert must have forgotten that it was I who had taken care to have this document multigraphed by the IHES (which was just starting up) and to send a copy to him and to other specialists in complex analytical spaces - just to draw their attention to this unexpected extension of their favourite theme. This was at a time when none of them was even pretending to be interested in basic bodies other than the real or complex ones - but you never knew... .

Remmert must also have forgotten that if I was then so interested in circulating among my friends this text attesting to the blossoming of a new geometric "universe", it was (among other things) because I had been closely associated with this birth. The very name of rigid-analytic space had been coined by me, before Remmert or anyone else (not even Tate!) had heard the name or even dreamed of the **thing** it was meant to express. I was the first to see Tate's "loxodromic" theory of elliptic curves as having to be a "quotient passage" for a kind of "analytic" varieties that didn't yet exist, and which should give rise to algebraic-analytic comparison theorems of Serre's "GAGA" type. There was another motivation pointing the way to the same kind of new objects: the need to be able to define a "generic fiber" for formal schemes of finite type over a discrete valuation ring.

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work bore (among others) such unforeseen and unwelcome fruit, it's undoubtedly because in this investment itself and in the spirit that animated me, there was not only this "best of myself" that I like to underline here, but that there was also "worst". This is something that had become quite clear in *Fatuité et Renouveau* (the first part of *Récoltes et Semailles*), but it's also something that powerful egotistical mechanisms keep pushing me to forget! I'm beginning to realize that this "worst" has only been **glimpsed in the** course of last year's reflection, that I haven't done a really thorough examination of it, or a "tour" that reveals its various faces to me in any real detail. That's why the knowledge I have of it remains superficial, as does the action of that knowledge (in my relationship to the Burial, in particular).

This fourth part, "The Four Operations" of Seeding Harvests, represents above all a meticulous recoiling of rough **facts** related to Burial. This "stewardship" work has, however, helped me to feel that a deeper understanding of Burial will come not so much from the kind of work I've been doing for nearly three months, but from a deepening of the work done in *Fatuity* and *Renewal*, that is to say, also from a deepening of my knowledge of who I was, in those distant days "before my departure".

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<sup>793</sup>(\*) See the note "Les félicitations - ou le nouveau style", n° 169, .

<sup>794</sup>(\*\*) Grundlehren der Mathematik, n° 261 (1984).

As a third indication along the same lines: I had heard that Krasner (well known in Parisian mathematical circles in the fifties and sixties, as an original who hosted an army of cats in his home, and who went around all the seminars with his big Russian-style coat and his always hilarious air.... ) - that this Krasner was "doing analytic extension" on non-archimedean valuated bodies. That's all I knew, and I'm not sure I'd ever met anyone who'd read Krasner's work on the subject - but it was intriguing. It has to be said that the term "analytic continuation" didn't in itself have the virtue of making my heart beat faster (on the contrary, it brought back unstimulating memories of my student days. . . ). ); but once I saw the need for a new type of geometric object, it was bound to click...

Returning to Remmert - if his memory is so faulty, Tate's original text (which he to boast of possessing) could, however, refresh her. In his notes, Tate makes no secret of the role that I had played in the conception of the theory<sup>795</sup> (\*), writing among other things (I quote here from memory) that it followed "from p . 1066

in a fully faithful manner" a master-builder (for a process of constructing the notion by "putting pieces back together") that he had inherited from me. I had also provided him with a certain type of "building stone" (or "localization procedure" in algebras of restricted formal series), for the purposes of formal schema fibers. He had supplemented these first "building blocks" (or "processes") with those of a second, somewhat complementary type.

This new notion would probably never have seen the light of day (nor would stellar cohomology, nor crystalline cohomology, nor many other things that followed in its wake, including even the latest "pie in the sky", the famous D-Modules... ) if it hadn't been for the common thread of "generalized spaces" (which later became **topos**), the theory of which had yet to be worked out, but had already been foreshadowed for four years. It was this intuition that showed me the way to a type of "variety" that, precisely, **broke out of** the context of ordinary (locally annelated) topological spaces.

From the moment when the **local theory** of rigid-analytic spaces had been started by John Tate, I was also the one who posed and popularized the statements of the first crucial "global" theorems to be proved about these new varieties, statements that had been present in my mind even before a first groundwork had been accomplished: algebraic-analytic comparison theorems for proper relative schemes on a rigid-analytic space, finiteness theorem for  $Rf^i_*$ , for a morphism of rigid-analytic spaces - problems solved by Kiehl in the years that followed<sup>796</sup>

<sup>797</sup>. But it is true that following the wind that blows these days, it is considered a thing of no importance, p . 1067

<sup>795</sup>(\*) More than twenty years have passed since those distant days, when a close friendship bound Tate and me, and his family and mine. It's been years since I received any sign of life from him. Nor am I aware that he, or any of my students and friends of yesteryear who could not fail to have read this book, has been moved by the evasion of my person in the introduction. Other times, other customs...

<sup>796</sup>(\*\*) I should point out that from the moment Tate laid the first foundations of a theory of rigid-analytic spaces, it was clear to me that the context in which he was placing himself was still provisional, and by no means exhausted the intuitive content I had tried to express by the name "rigid-analytic space" - any more than fin diagrams on a body exhaust the intuition associated with the word "diagram". A leading fil towards a substantial extension of Tate's context (which I have put in before to anyone who would listen. . . ) was provided by Tate himself, who had written a "universal Tate elliptic curve" on a certain topological ring (the sub-ring of the ring of formal series  $\mathbb{Z}[[t]]$  which are convergent for  $t$  in the open unit disk of the complex plane, if I remember correctly), which ring was obviously to be considered as "the ring of 'affixed coordinates' of a rigid-analytic space, of a type that didn't fit into the panoply proposed by Tate. Given the general contempt into which all questions of foundation fell after my departure, it's not surprising that the conceptual apparatus set up by Tate in 1962 hasn't moved a muscle since.

<sup>797</sup>(\*\*\*) (June 4) I was also the first to insist on the need to introduce, for rigid-analytic spaces, more general "points" than those envisaged by Tate (with values in finite extensions of the base body only). This necessity was suggested as much by the analogy with algebraic geometry, as by the desire to find a concrete interpretation of the "points" of the topos associated with the rigid-analytic space under consideration.

and, in the final analysis, simply smoky, than to foresee new concepts, to identify project managers, and to ask the questions that real mathematicians can solve. ...

In any case, my name is not mentioned in this introduction as having anything to do with rigid-analytic spaces. Nor, for that matter, is Krasner's - on the contrary, Tate's theory is presented as introducing "a structure rich enough to make the impossible possible: analytic continuation on totally discontinuous bodies" - even though, in 1962, said analytic continuation ("impossible") had already been Krasner's official "raison social" (so to speak) for ten years, if not twenty or thirty (I couldn't say). Nor is there any trace of Krasner or me in the abundant bibliography. My name does, however, appear in passing towards the end of the introduction, in the name "Grothendieck topologies"; for this notion reference is made to Artin's notes (from 1962), superbly ignoring (following the example set by the cohort of my ex-students in their entirety... .) the meticulous fine-tuning work done in SGA 4 (since 1963 and throughout the sixties, but under an obviously undesirable paternity. . .). No allusion either, of course, to the role I assigned to rigid-analytic spaces in the development of crystalline cohomology, at a time (1966) when Remmert (nor any of his eminent complex-analytic colleagues) was not yet showing the slightest inclination to take an interest in these strange (so-called) "rigid-analytic" varieties (we're asking you a bit . . .), that certain algebraic geometers had concocted in their corner - as if complex analytic spaces weren't enough to occupy the leisure time of serious analysts and geometers. ...

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□ It is enough to be informed first-hand about the true story of the genesis of the theory set out in the book, to see how this introduction displays the same cynicism that was also expressed in the response made by an anonymous referee to an unknown complainant (with the blessing of the same R. Remmert): obviously, in the minds of the authors, it's a simple question of "courtesy" again, of a "kindness." In short, they are free to grant or refuse, whether or not to include in their "history" (sic) the name of someone who played a crucial role in the genesis of the new theory. For them (and, it would seem, for almost the entire mathematical establishment, who take this kind of falsification in stride... .), "History" is not **what actually happened**, but something that can be **decided** sovereignly by whoever arrogates to himself the right to write it, or by the consensus of a handful of people who decide what has a right to be, as well as what has a right to have been.

These people like to make hot sips about what happened and is still happening in the Soviet Union, and won't miss a beat (I know what I'm talking about) to sign manifestos for the "defense of freedoms" (of thought and all that. . .) **in other countries**, while exercising the same dictatorship of lies, where it's **they** who have the power.

(June 3) When I mentioned Krasner's endearingly picturesque figure on the previous pages just a few days ago, I wondered whether he was still alive. He was a generation or two older than me, and it had been ages (well over fifteen years, if not twenty) since I'd heard his name. Although I remembered him vividly, it took me a few seconds before I remembered his name (admittedly, this is the kind of thing that happens to me a lot now, with age. . .). Krasner had a reputation for being very hospitable, and his Russian origins were another point in common that could have brought us together. But I was too immersed in my maths to have the time to make friends just "for the fun of it". Our approaches to mathematics must surely have been poles apart. We must have chatted once or twice, between two sessions of a Bourbaki seminar if that's what it was, but surely not about maths. And it was hardly just maths that really got to me then... ...

□ Today I receive a little note from Deligne, just a few lines on a question...  
I did this for no practical reason, perhaps to remind myself (it must be a few months since we last exchanged letters); or to place a postscript, which I'm taking the liberty of reproducing here (presuming his agreement):

"P.S. I was saddened to learn that Krasner had died two weeks ago. I still remember a lecture he gave in Brussels some twenty years ago, which of course went over my head, but in which I was one of the few remaining listeners. It struck me that he didn't appear in your picture of the fifties<sup>798</sup> (\*), where he was doing some fine things - albeit alien to the spirit of Bourbaki, and with a genius for badly twisted definitions."

So here's another eulogy, this time for one of my co-burials. In this one, I think I see a feeling of sympathy, or perhaps the reflection of such a feeling that had once been alive. But no more than in my Funeral Eulogy, my friend Pierre will bare his teeth to say, in honor this time of a departed without return, **what** were those "beautiful things" to which he likes to allude without naming them. He knows as well as I do, however, that these "things" paved the way for the advent of a theory that is now in full bloom - and that, for reasons he may know, the New Masters have prematurely buried (alongside me) this good-natured, muddled and "messy" precursor who has just passed away; one, surely, who was "doing analytical continuation" on ultrametric bodies, at a time when Tate, Remmert or I were still "doing" the equality cases of triangles and the Pythagorean theorem, and when our friend Pierre was still getting his nose wiped (and wiped . . . ) by his mother!

**(d) La Répétition Générale (avant Apothéose)** (April 16) But I must return to the series of worm-like "misadventures" of my posthumous pupil Zoghman Mebkhout. I have no idea what what went through Deligne's mind in □ June 1979, when he learned from the mouth of a vague stranger, se Grothendieck's ideas, the elegant solution to a crucial problem<sup>799</sup> (\*), on which he had toiled for a year ten years earlier without arriving at a satisfactory answer. Given his long-standing disposition, one suspects that he wasn't going to congratulate the young man for succeeding where he, Deligne, had failed. But I get the impression that his gravedigger's disposition is such a counterweight to his flair (which I'd known to be astonishing), that even now (six years later) he hasn't grasped the true scope of the vague unknown's ideas and vision. Like everyone else, in the end he only saw "the cream pie", the unexpected tool everyone was waiting for, the iron to fracture "problems of proverbial difficulty". One day, however, he had made his own a vast vision that someone else had communicated to him - only to bury both the vision and the person in whom it had been born, and seize yet another tool, also transformed into a "fracturing iron". . .

The first known trace of Deligne's reaction to Mebkhout's theorem is a short, undated handwritten letter to Mebkhout, received on October 10 1980<sup>800</sup> (\*\*).

<sup>798</sup>(\*) There's an obvious misunderstanding here about what I meant in the first part of *Récoltes et Semailles*, "Fatuité et Renouvellement". At no time was my aim to paint a mathematical "picture of the fifties", be it only that of the Parisian milieu or that formed around Bourbaki. My main aim has been to discover my past as a mathematician. This is what led me to talk about my relationships with colleagues or students, when these appeared to be important in my life, or could shed light on myself.

<sup>799</sup>(\*) (May 25) It's possible that Deligne had long since lost the sense for this "crucial" character. See note ". . . et entrave" (n° 171 (viii)).

<sup>800</sup>(\*\*) This is the document "communicated under the seal of secrecy, and of which I will not say another word here. . .", mentioned in the note "La victime" (page 309). With the benefit of a year's hindsight, Zoghman has kindly allowed me to reproduce it here.

"Dear Mebkhout,

I've sent Bernstein and Beilinson my copy of your thesis: they need your results for their proof of the Kashdan-Lusztig conjecture (I have a summary, in Russian, of their work, which I'll send you if you like). Could you send me another one?

Thank you.

P. Deligne"

I assume, from this letter, that Deligne must have informed the two Soviet mathematicians about the Good God Theorem, perhaps suggesting that it could be used to prove the conjecture in question; either he realized it himself, or it was already rumored that Brylinski had ideas on the subject. Mebkhout's talk, which had "triggered" Brylinski, was already in January 1980. The articles in

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Brylinski-Kashiwara on the one hand, Beilinson-Bernstein on the other, proving the famous conjecture using the unnamed theorem of an even less named unknown, <sup>were received</sup>, one on December 19, 80, the other on December 8, 1980, eleven days apart. Coincidence?

The thought even occurred to me why Deligne, who knew about the new tool before anyone else, as early as June 1979 (since no one, including Deligne, had bothered to read the paving stone of the vague unknown) - why didn't Deligne himself think of applying it to this conjecture, and thus reap new laurels instead of helping his Soviet colleagues to pick them? His mind is no less sharp than Brylinski's? It could be that, from that moment on, he saw the possibility of reclaiming paternity over the theorem of the good Lord himself, which (so he must have felt) should have been his for ten years already; that it was by some sort of inadmissible misadventure that this ill-behaved young presumptuous man had arrogated to himself the right to prove things that he, Deligne, had already worked on for a long time without any conclusive success. In the end, he'd only missed by a hair's breadth; it wasn't fair for someone else to reap the harvest where he'd sweated in vain... . But if he wanted to reclaim what was rightfully his (according to the unwritten law that had come to prevail in a certain high-flying milieu of which he felt himself to be the center and kingpin. . . ), he had to maneuver with an entirely different tact, and not try to swallow too much at once<sup>801</sup> (\*).

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In any case, Zoghman, already scalded by the strange episodes with Kashiwara and Brylinski, thought it prudent to go and inform MN. Beilinson and Bernstein about the theorem Deligne said they needed - in case such a great man as Deligne forgot to mention, when telling them about the theorem, who its modest author was. The timing was perfect: the following month, from November 24 or 28, 1980, there were held the "Conference on Generalized Functions and their Applications in Mathematical Physics" in Moscow. Mebkhout gave a talk on his theorem, published under the title "The Riemann-Hilbert Problem in higher dimension", and he takes great care to speak to Beilinson and Bernstein in person to explain the ins and outs of his findings.

The timing was perfect. It was barely ten days after the conference that the two authors sent their work on Kazhdan-Lusztig, in the form of a note to CRAS (t. 292, Jan. 5, 1981, series I - 15), "Théorie

<sup>801</sup>(\*) It is, of course, a mere presumption that the appropriation of the famous "correspondence" was present from the time Deligne became aware of it. I, for one, am convinced of this. It's true that the letter quoted above would seem to give rise to a presumption to the contrary. For my part, I see in it yet another sign of a challenge - one that he, Deligne, had absolutely no need to pay any attention to, as long as it concerned a vague stranger, **who would not budge, in any case**, when he was alone against all; one that he, Deligne, could afford to "compromise himself", just as he could also afford, by the provocative appellation "faisceaux pervers", to proclaim, symbolically and yet resoundingly, the true nature of its dispositions. On this subject, see the note "Perversity" (n° 76), and (in a rather similar, but less extreme, psychic context) the note "Joking - or "weight complexes"" (n° 83).



des Groupes - Localisation de *g-modules*", Note by Alexandre Beilinson and Joseph Bernstein, forwarded by Pierre Deligne. Fittingly, Mebkhout's name was not mentioned on their manuscript - apparently Deligne had entirely forgotten to tell them about the vague unknown, whose thesis he had shared with them, precisely for the purpose of...? Comprenne qui pourra! Mebkhout struggles to convince Beilinson ("the more honest of the two", he assures me with the utmost seriousness) that in the Kashiwara-Kawai article they cited in the bibliography, there is everything but the "construction" (here replacing the age-old "correspondence") which they too, like everyone else, only allude to, (surely Deligne, while communicating to them the thesis of the unknown where the desired result was indeed<sup>802</sup> (\*), must have suggested to them that it was perhaps more reasonable, if they wanted to give a reference, to quote an article by Kashiwara, and it didn't really matter which one, since nobody would look that closely. ) Still, we promised the said stranger, who appeared there in person, that we'd think of him and put things right for Kashiwara. Sorry - the story of my friend Zoghman's misadventures is decidedly repetitive! In the note from these brilliant authors, **forwarded by Deligne** (whose letter I have just reproduced, written just a month before), **Mebkhout's name is not mentioned**. Nor is Kashiwara's, for that matter (and I can already see here a piece of ear... ). There is, on the other hand, a double off-the-cuff reference, in the last part of the note (proving Kazhdan-Lusztig), to a "**construction expounded in** [4], [5] . . . "<sup>803</sup> (\*\*)' □ "construction" which (youp you guessed it!) is none other than the never-named functor of the even less-named service unknown. Reference [4] is to an article by Kashiwara (the temporary surrogate father). In this article, of course (no more than in Kawai-Kashiwara's, which is written off), there is nothing remotely resembling the "construction" referred to by these authors; this article dates from 1975<sup>804</sup> (\*), i.e. almost five years before the presentation of a vague stranger at a Colloque in Les Houches gave this same Kashiwara the idea that it wouldn't be so stupid after all to utter the word "derived category" and thus appropriate, according to the simple law of the strongest, credit for work done by others. As for the reference [5], it's Mebkhout's talk at the Colloque des Houches in September 1979 - the very one in which Kashiwara learned that derived categories could be useful, and for something other than ripping off a stranger left behind by his bosses and elders... ..

<sup>802</sup>(\*) (April 17) There was at least a very similar result in the thesis, even if the version in the form used by Beilinson-Bernstein (and by Brylinski-Kashiwara) did not appear in full. See the b. de p. note of the same day (note (\*\*)) page 1047) for further details.

<sup>803</sup>(\*\*) The vagueness of the expression "la construction **exposée** dans . . ." is to be admired. "(or "correspondence", or "relation" . . . ); this question would be resolved with the virtuosity we know only six months later, at the famous Colloque (see the note "Le prestidigitateur", n° 75)": we would learn, in the article Beilinson-Bernstein-Deligne, that the laconic reference [4] [5] (in two places where, surely, the construction must have been (luckily) "exposed") was pure courtesy, and that the brilliant father of "correspondence" is indeed who we guess... ..

But even apart from the sleight of hand I've just mentioned, it's already a swindle in itself to refer to a new, profound and diffi cult theorem by the term "the construction set out in... .", as if it were a simple "construction" that had just happened to be lying around, and whose authors had chosen, also by pure chance, to use it here for their brilliant demonstration. I recognize in this the same spirit as that of the "SGA 4<sup>1</sup> - SGA 5" operation, which consisted in recalling (in passing) "the construction exposed"<sub>2</sub> in SGA 4 and SGA 5 of a formalism of stale cohomology (as well as the "gangue of nonsense" from which the brilliant author had been obliged to extract it), before pretending to roll up one's sleeves and start doing "**real** maths. . .".

(May 25) On the subject of this "new style", see the note "Les félicitations - ou le nouveau style" (n° 169 ).<sup>9</sup>

<sup>804</sup>(\*) Check this out: it's Kashiwara's article, quoted above, in which he demonstrates his constructibility theorem, which plays out well. The Kashiwara theorem certainly plays a crucial role in defining "God's functors" (functors which, apart from Mebkhout, nobody had ever dreamed of before the 1980 rush). It's a gross swindle to confuse Kashiwara's theorem (which no-one would dream of disputing) with the theorem of the good Lord, which is incomparably more profound, and of a completely different scope. From a demonstration point of view, this theorem uses the full power of Hironaka-style singularity resolution. From a philosophical point of view, much more important still, it establishes bridges between topology, algebra and analysis that were lacking in cohomological formalism (while waiting for arithmetic, if some of those I see fossicking end up regaining the use of their healthy faculties... . ).

□No more than in the Brylinski-Kashiwara article, nothing that would give a reader the slightest suspicion that this brilliant note would not have seen the light of day, were it not for the appearance of a new and providential tool, euphemistically dubbed "the construction exposed in... .". I also recognize the tried-and-tested<sup>805</sup> (\*) method of drowning a fish, known as "by dilution", by "mating" the person you want to drown (even though you want to be "thumbed" and be able to say, if need be, that you quoted him or her... .) with another, who has nothing to do with the question or whose role is minimal, as if to say here (between the lines, and yet quite clearly): this vague stranger we've put there (purely as a courtesy and in view of his insistence) has no more to do with this famous "construction" (about which the newcomer consensus dictates speaking only by allusion and as about something well known to all. . .), than an article published in 1975, at a time when no one in the wider world deigned to utter the word "derived category" (if only in jest. . .).

**(e) A fool's bargain - or puppet theater** I'm not sorry to have taken the trouble, for my own sake as much as for that of any mathematician reader who might be interested, to review here the three preliminary scams surrounding the service unknown theorem. These scams are by Kashiwara, Brylinski-Kashiwara (with the assistance of an anonymous referee), and Beilinson-Bernstein, with a Deligne behind the scenes<sup>806</sup> (\*\*). There's a striking uniformity of style, which I needn't go into here. It's the style I've been reading about over and over again throughout my long investigation of l'Enterrement<sup>807</sup> (\*\*\*), and which is strikingly prefigured in

the 1968 article by my most brilliantly gifted student, the same Pierre Deligne<sup>808</sup> (\*\*\*\*). And this circumstance also suffices to remind□my good memory that through an attitude of ambiguity and complacency

With regard to Deligne and others, whom I saw as brilliantly gifted, I'm not without having contributed my share to the corruption I see everywhere today.

It's also becoming clear that the apotheosis of the Colloque Pervers in June 1981, barely six months after the third episode we've just reviewed, didn't come out of the blue. Strangely enough, this colloquium was (to the best of my knowledge) the first and only one after my departure to be devoted (admittedly without saying so, yet unequivocally) to exhuming a certain aspect of "Grothendieckian mathematics", through the unforeseen opportunity of a new tool suddenly appearing, which proved irreplaceable. This tool could only be used in an approach to things that fashion consensus had long since dismissed as obsolete and vaguely ridiculous<sup>809</sup> (\*). And by a strange twist of fate, due to the particular genius of my brilliant ex-student, this dazzling confirmation in practice, and under the pressure of need, of an approach disavowed by him and by all, was also the occasion, through the medium of this same Colloquium, of the total and definitive burial of the deceased and unnamed master, in the company of the posthumous student (just as unnamed) who had had the good fortune (or misfortune. . .) to bring all these fine people together.

<sup>805</sup>(\*) For other examples of this "dilution by assimilation" method, see the sub-note "Les vraies maths. . ." n° 169<sub>5</sub>), p. b. note (\*) page 885.

<sup>806</sup>(\*\*) (June 5) Deligne's role "behind the scenes" is clear at least in the third episode, and there are strong presumptions in the same direction for the second. But it would seem that Kashiwara "opened fire" (for the Mebkhout scams) on his own behalf as early as 1978, at a time when (it seems) Deligne knew nothing about it. On this subject, see part c) of this note ("Les prix d'entrée - ou un jeune homme d'avenir"), b. de p. note (\*) page 1050.

<sup>807</sup>(\*\*\*) On the subject of this style, see the end of the note already quoted "Les félicitations - ou le nouveau style", n° 169<sub>9</sub>.

<sup>808</sup>(\*\*\*\*) See the beginning of the note "Eviction" (n° 63).

<sup>809</sup>(\*) For the psychic mechanisms at work behind these "fashion consensuses", this overlapping with a certain "reaction visceral" rejection of a certain style of approach to mathematics, see the notes already cited "La circonstance providentielle - ou l'Apothéose" and "Le désaveu - ou le rappel" (n° s 151, 152).

This symposium didn't come out of the blue, no. One of my friend Pierre Deligne's particularities is that he knows how to wait and seize the right moment. The three episodes surrounding the "tarte à la crème", with the almost complete elimination of any mention of the stranger on duty, clearly showed him that the moment was ripe to discreetly pick up, with his characteristic smiling and affable nature, what was in any case supposed to be rightfully his. I presume that Verdier was carefully consulted, and made to understand that the moment had come to exhume with great fanfare derived categories and a long-repudiated "paternity"; at the same time, to bury in the limelight both the vague unknown, and the long-deceased master (in case anyone had the bad idea of remembering that he had had something to do with all these beautiful things that were suddenly appearing in the light of the "new world").  
"the latest technology. . ).

□Kashiwara as the father-à-la-sauvette of a certain theorem-of-the-good-God-never-named, it was fine for a while, as long as it was understood that the theorem in question would neither be named nor written down. Kashiwara himself must not have been too keen on this theorem, which he understood even less than Verdier himself - he must have picked it up inadvertently, as if by accident, opportunity and habit. Deligne, on the other hand, who knows how to wait, was well aware that this theorem would not remain the theorem without an address or a name forever. It was, in short, a theorem **in search of a father worthy of it**, and which would only be able to appear in the full light of day once "true" paternity, the one that should normally have been his (and for twelve years already. . . ), was the object of a general and intangible consensus. The "perverse" article, the jewel in the crown of the Colloquium of the same name, was a first milestone in this direction, laid down by the principal interested party with his customary skill.

I have the impression that Beilinson and Bernstein, no doubt flattered to see themselves unexpectedly associated with paternity over the so-called (but wrongly) perverse beams, and with an even more prestigious kingpin, were in fact manipulated by Deligne, so that they could be used as alibis "just in case... .". As the article is written, any uninformed reader can only assume that it is none other than Deligne, of course, who is the author of the providential "correspondence", though never named or spelled out (since everyone is supposed to know it already. . . ).

All that's left is this (carefully calculated) shadow of ambiguity, in this brilliant turn of phrase, about the unnamed "relationship" that "should have found its place in these notes... ."<sup>810</sup> (\*). This was the "inch!" way of delicately and clearly implying, without actually spelling it out, that the said relation (in the absence of any mention to the contrary) was at least due to **one of the three authors of** the brilliant article, or (at the very least) to all three jointly. But it was also clear that when the time came (for whoever who can wait... ), it would be neither Beilinson nor Bernstein who would compete with a Deligne for a paternity that was already all but assured. There must have been a deal<sup>811</sup> (\*\*), tacit if not ☐ expressly formulated: to Beilinson etp

Bernstein's Kazhdan-Lusztig conjecture and (for good measure, given that there was already Brylinski-Kashiwara

<sup>810</sup>(\*) On this subject, see the aforementioned note "Le prestidigitateur" (n° 75).

<sup>811</sup>(\*\*) The presumption of such a "market" came to me by association with two analogous situations. On the one hand, the market (perhaps (The latter "sacrificed" the Lefschetz-Verdier formula, which was written off, for the purposes of the "SGA 4<sup>1</sup> - SGA 5 operation", but in return "picked up" all the "duality" inheritance of the deceased, and the derived categories (discount article) as a bonus. (For the detailed story, see the sub-notes group "La

Formule", n° s 169<sub>5</sub> - 169<sub>9</sub> .) On the other hand, there's the "deal" Deligne struck with a master who had been declared deceased, and who had in any case disappeared from circulation and was unlikely to react, concerning the SGA 7 seminar held jointly during the

two years 1967/69, which was "shared" three years later by half and half, one for the deceased, the other for Deligne and a makeshift teammate. (For details, see e.g. "Episodes of an escalation", note n° 169 (iii), episode 2.)

It also goes hand in hand with the "deal" with this same (unsuspecting) deceased for the so-called conjecture (Mac Pherson dixit) "de Deligne-Grothendieck" (see episode 1 in the same note already quoted): the first half for "the letter carrier" Deligne, who had informed Mac Pherson of a conjecture (kept secret until then by the care of my cohomology students), and the second for the deceased, in his capacity as "collaborator" of the first... ..

famous nameless "relationship", awaiting the day, soon to come and without his modesty needing to be disturbed, when everyone would call it the "Deligne theorem". And the future "father" had a good enough nose to know at least this much about this child (whom he had previously repudiated rather than agree to give birth to. . . ): that he had concluded a "good deal"<sup>813</sup> (\*\*). As for Kashiwara, his role was over, and there is no more mention of him in the brilliant article, in connection with the providential "relationship", than there is of the service unknown. All against one when it's a vague stranger, all right - but once the place has been cleared of an intruder, every man for himself ... .

**(f) The parade of actors - or the mafia** The "family album", opened just three weeks ago<sup>814</sup> (\*\*\*), has just been unexpectedly enriched by a few new faces. The "family" has obviously grown a lot, and the old-timer that I am finds it hard to identify with it, especially as times have changed for the better. changed. This time, in order of appearance<sup>□</sup>, it was **M. Kashiwara**, **R. Hotta**<sup>815</sup> (\*), **J.L. Brylinski**, and the **anonymous referee** of the Brylinski-Kashiwara article at Les Inventiones. A group of "tough guys", that's for sure, with well-honed reflexes, and what's more, a finger-in-the-eye agreement when it comes to ripping off a vague particular, at a discreet sign from the Big Boss behind the scenes (or even, without waiting for a sign. . . ).

And once again, I'm back to the allure of a **mafia**<sup>816</sup> (\*\*), reigning supreme over their uncontested fiefdom, the heart of which is the cohomological theory of algebraic and other varieties. Brilliant, hard-working people with impeccable brains, whom I saw at work throughout the four successive episodes of the so-called "operation of the unknown on duty", culminating in the Colloque Pervers. In addition to the four bigwigs I've just mentioned (including one anonymous one), I'd like to remind you of the five other members of the "hard core"; that's nine who mobilized to bury **the Intruder, the one who wasn't one of them**.

There's the Grand Chef, **Pierre Deligne** - the man who always knows how to "wet himself" the least, while pocketing the most. There's his second-in-command, **Jean-Louis Verdier**, known as "the benefactor" - the same man who chaired the jury for a certain thesis by a certain unknown man, and the same man who was one of the two organizers of a memorable colloquium shamelessly plundering that same unknown man. Then there's the other main organizer, **B. Teissier**, who

co-signed the memorable Introduction to the memorable Proceedings of the memorable Colloquium. Unlike the others, it semblerait that he acted merely as a comparse and prête-nom, while he had nothing to gain for himself - except the pleasure of pleasing people he knew to be prestigious and unscrupulous. And last but not least,<sup>817</sup> (\*) **A. Beilinson** and **J. Bernstein** (whose work I've just done in this very room).

<sup>812</sup>(\*) See "Perversity", n° 76,

<sup>813</sup>(\*\*) It's a "good deal" that at the same time seems to me to be a very bad deal, even (and especially. . . ) in the case of where everything goes to plan for the interested party, wasting precious gifts and creative energy playing gangster.

<sup>814</sup>(\*\*\*) See note of the same name dated March 22, n° 173.

<sup>815</sup>(\*) An attentive reader may be surprised not to find in this "actors' challenge" (in the swindle-mystification around of Zoghman Mebkhout's work) the name of Kawai, co-author with Kashiwara of the oft-quoted article, par. 4 of which shamelessly plunders Chapter III of Mebkhout's thesis. (See on this subject the note "The five photos (crystals and *D* - Modules" n° 171 (ix), and in particular page 1005,) Mebkhout insists that Kawai cannot be lumped in "the same bag" with Kashiwara (be it would be content to follow, eyes closed... ). He described him to me as a guy who was a bit out of his depth, and I got the impression that he'd taken a liking to him - he's basically his "good Japanese", and there's no way he'd let me touch him! That's probably why he refrained from writing to him (as he had written to Hotta, another of Kashiwara's team-mates), to point out the frauds in his article with Kashiwara, and thereby oblige him to show explicit solidarity with his team-mate and boss.

<sup>816</sup>(\*\*) This unusual impression had already occurred to me last year, in the note "Le Colloque" (n° 75) (you can guess which one. . . ), in view of an atmosphere of racketeering such that it seemed as if we were dreaming, or witnessing "a film about the reign

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throughout the present peregrination through the misadventures of the vague stranger on duty... .  
<sup>817</sup>(\*) (May 25) This "enfi n" turned out to be premature - other gang members have since come to my attention. See

more acquaintance), delicately moved by invisible strings. ...

And I await, without impatience or illusions, what other Perverse Colloquia the future holds in store for us, with the unreserved acquiescence of the entire Congregation, for the greater Glory of "Science" and for the "honor of the human spirit".

## Roots and solitude

**Note** 171<sub>3</sub> (April 18) At the end of this fourth day spent following step by step the misadventures of my friend Zoghman, I understand better than last year attitudes and dispositions, towards me in particular, which had seemed strange to me only last year. In short, with his work, the scope of which he was well aware, he had thought he was entering "a big family", a bit like that of the deceased master, of whom nobody ever spoke, it's true, and yet who was present even without being mentioned. And now he found himself in a world of sharks with polite, even affable airs, and ruthless teeth - stripped in a jiffy of what he had brought with him, the fruit of eight long years of solitary labor; after which he was made to understand that he had been seen enough: an intruder and an intruder. There aren't many in his place who wouldn't have been traumatized. I don't know if he's ever opened up to anyone about his setbacks, except in bitter hints, so vague that they seem to testify against him, like an embittered man, a bit of an associate.

I may not have been named, but I was the "Father" of this unscrupulous world, and there was really no reason for him to trust me. It's true that our first meeting in 1980, when he was still a thousand miles from suspecting what lay ahead, laid the foundations for trust, and I feel that, against all odds, those foundations have been preserved to this very day.

Deep down, he knew, shark "father" that I am, that I wasn't going to do what they did. But there was a **grudge**, that's for sure, and she liked to take□the allure of a distrust that would have wanted to be visceral, and p . 1080 which (so I felt, at least) was "tacked on".

It's easy to "fight" for what you believe to be your right, when you're part of a group, however small, with which you feel in unison. But the one who is alone against all, the outcast, the unwelcome stranger, is like a tree deprived of its soil. The strength within him is of no help; it becomes bitterness that turns against itself, as if to chorus with the whole world, which rejects him.

When I held in my hands this book which consecrated the exhumation of the motifs as well as the burial of the worker who had brought them to light, this book signed by four of the most brilliant authors of a brilliant generation (which I had helped to shape) - when I finally became acquainted with it, by the greatest of coincidences (given that until then nobody had noted anything in particular that was worth pointing out to me. . . ) - at that moment I knew, for the first time in the thirty-six years I had been acquainted with the world of mathematicians, that I was alone against all. ) - at that moment I knew, for the first time in the thirty-six years I'd been acquainted with the world of mathematicians, **that I was alone against all of them**. A lot of things that had happened over the past eight years suddenly came together and made sense. It's a funny feeling when you suddenly rediscover that solitude. I had to catch my breath that day, and throughout the weeks that followed, taking in day by day the full dimension of L'Enterrement - a burial worthy of the work.

But this has nothing in common with Zoghman, who was "left behind" by his own people before he could really take root. Fate had smiled on me. Thanks to the elders who had taken me in (and it didn't really matter that they were dead or retired and perhaps hadn't been doing maths for a long time).

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b. de p. note (\*) page 962, in the note "Le jour de gloire" (n° 171 (iv)).

(May 30) Latest news: yet another member, R. Remmert, has just been identified. See part (c ) of this note ("Les mémoires défailantes - ou la Nouvelle Histoire").



myself. These roots have plunged and grown, and over the years they have become deep and powerful. These roots are firmly planted in a soil that is neither that of "consensus" nor that of any fashion - more deeply, no doubt, than in any of those who find satisfaction in making fashions and following them<sup>818</sup> (\*).

□ I can afford, in short, to be "alone against all" - say what I have to say, and go my own way.

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(May 25)<sup>819</sup> (\*) It doesn't take much imagination to understand the frustration of Mebkhout, who suddenly feels "swept away"<sup>820</sup> (\*\*) like a straw, once the strength of his central result is recognized. He writes to me (in a letter dated April 24, after his recent visit to my home): "It took me eight years to put together the results used in the Kazhdan-Lusztig demonstration. It took them a week to demonstrate it." A restrained him, this time again, from going to the end of what he really felt, surely, and I take it on faith that he will do so.

p. 1082

□ moi ici d'ajouter le "nondit": et une fois la chose faite, "ils" se pavant fiers entre eux avec l'outil that another had fashioned in solitude, letting the worker know that he'd seen enough... ..

It's such an enormous thing, however, that Zoghman doesn't quite believe the testimony of his healthy faculties - just as I had trouble believing the testimony of mine, on May 2 last year, when I read the Proceedings of the Luminy Colloquium<sup>821</sup> (\*). It was when he read these same proceedings in January last year, three years after the Kazhdan-Lusztig "Dress Rehearsal", that Zoghman finally came to realize what had really happened.

<sup>818</sup>(\*) Although I've never bothered to follow or follow fashion, whether in mathematics or elsewhere, I know that this is precisely one of the manifestations of the strong roots I was lucky enough to develop in my early childhood. Having had strong roots in myself from the outset, the energy mobilized in my major investments is not dispersed by compensatory cravings, such as the craving to set the tone, or to be and appear in conformity with the de rigueur "tone".

I express myself concretely on my childhood and these "roots" (without pronouncing the word, I think) in the note "L'innocence (les épousailles du yin et du yang)" (n° 107).

<sup>819</sup>(\*) The following two pages are taken from what was originally intended as a b. de p. note to the note "... et l'aubaine" (n° 171 (iii)). I had some hesitation as to where to insert them, and finally decided to include them in the present "Roots and solitude". Indeed, this is the only note in "L'Apothéose" in which I have tried, on the basis of my own experience, to grasp as best I could the way Zoghman himself experienced the events and situations I chronicled.

<sup>820</sup>(\*\*) The expression "swept away" is borrowed from a letter by Mebkhout (from the day before the one quoted in the main text), from which I reproduce here the relevant passage:

"It's true that [Kashiwara's] theorem of constructibility... .. allowed me to get started. In fact, from that moment on, someone like Deligne would have found all my results in the blink of an eye, including the theorem of the good Lord in all its forms, with demonstrations in four spoonfuls, as you say. That explains why it was all swept away in a few days."

It seems to me that Mebkhout has spelled out exactly the tacit "reasoning" of a Deligne, appropriating the fruits of others' labors because he **could** (and **should**) **have** found them himself (with his means, baggage and all) "in four spoonfuls". The only problem with this line of reasoning (which we're often tempted to adopt in similar situations) is that **it was all a matter of thinking about it** - and it was Mebkhout, not Deligne or anyone else, who "thought" about it. Creation is not a matter of **technology**, which, once it has seen something that no-one else has been able to see, "sweeps away" a situation in less time than it takes to write it. Creation is not in the "sweeping", but in **the act of seeing** what no one else has been able to see; of seeing with one's own eyes, without "following" anyone. And it's part of the probity of being a mathematician to distinguish between one and the other - between the act of creation, and the turning of a crank that goes round and round.

<sup>821</sup>(\*) On this Colloquium (June 1981), see "Iniquity - or the meaning of a return" or "The days of glory" (n° s 75, 171(iv)). In fact, the writing, in the first week of May last year, of "Cortège VII: Le Colloque - or bundles of Mebkhout and Perversity" (n° s 75-80) was still not enough to overcome this almost insurmountable inertia "according to the testimony of my healthy faculties", in a situation where one is rigorously alone in doing so. use. It was only five months later, when I came face to face with reality "in the flesh", so to speak, in the person of my friend Pierre (Deligne) who came to see me in my retreat, that a secret and tenacious incredulity finally vanished. On this subject, see the note "Le devoir accompli - ou l'instant de vérité" (n° 163), especially pages 782 to 784.



It was a terrible shock, I understand - Zoghman thought he was going to lose his life. Fortunately, he's a solid man - Zoghman is still alive today, and has even married and fathered a child in the meantime. ... But I think that even then, when he held these "Acts" in his hands, he still couldn't believe them completely. Something must have "stuck". As it happens, he still doesn't fully believe it, even as I write. It has to be said that, even in simply "rational" or "objective" terms, the thing is so incredible, so enormous, that to this day, **nobody** except me (except maybe him, and still. . . ) has dared to believe his eyes and see it, even though it's bigger than a cathedral!

But for those who have been hit head-on by the cynical, **gratuitous** iniquity at the hands of their admiring elders, it's not enough.

res, fulfilled with everything - surely this is one of those things you can never quite believe, one of those things **that "go beyond understanding"**. . . And it's also the kind of thing that can devastate a man's life.

What gives them this□destructive power is the obscure perception, desperately repressed and yet

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**for** the pleasure of crushing with a careless gesture that which is precious to you, the very thing (if possible) that makes up the substance and salt of your life. It's this perverse pleasure in malice "for nothing", which truly "passes understanding". . .

I don't think Zoghman ever really talked to anyone about it, either before or after the big move - except in monosyllables, indecipherable to anyone but himself. The Kazhdan-Lusztig episode alone was already too enormous, too implausible for him to expect anyone to believe it. Established consensuses sweep away the most obvious, the most obvious, the most irrefutable facts like chaff. And here, he was dealing with something so painfully close, so "raw" in his being, that the only risk was that the person to whom he opened up about it would reject the unwelcome message, that his distress at "what passes understanding" would not be accepted - this risk or probability took on the dimension of **the intolerable**, something to which one will not expose oneself for any price - even if it means dying on the spot, if one has to die... .

To me, two years ago, he spoke of it "in monosyllables". Perhaps deep down he was hoping that I would understand these monosyllables, not only in their literal sense, but that I would also hear in them everything he didn't dare to say out loud (perhaps not even to himself. . . ). It was a completely mad hope, to be sure (in a situation where everything seemed mad as hell!); I was a thousand miles from imagining anything of what I've since learned, with certainty. It couldn't have been any other way, in the absence of meticulous, detailed information<sup>822</sup> (\*). And Zoghman, for his part, was also a thousand miles from daring to give me this information. It was crazy, but that didn't stop him from being angry with me. He had to blame someone, someone close enough to him, someone tangible, on whom he could transfer at least part of what had been triggered in him by "what passes understanding", and free himself in some small way from what was eating away at him.

## Carte blanche for pillage - or High Works

**Note** 171<sub>4</sub> □(June 2) It's been two months since I had the satisfaction of putting the "final touch" under Enterrement, with the ultimate "De Profundis" note (of April 7) - and it's been two months too since I've been hard at work putting "the final touch" to the last part of l' Enterrement! It's the reissue, more or less of what happened around this time last year - when I was still

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<sup>822</sup>(\*) (June 1) It would be more accurate to say that it "couldn't be otherwise" in my state of limited openness and presence, except on very rare occasions. I believe, however, that we are all equipped with an "ear within an ear", perfectly capable of hearing the unspoken - but more often than not we take care to exclude from the field of conscious attention the messages picked up by that ear... ..

putting the finishing touches to what was to be the first part of the Funeral. It was, as it is now, the "last minute" that was dragging on and on - to the point where I was forgetting about eating, drinking and, above all, sleeping. It went on like that until my body gave up, at the end of its tether. That was exactly a year ago (give or take a few days), and I had to drop everything for more than three months, fully occupied with pulling myself out of a state of acute exhaustion<sup>823</sup> (\*). But this time I'm wary, and I'm very careful not to go down the same road again. I care about my skin...

Once again, it was the "investigation" that never ends. I was planning a ten-page memo called "The Four Operations", which would summarize and "tidy up" the results of last year's whirlwind investigation. And now it's been four months since the survey started up again, the ten pages have become three hundred or so, and it's not (quite) finished yet! I don't dare make any more predictions - this is the ninth month, since resuming work at the end of September, that I've been "on the verge of finishing"! I won't know it's **really** over until the last packet of notes has been typed up, proof-read and corrected, and handed in for duplication. (After that, the rest is no longer my job.) All I know is that I can't wait to get to that point, just as I can't wait to see the end of a long, grueling illness; and that I have to see it through to the end, as best I can, without letting imaginary deadlines get in the way. I won't stop to breathe until the end, when everything that needed to be seen and said **now**, will have been seen and said.

It's that damn "Apotheosis" that's given me the most trouble - I can't say why. These "four operations" are the only part of Harvest and Sowing that came in bits and pieces.

and struggling - when in principle it was supposed to be everything cooked, a simple "mise en ordre" yes; nothing that engaged or challenged my person in a **névralgique** way, so as to mobilize forces of resistance, a

**"friction"**. And yet God knows there was friction, and with the Apotheosis more than with anything else! Where does it come from?

Already with "Les manoeuvres" it was laborious. That's when it started stretching to infinity. It ended up being eighty tightly packed pages for that operation alone - and now, a month later, Apotheosis is well over double that. And yet, with the possible exception of a few pages (a little too "detec- tive" around the edges. . .) in "Les manoeuvres" (where I enter, perhaps, more than would have been essential into the intricate details of a certain impossible "scam". . .) - apart from this circumstantial "work on parts", which is no doubt a bit of a pain in the ass for a reader who's not "in the loop", I don't feel that these hundred-page packets I've ended up lining up here are superfluous, or even a rehashing, a splitting of hairs. What kept me on my toes was precisely the abundance of **new** and unexpected **substance** that was pouring in, and that I absolutely had to fit in, whether I wanted to or not - including, yes, mathematical substance! At times, I felt overwhelmed, so many things at once that I had to put down in black and white dare dare - things that were all hot, even burning, and yet you're obliged to deal with them one after the other... .

Yet such richness is in itself a powerful stimulus to work, and in no way does it create "friction" - quite the contrary. This friction, to be sure, comes not from the substance itself, but from the strength of my egotic investment in the work undertaken. Paradoxically, it's my very impatience to "get it over with", to "throw down the gauntlet" of what I have to say, about such and such things that are happening right now and that concern and affect me closely - it's this impatience (I believe) that creates the friction, the dispersion of energy. Friction is a sign of division, of forces pulling in opposite directions, each exasperated by the resistance of the other.

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<sup>823</sup>(\*) For this episode, see "The incident - or body and mind" (n° 98).

I've been in a hurry to "get it over with", to "let it go", ever since I first set my mind to it - and there's the need to follow through on what the present moment suggests to me, not to settle for anything less than that, not to let myself be pushed around, not to let myself be trapped in a "program" to be completed, in a "schedule" fixed in advance. I know that that as soon as I exclude the unforeseen, that impediment to going round in circles, my work loses its quality and its meaning. Il ☐ devient "du gratte-papier". I have become very sensitive, over the years, to this "little difference" p . 1086 that looks like nothing, but is everything. It still happens, rarely, that such a turn begins, in moments of great heaviness - but never for long. When it does, the kid just throws it all away.

- it's no use even trying to go on. The very desire to work, that **desire** which is something other than the urge to accumulate pages or to place a period - desire and desire suddenly gone, and you find yourself foolishly blackening paper. Then there's really no point - I'll just have to put things right, and right away!

There's always a certain **impatience** at work (an old acquaintance of mine. . . ), which constantly pulls me forward. It seems to me that it's not the same as the one that's been weighing heavily on me ever since I started working on these "Four Operations". The other impatience is not a weight that weighs down, but a force that pulls. It's the sign of an appetite, not of weariness or fatigue or satiety. It's not impatience to accumulate, or to be done with it, to "finish" a program, but impatience to know the unknown before me, about to deliver itself. It's the impatience of the naked child, alone before the infinite sea, to plunge into it to know it. ... <sup>824</sup>(\*)

But now it's time to return to the story of my friend Zoghman's misadventures, in this note intended as the last of the Apotheosis. As I've already said, Zoghman himself only gives me this account in bits and pieces, here and there, in the course of letters, phone calls and meetings, and surely this has affected the progress of my thoughts and the writing of L'Enterrement, at least in the part devoted to the vicissitudes of my friend. I now have a better sense of the meaning of this reticence, as any attachment to a "victim" role (which I thought I had detected last year) has vanished (assuming it was indeed present). There must also have been times when I felt a certain saturation, expressed in an attitude of "don't throw any more away, for pity's sake! ". That must not have encouraged him. I was annoyed, it has to be said, by a ritornello about "the Japanese" here and "Kashiwara" there, which Zoghman must have been singing for four or five years, and he'd seen it with them, it's true. But I knew that if he had seen them, and if his work had been plundered in this way, in an almost official: "Go ahead, good people, help yourselves to plenty, don't be shy... . ! ", it wasn't because of some distant Japanese ☐ . It was **because of "his" people**: those of the "little family"<sup>825</sup> (\*) - good people from p. 1087 whom he never named except to quote their work with all the respect due to their high reputation.

I didn't want to hear any more about Kashiwara et al! Zoghman had the wisdom and patience to let it go, without losing his interest in my work, and without ceasing to provide me with discreet and effective assistance here and there.

It was during his last visit to my home, at the beginning of April, that I finally got to know him, the "Japanese package". At first, I was a little reluctant. I thought I was going to be bored to death by inextricable, ultra-technical stories and illegible papers (in Japanese, if that's possible. . . ), which I'd never read anyway - but no! It was as easy as pie - a bit like a pick-pocket story in the Paris (or rather, Tokyo) subways. Fun even, to say the least (at least, as long as it's the other guy who's getting his wallet nicked... . ).

<sup>824</sup>(\*) This is the image already featured in the note "L'enfant et la mer - ou foi et doute" (n° 103).

<sup>825</sup>(\*) (June 16) Mebkhout would like to emphasize that he no longer identifies with the "little family" in question.

As a result, the situation between Zoghman and myself became more open, and I was treated to bits and pieces of his misadventures, in flashes here and there. Episodes that I'd written down a bit in the style of a "technical information sheet" were fleshed out by on-the-spot reminiscences; precisely the kind of things that seem forever banished from scientific texts, in their impassive "attention to yourself", and even from letters between colleagues - you wouldn't want that! I even had to shake myself, in "Les quatre opérations", not to fall back into that very style, the "conclusions d'enquête" style (or even, "feuille de récriminations" . . . ). These "snippets" delivered by Zoghman helped me to get out of it, and to keep in touch with a living substance.

I got back to Apothéose the very day Zoghman left my house, just to make another sub-note or two, while what he'd told me was still hot. The result was "Ecllosion d'une vision - ou l'intrus", "La maffia" (which I later subdivided into seven parts, each with a name), and "Racines et solitude". I sent the whole thing to him straight away, for his comments before I gave it to the typist. I felt I was speaking a little on his behalf, and I wanted to be sure that everything I reported, from what he'd told me, had his stamp on it. unreserved approval. He sent me his detailed comments by return (letters dated April 22 and 24).

□ In these comments there are quite a few of these "snippets", putting living flesh on a skeleton of facts that appears a little skeletal at times in my notes.

