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 end³¹³ (*).) 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 Reflexions³¹⁴ (**). 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

³¹⁴(**) (March 26) Finally, these reading notes will form (not the fourth, but) a fifth and final part of Récoltes.

635

p. 775

p. 776

³¹³(*) (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").

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

p. 777 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 Semailles³¹⁵ (*)) from taking indiscreet photos!

I'd first have to **put** this visit **in context**. I had originally intended to visit Pierre at his home³¹⁶ (**) 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 Renouvellement", 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

³¹⁵(*) 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!

³¹⁶(**) 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

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, $and \Box$ 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

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 years³¹⁷ (*)!), 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

p. 781

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....

Anyway, after this unsuccessful attempt on my part to "get off the rails", the conversation turned to short. Those minutes were I think the only³¹⁷ (*), 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.

³¹⁷(*) 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"³¹⁸ (*). 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 note³¹⁹ (*). 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.

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**

³¹⁸(*) See "Two turning points", n° 66.

 $^{^{319}(*)}$ See the note "Dotting the I's" (n° 164) which follows this one.

still[®]couldn't believe it

³²⁰(**) ! Incidentally, this isn't the first time something like this has happened to

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 disappear³²¹ (*). Any doubt is the unmistakable sign of work that needs to be done.

p. 784

 \Box 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.

³²⁰(**) 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.

³²¹(*) 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 Bernstein³²² (*), out of **scruples** (!) towards Kashiwara, not being sure (as a non-specialist) what was the share of one and the other in the said theorem³²³ (**) - 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 dispute³²⁴ (*)), but that his name is rigorously absent from the Colloquium Proceedings published in Asterisk³²⁵ (**). 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".

□ 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.

p. 785

p. 786

³²²(*) 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".

³²³(**) 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 defi nite the functor from a triangulated category of "continuous" coeffi cients (complexes of differential operators) to another formed of "discrete" (constructible) coeffi cients-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, we r e quick to bury after I left in 1970.

³²⁴(*) 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 h appened. 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.

³²⁵(**) 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. \Box legal action against Springer, forcing them to withdraw the pirate volume SGA 4 from circulation. ¹

published in Lecture Notes³²⁶ (*). 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 4¹ 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 copies¹ - to which I $_2$ retorted that all he had to do was change the cover, as he'd already done on another occasion and without any problems³²⁷ (**), 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.

```
p. 788 d
```

p. 787

On the contrary, to list the **obstacles** to doing so), but first of all to be clear about what you want to do.

³²⁶(*) On this volume, see in particular the four notes "Le compère", "La table rase", "Le feu vert", "Le renversement", n s° 63", 67, 68, 68'.

³²⁷(**) 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^{1} . 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"³²⁸ (*)).

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^{1} - SGA 5 operation". It's surely no coincidence, then, that 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 myself³²⁹ (**), 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.

 \Box 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 4¹", 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 4¹, it was only to indicate a relationship of **logical** dependence, not anteriority.

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 4^1 "), 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^{1} - 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.

p. 789

 $[\]overline{^{328}(*) \text{ On}}$ this subject, see the note "Le renversement" already quoted, n° 68'.

 $^{^{329}(**)}$ See the note "The massacre" (n $^{\circ}$ 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 \Box

p. 790 supplied by Deligne.

I Motifs ("Lecture Notes 900" volume).

1. Deligne told me that the main purpose of the LN 900 volume³³⁰ (*) had been to develop a "theory of the **motivic** abelian class field" on a number field $K \subset C$, a finite extension of 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 κ in the field of complexes 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 Gal(K/K) by the projective limit pro-tore of the (tores on ϕ associated with the) multiplicative groups L* of the finite subextensions L of 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)³³¹ (**), 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 absent³³² (***).

p. 791

2. Deligne pointed out that, contrary to what I had thought was true (according to a certain "mai- son 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 Milne³³³ (*). Deligne also explained to me the error in Saavedra's work, which obliged (if you wanted to have the

 ³³⁰(*) 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.