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That's also how I knew Zoghman had been there, on that memorable April 22, 1980, at the Goulaouic-Schwartz seminar. This was the day when Kashiwara announced as his own the theorem of the good Lord, which he had learned from Mebkhout a few months earlier, at the Colloque des Houches<sup>826</sup> (\*)! It's that big, and with Mebkhout still in the room, it may seem unbelievable. Mebkhout didn't explode on the spot (I wonder how he did. . . ). He waited politely until the end of the presentation "to protest publicly against these methods, reminding him of the Les Houches conference and his question<sup>827</sup> (\*\*). Goulaouic asked me to settle my affairs in private. The room suddenly emptied in a matter of seconds".

So here's one of the "snippets", delivered by this laconic description. I later got some details on the phone. The incident is worth noting. It says a lot about the state of mores in the mathematical world in the '80s. We're not talking here about the mentality of some long-toothed "kingpin", an extreme symptom of the breakdown of traditional values in the scientific world, or even of the "establishment" of prominent and well-connected people, whose class reflexes favor one of "their own". Here, the whole room empties out in the blink of an eye - no one left all of a sudden<sup>828</sup> (\*\*\*)! Work it out amongst yourselves - we don't want to hear about it. ...

I wonder what went through the minds of Goulaouic and the other peaceful listeners at this seminar, where a distinguished foreign lecturer was speaking (on a topic with which none of them, I believe, were overly familiar). This incident, after all, was food for thought. I doubt □ any of them I don't think any of them took the trouble, and rather assume that they all agreed to forget the sinister incident. But in the end, if you took the trouble to think about it instead of running away, there was **one** thing that was clear, in this dark story. The tone and words of Mebkhout (someone they knew from seminars, to say the least), left no room for doubt.

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<sup>826</sup>(\*) On the Colloque des Houches and the Goulaouic-Schwartz seminar episode, see the note "La maffia" (n° 171 ), section (b) "Premiers ennuis - ou les caïds d'outre-Pacifique".

<sup>827</sup>(\*\*) This was the question posed by Kashiwara at the end of Mebkhout's presentation at the Colloque des Houches in September 1979.

On this subject, see the note cited in the b. de p. above.

<sup>828</sup>(\*\*\*) this evocation irresistibly evokes in my mind the association of ideas with the very similar situation I'd experienced three years earlier, at the end of a Bourbaki seminar where I'd been given ten minutes to talk about a certain scurrilous law affecting foreigners. See "My farewell, or: foreigners", n° 24.

There was little doubt that there had to be a **swindler** in the story - either Mebkhout, or Ka-shiwara. It's possible, of course, that inwardly, they'd already made up their minds: Mebkhout was making it up, so how could anyone imagine the distinguished visitor looting the anonymous listener! This would mean that, in relation to an unknown person, the famous man, whatever he does, is above suspicion: it's **carte blanche for plunder**, given to the man of notoriety against the man who has no recourse. What he has to say goes unheard: "Work it out amongst yourselves!"

Or else they've buried themselves in a state of doubt: how can you tell who's telling the truth and who the lie? (It's true that the brutal nerve of a Kashiwara, publicly pillaging a vague stranger in the presence of the person concerned, hardly seems believable. But it would be an even more incredible thing after all, if a vague stranger (whom they all know, and who hadn't brought himself to their attention yet by crooked tricks nor by his nerve... .) dares to publicly accuse a Kashiwara of crude plagiarism, if what he has to say is pure fabrication... . And supposing that what he says is perhaps well-founded, to send him packing with a "sort it out amongst yourselves!" is once again *carte blanche* for looting. It's as if we were shouting to someone who's been robbed in the middle of the street by thugs in tuxedos and who cries out "au voleur!" - "work it out amongst yourselves!"

It seems that this is how it's been for ages, in the slums of New York and other big American cities, where no one wants to have anything to do with the mafia that rules there. At least, that's how things are these days (I can't say for how long), in the mathematical world and in what passes for the "beaux quartiers", such as the Séminaire Gaulouic-Schwartz<sup>829</sup> (\*), or among all those prestigious people who "do" cohomology of algebraic varieties.

□ In rational terms and taken at face value, this "work it out amongst yourselves" borders on debility, in a situation where it's clear anyway that one of the two parties must be acting in bad faith. From a psychological point of view, this idiotic formula reflects an **abdication of** responsibility in the face of a situation that is felt to be "embarrassing". It also reflects a deliberate ignorance of an obvious fact: the question of respect for the elementary ethical rules of the mathematical profession is by no means a purely "private" affair, to be settled between the one who arrogates to himself the right to disregard them, and the one who pays the price. It's a **public matter**, one that concerns **every** mathematician.

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General indifference and a panic to assume personal responsibility allow a gangster mentality to flourish with impunity in the scientific world, as well as operations as shameless as the Colloque Pervers. The panic of some and the impudence of others are like the other side of the **same corruption**. Those who ran away with their ears plugged on April 22, 1980, contributed to the Apotheosis of the memorable Colloquium the following year, just as much as the bigwigs who staged the grandiose hoax and proudly strutted their stuff.

(June 3) It was during Mebkhout's last visit to my home, too, that he gave me some edifying details about some of the participants in this same brilliant Colloquium, and the "new style" that is flourishing among them. I had a chance to leaf through the proceedings, in the second volume of the Actes, where there are articles by Verdier and Brylinski-Malgrange, and to take a look at Laumon's thesis (with a more informed and less distracted eye than the day I first received it). This thesis is in fact a collaborative work with N. Katz. I give some comments on the "new style" followed in this work, in the long b. de p. note to the note "Le jour de Gloire" (God knows it deserved that name. . . ), page 962. For further details, I refer you to this note

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<sup>829</sup>(\*) I'm happy to report that Laurent Schwartz was not in the room on the day of the memorable incident at "his" seminar. I don't know if he was subsequently informed.

(not yet written). A promise made, a promise kept!

p. 1091 Mebkhout told me how he had had the honor and advantage of talking to N. Katz twice about his ideas on duality and the links between continuous and discrete coefficients. The first time was at the Colloque d'Analyse  $p$ -adique in Rennes, in July 1978. He then explained  $\square$  "in small group" his theorem of global duality for  $D$ -Modules, on a complex analytic space - the theorem that caps Serre's duality and Poincaré's<sup>830</sup> (\*). There were Katz and Illusie, the same two mentioned more than once in L'Enterrement. Illusie, kind and gentle as usual, thought it was really very pretty - something like this<sup>831</sup> (\*\*). As for Katz, who I imagine was hearing about  $D$ -Modules for the first time in his life (at a time when it was far from being all the rage, as after the memorable Colloque), he simply declared curtly "C'est connu ça!", and turned on his heel. As long as it was a vague Monsieur Personne who was talking to him, N. Katz (who that same year was to give a speech in front of thousands of distinguished colleagues, in honor of the new Fields laureate Pierre Deligne... ), he was bound to be "famous".

The second time was shortly after the Colloque des Houches in September 1979<sup>832</sup> (\*\*\*). Katz was then at IHES. Given his well-known competence in  $p$ -adic differential systems, which Mebkhout sensed had something to do with the theorem of the good God he'd just talked about at Les Houches, Mebkhout went to IHES on purpose to bring him his paper from Les Houches, and to talk to him about his ideas and results. After the welcome he had received in Rennes, it's fair to say that he had the persistence to keep going! In any case, it was more of the same. Katz once again received a very high reception from this vague stranger, who took the liberty of coming to ask him a second time, and without announcing himself yet, if that turns out to be the case. When you're an important man, you sometimes don't know how to protect yourself from intruders... ..

p. 1092 Just one year later, these same ideas, long held and matured in solitude by a vague unknown, are trumpeted everywhere as the latest find by a Deligne (or a Kashiwara, we weren't sure. . . ), in the wake of such a brilliant Colloque that Katz was unfortunately unable to attend. to honor them with his presence, so that they take on  $\square$  for the great man both importance and weight. It must have been Laumon who explained the ins and outs to him - one of Deligne's most brilliant disciples. This same Laumon also knew first-hand the origin of these ideas, having been informed of them by the vague stranger himself. But the disciple takes pride in following in the Master's footsteps, and the latter had made it quite clear, and without the slightest equivocation, what conduct was to be adopted towards one doomed to silence and obscurity.

To the Delignes and Verdiers the limelight, and to the Brylinskis, Katzes and Laumons, who rushed in at just the right moment to get their share! To them, the music and flons-flons, and the standing ovations of a grateful crowd, who came out in jubilation to celebrate these High Works, in the hands of their New Masters.

## Epilogue beyond the grave - or the sacking

p. 1093 **Note 171**  $\square$  (June 14) Up until a month ago, it had seemed to me that the spirit of Burial was limited to what I sometimes call "the beautiful world" or "the great world" of mathematics, and more particularly, the

<sup>830</sup>(\*) This theorem is discussed in the two notes "L'oeuvre. . ." and "Three milestones - or innocence" (n° 171 (ii), (x)).

<sup>831</sup>(\*\*) It was a gratuitous "kindness". While the style of reaction was different from one to the other (in "yin"), it was the same from one to the other.

in Illusie, in "yang" in Katz), the bottom line was the same: as long as it comes from Monsieur Personne, it goes in one ear and out the other! On this subject, see the note "La mystification" (n° 85), in particular my comments on Illusie, page 351.

<sup>832</sup>(\*\*\*) On the Colloque des Houches and the Kashiwara scam at the Goulaouic-Schwartz seminar, see the note "La maffia" (n° 171 ), part (b), "Premiers ennuis - ou les caïds d'outre-pacifique".

The world I used to haunt and to which I myself belonged. At the USTL (Université des Sciences et Techniques du Languedoc, Montpellier), which has been my home institution for the past twelve years, I did not perceive any signs of ostracism, disrespect, discourtesy or even rudeness, in line with the burial that has been in full swing for the past fifteen years<sup>833</sup> (\*). A new fact has just burst into this peaceful picture, and drastically transformed it, and my own relationship with my home institution.

In keeping with ingrained mechanisms, I did not at first consider including this recent incident in my "Harvest and Sowing" testimonial, which at first sight seemed to come to me "like hair on the soup". It was against serious resistance that I finally admitted that it would be failing in the spirit of my testimony to pass over this episode in silence. It's still a fresh episode, of course, and one I've "taken in" rather hard - which, incidentally, gives added force to those "inveterate mechanisms" I've just alluded to. But the sheer force with which I took the eloquent and unwelcome lessons of this incident this time, is also a sign that it affects me very closely - and this at the level of my professional activity and my links with the professional milieu to which I belong. This, then, is typically the kind of thing that Harvest and Sowing is intended to be an in-depth account of, with no "reserved corner" that I would forbid myself from touching, whether out of misplaced "discretion" towards myself or anyone else.

Moreover, in the more specific context of my reflection on the Burial, I feel it is obvious that there are direct links between it and the incident in question. It's possible that these links are not those of a simple cause-and-effect relationship: that certain colleagues on the spot would have ended up taking note of the Burial, and would have concluded that they, too, could now "give it a go". Even if there were such a causal link, it would, it seems to me, only affect an incidental, accidental aspect of the situation. A more essential aspect, on the other hand, and one that struck me the most, common to what happens in "the world" of Science (with a capital S), or in a modest provincial university, is a certain

□ **deterioration**, unprecedented perhaps, in scientific and academic circles: deterioration at the level of p. 1094 quality of relationships and basic forms of courtesy and respect for others, as well as scientific ethics, itself indissolubly linked to respect for others and for oneself. The pages that follow can therefore be seen as a contribution (among the many others already provided throughout the reflections on Burial) to the "tableau de mœurs d'une époque", or of the end of an era no doubt, in the mathematical milieu.

Rather than give a more or less detailed account of the events, I prefer to reproduce four of them here **documents**, which will describe them as well. These are:

1. a "letter to my colleagues teaching mathematics at the USTL", dated May 28, in which I inform them of a certain situation and express the wish for a discussion at a General Meeting;
2. the "reply" from Mme Charles, who is in charge of the mathematics building at the USTL, in the form of a circular letter dated May 30, addressed by name to me, and in fact to all mathematics teachers;
3. the resolution passed by the EBU 5 General Meeting on June 6 on the agenda: "Information and discussions on the relocation of Professor Grothendieck's office"; and finally
4. a "Letter to my former colleagues in the Mathematics building", dated the following day, June 7.

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<sup>833</sup>(\*) I express myself along these lines in note no. ° 93 (page 396, paragraph 3).

I have refrained from including among the documents my letter to Mrs. Charles of May 21 (referred to in the first quoted document) and my letter to Mr. R. Cano, Provisional Administrator of the USTL (referred to in this same document, and in document 4°, or "Epilogue d'un malentendu"); these letters do not seem to provide any new information, compared with that contained in the documents reproduced below.

As my only comment to Mme Charles' letter ("it is in fact very difficult to contact him" - "him" meaning my modest self, to whom the letter is supposed to be addressed), I'd like to point out that letters from Montpellier to my home take a day to arrive, and that for years I've only been away from home when I'm at the USTL.



Institute of Mathematics

## RANSACKING THE MATHEMATICS BUILDING

Letter to my colleagues teaching mathematics at U.S.T.L. by  
Alexandre GROTHENDIECK

Montpellier, May 28, 1985

Dear Colleague,

I was informed last week, by an EBU secretary whom I had asked to pick up some work from my office on the fourth floor, that it had been emptied of all my belongings.

- which I was able to verify today: all that's left is the bare floor. I had not been informed that my office would be requisitioned without further ado, so I had been unable to give my consent to the operation, let alone authorize anyone to enter my office in my absence and touch my belongings. On the same day, I telephoned Mr Lefranc, director of the EBU, to inform him of the situation, which (it seemed) was the result of an initiative by Madame Charles, something which seemed to be confirmed by this phone call. I made it clear to Mr Lefranc that I was shocked by the procedure, that there was no way I would agree to an office transfer being carried out in such a brutal manner, and that I expected my belongings to be returned to their rightful place as soon as possible. He assured me that he would do whatever was necessary. On the same day, Tuesday May 21st, I wrote to Madame Charles, telling her that I considered the untimely "emptying" of my office to be an abuse of power, and felt it to be violence; that I expected a detailed explanation from her, and an unreserved apology. If not, I would submit the matter to the University Council, which would decide whether such behaviour towards a USTL lecturer should be considered acceptable.

Coming to the USTL today, I could see that Madame Charles had not seen fit to reply to my letter (a copy of which I sent to Messrs Cano and Lefranc). Nor has Mr Lefranc seen fit to send me any explanation for the fact that my office is still empty of my belongings, a week after he assured me that he would arrange for their return to my office. Neither he nor Madame Charles has seen fit to inform me of the whereabouts of the items that have been collected. I was told by the secretaries that the items would be stored in the office of one of them. Having bumped into Madame Charles in the meeting room, she assured me that she had only followed the instructions of the EBU director, Mr Lefranc, and asked me to speak to him about this matter, which did not concern her. Until the situation has been resolved, Mr Nguiffo Boyom has kindly agreed to share his office with me.

□ Maybe I'm the only one who finds there's something wrong - a violence and contempt; it's p. 1096

It's true that I'm the only one who's being thrown out without any further ado. (If there's anyone else out there who thinks this isn't the kind of atmosphere they'd like to work in at USTL, I'd really appreciate it if they'd let me know ... .<sup>834</sup>(\*)). For my part, I consider that it would not be a luxury if, following this "misunderstanding" (to use the charming euphemism of one of my colleagues), there were a meeting of the EBU, to give the Director, Mr. Lefranc, and Madame Charles, the opportunity to

<sup>834</sup>(\*) It goes without saying that such a gesture only makes sense to me if it is understood to be binding on the signatory, who authorizes me to make public mention of it.

to explain their intentions and motivations, and for EBU teachers to say whether they consider these procedures normal (when applied to others. . . ).

In the twelve years I've been at USTL, I've often had the opportunity to appreciate Mr. Lefranc's benevolent disposition, dedication and efficiency whenever it was a question of rendering a service - and I'm grateful to him for it. It is with all the more regret that I would withdraw my confidence from him, seeing that he is making himself an instrument in the hands of others and allowing an atmosphere of arbitrariness and contempt to develop. From now on, I urge him to assume his responsibilities as director of the EBU, or to step down. And I call on Madame Charles to resign from her position as "Head of Premises" at the EBU, a position she has abused to her heart's content.

We look forward to hearing from you.

Alexandre GROTHENDIECK

P.S. Being inclined to be of service, last year, at Monsieur Lefranc's request, I agreed to an office exchange with Monsieur Lapacher, who (I was told shortly afterwards) subsequently changed his plans. It goes without saying that my agreement did not mean that I authorized the ransacking of my office, at that time or any other.

MATHEMATICS

□Madame J. CHARLES "responsible for the premises at the Institut de Mathématiques".  
to Mr A. GROTHENDIECK, Professor of Mathematics.

p. 1097

Dear Colleague,

1. Where does the "work" of the "premises manager at the Institut de Mathématiques" begin and end?

This "manager" receives requests from Mathematics teachers

- or to house a new teacher (or researcher)
- or to house a teacher (or researcher) already accommodated elsewhere.

In this second case, requests are generally motivated by a work objective: bringing together members of the same group.

This "manager" then studies the possibilities first and foremost with the director of U.E.R.5, who is officially the manager appointed by the President of U.S.T.L., for the premises of the Mathematical Research building. He then works with the people concerned to find possible solutions; changes are made only after agreement has been reached.

2. This is what has been achieved in recent years:

- grouping of geometry group members
- mechanical group members

3. The difficulties encountered in this "work" :

- virtually every person contacted feels they "own" their office
- it seems impossible to force anyone to "change" their office.

4. The last request received by me and the evaluation of the search for "solutions" to the problem posed:

- the request made by Mr. LAPSCHER, a teacher, to put Mr. LAPSCHER and his secretary's office, Mr. MICALI, on the same level.

- the first solution envisaged: exchange of offices between the third and fourth floors, so that "applicants" could be grouped together on the fourth floor. This exchange concerned in particular Mr GROTHENDIECK and Mr THEROND. Mr GROTHENDIECK contacted by

the director of the UER 5 □lui specified that PEU LUI IMPORTAIRE L'EMPLACEMENT DE SON p. 1098  
OFFICE AS LONG AS HE HAS ONE. On the other hand, Mr. THEROND, who at one point agreed to the move, later refused.

- the second solution envisaged: I then asked Mr LAPSCHER to contact his colleagues himself to propose another solution; this was confirmed to him by the director of EBU 5. He kept us informed of his steps: the "occupants" of 5 offices had agreed to a swap, with Mr GROTHENDIECK's agreement resulting from his conversation with the director of EBU 5.
- the realization of this second solution: after taking note of this agreement, the Director of EBU 5 gave the "green light" for the proposed office modification.

Mr. LAPSCHER having told me about a problem with the keys during the period when the move would be discussed but not completed, I pointed out to him that

- no new keys were available,
- I didn't think it would be a good idea to drag out this move, which could be done in a few hours with the participation of all concerned.

Mr. LASPCHER then informed me that the equipment from Mr. GROTHENDIECK's office had been transported to his future office; this had been done without having been able to contact Mr. GROTHENDIECK beforehand.

It should be noted that Mr GROTHENDIECK lives far from Montpellier and is currently on secondment to the CNRS, making it very difficult to contact him.

5. My "responsible" impression of what would seem to be a "conflict":

- I had the opportunity to point out to Mr GROTHENDIECK that, acting on behalf of EBU 5, I could not respond to his letter myself; he would therefore have to ask the EBU 5 Director for a reply. Following this 2nd letter addressed to all of you, I feel I have to get out of the "obligation of reserve" I had imposed on myself.
- I would have thought it advisable to at least inform the people concerned before moving their equipment.
- I would also have liked the move to have taken no more than 1/2 a day.
- I thought the solution was a good one, but it didn't change the office occupancy rate of any of the people concerned.

I'm not waiting for an answer.

Yours sincerely

N.B. Copy of this letter sent for information to

- all Mathematics teachers who received Mr GROTHENDIECK's letter of 28.05.85.
- the Director of EBU 5 having also received a copy of the letter sent to me by Mr GROTHENDIECK on 21.05.85.
- the provisional administrator of the USTL, who received a copy of the letter dated 21.05.85 and to whom I enclose a copy of the letter dated 28.05.85.

Minutes of the meeting held on Thursday, June 6, 1985 at 6 p.m.

Present: M. AUBERSON, Mme CHARLES, MM. CIULLI, CONTOU CARRERE, MM. CUER, DE LIMA, DELOBEL, DE ROBERT, GROTHENDIECK, HOCQUEMILLER, ESCAMILLA, Mie HUBERT COULIN "M. LEFRANC, M. LOUPIAS, Mme MEDEN, M. MOLINO, MmePIERROT, M. PINCHARD, M. SAINT PIERRE, Mle VOISIN

After discussion, those present (19) adopted the following text by 16 votes in favor and 3 abstentions:

"The Mathematics teachers apologize to Mr GROTHENDIECK for the unacceptable conditions under which his belongings were moved. They undertake to ensure collectively that these regrettable events do not recur. In particular, it must be made clear that an office key cannot be used by anyone without the occupant's explicit agreement."

M. LEFRANC

Director

Institut de Mathématiques

Epilogue to a "misunderstanding

Letter to my former colleagues (teaching and technical staff, 3rd cycle students) in the Mathematics building

by Alexandre Grothendieck

. . on 7.6.1985

Dear Colleague, I'm writing as an epilogue to the ransacking of my office mentioned in my letter of May 28. That letter was addressed solely to math teachers, whereas it equally concerns all those who occupy an office in the math building. Inadvertently and indiscriminately, I omitted to address my letter to the technical staff and students of the 3° cycle, judging (hastily) that to do so would be to give the incident an extension that it did not deserve. I sincerely apologize to all concerned, and all the more so as I have received expressions of sympathy from several of them (supposedly uninformed. . . ), which have touched my heart. It was also no doubt due to this oversight that yesterday's EBU General Meeting on the incident was restricted to "EBU 5 members".

Among many other things, this incident will have taught me that it's not the first of its kind to occur at EBU 5 - it's just the first time that it's a "rank A teacher" who's been targeted. I don't know whether the pious resolution passed yesterday will prevent this kind of incident from happening again, in the general indifference (as before) towards non-tenured teachers or 3° cycle students in particular. I'll be sure to check with Mrs Mori and Mrs Moure that they have received instructions from the director of the EBU not to entrust the key to any of the offices to anyone, or to use it for anyone, under any circumstances, except with the express authorization of one of its occupants.

My previous letter ended with the words "awaiting your (or your) reply". In response to this expectation, I have received three expressions of sympathy and solidarity. They come from Louis Pinchard, Pierre Molino and Christine Voisin. I have also received a similar testimonial from Philippe Delobel, a 3° cycle student who (like Christine Voisin) had done a DEA with me. It was on his initiative that a few 3° cycle students attended yesterday's General Meeting. I'm delighted to express my esteem and gratitude to him, and to all those I've just mentioned, who have (without ambiguity or evasion) shown me their solidarity. It's one of the fruits of "hard" experiences like this one, to have one's friends recognized, when one is lucky enough to have them... .

I received yet another letter in reply to mine, from a colleague who was obviously delighted at what was happening, and took the opportunity to make fun of me. It's the only echo in this sense that I've received. From all the others, a great deal of total indifference on the part of some, embarrassment on the part of others (where more than once I sensed the unspoken fear of being badly seen and thus compromising one's chances of promotion, or a precarious situation). In all those, among them, who were so moved that they went out of their way to attend this General Meeting (called on the spur of the moment at the last minute, even though it had been scheduled for a week... . ), I sensed above all the deliberate intention to drown a fish, to the tune of "everyone's nice, everyone's cute". We finally settled on (after three quarters of an hour of palaver)

on the designated "villain", the absent (as it happens) Mr. Lapscher - the one who had taken (according to what had just been hinted at) □ the initiative to lend a hand. There was no question of going as far as to blame the poor guy by name - or anyone else for that matter, of course.

On the part of those "in charge" involved in one capacity or another in the sacking incident, I have been

shocked by the shameless brutality of one Lapscher, by the rudeness "for the fun of it" of a Mme Charles (who covered up the coup de main, once presented with a *fait accompli*, by adding some insolence of her own), and by the discourtesy of a M. Cano, Provisional Administrator of the USTL, dispensing with any response to the letter in which I informed him of the situation and asked him to refer it to the University Council. But most of all, I was disconcerted and saddened by the ambiguous attitude of Mr. Lefranc, Director of UER 5. From Monday May 20 (when I informed him of the situation I had just discovered and of my feelings about it) until yesterday, he had not seen fit either to inform me of what had happened, or to disassociate himself unequivocally from the brigandage of a Lapscher or the rudeness of a Mme Charles. By doing his utmost, from beginning to end, to maintain the fiction of an unfortunate "misunderstanding", he has succeeded in making behavior that I, for one, feel is intolerable seem harmless, even respectable, so as not to hurt anyone's feelings. I also took note, among other signs, of the silence of many of those I had thought to count among my friends (including three who were once my students); of the indifferent ostentation of one, the embarrassment of another, and the honeyed jubilation of yet another. And also the silence of one Micali (co-beneficiary of the helping hand, and who had had ample opportunity to convince himself, a few years ago, of the disadvantages of attracting the bad graces of Mr. and Mrs. Charles...), and the complacency of Mlle Brun, taking orders from a Lapscher to play the mercenary locksmith-mover (without a word of regret, once the nature of the operation was no longer in doubt).

Against the backdrop of all this, and finding yesterday what for twelve years had been my office, transformed this time into a battlefield - my belongings (plus the furniture) hastily rearranged (a good fortnight after a helping hand - lightning. . .) - I no longer have the heart to rearrange it again. I'm assured that the same incident is unlikely to happen to me again, and I can take the precaution of taking the second key, entrusted to Mmes Mori and Moure, with me. But insofar as this is materially possible, and in particular for the duration of my secondment to the CNRS, I prefer to forego the use of an office at the USTL, and leave the place, without a struggle, to the Lapschers, the Charles et al.

If I can avoid it, I won't go back to teaching at USTL. I'm sure I'll have spent my life there as a foreigner - one whose homeland is elsewhere - in terms of my approach to mathematics, teaching and lifestyle. What the academic microcosm had to teach me, I think I've learned, with the final "part" being the lessons of this incident, which has just closed to general satisfaction. Chances are that this EBU 5 meeting I've just attended will be my last, and that this letter will also be the last I have the opportunity to write to you (or to you). And this time I don't expect a reply.

Alexandre Grothendieck

### 18.5.5. The threshold

**Note 172** □ (March 22) I thought I'd have a day or two and a dozen pages at the most, with these p. 1102 the famous "four operations" that I've been planning to review since October. I've been hard at work on it for over three weeks now, lining up well over a hundred pages - and I'm still not quite finished! The first draft, from February 26 to March 1, already took me four days. It just provided me with the canvas, on which to embroider (after all) a "story", and not just investigative conclusions. When I reread this first draft the day after March 1, it gave an unfortunate impression of a "sheet of paper".

de doléances" that went on and on, and as it stood was probably incomprehensible to all but three or four truly expert readers (assuming they had the patience to read it. . . ). I realized that I had to at least explain roughly what it was all about, so at least give some context - otherwise there was no point<sup>835</sup> (\*).

This inevitably led to a few repetitions, compared to the first part of Burial - but there are cases where repetitions are not only useful, but even indispensable (in mathematics as much as anywhere else). In such cases, moreover, we soon realize that the so-called "repetitions" are not really so, for what is "repeated" is in fact **revised, seen anew** and in a different light. By situating certain aspects of my work as "context" for the four operations, I feel I've learned something about it, that I've been able to situate it better. I may not have learned anything really new about myself or others in the process, but I don't regret the trouble I've taken to rewrite this first grief draft over several days. I had put the best I had to give into this work, and it deserves the hindsight that maturity gives me to look at it anew and in a different light. At the very moment when I was about to make a detailed assessment of what this work has had to endure since I left it (in good hands, I had no doubt. . . ), it was right that I should reflect on it, on its place and on the unity that makes up its beauty, if only for the space of a few pages, as a way of once again showing my respect for what I have seen scorned.

□ But that wasn't all, far from it! Abandoning the "feuille de doléances" style, with numbered cross-references to the fleshier notes in the first part of L'Enterrement, I realized that these notes, which I was taking up again, like all the other sections and notes in Récoltes et Semailles, had to be intelligible and convey the essence of what they had to say, independently even of these references to notes that were part of **another moment** of reflection. Here again, this led me to a number of "repetitions" that were not repetitions at all, in other words, to revisit in a new light what I had noted down from day to day almost a year ago, in the fresh emotion of discovery. At the time, I was overwhelmed by so many unexpected and sometimes unbelievable facts, that there was no question of a real "investigation", even remotely methodical. At the time, I was content to try my best to take in what was tumbling down on me, and to "fit it in" as best I could, without going into too much detail. Most of my energy was then absorbed in **dealing** with the **crazy**, the unbelievable (as in the tale of the Chinese Emperor's robe... .<sup>836</sup>(\*)), and above all, to take on this "breath" of violence, cynicism and contempt that suddenly came back to me, "underneath these good-natured airs... . ..." that I recognized all too well; the breath of other times, that I had lived through and that I have not forgotten...

The last three weeks, however, have been an opportunity to complete last year's stormy investigation, by delving a little more closely into certain texts (SGA 5 and, above all, the so-called "SGA 4<sup>1</sup> "). This gave rise to a series (which at times never seemed to end!) of (more or less) detailed footnotes, some of which became sub-notes, and one of the latter (with the intended name "The Formula") occupying me over four consecutive days and splitting into four others<sup>837</sup> (\*\*). . . At times it seemed as if I was never going to finish - and then no, it ended up converging<sup>838</sup> (\*\*\*). I'll leave aside for the moment a dozen or so pages that are decidedly too crossed-out and need to be redone, as well as the footnotes.

<sup>835</sup>(\*) The only other moments in the Harvest and Sowing reflection when I made such a (admittedly smaller) departure from the "spontaneous" mode of writing, was in the section "The note - or the new ethic" (n° 33) and in the note "Iniquity - or the meaning of a return" (n° 75).

<sup>836</sup>(\*) See note of the same name, n° 77'.

<sup>837</sup>(\*\*) (June 1) Which have since become six. . .

<sup>838</sup>(\*\*\*) (June 1) A very temporary "convergence" indeed, since the note "L'Apothéose" ended up splitting into some thirty separate notes, sub-notes etc., running to well over 150 pages on their own!



of the last two notes ("Le partage" and "L' Apothéose") which I'll add later. That's enough for now! I'll come back to "l'intendance" later, but I can't wait to finish, and say p. 1104

Without further ado, I'd like to say a few words about the "four operations".

I distinguish two closely related, yet distinct "aspects" or "levels" in Burial.

They are quite clearly separated (to my eyes at least) by a **threshold**.

On the one hand, there's the "wind of fashion" aspect (sometimes going as far as that "breath of derision" I've spoken about more than once in Récoltes et Semailles). It manifests itself above all in what I have elsewhere called<sup>839</sup> (\*) "attitudes of automatic rejection" - attitudes that often cut short the simple reflexes of mathematical common sense, and are exercised against certain people and their mathematical contributions. In this case, it's me, and a few others who are classified (sometimes despite the best efforts of the interested party to distance himself from me) as having "connections" with me. In my case, it was certainly not possible to "reject" (or "bury") everything I contributed, even though much of it had already entered the common domain of everyday use, even before I left the mathematical scene in 1970<sup>840</sup> (\*\*). It's true, however (and I made this point for the first time in the note "My orphans" of a year ago (note n° 46)) that by far the largest part of my written or unwritten work on cohomology was buried, first and foremost by my students, in the aftermath of my departure. (Some of the themes I had introduced were unearthed four, seven or twelve years later without any mention of me.

- but here we are already touching on the "second level". . . )

We can certainly deplore such automatic rejections, which sometimes run counter to simple delicacy and the respect due to others, and are in all cases foreign to common sense and mathematical discernment. It is all the more regrettable when it strikes young mathematicians of sometimes brilliant means,

when the "bite of disdain" extinguishes a joy and denatures what had been a beautiful passion, in the bitterness of investments that appear as wasted (according to the consensus that makes law. . . ). And we can regret it<sup>1105</sup> after p.

also, when this rejection strikes simple, fruitful ideas that have amply proved their worth, to bring out of the void powerful tools that nowadays "everyone" uses without looking twice. In the first case (that of a devastated vocation), the damage is likely to be irreversible, but not in the second - because sooner or later, the simple and essential ideas, those "on the way", will eventually appear or reappear, and become part of the common heritage. In any case, it's unreasonable to try and force anyone to think **well of** a person, or a work, or an idea, which (for whatever reason) they want to think **badly of**, or forget altogether. This kind of question is certainly, and in a delicate and essential way, a matter of personal "ethics", but we cannot, it seems to me, make it a question of collective "scientific ethics"; or if we were to try, it's to be feared that the cure would be worse than the disease... .

The second "aspect" or "level" I was referring to, however, is precisely where such a collective ethic is breached. The **threshold** I was talking about is a **consensus** which, as far as I know, has been universally accepted in all the sciences, ever since they have been the subject of written testimony. It is the consensus that no one is supposed to present as his or her own the ideas<sup>841</sup> (\*) that he or she has taken from

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<sup>839</sup>(\*) In the note "Le Fossoyeur - ou la Congrégation toute entière", n° 97.

<sup>840</sup>(\*\*) It is true, however, that even some of the ideas and techniques that had already come into "everyday" use (at the very least within the limited circle of my students and close collaborators) were buried as soon as I left. This was particularly true of the *l-adic* cohomological tool, which I had developed in great detail in SGA 5 (based on the key results of SGA 4). It was kept under wraps by my cohomology students, led by Deligne, only to be exhumed in the form and spirit I know in 1977.

<sup>841</sup>(\*) When I say "ideas", I'm obviously not talking about "results" alone in mathematics. Often, a simple, well-posed **question** that touches on a crucial point that no-one has seen before is more important than a "result",

□ **autrui**

. This consensus obliges us, therefore, to indicate the provenance of the ideas we present,

let's use or develop them, at least whenever these ideas are not of our own making or part of the common heritage, already known (not by three or four insiders, but) by "**everyone**".

I don't recall ever having heard this consensus challenged. From the time I was part of the mathematical community, between 1948 (when I was a twenty-year-old beginner attending Cartan's classes at the Ecole Normale Supérieure) and 1970 (when I left the mathematical scene), I only very rarely had the opportunity, and with only one colleague and friend who was a little negligent in this respect<sup>842</sup> (\*), to witness or even to be informed of a clear breach of this consensus, or principle. As I pointed out in the first part of *Récoltes et Semailles* (in the section "Un secret de Polichinelle bien gardé", n° 21), respect for this principle is by no means a matter of course for anyone with a modicum of honesty and self-respect. On the contrary, it requires a great deal of vigilance, since inveterate reflexes from childhood naturally lead us to overestimate our own merits, and to confuse the work of assimilating ideas from others with the actual conception of those ideas - something which is in no way the same thing. When I wrote the above section over a year ago, I wasn't really sure what I was talking about.

obviously not yet clear with myself about the importance to be attached to this consensus. There was then a certain vagueness in my mind (which I didn't clearly realize at □ **this** stage of the reflection), in relation to this widespread feeling that a strict demand **on others** (my own pupils, for example) to respect this principle in their relationship with me, was a sign of a lack of generosity, of a pettiness unworthy of me. So there was an **ambiguity** in me at the time, which I only clearly detected in the reflection of the note of June 1, of the same name (n° 63"). This reflection completely dispelled this ambiguity, which (I then realized) had weighed heavily on my relationship with my students, from the beginning (in the early sixties) until just last year. I understood that rigor in the practice of the profession of mathematician (or, more generally, of scientist), means first and foremost great vigilance with regard to oneself, in respect of that crucial consensus between all, but also an equal requirement with regard to others, and all the more so, with regard to those whom

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even arduous. This is still the case, even if the question has not yet been condensed into a precise **statement**, which would constitute an embryo of a hypothetical answer, or even a more or less complete (and still conjectural) answer. It goes without saying that generating such a statement from an initially vague question is an essential and creative part of mathematical work. Presenting an elaborated version of a (perhaps profound) question while concealing its origin (even though the elaboration is the work of the presenter-prestidigitator), just as concealing the origin of a statement in deep form, under the pretext of presenting a demonstration of it, is plagiarism just as much as presenting as one's own a demonstration taken from someone else.

The same applies to the introduction of fruitful **notions**, which are often even more crucial than good statements - because the question of "good statements" only arises once the right notions have already been identified. Here again, using the pretext of having modified or even improved a notion taken from someone else, in order to hide its origin, is just as dishonest as "borrowing" the notion *ne varietur*. More often than not, it's the first step - raising a question (however vague), proposing a statement or notion (however imperfect and provisional) - that's the crucial one, not the improvements (in precision, breadth, depth) you make to it. But even if this weren't the case, it can't be taken as a "reason" for someone to make an original work by improving what they've received, in order to hide what they've received (or, which may amount to the same thing, to "debunk" it. . . ).

As I have already pointed out elsewhere (in sub-note n° 106<sub>1</sub> of the note "Muscle and gut (yang buries yin (1))", n° 106), the "value" of a conjectural statement depends neither on its presumed difficulty, nor on its more or less "plausible" character, nor whether the statement turns out to be true or false. In any case, the "value" one is willing to attribute to a mathematical idea (whether expressed in a question, a statement, a notion or a demonstration), or to a set of ideas, is to a large extent subjective and can hardly be the subject of a consensus of scientific ethics. This is why an honest scientific will indicate the origin of **all the** ideas it uses (explicitly or tacitly) and which are not part of the "well known", without indulging in the inclination to keep quiet about the origin of an idea which it has decided in its heart of hearts (and perhaps for the needs of a dubious cause . . . ) was in any case "obvious", "trivial", "unimportant" (or other qualificatives of the same water).

<sup>842</sup>(\*) This colleague's case is discussed in passing, in the first part of *R & S*, in the section quoted in the very next sentence.

With the benefit of over a year's hindsight, this "case" takes on a weight I hadn't given it before.

our job is to introduce them to our profession.

With each passing year, I've come to understand better just how much **more** there is to this profession than just a certain technical know-how, or even the ability to use one's imagination to solve problemly difficult problems. In a way, I knew this all along - but I underestimated the "ethical" aspect, or **collective**<sup>843</sup> (\*), as something that was supposed to be "taken for granted" between people of good faith and good company. In this way, I was ready for the "ambiguity" of which I spoke, and which was also (under cover of a false "generosity") an **indulgence** towards my students and assimilates, and in an even more hidden way, an indulgence towards **myself**.

I left this milieu of "people of good faith and good company", which had also been **my** world, with which I had been happy to identify. When I took a closer look (in the weeks following last year's April 19th), less than fifteen years after leaving it, I found a **corruption** I could never have imagined, even in a dream.

□ It's a mystery to me what **meaning** it can still have to "do math" as a member of This world - if not only as a means to **power**, or (for those of modest status) to secure a **livelihood** under material conditions that are, well, comfortable (when you're lucky enough to already be "settled" as best you can... ).

p. 1108

## 18.5.6. (5) The family album

**Note** 173<sup>844</sup> (\*)

**a. Un défunt bien entouré** (March 22) To put it more bluntly, there's a "fashion" level to funerals, and a "swindle" level. Perhaps I'm just being tardy, and that what was considered a swindle "in my day" has now become a perfectly acceptable and honorable thing, as long as those who practice it are part of the "beau monde". Perhaps the "threshold" has long since disappeared?

The "second level" consists of **a single, vast swindle**, targeting my entire work on the cohomological theme, and after it, that of Zoghman Mebkhout, the imprudent continuator, posthumous, obscure and obstinate pupil of the buried master. The great conductor of the operation was another pupil, by no means posthumous but on the contrary occult, that's right, playing on a tacit role of "heir" to my work, while disavowing and debunking both the work and the worker. This is my friend **Pierre Deligne**. His zealous lieutenants were none other than the four students who, with him, had opted for the "cohomology" option:

**J.L. Verdier, L. Illusie, P. Berthelot, J.P. Jouanolou.** The deceased is certainly well surrounded, both by the

<sup>843</sup>(\*) I don't mean to imply that the "ethical" aspect of a situation is always, at the same time, a "collective" aspect, touching on the relationship of a person to a group (in this case, a group of "colleagues" or "congeners"). This is certainly the case with the "consensus" I'm examining.

In keeping with the particular conditioning that has shaped my view of things since childhood, until last year I tended to underestimate (or even ignore) what was collective, in favor of what was personal. The "collective adventure" aspect of my personal "mathematical adventure" became clear to me last year, first in the "Galois legacy" section (n° 7), but especially in the sections at the end of the first part of R et S, "L'aventure solitaire" and "Le poids d'un passé" (n° s 47, 50).

<sup>844</sup>(\*) This note, "The Family Album", was originally written as an immediate follow-up to the previous note, "The Threshold", written on the same day.

day (March 22). This part now forms part a- ("A deceased well surrounded"), to which two other parts were added on June 10 and 11, b. ("New heads - or vocalizations") and c. ("The one among all - or acquiescence"). The following note "L'escalade (2)" (n° 174), again dated March 22, follows directly on from part a. (of the same day) of the present note. The b. de p. notes to parts b. and c. are dated June 13 and 14. Finally, a last part d. ("The last minute - or fi n d'un tabou") was added on June 18.

co-deceased<sup>845</sup> (\*\*) sharing the honors of the funeral with him, than by those who, during his "lifetime", were close to him. As auxiliary undertakers, coming to lend a hand in the double Burial, staged by the Great Chief, I see seven other "world-renowned" mathematicians (to quote the

p. 1109 □ terms from a certain advertising placard<sup>846</sup> (\*)), appearing episodically during the Funeral Ceremony reviewed in the family album (also known as "The Four Operations"). They are (in order of importance in the Ceremony) **B. Teissier, A.A Beilinson, J. Bernstein, J.S. Milne, A. Ogus, K.Y. Shih, N. Katz.**

I've now listed all the mathematicians I know to have played **an active part** in Operation Burial in one capacity or another. There are twelve<sup>847</sup> (\*\*). For the last four named, I cannot prejudge their bad faith, based on the facts known to me. However, I consider that their responsibility is just as much engaged as that of the others. For if they were unaware of what they were doing, this was a choice, which in no way relieves them of responsibility for their actions.

As for the participants in direct collusion, I'm certainly incapable of drawing up even an incomplete list, or of estimating their numbers, which are surely of an altogether different magnitude. Suffice it to say that these include all those who took part in the "memorable Colloque" at Luminy in June 1981 (known as the Colloque Pervers), as well as all those, among the readers of the volume entitled "SGA 4<sup>1</sup>", who 2 were even remotely aware of the meaning of the acronym SGA - and who "let it run".

I see two written texts that bear witness to a **disgrace** in the mathematics of the seventies and eighties, the like of which has probably never been seen in the history of our science. In one of these texts, the disgrace bursts forth in the name it has already given itself, which is in itself an im- posture (of genius. . . ): the text named "SGA 4<sup>1</sup>" (as a current reference acronym), and also "Cohomology".

p. 1110 Etale" - by P. Deligne, with the "collaboration" (among others and in addition to L. Illusie and J.L. Verdier) of A. Grothendieck<sup>848</sup> (\*\*\*). The second text is the Proceedings of the □ Colloque de Luminy of June 1981, especially the first volume, consisting of the Introduction to the Colloquium (by B. Teissier and J.L. Verdier) and the main Colloquium article (by A.A. Beilinson, J. Bernstein, P. Deligne).

It would surely be to everyone's benefit, and to the credit of the generation of mathematicians who have tolerated such disgraces, if at least one of those who have directly contributed to them, in one capacity or another, could find it in himself to make a public apology - or better still, to explain publicly what has happened, as far as **he is concerned**. But that's probably too much to hope for.

It's also too much to hope that J.L. Verdier will cease to occupy Henri Cartan's position at the Ecole Normale Supérieure. This is surely the key position in France for training the next generation of mathematicians. When I learned, a long time ago, that Verdier had been promoted to this position, he who had been one of my students and for whom I was very fond, I felt honored (and at the same time, secretly flattered). There was not the slightest doubt in my mind, then, that Verdier would fulfill Cartan's role perfectly, with regard to the most mathematically-motivated young people, who would learn their trade perfectly from him. If I see today (and since

<sup>845</sup>(\*\*) In fact, there are not one, but **four** "co-defuncts" of which I am aware, which are the subject of the four coffin notes (coffins 1 to 4) n° s 93-96.

<sup>846</sup>(\*) This is the IHES jubilee brochure published in 1983 to mark the twenty-fifth anniversary of its foundation. See

For more information, please refer to the Eloges Funèbre notes (1) (2) (n° s 104, 105), particularly page 454.

<sup>847</sup>(\*\*) The same "twelve" as in the section (of the first part of R et s) "Jesus and the twelve apostles", reviewing all the students who have worked with me up to the level of a state doctorate thesis. It's true that among the active participants in my Funeral, but this time on the company side of Springer Funeral Services GmbH (instead of the Congregation of the Faithful), there is still Dr. K.F. Springer (co-director of the esteemed establishment) and Drs. Peters and M. Byrne, who will be mentioned

in a later note (n° 175). And that's fifteen!

<sup>848</sup>(\*\*\*) On the meaning of this "collaboration", which forms part of the mystification created by Deligne, see the note "Le renverse- ment" (n° 68').

years ago, but never before with such brutal evidence) that I was mistaken, and if I say so clearly here, it's not to opprobriate him or anyone else. I believe he has disqualified himself from directing research. In saying this, I'm not denying my share of responsibility, for having badly taught (to him as to all my other students) this profession that I loved, and that I continue to love.

**b. New heads - or vocalizations** (June 10) Two and a half months have passed since I wrote the beginning of this note "The family album". Certainly, I had no idea that I'd have to come back to it again, following new twists and turns in the Burial investigation. Above all, it was the break-up of the modest five- or ten-page "apotheosis" I'd just written, into a grandiose one-hundred-and-fifty-page capitalized Apotheosis, which immediately introduced me to "new faces", who must have their place in the family album. There were also some familiar faces, including

that they too are part of the legion of those who actively participated, at the "swindle" level, in "Operation Burial". I'm reviewing them here "for the record", and to make sure that each of the p. 1111

interested parties feel in good company (but that's probably been the case for a long time now. . . ). I'm inserting the new photos in the order in which they came to my attention.

First of all, on the Springer Verlag GmbH side, there are **K.F. Springer** (one of the company's co-publishers), **K. Peters**, and Mrs. **C.M. Byrne**. I give more details in the note below "Les Pompes Funèbres - im Dienste der Wissenschaft" (n° 175). At the time of writing the beginning of this note, on March 22, I had just received K.F. Springer's letter (dated March 15), which dispelled my last doubts about the spirit reigning in the esteemed Funeral Home, faithful to its motto "In the service of Science".

On the Apotheosis side (via the burial of the service unknown), I'm aware of contributions from **M. Kashiwara**, **R. Hotta**, **J.L. Brylinski**, **B. Malgrange**, **G. Laumon**, and **R. Remmert**, not to mention an **anonymous referee** whose bad faith can't be doubted; but it's true that if we start adding up the complacent referees of articles or shady books, closely or remotely linked to the Burial, we'd surely need a new album. Also, my old friend N. Katz has reappeared, this time in such a context that the presumption of good faith (relative, at least) that I had with regard to him has vanished. This brings to fourteen (and fifteen, counting the famous anonymous referent) the number of mathematicians, all of international repute, who are known to me to have participated actively in one capacity or another in the "Colloque Pervers" scam. For duly documented details on this subject, I refer to the Apotheosis, and more particularly to the notes ". . . et l'aubaine", "Le jour de gloire", "La maffia", "Carte blanche pour le pillage - ou les Hautes Oeuvres" (n° s 171 (iii) (iv), 171<sub>2</sub>, 171 ).<sup>4</sup>

Finally, on the side of the "Motifs" operation, another of my former students appeared (better late than never), a little away from the main pack. Afterwards, I was almost forced to count him (as a sixth grader) among my "cohomology" students, even though "in my day" he hadn't the slightest idea what cohomology was. We're talking about Neantro Saavedra Rivano, who, obviously, was used (of his own free will, of course) as a "pawn" in the hands of others, rather than acting on his own behalf. his own account. His adventures, battling with Monsieur Verdoux (disguised as a "cavalier□servant"), were p. 1112 reconstructed page by page in the suite of notes "Le sixième clou (au cercueil)" (n° s 176<sub>1</sub> à 176<sub>7</sub> ), dated April 19th and 20th (except for the last one, which has yet to be written). This brings to six (out of twelve) the number of my "former" students who took an active part in the master's funeral. Saavedra's part in this burial stands out in that the "Tannakian Categories (sic)" operation, of which he was an active participant, is the first large-scale operation aimed at concealing the authorship of a part of the "Tannakian Categories".

of my work and the philosophy I'd developed (in the wake of, and on the occasion of, that of motifs, in this case).

Taking into account the new arrivals in the album, and putting aside the Springer-Pompes- Funèbres contribution, to retain only those from the Congrégation des Fidèles, this brings to nineteen<sup>849</sup> (\*) the number of notorious mathematicians known to me to have actively participated in the Burial, at the level of what in my day was called a swindle operation. Among these participants, there are only three, namely the three co-signatories with P. Deligne of the "memorable volume" Lecture Notes 900, whose bad faith I do not take for granted.

This list is by no means exhaustive of all my colleagues and/or former students or friends, who in one capacity or another and in a more or less active way took part in my funeral, without going so far as to associate themselves with a blatant swindle. I've listed around thirty of them, most of whom I've already mentioned in the course of my reflections on L'Enterrement; counting the previous ones, that makes about fifty - and these are only those of which I've been aware, as if in spite of myself, even in my distant retirement, over the last eight or nine years, or those who have come to my attention in the course of an investigation which, by deliberate design, has remained very limited.

These figures alone speak volumes, and give unexpected support to the impression I had already gained last year, namely that the burial of my work and my modest self is not the enterprise of a single person, nor of a strictly limited group (such as my students before I left),

or that of my "cohomology students"), but a collective undertaking, at the level of "the whole Congregation"; or at least, at the level of the part The mathematical establishment that had been □witness

and an integral part of the growth and development of my work as a geometer between 1955 and 1970. My departure in 1970 was the signal, in this part of mathematics at least, for **an** immediate and draconian **rejection** of "Grothendieckian" mathematics, seen as the symbol and embodiment of "feminine mathematics"<sup>850</sup> (\*): where vision constantly precedes and inspires the technical aspect, where difficulties are constantly resolved rather than cut and dried, where constant contact with the profound unity in the apparent disparity of things, enables us at every moment to detect what is essential in the amorphous mass of the accidental and the accessory. At the same time, my departure also signalled a spectacular halt to all conceptual work, or to put it another way, the **outlawing** of all such work, suddenly derided under the pretext of "deepening".

Thus, by mutilating one of the essential "sides" of mathematical creation, the "yin" or "feminine" side, the result was an astonishing "Verflachung", a "flattening", a "drying out" of the mathematical work<sup>851</sup> (\*\*). The thing was done (it seemed to me) by a brutal and draconian turn, practically overnight. It's such a strange thing, so unheard of, that it seems unbelievable. It took me over a year of intensive reflection on L'Enterrement to finally grasp what had happened and come to terms with it. I don't know if there has been a comparable turning point in recent years or decades, or at any other time, in any branch of science or any other human activity.

<sup>849</sup>(\*) Twenty, including the famous anonymous referrer.