³³¹(**) This is R.P. Langlands' article "Automorphic representations, Shimura varieties and motives. Ein M\u00e4rchen Corvallis", in Proc. Symp. pure Math. 33 (1979), AMS, vol II P. 205-246.

³³²(***) (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))

³³³(*) 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 theorie of a Galois-Poincaré groupoid³³⁴ (**)). 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).)³³⁵ (***)

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 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 C, ran into $\frac{\text{serious} \square \text{ses}}{\text{difficulties}}$, 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, Meb- khout's point of view and Mebkhout's bundles³³⁶ (*) 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 *I*) 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 vir- tual notion I was working with should correspond to a canonical **filtration**, by "increasing weights" (**).

³³⁴(**) 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 "fi ber functors" defi ned, not only on the base field k of the Ø-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 Spec(k'), where k' is an extension of k, or even a **fi nite** extension of k).

³³⁵(***) (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 176₁ à 176).⁷

³³⁶(*) 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"...

 $^{^{337}(**)}$ The heuristic reason that convinced Deligne of the existence of such a (necessarily unique) fi ltration of a pattern is that there are non-trivial extensions of abelian varieties by tori (whose motivic H^1 thus provides a non-trivial exten- sion 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 fi ni*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

p. 793

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, Ω^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 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, 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 C and of a motivic cohomology of a C-schema X (not necessarily clean, admittedly), and of a notion of "Hodge rea- lization" (suitable and to be found) of a pattern on 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.

□Bien entendu, cette filiation d'idées (164) était parfaitement connaissée de Deligne. It would have been in

p. 794

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 structures³³⁸ (*). 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"³³⁹ (**)

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 fi ltration "by increasing weights" for the l-adic Galoisian module associated with a pattern (an object already quite close to the pattern itself. . .).

³³⁸(*) 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. 1 2

³³⁹(**)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° 51₁ 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_pudiously under silence... $^{(*)}$

6. In the same "annotated bibliography", I read:

"From this "classical" point of view³⁴¹ (**) there is a regrettable gap in the literature: your conjectural des- cription 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, Sém. 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 \Box 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° 87₁ to the note "Le massacre" (n°

of a dream - or the birth of motifs, n° 51.)

p. 796

^{1970.} They don't go much beyond the initial idea of a motif, and can't give any idea of the fi nesse 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").

³⁴⁰(*) 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...

³⁴¹(**) 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 4¹", without further ado^{342} (*)).

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. . .).

p. 797

П

3. It was Deligne who took the initiative of asking Verdier for his agreement to include in "SGA $4_2^{\frac{1}{n}}$ 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^{1} (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 Frobé- nius morphism". This ambiguous expression, given the context and its spirit, had every chance of being read as meaning "SGA 4^{1} ", 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

the aforementioned pirated presentation of SGA 5, abound). On this subject, see also the comments in the note "Le

algebraic.

³⁴²(*) This act of dismantling (among many others) the SGA 5 seminar in favor of the volume called "SGA 4¹ "₂ 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 4^1 , and spelled out "between the lines" in the introduction-to both SGA 4^1 by Deligne, and SGA 5 by Illusie) of "SGA 4^1 " over SGA 5 (where references to SGA 4^1 , via

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.

He also refers to "the special case of the Frobé- nius 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 scratch³⁴³ (*).

 \Box 5. 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 (unnamed³⁴⁴ (*)) 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 coauthor 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 Grothen- dieck" 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 \Box Nielsen-Wecken theory³⁴⁵ (*), 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 stale cohomology!

Continuing in the same vein, in the introduction to the same talk (p. 225), Katz pretends to introduce Mrs.

³⁴³(*) 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).7

³⁴⁴(*) (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".