<sup>850</sup>(\*) On the subject of these reactions of rejection towards a certain style of approach to mathematics, see the notes "Le muscle et la tripe (yang enterre yin (1))", "Les obsèques du yin (yang enterre yin (4))", "La circonstance providentielle - ou l'Apothéose", "Le désaveu (1) - ou le rappel", "Le désaveu (2) - ou la métamorphose" (n° s 106, 124, 151, 152, 153). I'm trying to identify certain features of "feminine mathematics", alongside complementary "masculine" features, in the notes "La rising sea. ...", "The nine months and the five minutes", "The arrow and the wave", "Brother and husband - or the double signature", "Yin the Servant, and the new masters", "Yin the Servant - or generosity" (n° s 122, 123, 130, 134, 135, 136).

<sup>851</sup>(\*\*) For the beginnings of an observation about this "flattening", see the note "Useless details" part (c), "Things that look like nothing - or dry out" (note n° 171 (v)).

involving (among other forces) our creative abilities.

But let me come back to my album. I thought it would be useful to include here the names of those, apart from those already mentioned, whose participation in the Burial I have no doubt about. I'm not convinced either

that any of them would wish me harm, and there is more than one among them, surely, who feels □ even p. 1114

I'm sure there's not one of them who won't be genuinely surprised to hear of a "funeral" of my person and my work. There may not be a single one of them who will not be genuinely surprised to hear of a "Funeral" of me and my work, and even more, to learn that he is supposed to have participated in it in some way. The fact that he is mentioned by name here will already have the (welcome for me) effect of informing him about this, and (if he is himself interested) thus giving the opportunity for an explanation between us. I am, of course, entirely at the disposal of interested parties, to provide any clarification they may require on the subject of what I have perceived (rightly or wrongly) as a participation in my funeral, either directly or through "co-burials". There's no question of my questioning the good faith and honesty of my colleagues.

professional of any of them<sup>852</sup> (\*), and for more than one I can even add that their complete good faith and honesty are for me above suspicion. □ Rather than stupidly listing them in order p. 1115

alphabetical order (something a computer would do better than me), I prefer to list the names of the faithful, chorusing at my Funeral, in approximate chronological order; not according to the times of their appearance at the Funeral Ceremony (which are mostly unknown to me), but according to the times when I clearly became aware of their participation. On the other hand, I'll set aside all my students<sup>853</sup> (\*). With the sole exception of Mme Hoang Xuan Sinh, who works in Vietnam and is decidedly a little far away to lend a hand at my Funeral, there is not a single one of my students who, in one way or another, did not take part. I've already explained this in the note "Silence" (n° 84) and at the beginning of the note "Coffin 1 - or the grateful D-Modules" (n° 93), and this is not the place to return to it. This is

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<sup>852</sup>(\*) (June 16) Further to new information that has just reached me, this presumption of good faith is no longer valid in the case of A. Borel. According to correspondence between him and Z. Mebkhout last year, on the occasion of a seminar on the theory of  $D$ -Modules directed by Borel in Zurich, I already knew that Mebkhout had informed him of the fact that he was the author of the central category equivalence in the theory (known as "Riemann-Hilbert"), giving precise references and sending him all his works, where Borel could easily convince himself of the reality of the facts. But this didn't stop Borel from treating him with the condescension (and even discourtesy) he was accustomed to. In a Colloquium just held in Oberwolfach on the same theme (Algebraic theory of Systems of partial differential equations, Oberwolfach June 9-15 1985), where Borel gave the first three introductory talks (under the title "Algebraic theory of  $D$ -Modules"), preparing the ground for the "God's theorem", **Mebkhout's name was not mentioned** in any of these talks, or indeed in any of the following ones (except for a single "thumb-reference" in passing, in Brylinski's talk). According to Mebkhout's account, this Colloquium, in which Borel played the role of conductor (in place of Deligne, who wasn't at the party), was a veritable **re-run of the Colloque Pervers** that had taken place four years earlier. Virtually the entire "maffi a" was there: Verdier, Brylinski, Laumon, Malgrange and even (this time) Kashiwara (who had already played a leading role in the Zurich seminar, notwithstanding the detailed information Mebkhout had given Borel about the character). Needless to say, (no more than at the Zurich seminar) it was not deemed useful to ask Mebkhout to give a talk, and (apart from occasional interventions by the same Mebkhout, falling into freezing cold) the ancestor's name was not uttered (apart from his presence in the unfortunate "Grothendieck group"). The theory of biduality still goes by the name of "Verdier duality", including in Borel's presentations. Mebkhout emphatically reminded him last year that this biduality had been copied from SGA 5's Expository I - but apparently Borel has developed an allergy to a certain style and to a certain absentee, an allergy that forbids him to take such references into account. ... In fact, he was party to the same swindle in his book "Intersection Homology" (Birkhauser Verlag, 1984), published **after** Mebkhout had pointed out Verdier's deception to him.

I had maintained a presumption of good faith towards Borel to the limit of what was possible, having known him well in the years fifty, when we were both members of the Bourbaki group and worked together there. He is the first among the members of what I truly consider to be "my original milieu" in the mathematical world, whose direct participation, at the level of "swindling", in the Burial I have to acknowledge today without any possibility of doubt.

<sup>853</sup>(\*) When I speak here of "my students", I mean those who have worked with me at doctoral thesis level and who (with the exception of Deligne) have done a doctoral thesis with me. There are fourteen of them (including two "after my departure"), reviewed in the note "Jesus and the Twelve Apostles" (n° 19).

in the case of each of my students that an in-depth explanation of what happened seems to me the most desirable.

The "choruses à mes Obsèques" are set in the most diverse diapasons. I've picked out four main ones, which make for a first-class polyphonic funeral in grand style! There's the "discreet and effective" **boycott** of any attempt to develop grothendieckian-scented mathematics. There's the

**discourtesy** and lack of delicacy, the like of which I had not encountered in the mathematical world before my departure; in one or two extreme cases they take the form of thinly veiled **derision**. There's the

**deliberately ignoring** or minimizing the influence of my ideas and points of view in his personal work, or in any part of contemporary mathematics, in cases where this influence is nevertheless obvious and crucial, or attributing to a third party results or ideas that are due to me without any possibility of doubt. Finally, there is **the attitude** (known as "**ostriching**") of those who find themselves unfortunate enough to be confronted with a blatant scam, to bury their heads in the sand and pretend they haven't seen or felt anything.

Needless to say, more than one member of the faithful choir is vocalizing on several tuning forks at once.

All that said, here at last is the promised list<sup>854</sup> (\*) to complete our family album: B. Eckmann, A. Dold, N. A. Campo, B. Mazur, V. Poenaru, D.B.A. Epstein, P. Cartier, D. Quillen, N. Kuiper, R.D. Mac Pherson, H. Hironaka, F. Hirzebruch, J. Tits, S.S. Chern, M. Artin, R.P. Langlands, G.C. Rota, C. Goulaouic, W. Fulton, A. Borel, J. Tate, J.P. Serre.

### c. The one of all - or acquiescence

(June 11) I felt a little silly last night, typing this list of

names, when each of the names lined up there stupidly evoked, in itself, a whole rich cloud.

associations, none of which are apparent here. But I can't dwell here on each of these names and what they evoke - that would require another volume, and I can't wait to finish with this one! I

I apologize to those concerned for "sticking" them, rather cavalierly, in a not very inspiring "table" of presence (at my Funeral). It's true that most of them have already been mentioned in one capacity or another here or there during Harvest and Sowing, even if not necessarily as participants in my Funeral. Four of them are friends of mine from the Bourbaki group, with whom I had close ties, through work and (for two of them) friendship, thirty years ago and more. There are nine more, in this lapidary list, to whom I felt bound by feelings of warm friendship, and who have not died out even as I write these lines. But more than once, in the course of the years that have passed, finding myself confronted with one of these friends of yesteryear, or with one of those who were my pupils, I have been seized by the strange impression that the one to whom I still felt this surge of sympathy, which I found within myself intact, was no longer there - or at least, that contact with that one had been lost, perhaps irretrievably; that another had taken the place of the one I had known, filled with intense, quivering life, and seemed to have

<sup>854</sup>(\*) I have not included in this list the names of the eight "non-cohomology" students, which can be found in the note (n° 19) already quoted, together with the names of the cohomology students already reviewed above.

It would be fair to include in my "Family Album" the names of those of my colleagues and former friends who are known to me as "non-entrants", through unequivocal expressions of sympathy and esteem. First of all, in connection with my work "A la Poursuite des Champs" ("In Pursuit of the Fields") pursued in 1983 (work to which I intend to return), I received warm encouragement from J. Benabou, N.J. Baues, A. Joyal, and above all from Ronnie Brown and Tim Porter, who (in more ways than one) gave me efficient help throughout the duration of my work.

It's true that these colleagues belong to a rather different milieu from the one with which I used to identify myself, which is also the milieu in which my magisterial Funeral was naturally placed. As mathematicians who belong to or are close to this milieu, and from whom I have recently (over the past year or two) received testimonials along the same lines, it is a pleasure for me to name here B. Lawvere, J. Murre, D. Mumford, I.M. Gelfand and (last not least!) J.P. Serre. It is this last nominee who has the unique distinction of appearing on both "lists" at once - those of the "buriers", and that of the faithful friends!



erased all traces. It was like a **drying out**, a desiccation that had taken place, and a hard, watertight shell had appeared, where there had been sensitive, living flesh. ...

Before closing this family album, which I've only just opened, I'd like to focus a little on just one of those I've just inserted, in the blink of an eye. It's the one that comes last in this album. Even more than for any of the others I ended up including, there was serious resistance within me (unconscious, as it should have been) to parting with certain preconceived and long-standing images of our relationship, and surrendering to the humble evidence. This is Jean-Pierre Serre.

More than once in the course of Harvesting and Sowing, I've had the opportunity to talk about Serre, most often by name<sup>855</sup> (\*). The little I've said about him here and there will already have been enough, I think, to make it clear that he played a role in my mathematical past that belongs to no one else. This is something I'm not

I'd never stopped until I wrote *Récoltes et Semailles*, and which I discovered as the pages went by. p. 1118  
For twenty years, from the early fifties until my departure from the mathematical scene, he played the role of "privileged interlocutor" for me<sup>856</sup> (\*), and most of my major ideas and investments were directly stimulated by Serre's (sometimes "innocuous") ideas. At times, especially (I think) in the second half of the fifties and perhaps again in the early sixties, there was a kind of intense mathematical "symbiosis" between him and me, who were of complementary mathematical temperaments<sup>857</sup> (\*\*) - a symbiosis that proved to be very fruitful every time. The relationship between Serre and me was not of a "symmetrical" nature; for example, Serre was by no means inclined, as I am, to rely on one or more "privileged interlocutors" to keep him abreast of what might interest him or what he felt he needed. This does not prevent me (or so I presume) from having played an equally exceptional role in his mathematical past, and I can imagine that my unexpected departure in 1970 was a breaking point in his mathematical life (from a certain equilibrium, perhaps, where I represented the "yin" pole), a sudden turning point, through a kind of "void" that suddenly appeared. I don't know...

Still, Serre's close relationship with me and my work was certainly perceived in the mathematical world, even if it remained in the realm of the unspoken. Surely, apart from Deligne, Serre was perceived, with good reason, as the mathematician "closest" to my work. Deligne's relationship to my work and to me was very different - it was one of pupil and "heir". Deligne was nourished by my thought and my written and unwritten work, whereas none of my major ideas or investments were inspired or stimulated by him. He was "closer" to me than Serre, in the sense that during the years he spent with me (1965-69), there were no reactions of rejection in him.

with regard to certain aspects of my work and my approach to mathematics, as was the case with Serre; this is what enabled him, in the space of barely three or four years (given his exceptional means, and p. 1119 exceptionally favorable circumstances too), to intimately assimilate in its entirety the vast unifying vision that had been born and developed in me over the preceding years. But his relationship with me

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<sup>855</sup>(\*) I refrained from naming Serre two or three times, in *Fatuité et Renouvellement*; at a time when it didn't seem useful, more often than not, to refer by name to people about whom I was expressing some criticism. The passages in *Récoltes et Semailles* where I express myself most fully about Serre and the relationship between him and me are in the notes "Les neuf mois et les cinq minutes", "Frères et époux - ou la double signature", and "Les unnecessary details" (notes n°s 123, 134, 171 (v)).

<sup>856</sup>(\*) Between 1965 and 1969, while the relationship between Serre and me remained close, it was Deligne who took on the role of privileged interlocutor. The reason for this surely lies in our very strong affinities of temperament, and above all, in Deligne's openness (towards what I felt was the essence of what I had to contribute), which was often lacking in Serre. I'll come back below to the very different nature of the two relationships, which were the two closest in my past as a mathematician. See also the note quoted in the following b. de p. note.

<sup>857</sup>(\*\*) On this complementarity, and on the affinity between Deligne and myself, see the note already quoted "Brothers and spouses - or the double signature" (n° 134).

was profoundly ambiguous - and he systematically played on this tacit relationship of pupil and heir, which represented for him the means to **power**, while denying it and working to bury both the master and his vision... .

There was no such ambiguity in the relationship between Serre and me - at no time was there the slightest desire on either side to take "power" over the other, or to use this relationship for the purposes of power. I think I can even say that such power games did not exist in the "Bourbaki milieu" that welcomed me at the end of the 1940s, and I don't think I was a witness to, let alone a co-actor (even in spite of myself) in any such games, right up to the time of my departure in 1970<sup>858</sup> (\*). Another way of saying the same thing, concerning the relationship between

Serre and I (or the relationships I observed within the Bourbaki milieu): at no time did I detect the slightest element of antagonism<sup>859</sup> (\*\*), on either side. There were occasional<sup>□</sup>nelles frictions,

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I'm sure I've already mentioned them, and perhaps I'll have to come back to them, but that's another matter entirely. The relationship between Serre and me drew its strength, it seems to me, from our shared passion for a common master, mathematics, without any "parasitic" component of an egotistical nature, where the other would appear as a means, as an instrument, or as a target. This is undoubtedly why, when I recently resumed a correspondence with Serre that had been interrupted for ten or twelve years, I rediscovered, between the lines of the two or three letters I received from him, the signs of an intact friendship and delicacy, as if we had just parted the day before.

In fact, even though the opportunity to write to each other had not arisen for over ten years, the echoes that reached me from Serre, far and wide, all pointed in the same direction of an unchanged friendship - and by no means in funeral tones, as was the case for many of my friends of yesteryear. That's why, until just a few weeks ago, it never occurred to me that Serre would have played a part in my funeral. Everything I knew about him seemed to point in the opposite direction. It's certain, moreover, that his mere presence on the mathematical scene set certain limits to the Funeral (a most modest limit, I must admit. . . ). Leafing through J.S. Milne's book

"Etale Cohomology"<sup>860</sup> (\*), published in 1980, so **after** the incredible "SGA 4 operation<sup>1</sup> - SGA 5", I was

<sup>858</sup>(\*) I should, however, make a reservation, taking into account a certain game that has been played, entirely without my knowledge, among some of my students around my person and my work. This game began at least as early as 1966 (the year the SGA 5 seminar ended), with Deligne's 1968 article on the degeneration of suites

(On this subject, see the note "L'éviction", n° 63). I only began to learn about these games, which are indeed power games, last year, almost twenty years later. It's true that the active players were not members of the

I was integrated into the initial environment that had welcomed me (an environment in which I still can't discern such games, even with the hindsight afforded by greater maturity). They formed "the next generation". It's also true that the qualitative deterioration I'm seeing in this new generation, compared to the mother environment, is surely closely linked to a similar deterioration that has taken place in the past.

made in each and every member (or very few) of this initial milieu, of exceptional quality. On this subject, see the two sections "Bourbaki, ou ma grande chance - et son revers", and "De Profundis" (n° s 22, 23).

<sup>859</sup>(\*\*) I should, however, make an exception here for the episode *Survivre et Vivre*, in the early seventies. This episode had made it abundantly clear that my own ethical and ideological options, on many points that were important to me (and still are today), were the antithesis of those of almost all my friends in the mathematical establishment, including Serre. This suddenly put an end to my feelings of identification with this "establishment", which I had tended to confuse with an ideal (and idyllic) "mathematical community". (See

For more on this subject, see "The "Mathematical Community": fiction and reality", n° 10.) This unexpected revelation, and the resulting "change of camp" in the space of just a few months, then led me to adopt antagonistic attitudes towards some of my former friends, whom I was now inclined to classify as "reactionaries", and so on. I've since moved on from these peremptory and superficial classifications. Still, in an unsurprising turn of events, Serre has become one of those whom, for a time, I perceived as "adversaries", if not "haters". I was pleased to note that this episode left no trace of resentment or enmity in him - nor in me either, need I add!

<sup>860</sup>(\*) Published by Princeton University Press, Princeton, New Jersey. This is the same J.S. Milne who, two years later, participated in the "memorable volume" *Lecture Notes 900 scam* (discussed in the note ". . . and exhumation", n° 168 (iii)).

It was striking to see Milne follow "with confidence", practically verbatim, the terms in which Serre had expressed himself in a certain Bourbaki seminar (February 1974, n° 446) concerning the authorship of cohomology.

The theory had been "developed by Grothendieck, ☐with the help of Mr. Artin"<sup>861</sup> (\*). It is visible in more ways than one, that Milne has only occasionally read in SGA 4 and SGA 5<sup>862</sup> (\*\*), and he follows both Serre (casually expressing himself on SGA 4 and SGA 5, in the same Bourbaki exposé) and Deligne (shamelessly debunking these same seminars, in the volume he christened "SGA 4<sup>1</sup>" ) to present, in his introduction, Les textes originaux SGA 4 et SGA 5<sup>863</sup> (\*\*\*) as being difficult to access. This is precisely the situation that his book (following Deligne's three years earlier, a little thin on the ground) is intended to remedy; or, to put it plainly, to spare the user the useless and tedious work of reading the original texts. The opinion of the highest eminences (in this case, Serre first, followed by Deligne, with the deceased sitting mute in his padded coffin... ), an opinion that Milne, like everyone else, follows with his eyes closed (if not with eagerness, given the funeral context . . . ), peremptorily excludes the possibility that these texts present anything other than "useless details" (or even a "gangue of nonsense" . . . ), but rather the foundations of a new "general topology" version of topos (buried by unanimous agreement at the same time as the worker . . . ) - and that we'll be able to put into practice in the future. ) - and that, in the long run, we'll no more be able to do without this new topology, which (among other things) enabled the theory discussed in Milne's book to blossom, than we were able to do without ordinary general topology, which Milne, Deligne and Serre had the advantage (like myself) of learning on the school benches, and which they therefore meekly admit (as a matter of course) must have been worth the effort... .

p. 1121

☐I think it was last year that I first took a quick look at this presentation

p. 1122

Bourbaki de Serre, on which I recently commented, in the note "Les détails inutiles" (n° 171 (v)), part (a), "Des paquets de mille pages. . . ". The passage in which Serre ironizes the 1,583 pages of SGA 4 had then held so little of my attention, that I had even forgotten about it entirely, when I took this same exposé in my hands again, a month or two ago, on the occasion of writing Les Quatre Opérations. It has to be said that Serre's attitude of distancing himself from my famous "thousand-page packets" had been known to me for a long time, long before the SGA 4 seminar series appeared, so it came as no surprise. The first time (I think) that such a reaction of "visceral rejection" was triggered in Serre, towards a certain style of approach to mathematics that is mine, was on the occasion of the theory of coherent duality, which I had developed in the second half of the fifties. These were potential "bundles of a thousand pages", at least, especially if we consider that there was a whole new cohomological algebra at stake, in the derived categories version; but potential or actual "bundle", which was

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Leafing through Milne's book, I got the impression that it was written in good faith, and without any deliberate intent to bury. Even though his perception of things is clearly limited to following in the footsteps of eminences Serre and Deligne, he nevertheless has the merit (and originality. . . ) of expressing himself courteously on the subject of the SGA 4, SGA 5 mother seminar.

<sup>861</sup>(\*) Two years earlier, at the 1978 International Mathematical Congress in Helsinki, in the speech given by N. Katz (still the same Katz) in honor of the new Fields laureate Pierre Deligne, the theory of étale cohomology is presented as "developed by M. Artin and A. Grothendieck, in the direction envisaged by Grothendieck" - which goes to show that alphabetical order sometimes works well. ... The fact that Milne chose to follow Serre, rather than Katz, in his version of things, seems to me to be one sign among others of his good faith.

<sup>862</sup>(\*\*) In particular, I was struck by the fact that Milne (and Mebkhout, who was an attentive reader of my works... ) had even noticed the existence in SGA 5 of an explicit Lefschetz formula for general cohomological correspondences on an algebraic curve, a formula brilliantly concealed by the two conjurers Deligne and Illusie - a work of art, to say the least! On this subject, see the two sub-notes "Les prestidigitateurs - ou la

formule envolée" and "Les félicitations - ou le nouveau style" (n° s 169<sub>8</sub>, 169 ).<sub>9</sub>

<sup>863</sup>(\*\*\*) As far as the published version of SGA 5 is concerned, which (thanks to the "care" of the publisher-sic Illusie) only represents a defined ruin of the original seminar, Milne apologizes for finding it "difficult to access", Le bon samaritan Illusie has done all he can to turn it (following the good pleasure of the good samaritan Deligne) into an indigestible collection of "technical digressions"... .

Clearly, Serre didn't want to hear about it any more than Weil did to see a cohomology group written in black and white, or to hear the words "topological vector space" uttered.

This time though<sup>864</sup> (\*), when I came back to this text by Serre from 1974, against the backdrop of a year-long reflection on a certain Burial (which, in 1974, had been "going well" for four years. . . ), this passage finally clicked. It worked in me, slowly, over the days and weeks. I realized that this attitude of Serre's, to which I had become accustomed and which, before my departure, "had no consequences", acted as a kind of **green light** for the burial that took place. The first thing that occurred to me in this sense, with the force of evidence, was that Serre's very words (but "with malice and impudence to boot"), were eagerly taken up by a Deligne (or better said, with a secret delight) barely three years later, as "background noise" for his memorable Manoeuvres.

I first expressed myself in this vein in the aforementioned note of May 4, and this reflection is ap-  
deepens in part (c) (of May 27) of this same note, "Des choses qui ressemblent à rien - ou le dessèchement". This, too, is the first inkling of a reflection on the relationship between Serre and me, □ à la lu-  
l'Enterrement<sup>865</sup> (\*). As I wrote these pages, there must already have been in me a diffuse perception of the crucial role played by Serre in l'Enterrement. In the two weeks since then, the work of integrating and assimilating a whole range of facts and impressions must have continued, and the forces of inertia opposing a direct and nuanced perception of things have, I believe, resorbed, without struggle or effort. The time seems ripe to bring this work to a conclusion, now trying as best I can to formulate what is perceived.

One might think that this old-fashioned tendency to distance oneself from certain aspects and parts of my work would have acted as a kind of unfortunate coincidence, which would, alas, have favoured an equally unfortunate Burial. But that's a superficial view, and doesn't get to the heart of the matter. To come straight to the heart of the matter, it has become clear to me, given Serre's unique relationship to me and my work, and given his exceptional ascendancy over mathematicians of his generation and those that followed, that **the Burial could not have taken place, if there had not been in him a secret acquiescence to my burial.**

In addition to the decidedly absent "deceased", there were **two main actors** in this Burial, whose acts and omissions followed on from one another and complemented one another, without the slightest friction or burrs it would seem (but there's no question, for me, of any connivance here, so much so that the two protagonists operated on different tuning forks): they are Pierre Deligne, and Jean-Pierre Serre.

The former has been discussed at length since the very beginning of this long reflection on the Funeral; he represents the "foreground of the picture" of the Funeral, as the Grand Officiant at the Funeral, as well as the occult heir and principal "beneficiary" of the operations he initiates (and this, even before the symbolic "death" of the deceased. . . ). Serre, who is mentioned here for the first time as a leading figure in the funeral ceremony, represents the "third plane of the picture", formed by "the Congregation of the Faithful".

□ Departing from last year, or rather, even before I discovered L'Enterrement sous ses  
the crudest and most aberrant forms (and under that name), I was well aware that those who were burying me with such eagerness, in a world where I had known no enemies, were above all my **friends of yesteryear**, some of whom had not ceased to count themselves (albeit with lip service. . . ) among those who had been my friends for so **long**.

<sup>864</sup>(\*) In fact, it was only the third time I had this text in my hands that it "clicked".

<sup>865</sup>(\*) In a previous b. de p. note (note (\*) page 1117) I also noted two other notes where I expressed myself about the relationship between Serre and me, but in a rather different light - the "pre-Burial" light.

number of my friends. Now, it's also clear to me that among those friends who were also (and above all) my students<sup>866</sup> (\*), the one who was truly the **pillar of** the Ceremony, as representative of the Congregation and guarantor of the acquiescence of all the Faithful, was also the one, among all, who at the level of our common passion, had been closest to me.

For me, the most striking sign of Serre's acquiescence is certainly not a certain quip, sent with the casualness I know so well - a quip that almost escaped my attention (even if it wasn't lost on everyone. . . ). For me, the sign, which is truly astonishingly obvious once I stop to think about it, lies **in the ignorance in which he was happy to keep himself**, at the

about this Burial that was taking place right under his nose, so to speak<sup>867</sup> (\*\*) - the burial of a <sup>work</sup>□to which he had been linked from its very origins, and more closely than anyone else in the world. And it'sp . 1125

It's a total mystery to me whether reading Récoltes et Semailles (supposing he does read this "package" of over a thousand pages, yet. . . ) will finally encourage him to use his nose (which has been working hard for fifteen years now. . . ), and the rest. But I'm well aware that for him, as much as for any other participant in my funeral, accepting my message and making use of his healthy faculties also means accepting to question himself, profoundly.

It seems to me that Serre's role at the head of the Congregation of the Faithful who came to attend and chorus at my funeral is both typical and exceptional. If it's exceptional, it's because of its extreme character - as the one closest to me, closer than any other member of the Congregation; and also because of its sta-<sup>868</sup> (\*). This eliminates from the deeper motivations the usual "parasitic" components of antagonism "by compensation"<sup>869</sup> (\*\*). As I pointed out earlier, I can only detect □la relationp . 1126

<sup>866</sup>(\*) In the course of my reflections on Harvest and Sowing, it became clearer and clearer just how much the very fact of having been someone's pupil (mine, in this case) marks a relationship and gives it a special quality, making it akin to a relationship with a father or mother.

<sup>867</sup>(\*\*) It's fair to say that in his Bourbaki lecture of 1974, in which he presented Deligne's demonstration of the last part of Weil's conjectures, Serre had his nose right in the Burial - without, however, having the innocence to take note of it. I thought I sensed his unease at being confronted with this seemingly aberrant situation: that ten years after my paper (also at the Bourbaki seminar) in which I outlined the demonstration of a  $l$ -adic cohomological formula for  $L$ -functions, the crucial "fixed point formula" (which I had admitted there) had still not been demonstrated in the literature.

Serre then chose **to evacuate** this discomfort with a mood swing, ironically referring to SGA 4's infamous "1583 pages" (which, by the way, didn't **even** provide the formula we needed). This was the easy way out, evading an unpleasant reality (x). He knew full well, however (but had perhaps been pleased to forget. . . ) that in the SGA 5 seminar, I had demonstrated at length a formula for fixed points that went far beyond that for the Frobenius correspondence - and he also knew that the editing of my lectures had already been dragging on for eight years in the hands\* of so-called volunteer "editors". Although he was happy to forget the theme of SGA 5 (" $L$ -functions and  $l$ -adic cohomology" - the title says it all) and its content, he knew me well enough, having seen me do maths for more than twenty years, to know that I was not in the habit of doing things by halves, quite the contrary (and I even did them so "not by halves" that he was often annoyed, if not excused. . . ). It might have helped him to refresh his memories of what had happened at the SGA 5 seminar, where he'd been often enough, at least, to know the broad outlines of what I was doing there and where I stood.

Clearly, he didn't want to see his memories refreshed, or to ask himself any questions. And this is just one of many cases where my friend preferred to close his eyes and plug his nose, rather than face up to a reality he couldn't accept without deeply questioning himself.

(x) (June 22) Since these lines were written, I've come to realize that this kind of "unpleasant reality" is now welcomed with alacrity, almost as a godsend! See parts d. and e. of "The family album".

<sup>868</sup>(\*) There's a third circumstance that gives Serre's role in L'Enterrement its exceptional, or "extreme", character. He is one of a group of "benevolent elders" who welcomed me when I first came into contact with the world of mathematicians.

(I write about this group, for the first time in my life, in "L'étranger bienvenu" (section n° 9), and then in the Introduction to Récoltes et Semailles (I 5, "une dette bienvenue")). This is perhaps the main reason, in addition to the links of friendship and sympathy between us, which meant that it took me more than a year to come to terms with the fact that Serre had played a crucial role in my mathematical burial.

<sup>869</sup>(\*\*) I've already alluded two or three times, here and there, to this (apparent) "causeless antagonism", notably in the note

de Serre to my person or my work, and it's clear to me that there's no trace of it at the level of the deep forces at work in his acquiescence. As far as I know, apart from the famous quip, this acquiescence was expressed in a purely passive way, by **omissions** only. But this tacit "green light" given to a Burial of vast dimensions, accompanied by operations so enormous at times that they seem to define both common sense and decency, now appears to me as the indispensable and crucial "counterpart", the "negative" as it were, of the intensely active participation and Deligne's interest in the same funeral<sup>870</sup> (\*).

p. 1127

□ It seems to me that I have keenly perceived the force that was at work in Serre. It is at a level than that of a personal antagonism, or that of the search for a "profit", in the usual sense of the word.

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April 3 (below) "Le messenger (2)" (n° 182). There is no doubt in my mind that such "archetypal" antagonism is at work in the vast majority of the participants at my funeral - perhaps in all of them, with the sole exception of Serre. This force seems to me to be distinct from that which expresses itself in the process of repression (or "burial") of "the disowned woman who lives within oneself". But these two forces are nonetheless intimately linked, and in Burial they appear in a kind of amalgam, where it is often difficult to dissociate them. Yet I believe I have identified in them **the two great forces** at work in L'Enterrement. But I'd be hard-pressed to say whether one is more important than the other, and if so, which. I'd tend to think that it's the first of the two that I've detected, namely, the force of repression of the feminine side of one's own being.

If Serre's case struck me as "typical" (as well as exceptional), it's undoubtedly because it's the latter of the two forces at play (the one I tend to see as primordial) that appears there in all its force, to the exclusion of any trace of the other (qualified here as "parasitic" - in the sense that it would obscure a clear apprehension of what I thought I perceived as **the essential**). I presume (provided that the work of integrating and assimilating the raw facts and perceptions continues) that the coming months will bring me a more nuanced understanding of the part to be played by each force, both in the Burial and in other conflict situations in which I am involved in one capacity or another.

<sup>870</sup>(\*) There's a rather remarkable **inversion** here in the distribution of roles between Serre and Deligne, in L'Enterrement: Serre's appears almost exclusively passive, Deligne's intensely active (even if this role of "playmaker" is constantly concealed, for the sake of the argument and in keeping with my friend Pierre's particular style). In fact, it is Serre's persona that is strongly "masculine" dominant, and Deligne's equally "yin" (or "feminine") dominant; and this (for both) as much at the level of egotic mechanisms, of the "self" and its conditioning (i.e. that of the "**boss**"), as at that of the drive for discovery, of that which is original and escapes (in its intimate nature) conditioning (the level of the "**child**"). Between the extreme opposing temperaments of Serre and Deligne, the two "pillars" of L'Enterrement, the deceased represents a sort of middle ground, with a strong "masculine" dominance on the "boss" side, and an equally strong "feminine" dominance on the "worker" (or "child") side. (This

The distribution of "basic tones" appears in the note "Brothers and spouses - or the double signature", n° 134.)

The forces and mechanisms of "reversal" between yin and yang roles were also **the** main topic of discussion, giving rise to the long meditation "The key to yin and yang" and remaining present in *fi ligree* throughout. It appears implicitly in the very first note of the Key, "Muscle and gut (yang buries yin (1))" (n° 106), and comes more or less to the forefront of attention in eleven of the later notes (notes n° s 124, 127, 132, 133, 138, 140, 145, 148, 151, 153, 154). Here, I've just unexpectedly come across a somewhat similar "reversal" situation, driven by the internal logic of the deep forces at work in Burial.

Lately, I've been struck by yet another seemingly paradoxical aspect of the "reversal" of yin and yang roles in this funeral rich in apparent paradoxes! This time, it's a question of the respective roles of the premature "deceased" on the one hand, and of all the participants in his or her funeral, on the other. At the level of collective unconscious intentions, this Burial of the deceased (who is supposed to confine himself to the complete passivity befitting his state) is that, above all else, of "feminine mathematics" - of a style and approach to mathematics with strongly "feminine" connotations; while the burying Congregation is supposed to embody "pure and hard" virile values, delivering the soft feminine deliquescence to the appropriate disdain. (See, for example, the notes "Les obsèques du yin (yang enterre yin (4))", and "La circonstance providentielle - ou l'Apothéose", n° s 124, 151.) And yet, the internal logic of the situation forces each of these "hard" participants to play a typically "yin" or "feminine" game: a game of "velvet paw", of halftones and silences, omissions, insinuations placed there under the surface of nothing, or constantly suggesting such and such a thing while pretending to say the opposite - the "thumb!" style, in short, in which my friend Pierre is a master among all, and which each of the buryers had to make their own, by necessity. (See, on the subject of this style, the note "Thumb!", and especially the notes "Velvet Paw - or smiles".

and "Le renversement (4) - ou le cirque conjugal", n° s 77, 137, 138.) On the other hand, it's the "deceased", the embodiment of plethoric feminine sluggishness, who emerges from his cosy coffin when least expected, and takes on a "macho" role. who was familiar to him, putting his cards on the table, sticking his indiscreet nose and impertinent verb, electric torch in hand, into the most exquisitely ambiguous penumbras, rudely calling everyone by their name and a cat a cat and a rascal a rascal - a real misfit, to say the least, and a ~~fiendish~~ hindrance to going round in circles in the hushed purr of a beautiful

of the term. The recent exchange of letters with him was revealing in this respect. I feel that in the fifteen years since I left, my friend has undergone a **transformation**<sup>871</sup> (\*). This transformation will in the sense of this □ "visceral reaction of rejection" towards certain dominant aspects in my approach p. 1128 of mathematics. These are aspects that were also present, but to a less pronounced degree, in Serre's own approach, in the most fruitful years of his mathematical past - years of intense openness and creativity, before a process of **repression** of these aspects of his creative personality, of the "child" in him, set in. These are the "yin" or "feminine" aspects and traits of creativity. The transformation I sensed in my friend, with startling force, was that of a state of harmonious cooperation of yin and yang creative forces, with a pronounced yang (or "masculine") "dominance", into a state of "zinc-stranded virile" imbalance, where "yin" or "feminine" qualities are ruthlessly extirpated.

In fact, as I already hinted two weeks ago (in the note quoted earlier), this is the culmination of an evolution whose first signs I detected as early as the 1950s, and which became more pronounced during the 1960s. Already then, there was a gradual upset in the balance, manifested in a **narrowing of** vision, and in the range of creative faculties allowed to come into play. Reactions of rejection towards certain major aspects of my approach to mathematics, and progressively, towards everything that really made up the life, depth and strength of my work - this rejection was simply the outward projection, the tangible manifestation at the level of his relationship to me, of a rejection of an entirely different scope, towards an essential side of his own being and his own creative faculties.

It's possible (as I suggested earlier) that as long as I was around, the relationship with me acted as a brake on this evolution in Serre, that it represented a kind of counterweight in his life, in the fifties and especially in the sixties, and thus a factor of relative equilibrium. If this is indeed the case, my sudden departure must have given free rein to this force of repression of feminine qualities - a kind of force that has become familiar to me, as one of the dominant egotistic forces that have also acted in my own life; with this remarkable difference, however, that in my

however, this force of repression was confined to my egotistical mechanisms and my relationships with others' □ **without** interfering with my love affair with lady mathematics, or (more generally) with my p. 1129 spontaneous adventure of discovery, whether mathematical or otherwise<sup>872</sup> (\*).

To return to the subject of burial, I can do no better than to quote the lines that conclude the reflection of November 10, in the note "The funeral of yin (yang buries yin (4))" (n° 124, page 564):

". . . And all of a sudden, these funerals appear to me in a new, unexpected light, in which my own person has become an accessory, a **symbol of** what must be "handed over to disdain". This is no longer the funeral of a person, nor of a work of art, nor even of a work of art.

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funeral ceremony...

<sup>871</sup>(\*) This expression "**transformation**" is immediately associated with the "**metamorphosis**" in my friend Pierre, which I clearly perceived for the first time during his visit to my home last October. (I say more about this in the note "Le désaveu (2) - ou la métamorphose", n° 153.) The term "metamorphosis" is stronger, and corresponds to the fact that my friend Pierre has undergone a reversal of an original temperament with a pronounced yin "dominant", into "macho" borrowed attitudes with a hint of yin. Apart from that, the transformation I felt in both friends was in the same direction, driven by the same force of repression of traits felt to be "feminine".

<sup>872</sup>(\*) I discuss the role of this repressive force in my own life in the note "Le Suprême (yang enterre yin (2))", n° 108. I began to detect this force in 1976, the year that marked a crucial turning point in my spiritual adventure. This turning point is discussed in the two notes "Reunion (the awakening of yin (1))" and "Acceptance (the awakening of yin (2))". du yin (2))", n° s 109, 110. I note the predominance of "feminine" traits in my mathematical work (where said traits seem to have taken refuge, safe from suspicion!) in the note "La mer qui monte. . . ", n° 122.

of inadmissible dissent, but the funeral of the "mathematical feminine" - and even more profoundly, perhaps, in each of the many attendees who came to applaud the Eulogy, **the funeral of the disowned woman who lives within himself.**"

This last intuition appeared that day in a sudden flash, at the very moment of writing these last two lines, coming as an unexpected revelation, in addition to the one that was the subject of the previous lines. This intuition remained a watermark in my thinking over the weeks that followed, to be finally taken up and deepened in the three consecutive notes from December 23 to 26: "La circonstance providentielle - ou l'Apothéose", "Le désaveu (1) - ou le rappel", and "Le désaveu (2) - ou la métamorphose".

Neither on the day this intuition first made its appearance, nor in the first two of the three notes quoted, where I probe it further, did I have in mind a precise case in point, if not, to some extent, that of my friend Pierre (examined in more detail in the third note quoted). I was well aware, moreover, that this case was by no means typical of the entire Congrégation des Fidèles, forming

p. 1130 the famous "third plan" at my Burial. Also, in the absence of a precise case in point, my apprehension of a certain reality, suddenly glimpsed, remained marred ☐ encore by a certain vagueness - that of things sensed, "known" at some level, but not fully and clearly "seen". I vaguely remember being a little embarrassed by this vagueness, that there was a desire to find someone "representative", among those of my friends whom I knew to be involved in the Burial, to somehow "hang" this diffuse knowledge on it, to see it embodied in a tangible reality.

The thought of Serre never crossed my mind at the time - he was one of the very few of my old friends for whom it was clearly decided (on a conscious level, at least) that he, at least, was not a party to my Funeral! But if my groping mind couldn't find then (or even before...) the person who, at my funeral, was to embody "the whole Congregation", it must have been that somewhere inside me, it was clear that there was **only one person in the world** suited to play this role - and that it was precisely the person whom a heaviness within me had made me exclude from the outset, by a kind of tacit and peremptory taboo... .

Now that this heaviness has dissipated, following a slow and obscure subterranean work, it now appears to me in full light that it is also the one, of all, to whom this intuition-in-search-of-an-incarnation applies in such a perfect way, that you'd think it was none other than the very one who brought it out in me and gave it, from the very moment it appeared, that peremptory and unrepeatable force of things "known"<sup>873</sup> (\*).

p. 1131 **d. L'Enterrement - or the natural slope**

☐ (June 17) Every time, reality goes beyond any presen-

It's only when I come into contact with it, usually unexpectedly, that I can gradually absorb its taste and smell. Even though this contact might seem to simply **confirm**, without more, what was sensed or "known", it often disconcerts and shakes a certain, almost ineradicable **disbelief** in what is well and truly known, said, written, retold and rewritten - and yet, to

<sup>873</sup>(\*) I'm even inclined to think that this "one could believe" actually corresponds to the reality of things. This would attest, once again, to the extent to which our faculties of knowledge go beyond the pale and derisory reflection to which we allow access to the narrowly delimited field of the conscious gaze.

(June 14) The thought, or sudden intuition, that concludes yesterday's reflection also appeared in a "flash" at the moment of writing, without any apparent preparation or desire for examination. It presented itself with a kind of "force of evidence". It was only in retrospect that I remembered that in the note immediately preceding the one from which the quoted passage of November 10 is taken, I had mentioned Serre's person and the relationship between him and me in some detail (for the first time, incidentally, in Récoltes et Semailles).



At a certain level (that of an immense heaviness), it continues to remain a dead letter. More than once, I've detected this heaviness<sup>874</sup> (\*), and my impatience has been irritated by it - a stubborn heaviness that tenaciously wants to keep me in the rut of familiar ideas and images, or those that have more or less general assent - even though I also "know" (or that someone or something **else** in me knows...) that these well-established ideas and images are a sham, often an obvious sham, that they don't stand up. . . ) that these well-established ideas and images are a sham, often an obvious sham, that they don't hold water. . . Thought, even when driven by an intense desire to know the final word (of the thing both "known" and rejected) - thought alone is powerless to erase this heaviness, deeply rooted in the structure of the self. It's only the peremptory force of direct contact with reality that sometimes has the power to upset this heaviness, to dent it or shift it just a tad, if not actually erase it.

I phoned Serre yesterday. It was a simple question of information, about Tate's "Rigid analytic spaces" notes, which were discussed recently<sup>875</sup> (\*\*). I thought I vaguely remembered that there had been a short introduction to this text, mentioning the sources of this work - it seemed to me that this introduction had "jumped" from the edition made by the care of *Inventiones Mathematicae*, in 1971. In fact, Serre confirmed that there was no such introduction in Tate's notes. They were more or less day-by-day notes that Tate had sent to Serre on his rigid-analytical cogitations, like letters almost, and (of course) without any fixed idea of publishing them. I remembered taking care to have them distributed by care of IHES (with the subtitle "Private-notes published with(out) his permission □ - after the name of the author), but I'd forgotten that Serre had been an intermediary. In any case, apart from Tate and myself, it was Serre who had been most "in the loop", in the birth of rigid-analytic spaces, in 1962. It was he who had explained to me, perhaps a year or two before, the theory of elliptic curves known as "Tate's curves", on the fraction field  $K$  of a complete discrete valuation ring. I was a little taken aback by what I remember as a flurry of explicit (and, it seems, "classical") formulas, which went a little over my head, without "catching on". But a striking geometrical image remained, surely prompted by a comment of Serre's along these lines: that, in short, Tate's elliptic curve (or, at least, its "points") was obtained by "passing to the quotient" in the multiplicative group  $K^*$  by a discrete isomorphic subgroup  $Z$ . This was the analogue of the complex case, where we first divide  $\mathbb{C}$  by a first factor  $Z$ , to find  $\mathbb{C}^*$ , and then again by a factor  $Z$ , this time to find an elliptic curve. In this case, the passages to the quotient had a precise meaning, in the complex analytic domain, and Riemann-Serre theorems (GAGA type) ensured that the final quotient (which was a compact complex curve) had the canonical structure of an **algebraic** curve. In Tate's case, alas, working in the context of somewhat familiar analytic spaces, on the complete value field  $K$ , the quotient was a **totally discontinuous** compact analytic space, and there was no chance of deriving an elliptic curve. And yet (this is what Serre must have said to me then) everything was happening, as if... . In any case, Tate was able to produce a genuine elliptic curve in terms of  $K^*$  and its discrete subgroup, using explicit formulas.

I seem to recall that neither Serre nor Tate believed that there would indeed be an "explanation" in terms of a new notion of "analytic variety" over  $K$ , for Tate's computational construction<sup>876</sup>.

<sup>874</sup>(\*) On the subject of this "heaviness" and "incredulity in the face of the testimony of one's healthy faculties", see also the note "Le devoir accompli - ou l'instant de vérité" (n° 163), pp. 782-784, and in particular the b. de p. note (\*\*) p. 782.

<sup>875</sup>(\*\*) see note "La maffia" (n° 171), part (c), "Les mémoires défaillantes - ou la Nouvelle Histoire".

<sup>876</sup>(\*) (September 1985) As it appeared from a correspondence with Serre last July, there has been a distortion here of letters from Tate (4.8.59 and 16.10.61) and me (18.8.59 and 1.10 and 19.10.1961), addressed to Serre, enable us to reconstruct the film of events. It was Tate (not Serre, nor I) who first had the intuition and conviction that there had to be a "new notion of analytic variety", to simply explain the formalism of "Tate's elliptic curves", around August 1959. It didn't "click" with me right away (as I'd put it).

As for me, it clicked right away(\*) , and there was no question of me "seeing" Tate's curve as anything other than the result of a quotient passage, for a notion of suitable "variety" that had yet to emerge - the kind of work, precisely, that I have a crush on! It may well have been Serre too, skeptical though he was, who pointed out to me that there were people, and at least Krasner, who were "doing analytical extension" on ultrametric, i.e. totally discontinuous, complete value bodies. This might have seemed to support my (somewhat zany) hope that there would be, in spite of everything,

p. 1133 a "good notion" of analytic variety, smarter than the one we knew and close (by "connection" type properties) to real or complex, or even, algebraic  $\square$  analytic varieties. But then again, I was the only one in the trio who really believed it - at least, that's the impression I had at the time.

I couldn't get it out of my head for months, maybe a year. The situation reminded me of an old perplexity - the impossibility, in the conceptual context available at the time (using ringed spaces, like schemas and formal schemes), of making sense of the **geometric fiber** of a formal scheme on the discrete valuation ring  $A$  under consideration. It soon became clear that this was essentially the same perplexity - and that the kind of "varieties" I was looking for to give a geometrical meaning to Tate's construction, had to be the very one that would make it possible to give a meaning to this famous yet non-existent "generic fiber". Finally, I had a third thread (in addition to the rumor about Krasner), which appeared in 1968 - it was the intuition of "generalized topological spaces" (which at the time hadn't yet been given a name such as **site** or **topos**, since I hadn't begun any conceptual work on them), which was to make it possible to define the famous "*l*-adic Weil cohomology" entering (implicitly) into Weil's conjectures. This suggested to me that, as with Weil's cohomology, the new "species of structure" I was looking for should not be sought on the side of the endless ordinary "ring spaces", but perhaps in these "generalized spaces", provided with a bundle of suitable rings.

I don't know when these scattered intuitions finally became strong and convincing enough to prompt me to take a break from my day-to-day tasks (especially EGA and SGA), to begin an embryonic piece of work. What I do know is that this work was done, as is often the case, in solitude - I was the only one to "see" that there was something there, and the only one, consequently, who was in a position to do the initial work that would bring it to light. I remember starting to think about it for a few hours here, a few hours there, even a whole day, a bit like playing hooky (although there was no shortage of "regular" work!). Eventually, I got the bit between my teeth and had to stick with it for good - I must have spent at least a few days in a row, if not a week or two. The hardest part was overcoming the inveterate habits of thought that constantly seemed to want to pull me back into the rut of the known - that of "ordinary" analytic spaces.

now, I think, "flabby" - or "welk", in German). I had to go over it three or four times - to get out of  $\square$  the rut, when I saw that I was back in it, like a horse in its stable! But decidedly, here, it wasn't the old man who was going to do the trick. ...

p. 1134 At the end of the job, I knew for sure: modulo additional technical work, which I wasn't

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thought I remembered), my very first reaction to Tate's suggestion was rather skeptical, before I started to think about it. It wasn't long before I was convinced, once I realized that existing notions (notably that of the formal schema) were unable to account for the phenomena associated with Tate's elliptic curve. In the two years that followed, I think I was the only one to think of a definition principle for the new notion, while neither Tate nor Serre had the slightest idea how to approach it. It went on like that until October 1961, when I provided Tate with the blueprint for a theory. This immediately triggered him into developing the foundations required to get a grip on the local pieces (work which would have made little sense until he had a clear idea of how they could then be assembled to build global objects). For more detailed comments and quotations from the relevant letters, I refer you to the "Historical Comments" in Volume 3 of Reflections.

motivated to do so, I had set up a notion of "rigid-analytic space" (this is the name I gave it, to express by the word "rigid" properties like connectedness, close to algebraic varieties and at the antipodes of those of analytic varieties called "flabby"), sufficient in any case to answer the two desiderata that were then in my mind: to give an interpretation, in terms of these spaces, of Tate's construction, and of the generic fiber of a formal scheme.

I didn't think to look any further, as I was in a hurry to get back to the tasks I'd momentarily abandoned. If I'd played around a bit more, I'd soon have realized that some of the es-

paces as simple as the closed crowns  $r \leq r \leq R$  (which also deserved a "rigid-analytical") escaped my construction. It was Tate, whom I had made aware of my cogitations of course, who made the necessary adjustments to be able to include them. Apart from the conceptual work itself, which I had done for the most part, there was also work of a more technical nature to be done, to get a good grasp of the "building blocks" used, playing the role of affine diagrams. This is precisely the work that is done, with characteristic elegance and care, in Tate's 1962 notes<sup>877</sup> (\*).

□ It took me a while, moreover, before I came to the realization that the building stones I had used were a little short around the edges. They were sufficient for the two initial problems that had motivated me - so why look any further! I couldn't get over it. Tate finally convinced me, in his quiet yet thorough way, that there were more than just those two examples after all, and that even though I didn't seem to have encountered circular crowns yet in my life, that was no reason to rule them out. And there was no way, apparently, to "make up" for them with my own building stones (except by using an infinite number of them, which more or less put me back in the "flabby" rut).

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<sup>877</sup>(\*) To put things in perspective, I think it's fair to say that both my work and Tate's were equally essential stages in the development of the theory of rigid-analytic spaces. My part had been in the initial vision (which had been lacking in both Tate and Serre) and in a mostly conceptual work, which was by no means exempt from certain technical aspects that had to be tackled head-on. The work at Tate had been mainly technical, although there was also a certain amount of conceptual work. My work was predominantly "yin", "feminine" (and that's why, in addition to my absence from the scene, it's the object of general disdain), while Tate's was predominantly "yang", conforming to the canons of good taste and good manners.

If I hadn't intervened, pushing my work far enough to remove any doubt about **the existence** of a good "rigid-analytic" notion, and for a clear vision of a theory's master builder, it's likely that this notion would still not have seen the light of day today. Indeed, while it was inevitable that this notion - which is by no means an "invention" - would be discovered and developed "sooner or later", the need for it has not, in the twenty-three years since then, been sufficiently pressing to "force" people to "take the plunge". I was apparently the first to foresee (in 1966) another field of application for rigid-analytic theory, apart from the two initial motivations, with the development of crystalline cohomology.

I'm not aware of any geometrical uses other than the three I'd planned - including, of course, the generalization of Tate's theory to general Abelian schemes. It would seem that the people who subsequently "worked on the subject" saw it mainly as an opportunity to develop the theory in a vacuum (since it existed, and there was a consensus that it was a "serious research topic"), without inserting it into a broader geometrical vision. This is a striking example of the **atomization** and compartmentalization of mathematical thought, linked to the contempt in which any kind of fundamental work has fallen, as well as any work that is not reduced to some technical tour de force, enabling the solution of some "competition problem". A particularly eloquent sign is the absence of any attempt to develop a more general notion of rigid-analytic space, which would be to that developed by Tate as the notion of scheme is to that of algebraic variety over a body - so as to be able to link together rigid-analytic geometries over "variable" complete value bodies (and in particular, of variable characteristic, and including both real and complex cases, as well as "ultra-metric" cases). This absence is one of many signs of the astonishing stagnation of mathematics over the last fifteen years, at the level of any work on foundations (obviously crucial, in this case).

Getting back to Tate and me, it's just as likely, of course, that if my first "breakthrough" hadn't "clicked" with Tate and set him off for a "second round", rigid-analytical spaces wouldn't exist any more I'd have talked about it here and there around me, but as there's never been a shortage of juicy questions (including ones that seemed even more "urgent"), it's doubtful that anyone would have taken to it - and certainly not these days, when the very idea of introducing such crazy things would have sounded a bit too much like someone it's more charitable not to name here. . .

I'd done my share of the work on my own, as was normal, when I was the only one who believed in it - but that didn't stop me, of course, from talking to both of them once I'd reached the (provisional) end.

main (and practically only) concerned, namely Serre and Tate. At Tate's it obviously clicked, and I think Serre must have been convinced too, when I told him what I'd come to. I don't remember exactly, but

if by some extraordinary chance it had been otherwise, I'm sure I would have remembered. So when I phoned Serre yesterday, I took it for granted that he knew, almost as well as I did, what my part had been in

the birth of the new notion of variety. I didn't expect him to mention it, but when I told him about Tate's notes, he pointed out that they had been published *ne varietur* in the *Inventiones*, and that Remmert and two other authors had just published a book devoted to the famous rigid-analytic varieties. This is the book I had occasion to mention recently, in the note "La maffia", part (c<sub>1</sub>) "Les mémoires défaillantes - ou la Nouvelle

Histoire", where I accuse Remmert of a "faulty memory" (even though Tate's own notes could well have refreshed it), in the service of a bad faith that seemed obvious to me. I touched on this in passing to Serre - I

had already had occasion, in my last letter to him, to allude to a certain Funeral<sup>878</sup> (\*), and there was a rather blatant illustration.

The first crazy thing was that Serre (God knows he'd had a front-row seat in the past!) - well, he didn't remember that I'd had anything to do with those famous rigid-analytical varieties either! I was literally speechless! It was really crazy

- when I alluded to a modest part I thought I had played in it, based on the two examples that had triggered me, it was **just the opposite that** he, Serre, thought he remembered: almost that I wouldn't have wanted to know anything about these new varieties, saying (according to him) that with formal schemas, we had already

everything he  $\square$  fallait! I could hardly believe my ears at the time<sup>879</sup> (\*) - and yet, a few days just before, I'd written a few pages in the most serene fashion, about a crucial role, a "pillar" role, that Serre would play in a certain Burial. Well, there I was, right in the middle of the Burial, in front of my nose at the other end of the line, and in the very person of this same Serre, very much at ease as is his wont, and obviously in the best faith in the world! (And I can't imagine Serre acting in bad faith anyway, especially when it comes to maths... ).

I didn't feel like chatting, that's for sure, and Serre even less so, but we did have an off-the-cuff conversation, for five or ten minutes. Ten minutes well spent if ever there was one, to rub shoulders with the tangible reality, color, taste, smell and all, of a Funeral that had become a little distant, by dint of my limiting myself to looking at nothing but paper!

The first thing I had to think about saying was that the **name** itself, "rigid-analytical spaces", was me.

<sup>878</sup>(\*) It is in the reply to this letter (in the last letter from Serre that I received) that Serre quotes Siegel's expression, on the "Verflachung" ("flattening") of contemporary mathematics, on which I comment and continue in the note "Useless details" (n° 171 (v)) part (c), "Things that look like nothing - or desiccation". As I say in this note, Serre had dismissed this impression of Siegel as "**unfair**" - yet I had the impression that it turlipinated him a bit, that Siegel thinks like that. And it's that same term again (probably unintentionally) that he uses, also to dismiss my allusion to a Funeral.

Needless to say, it didn't occur to him to ask me **what it was that** made me say there was a funeral (I hadn't said a word about it in my letter, preferring to wait for him to ask). The cause, obviously, had already been decided...

<sup>879</sup>(\*) In retrospect, I've come to understand the deformation that took place in my friend's memory (a little faulty around the edges). Since I'd used formal diagrams as my main and virtually only guide to defining a rigid-analytic space (so as to be able to associate a rigid-analytic generic fiber with a formal diagram), he'd remembered (twenty-three years later) that I'd stubbornly maintained that there was no need for a new notion of variety, since "my" formal diagrams would suffice for everything (as memory lapses often do...).

However, already  $K^*$  (my second conductive wire) does **not** come from a formal scheme. In any case, here again, the case had already been made!

who had given it (implying, if I didn't say so clearly: at a time when I was still the only one dreaming about it, about these things I called them... .). Serre was a little taken aback - obviously, he couldn't remember either, but it was also obvious that I wasn't having fun making up stories. But never mind, a name is just a name after all, and **such a natural one at that**... . That "so natural" clearly implied that it was so natural that it didn't even mean anything anymore, that anyone with their nose in front of it couldn't help but call it just that: "rigid-analytical". In short, my friend was unintentionally paying me a compliment on the name - but with the air of "if that's all it is...".

...!". Besides, I hadn't published anything about it, had I? So there was nothing to say...

I was more and more dumbfounded. Published or unpublished, it made no difference to me. A woman who carried a kid nine months and brought him  $\square$  into the world and here he is frolicking and in good shape, someone

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tell her it's not her kid, since nothing's been published and she can't even show off the birth certificate - she's sure to laugh in the face of anyone who says such a thing. To tell the truth, I didn't laugh at Serre, which isn't my style, and anyway, I was still too blown away. Nor did I think to discuss the fact that Tate himself, in his notes, made no secret of the part I'd played in starting the theory (something Serre had apparently forgotten, as had Remmert<sup>880</sup> (\*)). - and that in 1972, when I wrote the *Esquisse Thématique* in which I alluded to it<sup>881</sup> (\*\*), Serre hadn't even pretended to notice (his memory must have been working since then). It would have been a wasted effort anyway, obviously - as long as nothing was published, anything I said would count for nothing. ...

But the "unpublished" had struck a chord, and I went on to say that a major part of my work consisted of unpublished stuff, communicated by word of mouth. I sensed that Serre was still taken aback.

this was an idea that seemed to him to be a little preposterous, like a contradiction in terms "work - not published. . .", for him it didn't seem to  $\square$ aller together. I said the word "motif", and he jumped on it right away: now he was going to disabuse me of the ideas I'd had about burials, and he was happy to tell me that two or three years ago, a whole book had been published on motifs - really, I couldn't complain about the "motifs" chapter!

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"So, have you held it in your hands, this famous book?" I asked her (it was fitting, I'd been thinking about asking her this interesting question for a while).

Holding it in his hands - but perhaps I was joking, Serre retorted, for sure he knew this book; he even spoke of it as if he'd read it at length, and that's because he had to have read it

<sup>880</sup>(\*) I felt, once again, that "**in any case**, the case had been made". If Tate said he was following "in a totally faithful way" a masterpiece I'd provided, well, never mind - it was only a masterpiece after all, a vague drawing that any kid could draw in the sand, a vague Grothendieckian sauce, for sure - it was still nice of Tate, really chummy as hell, to take the trouble to mention it... ..

<sup>881</sup>(\*\*) This is the text, dated 1972, presenting a rather dry (and not very inspiring) sketch of my mathematical contributions to that date, written on the occasion of my application for a position at the Collège de France (a position which was awarded to J. Tits). This text, supplemented by more detailed historical comments, will appear in volume 3 of *Réflexions*. It is discussed in particular in Introduction 3 (Boussole et Bagages). In the *Esquisse Thématique*, 5 e), I write:

**"Rigid-analytic spaces** . Inspired by the example of the "Tate elliptic curve", and the needs of "formal geometry" on a complete discrete valuation ring, I had arrived at a partial formulation of the notion of rigid-analytic variety on a complete value field, which played its part in J. Tate's first systematic study of this notion. Moreover, the "crystals" I introduce on algebraic varieties over a body of characteristic  $> 0$  can sometimes be interpreted in terms of vector fields with integrable connection on certain types of rigid-analytic spaces over bodies of zero characteristic; this hints at the existence of deep relations between crystalline cohomology in char.  $p > 0$ , and cohomology of local systems on rigid-analytic varieties in zero characteristic."

That's Grothendieck all over again!

indeed. I could have left it at that if he hadn't found anything peculiar in it - he obviously hadn't, and yet (that's how we're made, I can't help it!) I asked him anyway! And as he didn't seem to understand the meaning of the question, I told him that when I picked it up last year, I could hardly believe my eyes.

I had to say the word "swindle", but I felt it was an understatement. As I had really felt it, and still feel it as I write these lines, it was an **indecent** - but I refrained from reading it. Deep down, I sensed that it didn't matter what term I used; nothing had passed in the fifteen years since "it was hard" and Serre chose not to feel anything (which is what I'd just written, a few days earlier), and no matter what I said, it wouldn't "pass".

It was as if he'd been waiting for it. Swindle? You want to dream, my poor fellow, but it was Deligne himself who wrote this book, and a fine piece of work it was too - okay, everyone knows very well that it was you who introduced the motifs, but that's no reason to repeat it every time the word "motifs" is uttered, is it? Not to mention the fact that you've never published a line, and that your yoga depended on unproven conjecture (here I thought I was hearing someone else speak to me through Serre's mouth...), whereas the whole point of the book is that it doesn't use any conjecture - in fact, it doesn't use **anything** you've done in the past...

The tone was crisp and unapologetic, of one who knows very well what he's talking about and has nothing more to learn - with a hint of annoyance, of the un□peu pressured man, taken to task by a lump who stubbornly refuses to

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understand the most obvious things. It wasn't the right mood to ask about anything.

- everything had already been settled and awarded. Serre's axioms of business ethics and what's important and what's incidental had obviously changed - and there was nothing I could do about it. I had to take it as it was, with its new axioms.

So I hit on "conjectural", in desperation! I could have told him that Weil's conjectures were conjectural too - and yet, there was no question of him or anyone else treating them underhand - but it's true that Weil had taken care to publish these conjectures! But as I'm just at the "Sixth Nail" (in my coffin)<sup>882</sup> (\*), I turned to the "Galois motivic group" instead; there was nothing "conjectural" about it, I had developed a whole theory of great precision on Galois-Poincaré type categories, which was one of the basic notions used in this famous book, without it seeming necessary to make the slightest allusion to myself.

Serre jumped at the hint, again, so that he could disabuse me of my Burial ideas - the whole theory was published in black and white in a book by another of my students, Saavedra<sup>883</sup> (\*\*)

- Wasn't it me who even got him to do this thesis? Here again, obviously, it was a book he knew perfectly well, he'd had to refer to it more than once<sup>884</sup> (\*\*\*). "And then, in that book, nothing struck you either" - I asked him again (and this time, it was clear that I already knew what the answer would be).

No, it obviously didn't strike him that my name shouldn't be mentioned in this book, nor for the theory

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<sup>882</sup>(\*) This is the group of notes (n° s 176<sub>1</sub> à 176<sub>7</sub>) to which I'm putting the finishing touches, and in which I unscrew the con, precisely, around the notion of motivic Galois group and Galois-Poincaré-Grothendieck categories (christened "tannakiennes" for the occasion) - scam set up by a Deligne and (initially) through the "pawn" Saavedra interposed. ...

<sup>883</sup>(\*\*) This is the famous book "Tannakian Categories" (sic) by the same Neantro Saavedra Rivano, published in Lecture Notes 265 (1972), Springer Verlag.

<sup>884</sup>(\*\*\*) In fact, I understand that when Serre has the opportunity to quote from this book, in which my name is not mentioned (so to speak), and in which he finds (as far as he is concerned) nothing abnormal, he nevertheless takes care (I don't know what scruple) to refer to me at the same time. He must be the very last person still to take this kind of trouble... .

nor for the ancillary notions (such as motif, crystal and tutti quanti) introduced therein.

□ ab ovo and developed as examples. Here, however, Serre did not seem to have any p. 1141

memory - he still remembers (for the moment at least. . . ) to whom these notions are due, which appear there, under the pen of another of my pupils, without my name being mentioned either. If there is indeed a "failure" here, in my friend, it's not in any case at the "memory" level... .

We talked for a few more minutes about the name "Tannakian categories", which I implied I considered a hoax, whereas Serre, with the evidence to back him up, thought it was a perfect fit. Here too, I knew it well before I even raised this new issue; just as I know **why** this name suits my friend so well, while I, who bore and gave birth to this thing, find fault with it.

As is usually the case between us, it was Serre who cut me short - and indeed, it's true that the conversation had gone on long enough. There had been no "communication" at any point, and that's surely why it left me with this feeling of dissatisfaction, of disharmony. And yet, just like the two or three short letters I've received from him lately, and with even more peremptory force, this short conversation taught me a great deal. Things "known", surely, but half-rejected; known and not believed! And surely this feeling of frustration (which hasn't dissipated even today) is a sign of my resistance to welcoming and accepting the message.

An unwelcome message, to be sure. Just a few months ago, I had no doubt that Serre (as I vividly remembered him, the embodiment of incisive elegance and probity free of all complacency), when he became aware (better late than never. . . ), thanks to the reading of the providential text "Récoltes et Semailles", of the turpitudes of a certain Burial (of which he was certainly a thousand miles from suspecting, poor fellow. . . ),

well, his blood would run cold and he'd throw himself into the fray, this time<sup>885</sup> (\*). This image

d' Epinal has dissipated over the course □ de the last few weeks, a harmless exchange of letters helping. And yesterday he p. 1142

that it's been a long time since Serre has been in the thick of it, in l'Enterrement, and that he's quite happy with it. And this, needless to say (and without any hint of irony on my part), with the best faith in the world!

It's been a while since I realized that "good faith" is by no means as simplistic and clear-cut as it had seemed for most of my life. A certain type of "good faith", one of the most widespread, simply consists in giving oneself the lie, like a good-natured flag used to cover sometimes dubious merchandise. Our psyche is made up of superimposed layers, and as our eyes become sharper, we see the "good faith" of one layer sometimes serving as a cover and alibi for the deceptions of the one below.

As for Serre's good faith, I continue to give him credit for the fact that he will never write a book that makes essential use of someone else's ideas without saying so clearly - even if these ideas have never been published, and would be known only to the person who communicated them to him (assuming he's still alive) and to himself. In other words, I think I know that Serre will never write a book like the ones we discussed yesterday. I think I can even say that the mere fact that someone

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<sup>885</sup>(\*) When I wrote "this time", I thought of the two other times I'd gone out on a limb, trying to get a message across to the famous "mathematical community" - and even, on both occasions, to mobilize it. The first time was in 1970, when I left the mathematical scene, on the occasion of the connivance of the scientific establishment with the military apparatus. The second, at the more modest level of French colleagues alone, was in connection with a certain iniquitous article concerning foreigners. in France. (See "My farewells - or: foreigners", n° 24.) Both times, my efforts were met with general indifference, where Serre, no more than any of my other friends in the milieu I had just left (with the only with the exception of Chevalley and Samuel), was no exception. All bets are off as to the effect (or non-effect) that the "Récoltes et Semailles" paving stone will have in this very establishment - starting with Serre himself. ...

like Serre or like me<sup>886</sup> (\*), to write a text (mathematical in this case) addressed to a public, brings into play inveterate reflexes of professional conscience, which will tend to eliminate or at least correct (I believe) certain "memory failures", which are not so consequential in

a simple off-the-cuff conversation like yesterday's<sup>887</sup> (\*\*). This is all in line with what I wrote three weeks<sup>□</sup>encore ago, in the note "Things that look like nothing - or drying up"

(n° 171 (v), part (c)): "I'm well aware that Serre, no more than I, wouldn't dream of howling with the wolves, of looting, scheming and debunking, where "everyone else" is looting, scheming and debunking".

Having said that, I can see that all this does not prevent Serre from enjoying, in some cases at least, the plundering, scheming and debauchery of **others**, openly and overtly, "in the public square" and "under the spotlight". He can certainly do it "in the best of faith".

- he doesn't get his hands dirty, merely giving his unreserved blessing to the plundering, scheming and debauchery of others, and all the more so as he doesn't pocket any visible profits: he doesn't boast about the fruits of others' labors, while finding it good that others (appointed dealers, I might add) play such a game, in plain sight. The "profits" he reaps are more subtle than the publications (a little shady around the edges) and bank accounts that others are so fond of. And yet they are of consequence, giving rise to the astonishing metamorphosis of the man I once knew, who is now (I can't say how long ago) participating, eyes closed and nostrils plugged, in the general corruption<sup>888</sup> (\*).

**e. The last minute - or the end of a taboo** (June 18) Yesterday, I hesitated to add a fourth part to the note "The family album" (n° 173), in order to give an "on-the-spot" account of the phone call with Serre the day before. This phone call, it's true, had left me with a "feeling of dissatisfaction, of disharmony" (as I wrote yesterday) - and these are euphemisms, even, to express such uneasiness.

incisive, that he was approaching anguish. This malaise gave rise to the need to return to this episode, as to a ripe abscess now, and which it would be high<sup>□</sup>temps to empty. And then there was the usual procrastination. That

For weeks now, the USTL duplication department has been waiting for someone to bring them the continuation of this famous fascicule IV of Récoltes et Semailles (Harvest and Sowing), which is still giving birth; already, it's just-August to manage to pull and stitch everything before the annual closing of the Fac (July 15), especially as it's not just me - at the end of this academic year, there's an influx of theses of all kinds, which have to take priority. In short, I told myself that you've got to know how to finish a book; that if I kept adding "last minute" stuff, I wouldn't be able to finish it again next year, that it had gone on long enough... .

And yes, I've finally got around to it - and too bad if the Harvest and Sowing issue is only due back in September! It's waited fifteen years (not to say thirty), now it can wait another two or three months, but let me take the time to look at what I have to look at, and to say what I have to say, without letting myself get carried away.

<sup>886</sup>(\*) When I say "Serre ou moi", I'm actually thinking of any of the members of the milieu to which we both belonged in the 1950s - a milieu I try to define to some extent in parts III and IV of "Fatuité et Renouveau", and more particularly in the section "Bourbaki, ou ma grande chance - et son revers". It's true, however, that even in this restricted milieu, I'm aware of two members who have "gone wrong" (mentioned in due course in Récoltes et Semailles).

<sup>887</sup>(\*\*) Thus, I have no doubt that if Serre had been the author or co-author (as R. Remmert is) of a book on rigid-analytical spaces, he would not have indulged in the "natural inclination" to pass over in silence that which must be passed over in silence; that he would go beyond somewhat complacent "lapses" of memory to the said natural inclination, to which it pleased him to indulge in a private conversation. It's also true that even fifteen years ago, with the rigor I knew him for then, he wouldn't have indulged in such a slope, it seems to me, even in private conversation... ..

<sup>888</sup>(\*) This observation of participation in corruption echoes that made (for the auditors of a certain seminar in March 1980) in the note "Carte blanche pour le pillage - ou les Hautes Oeuvres" (the name says it all), n° 171<sub>4</sub>, in particular page 1090 second paragraph.



rushed by "deadlines" . .

It's been a hard day's work, or rather a night and part of a morning - I wanted this "extra" text for typing to go out with today's mail. And so it did.

At this point, I feel as if I've come to the end of some work that **needed to** be done. Suddenly, I feel light, as if I've been relieved of a great weight that I've been carrying around, probably without realizing it, and I can't say for how long. It must be the weight of a certain tenacious illusion, which must have started to settle in me from the end of the forties, when an adopted identity began to blossom in me, that of a member of a certain (mathematical) "community", of a certain milieu, which for me was filled with warmth and life. I talk about this blossoming of a new identity in *Fatuité et Renouveau*, in the sections "L'étranger bienvenu" and "La "Communauté mathématique" : fiction et réalité" (n°s 9, 10), and also in "Bourbaki, ou ma grande chance - et son revers" (section n° 22). It's true that this identification was swept away without return by the events surrounding and following my departure in 1970, in the wake of my involvement in militant activity. With hindsight, I now realize that there was still a **link** to the milieu I had left behind, in which I no longer saw myself; an invisible link perhaps, but one of great strength, forming part of this "weight of a past" (which I began to glimpse last year, in the section of the same name).

name, n° 50). While I had left this environment with no desire to return, a certain **image** of what had been this "family", in short, which I had ☐quitted for another adventure, remained alive in me, and maintained this link. This image must have remained more or less static, it seems to me, from the moment I left (and long before that, of course) until the moment of reflection in *Harvest and Sowing*. The latter began to nuance the image I had of a certain past, and to incorporate as best I could elements of the present, often disconcerting and unwelcome. Eventually, I came to realize that there had been an astonishing **deterioration** in the state of mentalities and mores in what had taken over from the milieu with which I had identified myself, and (it would seem) in the mathematical world in general. This deterioration, I realized, had been going on for some time, and I had had time, even before I left, to play my part in it. (At least, in the course of my reflections in *Fatuity and Renewal*). I did get the impression, however, that there was a kind of unbridled escalation in this degradation after my departure, in which some of my ex-students played a leading catalytic role.

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Be that as it may - throughout the revelations that followed one another in my investigation of the Burial, I maintained in my mind a sort of tacit "taboo" around those of my old friends who were part of the milieu that had welcomed me in my younger years. I simply couldn't conceive that any of them had been seriously affected or "damaged" by the profound degradation I was witnessing. When I sometimes spoke of the complacency of the "whole congregation" towards operations which (for me at least) were beyond imagination, surely there must have been some kind of inner "clause" in me, absolving those who, for me, had to remain "above suspicion". They didn't suspect a thing, obviously - they must have been busy elsewhere, surely - you can't blame them! A bit in those tones. And for the oldest of my elders, this way of seeing things corresponds, I'd like to think, to reality, or at least to a certain aspect of reality. But certainly not for people like Serre, Cartier, Borel, Tate, Kuiper, Tits and others whom I've known well, who are of the same generation as me, in full activity, fully integrated into the milieu I'm examining here and who continue, even today, to wield a not inconsiderable power and to set the tone, just as much as certain newcomers who have ended up forming an unscrupulous "mafia", with the unreserved blessing of their elders.

So there was a stubborn and flagrant contradiction in my image of reality, as it appeared through the first-rate "revealer" that is Burial. It was this contradiction

surely' perceived at one level and rejected at another, which created that "malaise" I spoke of earlier, at the anguish - anguish revealing a **division**. And the person who, more than any other, embodied for me this milieu, of people whom someone in me persisted in perceiving as "close", and the one who had been "closest" of all among them, was Jean-Pierre Serre. As such, it was in him, more than in anyone else, that lay the crux of the eluded contradiction.

I timidly began to address this contradiction only six weeks ago, in the first part (dated May 4) of the note "Useless details" (n° 171 (v)). This reflection deepens considerably in the third part of the same note (dated May 27, three weeks later), "Des choses qui ressemblent à rien - ou le dessèchement". I return to the person of Serre again, against perennial inner resistance, a week ago (June 11) in part c. ("The one among all - or acquiescence") of this note. This time, Serre's crucial role in l'Enterrement finally came to light. This was another major step forward in my understanding of L'Enterrement - but the crux of the contradiction remained unaddressed! Serre remained for me (as if nothing had ever happened) the embodiment of "elegance" and "probity" without fear or reproach. The "taboo" remained safe and sound!

It was the phone call the day before yesterday that exploded the contradiction, rubbing my nose in it (l'Enterrement), whether I liked it or not. As is only natural, considerable forces of resistance (mentioned earlier) were immediately mobilized to maintain the status quo, rather than accept the contradiction: to acknowledge it, one way or another, and thereby resolve it. I was free to do so, or not.

I took the plunge - and I'm glad I did. The reward was immediate: a sense of **liberation**, a feeling of lightness, of relief; relief from inner tension, of course, but more than that, liberation from a weight.

The only other moment in Harvest and Sowing when there was a similar sense of liberation was the one that marked the first major turning point in our thinking, in Fatuité et Renouveau, with the section "La mathématique sportive" followed by "Fin le manège!" (n° s 40, 41)-I have the impression, moreover, that this new step I've just "taken" follows on from the one I took last year. I couldn't say enough, at the time, about why and <sup>en</sup>quoi. The triumphant exclamation then, "No more merry-go-round!", was premature that's (as I realized as early as the following month). But the new step I've just taken is, to say the least, a step further away from the merry-go-round. Time will tell to what extent this is the case.

After yesterday's reflection and that of June 11, I feel I've arrived at a less hazy vision of the Burial. It was mainly this "third plan" that remained vague. The reflection of the 11th will have made it "incarnate", in a tangible way, in the person of Serre, and this in turn took on very concrete outlines (so to speak) during yesterday's reflection.

Finally, in this entire fourth part of Récoltes et Semailles, it is the reflection on the relationship with Serre that seems to me to be the most crucial part, for my own understanding of l'Enterrement, beyond the "complements of investigation" and the colorful tables from the shallows of the mathematical megapolis. It's also true that if I hadn't taken the trouble, out of respect for the subject I've set myself the task of investigating, to stick to this "tidying up of an investigation" with all the care I'm capable of, taking great care also to illuminate as best I could all the slightly dark corners that presented themselves along the way, this reflection on Serre would probably not have seen the light of day either, and my understanding of the Burial (and my involvement in it) would have remained blurred as before. Everything fits together in a research project!

The most substantial part of the reflection, in this last part of Burial, appeared in

done "last minute". In principle, the "period" under this section had been set two and a half months ago (April 7). There were just ten or so pages left to retype, and a few footnotes to add (as had also been the case a year ago, towards the end of May. . . ). The unexpected started to happen in the following days, with the visit of Zoghman, who came to read this last part (in principle finished) and give me his comments. They materialized in some three hundred pages of additional text - and among them, these pages where I return to the relationship between Serre and myself, in the light (hitherto eluded) of the Burial.

### 18.5.7. Climbing (2)

**Note** 174 □(March 22)<sup>889</sup> (\*) As I have pointed out elsewhere, there are not actually four operations p. 1148 (for a Funeral), but a single "**Operation Funeral**". Its division into four major parts was convenient for exposition, but is artificial and (if taken too literally) apt to mislead. Surely, in the *Metteur en scène* - Chef d'orchestre - Principal Officiant aux Obsèques, there weren't **four** little devils in four different corners of his head telling him what to do, but one and only one! During the long meditation on yin and yang<sup>890</sup> (\*\*), I tried to get to know this little devil better than I had in the past, when I'd merely noted from time to time that he was always there stirring, and moved on to something else the next moment. I don't claim to have fully succeeded in making his acquaintance, and perhaps that's not my job after all. One thing's for sure, though: he's still out there waving his arms, and there's no guarantee that he'll stop before my friend breathes his last. Still, the famous "Operation Burial" continues, even as I write these lines. And I wonder whether the publication of this "Family Album" will at least put an end to the biggest (and most iniquitous) of all partial operations: that of burying alive a young mathematician, Zoghman Mebkhout, whose ideas and results have been used by "everyone" working in the cohomology of algebraic or complex varieties for the last four or five years. ...

Abandoning the fiction of "four" operations where there is clearly only one, it would be interesting to sketch, in chronological order, the main episodes and stages known to me. I won't do so here, as I feel I've done enough in the four main notes above ("Silence", "Manoeuvres", "Sharing", "Apotheosis", n° s 168, 169, 170, 171) to bring together all the episodes known to me, which the curious reader can arrange in chronological order. Curiously, from a "second level" or "operational" point of view (to use euphemisms), the year of my departure from the mathematical scene, 1970, doesn't seem to mark a discontinuity in the succession.

episodes, which have been continuing at a fairly regular pace, it seems to me, since the end of the SGA 5 seminar in 1966, until 1977 with the double publication of "SGA 4<sup>1</sup>" and the -□Illusie edition of SGA 5<sup>891</sup> (\*). This p. 1149 operation seems to me to mark a sudden and striking **qualitative change**. Before, there was a discreet "reaping". Now I feel the sudden eruption of a gust of violence and contempt, raging against the work of someone absent, declared "dead".

After this sort of collective **outburst** by all my cohomology students (under the compliant eye of the "whole Congregation"), it seems that there's been a lull for four years. Whereas

<sup>889</sup>(\*) (June 14) This note follows on from part a. ("A deceased well surrounded") of the previous note, written on the same day.

<sup>890</sup>(\*\*) This is the reflection forming the major part of the third part of *Récoltes et Semailles*, with notes n° s 104 to 162".

<sup>891</sup>(\*) (June 3) This impression should be corrected, taking into account the large-scale operation "Tannakian Categories" (sic), whose first episode (with the "straw father" N. Saavedra) takes place in 1972 (and the epilogue in 1982, with the "real Father" P. Deligne taking over). On this subject, see the series of notes "Le sixième clou (au cercueil)" n° s 176<sub>1</sub> - 176<sub>7</sub>.

while in the eleven years between 1966 and 1977, I detect a typical "episode" every one or two years, I know of none between 1977 and 1981 (the year of the Colloque Pervers). On the contrary, Deligne's long article "La conjecture de père, II", published in Publications Mathématiques in 1980, i.e. the year before the incredible Colloque, can almost be considered normal, these days. ... <sup>892</sup>(\*\*). It was also the year in which Deligne learned of the "theorem of the good Lord" (alias Mebkhout)<sup>893</sup> (\*\*\*), at a Bourbaki seminar and from the author himself. This was the start of a sudden melting of the ice in a long stagnation of the cohomological theme. And it was also the signal the following year for the second and ultimate (?) culmination of Operation Burial, this time on the iniquitous diapason, when all restraint, and even simple prudence, were blithely thrown overboard.

The episode of the "memorable volume" LN 900 the following year (consecrating the exhumation of the motifs without men-

tion of my person, an episode that so moved me on a certain April 19th of last year. . . ), just like Berthelot's report of the same year (consecrating the elimination of my humble self of the "history"-sic-of crystalline cohomology), appear to me afterwards□as the natu-

The Colloquium's name will perhaps go down in history (or what remains of it) as a **warning**. And the "Funeral Eulogy" the following year, incredible as it may seem to anyone who "poses" on it, also appears as such a pro- longing, or (as I wrote earlier<sup>894</sup> (\*)) as an "epilogue". As for the two years that have elapsed since then, they have merely confirmed, in writing and in the minds of many, the "achievements" of a brilliant Colloquium and its follow-up. ...

It's a remarkable coincidence - or rather, it's clearly **not** the effect of a "coincidence" - that as early as last year, and before I'd even become acquainted with the "SGA 4<sup>1</sup> - SGA 5" operation or the Colloque Pervers, I noted two "turning points" in my friend Pierre's personal relationship with me, set in the same years 1977 and 1981. I include them for the first time in a common attention and try to fathom their meaning, in the note "Deux tournants" of April 25, six days after I discovered l'Enterrement (by reading the memorable LN 900). At the time of the two turning points, years before, I was far from suspecting (not on a conscious level, at least) that the Burial was taking place, and I would have been hard-pressed to link either of them to any event known to me that might have shed light on them.

### 18.5.8. Funeral parlours - "Im dienste der wissenschaft" (In the service of science)

**Note** 175 (March 23) To complete my tour of "Operation Burial", it remains for me to review the role of one last active and eager participant, whom I've had occasion to mention "in passing" many times in the course of this long reflection on the said Burial. I'm talking about the esteemed Springer Verlag GmbH (Heidelberg), a well-known publisher of books and scientific periodicals, ho-

<sup>892</sup>(\*\*) Of course, no allusion is made to me in connection with the main result of the work, the statement of which was part of the yoga of motives that Deligne took from me. On the other hand, I was struck by the fact that my name appears, along with Miller's, in one of the paragraphs of the work, in connection with De Rham's power-divided complex, which had been introduced (around 1976) independently by Miller and myself. I had given a talk on this theme in 1976, at the IHES (it was, incidentally, the last public lecture I gave in my life), but it was clear that I wasn't going to publish anything. No one would have noticed, or even objected, to the author's failure to mention this official co-paternity... ...

<sup>893</sup>(\*\*\*) (June 1) In fact, this episode took place the previous year, in June 1979, at the Bourbaki seminar.

<sup>894</sup>(\*) In the "Jewels" note, n° 170 (iii).

norant d'ailleurs de la devise "Im Dienste der wissenschaft" - au service de la science<sup>895</sup> (\*\*). In the company's mathematical edition, the Lecture Notes in Mathematics series is undoubtedly the most important.

most famous of all. It is also perhaps the world's most successful series of scientific texts □ la p. 1151

more prodigious: over a thousand titles published in twenty years or so. In fact, I believe I played my part in this unprecedented success, by lending my support to this series, still in its infancy, through the publication of numerous texts by students or myself, during the sixties and into the early seventies. I was also associated with Springer as one of the editors of the "Grundlehren" series (der Mathematik und ihrer Grenzgebiete), where three books (including the reprint of EGA I) were published by myself<sup>896</sup> (\*).

After my departure from the mathematical scene in 1970, I refrained from any activity as editor. By a simple inertia effect, I continued to be one of the editors of the series until just last year, when I finally "officially" withdrew from any editorial responsibility at Springer. I was prompted to do so by two concordant motivations. On the one hand, at a time when I'm returning to "orthodox" mathematical activity, by publishing maths again, I want to draw precise limits to this "return", which for me in no way means a return to a "powerstructure" (a structure of power and influence), but solely to personal mathematical **work** destined for publication. On the other hand, since 1976 (with the episode of Yves Ladegaillerie's thesis), I had had occasion to smell a certain air of En-terrement, long before I had the slightest inkling of the large-scale operation I discovered last year. (See "On n'arrête pas le Progrès" (n° 50), and especially the more detailed "Cercueil 2: ou les découpes tronçonnées" (n° 94), about the episode in this thesis, one of the most brilliant I've had the honor of inspiring. This made me realize that "the kind of mathematics I like and would like to do

encourager no longer has a place in Springer Verlag"<sup>897</sup> (\*\*); and perhaps even more than that, that the spirit I felt □ didn't encourage me to continue or resume any kind of close ties with this house. p. 1152

The year that has passed since I resigned from the Grundlehren editorial board in February last year has only confirmed and strengthened this feeling.

But this is on the fringe of "Operation Burial" proper - that "second level" I mentioned yesterday, to which it's time to return. To my knowledge, there are **five books** directly linked to the operation in question<sup>898</sup> (\*). They are, in chronological order of publication, the volumes SGA 7 I (published under my name in 1972) and SGA 7 II (published under Deligne-Katz's name in 1973), presenting the SGA 7 seminar.

<sup>895</sup>(\*\*) (June 1) On enquiry with Dr. J. Heinze, it appears that this is not really a "motto", but rather an advertising slogan. Its English form is "Springer for Science".

<sup>896</sup>(\*) The other two books are Jean Giraud's and Monique Hakim's theses (on the formalism of fields and non-commutative 1-cohomology, and on relative schemes on general ring topoi).

<sup>897</sup>(\*\*) This quotation is taken from the short letter (addressed to Dr. Peters) of February 18 last year, in which I informed him of my decision to retire from the Grundlehren editorial board. Dr. Peters had in fact already left Springer Verlag (he now works at Birkhäuser Verlag), and correspondence continued with Dr. J. Heinze, in charge of Grundlehren at Springer. I had asked for a copy of my letter to be sent to each of the eighteen co-editors of the Grundlehren, and had repeated this request to Dr. Heinze on two occasions (in April '84 and January '85) without him seeing fit to tell me whether or not it had been complied with (as it turned out, it **hadn't**). I took the trouble myself to send a copy of my letter to each of the eighteen publishers, with a few words of explanation as to why it had been sent. I know seven of them well personally, and counted five of them among my friends. Only one (Artin) took the trouble to reply, and none of them apparently found it unusual (if only to **themselves**) that Springer didn't take the trouble to send them the letter in question (and as early as February 1984).

<sup>898</sup>(\*) (June 1) Since these lines were written, it has come to light that a sixth book, whose very name is a mystification, should be added to the following list: "Tannakian Categories", by Neantro Saavedra Rivano. Remarkably, this book too appeared in the same series of Springer's Lecture Notes in Mathematics. But in this case, Springer's responsibility does not seem to be engaged, as it is for the other five volumes. For

For details of the "Tannakian categories" operation, see the "Sixth nail (in the coffin)" suite of notes, n° s 176<sub>1</sub> - 176<sub>7</sub>.

on monodromy groups, 1967/69; the volume entitled "SGA 4<sup>1</sup>" (by Deligne) and the Illusie edition of SGA 5 (published under my name) in 1977; finally, the "memorable volume" devoted to the exhumation of motifs, published under the joint Deligne-Milne-Ogus-Shih signature in 1982. Remarkably, all **five** volumes were published by the **same** publishing house, and in the **same** Lecture Notes series<sup>899</sup> (\*\*). The first four volumes were published when Dr. K. Peters was in charge of Lecture Notes<sup>900</sup> (\*\*\*), the last with Mrs. M. Byrne in charge of this series.

These five publications took place under conditions that seem to me to be grossly irregular. As I have already pointed out elsewhere, the two volumes SGA 7 I and SGA 5 **published under my name** in 1972 and 1977 (LN 288 and 589) were published without Springer deeming it necessary to contact me, either to request my agreement or merely to inform me of the publication project. The publication of two volumes of the name "SGA 7 II" and "SGA 4<sup>1</sup>", thus presenting themselves under the acronym "SGA", which I believe is not

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p. 1153 not available to all, but notoriously linked to my work and person, have been published without asking for my agreement to the use of this acronym for the planned publications, while I do not appear (as would have been expected) as the author, or director (or one of the directors) of the volume, or of the seminar of which it presents a redacted version. Finally, volume LN 900 presents, without naming me, notions, ideas and constructions that are well known among well-informed mathematicians to have been introduced by me. In this case, it was obvious (without having to be one of the few insiders at an SGA 5 or SGA 7 seminar) that this volume constituted what is commonly known as **plagiarism**. I certainly don't expect Mrs. Byrne, who was in charge of LN (unless I'm mistaken) at the time of this volume's publication, to have the competence to recognize the fraud on her own, in view of the manuscript. But it is, I imagine, part of the job of a serious publishing house to ensure the seriousness of its publications, by surrounding itself with competent advisors.

These same advisors were also in a position, if they were honestly doing the job for which they are (I imagine) paid, to point out to those entitled that the APG sign is not an acronym to be taken lightly, that it has a **meaning**, which should be respected by consulting the only person qualified to decide on the use of this acronym, namely myself. Finally, as an aggravating circumstance concerning the publication of the volume presenting itself under the misleading name "SGA 4<sup>1</sup>", one need only peruse either the introduction to the volume, or the "Ariadne's Thread" which follows it, or the introduction to the first chapter, to note the casual disregard with which the SGA 4 and SGA 5 seminars are treated; it's also common knowledge among even the least well-informed that the latter seminars took place in the mid-sixties, whereas the volume presenting itself as "SGA 4<sup>1</sup>" is made up of apocryphal texts from the seventies. In my opinion, therefore, for a reasonably well-informed person in possession of all his means, the deception was obvious. All the more reason, therefore, not to publish such a volume under such a name, without first seeking my express consent.

I therefore consider Springer Verlag to be entirely responsible for the publication of each of these five volumes, which constitute key episodes in the monumental swindle that has been perpetrated on my work on cohomology. Through these publications, Springer acted as an auxiliary and **conveyor** for this unusual operation. I cannot, of course, assert that it was by

full knowledge of the facts. But I can say that the repeated discourtesies I have experienced from this house in its relationship to me, since the year 1976 (I have not had the opportunity, I believe, to have with her between 1970 and 1976) are also in line with this operation and are part of a certain

p. 1154

<sup>899</sup>(\*\*) These are volumes n°s 288, 340, 569, 589, 900.

<sup>900</sup>(\*\*\*) As stated in the penultimate b. de p. note, Dr. Peters has since left Springer Verlag for Birkhäuser Verlag.

**spirit**, which is inseparable from it.

In the sub-note "The eviction" (n° 169<sub>1</sub>) of the note "The maneuvers", I alluded to my letter to Mrs. Byrnes concerning the publication of SGA 5, and to her reply, which blew me away I must say. (It's certainly not the first time nor the last that I've been "blown away", in this brilliant operation "in the service of science". . . ) I learn from his letter (dated February 15, '85) that, in accordance with "the usual way of acting when a work contains contributions from several authors" (sic), there was no need to address myself specifically, as I was only the **director of** the Seminar. ... The five "authors" of SGA 5 are Bucur, Houzel, Illusie, Jouanolou and Serre, to the exclusion of my humble self, who appears only as "director" - no doubt purely honorary, I had said too much<sup>901</sup> (\*) - for this brilliant seminar.

As soon as I received this instructive letter, and finding the time long (having received nothing for a month), I took up my best pen (in German) to write to Dr. K.F. Springer himself, who is one of the directors responsible for Springer. It was a fine two-page machine letter, explaining to him that I was very saddened by a long series of unpleasantnesses in my relationship with Springer, and beyond these, by a number of gross irregularities against me, of which I was content for the moment to submit two, which seemed to me particularly flagrant: the publication of two volumes of Lecture Notes (n° s 288, 589) published under my name and without deeming it necessary to consult me. That in these two texts, the ideas, methods and results I had developed in oral seminars were shortened or mutilated, sometimes to the point of being unrecognizable. That the coincidence of this last fact, with the unusual circumstances surrounding the publication of these two volumes, could not be for me the effect of pure chance. And that I expected a public and unreserved apology from Springer, in a form to be determined by mutual agreement, once an agreement in principle had been reached. That I hoped

that he, like me, would be keen to put an end to an unpleasant and unacceptable situation and to find a solution that was equal to the circumstances □ ("eine dem Fall geziemende Lösung zu finden", which is p. 1155 even more distinguished), "hoachachtungsvoll" (as it should be) signed by my finest hand.

To put my cards on the table, it seems to me that I have put my cards on the table! He won't be able to say, Sir K.F. Springer, that he was not personally informed of the situation, and not even at first hand, by anyone other than the main interested party himself!

As luck would have it, I finally received a reply (a good month later) just yesterday. It's so short that I can't resist the temptation of reproducing it here (translated) in extenso. It took me a moment to realize that it was in fact a reply to my beautiful letter of last month. So here it is.

Heidelberg 15.3.1985

Dear Professor Grothendieck,

I must thank you again for your letter of February 9. Mrs. Dr. Byrne's letter of February 15 will no doubt have answered your questions.

Receive etc

K.F. Springer

At least now I know! The "well-informed" people (already mentioned) must have explained to him that he didn't need to tire himself out for the slightly excited gentleman who wrote to him there - that he decidedly didn't do any

not part of the beautiful world. And it's true, too. . .

We look forward to receiving this enlightening reply from the management of Springer Funeral Services.

<sup>901</sup>(\*) In this famous "Ariadne's thread" (through SGA 4 etc.) in the volume entitled "SGA 4 $\frac{1}{2}$ ", nothing could lead the reader to suppose that I had the honor of making presentations in SGA 4 and SGA 5 (on the other hand, I did have the honor of "collaborating" in "SGA 4<sup>1</sup>". . . ). On this subject, see my comments in the note "Les double-sens - ou l'art de l'arnaque" (n° 169<sub>7</sub>), p. 899.

Verlag GmbH (it was kind of them to honor me with a reply signed by the director himself), I had time to sound out my own intentions. The role played by the esteemed company seems to me to be really big, and I thought about the possibility of a show trial, in which I would ask for astronomical damages, as an outraged "gentleman", victim of unspeakable preferential treatment. But I also told myself that a trial like that must take a lot of energy. Even if I were to win the case and collect dizzying damages (let's be optimistic!), after X number of years of course - what would I have to gain? I'm not in need, and I don't need more than I've got - and a scam is no more or less a scam, because a certain lawsuit has been won, or lost. I'm not going to

improve the world, nor myself, nor the manners of Mr. K.F. Springer and certain employees of the company  
☐ which

p. 1156

he runs, and in any case not their way of conceiving their profession, mobilizing lawyers and by making them mobilize their own<sup>902</sup> (\*). Nor will I improve a certain spirit in a certain beautiful world I've left behind, the spirit that makes possible the kind of operation that Dr. Springer and his esteemed house have made themselves (for thirteen years) the servants of. I have (I hope) a few years left to live - time flies, and I see plenty of exciting things to do in the time I have left. It can't be very exciting to gather evidence to convince judges that I have something to do with SGA. It's not for them, any more than it is for Mr. K.F. Springer, that I've bothered to write them... ..

As for those (apart from myself) for whom I've written the SGA, their relationship to what (for me at any rate) remains a part of myself, is by no means indifferent to me. It's part of their relationship to me. Strangely enough, I only know this relationship (or at least a little bit about it) from my five cohomology students: the very ones who have made it possible for a Dr. K.F. Springer to dismiss me like a scoundrel who has nothing to say about what people do or don't do with texts bearing the SGA acronym, whether or not the person in question appears on the cover.

The mathematician reader who may have followed me here, and who may one day have haunted the SGA (the real ones, I mean), may have the idea to drop me a line about what he thinks of it himself. It would certainly please me to receive a note from someone who thinks that the work into which I alone poured all my energy for ten years of my life, and which **no one** in the world had the heart to continue once the worker had left - that this work does indeed bear the imprint of the person who conceived it and carried it inside him for as long as it took, before it took shape under his hands and became a **home for all**<sup>903</sup> (\*\*). And that a house for all is not a vespasian in a slum, where everyone feels free to relieve themselves as they please and scribble their obscenities on dilapidated, sticky walls... ..

☐ And if he who reads me is one of those who were my pupils, or of those who were my friends, and he doesn't feel

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to write or speak to me, at least on this subject if no other, let him know that his silence is also eloquent, and that he will be heard.

<sup>902</sup>(\*) Incidentally, I've also considered the possibility that the situation might be reversed, with the esteemed company suing me for damage to its reputation. These people "in the service of Science", they must be fastidious in this respect (as long as it's their reputation that's at stake. . . ).

<sup>903</sup>(\*\*) This idea of building "houses" that are good "for everything" has played a considerable role in my mathematical work since the early fifties. It has been the concrete expression in my work of what I've called the "service impulse", which has been part (without my even detecting it before the reflection "The key to yin and yang") of the profound forces giving my mathematical work its living force. The "house" archetype appears for the first time I had not foreseen it, but it came to my mind with great force in the November 26 note "Yin the Servant, and the new masters" (n° 135).



## 18.5.9. The sixth nail (in the coffin)

### 18.5.9.1. a. Pre-exhumation

**Note** 176<sub>1</sub><sup>904</sup> (\*) (April 19) I finally had the opportunity to read (on April 10) the article by R.P. Langlands cited in the note "La pré-exhumation" (n° 168<sub>1</sub>). According to the "commented bibliography" on motifs that Deligne sent me last August, this article by Langlands is, along with Deligne's article in the same volume (which is the subject of the cited note), the first in which motifs have been used, since my departure in 1970<sup>905</sup> (\*\*). I'm excused for not having been aware of Langlands' article until last year (nor of Deligne's), since the author didn't deem it necessary (nor did he want me to!) to publish it.

than my ex-student) to send me a separate print. One wonders why he would have bothered,

when it's clear from reading his article that my humble self has strictly nothing to do with the subject p. 1158

"Automorphic representations, Shimura varieties, and motives" discussed in his article. My name (to use a formula that my typewriter knows by heart, for a year to the day!) is nowhere to be found in this article, nor in the bibliography. However, I thought I recognized certain ideas I had developed around 1964 (or dreamt I had developed them - I'm definitely repeating myself again. . . ), and I even wrote down in black and white this memory of a dream (or perhaps the dream of a memory of a dream. . . ), on that same nineteenth of April 1984<sup>906</sup> (\*). I'd think I was back on that very day, a year ago.

It's true that I've had time to become blasé in the intervening year. If there was any displeasure, it was hardly a surprise (considering how little, one might say. . . ), and certainly not a shock. There is, moreover, a major difference between this article, the precursor of the memorable LN 900 volume that was to follow three years later, and the latter: I didn't have the honor of meeting Langlands in person, and it wasn't from my mouth that he learned (as Deligne did around 1965 or 66) about the yoga of the Galois group (or "fundamental group") known as "motivic". But, throughout the second half of the sixties, I talked enough about it around me, to whoever would listen (and Langlands, after all, hasn't just arrived. . . ), to have a presumption that Langlands knows full well where this new "geometrical" philosophy concerning Galois and fundamental groups of all kinds, seen as suitable affine pro-algebraic groups, comes from. I presume he knows full well that this philosophy was not born in 1972 from the brain of a certain Neantro Saavedra Rivano, who has since disappeared from circulation without a trace<sup>907</sup> (\*\*\*) - I feel that it would not be a luxury for Langlands to explain himself on this subject, if he deems it useful of course. Admittedly, given the times we live in, it's perhaps over-optimistic of me to hope he'll take the trouble... ..

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<sup>904</sup>(\*) (June 16) The following group of notes (n° s 176<sub>1</sub> to 176<sub>7</sub>), entitled "Le sixième clou (au cercueil)" ("The sixth nail (to the coffin)"), should be seen as a natural sequel to the group of notes entitled "Le silence" ("Silence") (n° s 168 (i) to (iv)), devoted to the "Motifs" operation, and in particular to the last of these, "La pré-exhumation" ("The pre-exhumation") (n° 168 (iv)), dated April 8. The following notes, with the exception of the last one (n° 176<sub>7</sub>), are dated April 19th and 20th. If I have preferred to reject them here, at the end of the "Four operations", instead of attaching them to the "Motifs" operation, it's because the reflection that had been going on in the preceding weeks on the other three operations had not yet been completed.

operations, and especially the one (known as "the Perverse Colloquium" or "the stranger on duty") which is the subject of the "Apotheosis" group of notes, threw unforeseen light on the (equally unforeseen) "new fact" which had just appeared. I recall that at

At the time of writing the following notes, I had already, in principle, set the "final point" under Burial (whose final note, "L'amie" (n° 188) is dated April 7), and I expected to have the complete manuscript of Burial III typed up any day now. In other words, these notes were written as "last-minute complements"... ..

<sup>905</sup>(\*\*) With the exception, however, of the presentations by Kleiman and Saavedra in 1972, in line with the few modest "ranges" on the description of the category of motifs (compare with the b. de p. note (\*\*) on page 794, in the note "Les points sur les i", n° 164).

<sup>906</sup>(\*) On this subject, see the note "Souvenir d'un rêve - ou la naissance des motifs", n° 51.

<sup>907</sup>(\*\*) According to what Deligne told me during his visit to my home last October, Saavedra has practically changed profession (he is now "in economics"), and hasn't done maths at all since defending his thesis in 1972.

### 18.5.9.2. b. The pleasant surprise

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**Note** 176<sub>2</sub> □ As good surprises never come alone, the day after I got to know-

In the wake of Langlands' article, I also had the opportunity to peruse Neantro Saavedra Rivano's volume (to which Langlands refers extensively), entitled "Tannakian Categories" (Lecture Notes in Mathematics 265, 1972).