³⁴⁵(*) 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.° 67₁ 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 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 of my thoughts on Griffiths' theorem and Lefschetz's brushes³⁴⁶ (**)), which happen to "work".

p. 800 □(without conjectural ingredients what's more) in the case where X is a complete intersection. Moreover, in 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, or (while we're at it) than mastering the stale 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 car. *p*, my name does not appear³⁴⁷ (*) - not even for the ordinary cohomological expression of the L function. In fact, the analogous expression in terms of crystalline co- homology (which remained conjecture), 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).

³⁴⁶(**) 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.

³⁴⁷(*) That's not entirely true - he fi gures there (so it's a fourth reference to me), in a hale 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 \Box première "edition"). This may suggest that five years before the APG 4 operation¹ - APG 2 p. 801 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 5³⁴⁸ (*).

III Mebkhout's philosophy (Colloque de Luminy June 1981, paper on the "pervers 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"³⁴⁹ (**) 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 conjecture³⁵⁰ (***). 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 Kashiwara³⁵¹ (****).

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).

 \Box . Deligne tells me that he was unaware of Verdier's article³⁵² (*), 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^{1} 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.

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

³⁵²(*) 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- guïté" (n° 170(ii)), pages 930,931.

p. 802

 $[\]frac{348}{748}$ (*) 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 4¹ - SGA 5 operation. 2

³⁴⁹(**) See note "L'inconnu de service et le théorème du bon Dieu", n° 48'.

³⁵⁰(***) 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").

³⁵¹(****) 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".

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 Luminy³⁵³ (**), the duality in staggered cohomology (which I had developed in 1963) is called "Verdier duality"³⁵⁴ (***).

p. 803

p. 804

□ 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 4¹", and at the "Etat 0" of Verdier's "thesis" exhumed there. (For details of the latter, see II 3 above).

V The Eulogy

1. The IHES jubilee booklet containing my Eloge Funèbre³⁵⁵ (*) 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.

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 \Box 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

³⁵⁴(***) This operation took place in several stages. After 1963, at my suggestion, Verdier developed a theory of

³⁵³(**) On this "memorable Colloquium" and the article in question, see the note "L'Iniquité - ou le sens d'un retour", n° 75.

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...

³⁵⁵(*) 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 me³⁵⁶ (**). 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 ¹³⁵⁷ (*) (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 4¹, so as to give the impression (to an inattentive reader) that SGA 5 does indeed depend on SGA 4¹, 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 \Box 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° 46₉ 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 *I'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 *I*. (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 1-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

³⁵⁶(**) (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).

³⁵⁷(*) 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 4¹ - SGA 5 operation. See this subject the last part (dated February 18) of the previous note "Duty done - or the moment of truth".

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 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.

of finite type on 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 \Box 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 point³⁵⁸ (*), and that of the "realization" functor i.e., essentially, the construction of a cohomological theory on separate 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 August³⁵⁹ (**), 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 me³⁶⁰ (***) 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. . .).).

 $^{^{358}(*)}$ 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.

³⁵⁹(**) 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).

 ³⁶⁰(***) 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.

18.4. The Dance of

p. 807

p. 808

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

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...... ³⁶¹(*).

□ 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 Scientifique³⁶² (*). 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 elsewhere³⁶³ (**). 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.

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"³⁶⁴ (***):

□"In Paris, I attended Grothendieck's seminar and J.P. Serre's course. Three hours of lectures

such an association would have been strengthened by mentioning Cartan by name.

655

p. 809

³⁶¹(*) 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.

³⁶²(*) 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 ofprevious note n° 164 (part V 2).

³⁶³(**) 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 Hodege theory. For further details and comments, see sub-note n° 78' of the note "La victime" (n° 78').

³⁶⁴(***) 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

per week but, despite working happily and relentlessly, the rest of the week was barely enough for me to assimilate them (165_1) . From Grothendieck I learned the modern techniques of algebraic geometry, from Serre the fascinating beauty of number theory (165_2) . 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. $..165_1$).