Of the nine (male) students I had before I left, Saavedra had been the only one of whom I had never heard another word, nor had I heard anything to indicate that he had taken on the "color" or "smell" of a certain Burial. I had hastily concluded, with my customary naive confidence, that (if only for lack of opportunity, perhaps, having left the mathematical waters from what I heard. . . ) he was the pupil of all who had remained entirely alien to the spirit of the Burial "operation". Yet, as in Jouanolou's case, I had heard so little about it, that it might just have tipped me off. I knew, of course, that what was supposed to be his thesis when he was working with me, had finally appeared in Lecture Notes in 1972 in the volume cited, which I don't recall ever bothering to look at until last week<sup>908</sup> (\*). Fully absorbed in other tasks, the thought hadn't occurred to me that it was a little strange that Saavedra hadn't given me any sign of life, even if only to inform me of his thesis defense, and to ask me to sit on the jury, as the person best placed to know what it was all about. It's when I read this volume that it becomes clear why he preferred not to disturb me in my other occupations, and to pass his thesis "à la sauvette", before a jury whose composition I'm entirely unaware of<sup>909</sup> (\*\*). The burial was already well underway, since no one of the members of the jury saw fit to even inform me of the defense, let alone ask for my participation in the jury (as had also been the case for Jouanolou's thesis, which must have taken place around the same time)<sup>910</sup> (\*\*\*)).

p. 1160

□ This volume exposes a crucial aspect of this "arithmetic geometry" whose vision was born and developed in me throughout the sixties (without yet having been given a name), and of which the yoga of motives was (and still is<sup>911</sup> (\*)) the soul. Essentially, Saavedra's book is a careful and detailed exposition of my ideas on a kind of "Galois-Poincaré theory" of certain categories (which I would never have dreamed of calling "Tannakian"...), ideas that I explained to Saavedra at length and patiently, at a time when it was still doubtful whether he would make the effort of familiarization and assimilation necessary to be able to include them in an "expository" part of his thesis work. I had entrusted him with detailed handwritten notes, complete with four-pin statements, demonstration sketches and all, and I'm still waiting for him to send them back to me<sup>912</sup> (\*\*). Of course, the subject of the thesis itself

<sup>908</sup>(\*) (June 16) Saavedra must not have seen fit to send me this book, of which I don't own a copy, but I may have held it in my hands in the seventies. I had remembered, but no more than that, that he had made a careful work and perfectly usable as is, but I can't pinpoint the exact source of this impression. It had been present, in particular, when writing the note "La table rase" (n° 67, and in particular p. 252-253), where I comment on this "mystery" of a Deligne "copying" practically the thesis that Saavedra had done with me.

<sup>909</sup>(\*\*) The mystery of this jury's composition is elucidated in an entirely unexpected way in the seventh and last of the notes of the "sixth Clou" (n° 176<sub>7</sub>), of which I will say no more here. . .

<sup>910</sup>(\*\*\*) For a correction, see the note quoted in the previous b. de p. note.

<sup>911</sup>(\*) But in the meantime, this "soul" has been enriched by "Anabelian" yoga, which is mentioned in some detail in "L'Esquisse d'un Programme". (For more on this text, see Introduction 3 "Compass and Luggage". It will be included in volume 3 of Réflexions 4.

<sup>912</sup>(\*\*) It was my habit to distribute my handwritten notes right and left among my students, as needed - and one of the

dite was not to expose the ideas of another, whose motivations completely escaped him. The point was to explain a "useful" intrinsic characterization of "tensorial" categories that I'll call here "de Galois-Poincaré"<sup>913</sup> (\*\*), i.e. a category admitting a description "à la Galois-Poincaré- Grothendieck", in terms of linear representations of a "(pro)algebraic affine sheaf" on the base ring  $k = \text{End}(1)$  of the category under consideration. When the latter is a body, I had indicated such a condition by the so-called "rigidity" property (in the terminology I had introduced), and I seem to remember that I had written a complete demonstration (from my first thoughts on the motivic Galois group, on p. 1161 1964/65)<sup>914</sup> (\*). I had to show him the principle, but refrained from giving him my written notes on the subject, since it was up to him, not me, to learn his future trade by doing the work himself. If I remember correctly, the only outstanding question for me was to determine the natural domain of validity of such a theory à la Galois-Poincaré, as regards the hypothesis to be made about the base ring  $k$ , being interested in particular in the case where this would be a ring such as  $\mathbb{Z}$  (because of the applications to pattern theory).

Of all the students I had before I left, Saavedra, the latest to arrive<sup>915</sup> (\*\*), was also the least well prepared, and (initially at least) the least motivated to "give it a go". That's why I didn't expect him to go beyond the very limited technical problem I'd given him, which required only the most modest knowledge (a little diagram language, linear algebra, flat descent, sheaf language, and nothing more). The more delicate questions that are the subject of Chapters IV to VI of his book (filtrations of fiber functors, polarization structures on a Galois-Poincaré category over  $\mathbb{R}$  and a list of such categories that are "polarizable", applications to pattern categories and numerous variants) required somewhat "all-round" knowledge<sup>916</sup> (\*\*\*), and hence a considerable effort to get up to speed, which I didn't believe Saavedra would be able to provide; at most, I hoped that he would perhaps append to his work a summary (more or less dictated to him by me) of the following points important aspects of the theory that would not have been included in a formal exhibition. I was disabused than last week, and I realized that Saavedra has done a really impressive job and in p. 1162 in record time<sup>917</sup> (\*). The result is a book with a detailed and careful presentation, impeccably mimed and perfectly usable as it is, presenting a virtually exhaustive (as did I...

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first thing they had to learn, was to decipher my handwriting. It was always understood that I wanted them to return my notes as soon as they had finished using them - but I don't think this was ever respected. This is just one of the many signs that my students didn't fear me at all, but rather saw me as the "good guy", demanding in terms of work to be sure, but otherwise accommodating like no other. . .

<sup>913</sup>(\*\*\*) So as not to call them "Grothendieck categories"! And yet, among the many species of categories (and other new notions) that I've had the honor of introducing and naming (and which, for this reason, don't bear my name), if there's one for which this appellation would be appropriate, out of simple decency I'd be tempted to write, it's this one! (Apart from the topos, whose name seems perfect as it is...) As for the name "Tannakian categories", surreptitiously slipped in by a brilliant ex-student (and complacently adopted by a unanimous Congregation), it's nothing short of a *mystification*.

(See the note following "He who knows how to wait... ", n° 176<sub>3</sub>.)

<sup>914</sup>(\*) I didn't want to take the time to check this in my notes on the motivic Galois group (or rather, on what which I didn't give to Saavedra). In any case, I'll come back to this in volume 3 of *Réflexions*, probably in the chapter entitled "Les motifs mes amours".

<sup>915</sup>(\*\*) If I remember correctly, Saavedra asked to work with me in 1968 or 69, a year or two before my unexpected departure from the mathematical scene.

<sup>916</sup>(\*\*\*) Above all, it required a thorough knowledge of the structure theory of reductive algebraic groups, of their classification on the field of reals, plus familiarity with a whole range of notions such as motif, crystal, F-crystal, stratified modules, local systems (for someone who had at most a vague notion of the singular fundamental group of a topological space), plus Hodge theory, and delicate "polarization" properties that had never been spelled out in the literature but remained "between the lines" in current reference texts.

<sup>917</sup>(\*) For more information on this "record", and its (obvious) explanation, see the note "Monsieur Verdoux - or the cavalier servant" (n° 176).<sub>5</sub>

the geometric-algebraic formalism I had developed in the sixties. From this point of view, then, I feel that he has done a useful and thoroughly creditable job, and the "surprise" I referred to earlier has indeed been "a good surprise".

This work consisted, very precisely, in putting into "canonical" and publishable form (according to the rigorist criteria that were still mine at the time) a set of ideas, statements and demonstrations that had been supplied by me. It's part of the mathematician's job, of course, to present one's own ideas and results, as well as those of others. Unlike many of my colleagues, I don't believe that such work should be counted as a negligible quantity when it comes to assessing the quality of a thesis or any other publication, and even when it comes to awarding the title of "docteur" in mathematics to the person who does it - in other words, to consider him or her a mathematician in his or her own right. On the other hand, it seems to me essential that a certain elementary ethic of the profession be respected, and that where a job consists of exposing and developing the ideas of others, the matter be clearly indicated, so as not to leave the slightest ambiguity in this respect.

In this case, however, nothing in the entire volume, apart from three lines of vague, perfunctory "thanks" lost at the end of a brilliant introduction<sup>918</sup> (\*\*), could lead the reader to suspect that my modest self had anything to do with any of the themes developed, starting with the one that is the very subject of the book. I'd have thought I'd returned to the day of my first encounter with the

memorable volume-exhumation of motifs (exactly one year ago today, to the day)! My name appears practically nowhere in the volume, except on two or three occasions, when some references in form are necessary, and none are available that are not from my pen.

p. 1163

This is by no means the only effect of **embarrassment, not to** seem to recognize that the author is "only" presenting the ideas and results of another - which (especially in this case) is not bad, when the work is done with intelligence. But I've come to realize, through a number of unmistakable "little details", that this is by no means just a bit of "reaping" to burnish one's reputation, before disappearing into the wings. It really is **a funeral for a funeral**. To give just **one** example - God knows I spent days and weeks explaining at great length to Saavedra, who had just arrived and knew nothing about anything, the notions of crystal,  $F$ -crystal (replacing in  $\text{car. } p > 0$  the missing  $p$ -adic "coefficients", enabling  $L$  functions to be defined. . . .), stratified moduli (and their relations with local systems), and finally a minimum of pattern yoga (taking as a provisional heuristic basis the standard conjectures); all this to make him understand, through a wide range of examples, where I was going with these Galois-Poincaré categories, and for the case (one never knew. . . ) that he would find the courage and perseverance to include at least, beyond the planned "minimum program", a chapter of typical examples. As he knew very well, without my having to explain it to him at length, these are crucial geometrical notions that don't go back to Adam and Eve; it was none other than I, who explained them to him over and over again without tiring, who had introduced them over the previous five or ten years, to serve as tools for a certain vision (even if it went over his head, as it went over the heads of all my students except one<sup>919</sup> (\*)). But my name doesn't appear either where he introduces and develops these notions (in Chapter VI devoted to

<sup>918</sup>(\*\*) This introduction essentially consisted in copying verbatim the four main statements I had indicated to Saavedra as being the "pillars" of the Galois-Poincaré yoga to be developed (excluding questions related to filtrations on fibres functors, which were difficult to summarize in a single lapidary statement) ; but by augmenting one of these statements, the one that was supposed to constitute the "minimum program" of his thesis, with a monumental and obvious error, which rendered it trivially false! This is discussed in the next note ("He who knows how to wait... ", n° 176<sub>3</sub>), and especially in the note already quoted "Monsieur Verdoux - ou le cavalier servante (n° 176<sub>5</sub>)" and the one that follows it "Les basses besognes" (n° 176<sub>6</sub>).

<sup>919</sup>(\*) Who stopped himself from burying it, as soon as the master's back was turned. ...

to examples), than in the part of the text devoted to the development of the theory of which he pretends to be the author. However, I can't imagine Saavedra imagining that the reader, however ill-informed he may be and however willing he may be to believe him the father of these categories (which he generously calls "Tannakian"), would go so far as to think that it was this same Saavedra who, for the sake of the argument, invented the F-

crystals, patterns and other gadgets of the "Tannakian" (sic) panoply. If these notions are treated like any-thing we'd just improvised, or picked up at □the nearest orphanage, I've well recognized a **style** that I know all too well, from the year I've been touring l'Enterrement. ...

p. 1164

Mebkhout had brought me the volume in question, delighted to be able to show me the case of one of my students who, at least, had been "honest"<sup>920</sup> (\*). He had been visibly dazzled by the three lines of thanks at the end of the introduction - it's true that in 1972 it wasn't very popular to thank a certain deceased person, and since then it's been more the tone of a persiflage or a joke that's become the order of the day with more than one of my ex-students, if not complete silence. The fact remains that this time I'm entitled to "deep gratitude", for "having introduced [the author] to this subject", and for my "advice and encouragement... .." which were indispensable in bringing this work to a successful conclusion... ."<sup>921</sup> (\*\*). That's what we call paying lip service, when simple honesty in the presentation of one's work would have seemed to me a more convincing way of expressing "gratitude", at a time when the Burial was definitely going well.

### 18.5.9.3. c. He who knows how to wait

**Note** 176<sub>3</sub> □In fact, it was enough for me to hold this book in my hands to realize that before the memorable "operation SGA 4 $\frac{1}{2}$  - SGA 5", there hasn't been a single episode in the whole of Burial, which is comparable in scope to this LN 265 volume, aptly named "Tannakian Categories". Previous episodes<sup>922</sup> (\*) were all limited to a more or less discreet "reaping", concealing the filiation of certain important ideas. Here, a crucial part of my vision of "arithmetical geometry" has been "hijacked", and this, by means of the one who may have seemed the most "insignificant" of all my students!

p. 1165

<sup>920</sup>(\*) (June 16) By the way, he was absolutely sorry that it had failed, and did his best to win me over - it reminds me of the case of Kawai (see b. de p. note (\*) on page 1078), or that of Beilinson, whom Mebkhout found "more honest" than Bernstein (see page 1072). (\*) page 1078), or that of Beilinson, whom Mebkhout found "more honest" than Bernstein (see page 1072) - like Diogenes with his lantern, but this time looking for an honest mathematician in the "gang" of those who dabble in the decidedly ill-famed theme of cohomology of all kinds... ..

<sup>921</sup>(\*\*) These "thanks" are a joke, given the circumstances: you'd think I'd "introduced" the author to the "subject" of functions of a complex variable, or to any other classical subject of the same kind. In fact, the "subject" in question didn't **exist** when I spoke about it to a Saavedra in need of a thesis, except in a vision that had developed within me in symbiosis with that of the motifs, and in my handwritten notes that gave it shape. I write about the birth and development of this vision in the note "Souvenir d'un rêve - ou la naissance des motifs", and about the contempt in which it is held. with which one of my former students (and under the complacent eye of all) wipes the slate clean of these roots, in the note that follows "L'Enterrement - ou le Nouveau père" (notes n° s 51, 52).

(June 16) Saavedra's thanks are all the more "a joke", as the author never bothered to tell me about his work. to send only a copy of his book and his bogus thanks. Now that I've seen the whole "Tannakian categories (sic)" operation through, I understand all the more how my ex-student had no reason to be proud of his "work"-sic, and that he was in no hurry to see me take note of it. And as things looked then, and as they did until two years ago, it seemed very unlikely that the workman would ever see it... .

<sup>922</sup>(\*) The "episodes" in question are briefly outlined in the note "Burial. . . " (n° 168 (ii)), part of the suite of notes devoted to the "Motifs" operation.

It's true that, behind this one, I clearly recognize, in a style that's not misleading, who's pulling the strings - and who, incidentally, figures prominently among those to whom my ex-student lavishes his thanks<sup>923</sup> (\*\*). The very **name** given to the volume from Saavedra's pen, and to the crucial notion I had introduced, is a subtle act of **dispossession**. It would not be surpassed, in its lapidary effectiveness, until five years later, by the sole virtue of yet another name, given to another volume, but this time from the pen of Deligne himself<sup>924</sup> (\*\*\*)).

If the name " $\mathrm{SGA}_2 4^1$ " given to a certain saw-cut volume is a sham of genius, the name "Tannakian category" is a **mystification**, just as genial. Even in the case of a "trivial" or "neutral" Galois-Poincaré category, equivalent to that of finite-dimensional linear representations of an affine group scheme  $J$  over a body  $k$ , the yoga I had developed is typically "Grothendieckian", inspired as it is by the analogous yoga I had developed in the case of the fundamental group of a topological space, a scheme or (more generally) a topos. The idea of defining the fundamental group as the group of automorphisms of a fiber functor on the category of coverings of a "space" or "topos", and the idea

p. 1166 The idea of systematically working with the not **necessarily**  $\square$  **connected** category of slab coatings (which is just as crazy because it's new, and therefore unusual) had, in the past, earned me a great deal of sarcasm. I don't

I never bothered, knowing that none of those jokers who thought they knew Galois or Poincaré's theory because they had learned it at school, had really understood it - and none of them, to this day, could take even **the first** elementary **steps** in Galois theory of coverings of a (let's say) somewhat general scheme<sup>925</sup> (\*), without repeating verbatim the work I've done on this subject, and the formulation I've given of Galois-Poincaré theory of coverings in terms of category equivalence<sup>926</sup> (\*\*).

And similarly, the idea of reconstructing an affine group scheme (over a body, to fix ideas) from the "abstract" category of its finite-dimensional linear representations, equipped with its natural multiplicative structure and its natural "fiber functor" "forgetting the operations of  $J$ ", as the **diagram in groups of the automorphisms of this functor** - this idea is due neither to Tannaka (who never asked for so much), nor to my modest ex-student Saavedra, nor to my most brilliant student Deligne (to my great regret - but he wasn't around yet), but it's a typically "Grothendieckian" idea. And the same goes for the fact that we thus find a perfect correspondence between affine group schemes over  $k$ , and rigid tensorial  $k$ -categories equipped with a fiber functor over  $k$ . And the same goes for the idea that, if by chance (as tends to be the case for categories of patterns on a body of non-zero characteristic) we have a rigid tensorial category which (by misfortune, or by extra good fortune. . . ) does **not** have the advantage of possessing a fiber functor, then the "algebraic group" must be replaced by an "algebraic **sheaf**". This idea was spelled out at length when young Deligne had yet to hear the word "sheaf" used in maths, and it's still true today.

had never yet dreamed of anything like it. There too, when Giraud took it upon himself to develop an arsenal of  $\square$  non-commutative cohomological algebra in dimension  $\leq 2$  in the sixties, blows

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<sup>923</sup>(\*\*) On the "mathematical" side of things, these people are (in order of appearance) myself (out of alphabetical order, that was nice), Berthelot and Deligne.

<sup>924</sup>(\*\*\*) As will become apparent below (in the note "Monsieur Verdoux - ou le cavalier servant", already quoted), there is at the very least a strong presumption that instead of reading here "but this time from the pen of Deligne in person", it would be licit to read "and also from the pen of Deligne in person".

<sup>925</sup>(\*) "So-so general" could be interpreted here, precisely, as "a non-normal scheme". Before me, the fundamental group of an algebraic variety had only been introduced (by Lang and Serre) in the case of normal varieties, by describing it as a suitable quotient of the "absolute" profi ni Galois group of its function field,  $Gal(\bar{K}/K)$ .

<sup>926</sup>(\*\*) Today, this way of formulating the relationship between fundamental groups and coverings, even in the "school" (if you will) special case of ordinary topological spaces (locally simply arc-connected), is beginning to be seen everywhere, without any allusion to the ancestor, need I say... .

of fields, sheaves and links<sup>927</sup> (\*), there was no shortage of sniggers. It's the kind of thing that Deligne and co. have been calling a "gange of nonsense" for a long time now. These sniggers didn't bother me<sup>928</sup> (\*\*), I knew where I was going - and it was with "rapture" (as I write elsewhere) but no real surprise, that I saw this "gange" capture with perfect finesse delicate and profound relationships that I knew no other "language" would be able to capture.

That said, when the same sneerers one day realize a "cream pie" that had escaped them, whether it's the categories that some are quick to christen "Tannakian" (while waiting for something better. . . ), or a certain "correspondence" or "relationship" or "construction" (a little neo-Grothendieckian around the edges) that is euphemistically dismissed or dubbed "Tannakian". ), or a certain "correspondence" or "relation" or "construction" (a little neo-Grothendieckian around the edges) that is euphemistically dispatched or dubbed "Riemann-Hilbert" (while we wait for something better. . . )<sup>929</sup> (\*\*\*) - then everyone rushes in, and it's a race to see who can play the genial inventor. That's the mathematical "zeitgeist" of the seventies and eighties of this century... . What's certain, in any case, is that it wasn't a Saavedra who would have had the idea of calling these categories (which I had explained to him at length) by the truly brilliant name of "Tannakian categories". Left to his own devices, he wouldn't have he'd ever dared change the terminology he'd inherited from me, without at least asking for my agreement - and that was

It was necessary for the example and encouragement to come from on high, so <sup>that</sup>□ he would per- p . 1168 to treat me like a negligible quantity. What's more, the unfortunate man already had enough work to do to bring himself up to speed on what was essential if he was to achieve even part of the ambitious writing program I'd submitted to him<sup>930</sup> (\*), without having to delve into the literature and read Tannaka and what-have-you, which he'd surely never heard of back when he was still working with me<sup>931</sup> (\*\*).

The name is "genius" through the subtle combination of two qualities, which might seem contradictory. One is that, to a superficial observer, the name doesn't sound totally zany. "Everyone vaguely remembers that there's such a thing as "Tannaka duality", in which the multiplicative structure plays an important role. role - and it does seem a little like what happens with those famous  $\otimes$ -categories that a certain Saavedra (who's that one?) calls "tannakiennes"; so go for "tannakiennes", why not!

But for those who know how to wait, things mature on their own. Thirteen years have gone by since then, and instead of a book by an unknown author whom nobody has ever seen, there has been for the last three years a far more prestigious reference, in the brilliant volume LN 900, from the pen of none other than Deligne, and a man called Milne working in tandem. These well-known authors develop ab ovo all the formalism of the categories they

<sup>927</sup>(\*) This suggestive terminology was introduced by Giraud, in place of a provisional terminology (somewhat ad hoc) that I had been using since 1955 (such as "finite categories of a local nature" and other unwelcome names, for notions whose fundamental nature required terse, striking names).

(June 16) On the first page of the introduction to his book, Saavedra talks about the "formalism for non-commutative homological algebra **introduced** by Giraud". This is one of the many places where I could sense someone smarter than the author of this book, who "held his hand". . . the same one who likes to speak of "derived categories" only to add "**introduced** by Verdier" (when he knows perfectly well, in both cases, what he's talking about... . ).

<sup>928</sup>(\*\*) But Giraud did - who distanced himself without return from the theme he had pursued with me, only just beginning it. On this subject, see the note "Les cohéritiers... ." (especially pp. 386-387), and the following note ". . . and the chainsaw" (notes n° s 91, 92).

<sup>929</sup>(\*\*\*) On the subject of this last "en attendant mieux", see the entire "Colloque Pervers" package, and in particular the notes on "Le prestidigi-

tateur" and "Marchés de dupes - ou le théâtre de marionnettes" (n° s 75", and 171<sub>2</sub> (e), the latter being part of the long note "La maffi a" n° 171 ).<sub>2</sub>

<sup>930</sup>(\*) He completed this program in the record time of just two years, from the time of my departure, when this program had hardly even begun (beyond a basic introduction to schematic techniques). Even with the support of a Deligne (who had shown no interest in this pupil before I left), this performance was nothing short of a prodigy - which "prodigy" is examined a little more closely in the note "Monsieur Verdoux - ou le cavalier servant" (n° 176 ).<sub>5</sub>

<sup>931</sup>(\*\*) I recall that Saavedra worked with me for just a year or two before my departure (around 1968, 1969), after which I

lost sight of him almost entirely. His background at the time was no more or less extensive than that of any other 3<sup>rd</sup> cycle student from the Third World (or from one of our provincial faculties).



call them, too, tannakian. Clearly, this is a fundamental notion, used for years by the likes of Langlands, Deligne, Serre and others, and destined for a bright future. No one, of course, will believe that it was a certain Saavedra, quoted two or three times in passing in this article, who was the author of this crucial notion, and of the highly refined formalism to which it gives rise. The very tone of the article of the two brilliant authors, taking up the subject with all the mastery we know of the main author, doesn't leaves no room for doubt on this subject<sup>932</sup> (\*). Not to mention the fact that, in the theory presented in Saavedra's book such a gross error (which even forces them to start from a completely different definition, which finally seems the right one<sup>933</sup> (\*\*)) that one is justified in wondering whether this unfortunate Saavedra (to whom someone - and we can guess who. . . - had to try to explain what he was talking about) had really understood what he was talking about. And it's not Milne, brilliant as he is, and who had the honor of co-signing with the prestigious Deligne an article developing a visibly fundamental idea, who would have the idea that he could pass for father or only cofather of it; nor would Beilinson or Bernstein come and claim that they invented (or even co-invented. . . ) the famous "relationship that should have found its place in these notes. . . ." which they had the honor of co-signing with the same prestigious Deligne, after the latter had been kind enough to point them in the direction of a Kazhdan-Lusztig demonstration... . And who could **seriously** believe that this famous Tannaka, who lent his name (without being consulted) to designate this fundamental notion, **really had** anything to do with it? Nor would he be the one to come and claim, assuming he's still alive, the day when it will be clear to everyone who is the **real father** of this notion, and of the whole theory of perfect delicacy that goes with it. If anyone has the slightest doubt about this, all they have to do is go through Tannaka's work, or if his patience is too thin, the work on "Tannaka's duality", to realize that it has nothing to do with anything... ..

Here again, once a few milestones have been set, all we have to do is let time take its course. Clearly, this theory, which will increasingly reveal itself as the technical means of a new **philosophy** for linking geometry and arithmetic, is destined to come more and more to the forefront of the mathematical scene in the years to come. In five or even ten years' time, no one will have the slightest idea of referring to a certain book by an unknown author on this subject, when the person who undoubtedly held his hand took the trouble to write the paper that

was necessary, with the assistance of a brilliant collaborator, to form the heart of the no less brilliant volume where the notion of motif is finally developed on solid □ terrain. (Volume in which he appeared more charitably,

to make no mention of the usual conjectural "gangue of nonsense" on this theme, which obviously went beyond him, of a vague, precursory draft, long since forgotten. ...). ) It will become second nature to quote "Tannakian categories" by P. Deligne and J.S. Milne as one would quote FAC or GAGA (de Serre) or the SGA (the well-known anonymous seminar at IHES, known as "du Bois Marie"). And in so doing, there will be no ambiguity in anyone's mind as to the authorship of these innovative ideas - which certainly does not lie with co-author Milne, and even less with Tannaka, or even with a certain rigorously unknown author (a fellow by the name of Saavedra), named two or three times in passing in their article, for having written (in the introduction to a volume by his pen) an "excellent summary" (with a few reservations) on the subject.

But we wouldn't expect the father of the theory to do violence to his well-known modesty, to the point of calling "Deligne's categories" (or "Deligne's correspondence", in a completely different field. . . ) what, by all accounts and by the unanimous consensus of the "good" people who decide in these matters, should be "Deligne's categories".

<sup>932</sup>(\*) On the article in question, see in particular the notes "L'Enterrement - ou le Nouveau père" (n° 52, especially p. 214) and "La table rase" (n° 67, especially pp. 252-253).

<sup>933</sup>(\*\*) See, on the subject of this feat by Deligne (assisted by Milne acting as fi gurant), the beginning of the oft-quoted note "Monsieur Verdoux - or the cavalier servant" (page 1176).



yet it's really called that...

#### 18.5.9.4. d. The father waltz

**Note** 176<sub>4</sub> (April 20) Yesterday's reflection made me see with new eyes something that last year, when I had just landed in l'Enterrement, had left me flabbergasted: ". . this seemingly absurd thing: Deligne "redoing" Saavedra\*'s thesis ten years later! It all started on April 19 of last year, when I discovered the "memorable volume" LN 900, in which (among other beautiful things) Saavedra's thesis<sup>934</sup> (\*) is reproduced practically verbatim. I return to it a week later, in the note "La table rase". By this time, I had come to the "intimate conviction" that the meaning behind this nonsense was the desire of the brilliant Deligne (acting as Saavedra's scribe) to

"to give himself the illusory feeling of liberation from something he surely felt to be a painful obligation: to have to constantly refer to the very one he was trying to supplant and deny, or even to such and such another who referred to him."

But last week, taking the trouble for the first time to leaf through the work of this "so-and-so", I to my surprise that he did not  $\square$ songe at all to "refer to me" (except by the three lines (the "deep gratitude" I'd been quoted as saying, obviously intended to give the impression). All of a sudden, my "intimate conviction" of a year ago became lame - there must have been something in it, surely, but it was still a mystery: it's not the three lines in question, which no reader will ever think of unearthing at the end of the introduction, that will have motivated a Deligne to play copyist to the most obscure pupil of a master long dead! Not to mention the fact that, at the end of the introduction, I'm almost at one with him and Berthelot, who are entitled (as am I, we'd say<sup>935</sup> (\*)) to thanks for their "help and advice which they have generously given during this work". . .

p. 1171

This "mystery" became completely clear during yesterday's reflection, and without my having to look for it, and without my even having to think about it. Thinking about it again, after I'd stopped writing, various associations surfaced - they must have already been present when I wrote, without my even being aware of it, and guiding my pen without my knowing it. I was struck by a similarity not only of style, but of **patented process** of appropriation, across the three major "operations" in L'Enterrement (of the four in which Deligne himself is the principal (if not sole) "beneficiary"). We're talking here about what we might call the "provisional substitute father", surreptitiously introduced into the mathematical racket to conceal a real paternity, while the person of my friend pierre remains temporarily in the shadows. Once the natural father has been completely eliminated from the scene, to everyone's satisfaction, the substitute father is himself retracted as if he had never existed, and the **real father**, modest and smiling, appears on the scene, without even having to say that it's him; because for the one who has quietly known how to pull the strings and who has known how to wait, things take care of themselves without any resistance whatsoever: the unanimous agreement of the entire Congregation has already invested him with the role that rightfully belongs to him.

This process only began to dawn on me a few days ago, as I recounted the misfortunes of the past. tures of my friend Zoghman through the various episodes of Operation IV, the so-called "service unknown". The "surrogate father" in this case (for a certain "correspondence". . . ) was **Kashiwara** - I wouldn't know... say whether it fell out of the sky like that, provi $\square$ dentially and by the greatest of chance, or whether the future true p.

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father gently made him understand that this result of a stranger, who was hanging around without a father worthy of the name

<sup>934</sup>(\*) See the notes cited in the penultimate b. de p. note.

<sup>935</sup>(\*) With the difference, however, that I "introduced him to the subject" (sic), and that he "owes a large part of his mathematical training to me" (that's really too much of an honor).

name, was by no means to be sneezed at<sup>936</sup> (\*). In any case, our friend Pierre was able to play perfectly on a supposed ambiguity of paternity, fabricated from scratch by the peremptory consensus of "experts", even before the significance of the new thing was generally recognized. The surrogate father Kashiwara appears as early as March 1980<sup>937</sup> (\*\*), if not already at the Colloque des Houches six months earlier; he is retracted without trace (and without too much formality, it would seem) at the memorable Colloque of June 1981, fifteen months later. Here, the retraction is carried out with perfect dexterity, through the introduction of two others, this time let's call them "presumptive co-fathers" (and purely formal) Beilin-son and Bernstein, who enter the scene as a simple clause of style - "pouce!", when of course no one would imagine that it was either of them who would have made the child (even if both of them did benefit from it. . . ).

The analogy with "Operation Motifs" is truly striking! While the paternity of what could be presented as the "nonsense" of all things Motifs was still too notorious (especially in the early '70s) to be open to manoeuvring, there were **two crucial strands of Motifs yoga** that had never yet been the subject of a single published line, even if only in allusive form. One of these, the "yoga of the weights", had been appropriated by the Mega-father as early as 1970 without a hint of a wrinkle - what had been glossed over was in any case only "conjectural" and worth no more than a token allusion. The other part, on the other hand, had been perfectly worked out by the second half of the sixties, with nothing conjectural about it. A vague, slightly out-of-touch student was supposed to present at least the starting mechanism of yoga - not a technically arduous task, but one which (until around the moment of "death" of the natural and unwanted father, at least) seemed rather beyond the unfortunate.

□ It was this student, Saavedra, who was the ready-made surrogate father, credible enough, thanks to the p. 1173 to the provisional guarantee of the one who remains behind the scenes, to win the assent of a Congregation that is only too eager to forget the one who must be forgotten; but at the same time (and this is the point) this "father"-there obviously doesn't "fit the bill". When the time comes, it would never occur to anyone, and certainly to Saavedra less than to anyone else<sup>938</sup> (\*), to put forward the supposition that he could be the father of a new philosophy - a supposition that is quite simply preposterous, if you care to dwell on it for even a moment... Here, the evacuation of the surrogate father, who has had his day, doesn't take place until ten years later, with the publication of the memorable LN 900 in 1982. It has to be said that between 1972 (when the "surrogate father" came to the fore in Operation I, known as "The Motives") and 1980 (when the equally providential surrogate father appeared in Operation IV, known as "The Service Unknown"), a lot of water had passed under the bridge, and there was no longer any need to beat about the bush! Remarkably, here too, a "presumptive and token co-father" is introduced, to make the transition "smoothly" (and without anyone seeming to put themselves forward) between surrogate paternity (the paternity of a bungler, in short. . . ) and **real paternity**. And I'm sure Milne didn't see the invisible wires that maneuvered him at someone else's whim any more than Beilinson and Bernstein cared to see them. Everybody got their crumbs, and everybody (at least those with a say in the matter. . . ) got everything.

<sup>936</sup>(\*) (June 16) It would appear that the initiative for the pick-pocket operations on Mebkhou's work did indeed fall to the entrepreneur Kashiwara, and this as early as 1978, just a few months after Mebkhou had sent him Chapter III of his thesis, which he had just completed. See "La maffia" part (b) ("Premiers ennuis - ou les caïds d'outre-Pacifique"), b. de p. (\*) p. 1060.

<sup>937</sup>(\*\*) (June 16) In fact, it had already begun to show its face two years earlier - see the previous b. de p. note. The March 1980 episode is that of the Goulaouic-Schwartz seminar, referred to in the note quoted, as well as in the note "Carte blanche pour le pillage - ou les Hautes Oeuvres" (n° 1714 notably pages 1088-1090).

<sup>938</sup>(\*) (June 16) At the end of the "deal" that had to be made between him, Saavedra, and a Deligne (provisionally) in the wings (ready to reappear when the time was ripe. . . ), Saavedra's "share" was a state doctorate thesis in his pocket and the relative notoriety acquired by an author of the prestigious "Lecture Notes" series - which would give him the start he needed for a career in his own country, far from the arid mathematical pursuits he had only glimpsed from afar. ...

to be fully satisfied.

All this also made me think last night of the third major operation for the direct benefit of the "future father of all azimuths", the "Spread Cohomology" operation. I had previously convinced myself that the initial motivation for this operation<sup>939</sup> (\*\*) was the appropriation of a certain **fixed-point formula**,

of the fact that a certain "formula of *L-functions*" with undesirable paternity could be presented as a trivial co-roll of said formula. The trouble was that the trace formula in question was tainted by the **same p**.<sup>1174</sup>

unwanted fatherhood. Fortunately, there was also another possible father, a good friend of mine (Verdier, not to name him), who had even made two formulas, one too general (but heuristically crucial), the other a little narrow but still sufficient to "cap" what we wanted. But buddy or not, it's certainly not the buddy, nor the unwanted deceased, who is **the** appropriate "father" here, even though it's the key formula for "**the**" famous conjecture<sup>940</sup> (\*). Unfortunately, given the notoriety of the *L-function* formula and its unfortunate paternity, the delicate point here was not the friend (friends always work things out in the end. . .), but the deceased. To make matters worse, his demonstration of the "corollary" was published in black and white in a Bourbaki seminar in 1964, but at a time (fortunately) when the routine case (sorry, the crucial case, I meant!) of this formula (or of the formula of traces is kif kif, but it's better not to say it...<sup>941</sup>(\*\*)), had not yet had time to be verified.

Here, the manipulation consisted in using the friend in question to pretend to be the father of his ultra-general formula (which was the exact truth, except that he never bothered to demonstrate it...), but in the process confusing it with the **explicit** formula demonstrated by the cumbersome deceased (a formula to which no allusion is made at any point), and **debunking** the ultra-general formula (as conjectural, incomplete and, to put it bluntly, unusable). This was a way of drowning a fish, and of depriving the reader of any desire to go and look in a certain SGA 5 seminar (which, incidentally, he is made to "forget") for what he might have to say on the matter. As for the friend's explicit formula (a little narrow-minded around the edges, but perfectly valid), by mutual agreement there's no more mention of it either, except for an ambiguous and token reference, drowned at the end of a stringy and discouraging text, which no reader in the world will have had the courage to read to the end. In short, then, the

"surrogate father" (Verdier in this case) has indeed intervened, but less by his tacit agreement to a "paternity" on a result (that of **the**  $\square$  **défunct**) which it is a question here of **completely escamoter**, than by his connivance p. 1175 rather, in a game of scrambling and debunking two "children" of whom he is indeed the father, in order to conceal in the fray the third child, of an unacknowledged father, an orphan whom no one can find, nor, above all, cares to find<sup>942</sup> (\*). Illusie plays a supporting role, somewhat similar to that of the "co-presumptive fathers" from earlier - except that his paternity, like Verdier's, is never supposed to be based on the sacrosanct **Frobenius** formula of traces, the only one that counts and reserved (with all due dexterity, of course) for Deligne alone, but that it too relates to the unavowable child that needs to be concealed - something to which Illusie collaborates with that exemplary devotion that characterizes him.

<sup>939</sup>(\*\*) On this subject, see the group of notes entitled "La formule" (n° s 169 -169<sub>59</sub>). This initial aim was later considerably broadened - see in particular the notes "L'Eloge Funèbre (I) - ou les compliments" (n° 104) and the note "Les joyaux" (n° 170 (iii)).

<sup>940</sup>(\*) This is, of course, Weil's conjecture. On this subject, see the note "La Conjecture" (n° 169).<sup>4</sup>

<sup>941</sup>(\*\*) These two formulas are in fact each an immediate corollary of the other. As my authorship of one of them (the functions *L*) was notorious, Deligne managed (in the memorable text entitled "SGA 4<sup>I</sup>") to present it as the corollary of the other, doing his utmost to give the appearance of being the father of the latter, by means of prestidigitation-artifice tricks. infinitely more arduous, than my modest demonstration (and key statement) for the said formula. See the group of formulas already quoted, for this tour de force undoubtedly unique in the annals of our venerable Science (notes n° s 169 -169).<sup>59</sup>

<sup>942</sup>(\*) See on this subject the note "Les prestidigitateurs - ou la formule envolée" (n° 169<sub>8</sub>) - and also the b. de p. note (\*\*) page 1121 to the note "The family album, showing the extent to which the escamotage-enveloppe efforts of good Samaritans Deligne and

Illusie were a great success.

### 18.5.9.5. e. Monsieur Verdoux - or the cavalier servant

**Note** 176<sub>5</sub> But I'd like to come back to Saavedra's "thesis". It was around the time of my departure from the mathematical scene, at the beginning of 1970 (if my memory serves me correctly), that Saavedra had at last pretended to be really "hooked" on his work, after a year or two during which he hadn't seemed too decided. He then told me that he had worked out a formulation and a proof of the initial statement I had proposed to him, so as to apply to the case of a ring of base  $k$  **whatever**. He even gave me a sketch of a demonstration, which I had to listen to with a slightly distracted ear. Almost all my energy was taken up by the change in my life I was now experiencing. Without thinking to check carefully what Saavedra was telling me, I had the impression that he had finally started, and that he would now be able to manage on his own. Perhaps I was in a bit of a hurry to take his wishes for granted, at a time when my availability for real research direction had become almost nil<sup>943</sup> (\*\*). After that, I heard nothing from him, as far as I can remember<sup>944</sup> (\*\*\*). I

assumed until last week that he must have completed the minimum program I had proposed, and just a little beyond that perhaps in dealing with the case of the motives (from what Deligne had me written last August, with its annotated bibliography on the motifs).

I've only just realized **that this is not the case**. After three or four years spent on the subject, the unfortunate fellow has found a way of making a gross error in the very **definition of** what he calls a "Tannakian category" (the definition by intrinsic properties, I mean<sup>945</sup> (\*)), which he had to prove implies the "Galoisian" description in terms of representations of a suitable sheaf. Theorem 3, which he states in the introduction (the introduction in which he is supposed at least to **state** the four essential theorems of the theory, as I had given them to him), is therefore **trivially false**. Deligne and Milne make a point of pointing out the mundane error, proposing as a "new" definition of the categories studied the description in terms of sheaves (which it is obvious a priori is the right one, even if it means modifying the intrinsic description if necessary. . . ), and gravely question whether "Saavedra's" definition (once rid of the idiotic error) really implies "theirs" (sic)<sup>946</sup> (\*\*) - which was exactly the subject that was supposed to constitute Saavedra's thesis work!

The situation is pure Father Ubu! And in thirty-six ways at once. So, what was the subject of Saavedra's proposed work, the only part that required an original contribution, however modest (finding the right intrinsic conditions for a Galois-Poincaré category on a base ring as general as possible) has not been dealt with even in the case (which I think I had dealt with long ago<sup>947</sup> (\*\*\*) by the time I met Saavedra) where  $\square$  the base ring  $k = \text{End}(0)$  is a **body**! Saavedra's "thesis" work therefore consisted, very precisely, in piously copying the part of the theory (beyond the beginning of yoga

<sup>943</sup>(\*\*) In comparison, at least, with the availability I had before I left; but not with that which I can observe in most of my colleagues, who are in charge of research.

<sup>944</sup>(\*\*\*) My memory betrays me a tad here - see note n° 176<sub>7</sub> for unexpected revelations on this subject.

<sup>945</sup>(\*) The error stems from confusion in Saavedra's mind as to what I meant by the **basic ring** of a

It's not just any ring with respect to which the said category is "linear", and the tensor product is "bilinear", but the canonical ring  $\text{End}(1)$  (where 1 is the unit object of the category). By the time I explained the theory's ABCs to Saavedra, he must have been so "out of his depth" that it must have gone right over his head, and into oblivion. Deligne, who seems to have more or less taken over from Saavedra (obviously with an idea of his own in the back of his mind. . . ), was careful not to make him rectify the situation. This enabled him (ten years later) to discreetly bring down the Saavedrian house of cards, and to appear as the Angel Saviour and (this time again) the True Father everyone was waiting for... .

<sup>946</sup>(\*\*) Loc. cit. page 160 (I'm not making this up!).

<sup>947</sup>(\*\*\*) This was in 1964 or '65, so seven or eight years before Saavedra's famous "thesis"-sic, and seventeen or eighteen years before a Deligne-Milne tandem came to the rescue, not to **do** this modest work either - the only "original" work I had expected from the most modest of my students... ..

grothendieckien), on top of a basic body, which had already been entirely completed by myself, and to present, instead of the work that was a prerequisite for everything that was to follow, a cannulated definition and a "demonstration" of a false theorem, a demonstration reduced (as Deligne makes a point of pointing out - loc. cit. p. 160) to a simple vicious circle!

And that's not all. The thesis doesn't stand up - and the thesis jury doesn't even notice! I guess none of them really understood what it was all about. Yet that didn't encourage any of them to let me know that at least one of them was in a position to guarantee the seriousness of the work they were seriously pretending to judge<sup>948</sup> (\*). If the defense did take place, and without my involvement, it could only have been thanks to the support of Deligne, who (as Saavedra's acknowledgements make clear) must have followed his work to some extent, once I had practically disappeared from the scene<sup>949</sup> (\*\*).

It seems unimaginable to me, then, that Deligne wouldn't have noticed this error, he whose sharpness and acuity I know down to the smallest detail - and there's no question of "small detail" here! Of course, I'd told him all about the yoga I'd arrived at, and it's simply not possible that among the very first things I explained to him, there wasn't this counter-example that he and Milne pretend to bring out as the latest novelty, and which was known to me from the very beginnings of my thinking on yoga (which I'll finally call "Grothendieckian", instead of referring to Galois- Poincaré, who don't ask for so much. ...). If he has allowed such a gross error to persist in the "thesis" (sic) of his "protégé" (resic), so as to be able to purely and simply discredit the "surrogate father" (all provisional) as soon as he sees fit, it's surely not without good reason. Yesterday's reflection makes these quite obvious.

□ We may say that I'm affabulating, and that the "help and advice" Saavedra refers to, does not imply p. 1178 necessarily that Deligne had taken the trouble to read with any care the four statements in the introduction that summarize the essentials of the<sup>950</sup> theory (\*). Of course, these statements had been familiar to him long before he met the person in question, so it would have been sheer levity to endorse a work without having at least taken the trouble to check, for the space of a quarter of an hour, that the main statements announced in the introduction were correct. But in fact, there's no doubt in my mind that Deligne did indeed take the trouble. After all, this wasn't **just any work**, submitted by a slightly clueless student in need of a thesis. After me (and even before Serre), Deligne was in the best position to appreciate the full scope of the formalism presented, as a crucial part of the unwritten (or at least unpublished) legacy left by the late master. And while he may have been more than happy to take his usual casual approach to the subject<sup>951</sup> (\*\*), in the end he knew better than anyone what he was talking about. If he, the brilliant Deligne, the elitist through and through, took the trouble to follow the work of someone who was clearly mediocre, it was certainly not for his own good and with the aim of helping him obtain what, according to the current consensus (and even more so, according to the criteria of exactingness pushed to their extreme degree, which he prides himself on professing) is a **bogus thesis**.

Once that word is uttered, we are immediately confronted with a strange contradiction. On the one hand, an error so

<sup>948</sup>(\*) The composition of this lamentable jury will be revealed (to the reader who has resisted until then) in the final note 176<sub>7</sub> of "The Sixth Nail" to my coffin. ...

<sup>949</sup>(\*\*) This sudden interest on the part of a Deligne in an obscure student in need of a thesis only appeared, one wonders why, after the death of the natural (and unwanted. . . ) father of the theory that the aforementioned student (obviously overwhelmed by the task. . . ) was supposed to expound.

<sup>950</sup>(\*) Apart from the results on filtrations of fiber functors, which are more technical and harder to compress into a single, striking statement.

<sup>951</sup>(\*\*) On the subject of these tunes, and the appropriation technique they serve, see the note "Appropriation et mépris" (n° 59').

it's hard not to interpret it as a sign of fundamental incapacity - it would seem that the very problem that was being posed, even in its merely technical aspect (which wasn't all that rocket science, though), simply hadn't been grasped at the time of the defence, and at the time of publication of the book in question. On the other hand, this same student, after spending a year or two with me without doing much of anything, suddenly acquires, in less

of two years, a mathematical culture that may rightly seem impressive: structural theory of algebraic groups, both on general bodies and on the  $\mathbb{C}$ -corps of reals, theory of stranded schemes of

p. 1179

zinc, Hodge theory, motifs... . Not only that - but while I can't recall ever having read a mathematical text written in his hand, even just a few pages long, and knowing full well how difficult it is (especially for students of modest means) to learn how to write maths - I was struck, while browsing through the book published under his name, by its exceptionally high quality "dress". The thought had occurred to me that, technically speaking at least, this text, which obviously aims to be a standard reference text on a par with the EGA and SGA texts, could have been written by me, or by Deligne, or by one of the four or five other students I've had, all remarkably gifted, who are well versed in the task of presenting a complex, interwoven set of ideas and facts in precise, complete and elegant form. I am well aware that, even less than a mathematical culture, such writing virtuosity is not something that can be improvised (except in the case of exceptionally gifted individuals, such as this same Deligne and a few others), and that it can only be acquired (if at all) after many years of practice. It took me over ten years to acquire it, even though I had a very strong contact with the substance I was trying to express. Certainly, this contact was in no way comparable to Saavedra's for his thesis topic, which he still didn't understand after writing on it, and which turned out to be (at least until 1982. . . ) **the** "right reference" for a delicate and crucial formalism. Decidedly, there are two things here that simply don't "fit" with each other... .

The thought that occurred to me last night, and which now comes back with the force of evidence, once I take the trouble to recount the situation to myself in black and white, is this: it's unthinkable that it's Saavedra, whom I've known well and whose possibilities and, above all, limitations I'm well aware of - it's unthinkable, come to think of it, that he's the author of this brilliant book, setting out - in its exclusively technical aspect, it's true, but in an exhaustive and (in this respect) all-encompassing way, the foundations of a "philosophy" that was entirely beyond him, perhaps the first three chapters, two of which consist mainly of basic generalities that everyone already knew, and the third of which presents Saavedra's completely canned version of the book's central notion - these chapters, then, which were

supposed to constitute the "minimum program" he never completed - perhaps these are entirely Saavedra's own handiwork. Central Chapter III may be cannular, but it nevertheless suffices to deliver an idea of what what we were getting at - namely, the "Grothendieckian" (not to name him), or "Gerbian", vision of certain

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⊗-categories, a vision that gives meaning to later chapters IV to VI. Once we have accepted the description by sheaves (wisely taken as a **definition** of the so-called "Tannakian" categories, in the Deligne and Milne's doubly pirated text), it's these last three chapters that constitute the heart of the formalism I was trying to appropriate. I presume that these chapters were written in toto by Deligne, or perhaps partly by him, partly by Berthelot; and in even greater detail than the notes I had passed on to Saavedra, so that he practically only had to copy them verbatim, if he was even asked to go to the trouble of this formality. He must have felt like a "winner", because he was being given the "gift" of a thesis and the title to go with it, whereas he must have felt that what he had done himself (and even under the illusion that it made sense) was probably a bit meagre for a thesis.



state doctorate. And Deligne (disguised as a Samaritan again. . . ) wins: here was the reference that was needed, if not for now at least for "later" (for those who know how to wait. . . ), and where the undesirable name no longer appeared, for all practical purposes at least.

To add to the joy, I'd like to add that the man known as Saavedra seems to have disappeared from circulation without a trace. Last year, in anticipation of the (imminent) mailing of the printed and paperback copies of *Récoltes et Semailles*, I leafed through the *Annuaire International des Mathématiciens*, which is a big one - everyone's listed (and that's what the directory's for), with the sole exception of the person concerned, who doesn't appear under Saavedra or Rivano (or even Neantro, which I looked at out of conscience). As a result, the story takes on the trappings of a dark detective story. One shudders to imagine the smiling, affable Deligne, like a second Monsieur Verdoux (alias Landru), once he'd achieved his torturous ends with this "good reference" of his own (four years before his friend Verdier's!<sup>952</sup> (\*) ) - one shudders, I say, to see him make the "evidence" of his diabolical plot disappear, namely the unfortunate Neantro Saavedra Rivano himself, by burning him at length in a coquettish fireplace from the Ormails<sup>953</sup> (\*\*), specially designed for such purposes.

□ I reassured myself that I hadn't heard that Kashiwara or Verdier had disappeared from this world - in fact, I had him on the phone as recently as the day before yesterday, to ask him (without much conviction and without success, it seems to me) if he could give me some news about another "disappeared", that everyone's talking about and that nobody seems to have ever seen - I mean, Jouanolou's thesis. I still don't know much about this thesis, but it would seem that Verdier is still alive and kicking, whatever "evidence" he may be - and I'm confident that the same is true of Neantro Saavedra Rivano.

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### 18.5.9.6. f. Lowly jobs

**Note** 176<sub>6</sub> With all that, I haven't even finished circling the ubu aspects of the history of Saavedra's thesis - I'm definitely collecting them, theses and theses like no other! By then, I'd come to the presumption (not to say, the intimate conviction) that if Deligne (assisted by an eager, volunteer collaborator) pretended to make a serious copy of Saavedra's thesis ten years after it had been defended, he was undoubtedly only "taking back" what he had "lent" him for a time (the time it took Saavedra to pass his thesis and disappear), and that this was therefore only fair return - except that what he had "lent" for a time, he had "borrowed" from the unnamed deceased. But since it's not customary to return to the deceased what one borrows from them (that's all we need!), all's for the best in that respect too.

The best part was that even after a second ex-student came along (the brightest of all those I had, to boot), the humble problem I had given Saavedra, who had been my starting point more than twenty years ago, and the first thing I believe I solved from that moment on, in the case where the defining ring of the  $\otimes$ -category under consideration is a body - this humble problem is still only

<sup>952</sup>(\*) On this subject, see the note entitled (appropriately enough) "Les bonnes références", n° 82.

<sup>953</sup>(\*\*) "Les Ormails" is the name of the residential part of the IHES (Institut des Hautes Etudes Scientifiques), where our friend pierre - alias Monsieur Verdoux - alias Landru (and disguised as the cavalier servant) - has taken over at just the right moment from a certain deceased, ousted from the place and sent to oblivion by the kind of coup-mine-de-rien my friend has a secret for. The residential part consists of a dozen family pavilions, and a larger building of comfortable studio flats, each of which will soon have its own individual, all-purpose fireplace... .

not yet "solved", even in this case! Deligne was content to point out Saavedra's gross error (surely spotted more than ten years ago, but he was biding his time... ). He didn't care, though.

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by copying on 128 pages the previous reference text, to repair this error. Why would he give himself This would have required something more than a simple craving for appropriation. For this to have happened, he would have had to have had **more** than just a craving for appropriation, but a keen interest, a **respect** for the mathematical substance he was dealing with, and a vision that went beyond the prospect of immediate "gain".

If I took the trouble, around the years 64-65, to derive a "Grothendieckian" yoga for the  $\otimes$ -categories representable in terms of "algebraic sheaves", instead of contenting myself with those that can be described by a scheme in groups, it's because in the example that "motivated" me most, that of patterns over a body, it was well known (by a very simple Serre argument) that when this body is of char.  $p > 0$ , there is no fiber functor "rational over  $\mathbb{Q}$ " (or even over  $\mathbb{R}$ ). This **forced my hand**, then, to express the theory in terms of something as "unserious" as the formalism of sheaves and links, and at the same time, of course, to find intrinsic criteria of a simple algebraic nature, ensuring that this "Galoisian" or "Grothendieckian" vision practically "always" worked, and in any case, at very little cost. Twenty years later, the characterization I had derived (and, if I'm not mistaken, proved), by the existence of a fiber functor on an extension of the  $k$ -field of the base  $k$ -field, has still not been established in the literature! Even today, in terms of what is written by the care of the Saavedras, the Delignes and consorts, even admitting whatever one wants about a formalism of "motivic cohomology classes" over a finite (let's say) body, it is still not established (not in the literature, at least) that the category of semi-simple (let's say) motives over such a body is "Grothendieckian" (or "Tannakian", as these gentlemen say). Here's 418 + 128 = 546 pages of text, from the pen of Saavedra (assisted by Deligne and Berthelot), then Deligne and Milne, and all this without even managing to extract what had been my starting point twenty years ago, convincing me that "motivic Galois groups" **did exist**.

Yes, why would a Deligne have bothered, when he'd long since forgotten the vision, the credit he was seeking had been earned anyway, and the bodies he was working on to make **his** theory of patterns (which has nothing to do especially with that of a certain deceased. . . ) are all bodies of zero characteristic - so that his famous so-called "Tannakian" categories are all "neutral" (or "Tannakian").

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"trivial"). In that case, there was certainly no need to make a big fuss about wreaths and the like, which from then on was nothing but window dressing. There was no point, except **to appropriate the letter of something whose soul and spirit have been forgotten**.

And I see that the epilogue to this breathtaking and lamentable story is that, just as with the B.A.BA of the vision of patterns buried fifteen years ago, it's the crusty one again, barely finished with the tour of the brilliant Burial and its prowess, who's going to do this little job that none of his pupils after his "death" have yet had the heart to do. After all, they've been far too busy being masters to have the time, even for a few days, to be **servants**<sup>954</sup> (\*).

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<sup>954</sup>(\*) I've been a little hasty here, pretending to lump all my students together with the brightest among them. I apologize in advance to any of them who don't feel flattered to find themselves in such brilliant company! In any case, I'm happy to remember Giraud, taking on the task (which fell to him unexpectedly) of reading Contou-Carrère's thesis, in an attitude of "service", that's for sure, towards Contou-Carrère and myself at least, and perhaps also towards the mathematical community; on this subject, see the last paragraph of the note "jésus et les douze apôtres" (n° 19, page 151).

### 18.5.9.7. g. Five theses for a massacre - or filial piety

**Note** 176<sub>7</sub> (June 19) It's now exactly two months since I set about writing the above notes (dated April 19 and 20), with the ready-made name "Le sixième clou (au cercueil)" (n° s 176<sub>1</sub> à 176<sub>6</sub> , not including this one, which is part of the batch). Zoghman Mebkhout had just brought me Saavedra's book the week before - and it only took a glance to realize what it was all about.

I must confess that this discovery was a thrill, scarcely less so than that of the "memorable volume" exhumation of the motifs (Lecture Notes n° 900), a year before to the day. To put it better, last year's emotion was rekindled, as it were, unexpectedly by the discovery of an "operation" intimately linked to this exhumation; an operation (it was obvious from the outset) that had prepared it, and of a comparable scale. I was then seized again, not to say suffocated, by the feeling of quiet impudence - **the same** impudence (this too was clear from the outset, by many unmistakable signs), attacking something intimately linked to me, something that no one else in the world had long carried and nurtured. . . It was so strong, to the point of anguish, that I myself was shaken... astonished.

□ The spontaneous reaction, and natural outlet, would have been to do as I did last year - to say my emotion while it was still fresh, and thus get to the heart of this new chapter in my burial by those who were once close to me. I held back, however<sup>955</sup> (\*), because I needed to be available for Mebkhout's visit, not to mention the fact that he had things to tell me which, even if they didn't affect me in such a neuralgic way, I felt were just as "neuralgic" for him, in any case, and just as significant for the Burial. What's more, I felt it was important to make a note of the things I'd just learned from him that I wasn't yet familiar with, while they were still fresh in my mind - whereas the ins and outs of this famous book-burial were unlikely to escape me, even if I only got around to it later. That's why, the day after my friend's departure, I set about (from April 15 to 18) recounting his misadventures, in the group of notes (n° s 171<sub>1</sub> to 171<sub>4</sub> ) that now form the end of the Apotheosis. p. 1184

So, before coming to the famous "Sixth Nail", I'd had time to compose myself. To tell the truth, looking back over the first few pages, I can't find any trace, in my sarcastic (and a tad distant) description of the new pot-aux-roses, of the emotion that had first assailed me, to the point of making me spend a sleepless night, at a time when I badly needed sleep. I actually felt it, yes, the "weight of a past"!

It was the tenth of June, three days after I'd put the famous "period" under Burial - which was now off to a flying start! Of course, I had no idea how much it had restarted - that there were still three hundred pages (give or take) left to write! By the time I'd finished with the sixth of the notes ("Les basses besognes") making up the "Sixième clou", I thought I'd come full circle, and so had the "Quatre opérations" - apart from about ten pages (for operations III and IV) to be retyped and footnoted. In a few days' time, I thought I'd be able to hand over the entire Burial III manuscript for typing.

However, in the days that followed (perhaps even the day after or the day after I thought I'd finished with the last "Clou") there was an unforeseen turn of events, which I'll come back to. Here again, my spontaneous reaction would have been to get on with it straight away. But I waited another two months

<sup>955</sup>(\*) I did write four or five pages in the heat of the moment, but there's hardly a trace of it left in the text I wrote nine days later, on April 19.

It's not that I didn't want to, of course. □ But there were more pressing things to do.

prepare for typing. Rereading the Four Operations from the beginning, it became clear that there was a great need to flesh out the details here and there - and the rest is history!

And so, today (barring any further unforeseen events - knock on wood!) is the day when I put the finishing touches to Burial, practically speaking I mean: the day when I write the very last pages, which are supposed to be part of my reflections on Burial, within Harvest and Sowing at least. After that, all that's left to do is to write this "Letter" that's supposed to take the place of a foreword to Harvest and Sowing - after which I'm thinking of taking a few days' well-deserved and much-needed rest... ..

A few days after writing the six preceding notes, I learned of the composition of Saavedra's thesis jury - the very jury I had showered with well-deserved sarcasm in the penultimate note "Monsieur Verdoux - ou le cavalier servant". The thesis was defended on February 25, 1972 at the Faculté des Sciences d'Orsay, before a jury made up of **J. Demazure** (rapporteur), **Castelle** and **A. Grothendieck**.

For a "coup de théâtre", it was a coup de théâtre! The coronation of Ubu! I found it hard to believe this information from an official source, even though I'd never had the slightest recollection of attending such a thesis defense. Monsieur Verdoux-Landru's story was getting even more complicated! I telephoned Demazure on the off-chance that he might remember sitting on a thesis jury with me for a man called Saavedra. Demazure didn't remember much either, but enough to assure me that the defense had indeed taken place (although he couldn't say when or how), and that we'd both been there, along with Castelle (whose name I couldn't even remember...). He didn't know much more than that, except that he'd been the thesis rapporteur. It was I who told him that the thesis, officially, would have consisted of a 25-page text (which must have made his job as rapporteur easier, I imagine). So it was he who was surprised. He promised to send me a copy of the thesis. I'd have been interested to know what it looked like, but I'm still waiting for it - apparently (according to what Demazure finally told me a few weeks later) this thesis is nowhere to be found; maybe he didn't make much of an effort either. In any case, he apparently has no trace of it in his papers any more than I do. But that's just a detail...

□ All of a sudden, I looked thin! With the hot sips I'd bought myself about this jury, obviously inane, "pretending docently to judge" a work he "must not have understood very well what it was all about"! You can imagine that I had a mad desire to repackage these sarcasms, to save the furniture in short, to keep a composure - but no, that would have been cheating. There's enough cheating as it is in this whole Funeral, without me putting any more effort into it. Once again, the sarcasm was entirely justified. Now that I know the composition of the jury, I can even say that it was I, above all others, who fully deserved the sarcasm. After all, what Demazure and Castelle must have remembered most of all was that Saavedra had prepared this thesis with me, or at least had started it with me, on a subject I had given him. I was supposed to be in on it, and they trusted me. Maybe those 25 pages Demazure was supposed to have reported on made sense - and even if the same monumental blunder was there, in a simple summary of a theory, Demazure, who wasn't in the loop and who trusted me, had no chance of noticing.

As for me, who'd practically given up maths two years earlier, apart from my courses, this defense, which I was probably rushing off to, between a course at Orsay and some meeting of Survivre et Vivre or some public discussion (if any) on atomic waste stored nearby (at Saclay), must have been nothing more or less than a simple administrative formality. One thing's for sure: I had no more

that I hadn't followed Saavedra's work for two years, any more than I had followed anyone else's - and that I had no doubt that Saavedra's work stood up to scrutiny. I can't say exactly where this conviction came from. Unlike all the other students I'd had up to that point, I had no direct presumption, through work already done with me, of Saavedra's seriousness. Would I have taken my academic duties in those days so lightly that I would have trusted him on his word, so to speak? If the text of the book (published the same year), of which the 25-page thesis no doubt constitutes a summary, was already ready at the time and served to give me an idea, it's true that "at a glance" it looked so good, that the idea may not even have occurred to me to check the part of the work that was supposed to constitute Saavedra's personal contribution. It's also possible and even probable (but I have no recollection of this) that I relied on the advice of Deligne, who after my departure had followed the work<sup>956</sup> (\*).

□ In either case, I have to admit that my responsibility is engaged to the same extent.

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title, for having awarded the title of Doctor of Science on the basis of a thesis which, twenty-three years later, appears to be a **bogus thesis**, to use the expression of the note already quoted. But the fact that I myself was unwittingly an instrument in this deception, and bear responsibility for having given my (light-hearted) guarantee, does not make it any less of a deception. It only makes it all the more brilliant. After all, the real motivation (for whoever was pulling the strings) was certainly not to allow a vague PhD student in distress to have a cheap title, before changing jobs and disappearing behind the scenes - but rather to allow someone who was in no way clueless to take ownership, delicately and under the radar, of a certain vision born in me and brought to fruition before he had even heard (in mathematics) words like "sheaf" or "pattern". It was thanks to my sudden and intense activity for the survival of the species and other fine and urgent causes (from which this same ex-student and friend had told me he had to distance himself, because of his complete and absolute dedication to mathematics alone<sup>957</sup> (\*)), at a time when my energy was fully absorbed elsewhere, that my brilliant student and friend succeeded in this truly unique sleight of hand, of making me the instrument of my own dispossession! In the state I was in at the time, completely disconnected from my former mathematical interests and placing blind trust in those among my students, led by Deligne, who since the end of the SGA 5 seminar had already begun to play a little game of their own, any name (for example) that someone had concocted for his famous categories, which I now only vaguely remembered, I would have said yes and amen! Just as I said yes and amen to Verdier's announcement that there would be no book on new-style homological algebra, or to Deligne's announcement that half of the SGA 7 seminar we'd done together was suddenly going to change authorship. . . But the fact that the person who pays for a scam gives his or her benign, unsuspecting consent does not change the nature of the scam, except that it is coupled with a breach of trust. And the fact that Serre and other augurs are also in it for the money, and give in their unreserved blessing<sup>958</sup> (\*\*), gives the thing a □inhabital dimension - that of the corruption of an entire without making it honorable, however brilliant it may be, or removing one iota of its indecency.

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As surprises never come alone, just a few days after the revelation of the composition of the jury for my ex-student Saavedra's thesis, I also received the relevant information for Jouanolou's thesis, which is also a bit special, and which I've had the opportunity to talk about so much.

<sup>956</sup>(\*) I don't even remember that Deligne was involved in Saavedra's work. This is something I learned in April, looking at the introduction to Saavedra's book.

<sup>957</sup>(\*) On this subject, see the note "Brothers and spouses - or the double signature" (n° 134), especially pages 614-615.

<sup>958</sup>(\*\*) For this most explicit blessing, see the note "L'album de famille", part d. ("l'Enterrement - ou la pente naturelle").

a copy of his famous thesis ("which everyone quotes (since the Colloque Pervers) and which no-one has ever seen"), so I ended up writing him a rather dry letter (dated April 25) to ask him a number of questions about the strange vicissitudes of this thesis. He replied practically by return" on May 1, evasively as regards questions of substance (as it was "always very painful to return to the past"), but with information that could not have been more precise as regards administrative details: the thesis was defended on July 3, 1969 at the IHES (Paris), before a jury presided over by **P. Samuel**, with **J. Dixmier**, **A. Grothendieck** and **J.L. Verdier** as examiners. My correspondent adds, with a touch of mischief: "As far as I could tell, all the members of the jury were present! (a fact also confirmed by J.L. Verdier, whom I spoke to on the phone shortly afterwards).

Here again, I had not the slightest recollection of this thesis defense, which had obviously also taken place on the sly (sorry to spoil my image in this way!)<sup>960</sup> (\*\*). If I thought that the defense had taken place in Strasbourg (and therefore placed it in the early seventies, knowing that Jouanolou had a post in Strasbourg in those years), it was probably because of a cryptic reference by J.L. Verdier to this thesis (in a Bourbaki presentation of February 1975, n° 464), quoted as "J.P..

Jouanolou Thesis, Fac. Sc. Strasbourg" (no date, no title). Yet, like me, he had been on the jury - would his memory be as faulty as mine, or rather, capricious, in placing the IHP (Institut Henri Poincaré) in Strasbourg? Comprenne qui pourra!

The same Verdier was kind enough to send me his own copy of the thesis. At first, I thought, looking at this packet of 208 loose sheets<sup>961</sup> (\*), that it was a photocopy of a draft, which I remembered holding in my hands and commenting on in detail, when Jouanolou was working with me on this thesis that was dragging on and on. But Verdier confirmed that this was indeed the definitive copy of the unfortunate thesis, which apparently never had the honor of being printed in more than three or four copies (mine, with my annotations, must have gone back into Jouanolou's hands, and I never saw it again... ), nor to be bound.

The slightly more detailed explanations Jouanolou was kind enough to give me later (in a letter dated June 3), plus the phone call to Verdier, enabled me to get back into the swing of things. Jouanolou had obviously reached a "saturation point" for his thesis work, which he had been pursuing half-heartedly from the start (but without my bothering to make myself clearly aware of the situation<sup>962</sup> (\*\*)). By 1969, he must have reached a point where he was unable to resume his work even a little, to take into account my numerous observations. So I had to face the facts and "let it go". In any case, when I looked at it again, it seemed to me that this text represented a serious and usable work of formatting, even if it was far from perfect - it was clearly better than "better than nothing", and could pass as providing an indispensable reference text, in the absence of any other that would have fully satisfied me<sup>963</sup> (\*\*\*)).

<sup>959</sup>(\*) This thesis was discussed in sub-note n° 85<sub>1</sub> (p. 349) to the note "Solidarity", and also in the note "Co-heirs. ... " (n° 91), p.387- 88. See also "The Student and the Program" (n° 25).

<sup>960</sup>(\*\*) The thesis defense took place at a time, I believe, when I had already "dropped out" of maths, to become interested in the biology (and more specifically, molecular biology).

<sup>961</sup>(\*) At the Service des Thèses de la Sorbonne, there's a registered thesis of 215 pages - apparently the Verdier copy is missing six pages. For all we know, the copy deposited with the Service is the only complete one in the world - and a stapled one at that, I'm assured. They must have a binding service for foundling theses, which arrive in pieces... ...

<sup>962</sup>(\*\*) See the aforementioned section "The student and the program", n° 25.

<sup>963</sup>(\*\*\*) In any case, this is still the only text in the world that presents the theory of *l-adic* coefficients, version derived categories - and an unobtainable text on top of that, to bring the joy to a climax. The chainsaw went through

Of course, the idea never occurred to me ("even in my dreams") that Jouanolou would take his revenge at his way, about the lack of conviction with which he had pursued this work with me, scuttling it himself and practically erasing all trace of that famous "reference" I was so keen to have! Here again, it's a "return of things" that I'd be wrong to complain about (although I don't lack the desire to do so!). In my relationship with Jouanolou, what counted for me was to find in him "arms" to push the wheels of a certain cart of imposing dimensions. I took it for granted that he, Jouanolou, was a stakeholder in my plans, without at any time stopping to consider the insistent signs that this was not the case. It's true, of course, that it was Jouanolou himself who had chosen to come and work with me (he must have enjoyed working with a prestigious "boss", without suspecting what he was getting himself into. . . ), and it was he too who had chosen to work with me. ), and it was Jouanolou himself who freely chose the subject of his work, from the wide range of subjects on which I was willing to work with him (all related, of course, to the same "cart", which no doubt, deep down, didn't mean anything to him). To put it another way: like everyone else, Jouanolou was grappling with certain contradictions within himself, in terms of his own desires and choices, in this case in his work. p. 1190

My own contradiction lay not in my relationship to my work, but in such a polarization on my tasks that I was unable to see in my students anything other than welcome arms, and to imagine that any of them could be divided in the work they did with me. With the additional hindsight afforded by long reflection on Burial, I realize that Jouanolou was far from the only one of my students to be "divided" in one way or another in this work. But he represents an extreme case, in that he is the only one among them who was unable to identify with the task he had chosen, and whose work was carried out without conviction or joy. My responsibility in this situation is not to have consented to really take note of it, preferring to put what should be accessory (the accomplishment of my tasks) **before** what is essential (that the task "chosen" by the pupil be truly **his** too, and pursued with joy).

That's why Jouanolou is probably the only one of my ex-students in whom I've ever perceived a grudge (which never says its name, of course). Cultivating such a grudge is an outlet and a diversion, which of course achieves nothing except to avoid one's own problems (and one rarely looks any further). That doesn't change the fact that it's well-founded, and that I have nothing to complain about if today (twenty years later) I reap the rewards of it. certain fruits.

□ To find myself confronted blow after blow, less than two months ago, with the unusual episodes of Saavedra's thesis, and then Jouanolou's, made it clear to me that, as I had just glimpsed in the first part of Récoltes et Semailles, even before I left and in the years immediately following, all was not well (as I took it for granted!) between me and my students. Thus, of the twelve theses that were passed by the students who worked with me at the level of a state doctorate thesis, **four** of these theses constitute, blatantly, "theses of Burial" of the master! They follow each other over a period of five years, between 1967 and 1972, and two of these burial theses take place before I leave. The first was Verdier's in 1967, a thesis reduced to a 28-page summary, a prelude to the burial of the new homological algebra I had introduced, and which Verdier had taken on the task of developing. This has already been discussed in detail at<sup>964</sup> (\*), so there's no need to go over it again. The second is that of Jouanolou in 1969, which consecrates the burial of the *l-adic* cohomology formalism, from the point of view (obviously crucial for the six operations) of the p. 1191

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there...

<sup>964</sup>(\*) On this subject, see in particular the notes "Thèse à crédit et assurance tous risques" and "Gloire à gogo - ou l'ambiguïté" (n°s 81, 170 (ii)).

is that of Deligne in 1970 (?), a brilliant thesis that was also deeply rooted in the ideas that he took after me<sup>965</sup> (\*\*), without my name even being mentioned! The fourth is Saavedra's thesis, which has just been discussed at length, in which someone other than the presumed author<sup>966</sup> (\*\*\*) sets out, with maestria technique, the ideas and results of a third on the motivic Galois group (via a complete theory of the so-called "Tannakian" categories, and four of them!) without alluding to my modest, late self!

These four burial operations (which prelude the capitalized "Four Operations"! ) are visibly linked, and in many ways<sup>967</sup> (\*). They follow each other in the space of less than five years, beginning the very year after the end of the SGA 5 seminar. This seminar seems to have been the starting point and rallying point for the fossilizing dispositions in my ex-students, long before I left! That they predate my departure is a remarkable circumstance, concerning this "second plane" of the Burial formed by all my ex-students "from before" - a circumstance that I haven't yet really been able to integrate into an overall understanding. It's this "second plan" which, at the moment, seems to me to be the least well understood of the three. But now's not the time to start thinking about it again, surely the coming months will bring me many new elements, from my ex-students themselves. Then it will be time to assemble them into a living picture of the "second plan".

There's a fifth thesis<sup>968</sup> (\*\*) which, for me, fits into the series of theses-Enterrement, but a thesis "after", and even ten years after the previous series. It's Contou-Carrère's, written in December 1982, and special in more ways than one. It differs from the previous four in that Contou-Carrère's valiant grave-digging efforts, to please the people who count and to make a name for himself, are not enough.

forgive him for having been more or less my pupil, did not spare him from Verdier (whom he had wisely chosen as his thesis supervisor<sup>969</sup> (\*\*\*)) unexpectedly pretending to "sink" him □ without warning.

Whereupon, for want of anything better, he fell back on me again. It wasn't necessary for me to act as thesis director, given that Contou-Carrère had found his theme and developed his methods on his own.

<sup>965</sup>(\*\*) This is Deligne's "Hodge Theory II" work. I give details of the roots of this work in pattern yoga and in my vision of "coefficient theories" (including a theory of "Hodge coefficients"), in the note "Les dots on the i's" (n° 164), in particular pages 739-740, as well as sub-note n° 164<sub>1</sub> (pp. 805-806). Like M. Raynaud and C. Contou-Carrère, Deligne chose the themes of his work and, in particular, that of his thesis, without waiting for me to suggest one.

one, and pursued this work entirely independently, without even talking to me about it until it was virtually complete. Nevertheless, his work (on mixed Hodge structures) is rooted more deeply in my ideas than is the case with Raynaud and Contou-Carrère, who mostly use the language and techniques I brought to the table, whereas the problematic pursued by both is entirely original.

It's true that (depending on how the wind blows these days) ideas can blow away, especially if they're not published out of hand (as Serre peremptorily explained to me just a few days ago). ...

<sup>966</sup>(\*\*\*) This, at least, is the conviction I arrived at in my penultimate note "Monsieur Verdoux - ou le cavalier servant" (n° 176 ).<sub>5</sub>

<sup>967</sup>(\*) It would be interesting, of course, to probe these links further - but as I say a few lines further on, it's not now is the time.

<sup>968</sup>(\*\*) Out of a total of fourteen theses, written by the fourteen students (both "before" and "after") who worked with me on a state doctorate thesis. That's more than **one out of every three theses** that I've worked on - which isn't bad at all!

<sup>969</sup>(\*\*\*) At a time, moreover, when I still believed (according to what Contou-Carrère himself assured me) that I was his official thesis director. I only learned of the existence of a "parallel" thesis director (in a pair where it was rather I who was to act as "back-up" thesis director, just in case... ) until Contou-Carrère was forced to fall back on me, and at the same time (given the situation, which had become a bit too, well, shitty) to reveal Verdier's role to me. It's not surprising that, with such unbelievable shenanigans taking place over the years, Contou-Carrère ended up practically ceasing to do maths any more. It has to be said that he's not the only one...



means, and that I hadn't been following his work, which was set in a context (that of reductive group schemes) that I'd somewhat lost sight of. Nevertheless, the starting point of his work, namely a certain method of resolving "equivariant" singularities, for the adhesions of Schubert cycles, was directly inspired by an idea I had explained to him in detail (around 1975 or '76), concerning a resolution of the canonical and simultaneous singularities of the adhesions of orbits, for the adjoint representation of a reductive group on itself<sup>970</sup> (\*). Needless to say, Contou-Carrère, who has long since sensed how the wind blows in the beautiful world to which he has the legitimate desire to accede, doesn't breathe a word about this filiation. Where would we go if we again began to mention such imponderables that one **idea** (and not yet published), supposedly **sparking** another (or asking you for a bit . . .) - except, of course, when the person we're proud to mention is one of those whose name enhances the brilliance of the work presented (in which case, moreover, it's entirely superfluous to specify why we're lavishing thanks on him, which can then only be well-founded... .).

## END OF THE "FOUR OPERATIONS (ON A BODY)" SECTION

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<sup>970</sup>(\*) Towards the end of the sixties, I was intrigued by Brieskorn's fine work on so-called "rational" (surface) singularities, and their links to certain systems of simple roots (those where the roots are all of the same length), and I asked myself the (nonsensical, needless to say) question of finding a direct description of a rational singularity, in terms of the simple algebraic group corresponding to its root diagram. That's how I came up with a very simple (even obvious, to say the least) geometrical description of the resolution of the singularities in question, using Killing couples, with a whole set of conjectures that I've since forgotten, and that I told anyone who would listen. But since I haven't published anything, and according to the new axioms that Serre has just kindly explained to me, it's the first one to pick up the prize that gets awarded - and I've noticed that there are some who pick up a lot like that, of course. It's handy sometimes to change axioms...

## 18.6. Desolate building sites

### 18.6.1. (1) What remains to be done

□

p. 1194

**Note** 176 (March 25) Last night, I spent several hours in bed getting back into the swing of "yoga". patterns", instead of falling asleep quietly as I should. And sometimes, instead of going back to my notes, I spent another hour or two scribbling out implication diagrams for the intrinsic conditions known to me on a class of De Rham cohomology (of a nonsingular projective variety over a zero-char. body, say) for it to be "algebraic". I found **twelve** variants, in all, of the Hodge and Tate conjectures<sup>971</sup> (\*). At the same time, I was able to convince myself that we must have more or less what it takes to define "the" (triangulated) category of patterns on a finite-type scheme over  $\mathbb{Z}$ , or at least a very tight approximation to it (assuming it isn't "the" one yet), provided we have a theory of the "mysterious functor", which I had postulated towards the end of the sixties<sup>972</sup> (\*\*).

This is not the place to dwell on the subject, of course. But now is as good a time as any, given the lamentable state of neglect in which I see the motivic theme fifteen years after leaving it in dubious hands, to trace some of the main lines of the ideas I arrived at a short while ago. I don't have the

coeur d'attendre encore, le temps de trouver le loisir (une fois achevé "A la Poursuite des Champs") d'écrire "le" livre systématique qu'il faudrait écrire; ce récit circonstancié d'un rêve, comme premier grand pas pour that the **dream** may take root, at last! in the soil of carefully matured (and published. . . ) formulations, and blossom according to its own nature. In addition to a first milestone already planned and announced for this book of "mathematical fiction", namely a sketch of the algebraic formalism of duality known as the "six operations", I shall therefore be attaching to volume 3 of *Réflexions*<sup>973</sup> (\*) a short work in which I intend to pose some crucial questions linked to algebraic motives and cycles. It was painful to see them languish in a tomb, and I can't wait to see them return to the light of day and participate once again in the rhythm of the seasons... ..

p. 1195

It's been more than five weeks since my thoughts returned to L'Enterrement, without leaving it. This is no doubt why the thought of "orphans", left to fend for themselves in a sick world, has come back to me of late with some insistence. The last note in which one of these orphans is mentioned in detail is "La mélodie au tombeau - ou la suffisance" (n° 167), on a theme very close to that of last night's motivic reflection and that of earlier (which I have just mentioned). That was a month ago to the day

<sup>971</sup>(\*) (March 27) Each of these twelve variants should give rise, for any basic null-characteristic scheme  $X$ , to a "category of coefficients" of a corresponding type on  $X$  (where the notion of "type of coefficients" is that discussed in the note "La mélodie au tombeau - ou la suffisance", n° 167). If the conjecture under consideration is true, this category of coefficients should contain that of the patterns on  $X$  as a full (triangulated) subcategory (the conjecture being none other than that the same assertion, in the particular case where  $X$  is the spectrum of a body. . . ). For further details, please refer to the section of Volume 3 of *Reflections* devoted to the theory of motifs ("Les motifs mes amours").

In other words, these twelve variations on well-known conjectures give rise to as many different notions (a priori at least) of a notion of "pattern" on a body of zero characteristic. In future, this will enable eleven of my friend Pierre's emulators to "discover" their own notion of pattern, while pretending to ignore those of the others and, above all (as has been de rigueur for the last fifteen years. . . ) a certain deceased (known above all for his predilection for useless details. . . ).

<sup>972</sup>(\*\*) This question of the "mysterious functor", establishing the "missing link" between crystalline cohomology in char.  $p$  (via the notion of  $F$ -cristallin,  $F$  as "Frobenius"), and  $p$ -adic cohomology in char. null, a question obviously crucial to our understanding of the cohomology of algebraic varieties, has still not been seriously addressed, almost twenty years after I raised it in the clearest terms. ...

<sup>973</sup>(\*) As my publication plans currently stand, the first four parts of *Récoltes et Semailles* (ending with the third and final part of L'Enterrement) are to form volumes 1 and 2 of *Réflexions*. Volume 3 will consist of the fifth part of R et S (reading notes on C.G. Jung's autobiography) and a number of shorter texts, most of which were announced in the Introduction. The first volume of "A la Poursuite des Champs" is thus planned as the fourth volume of *Reflections*.

day, on the eve of the day when I was going to launch myself (without yet suspecting what was in store for me!) into a note that would be called (it had already been decided in advance) "The four operations". In the end, it turned out to be sixteen notes instead of one. I thought I'd never get round to it - but I did, and in the end, I did get round to it, all those long-winded "operations"<sup>974</sup> (\*\*)!

And right now, I'd like to get back to these orphans, to at least call each of them by name, because it might do them some good, and me some good. The first time I mentioned them was a year ago, in the note of that very name, "My orphans", from the end of March last year, in one breath with the note that follows it "Refus d'un héritage - ou le prix d'une contradiction" (notes n°s 46,47). In writing these notes and when I gave them these names, as if guided by an obscure prescience, I had no idea of the extent to which the how these things I had left behind had been orphans indeed - □ in a stronger, more poignant sense p. 1196 I couldn't have imagined it, even in a dream; nor how far this "contradiction" went, of which I was then making a first, timid observation. And this memory immediately reminds me of another, from the month before, when I saw myself writing, as if it were another, more penetrating than myself, writing through my hand: "**you can't fight corruption**". It was while writing the section "The world without love" (n° 19). I still remember, seeing that word "**corruption**" in black and white, I was taken aback at first. Someone "reasonable" inside me was scolding me: really, you're not going to mince your words - "corruption" is a big word, don't kid yourself! You'd better change your tune!

I had to probe myself for a few moments, maybe minutes. Then I knew I wasn't going to change that "big" word, nor was I going to add a note to explain that the word had escaped me in the rush of the pen, and shouldn't be taken too seriously. Someone deep inside me, more perceptive than the "me" who decides on "reasonable" labels, knew what these "whiffs" of this world that had come back to me here and there meant, even before I'd bothered to try and recount them to myself<sup>975</sup> (\*). . .

I also remember well the precise moment when the reflection of that day suddenly changed quality, when that **other** in me took over to write. It was just after I had recalled the warmth of affection that had surrounded my first years in the mathematical round, thanks to the welcome I had received from my elders, and even from their families: the Schwartzes, the Dieudonné, the Godement... . The change comes when I follow up with "Obviously, for many young mathematicians today, it's being cut off... .. from any current of affection, of warmth... .. that clips the wings of work and deprives it of a deeper meaning than that of a dull and uncertain livelihood. . . "and when, at the same time, this **loveless world** suddenly appeared and came to life before my eyes, once again calling out to me...

Without having to look for it, last year the name "my orphans" came to me, for what I had left behind when I left (declared "dead" by the relatives I had entrusted them to... ). It's undoubtedly that this name □ expressed a simple, tangible **reality**: what I had "left" or "entrusted" were not p. 1197 They were not "objects" or "property", but **living things**. When I think of them, I always think of them as living things, vigorous and fertile, made to grow, to flourish and to conceive and beget other living things, vigorous and fertile. If I have a feeling of "wealth" that I've left behind, it's not the banker's wealth, but that of the gardener, or that of the bricklayer, whose hands have created these exuberant gardens and spacious, welcoming homes. This feeling of something precious (or even fragile) links me above all to the **notions, questions and major themes** I'm familiar with

<sup>974</sup>(\*\*) (May 9) Barely two weeks after writing these lines, new facts have emerged in extremis, re-launching the "four operations" investigation, which has already been augmented by a good twenty new notes and sub-notes.

<sup>975</sup>(\*) I report on this, first in March last year in the section "The note - or the new ethic" (n° 33), then two months later, after the discovery of the Burial, in the much more detailed set of notes forming the Cortège X or "Funeral Van" (with the Gravedigger), notes 93-97.

that I had left in younger hands - those things that still need work and care; far more than the finely honed tools I had fashioned, or the "houses" I had finished building and furnishing<sup>976</sup> (\*). Others will be busy cooking and lounging in them; if one turns out to be too small, they'll enlarge it according to their needs, just as I myself have often had to enlarge and enlarge again, where it had once seemed that I was "thinking big". But it's through **what's left unfinished**, through the building sites that have just begun on these splendid sites and with these beautiful stones (and already the workmen have left, having taken away what they liked and damaged the rest. . . .) - it's through this that we'll be able to see what's left. ) - this is where my mathematical past continues to have a hold on me. It's these derelict **building sites**, which I now find looted and dilapidated, that I'd like to review.

## 18.6.2. (2) The miser and the crumbler

**Note** 177 (March 27) Yesterday was taken up with housekeeping. I had to reread the first fifty pages of the third and final part of *L'Enterrement*, and entrust them to the typesetter. It took me no less than five hours, with a few minor tweaks of expression here and there, and a few more additions.

footnotes. The typing of "La clef du yin et du yang" is about to be completed. After all the trouble I went through typing that part<sup>977</sup> (\*\*), I ended up relying on the services of one of my friends.

p. 1198

secretary, who does the work outside her official job. The trouble is over, thank God - she does a conscientious and efficient job, about thirty impec pages a week. We'll get there in the end. It's about time!

Apart from that, the question of a shaped construction of the triangulated category of patterns on a finite-type scheme on the absolute basis  $E$  kept running through my head - I still spent most of the night thinking about it in bed, instead of sleeping - watch out! At first, it seemed that the idea I had would only work for schemes of zero characteristic (of finite type on the body  $Q$ , let's say), already on the  $Spec(Z)$  basis itself it didn't seem to work, then I remembered that I had determined in principle the structure of the category of patterns on a finite body, back in the sixties. Assuming that the work I'd done back then had been clarified, I finally saw the principle of at least a complete description in the general case, quite screwed up it must be said, but by no means unapproachable it seems to me. The only new ingredient compared to my ideas of the sixties is Mebkhout's philosophy, expressed in his "God's theorem" of strange memory. Apart from that, I use the "mysterious functor" theory as a hypothetical ingredient. If it's not available right now, it's certainly not because it's "unaffordable" (to use an expression I've already encountered<sup>978</sup> (\*)), but because the people I've known to work on the cohomology of algebraic varieties have lost, even in maths, the sense of essential things, too absorbed certainly by a funeral that requires all their care... ..

To be fair, Deligne's work on Weil's conjectures, in "Weil I" and especially "père II", will surely come in handy when it comes to constructing the six operations on the categories of coefficients that are supposed to express the motifs. However, after fifteen years, a "confused" and crumbling deceased had to come up with the idea of stepping out of the padded coffin in which his dear pupils and heirs had been lying

<sup>976</sup>(\*) On the subject of the impulse within me to "build houses" (mathematics), see the note "Yin the servant, and the new masters" (n° 135).

<sup>977</sup>(\*\*) On the subject of these "troubles" (to put it euphemistically), see the beginning of the note "Prayer and conflict" (n° 161), as well as those in

of the note "Jung - or the cycle of 'evil' and 'good'", which opens the fifth and final section, Harvest and Sowing.

<sup>978</sup>(\*) This is the peremptory term with which my brilliant ex-student Deligne liked to bury the "standard conjectures" - which none of my bold contemporaries dared to tackle for almost twenty years. For a complete quotation, see the note "L'Eloge Funèbre (1) - or the compliments (n° 104).

more to assign him, who knows nothing and has forgotten as much as to say the little he knew, so that the problem of describing the category of patterns above a basic schema  $S$  can only be **posed**. in full, and at the same time□as if by chance, that the principle at least of a p . 1199 construction form (which takes into account all known structural elements associated with a pattern) is finally clearly explained<sup>979</sup> (\*).

After the "memorable volume" of 1982 on motives, it would seem that the "motive hoard", which for ten or twelve years had been the reserved and secret domain of one, has become a common hoard for three or four, who communicate with each other with the air of conspirators, or like Grand Initiates of some secret and ultra-selective sect. However, it only takes a few days to ask a few simple questions in black and white and bring them to everyone's attention, and a few weeks if you want to define them with some care, clearly indicating which ingredients you have, and which others need to be developed. If in the fifteen years since 1970, and in the three years since the "memorable volume", neither one of them, first, nor any of the few afterwards, has wanted to take these few days of their admittedly precious time, let alone weeks, it's surely for excellent reasons, which none of them has cared to fathom. But this atmosphere that they like to maintain, and this spirit in which they keep themselves, are in themselves already a degradation of an adventure of discovery, which has become a simple means to elevate themselves above others, if not to despise them. Such an atmosphere is likely to spread corruption, and is the antithesis of creation, even though those who indulge in it would be the most brilliant of geniuses. By maintaining such an attitude - that of the avaricious brooding over his treasures - they cut themselves off from the creative force within themselves, just as they like to stifle it in others.

### 18.6.3. (3) Site tour - or tools and vision

**Note** 178 ( March 30) The day before yesterday was my fifty-seventh birthday, and I took a bit of a break. I made just a few typing corrections for the end of "The Key to Yin and Yang", which I continued yesterday. It's relaxing and pleasant work - provided, at least, that the person doing the typing also puts in some effort, and that a text in which I invest all my energy doesn't come back disfigured. It's a recreation I've been treating myself to two days, to reread with□soin about fifty pages on the net, to detect here and there a comma again p . 1200 that's out of place...

The work tonus is not at its zenith. For weeks now, a sadness inside me has been warning me that there are more essential things awaiting me than bringing these notes I'm writing to their natural end. I write as if against the current, and yet I know that, barring accidents and force majeure, I won't stop until I've finally put the finishing touches to Burial. But the fact of compressing and exiling this sadness, which is now as heavy as a stone, of not giving it a voice in these notes (except allusively and in passing at this very moment), is a pretty clear sign that for some time now, my reflection has no longer had the quality of "meditation". It's part of the division between those who write (without stopping to put their whole selves into it<sup>980</sup> (\*)!), and those who live and feel (without stopping, however, to "rest" on what they're experiencing and soak up its meaning). At this point, I feel it's high time to arrive at this "final point" (without

<sup>979</sup>(\*) As I announced in yesterday's reflection, I'm thinking of including this description in the next volume of Reflections, along with a (very brief) overall sketch of the "vast array of motifs" - judging that the occult motif trickery has gone on long enough. I'd like to point out right now that the construction principle envisaged does not depend on any kind of conjecture about algebraic cycles, such as "Hodge" or "Tate" (or any of the twelve variants mentioned yesterday).

<sup>980</sup>(\*) However, in the previous paragraph I just wrote (without any inner reservations) that I "put my whole self" into the texts I entrust to the typist. So the same words (or almost. . . ), depending on the context, can have a different meaning or indicate a different nuance.

As well as working on my notes, there's something else that's been distracting me in recent days. It's the resumption, as if in spite of myself, of a mathematical reflection. Over the past few days, I've come to realize that the construction of a pattern theory, with all the scope I saw for it twenty years ago, is by no means as far "on the horizon" as I had thought. It might even be that a "fully grown-up" theory, with the complete formalism of the six operations (plus biduality), is a matter of only a few years' work, for

someone who would invest his whole self in it (without degrading his creative energy through fossilizing dispositions). It also seems to me that there are two "keys"<sup>981</sup> (\*\*) to the explicit description of "the" category of motifs  $\square$  sur un

scheme, let's say of finite type on the absolute basis  $Z$  (a case we should always be able to get back to). On the one hand, there's the theory of the "mysterious functor", with sufficient generality and flexibility to pass to idiosyncratic triangulated categories, making it possible to link De Rham - Mebkhout coefficients and ordinary  $p$ -adic coefficients (in null car.). On the other hand, there's the question of the explicit construction of the category of motives over a **finite** field  $k$  (by a "purely algebraic" construction, preferably, without reference to algebraic geometry over  $k$ ), and moreover, of the "motivic cohomology" functor going from separate schemes of finite type over  $k$  (and to begin with, projective and smooth schemes) to this category. I had constructed the latter to near equivalence, heuristically using Weil's and Tate's conjectures<sup>982</sup> (\*).

<sup>981</sup>(\*\*) There is, however, a third "key", which I won't mention here because the problem in question seems to me (rightly or wrongly) less tricky. This is the correct definition of the "De Rham-Mebkhout coefficients" (initially without filtrations or  $F$ -structures) above, say, a smooth scheme on the absolute basis  $Z$ . This definition should at the same time provide the key of "the" correct definition of general crystalline coefficients as  $p > 0$ , which my dear ex-students (Berthelot in the lead this time) still haven't been able or willing to get out of the way.

When, in June 83 (two years ago), Mebkhout explained his "philosophy" around the Good God Theorem to me, I had the following idea

the impression that his "purely algebraic" ("De Rham" type) description for the category of constructible discrete coefficients (on  $C$ ) of a smooth scheme on the field  $C$  of complexes, was dual to the approach (never published) followed by Deligne in the seminar (already mentioned elsewhere) given by him at the IHES in 1969/70 (unless I'm mistaken), using connection promodules.

I assume that the transition from one point of view to the other is made by the dualizing functor  $RHom(., \mathcal{O}_X)$  with respect to the structural bundle of the scheme under consideration, which transforms  $D_X$ -Modules of finite type (which can be considered as " $\mathcal{O}_X$ -Modules ind-coherent" provided with an integrable connection) into "pro-coherent" modules (also provided with an integrable connection). The advantage of Mebkhout's point of view is that he provides a simple and deep algebraic expression ( $M$ -coherence, homology, regularity) for "good coefficients", which Deligne lacked. The advantage of Deligne's point of view is that it provides an equivalence (instead of an anti-equivalence) with the coefficients of transcendental nature that need to be expressed, and

that it lends itself better to expressing the multiplicative structure (tensor product) for the category of coefficients under consideration. I assume that, in practice, it will often be in our interest to work on both tables at the same time, mutually dual to each other.

Deligne's interpretation seems to me closer to a direct geometrical intuition, via that of an integrable connection module (or promodule). This is expressed in particular by the fact that (if the base field is  $C$ ) a constructible bundle of  $C$ -vectorials corresponds to a promodule with unique connection, instead of a complex of such promodules. This is why (in my with great regret, as you can guess. ...) I predict that this is his point of view (which he had buried without regret, as if to thereby burying the coefficient problem bequeathed by the disowned master. ...) that will be best suited to developing the six-variance formalism, and as the third key ingredient in the construction of pattern categories.

(May 9) See also the sub-note ". . . and hindrance", n° 171(viii), as well as "The five photos" (n° 171 (ix)).

<sup>982</sup>(\*) If I remember correctly, I confined myself to describing the category of semi-simple patterns. An immediate variant of the construction (following the same principle) provides a plausible candidate for the category of not necessarily semi-simple patterns. When I speak of "patterns" here, I'm actually referring to "isomotive" or isogeny-adjacent patterns. But by using the " $l$ -adic realization" functors for any prime number  $l$ , we can reconstitute the category of not-isomotive patterns from there.

(where  $Hom$  will therefore be modules of finite type on  $Z$ , not on  $Q$ ).

When I say that my construction made heuristic use of Tate's conjecture, I don't mean it literally. If it is true that there exist (over a finite body, in this case), on a smooth projective scheme, cohomology classes that are "motivic" (in a sense that remains to be clarified) without being "algebraic" (i.e. without coming from algebraic cycles), then

Tate's conjecture (like Hodge's, this time above  $C$ ) can be re-stated by replacing it with "algebraic classes" by "motivic classes". Assuming that we manage (as I suggest below) to define the canonical cohomological functor (and presumed "universal" in a suitable sense) on the category of projective and smooth patterns on the body finite  $k$ , to the category (called "semi-simple patterns on  $k$ ") already constructed, this will ipso facto provide a We'll call them "motivic" cohomology classes, such as the elements of  $Hom(T^i, H_{\text{wor}}(X))$  (in dimension  $2i$ ), where  $T$  is Tate's object, and  $H_{\text{wor}}$  is the hypothetical functor considered. This is why the construction of this

I have no doubt that this construction is correct. The work that remains to be done, no doubt clearly more delicate, consists in "pinning down" this category in terms of the given finite field  $k$ , and above all, in defining the "motivic cohomology" functor, if only first of all on the category of abelian schemes on  $k$  (this which should be enough to "pin" the category you're looking for... ). This second problem seems to me less technical, more directly "geometrical", than that of the mysterious functor. What's more, it seems to me to hold **the** key to a solution of the standard conjectures<sup>983</sup> (\*) and, by the same token, to the irritating questions of completeness that arise in cohomological theory with characteristic  $p > 0$ . For all these reasons, this question has a powerful attraction for me! This is the third evening that I've gone back to the drawing board, with the idea of rapidly reviewing the themes that seem to me to be the most burning, among those left behind by my students and by everyone else, when I left the mathematical scene fifteen years ago<sup>984</sup> (\*\*). This time I'm finally going to do it!

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□ **Project 1: Topos.** I mention them here mainly for the record, having expressed myself quite circumspectly-  
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I wrote about them in the note "My orphans" (n° 46). Given the disdain with which some of my former students, led by Deligne, have treated this crucial unifying notion, it has been condemned to a marginal existence since my departure. As I pointed out in the note quoted above, topos and multiplicities of all kinds are to be found at every step in geometry - but we can of course do without seeing them, just as we did without seeing groups of symmetries, sets or the number zero for millennia.

In the first two volumes of SGA 4 (the famous "gangue de non-sense" referred to by Deligne in the introduction to the first presentation of the brilliant volume entitled "SGA 4<sup>1</sup>"), a flexible and delicate language of topos was carefully developed, intimately "sticking" to topological intuition. This is the natural culmination of the language and intuitions around the notion of "faisceau" introduced by Leray; this second stage (or "second breath") in the development of the "faisceutique" intuition and tool, seems to me of comparable scope to the first (finding its provisional expression in Godement's well-known book). From the outset, it was this vision that made the appearance of the  $\ell$ -adic and crystalline cohomological tools possible, before it was buried sine die by the very people who pretended to approach these tools.

SGA 4's developments on the subject of topos do not claim to be complete and definitive, but I think they are more than sufficient for most immediate geometrical uses of the topossic view. Like general topology or ordinary beam theory, "topossic general topology" doesn't seem to me to pose any really profound questions of its own. It's a carefully crafted language in the service of a certain extension of topological and geometrical intuition of forms, which is dictated to us by things themselves. The discredit in which this vision has been held, and the **derision** which

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It seems to me that this is **the** most crucial question of all, for the actual (and no longer hypothetical, as in the sixties) construction of a theory of motifs.

<sup>983</sup>(\*) The term "standard conjecture" is not to be taken literally here, nor is "Tate's conjecture" in the previous b. de p. note. Rather, in stating these conjectures, it would be appropriate to broaden the class of cycles envisaged (initially reduced to algebraic cycles only). In the "definitive" expression of the "readjusted" standard conjectures (and even though they would be valid as they stand), "algebraic" cohomology classes will again be replaced by "motivic" classes. I'll come back to standard conjectures in more detail, in "Les motifs mes amours" (in volume 3 of *Réflexions*).

<sup>984</sup>(\*\*) For a brief overview of these themes, see last year's note "My orphans" (n° 45).

Here, we're not talking about a "dilapidated building site" that needs to be brought back to life, but a fully completed and installed house, which those who lived in it and were called to make it a place to work and live, have chosen to leave, by unloading the workman who built it. The house is spacious and healthy, and everything is as it should be.

p. 1204 The day the worker leaves for other tasks. If she needs something, it's not

□ not from the work of his hands, or anyone else's. Perhaps the act of respect from the worker himself, for those things which these hands have made with love and which he knows to be beautiful, will he dispel these effluvia of violence and contempt, and make welcoming again that which was made to welcome.

**Project 2: Cohomology language.** This involved first and foremost the language of derived categories, and secondly the points of view I had introduced for non-commutative cohomology, both in the second half of the 1950s.

The first trend was supposed to be the subject of Verdier's famous "thesis", and Verdier's own burial of his<sup>985</sup> (\*) thesis was at the same time that of the point of view of derived categories in homological algebra. The latter had played a crucial role in the sixties flowering of the cohomological theme in algebraic geometry, notably for the duality formalism, and the development of fixed-point formulas (Lefschetz-Verdier type). Practical needs had revealed the inadequacy of the framework of triangulated categories developed by Verdier in the early sixties, a framework that has still not been renewed as it should.

On the current "non-commutative" side, we have a good foundation with Giraud's thesis, but this is limited to a 1-field formalism, lending itself to a direct geometric expression of cohomological objects up to dimension 2 only. The question of developing a non-commutative cohomological formalism in terms of *n-fields* and *n-gerbes*, urgently suggested by numerous examples, ran up against serious conceptual difficulties. Given the disaffection or, better still, the general contempt into which questions of fundamentals have fallen in a certain "beau monde", these difficulties never arose.

before I got to grips with them a little over two years ago<sup>986</sup> (\*\*).

p. 1205 □ I now see the two currents coming together in a new discipline, which I have proposed elsewhere<sup>987</sup> (\*)

to call it **topological algebra**, a synthesis of traditional homological algebra (derived categories style, of course), homotopic algebra, the formalism (still in limbo) of *n-categories*, *n-groupoids* and idiosyncratic fields and sheaves, and finally the vision of topos, which now provides the most extensive "purely algebraic" nature framework known, to implement topological intuition. The initial ideas for such a synthesis were already present in the 1960s, including that of the **derivator**, which was to replace the inadequate notion of the triangulated category, and also apply to "non-additive" contexts. Some important developments in homotopic algebra, such as the notions of homotopic limits and colimits developed by Bousfield and Kan in the early '70s, without their being aware of my ideas (treated as Grothendieckian bombast by my dear students), are to be found in the "non-additive" context.