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" (suggérant que mon oeuvre n'aurait rien à voir avec les formes modulaires³⁶⁵ (*)), and above all the "tant avec" with which I have the honor of being introduced, to equate the vast groundwork I had done³⁶⁶ (**), 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."

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

p. 810

³⁶⁵(*) 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)!).

³⁶⁶(**) 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".

p. 811

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 others³⁶⁷ (*) 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 π_i higher ($i \ge 2$) 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 follows³⁶⁸ (*). 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 buried³⁶⁹ (**), as nothing more than a "vague skeleton".

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 Funèbre³⁷⁰ (***), 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 \Box de étale or *l-adic* cohomology, as a new and essential tool that I have developed from nothing, p. 812

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 scene³⁷¹ (*).

Note 165₁ 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

³⁶⁷(*) (February 27) For further details, see "La Mélodie au tombeau - ou la suffi sance" (n° 167).

 $^{^{368}(*)}$ This final paragraph will be the subject of the note (n° 165) which follows this note.

³⁶⁹(**) 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, cf₁ 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**.

³⁷⁰(***) 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.

³⁷¹(*) 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, v), 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_2 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 aprendices 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 vant 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:

"In conclusion, I would like to emphasize how precious to me is contact with the work of past ma-hematicians (from 1800 to the present day), whether direct or relayed by scholars more erudite than myself, such as A. Weil and J.P. Serre. 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

p. 815

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 world³⁷² (*), 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.

a "freshness" in his approach to mathematical things, whereby he was called upon to do great things, as few mathematicians \Box 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 giant³⁷³ (*).

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,

the costs! In place of Celui-qu'on-nommais-jamais³⁷⁴ (**) (and who nevertheless remains ever present. . .) we verbally substitute "the great men of the past", to whom in the \Box preceding paragraph we come from elsewhere all

p. 817 just to tacitly attr

p. 816

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.

660

³⁷²(*) Including, incidentally, Pierre Deligne himself!

³⁷³(*) (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.

³⁷⁴(**) 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 everyone³⁷⁵ (*)!

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 reasons³⁷⁶ (**) \Box 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-heir³⁷⁷ (*), 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 apparence, that is to say, also, the question of **the** \Box **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 compensation³⁷⁸ (*) - this question, it seems to me, cannot be settled by a "demonstration", i.e. by a so-called "demonstration" approach.

³⁷⁵(*) 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).

³⁷⁶(**) 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. ...

³⁷⁷(*) 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).

³⁷⁸(*) 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 gatifications 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 voice³⁷⁹ (**) 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

 \Box 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 work³⁸⁰ (*), 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.

³⁷⁹(**) 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.

 $^{^{380}(*)}$ On this subject, see the note "L'enfant et la mer - ou foi et doute", n° 103.

p. 822

thesis by this student³⁸¹ (**). These are striking signs of the general disaffection with the program of foundations that $\Box I$ had undertaken in the sixties, and which I certainly would not have suspected would not Ω . 821

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 I is **nilpotent** on the scheme X, the category of "l-adic coefficients on X", $Z_l * (X)$ let's say³⁸² (*), should be none other than that of "crystalline coefficients", with Frobé- nius 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 *I*, it is still not done even in the simplest non-trivial case of all, $X = Spec(Z_l)(*)$ 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* categories* (*X*), in terms of conditions of finiteness, holonomy and regularity on complexes of "crystals" (absolute - i.e. relative to the absolute base *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

³⁸¹(**) 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)".

 $^{^{382}(*)}$ 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 coeffi cient 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 $Z^* \otimes_Z Q$, obtained by "tensorizing" the product of all categories of coefficients $Z^*(X)$ by Q_{A} .

Note that when the prime number l is not prime to the X scheme, then in the description of the "coeffi cients l-adic" elements on X, the nilpotent elements of Q(X) cannot be neglected - they intervene in the vicinity of the fi bre X(l) of X in l. A fortiori, the same will be true of the adelic coeffi cients on X, which brings them closer to the coeffi cients (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 coeffi cient, the adelic coeffi cients and De Rham-Mebkhout's coeffi cients (provided the latter are equipped with all the richness of structure alluded to below), are of comparable "fi delity", 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 "fi leness", 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 coeffi cient (adelic, De Rham-Mebkhout, the latter taken here "within isogeny"). The advantage of the latter over the former, which makes them appear "more fi ne" 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.

³⁸³(*)(May 12) As we'll see below, this "improvised" name and notation prove to be inappropriate. I have fi nally opted for the notation *DRM*^{*} (*X*) or *Meb*^{*} (*X*), dual to *DRD*^{*} (*X*) or *Del*^{*} (*X*), for the coeffi cients of De Rhammebkhout 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 coeffi cients) 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 "hatibual" (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* notation* (*X*)) or, at a pinch, "De Rham-Hodge-Mebkhout coefficients", *DRHM** (*X*). When *X* is of finite type over the complex field C, we should be able to reconstruct the hypothetical *Hodge*=*HDG* coefficient categories* (*X*) (which I certainly wouldn't call Hodge-Deligne, whereas Deligne

p. 823

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 varieties³⁸⁴ (*) are currently as follows:

- 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* (*X*) of "De Rham-Mebkhout coefficients" for any scheme *X*, or possibly, of analogous *DRM* categories* (*X*/*S*) for relative schemes (

$$DRM^*(X) = DRM^*(X/Spec(Z))$$

), 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 Ksur 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"

$$\mathbf{K}(\boldsymbol{p})^{(p)} \to \mathbf{K}(\boldsymbol{p})$$

where K(p) denotes the restriction to the subschema 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

³⁸⁴(*) 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 \Box 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)³⁸⁵ (*). 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)³⁸⁶ (**) 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 $Bet(K) \otimes_Z Z_l$ (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 variations³⁸⁷ (*).

³⁸⁵(*) 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 infi ni regularity conditions for Mebkhout coefficients, and also (if we put a De Rham fi Itration as an additional structure) the Griffi ths conditions linking De Rham fi Itration 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 fi lefor recognizing oneself in that disparity.

³⁸⁶(**) (May 2) In fact, it was as early as 1966.

³⁸⁷(*) (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 fi nor on the absolute basis 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 $a \square$ 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 388 (*).

p. 826

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**"³⁸⁹ (**) 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, t h a t 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

P. 827 **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 note³⁹⁰ (*)). 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-notes³⁹¹ (**). They are grouped into eight parts (1) to (8), with (I hope) suggestive names, from (1) "The

now!) of the "mysterious functor", which plays a crucial role in the complete description I'm now looking at.

³⁸⁸(*) (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.

³⁸⁹(**) 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).

 ³⁹⁰(*) (May 12) After splitting this former note "Silence" (n° 168) into four, the "next note" is "The four operations".
(n° 167).

³⁹¹(**) (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 notes³⁹² (***) 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)³⁹³ (***), devoted successively to ^{Sketching $\Box d$} 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° 169₁), 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' Apothéose" (n° 171), concerning Zoghman Mebkhout's strange adventure: "Eclosion d'une vision - ou l'intrus", "La maffia", "Les racines", "Carte blanche pour le pillage" (n° s 171₁ à 171₄). These are eight sub-notes (from a total of twenty-one³⁹⁴ (*)) that I particularly recommend to the reader.

As for 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 rocambolical 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 in-nocent 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. . .