<sup>985</sup>(\*) On this subject, see the note "Thèse à crédit et assurance tous risques" (n° 81), and "Gloire à gogo - ou l'ambiguïté" (n° 170(ii)).

<sup>986</sup>(\*\*) This is the reflection in my February 1983 letter to Daniel Quillen, in which I discover how to "jump in with both feet" at the on top of the gaping "purgatory" of increasingly screwed-in compatibility relations, which seem to creep into the description in the form of *n-categories* (not strict, or *n-fields* as I call them now), for increasing *n*. The *n* = 2 case is already no picnic, and nobody, I believe, has yet found the courage to spell them all out for *n* = 3. This letter became (as I recall below) the "kick-off" for the long journey "In Pursuit of the Fields", which began the following month on the momentum of the reflection begun.

This letter was not deemed worthy of being read by the addressee, nor of receiving a reply. I eventually received a comment from the person concerned more than a year later, on which I comment in the section "The weight of a past" (n° 50). (Cf. p. 136, second paragraph.)

<sup>987</sup>(\*) See sub-note no. ° 136<sub>1</sub> to the note "Yin the Servant - or generosity" (especially p. 638).



right in line with them.

Two years ago, with my letter to Daniel Quillen<sup>988</sup> (\*\*), I began to outline the work I see ahead. This was the kick-off for the writing of "A la Poursuite des Champs", of which a first volume ("Histoire de Modèles") is practically finished, and will probably appear as volume 4 in *Réflexions*. I foresee that I'll need one or two more volumes, and one or two years of work, to complete this preliminary exploration of a rich substance, which twenty years on I still seem to be the only one to grasp. It's a project that was abandoned for some fifteen years, but which I've been working on for almost a year. The writing of *Esquisse d'un Programme*, followed by *Récoltes et Semailles*, interrupted this work, which I nevertheless intend to resume and bring to a successful conclusion, as soon as I have finished writing *R. et S.* and the texts (all of limited dimensions) which, with the last part of *R et S.*, are to make up volume 3 of *Réflexions*.

□ **Workshop 3: Six operations, biduality.** This is the point of view I introduced into the formalism of p. 1206 duality à la Poincaré or à la Serre, with discrete or continuous coefficients. The name "six operations" that I had introduced has been carefully eradicated by my cohomology students. They confine themselves to using here and there those that suit them, while jettisoning the structure they form as a whole (along with the biduality formalism), and above all, the irreplaceable thread provided by the point of view (not least to derive good "coefficient categories", cf. below). In the more than twenty years that this formalism has existed and proved its worth, no one "in the know" has taken the trouble (except in papers destined to remain secret and of which I have no knowledge) to identify the algebraic "form" common to the many situations where such a "boilerplate" duality is available, expressed in a formalism of six operations<sup>989</sup> (\*).

We can see that this is not strictly speaking a "derelict site" (since the formalization work to be done here is derisory), but rather a fertile point of view that has been systematically avoided (as has been the case with topos). I'm sure this abandonment has had a lot to do with the lamentable stagnation I've observed (with a few exceptions<sup>990</sup> (\*\*)) on the subject of the cohomology of algebraic varieties, especially in comparison with the vigorous development I gave it between 1955 and 1970.

As I already announced in the Introduction (I 8, "The end of a secret"), following Harvost and Semesh<sup>991</sup> (\*\*\*), I intend to include a short sketch of the essential features of the "six operations" formalism. Thanks to the care of my □ pupils, its very existence is today unknown to all, with the sole exception p. 1207 of those who were directly involved in one or other of the two seminars SGA 4 (1963/64) and SGA 5 (1965/66)<sup>992</sup> (\*), and who have obviously forgotten it. In this way, I will have done what I can to bring back into the limelight (if there are workers on the lookout for good tools) a perfectly effective tool, and a fruitful point of view which, in the cohomological theme, constantly leads us straight to the crucial problems.

<sup>988</sup>(\*\*) For more on this letter, see "The weight of a past" (n° 50, page 136, 2nd paragraph).

<sup>989</sup>(\*) (May 9) In one of the first presentations at SGA 5, I took great care to explain this form in detail.

was to be the nerve-centre of the entire seminar. This talk, the most crucial of all in SGA 5, has disappeared from the massacre edition. There is not a single hint of its existence in the entire volume! See b. de p. (\*) page 942 in the note "L'ancêtre" (n° 171(i)).

<sup>990</sup>(\*\*) The "few exceptions" are mainly (before 1981) Deligne's two important works Weil I,II, and some results sporadic in crystal cohomology, and in Dieudonné theory of Barsotti-Tate groups on general char.  $p > 0$  bases (which I had initiated around 1969). As I have pointed out elsewhere, there has been a revival in the wake of the theorem of the good God - Mebkhout (one still as ignored as the other. . .), with in particular the theory of Mebkhout beams (wrongly called "perverse" instead of *qui de droit*. . .), developed by Deligne et al.

<sup>991</sup>(\*\*\*) I would remind you that this is volume 3 of *Réflexions*, which in principle also contains the last part of *Récoltes et Semailles*.<sup>992</sup>(\*) These are also the two seminars, as if by chance, that the text which presents itself as "central" and is called (oh irony!) "SGA 4<sup>1</sup>" recommends not to read. ... (May 29) For the scope of the six operations vision, see the note "Details".

useless. ... " (n° 170 (v)), part (b) ("Machines for doing nothing. . .").

The three abandoned "building sites" (or houses, or tools. . . ) I've just reviewed have more to do with a common **algebraic language** to express the most diverse geometric situations, than with a particular geometric situation, such as the cohomology of algebraic varieties. If, in the second field, the one I call "topological algebra", I happen to come into contact with questions that are undoubtedly profound (such as those linked to the homotopy groups of spheres), it's by accident, not by design. Here again, my main motivation has been, and remains, to develop algebraic tools of sufficient generality and flexibility for the development of this **arithmetical geometry**, still in its infancy, that I've spent fifteen long and fruitful years of my life nurturing, bringing into the world and nourishing, from the embryo that was Weil's conjectures. It's in this geometry that we find the geometric substance itself, which for all those years was at the very heart of my love affair with mathematics, and remains so to this day. It's this substance that I'm now going to discuss in the three "hottest" topics I've yet to review.

**Area 4: "Problem of coefficients"**. This problem was already present in the formulation of Weil's conjectures<sup>993</sup> (\*\*). It was at the heart of my interest in cohomology throughout the 1960s.

It was clearly<sup>994</sup> posed, with all the necessary generality and precision, for the main types of coefficients then glimpsed<sup>994</sup> (\*\*\*). I'm speaking about this obviously crucial problem □ for

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an understanding of the cohomology of algebraic varieties, from the first return to my work and the act of respect that is the note "Mes orphelins" (n° 46), and I return to this subject in the note "La mélodie au tombeau

- or sufficiency" (n° 167). There are two essential threads: on the one hand, the formalism of the six operations and biduality just mentioned. On the other hand, the need to find adequate generalizations, above a more or less general basic scheme, of the types of "coefficients" already known above a basic field, which intervene (even if only tacitly) in the description of cohomological functors already known on the category of projective and smooth schemes on this field: *l-adic*, crystalline, De Rham cohomology, or finally (when  $k = \mathbb{C}$ , field of complexes) Betti or Hodge cohomology.

I don't think it's an exaggeration to say that this problematic contains in its germ<sup>995</sup> (\*), both the "Hodge-Deligne theory" "in full maturity" which is still waiting to emerge, and the "De Rham-Mebkhout coefficient theory" which is also waiting<sup>996</sup> (\*\*); and it's for one and the same reason that both

<sup>993</sup>(\*\*) On this subject, see the beginning of the note "Les manoeuvres" (n° 169), where I comment on the initial problematic of Weil's conjectures. (May 29) This beginning has become autonomous in a note "Le contexte "conjectures de Weil"" (n° 169 (i)).

<sup>994</sup>(\*\*\*) It doesn't seem that any new types of coefficient have appeared, compared to those I had already foreseen in the second half of the sixties.

<sup>995</sup>(\*) In making this observation, I have no intention of minimizing the originality or importance of the contributions in question by Deligne and Mebkhout, any more than I intend to diminish the originality and importance of my own contribution to the birth and initial impetus of arithmetic geometry, by noting that it "was already in germ" in Weil's conjectures.

<sup>996</sup>(\*\*) The contributions in question, first by Deligne (around 1969) and then by Mebkhout (after 1975), can roughly be said to address the problem of finding suitable "De Rham coefficients" (which would enable the ordinary De Rham cohomology of smooth schemes to be inserted into a six-variance formalism), in two very different directions.

different. Deligne defines a "good" category of coefficients over the  $\text{spec}(\mathbb{C})$  scheme only, and the functors  $Rf_!$ ,  $Rf^*$  in the case of the structural morphism  $X \rightarrow \text{Spec}(\mathbb{C})$  of a separated finite scheme over  $\mathbb{C}$ , and for constant coefficients (alas!) over  $X$ . Mebkhout defines a "good" category of coefficients, valid in principle for any separated  $X$  of type

finite on a field of zero characteristic  $K$  - but he doesn't go so far as to define functors  $Rf_!$  and  $Rf^*$  for a morphism  $f: X \rightarrow Y$  of such schemes on  $K$ , and to develop a duality theorem for  $Rf_!$ , and  $L_f^*$  (except for  $Y = \text{Spec}(K)$  - and even then, only in the transcendental context, undoubtedly much more difficult, of complex analytic varieties). A

Another limitation of Mebkhout's theory so far (in a very discouraging atmosphere, it has to be said) is that it is now only developed for smooth  $X$  (for want, I presume, of systematically using the crystalline point of view, which provides a satisfactory substitute for the ring bundle of differential operators, so convenient in the smooth case).

For desolate building sites, these are desolate building sites! They speak eloquently of my systematic disaffection.

It's *the* eagerness of my cohomology students, led by Deligne, to bury the problematic bequeathed by the master, at the same time as the master himself.

□ For the fragmentary steps taken on the one hand by Deligne (showered with all the facilities of the spoiled child of science), and on the other by Mebkhout (in the complete isolation imposed on him by those very people who were best placed to welcome him), they nonetheless provide invaluable guidelines for identifying certain crucial categories of coefficients. These important contributions were present in my mind when I wrote the aforementioned note "La mélodie au tombeau". Since then, I've delved a little deeper into the "yoga coefficients and patterns" that had already emerged in the sixties, and I now have a more precise and complete picture. So I plan to return to the problem of coefficients (and that of patterns at the same time) in volume 3 of *Reflections*, following the sketch of the six-variance formalism.

Suffice it to say that I see essentially three types of fundamental coefficients<sup>997</sup> (\*), on a more or less arbitrary base  $X$ :  $l$ -adic coefficients (**any** prime number),

the **De Rham-Mebkhout** coefficients<sup>998</sup> (\*\*) (of particular interest for  $X$  of finite type on a base scheme)

$S$ , the most □ important cases being those where  $S$  is the spectrum of the rings  $\mathbb{Z}$ ,  $\mathbb{Q}$ , or  $\mathbb{C}$ ), finally the coefficients of

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Betti (for  $X$  of finite type on  $\mathbb{C}$ ). Only the third of these categories seems to me to be determined at present without any hypothetical elements. To define the first (if only for  $X$  of finite type on the absolute basis  $\mathbb{Z}$ ), or to describe its relations with the second, the existence of a mysterious functor theory (which I had postulated as early as the end of the sixties, a problem which also seems to have sunk with the rest. . . ) seems to me to be the crucial ingredient, to which I'll have to return in greater detail in due course.

**Project 5: Motifs.** I've already said enough about the burial of motifs by my friend Pierre Deligne, with the blessing of the entire Congregation, that there's no need to dwell on the subject again here. Instead, I'd like to highlight a new fact that has just come to my attention, and which should have appeared fifteen or twenty years ago. Even a month ago, the "in form" construction of the category of patterns on top of a more or less general basic scheme (a scheme of finite type over  $\mathbb{Z}$  let's say, or only over the spectrum of an algebraically closed body. . . ) seemed to me something decidedly "on the horizon", drowned in the mists of a distant future. This state of mind was undoubtedly a tenacious inheritance from the already distant days, when motivic reflection had started on a very hypothetical basis, when the formalism of  $l$ -adic cohomology was not yet available. There's also this "mitigating circumstance" for me, which is that my tasks of writing foundations, for

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of ex-students (and those marked by the ascendancy they may exert) vis-à-vis the main ideas I had introduced, and developed in certain directions, during the sixties.

<sup>997</sup>(\*) If I speak of "fundamental" types of coefficient, it is to suggest that all the other important types of coefficient that I can now glimpse must be describable in terms of these, either by "combining" them in a suitable way, or by adding suitable structural enrichments, or both. Among the structure enrichments envisaged on De Rham-Mebkhout coefficients, there is (in addition to "filtration by weights", which seems "internal" to the category of coefficients envisaged), a "De Rham filtration" which plays a leading role in motivic applications. It may be that this additional structure only makes sense (from the point of view of a six-operation formalism) when combined with a Betti-like "discrete" structure, which should make it possible to formulate the right properties for this filtration to satisfy. I intend to return to these questions in greater detail in "Les motifs mes amours" (in Vol. 3 of *Réflexions*).

<sup>998</sup>(\*\*) I recall that for this type of De Rham-Mebkhout coefficient, I now see two dual variants of each other, that of Mebkhout and that which I hesitate to call "de Deligne", even though it is a child repudiated by him!

(May 29) For comments on the repudiated child, see the note ". . . et entrave" (n° 171 (viii)). For details of De Rham's coefficients, see the note "Les cinq photos(cristaux et  $D$ -Modules)", n° 171 (ix).

the things that were at hand, absorbed so much of my energy between 1958 and 1970, that my motivic reflections (and others, on themes that seemed like "luxuries" in relation to my imperious tasks of the moment) were constantly reduced to the smallest portion, that I granted myself almost against the bad conscience of one who would "play hooky"! In any case, I was left with the impression that coefficient problems were what was ripe to be done right away (but by others, since I was already busy elsewhere. . .), while motifs, for me, were what was ripe to be done right away (but by others, since I was already busy elsewhere. . .), while the patterns, for the moment, were just right for a "mathematics-fiction" book, if I'd found the leisure to write it, surely, things would have changed very quickly, if I'd actually started writing it, instead of toiling away on tasks that nobody in the world then had the heart to continue, while everybody is quite happy to make use of what I've done... .

□I've come to realize that this thing, in itself self-evident yet once is that as long as we take the trouble to describe sufficiently "fine-grained" coefficients, i.e., taking into account all the known structures associated with a pattern, we end up describing **the pattern itself**. Or perhaps more correctly, we end up describing a category, which will contain the (triangulated) category of patterns as a **full subcategory** (which is already not bad) - just as the category of patterns over the field of complexes appears (if we admit a strong enough version of the Hodge conjecture) as a full subcategory of the category of Hodge-Deligne structures. As for characterizing exactly, in "algebraic" terms directly adapted to the coefficients we're working with, exactly what this full subcategory **is**, i.e. exactly **which** coefficients "are motives", we fall into questions that are likely to be much trickier. These are those concerning the **compatibilities** between various geometric-arithmetic structures associated with a pattern (compatibilities to which I have already alluded, I believe, in the quoted note "Melody at the tomb"). It's the solution to these problems (which seem to me irrelevant to the actual construction of a "theory of patterns") that may well be "a hundred years away". In any case, experience shows us again and again that such prognostications (on the more or less "unapproachable" nature of a question) make little sense, except to discourage those whose courage isn't quite up to scratch...

(April 1) A few more comments on the formalism of the **"Galois (or fundamental) motivic group"**. This notion (which I identified and began to develop in 1964, before I had the honor of knowing my future ex-student Pierre Deligne) gives rise to intuitions and a formalism of great precision and finesse. Its existence and essential features are independent of the particular construction that would have been adopted for the notion of a pattern on a body (or of a "smooth" pattern on any schema), as long as this satisfies a few reasonable conditions. I had entrusted Neantro Saavedra with the task of putting into publishable form, in as general a context as possible, the dictionary I had drawn up around 1964 between, on the one hand, geometry in categories I called "rigid tensorial" ( $k$ -linear categories with "tensorial product" operation satisfying suitable conditions,  $k$  being here a **body**), and on the other hand the theory of linear representations of pro-algebraic groups on  $k$  (or, more precisely and □ more generally, "pro-algebraic sheaves" on  $k$ ). He completed this task in his thesis, published in Lecture Notes in 1972 (LN 265)<sup>999</sup> (\*). I had taken this dictionary a step further

<sup>999</sup>(\*) (May 10) Since these lines were written, I have had the opportunity to acquaint myself with the book in question, a copy of which the author had not seen fit to send me. I was able to see that in this book, Saavedra is the brilliant inventor of the new philosophy set out in it, faithfully following the notes I had passed him, and without practically mentioning my name (neither for the notions introduced in this book and for the crucial results, nor for already known notions such as crystal, stratified module or pattern). The very name "Tannakian category", which he has renamed the main notion, is such a brilliant mystification that he surely didn't invent it himself any more than he invented the theory he presents himself as the author of. Moreover, this "parternity" was only temporary, and my friend Pierre, ten years after the publication of

(notably as regards the translation of filtered or graded structures etc. on certain fiber functors, or that of a notion of "polarization" associated with a tannakian category), than is done in Saavedra's thesis<sup>1000</sup> (\*\*), or in the "memorable volume" LN 900 (where Saavedra's thesis is redone and the notion of motivic Galois group is at the center of the problem, without my name being more pronounced on this subject than on any other concerning motives).

I would also like to point out that the first step in determining (with equivalence) the category of patterns over a finite field, discussed previously at<sup>1001</sup> (\*\*\*), was the determination of the group of Galois motives of said finite field, which must be commutative (being generated topologically by the element of Frobenius), and is in fact an extension of  $\hat{\mathbb{Z}}$  (generated by Frobenius) by a certain algebraic pro-tore on  $Q^{1002}$  (\*). The second step was the description of the element of  $H^2(Q, F)$  which (according to Giraud's theory) classifies the G-gerbe of fiber functors<sup>1003</sup> (\*\*).

As expressed in the note "Souvenir d'un rêve - ou la naissance des motifs" (n° 51), I came across the motivic Galois group while looking for the link between  $l$ -adic representations, for  $l$  variable, of a profinite Galois group  $Jal(K/K)$  in  $l$ -adic modules, obtained for example by taking the  $H^i(X_K, Q_l)$ , where  $X$  is a smooth projective scheme on  $X$  and  $i$  an integer (or possibly, a suitable submodule thereof). Serre was looking at the image of the Galois group in  $Aut(V(l))$  for any  $l$ , which is a reductive  $l$ -adic Lie group, and it did seem that its structure (in the sense of Lie theory) was independent of  $l$ . It was while searching for the deeper reason for this phenomenon (itself hypothetical to this day), and relating it to Tate's conjectures, that I discovered the notion of motivic Galois group, following on from that of "pattern" and "motivic cohomology".

If there was one simple, profound thing that I brought to light, and if there was one creative act in my life as a mathematician, it was the birth of this crucial notion, linking geometry and arithmetic. That's why, on that memorable April 19th last year, I was suffocated by a feeling of unimaginable **impudence**, seeing this thing appropriated with such superb casualness, like the last of the trifles just improvised there and then in the bend of a technical paragraph: see, it's as silly as cabbage, all you have to do is apply proposition 4.7.3 of our modest article exposing the theory of Tannakian categories. ....<sup>1004</sup> (\*\*\*). This is how mathematics is done in the 1980s, after many

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volume, to do whatever was necessary to ensure its return (according to everyone's expectations) to the one already designated for this purpose. For details of this brilliant operation on a corpse (the first and only of this scale, prior to the "SGA 4" operation<sup>1</sup>

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SGA 5" done in the same inimitable style), see the suite of notes "The sixth nail in the coffin" (n° s 176<sub>1</sub> to 176<sub>7</sub>).

<sup>1000</sup>(\*\*) (May 10) This presumption turned out to be wrong. It was due to my conviction that Saavedra would be absolutely to "complete" the program I had indicated to him, even though his mastery of "linear representations of pro-algebraic sheaves" alone seemed for a long time to have outstripped him, and his mathematical background was extremely limited. Given Saavedra's less-than-exceptional means, it's unthinkable to me that in the less than two years between my departure (when he had no notion of cohomology, or of the structure of algebraic groups) and the publication of the book, he should have had the opportunity to assimilate (and to do so perfectly, as the book's appearance testifies) the host of all-round notions with which it juggles. On this subject, see the note "Monsieur Verdoux - ou le cavalier servant" in the aforementioned series of notes "Le sixième clou au cercueil".

<sup>1001</sup>(\*\*\*) (May 10) I note that this determination, too, figures in Saavedra's inexhaustible book (without reference to my modest person, needless to say). It uses the cohomological theory of the global class body (determination of the group  $H^2(Q, T)$ , where  $T$  is a group of multiplicative type on  $Q$ ) - so it's also among the things my ex-student (with apparently superhuman means) would have assimilated in less than two years. ...

<sup>1002</sup>(\*) This is the motivic Galois group that classifies **semisimple** patterns. To obtain the general patterns, we need to make its product by the additive group  $G_a$  on  $Q$ .

<sup>1003</sup>(\*\*) The crucial point is that this class becomes zero (thanks to the existence of the " $l$ -adic cohomology" fiber functors) in all places  $l \neq p = \text{car.} k$ , and the existence of the crystalline functor-fiber gives us sufficient information about the fate of this class in the missing place  $p$ .

<sup>1004</sup>(\*\*\*) As I wrote these lines, the association with the very similar way of introducing the definition of the function  $L$  with coefficients in an  $l$ -adic bundle came to mind, without reference to anyone and as the last of the banalities that would come

p. 1214 brilliant antecedents in the 1970s<sup>1005</sup> (\*).

But I'm getting off topic, all right - I was supposed to be giving a tour of a building site, not sentiment. So I'll point out that, as in the case of the profinite fundamental group, if  $X$  is a geometrically connected scheme over a field  $k$ , there's a distinction to be made between the motivic fundamental group of the scheme  $X$  itself, and the "geometric" motivic fundamental group. The two do not coincide, **even** if  $k$  is algebraically closed - because the motivic fundamental group of  $k$  is not trivial (it is connected, but no more!). We must therefore introduce the "geometric" motivic fundamental group of  $X$ , which is supposed (among other things) to establish a link between the various  $l$ -adic Lie groups associated (as quotients) with the geometric profini fundamental group  $\pi_1(X_k)$ . It is defined as the kernel of the natural homomorphism

$$\pi_1^{\text{mot}}(X) \rightarrow \pi_1^{\text{mot}}(\text{Spec}(k))$$

(relating to the choice of a functor-fibre on the category of smooth patterns on  $X$ ).

The point I wanted to get to is that this kernel, which we might note  $\pi^{\text{mot}}(X/k)_1$  should be the first step towards the construction of a "motivic (geometric) homotopy type of  $X$  over  $k$ ", to which I have already alluded in passing previously<sup>1006</sup> (\*\*). The formal description of this "homotopy type"<sup>1007</sup> (\*\*\*) ,

whose "cohomology" should be none other than the motivic cohomology of  $x$ , is part of the interesting conceptual work in prospect on the "motifs" site, in a decidedly different (and broadly

The central task is the actual construction of motif categories and the formalism of the six operations for them.

**Job 6: Standard conjectures.** As I explained in a previous footnote (note(\*) p.1202), these conjectures can be understood in two different senses. Firstly, in the literal sense, as I formulated them at the Bombay Colloquium in 1967<sup>1008</sup> (\*). In this form, they seem to me to summarize the most crucial questions that now arise in the theory of algebraic cycles, at least from the point of view of "homological" equivalence for these cycles.

In formulating these conjectures, however, my main motivation was not directed towards cycles for their own sake, but towards the means they provide (perhaps. . . ) for constructing a theory of semi-simple patterns on a body, satisfying desiderata that should have been "common knowledge" for fifteen or twenty years (and yet still remain occult. . . ). In volume 3 of *Réflexions*, I shall indicate some weakened variants of these conjectures, which would suffice to build such a theory (and of which the weakest is practically necessary and sufficient for this purpose). As I've already pointed out elsewhere, even if the conjecture in its initial form were to prove valid on a given body  $k$  (for finite  $k$ , for example, or even for all  $k$ ), this would not in itself mean that the cohomology classes that we should call "motivic"<sup>1009</sup> (\*\*) (and which we can hope will make true various conjectures of the Hodge type) would be sufficient to build such a theory.

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the same brilliant author. On this subject, see the sub-note ". . . and nonsense" (n° 169<sub>6</sub>) to the note "Les manoeuvres" (n° 169), p. 891.

<sup>1005</sup>(\*) And even as far back as the sixties - see the note "L'éviction" (n° 63).

<sup>1006</sup>(\*\*) In the note "Requiem pour vague squelette" (n° 165).

<sup>1007</sup>(\*\*\*) as an object type, I expect it to be a relative homotopy type (in Illusie's sense) in the "extension" topos (in Giraud's sense) of the  $\text{Spec}(\mathbb{C})$  topos  $\text{fpqc}$  associated with the sheaf (on this topos  $\text{fpqc}$ ) of functor-fibre on the category of smooth patterns on  $X$ . The relative cohomology (on the basic topos just described) of this type of homotopy is quasi-coherent (and even "coherent"), and can be identified with the motivic cohomology of  $X$  on  $K$ . Using a complex point of  $X$  (where  $K$  de car. nulle) to have a Betti functor-fibre, the corresponding homotopy-fibre type must be canonically isomorphic to the homotopy  $Q$ -type (neglecting torsion phenomena. . . ) associated transcendentally with  $X \otimes_K \mathbb{C}$ , at least when  $X \otimes_K \mathbb{C}$  is 1-connected.

<sup>1008</sup>(\*) Algebraic Geometry, Bombay 1968, Oxford University Press (1969).

<sup>1009</sup>(\*\*) I think I can propose a reasonable definition of motivic cohomology classes on an algebraic variety.

and Tate classes, for example) are necessarily algebraic. If it were ever discovered that there are non-algebraic motivic cohomology classes, this would undoubtedly mean that the importance of algebraic cycles in the theory of motives, i.e. in the arithmetic-geometric study of the cohomology of algebraic varieties, would be less than I had reason to believe in the early days of the theory. In any case, the actual construction of a theory of motives that I now foresee is independent a priori of common conjectures (Hodge, Tate, or "standard") about algebraic cycles.

□ This does not prevent the standard conjectures and their variants on the one hand, and those of Hodge, Tate and their p

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numerous variants of the other, conjectures which involve in particular statements of **existence** of algebraic cycles (i.e. of algebraicity of cohomology classes), or (in modified versions) statements of existence of so-called "motivic" cohomology classes, are intimately connected to each other, as well as to the description of the main "types of coefficients", and, in the limit, to that of the category of motives itself<sup>1010</sup> (\*).

Here again, the work of decanting, tidying and informing, which has been needed for almost twenty years, has not been done (nor, above all, made public) by those who have preferred to bury fruitful ideas (when they weren't published) or debunk them (when they were), and reserve for themselves the benefit (immediate) and the credit (later), rather than informing and making available to all the fascinating issues crucial to our understanding of the links between geometry, topology and arithmetic. I see that what is lacking here is not competence or even brilliant gifts, but simple honesty, and a certain **decency** too in the relationship to a "scientific community" that dispenses prestige and power, among those who do not feel bound by the slightest obligation, the slightest "return" in the form of even the slightest attitude of "service". That's why, although I lost touch with the subject over fifteen years ago and am no longer "in the loop" of anything, I'm going to make the effort to get back into the swing of things that were once familiar to me, at least to do my best, in volume 3 of *Réflexions*, to make up for the omissions of those younger and more gifted than myself, and to do at the end of the day what they didn't have the generosity to do.

At this point I think I've covered all these "building sites" which seem to me to be now (and already have been since the moment of

my departure from the mathematical scene) "the□most burning", with a view to building this "geometry arithmetic", the foundations of which I laid throughout the sixties. I don't mean to say that I've summarily covered **all** the substantive questions that perhaps only I can see and that are close to my heart. As far as I know, these are still at the point where I left them when I left the mathematical scene, and many have not even had the pleasure of being made explicit in the literature. Among these, I'd like to mention the **discrete Riemann-Roch conjecture** in the schematic framework<sup>1011</sup> (\*). Egale-

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projective and smooth, at least when the base body is of zero characteristic. For the general case, the crucial case (discussed above) is that of a fi ni basic body. Modulo the description of motivic classes in the latter think we can advance "the" right definition of motivic classes. Compare with comments in b. de p. (\*) on page 1202.

<sup>1010</sup>(\*) This does not contradict the assertion I've just made, namely that the construction I see of the category of patterns (on a body, let's say) is "independent" (i.e. "technically" or "logically" independent) of the various conjectures envisaged. These "intimate links" I'm talking about (which mean, for example, that the twelve variants I've seen to Hodge- and Tate-type conjectures suggest as many different types of cohomological "coeffi cients") are heuristic, not technical - just like the link between the (baptized "conjectural") Lefschetz-Verdier formula, and the trace formula for the Frobenius correspondence. In the latter case, this essential heuristic link is not a logical dependency, has been duly underlined in the two sub-notes "Real maths. . . ", ". . and 'nonsense'" (n° s 169<sub>5</sub> , 169<sub>6</sub> ) to the note "Les manoeuvres".

<sup>1011</sup>(\*) This conjecture is spelled out for the first time, it seems, in sub-note n° 87<sub>1</sub> of the suggestively named note "The Massacre" - given that the conjecture is one of SGA 5's massacred things, gone without so much as a trace of a **name** in Illusia edition.

ment, there's the generalization of the theory of the **local, global geometric class body** into a **duality** statement that is essentially "geometric" in nature (while giving the classical "arithmetical" statements as corollaries). This is discussed in letters to Larry Breen from 1976, reproduced as an appendix to Chap. I of "A la Poursuite des Champs" (which will therefore appear in vol. 4 of *Réflexions*). In these statements, the main work in perspective will be in a careful description of the categories of "coefficients" in which we are working. An important role here is played by a certain autoduality, discovered by Serre<sup>1012</sup> (\*\*), in the category of unipotent algebraic groups with near radicial isogeny, over a  $k$ -body of char.  $p > 0$  (an autoduality that is still unknown, it seems to me, outside the handful of people I've happened to tell about it). The question of generalizing such statements to higher dimensions is (for me at least) a total mystery (but Milne would have some light in the case of an algebraic surface. . . ).

These questions of duality go back, I believe, to the late fifties, when I had also branched out into the construction of a "**Jacobian**" **complex** (of chains) of proalgebraic groups, associated with a finite-type scheme over a body (to begin with. . . ), in terms of suitable "local Jacobians" associated with these various local rings, in analogy with the "residual" or "dualizing" complex I had constructed some

years before in coherent duality. All these questions of duality had been relegated to second place in the sixties, by the tasks in particular of developing  $\square$ ment of the "nonsense" of stale cohomology.

and  $l$ -adic and the language of topos. A certain part of my program, concerning relative local and global Jacobians, was completed around 1977 (without any mention of my modest person) by C. Contou-Carrère, who hastened to pack up in view of the welcome he received from Deligne and Raynaud<sup>1013</sup> (\*). Today, it takes a certain amount of courage to take up and develop ideas that bear my mark all too clearly (even though one would do one's best to hide it). The only one who has persisted in doing so is Zoghman Mebkhout, and the fate that befell him, culminating in the prowess of the Colloque Pervers, clearly shows the risk we run.

If I wanted to make a list of all the great questions I'd discovered between 1955 and 1970 (and which I've talked about here and there), I'd have to go on for days, and probably weeks if I wanted to be at all explicit and go into the ins and outs. This is not the place to do it, and I doubt I ever will. Not to mention that if one day (who knows!) I want a young mathematician to get involved in one of these questions, just to get his hands dirty and make a name for himself, it would be better for him to rediscover it himself, rather than run the risk of having a certain label applied to him.

Beware of the Perverse Colloquia that the future holds... .

## 18.7. (7) Evening fruit

### 18.7.1. (1) Respect

**Note** 179 (April 2) It's been five weeks (since February 26, with the note "Le silence", opening the series of notes grouped under the name "Les quatre opérations") since I've been reviewing the main facts of a "material" or (at least) "technical" nature concerning the Burial. In "The Four Operations", I had confined myself to the "swindle" aspect in the strict sense of the term - the one where the

<sup>1012</sup>(\*\*) In addition to Serre's beautiful idea, I was also influenced by the "geometric" point of view introduced by Lang in the geometric global class body, and by Serre in the local class body.

<sup>1013</sup>(\*) See note "Cercueil 3 - ou les jacobiniennes un peu trop relatives" (n° 95), and sub-note n° 95<sub>1</sub>, about some of Contou-Carrère's misadventures in the great mathematical world.



that "threshold" mentioned in the note of the same name (n° 172), which separates **bad dispositions** (expressed in "automatic rejection" reflexes, often in spite of the most elementary mathematical instinct) from blatant **bad faith** and outright plagiarism. In the section I've just written, "Les chantiers sorry", I see myself confronted above all with the "first level" of the Burial, below the "threshold" - the burying of a vast vision□and powerful ideas-force, which certainly no one is obliged to take up, and which every the world has the right to ignore or forget - even if it means "burying itself", by condemning its work (or at least the part of it directly affected by the rejected vision) to more or less complete sterility. p. 1219

Now I feel like I've come full circle, at last! As far as the (abandoned) "tour des chantiers" is concerned, it has given me a more detailed apprehension of the Burial of my work, while at the same time getting me back in touch, if only a little, with themes I'd lost sight of fifteen years ago. Above all, it gave me a clear idea of the orders of urgency in what I intend to put down in black and white in the next volumes of *Réflexions*. My aim will certainly no longer be to lay meticulous foundations for sciences in the making - that's something I've done enough of, and if no one else can be found to give themselves to such a task, as I once did, so much the worse for one and all! Instead, my aim will be to highlight a number of key ideas, in the service of an overall vision born between 1955 and 1970, which I now find (thanks mainly to the efforts of some of my former students, and with the acquiescence of all) either forgotten, ridiculed, or shamelessly appropriated, mutilated and stripped of their essential force. By taking them up again today, I'm at last loosening the reins on a drive for knowledge within me that, during the sixties, I often kept to a minimum, for the sake of endless "service" tasks. Those days are gone - and yet, I know that in this new phase of my mathematical passion, the impulse to serve is no less present than it once was. I will "serve" no less than I once did that ideal "community" of minds eager to know<sup>1014</sup> (\*), which continues to give my mathematical investments a deeper meaning than that of a personal hobby and a means of self-aggrandizement.

□ Within these investments, "the boss" is certainly no more absent than in the past. Faced with the Malice and derision on the part of those who had been "close to me" in the mathematical world, wounded many times in an elementary sense of decency by those I had loved and trusted unreservedly, there is in me this irrepressible movement, before those who have lost the feeling of respect, to **testify to my respect for myself**, through respect for these living, vigorous and beautiful things that with my hands I have brought to the light of day. Perhaps the best testimony I can give to this respect is to make myself the servant of these things for a few years. p. 1220

<sup>1014</sup>(\*) I first wrote about the "mathematical community" in the first part of *Harvest and Sowing*, in the section "The 'mathematical community': fiction and reality" (n° 10). By referring here to an "ideal community of minds eager to know", it might seem that I'm once again falling back on something whose fictional character had become clear in the above-mentioned section. But in Part VIII of *Fatuity and Renewal*, I had already been led for the first time in my life (better late than never. . . ) to the realization of a collective dimension in my own

"(On this subject, see the two sections "L'aventure solitaire" and "Le poids d'un passé", n° s 47, 50, and more particularly, pages 134, 135). It's also clear that the "community" (or "collectivity") that lives this collective adventure is of a completely different nature to any sociological entity, embodied in a given **environment** at a given time.

This "ideal community" to which I refer, "without **frontiers in space or time**", is no less "real" to me than the **sociological entity**. This "ideal community" to which I refer, "without borders in space or time", is no less "real" to me than the sociological entity. It is more essential, in the sense that it is indeed this community (as I write in the rest of the same sentence) that "continues to give my mathematical investments a deeper meaning than that of a personal pastime and a means of self-aggrandizement". It's no more "fictional" than I am myself, who feels part of it, more lucidly than I once did. The "fiction" consisted, not in the perception of the existence of such a "community", but in the confusion between it and a milieu with which I had identified myself.

on the precious years that still remain to me. So the mathematical reflections I intend to develop over the next few years, in the continuation of *Réflexions*, will still be, at the same time as the resumption of a **child's game** and the **gift of a service**, an **act of respect**.

Before putting the finishing touches to the Burial, I'd like to take a brief look beyond the "material facts" to see what this reflection has taught me. I'll start by looking at what it has taught me about others, and end with what it has taught me about myself.

The most striking fact, of all those that have come to light in the course of this reflection, is the **degradation of morals and spirits** in the mathematical world of the 70s and 80s. This degradation is expressed, among other things, by a hundred and thousand "little nothings", such as those that have come back to me in spurts over the past eight or nine years - "little nothings" that are nonetheless sufficiently disconcerting to prompt the reflection of the first part of *Récoltes et Semailles* and its main question: how (and when) did things come to this? And what was my role and what is my place in this degradation?

that I see today?

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This degradation culminates in operations such as ""SGA  $4_2$  " -  $\frac{1}{2}$ SGA 5" or the (even more incredible) *Colloque Pervers*, far surpassing in cynicism and contempt anything I could have imagined, the day before I unwittingly discovered them.

This is not the place to go back over these "nothings" (more than one of which has been pointed out in passing in my reflections, here and there), nor over the big operations (served by the little manoeuvres). The spirit expressed in both, the "nothings" and the vast swindles, is the same. The "threshold" that can sometimes be drawn between the acceptable and the villainous is itself very fragile and very artificial, a sort of guard-rail that, in any case, nobody (it seems) cares about anymore. I don't regret having had the opportunity, through this funeral in which my person is crucially involved, to take a closer look than ever, perhaps, at this spirit, which is certainly the privilege neither of this funeral (set in motion in honor of my modest person) nor of the world of mathematicians alone. I can only say that I'm not aware of this spirit having reigned in that world, or in any other science, at any time other than our own. This is a sign, among many others, of the terminal stage in the decomposition of a civilization and of what, in spite of everything, continued to give it meaning.

Over the last few days, my thoughts have more than once dwelt on the strange coincidence that my departure from the mathematical scene, over fifteen years ago, came under the impact of a certain corruption in the scientific world, to which I had long chosen to turn a blind eye (while believing I was staying away from it). I was suddenly confronted with it, in the very institution where I had intended to end my days<sup>1015</sup> (\*). In this case, it was a question of the almost universal self-interested connivance of scientists with military apparatuses. This insidious military stranglehold on the scientific world as a whole is also a recent phenomenon, having only emerged (at least to the extent we know it today) since the last world war. Certainly, if this "shock" disrupted my planned trajectory (planned by myself as by everyone else) to the point of triggering my departure without return from a world to which I had been

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identified until then (with one tacit reservation. . . ), was that there was a pres- sant and urgent need for renewal within me, which I only became aware of with hindsight. I subsequently had a □ tendency to minimize what was

had been the particular opportunity to trigger this unusual departure. Yet I also know how immense (and yet invisible) are the forces of inertia that tend to keep us indefinitely on the same "trajectory", and that oppose inner renewal - and this makes me wonder how many times I've had to face up to these forces of inertia.

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<sup>1015</sup>(\*) On this subject, see the note "L'arrachement salutaire" (n° 42), and also "Frères et époux - ou la double signature" and its sub-note (n° s 134, 134 )<sub>1</sub>

I can also tell you how powerful an inner shock it took to tear me away from a trajectory as firmly mapped out as my own.

What I'm getting at is that the "special occasion" that triggered my departure is not without **meaning**. In any case, this meaning was very strongly present in the first few months, and probably even throughout the first year, following my departure. Subsequently, under the influx of new impressions and in the very dynamic of this first, tumultuous renewal, it was only natural that this sense should recede into the background and eventually disappear from my view. But even as I cease to perceive such and such a "sense" of my past or present actions and their fruits, this sense has not disappeared for all that. And my return to mathematical activity, with the more detailed contact it implies with the world I left behind, has unexpectedly brought me back to this forgotten past. For one of the very first fruits of this "return" (a return just as unexpected as my departure had been a short while before. . . ) was the discovery, in this world that had once been mine, of another corruption, which I don't think I had ever known. If I try to give a name to this new thing, it comes to me: **the loss of respect**. I've felt this painfully more than once in recent years, when I've seen "one of those I've loved discreetly crush another whom I now love, and in whom he recognizes me". In the course of reflecting on L'Enterrement, I came across it more than once again, and in more virulent tones, this time directed against such things as I had brought into being by my hands, or against such continuators who had dared to draw inspiration from them. At such times, I've become truly acquainted with the "breath" and "smell" of this spirit, where the sense of respect has been lost. But I'm also well aware that this spirit "doesn't just breathe around my home", even though it's through its breath on me, and on those I care for, that I truly "know" it - as one only knows the taste of bitter fruit by eating it. This spirit today has become the spirit of the times... .

And I can see that these two corruptions, the one that triggered my departure and the one that awaited me at my "are not unrelated. If I try to put into words this diffuse feeling of a link, I would say that □d<sup>in</sup> the easygoing attitude of scientists towards the seductions of military money (not to

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To speak only of this aspect) and the conveniences it offers, I detect a lack of self-respect, on both an individual and collective level<sup>1016</sup> (\*). And it is in the loss of self-respect that I recognize the root of the loss of respect for others, and for the living work that has come from their hands or those of the Creator.

I don't claim to have "understood" either "corruption". On the one hand, there is the "spirit of the times", whose particular dynamic escapes almost entirely (it seems to me) individual action. For me, this collective dynamic remains a total mystery, and one I've never thought of trying to fathom. On the other hand, there's the way in which each individual being, endowed with his or her faculties of perception and creativity, and weighed down by the weight of his or her particular conditioning, responds to this spirit of time and makes this response (knowingly or unknowingly) one of the crucial elements of his or her particular adventure.

In the course of my reflection, I tried at length to identify certain choices, and the forces at work behind these choices, in the case of the two main protagonists of the Burial: the deceased, and the Principal Funeral Officiant<sup>1017</sup> (\*\*). What's certain is that I've learned a few things along the way, but by no means have I succeeded in my task. In fact, as far as my protagonist is concerned, I'd go so far as to say that I haven't succeeded entirely. I've assembled the pieces of a puzzle, I've put them together, and I'm even convinced that the pieces are the right ones and that the assembly, more or less, is correct - but the knowledge of the **whole** makes me

<sup>1016</sup>(\*) I'm sorry to run the risk of offending some of my old friends who have adopted this "easy attitude", without, of course, feeling any lack of self-respect! It's by no means certain that scientists of other eras, had they found themselves collectively faced with "seductions" of the same order, would have reacted differently. Opportunity is often the thief!

<sup>1017</sup>(\*\*) (June 22) A third "main protagonist" appeared to me, at the "last minute", in the note "L'album de famille" (n° 173), part c. (The one of all - or acquiescence), d. and e.

still missing. It remains an assembly of parts that, at present, remain **foreign to** me - foreign to my person and my experience, and, by the same token, misunderstood. The work I've done will no doubt help me, on other occasions, to recognize myself as best I can, to be careful where it's in my interest to be careful (and the older I get, the more I realize that it's often in my interest to be careful... ). But all this falls short of true understanding.

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And the question arises as to whether, in the end, the effort made in this direction was not a lure - or that the **goal** at least (that of "understanding others" in such and such a conflict situation) was ☐ not a lure (whereas the **path** followed has

rich in lessons learned... ). I tell myself that to truly understand the conflict in this **person** (or in any other to whom I've been closely linked and where I see similar contradictions erupt), is undoubtedly also to **understand the conflict itself**. And I know that such an understanding cannot come from meditating on others (who are forever beyond my immediate knowledge), but only from meditating on myself. If the long reflection on "The key to yin and yang" is to prove fertile, it's not through occasional escapes into other people's lives, but through looking back at my own life and experience, and the understanding I had of it.

## 18.7.2. The gift

**Note** 180 (April 3) I don't feel inclined, after all, to attempt a retrospective in a few lines, or a few pages, of what has occurred to me about my main protagonist in L'Enterrement. As things stand, it seems to me that this would be little more than an exercise in style, and not the means for a renewal of a most fragmentary understanding. For the moment, I'm rather looking forward to the end of this reflection on L'Enterrement!

I'm well aware, moreover, that this final point will not be the end of L'Enterrement itself; surely, the months to come, with the echoes of all kinds that will come to me from these notes, the fruits of solitude, will be rich in surprises and lessons that solitary reflection could not have brought me. Nor is it certain that all the surprises that come my way will have a bitter taste, and perhaps the very near future will also hold some joy for me - all the more appreciated as it will undoubtedly be rare; as I also had the joy, last year alone (a banner year!) of receiving letters full of warmth from three of my colleagues or friends of yesteryear whom I held in particular esteem or affection<sup>1018</sup> (\*).

As for the overall effect, however modest, of Récoltes et Semailles on the "spirit of the times" in the mathematical world, needless to say, I have no illusions whatsoever. Perhaps, at the very most, the publication of these notes will put an end to such unprecedented iniquity, and that it will make

to readjust some glaring anomaly - and even so, I may be optimistic. And it's also possible that the unexpected reappearance of the deceased himself, thought dead and done for ages, will ☐ une fin, or at least

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At least a more circumspect mute to the muffled concert of derision that surrounded the work of his hands, which he had left behind. And if this reappearance does not at the same time put an end to the fashionable boycott of a vision and of strong, fertile ideas, perhaps it will at least encourage some young mathematician, more generous than others, to draw inspiration from them without reserve (at the risk of displeasing) and to make them his own with respect.

Yet, if I wrote Récoltes et Semailles, it wasn't for any of these things, some of which may come in the future, who knows! I wrote it "for me", of course, like everything else I write - as a means to an understanding that I'm still groping for. But at the same time, the thought of others, of those I have loved and left behind one day, as my adventure took me **elsewhere** - this thought hardly left me throughout the writing of Récoltes et Semailles<sup>1019</sup> (\*). These notes, as well as a reflection,

<sup>1018</sup>(\*) These are letters from D. Mumford, I.M. Gelfand and J. Murre.

<sup>1019</sup>(\*) This thought is expressed more than once in Fatuity and Renewal (the first part of Harvest and Sowing).

and sometimes a meditation, have been and remain for me a **gift to** those to whom, beyond myself, I address myself. And I know, of course, that this gift may not be received by anyone but myself. But that doesn't mean I'll regret having done it. What's more, if it isn't received today by some of those for whom it's intended, perhaps it will be tomorrow. This testimony, at once spontaneous and long matured, where each page and each word comes in its own time and place, will be no less true tomorrow than it is today. But whether it's today or tomorrow, if there's one unforeseen thing that will be welcomed with joy, it will be to learn that my gift has been received, if only by one, who would have recognized himself through me... .

### 18.7.3. (3) the messenger (2)

**Note** 181 No more than for the "foreground" of the Burial painting, do I feel prompted to give a detailed retrospective of my insights and perplexities concerning the other two foregrounds, formed one by the "bustling group of my pupils, carrying shovels and ropes", and the other by the "entire Congregation". On the subject of the latter, and its role in the Burial, I expressed myself in some detail in the note "Le Fossoyeur - ou la Congrégation toute entière" (n° 97)<sup>1020</sup> (\*\*). With regard to

As for my perplexities regarding the role and motivations of my dear ex-students, they appear most clearly in the note "Silence" (n° 84), without being seriously re-examined at any point p. 1226

of the reflection. So it's at this level, that of the "second plane" of the Burial painting, that my work leaves the most to be desired!<sup>1021</sup> (\*). There was no work here comparable to the one I did in the note quoted "Le Fossoyeur. . . .". This part of the picture is further developed in two subsequent notes, in the light of the dynamics of yin and yang: "La circonstance providentielle - ou l'Apothéose" and "Le désaveu (1) - ou le rappel" (n° s 151, 152).

This note "The Gravedigger - or the whole Congregation", which is the last of those written in the "first breath" of reflection on the Burial, is also undoubtedly its culmination. With the benefit of almost a year's hindsight, I'm no longer convinced, however, that a certain collective motivation behind the Burial of my modest self (seen as an act of "retaliation for dissent") really touches the real **nerve of** the Burial, at the level of collective will. What makes me doubt it is that this motivation seems to me to be entirely absent, or else of derisory significance compared to other forces at play, in the case of each of my students<sup>1022</sup> (\*\*). And yet, one of the most striking facts in the whole Funeral is precisely the "unanimous agreement" that exists between its three successive "plans", whose acts and omissions follow on from and complement each other (as if orchestrated by a common will of "flawless coherence"), as perfectly as in a funeral ceremony in the true sense of the word! In such remarkable unanimity, in such uniformity of disposition and action, we can also discern a common motivation, the same "nerve" that drives everyone.

I don't mean to suggest that this "diffuse resentment" I've noticed here and there, caused by my "dissidence" felt (superficially) as desertion, and (more deeply) as an inadmissible challenge - that this resentment is null and void, and that it doesn't play a certain role. But I now doubt that this role is decisive, that this is the common "nerve" - which would be common to all **except** those whose role in the Burial was the most crucial of all! (Namely, those who were my

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It may be less apparent in later parts, but it's no less present.

<sup>1020</sup>(\*\*) (June 22) My still hazy perception of the Congregation has recently been given unexpected shape in the aforementioned note "The family album" (n° 173), parts c., d., e.

<sup>1021</sup>(\*) (June 22) For a (modest) continuation of the reflection on the "second plane" of the painting, see the note of June 19.

"Five theses for a massacre - or filial piety" (n° 176).<sup>7</sup>

<sup>1022</sup>(\*\*) This fact appears in the reflection in the note "Patte de Velours - ou les sourires" (n° s 137), p. 644-645.

students, and thus the first guardians of a certain heritage).

This (seemingly relatively rational) "cause", which is my "dissidence", seems to me to have nothing in

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common, however, with the breath of violence that ☐ I felt in an operation like the massacre of a  
It's not that I was a "splendid seminary", under the complacent eye of the Congregation; nor is it that I could  
compare with the equally violent ini- quity of a Colloque Pervers, to the applause of the assembled crowd.  
Nor was it that I was an odious colleague or boss, and so feared that the accumulated animosity he provoked  
was discharged while he was around; that it waited until he was declared dead and buried to finally be  
discharged against him and those in whom he was "recognized" in the slightest. Nothing, in the echoes that  
reach me here and there, goes in the direction either of a **fear** that my person would have inspired and which  
would have found its belated revenge<sup>1023</sup> (\*), nor of acts or behaviors the least bit precise, of which one  
would **reproach** me and which could nourish animosity or violence (which however never says its name).

This is a typical situation for the kind of violence I've called "gratuitous", or "causeless". If this kind of  
violence has ended up at the center of my attention, in the long meditation "The key to yin and yang"  
(which itself constitutes the heart of Harvest and Sowing), it's surely not by chance. I don't just know this  
violence from yesterday, and it wasn't in my life as a mathematician that I was confronted with it for the  
first time, face to face. And if I've sometimes forgotten its existence in the world of men, it's never been  
for very long, because it's taken care of itself soon enough to remind me of it. And to talk about today - by a  
strange "coincidence" and (I'll say it myself)

often unwelcome (or at least, unwelcome. . . ), I don't recall ever having been confronted in my life with the  
☐ familiar signs of such violence in such an insistent, repetitive, harassing way, that

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since my "return to maths" and especially since I wrote Récoltes et Semailles; and even more strongly, in  
these very last months and weeks.

Surely, there's an insistent message here, one that comes back to me again and again, and will no doubt  
keep coming back until it's heard. I began to listen to it, in the final weeks of the long meditation on yin and  
yang - knowing that I hadn't yet reached the end of what it had to say. In the two months since then,  
however, a subterranean work must have continued in silence. It seems to me that what is essential and  
hidden<sup>1024</sup> (\*) has begun to unravel into more incidental things.

<sup>1023</sup>(\*) It's true that, in "Fatuité et Renouvellement", I spoke at length about the **fear** that surrounded the "man of notoriety", from a  
moment I couldn't place, and whose signs I sometimes perceived around my person. But this was the diffuse fear attached to  
notoriety, not to my person - it disappeared as soon as a slightly personal contact had been established. I have the impression  
that, in terms of personal contact, I was perceived more as the "good guy" than as the person who would be feared.

It was no different, I'm convinced, even for the student mentioned in the section "The blunder - or twenty years later" (n°  
27), in whom a certain "stage fright" continued to manifest itself for quite a long time, with each new encounter. This stage  
fright appears to me today as the sign of a pervasive inner insecurity ("Unsicherheit"), which later  
found compensation and an outlet in attitudes of domination and contempt. Among his many students, the three I've come to  
know have each been severely tested by his apparently "gratuitous" attitudes of malice. Clearly, the spirit that has taken root and  
reigns just about everywhere in the mathematical world has encouraged the emergence of such aberrant behavior, which in  
turn contributes to shaping this spirit and imprinting it with the disconcerting mark of hushed brutality... ..

<sup>1024</sup>(\*) As I wrote this line, I was aware that the term "hidden" here was a pis-aller, a kind of concession to "Consen- sus". I've often  
found, on discovering something I'd ignored all my life, that it wasn't "hidden" at all, but on the contrary in plain sight, so  
obvious it was sometimes eye-popping, without my consenting to see it. This is most often the case in the discovery of the  
new, whether it's mathematical work or self-discovery. The cause for such blindness, for this blocking of the faculties of  
common sense or elementary intuition, is by no means a deficiency of these faculties. Rather, it lies in an almost  
insurmountable inertia of the mind to deviate from the rut of well-established consensus - whether this is accepted in society as  
a whole, or in the more limited milieu of which we are a part, or even, whether it is concluded and sealed only within  
ourselves, like the articles of a treaty that the "boss" would have concluded with himself and for his own convenience alone... ..

(or, at least, less difficult to admit). The image of the "dwarf and the giant" (provided by my friend Pierre) has continued to haunt me. Behind this image, I believe I detect an archetype of considerable strength, which would be like the shadow, or one of the shadows, of the repression suffered in early childhood. Its role would be that of an outlet, and compensation, for the repression of the creative force, a repression long since internalized in that "unspoken conviction of powerlessness"... . In this presumed archetype, I believe I sense a powerful driving force behind acts of gratuitous violence, striking at those perceived as "giants", as bearers of untouched strength - acts triggered without any "cause" other than that of a **propitious occasion**, when the risk involved seems nil, or minimal.

Perhaps I've already said too much, even though in these lines I've just touched on a tenuous intuition that's not so far-fetched. insistent, alerting me to a job that needs to be done, and that lies ahead of me. For this work, Burial is just one of the materials, along with many others that come to me from my so-called □ "private" life. This is not the

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instead of pursuing it, or just touching on it. It doesn't belong in notes intended for publication.

#### 18.7.4. (4) Paradise lost (2)

**Note** 182 (April 4) In this promised retrospective of what my reflection has taught me about others, my thoughts, as if in spite of myself, return insistently to my own person. For me, this is a good sign - a sign of my strong need to return to what is essential. It's from knowing myself that I gain an understanding of others, not the other way round. And on more than one occasion since I've been meditating, the preoccupation with "understanding others" has been a diversion from the essential task of getting to know myself.

Before deliberately coming back to myself (and against my impatience to reach the famous "period"! ), I'd like to include one more testimonial that came to my attention recently, concerning my friend Pierre. It's the only testimony of its kind I've heard since I left the mathematical scene. It sheds a very different light on my friend than those I know of elsewhere. It also serves as a timely reminder that reality is constantly more complex and richer than the images I try to conjure up for myself<sup>1025</sup> (\*).

The testimony in question is not direct. It's about the impressions of a (more or less fortuitous) meeting between a foreign mathematician and Deligne, which this colleague told (still hot, I presume) to my correspondent, who passed on the story to me in a letter. With the permission of my correspondent and the colleague (whom I'll call "Z" in the following) who gave him the account, I give here the translation of the part of the letter concerning this encounter. My correspondent assumes that the scene must have taken place in 1981 (NB that's also the year of the Colloque Pervers, a colloquium that had not been discussed between my correspondent and myself).

□ "... One day Z. had gone to Bures for a conference, and found himself there in a room ["la salle du thé" at the IHES, obviously] where tea was served, and where there were a lot of mathematicians. Then the door opened and Deligne entered the room. Monsieur Z. describes the scene quite vividly: he looked flabby, his arms were flailing, and there was a certain iso- lem around him. All the others seemed to be staring at him, a bit like a rare bird, with no

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<sup>1025</sup>(\*) I don't mean to suggest that the effort we make (and that I constantly make myself) to form an image of reality, as "fi del" as possible, and to adjust this image to the fi le of "information" of all kinds that comes our way - that this effort is vain or sterile. On the contrary, it's a highly effective dialectic that puts us in touch with reality and enables us to "know" it. Only insofar as the image (burdened, by the nature of things, with an inertia of its own) remains entirely inert, fi gured, does it also become an obstacle to the apprehension of reality, or better put: an effi cient **means of** thwarting our faculties of apprehension, and of "evacuating" the knowledge we do have of reality.

that no one knew how to tell him anything. Z. was sitting a little apart, near the window, and Deligne, rather indecisive, sat down next to him. Z. wasn't sure what to say. Then the thought came to him to say simply, how extraordinary he found the set of ideas around "étale topology" etc., and the new ideas you brought. ["You", here and in the following, means me, Grothendieck, to whom my correspondent is addressing himself]. Immediately Deligne's eyes began to shine, he told him, yes, this is one of the best things there is in mathematics; and how beautiful it was, listening to your<sup>1026</sup> (\*) lectures. . . and he recounted: just think about this, and that... .. enumerating a lot of things where Z. didn't understand anything (according to what he told me himself), but he could see the enthusiasm, which had suddenly appeared in his interlocutor. And Deligne added: "What a pity, that you(\*) have withdrawn! He was sure that crystalline cohomology and many other things would not be in this rather re-barbed state, but would now be standing constructions just like stellar cohomology, if you<sup>1026</sup> (\*) had really tackled it again... . "

Two things struck me about this story. There's the sense of isolation, which seems to have hit Mr. Z hard. I'd be hard pressed to say whether this impression stems from a very particular moment in Deligne's life, or whether such isolation has come to permeate his relationships with all his fellow dogs. I've heard no other evidence of the latter.

The other striking thing, and also unique among the echoes that came back to me, was the sudden appearance of this enthusiasm, this warmth, at the mention of my name and a certain past. It was a past he had long since decided to declare null and void. And the roots too, which he had in this past. And in that past, too, there was still a freshness of childhood, that freshness he'd banished from his life as an "adult" <sup>p. 1231</sup> □ an important and admired man. It must have been part of the etiquette, around him, not to do allusion to his past, when he was just another student in love with a beautiful passion... . - nor in the home of the well-to-do man, surrounded by stylish furniture, is there any talk of modest, even laborious beginnings... ..

And now this stranger, sitting next to him by the greatest of coincidences, suddenly starts talking warmly, as if it were the most natural thing in the world, about something no one ever talks about (not in front of him, at least. . . )! Surely, it was as if the selective, staid atmosphere had suddenly vanished, and the warmth of a stranger awakened the same warmth in him, and - for the space of an instant - linked him once again to a distant, raw source forever forgotten and lost...

## 18.8. Discovering a past

### 18.8.1. (1) first breath - or the observation

**Note** 183 At last, I've come to the most personal part of this retrospective-balance sheet, which I started over a month ago. It remains for me to briefly review what this reflection has taught me **about myself**.

The first thing that reflection led me to discover was a certain **past** - my mathematical past, which I had never previously bothered to dwell on, even for a moment. Behind the apparent platitude of an unproblematic surface, I once again saw the depths of everything that is commonly overlooked, removed (as if by a well-aimed sweep) from the comfortable conscious image we're accustomed to forming of ourselves and our surroundings. Among the

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<sup>1026</sup>(\*) As before, "you" here refers to me, Grothendieck.



In addition to the "burrs" (or sweepings. . . ) never examined, at least not in my life as a mathematician, there is the insidious, and sometimes pervasive, action of fatuity in the relationship to such of my friends. Right from the start, this fatuity had taken the form of a kind of mathematical elitism, which remained unspoken and of which I was totally unaware, so much so did my attitude seem to go without saying. This elitism (or "meritocracy", as Chevalley and Guedj called it) must have hardened over the years. It crystallized into the "sporting" attitude I came to discover towards the end of the "first breath" of reflection. Underneath its good-natured exterior, this attitude sanctioned a jealous possessive attitude towards what were perceived as "guarded possessions".

for myself, and for those I was pleased to welcome, given their brilliant qualities.

□ These very "boss" provisions do not, fortunately, exhaust the content of what was, between 1948 and 1970, my relationship to my friends, colleagues and students in the mathematical world, or to mathematics itself - far from it. Nevertheless, they constituted an insidious background note, which I never bothered to note until last year, in the first part (or "first breath") of *Récoltes et Semailles*. This gradual discovery culminates in the section "La mathématique sportive" (n° 40). This seems to me to mark the moment of a qualitative change in thinking. I felt it in the moment, like the **crossing of a pass**, which opened up a sudden escape into a new panorama. ...

With the hindsight of yet another year, I now see this first long period of my life as a mathematician among mathematicians, between 1948 and 1970, as a kind of **barter** of the "birthright" that belongs to me (as it belongs to everyone), to live fully (if I so choose) a particular and unique adventure, against the "lentil dish" of an identification (that I would have liked without reservation, without ever quite achieving it...) with an idyllic and fictitious "mathematical community", and at the same time dispensing comfortable advantages. . . ) with an idyllic and fictitious "mathematical community", at the same time dispensing comfortable advantages<sup>1027</sup> (\*). With this image, I don't pretend to have said everything about this period, which is certainly too rich to be encapsulated in a cookie-cutter formula. But the image does seem to capture an important aspect that first appeared in this first phase of reflection. This aspect reappears in the name "Fatuity and Renewal" that this part of Harvest and Sowing took on (after the fact).

The most personal and profound part of this first phase is formed by the last three "chapters"<sup>1028</sup> (\*\*) VI to VII: "Récoltes", "L'enfant s'amuse" and "L'aventure solitaire". In "Récoltes", I first reconnect with certain moments in my life (not just my life as a mathematician, this time) - moments charged with the power of renewal. It was as if, moved by some unknown force, by some secret, imperious voice, I sought to rediscover those same **innocent** dispositions', □ to cross the threshold that I still felt obscurely ahead of me. Although I couldn't have predicted it at the time, of course, I still had to make the discovery of a possessive attitude towards mathematics itself. I continued up a slope, unhurried and unhesitating, as if my feet were following an invisible path that only they "saw". I knew, without having to tell myself, that it was taking me where I needed to go, as little by little, step by step, the mists dissipated.

That's how I reached this new threshold in my journey, or **pass** rather:

". . . And as soon as I reached this point, I had the impression of someone arriving at a belvedere, from which he could see the unfolding landscape he had just traversed, of which at any given moment he could only perceive a fragment.

<sup>1027</sup>(\*) This is the ambiguity referred to in a previous b. de p. note (note (\*) on p. 1219).

<sup>1028</sup>(\*\*) Of course (and as I make clear in the Introduction to R et S), these "chapters", grouping consecutive sections linked by a common theme or by particular affinities, were introduced as an afterthought, once the writing of what was to be (only) the first part of *Récoltes et Semailles* had been completed. In *Fatuité et Renouveau*, I occasionally refer to them as "parts" of R and S (not to be confused with the five parts of "Fatuité et Renouveau" etc., in which all the thinking from February 1984 to the present day has been grouped).

portion. And now there is this perception of expanse and space, which is a liberation. ... "

As soon as I reached this sensitive point in my thinking, it deepened into a meditation on myself. Already the next day, I felt the need to introduce the image of the "boss" and the "worker", aka the child, an image that had become familiar to me two or three years earlier. But little did I know how useful it would prove to be in the reflection still to come, when for almost two months now I'd been thinking I was about to come to an end, only to get right back to my mathematical notes with "A la Poursuite des Champs"!

In the four sections that make up the "chapter" "L'enfant s'amuse", I'm back in touch with certain aspects and twists of my relationship with mathematics. I had already probed them at length some three years earlier (between July and December 1981), but had had ample time since to forget them. My aim this time is above all to put myself in a position to probe the meaning of my unexpected return to a long-term mathematical investment, and to find a "place" for myself between the two seemingly mutually exclusive passions that now dominate my life: mathematics and meditation.

This mutual "exclusion" of these two passions now seems less draconian than it did two years ago. In "A la Poursuite des Champs", mathematical reflection sometimes gives way, or even becomes the occasion, for a somewhat personal reflection, where my person, as a gifted being of sensitivity and feeling, of curiosity (not just mathematical) and destiny, is no longer entirely absent. And in the opposite direction, in this reflection on myself that is Récoltes et Semailles, this reflection even brings me back in touch with old mathematical loves, and becomes the occasion here and there for the beginnings of mathematical reflection<sup>1029</sup> (\*).

It's possible that the possibilities of coexistence, or even symbiosis, between these two different expressions of the drive for knowledge in me, must, by the very nature of things, remain rather limited. But it was clear to me, in any case, during last year's reflection (and indeed, already since the long meditation pursued three years before), that these two passions are by no means antagonistic in nature, nor even different in essence. In the last part of this reflection, "L'aventure solitaire", I try to pinpoint exactly how these passions differ, and the "adventures" they open up for me. It's during this interrogation that I discover an obvious fact, which I'd pretended to ignore all my life: that mathematics is "**a collective adventure**", and that my own mathematical adventure only takes on its meaning through its links to the wider collective adventure of which it is a part.

To tell the truth, I only touched on this fact in passing, in the "Lonely adventure" section, whereas my aim at the time was rather to put into words something that was well known to me on the other hand, and which I continued to have difficulty in fully accepting: that meditation, for its part, is a **solitary adventure**. This effort to formulate something "known" was certainly not in vain, far from it! It helped me to deepen this knowledge, while at the same time helping me to discover the obvious and new (to me at least) fact of the link that connects me to **another** adventure (from which at that moment I would have liked, or someone or something in me would have liked, to distance myself... ), the mathematical adventure, which is a collective one.

The stage is now set for me to get to the heart of my perplexities the very next day, in the section entitled "Observation of a division". First of all, it's the observation that the "boss's bet", even if he'd like to delude himself (as would be his nature. . . ), can only be a collective adventure.

- the only one likely to bring him substantial "returns". "The child who is alone by nature is lonely.

the lonely child who can attract an adventure no one else in the world wants, and an acquaintance,

<sup>1029</sup>(\*) (May 10) These "food for thought" have already borne fruit, renewing my understanding of certain themes that have been neglected for fifteen years.

tangible and often self-evident, but which he won't be able to share with anyone else. And that's where the "kid's preference" in **my** "company's" case now lies, quite unhappily at the whim of the "boss".

This led to the discovery of a **division** within me, **the boss-child division**. It's the first time I've made such an observation in a state of extreme attention and rigor. It's not a **decree** that I've formulated in accordance with this or that "way of seeing" or philosophy or whatever, and which claims to be more or less universally valid. It's a simple **observation**, the result of a careful examination of a very specific case, that of my modest self, at a certain stage of my development. Perhaps this division will disappear one day, without the boss having to stop doing what's necessary, while leaving the child-worker to work as he pleases. That's not my concern today, nor should it be. One day at a time...

(April 5) It's true that this division was revealed to me nine years ago, in a dream, through a parable staged with overwhelming force. It was two days after I had discovered meditation, this long-ignored power within me, at my disposal at any moment - and it was by getting to the bottom of the meaning of this dream that I rediscovered that in me which is not divided, **the other** in me, so long silent and invisible, "a very dear being, believed dead for a long life...". The new thing, the essential thing that appeared then, was **not** the division, which I knew only too well, nor what the dream revealed to me with such force about the nature of this division, incarnating itself in two familiar and beloved beings, neither of whom had a name and yet were **the same** - but it was this **reunion**, coming after four hours of intense meditation, like the intense labors of childbirth.

I knew then, and in the days and weeks that followed, that this reunion was not the end of the division. But thanks to them, I saw this division with new eyes - as something important, of course, but all in all "accessory" to another, more essential reality, that of an **undivided** unity,

indestructible, of that in me which I had rediscovered, and later recognized as "the child". This was present then in a very vivid and acute way. It became blunter in the years that followed have followed, in that knowledge of this "accessory" yet very real and tangible division has tended to be glossed over. While "the boss" had allowed himself to be drawn into "betting" on meditation (the famous "three-legged horse". . . ), he was keen to suggest (without having the audacity, or the awkwardness, to ever say so in plain English. . . ) that with meditation and all that, division was now a thing of the past. ) that, with meditation and all that, division was now a thing of the past, that there was no division at all, that there was barely a blunder here and a blunder there, but that it was almost as if there were none at all; just look at the kid-worker so happy to have a good time, and the boss-cake tiptoeing around so as not to disturb him-the real idyll, I might add! I wonder if last year's reflection, the one before the turning point (with the "sports mathematics"), especially where I'm doing a very unexpected retrospective on "my passions" (in the section of the same name, n° 35), isn't still a little in those tones, where the lighting forces a hint of pink... ...

In any case, this "observation of a division" put me right back in touch with a reality I'd tended to lose sight of for many years. At the same time, it gave me a new perspective, new eyes, on the division I had seen so clearly eight years earlier. I can say this without the slightest reservation or doubt, because I remember well that at the time of this "realization", there was no association with the reunion episode, and with what it had taught me about a certain division and its nature! This association didn't come to the fore until a short while ago, when I picked up the thread of the previous day's notes. This just goes to show the extent to which the content

double□cognition

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The "accessory" (and undesirable!) content of the knowledge that appeared during this episode, was retracted. This must have been all the easier, given that at the time, and after the crucial turning point of the reunion, there was no reflection on this content, and that the image (which emerged years later) of the "boss" and the "worker-child", perhaps best suited to expressing this content, was still lacking.

It now seems to me that it's this renewed "realization" of the division that represents the most important thing in our lives.

p. 1237 important thing I've learned about myself in this first part of Harvest and Sowing. This observation fits into a few lines of one of the shortest sections of that part of the reflection. We pour<sup>1030</sup>rait penser que si had it come to this, there might have been no need to go on for a hundred and fifty pages about the arcane manifestations of fatuity in my life as a mathematician. Nothing could be further from the truth, in terms of common sense. But it's also true that this "common sense" is in no way suited to apprehending the delicate and profound paths of a work of discovery, be it self-discovery, or the cruder<sup>1030</sup> (\*) work of mathematical discovery. I firmly believe that in this long reflection on Harvest and Sowing, everything comes in its own time and place, prepared and matured by all that has gone before.

## 18.8.2. (2) Second wind - or the survey

**Note** 184 (April 6) With this brief observation of a division, towards the end of March last year (a little over a year ago), I thought at first that I had completed the Harvest and Sowing reflection. Little did I know that five times as many pages were still to come! In the days that followed, I kept myself busy with other things, and my thoughts began to return to mathematical themes. However, one "small point" still lingers in the back of my mind. Beyond a perplexity that might have seemed purely a matter of detail, I must have had the vague feeling that I hadn't really got to grips yet with the forces at work in "tipping" the pattern towards a long-term mathematical investment. Or, if I had uncovered the essential springs, my understanding was still pale and fleeting, for want of having "laid" enough on the thing for it to penetrate further. This "last petit point" was to become the means by which I would return to what remained imbued with an impression of vagueness. This resumption of reflection took place in the section<sup>1030</sup>which was then (and for three more weeks)

p. 1238 which was supposed to close Récoltes et Semailles, and which immediately took on the name "Le poids d'un passé" ("The weight of a past"). This name expresses the unexpected discovery of the **weight** of my past as a mathematician, as well as the strength of the link that continues to bind me to the collective adventure. And yet, what I glimpsed that day was only the modestly-proportioned tip of an iceberg, the colossal submerged part of which would gradually appear, over the course of the months and the whole year that followed. ...

This section, which brings this first breath of reflection to a close, is at the same time like a start and a call for the second. This "weight of a past" is obviously rooted in my attachment to a work, and even more than to the finished work, to the attachment to ideas and visions whose fecundity and power I feel and "know" intimately, and of which I am more or less aware.

<sup>1030</sup>(\*) If the work of scientific discovery seems to me to be "cruder" than that of self-discovery, it's for two reasons. On the one hand, it involves only our intellectual faculties, i.e. an infinite part of our being (scientific work tends, moreover, to hypertrophy this part of our faculties, at the expense of the others and of the overall balance of the person, and ultimately to transform the latter into a kind of monster-computer. . . .) On the other hand, the inner resistances (opposed to the discovery of reality) brought into play by scientific work are often out of all proportion to those opposed to self-knowledge. This is also why the "scientific adventure" is rarely, if ever, an "adventure of truth" - an adventure, therefore, that calls on our capacity for humility and courage to accept an unwelcome truth, first from ourselves, and then from the outside world.

less confusingly and for years now, that they have been vegetating in an ungrateful and arid land, secretly and insidiously hostile. ... So this reflection, "The Weight of a Past", which reminds me of both the work and my links to it, becomes the occasion for a long note in which, for the first time since my "departure", I express myself on the subject of this work and the fate that has befallen it. What had been felt in a vague way for ten or fifteen years, finally takes shape and manifests itself in words, sometimes hesitant to come, and which, once written in black and white, clearly tell me a message that until then I had avoided hearing. Later, given the length of this note written in one go, I subdivided it into two, with the names "My orphans" and "Refus d'un héritage - ou le prix d'une contradiction" (n° s 46,47).

This double note can be seen as the kick-off for the reflection on the Burial<sup>1031</sup> (\*). This was followed three weeks later, on April 19, by the emotional response to the "me- morable volume" LN 900, consecrating the exhumation of the motifs under the leadership of the "new father" Deligne. This "second wind" of reflection continues intensely until late May - mid-June, when it takes on a new dimension. end (just as I think I'm about to put the period again, the real period!) by □l' épisode-disease<sup>1032</sup> (\*).

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This second wind is not, strictly speaking, a reflection on myself or my past, but rather an "investigation" into the Burial that I had just discovered, as well as an effort to "digest" as best I could and as I went along, the obvious and yet (given, no doubt, my ineradicable naivety!) mind-boggling, unbelievable facts. If it taught me anything about myself, it was the strength of my attachment to my past and my work. I was touched to the core, seeing the work torn to shreds, some pieces for the dustbin, others to be laughed at, and still others shamelessly appropriated, like trifles for all to see... .

I knew then that I wasn't "off the merry-go-round" yet, as much as I'd believed in the exultation that had followed the crossing of a certain "pass" and the vast panorama that had then opened up before me<sup>1033</sup> (\*\*)! Or to put it another way, I was then able to measure the full **weight of** that past, and the strength of the egotistical mechanisms that continue to bind me to it. It was a great surprise!

Yet there's something else about myself that I'm discovering in this second phase of reflection, which completes what I'd learned in the first. In the latter, I had uncovered above all a certain "other side" of an attitude of fatuity within myself, through attitudes of **exclusion** towards such colleagues or even friends whom, for one reason or another, I didn't place in the world of the "elite" of which I myself felt part (tacitly, of course!). **The other side of the** same coin is an attitude of **complacency** and ambiguity in my relationship with younger mathematicians (and, in particular, with my students), whom I had co-opted as being, so to speak, part of "my world"; either because of their brilliant means, or simply because I had accepted them as students and they were therefore perceived by me as being under my "protection". I'm beginning to put my finger on this attitude in the note "L'ascension" (n° 63') of May 10, followed by the note "L'être à part" (n° 67') of May 27, one and the other devoted to my relationship with my young and □brilliant friend Pierre. This reflection is deepened in p.

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note "L'ambiguïté" (n° 63") of June 1, in which she focuses on my relationships with my students in general. This is where

<sup>1031</sup>(\*) Unfortunately, this circumstance does not appear in the table of contents for Burial I (or The Robe of the Chinese Emperor), where the double-note in question forms Cortège II (The Orphans), and not Cortège I (which is The Posthumous Pupil). This is due to the sequence of references to the "notes" (n° s 44 to 47) within the final section "Le poids d'un, past" (n° 50) from Fatuity and Renewal, the section on which these notes are intended to comment.

<sup>1032</sup>(\*) On this episode of illness, see the two notes "The incident - or body and mind" and "The trap - or ease and exhaustion" (n° s 98,99).

<sup>1033</sup>(\*\*) This exultation is expressed in the section "Fini le manège!" (n° 41), and is muted five or six weeks later, in the note "Un pied dans le manège" (n° 72).

that I'm finally uncovering a certain ambiguity which, because it had never been spotted by me and examined, had followed me into recent years. I was recently confronted with this ambiguity again, in a slightly different context, in the sub-note "Eviction (2)" (n° 169<sub>1</sub>) (in the second part of the sub-note, dated March 16). In it, I note that the eviction of my person from the SGA seminar (which represents the sum total of ten years' investment in my life)<sup>1034</sup> (\*), an eviction carried out mainly by some of my closest former students, is simply the natural outcome of an ambiguous attitude I had taken pleasure in maintaining with them, concerning their rightful place and mine in the vast SGA work, in which one or other of them had invested a year or two.

### 18.8.3. (3) third wind - or the discovery of violence

**Note 185** It remains for me to review what the "third breath" of reflection has taught me about myself, beginning last September 22 (after the end of the illness-episode) and about to come to an end<sup>1035</sup> (\*\*). Above all, I'm referring to the reflections in "The key to yin and yang", which I consider to be the most personal and profound part of Harvest and Sowing. Without any deliberate intention, it is my person and my relationship to the world that is most often at the center of attention. When this attention seems to wander from time to time, to seemingly more general themes, or to linger on the person of my friend Pierre, it always returns to the center, however, to the actor-observer, the one who feels, perceives, questions and probes, as if drawn by an invisible force. Above all else, and without wishing to be, it is a **meditation on my life and myself**, approached through an unexpected angle: that of the funeral.

This is also the part of the reflection that seems to me the richest, the one through which I learned the most. Many "known" things have situated themselves in relation to each other, and things that were only glimpsed or hinted at, or "known" but neglected, drowned in the confused penumbra of the tout-venant, began to emerge from the shadows and reveal both their weight and their contours. It was like a new opening, an invitation to a great new departure into the unknown - at a time when it had seemed that this famous "long-term mathematical investment" was going to put an end to the work of self-discovery for years to come. ...

It's not my intention here to review in detail the various stages of this long reflection, nor to make a "list" of all that it has taught me. Instead, I'd like to say a few words about what seems to me to be the most important for my self-knowledge, as material for a maturing process that continues day by day, month by month and year by year.

This reflection began in the spirit of a "parenthesis" I was opening (for the space of a note or two at the most. . . ) to get the reader (and myself) "into the swing" of a dialectical yin-yang (or "feminine-masculine") vision of things. The reason for opening such a parenthesis was the need to situate, in terms of an intuition of yin and yang, a striking impression that

<sup>1034</sup>(\*) For the latest episode in this eviction, see the note "Les Pompes Funèbres - "im Dienste der Wissenschaft" (n° 175).

<sup>1035</sup>(\*\*) I'm setting aside here the fifth part of R et S, which was originally a "digression" within the Funeral Ceremony (or even, within "The Key to yin and yang"). This part has not been completed at the time of writing, and is not included in this retrospective on Harvest and Sowing.

(June 22) Over the following weeks, it became clear that the part of the reflection entitled "The Four Operations" (or Burial (3)), following "The Key to Yin and Yang", constitutes a "fourth breath" of Harvest and Sowing, which is not included in this final retrospective.

had given me the opportunity to examine a certain "Funeral Eulogy"<sup>1036</sup> (\*): that of a deliberate "reversal" of roles in an original yin-yang relationship. This "parenthesis" opens on October 2. It was only on November 10, after a hundred pages of close reflection on the interplay of yin and yang in my life in particular and in existence in general, and (finally) in the game of mathematical discovery, that the moment finally seemed ripe to **formulate** at least<sup>1037</sup> (\*\*) this association of ideas that had appeared six months earlier, in the expectation of being able to fathom it in full knowledge of the facts, fourteen days later still<sup>1038</sup> (\*\*\*). (And it was almost two months later, on January 14, that the famous parenthesis on yin and yang finally closed, I didn't even realize for a while that it had already closed... . )

□Very quickly and without having sought it or foreseen it, it is "the conflict" in human life and in the person that p.

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takes center stage. The egotic energy suddenly and powerfully mobilized by the discovery of the Burial, came there as an unexpected supporting force, to confront me once again, and on the spot, with the "mystery of conflict" that for years had been calling out to me<sup>1039</sup> (\*). Over the past few years, this mystery had gradually come to the forefront of things I'd wanted to probe and understand, as far as I could, without ever having "taken the plunge" and thrown myself fully into it. ...

Little by little, in the course of my reflection, I came to realize what, in my life, has been the "hard core", the formidable center of this mystery, the very heart of the "enigma of Evil": violence that can be called "gratuitous", or "causeless", violence for the sheer pleasure, one might say, of wounding, harming or devastating - a violence that never says its name, often hushed, under an air of innocent, affable ingenuity, and all the more effective at touching and ravaging - the "claw in the velvet", delicate, vivid and merciless. . . It is this violence that our attention eventually turns to, in the course of the reflections pursued in the suite of notes "La griffe dans le velours" (n° s 137-140), and it too remains the focus of attention right up to the end of the Clef. It again forms the climax, in the final note evoking the "endless chain" of karma, passed down from parents to children and from children to grandchildren, from generation to generation since the dawn of time.

This is the first time in my life that I have come face to face with the mystery of violence "without hatred or mercy". - a violence deeply rooted in human life, and which has left an indelible mark on my life since my youth. It's also the first time I've noticed this imprint on my being. At the same time, it's also the simple fact of **the existence** of this violence, of its fearsome omnipresence, in my own life as well as in the lives of others<sup>1040</sup> (\*\*). This simple fact alone also contains the seeds of an **acceptance** of this formidable fact. It is in this realization, perhaps, that lies the most important thing I□ve learned (or at least **begun** to learn), in the course of the entire Harvesting and sowing.

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This is not the culmination of a thought process. Rather, it's yet another first step, taking me beyond a threshold into the unknown. For my development and my maturation, this humble step seems to me of greater significance than the embryonic "answers" I glimpsed (in the days that followed) to the question of the "**cause**" of the "causeless violence"<sup>1041</sup> (\*). This question itself only takes on its full meaning, which is far more significant than a simple question of "psychic mechanics", once it has been answered.

<sup>1036</sup>(\*) For this "Funeral Eulogy" (with its skilfully measured and administered compliment. . . ) see the two notes of this name (n° s 104, 105), as well as the note "Les joyaux" (n° s 170(iii)) which gives a partial summary.

<sup>1037</sup>(\*\*) In the note "Le renversement (3) - ou yin enterre yang" (n° 137).

<sup>1038</sup>(\*\*\*) At the beginning of the note "Patte de velours - ou les sourires" (n° 137).

<sup>1039</sup>(\*) This "interpellation" began to be perceived especially since my long meditation on my parents, which continued between August 1979 and March 1980.

<sup>1040</sup>(\*\*) This observation constitutes the high point of the reflection pursued in the note "Without hatred and without mercy" (n° 157).

<sup>1041</sup>(\*) See note of the same name (n° 159).

the very existence and scope of the fact being investigated.

Some will say that I'm getting off the subject, that the observation of a general psychological fact (or that I claim to be such), concerning each and every one of us, falls within the realm of objective knowledge reserved for scientific disciplines (such as psychology, psychiatry, sociology or whatever), that it's not within the realm (felt to be vague and impalpable, if not entirely far-fetched) of the famous "self-knowledge". But I see (not vaguely and impalpably, but as clearly as a familiar and patent mathematical fact. . . ) that, apart from self-discovery, such a statement loses its living meaning - it loses what makes it anything other than an exercise in philosophical-psychological style, than the development of a "thesis" (very interesting indeed and all that. . . ). This observation in itself is a **discovery**, an intimately personal discovery that no one in the world can make in my place, and that I cannot make in place of any other person in the world. This discovery is a step, the last one or so, in a journey of self-discovery. It situates me in relation to something important, something dreadful, something that has left its mark on me and that I had hitherto insisted on neglecting, as if it were by some kind of particular misfortune (perhaps due to some peculiarity or other in my modest person) that I have seen myself exposed to it throughout my life, and that I have seen others exposed to it or inflicted by it, if only I took the trouble to open my eyes and look around me.

It's no coincidence, surely, that right from the start of this reflection on violence, I've been thinking in terms of violence.

I was led, by the very inner logic of reflection, to look back (also for the first time in my life) on the few cases I can remember where it was I myself who ☐ was subjecting others, and without there think twice about this "beyond comprehension" violence<sup>1042</sup> (\*). The point of this comeback is not that it gives me an opportunity to beat myself up (and in public, no less) - something I have entirely failed to do. The point is that it has opened a door to a deeper understanding of violence - a door that is now mine to cross, at a time of my choosing,

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#### 18.8.4. (4) La fi délité - or feminine mathematics

**Note** 186 This is what I consider to be the most important part of the journey of self-discovery. This final phase of the yin-yang reflection, centered on violence, continues throughout the last four parts: "The claw in the velvet", "Violence - or the games and the sting", "The other Self" and "Conflict and discovery - or the enigma of Evil", from December 7 to January 14 (which represent just over a third of the Key).

Looking back, it seems to me that the main role of the previous eight parts of the Key is to have finally brought me to this crucial point. Many of the things I develop in this preliminary part are things I've been familiar with for years, and yet which I had to "remind" to enable a "new" reader to follow, and to give the reflection an internal coherence, which might otherwise have been lacking, or apparent only to me. At times, the style feels like the inner disposition of someone who can't wait to get these reminders over with, so as to finally get to the "heart of the matter" - whereas often these so-called reminders were of a far greater scope, and worthy of my putting some thought into them, than the "heart" I was in such a hurry to get to (and which, hurry or not, I didn't get to until over a month later... ). These dispositions seem to me to be particularly noticeable in the three consecutive parts "The Couple", "Our Mother Death" and "Refusal and Acceptance". Even there, it's true, as I got back in touch with things that were supposed to be "known", I couldn't help but renew my acquaintance at the same time,

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<sup>1042</sup>(\*)See the note "La violence du juste" (n° 141), which follows the "La griffe dans le velours" section of the Key.



and in a sometimes new light - even for things as impersonal, if at first sight, as the inventory of those "doors to the world" that are each group of yin-yang couples (or "keyholes") linked by immediate affinities.

But it's with the next three parts (which also precede the last four, focusing on the theme of violence) that I once again □ tackle hitherto unexplored shores: "Yin mathematics and p . 1245 yang", "The reversal of yin and yang", "Masters and Servants".

It was in the first of these parts that the "big surprise" took place, which was to shed new light on the meaning, or at least a certain meaning, of Burial. It's about the fact that in my approach to mathematics, and more generally, in my spontaneous approach to discovering the world, the basic tonality of my being is **yin**, **"feminine"**. To put it another way, while the conditioned structure of the ego, the "boss" of my business, is yang (not to say, "macho" with a zinc strand), my original nature, the "child" in me (who is also the worker who shapes what the child discovers at play. . . .) is predominantly "feminine". ) is predominantly "feminine". And it's not this particularity alone that distinguishes my personal "style" of approach to mathematics from that of anyone else. It seems to me, in fact, that even among mathematicians, it's not that uncommon for this original background note (or "dominant") to be yin. What's exceptional in my case (it seems to me), however, is that in my approach to discovery and, in particular, in my mathematical work, I've been fully faithful to this original nature all my life, without any desire to make alterations or rectifications, either by virtue of the wishes of an inner Censor (who, in any case, has never seen anything but fire, so far would one be from suspecting a "feminine" sensitivity and creative approach in a "man's" business like mathematics!), or out of a desire to conform to the canons of good taste in force in the outside world, and more particularly, in the world of science. There's no doubt in my mind that it's above all thanks to this fidelity to my own nature, in this limited area of my life at least<sup>1043</sup> (\*), that my mathematical creativity has been able to unfold fully and without hindrance, like a vigorous tree, firmly planted in the ground, unfurling freely to the rhythm of nights and days, winds and seasons. And so it has been, despite the fact that my "gifts" are rather

modest, and that the beginnings were by no means auspicious<sup>1044</sup> (\*).

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As I make this unexpected observation about my approach to mathematics, in the note "La mer qui monte. . . ." (n° 122)<sup>1045</sup> (\*\*), it comes as a kind of unexpected curiosity, a little "on the fringe" of my life, where relationships with others all bear the mark of my yang and superyang options. It's only in the continuation of this reflection, centered on the dynamics of conflict, and on the occasion of a return to the

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<sup>1043</sup>(\*) As I've had occasion to say over and over again in the course of R and S, one of the two strongest egotistic forces that dominated my life from the age of eight (and until 1976, when I was forty-eight), was the repression of the "feminine" traits in me, to the benefit of the traits felt to be "virile". It was only during the "Key to yin and yang" reflection that I realized that this repression was not exercised in my mathematical work (nor, later, in meditation, or work of self-discovery). The original "feminine" dominance of my being was able to have a field day, in an activity generally perceived (and rightly so) as "virile" par excellence! (On this subject, see the note "The most 'macho' of the arts," n° 119.)

<sup>1044</sup>(\*) If I speak of "modest gifts", it's in no way out of false modesty. It's something I've seen again and again I was still in contact with brilliant mathematicians who were incomparably quicker than I was to grasp the essentials and assimilate new ideas, as well as in working relationships with anonymous students with no serious mathematical background, but whose curiosity and mathematical inventiveness were momentarily aroused.

I talk a little about my "beginnings" (at least, the beginnings of my contacts with the world of mathematicians, in 1948) in the section "The welcome stranger" (n° 9). It was three years earlier, however, in 1945, that my "life as a mathematician" began, with most of my energy devoted to mathematical research. Until around 1949 or 1950, the

The prospects for me, as a foreigner in France, to find a livelihood as a mathematician, however, seemed most problematic. In the event that such a possibility didn't present itself, I planned to learn carpentry, as a livelihood that might be to my liking.

<sup>1045</sup>(\*\*) See also the later note "The arrow and the wave" (n° 130).

It's now that I realize just how much my fellow mathematicians' relationship with me, and above all with my work, has been marked by this unusual peculiarity, bringing into play reflexes of reserve (if not rejection) in the face of a style of approach obscurely felt to be "out of place" (not to say unseemly). Such reactions were common in my early days in the mathematical world, but tempered in those clement times by the atmosphere of respect for others that prevailed at the time, at least in the mathematical circles where I had the good fortune to land. Later, they had to be suppressed without further ado, in view of "the power of Grothendieck's results" (to quote a letter from Borel to Mebkhout, in which these "reservations" are evoked). On the other hand, they have become the rule, and are sometimes at ease behind a certain discretion of tone (which remains *de rigueur*) since my departure from the mathematical scene, while the respect of yesteryear has eroded and disappeared a long time ago, and the person concerned (supposedly dead and buried) is no longer present to give the reply... .. This unforeseen aspect of l'Enterrement, as the symbolic burial of the "mathematical feminine" in my modest person, is probed in the two notes "La circonstance providentielle - ou l'Apothéose" and "Le désaveu - ou le rappel" (n° s 151, 152), from December 23rd and 24th, right in the middle of the meditation on violence.

□ There

is one last aspect of myself that I would like to mention again, which came to light while writing The

Key to the

yin and yang, in the last of the parts quoted, "Maîtres et Serviteurs" (which immediately precedes the turn of thought begun with "La griffe dans le velours"). It's about the "service impulse", and the leading role it has played in my choice of investments in mathematics and as a driving force at work in vast, interminable foundational tasks, which no one else after me has yet found the courage (or the humility. . . ) to take up and pursue. This aspect, present in me with exceptional strength, is eloquent testimony to the "feminine" dominance of my original nature, which has preserved itself (or even taken refuge . . . ) in mathematical activity (where no one would have the idea of going to look for it . . . ).

It occurs to me at the moment that it's even possible that this impulse contributes its part, of a non-egotistical nature this time, to this "shift" that has taken place in favour of intense mathematical activity, pushing the work of meditation into the background, for an indeterminate period of time. The latter, by its very nature, is solitary work, work which (it seems to me), unless we are deluding ourselves, cannot be seen as an investment in the service of all, or of some "ideal community of beings eager to know". It would seem, then, that there is a deep-seated impulse, distinct from the egotistical desire for confirmation or approval, an impulse expressing a person's deep ties with the species of which he or she is a part, which must be frustrated in long-term meditation work, as I understand it. And this is perhaps another cause, in addition to those (powerful enough on their own) that come from the structure of the ego (the dispositions of the "boss", in other words), which makes such work seem such a rare thing, that I'm not sure I've ever come across any trace of it in another person.

## 18.9. De Profundis

### 18.9.1. (1) Gratitude

**Note** 187 (April 7) I think I've come to the end of this retrospective-balance sheet, on what the whole Harvest and Sowing reflection has taught me. I have only excluded from this retrospective the fifth part of Harvest and Sowing<sup>1046</sup> (\*), which has not yet been completed. It began as

<sup>1046</sup>(\*) (June 22) And also, the fourth (which I'm currently writing)! See b. de p. note (\*\*) page 1240.

a "digression" in the "Key to yin and yang", a digression that eventually extended over a whole month, and materialized in a hundred pages of "reading notes" on C.G. Jung's autobiography. Like the end of this digression was still not clearly <sup>en</sup>vue, I put it off until later. I was especially looking forward to p. 1248 to bring the Burial to a successful conclusion, so that it can be written, typed, printed and sent to right and left, finally - and let there be no more talk about it!

I have a feeling that this fifth part will shed some unexpected light on this same En- terment - but yes! - through my planned examination of Jung's relationship with Sigmund Freud, who for years had been a master to the young Jung, still seeking his own path. On first reading the chapter (of the autobiography) devoted to this relationship, I saw nothing but fire - then a number of unusual things caught my attention, I went back over some of them, I went through the chapter again. Visually, this relationship is fraught with ambiguity, which Freud himself seems to have sensed strongly, and which Jung likes to ignore completely (as the first seminarist would do. . . ), blaming Freud's malaise solely on his "neurosis" (which he takes pleasure in describing in vivid colors, perhaps even a little too vivid to be entirely true. . . ). In any case, various associations have come to mind with my friend and (also) non-student Deligne's relationship with me, associations which I intend to follow up and perhaps delve into a little. I have a feeling that what happened with the Burial, in terms of the psychic mechanisms involved, is by no means a unique and atypical set of circumstances - quite the contrary! And I have a hunch that Jung's relationship with Freud may well provide further insights in this respect.

But for me, now at least, this fifth part (which may be called "Jung - or the bogging down of an adventure"<sup>1047</sup> (\*)), it's no longer the Burial, even if it has come out of it - and I'd even go so far as to say: it's no longer Harvest and Sowing! It's "I'Après" - as are the echoes of all kinds, including the green and unripe ones, that will come back to me when I send you the three parts "Fatuité et Renouvellement", "L'Enterrement (I) - ou la robe de l'Empereur de Chine", and "L'Enterrement (III) - ou les Quatres Opérations"<sup>1048</sup> (\*\*). It's going to be a thousand pages or more, once this part's finished being typed. to the net - that's quite a lot! One day at a time...

□ This hurry to get it over with and "send it on" is undoubtedly, above all, the hurry of the warhorse who smells

1249

powder, eager to get into the fray<sup>1049</sup> (\*). But perhaps, more deeply, there's also a desire to see a certain past detached from me. These "thousand pages" are a striking materialization of the **weight** of that past - and to see this work completed, right down to the last of the housekeeping tasks (the very last of which will no doubt be sending Récoltes et Semailles to the one hundred and thirty recipients already on my provisional mailing list... <sup>1050</sup> (\*\*)), it also seems to me, almost instinctively, to be the moment when I will have **shed** this weight. An illusion? Only time will tell...

And so I come to the "final agreements" before that famous "period", which for over a year now I've thought I saw before me, and which day by day, week by week, month by month has found itself pushed back, by the influx of the unforeseen claiming its place.

<sup>1047</sup>(\*) Thinking of writing "enlissement", I found myself writing "enterrement" instead. There's no guarantee that the new name suggested by this slip of the tongue: "Jung - or the burial of an adventure" won't be just as appropriate, or even more to the point, than the one I'd intended.

<sup>1048</sup>(\*\*) Do not confuse the fourth part of Harvest and Sowing, subtitled "The Four Operations", with the series of notes grouped under this name, which appear in this part (notes n° s 167'-176 ).<sub>7</sub>

<sup>1049</sup>(\*) Such provisions are already covered in the final section "The weight of a past" (n° 50) of "Fatuity and Renewal", in a slightly different light (where the "warhorse" is replaced by the bull, chasing a piece of cloth). red that you "wave in front of your nose" . . . ).

<sup>1050</sup>(\*\*) The famous "weight" will then become even more "striking", with two hundred thousand pages (200 X 1000) instead of one thousand!

What's left to say in these final agreements? There's gratitude, expressed in "thanks". This reflection is the fruit of solitude, and yet I have been helped in many ways.

The most obvious help came from Zoghman Mebkhout, in many ways too: by the patience with which he got me "into the swing" of philosophy around the theorem of the good God-Mebkhout; by the trust he showed me by sharing with me, against all odds, the difficulties and setbacks he experienced in his dealings with those who were my students ; by the help he gave me in finding my way through a dense mathematical literature, with which I had lost touch; finally, by the friendly and unreserved interest he showed, from the moment he became aware of it, in this work in which he saw me engaged, and in which he especially (I believe) perceived and welcomed the **testimony**.

p. 1250 □ 1051 I'm also grateful to Pierre Deligne, who came to see me and get to know me better. sance (last October) of the then-written part of L'Enterrement, and to let me know about his comments (\*). This visit also helped me in more ways than one.

Finally, I was helped by the good will and friendly atmosphere I found among the USTL secretaries who did the typing: Mlle Boulet, Mme Boucher, Mlle Brun, Mme Cellier, Mlle Lacan, Mme Mori. Two of them took time out of their busy schedules to do some of the typing, without accepting any payment for their work - a gesture that touched me deeply. It was Mlle Lacan, on the other hand, who single-handedly typed the entire second half of my notes for Récoltes et Semailles, with exemplary care and efficiency. To each and every one of them, I am happy to express my gratitude.

I'm also thinking of all those who, at times during the course of my work, have seemed to disturb it and my peace of mind, often in unwelcome ways<sup>1052</sup> (\*\*). Surely, these "disturbances" themselves, which at times have tested me and some of which still leave me with the residue of sadness, also have their role to play in my work, and to bring me a message that it's up to me to listen to and assimilate. When sadness or resentment resolve themselves into gratitude, I'll know that this message has been received... .

## 18.9.2. (2) L'amie

**Note** 188 For almost a year now, these ultimate chords of burial have had their own name: De Profundis! In the Introduction (I 7, "L' Ordonnancement des Obsèques") I went even further, announcing (imprudently perhaps. . . ) that it is the "complete satisfaction" of the deceased which forms "the final note and the ultimate chord of the memorable Burial". I was excusable at the time for making this prognosis (as if it were a thing of the past) - at the time of writing (in May last year) it did indeed seem a very short-term prognosis, as I thought I was on the verge of reaching precisely those final chords of the "De Profundis".

p. 1251 □ It's true that, in a far more acute way than last year (when the "second wind" of reflection was about to come to an end), I'm realizing how far I am from having really "done the trick" of the Burial, apart from the material facts alone (which I seem to "hold" to my full smugness<sup>1053</sup> (\*)). If it's true, as it seemed to me at times, that to understand the Burial is also to "understand the

<sup>1051</sup>(\*) For this visit and the details Deligne gave me, see the two notes (n° s 163, 164) forming the "Les derniers devoirs (ou la visite)" section of L'Enterrement (III).

<sup>1052</sup>(\*\*) These "disturbances" are alluded to here and there in the notes of recent months. See, in particular, the note "Le messager (2)" (n° 181).

<sup>1053</sup>(\*) (May 10) However, after these lines were written, more than a month went by "fitting in" newcomers as best we could. facts, in a score of sub-notes added in extremis!

conflict", it's likely that the time left to me won't be enough to do this "tour" - not in depth, at least.

So I can say that I'm writing this final note in a very different frame of mind from the one I was in when I wrote Introduction à l'Enterrement. Does this mean that I'm ending this reflection without that feeling of "complete satisfaction"?

I don't think so. As soon as a vision deepens, the work that gave rise to the vision and prepared for its deepening, and which may have seemed "completed", turns out to be **unfinished, with the** appearance of something "beyond" what had been done. Yet the **meaning of** work, and of the satisfaction or dissatisfaction it gives us, does not lie in its completion, nor does it depend on whether or not the work is destined to be completed. The meaning of work is in the work itself, it's in the **present moment** - in the dispositions in which we do it, in the love we put into it (or in the absence of love. . . ).

- not in some hypothetical future beyond our reach.

In March last year, before I had even discovered Burial, I wrote in the introduction (I 1, "Dream and Fulfillment", p. iv):

" . . . I leave this work with the complete satisfaction of someone who knows he has completed a job. There is nothing, however 'small', that I have avoided, or that I would have cared to say and would not have said, and that at this moment would leave in me the residue of a dissatisfaction, of a regret, however 'small' they may be."

I know now that this work, which I thought was "finished", is not yet, and may never be. But I also know that this is, all in all, an incidental thing. This "satisfaction complete", which I felt strongly at the very moment I was writing these lines that attempt to define it.

as closely as possible, she followed me throughout the writing of Récoltes et Semailles. She's an old friend of mine, who

p. 1252

had already accompanied me throughout my life as a mathematician, letting me know in a low voice that I was on the right track. I found it again later, in my meditation work - it's the same.

When I stop hearing it, the work loses its meaning. That's why his voice is so precious to me, and why I take great care in my work never to stray from it. It's because of this that work has been a source of joy throughout my life, in the "complete satisfaction" of those who give their all to it.

It has been no different in the work that is coming to an end - the work that is "**Harvest**", and at the same time "**Sowing**".