 ³⁹²(***) (May 12) These notes, having reached prohibitive dimensions, were fi nally split into several, into notes n° s 168 (i) - (iii), 169 (i)-(v), 170(i)-(iii), 171 (i)-(iv).
³⁹³(***) (May 12) These notes, having taken on prohibitive dimensions, were fi nally split into several, into the following notes

³⁷⁵(***) (May 12) These notes, having taken on prohibitive dimensions, were fi nally split into several, into the following notes n° s 168 (i) - (iii), 169 (i)-(v), 170(i)-(iii), 171 (i)-(iv).

³⁹⁴(*) (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 $167^{"}$ (February 26)³⁹⁵ (*) 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, \Box 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 silence³⁹⁶ (*) 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.

³⁹⁵(*) 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.

 $^{^{396}}$ (*) (April 19) For a correction concerning these "twelve years", see the sub-note "Pre-exhumation", n° 168₁.

May, but was discarded (somewhat arbitrarily no doubt) in the later and ultimate procession "The Funeral Ceremony", part of "Burial II". I would en \Box core 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),³⁹⁷ (*), 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 October³⁹⁸ (**).

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' \Box 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 work³⁹⁹ (*) 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

³⁹⁷(*) 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).

 $^{^{398}(**)}$ For more on this visit, see "Duty done - or the moment of truth" (n° 163).

³⁹⁹(*) 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 onwards⁴⁰⁰ (**), 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 come⁴⁰¹ (***), 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". 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 \Box ("The End of a Silence", p. xviii):

p. 833

"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 period⁴⁰² (*) 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 texts⁴⁰³ (**) 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

⁴⁰⁰(**) 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). $^{401}(***)$ (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.

⁴⁰²(*) 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".

⁴⁰³(**) 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 \Box cohomologists, as a fundamental structure in homological algebra. ...), 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° 164₁) 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 silence⁴⁰⁴ (*), 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 reaction⁴⁰⁵ (**), 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 forma $\Box tion^{406}$ (*). 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 change⁴⁰⁷ (**)). 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,

⁴⁰⁴(*) (April 8) For a correction to these "twelve years", see the sub-note "Pre-exhumation" (n° 168(iv)) which follows this "Silence" note.

⁴⁰⁵(**) 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".

⁴⁰⁶(*) 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').

⁴⁰⁷(**) 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_1 already cited) - and that he didn't find it in Demazure's exposé on the ABCs of definition 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_1 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

p. 836

to draw inspiration from them⁴⁰⁸ (***). As far as I know' \Box this problem isn't mentioned anywhere before he was solved by Deligne in his 1980 article "Weil II" (in the case of Rf_1), 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. . .).

ensure total hegemony, instead of making them available to the scientific community and allowing everyone

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 1982⁴⁰⁹ (*) a deathly silence around the notion of motif. The only exception (unless I'm mistaken⁴⁰⁹ (*)) is the "half witness line" of 1970, just as incomprehensible⁴¹⁰ (**) 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).

*a*₃ and exhumation

Note 168(*iii*) A sudden change of scene with the publication of the "memorable volume" Lecture Notes 900⁴¹¹ (***). 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

p. 837

yoga of the weights, which is the first time it has been explained in the literature, it is nowhere mentioned in 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

⁴⁰⁸(***) 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- 169₈) referring to the first two of the "four operations" around my work.

⁽x) This fi n became the note "Le magot" (n $^{\circ}$ 169(v)).

⁴⁰⁹(*) (April 8) For a correction, see the sub-note already quoted "Pre-exhumation" (n° 168 (iv)).

⁴¹⁰(**) 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.

⁴¹¹(***) 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 so⁴¹² (*), 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"⁴¹³ (**). 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.

 \Box 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.

. 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 Marchen 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

⁴¹²(*) 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)).

⁴¹³(**) 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 was therefore particularly notorious. (On the other hand, it would seem that the concealment of my persona

p. 839

- 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^{1} - 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' Apothéose", 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 coffin⁴¹⁴ (*), 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

to 175_7) 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'Apothéose" (n° 171), and its four sub-notes⁴¹⁵ (**), seem to me essential to situate this "suite" properly and give it its full meaning.

18.5.2.2. Maneuvers ("Staggered Cohomology")

b₁ . The "Weil Conjectures" context

p. 840 Note 169(i) \Box (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

⁴¹⁴(*) 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")...

⁴¹⁵(**) (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 abé- liennes 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 I 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 1-adic theory (with a grain of salt for I = p) for **any** prime number I - and in a conve- nable 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 pro- priétés voulues⁴¹⁶ (*), and they were still to give "the same result" as the hypothetical cohomologiesp .841 l-adic.

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 collaborators⁴¹⁷ (**)), a formalism for **cohomology** /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 questions⁴¹⁸ (***), of a technical nature, we can say that we had, "in principle" as early as 1963, and "in fact" as early as 1965/66

⁴¹⁶(*) Back in the 50s, I developed the formalism of cohomology classes (Hodge and De Rham) associated with an algebraic cycle.

 ⁴¹⁷(**) 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 pu- blier, 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).

⁴¹⁸(***) 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

p. 842 (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 stale 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 stale 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 variety⁴¹⁹ (*)). 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 car. p > 0, in 1968 I came up with a general definition for a "*p*-adic cohomology", which I also call **cris- cohomology**.

talline⁴²⁰ (**). This \Box 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's⁴²¹ (*).

c. Validity of the "dibualité theorem" on an excellent regular pattern. Situation similar to b).

assumptions of singularity resolution and "cohomological purity" (cf.a)), which for the moment do **not** apply to algebraic varieties of car. p > 0. I would point out, however, that in the context of torsion coeffi cients (as opposed to *l-adic* coeffi cients), the duality formalism of the six operations (thus including Poincaré duality) had been established by me in 1963 without fi nitude conditions. This implied, for example, "fi nitude" for H^i with constant or locally constant coeffi cients (torsion or *l-adic*) for a smooth (not necessarily clean) scheme over an algebraically closed body.

The situation was significantly improved by Deligne's elegant (1973?) proof of the fi nitude theorem, for a morphism of fi ni type schemes over a regular *S*-scheme of dimension ≤ 1 . This case covers most applications (algebraic schemes over a body, fi ni type schemes over Z in particular). In the same situation of a scheme X of type

fi ni on a regular 1-dimensional scheme, and using similar simple arguments, Deligne also manages to prove the biduality theorem.

 $^{^{419}(*)}$ On this subject, see sub-note no.° 46 to the note "My orphans" (no.° 46).

 $^{^{420}(**)}$ 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 infi nitesimal) 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 **coeffi cients 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 sugges- tions in this direction (to Berthelot in 1978) obviously fell on deaf ears, coming from such an insignificant character....

⁴²¹(*) 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 etale 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."⁴²² (*)

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)⁴²³ (**). The first is in collaboration "with d'autres⁴²⁴ (*), and develops, in addition to the language of topos, the key results of coho-

p. 844

mology, including key duality start-up statements (six-operation style). The second, in which I practically went it alone⁴²⁵ (**), develops a complete formalism in much greater detail.

"the image of a "geometry" that would be developed "above the absolute base" SpecZ, and which admits "specialisations" both in the traditional "algebraic geometries" of different characteristics, and in "transcendental" geometric notions (above the basic bodies C, R, or $Q_A \dots$), 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 stale 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."

⁴²³(**) 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.

p. 845

crystalline cohomology for the ordinary *L*-function of such a variety over a fi ni 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 coeffi cients". These (according to what Deligne recently told me) have not yet been **defined**, any more than the right "Hodge coeffi cients" (above complex algebraic varieties)! For some comments on the "coeffi cientproblem", 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.

⁴²²(*)I propose elsewhere (in sub-note n° 136₁ 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

⁴²⁴(*) 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.

⁴²⁵(**) The only exception (if my memory serves me correctly) was provided by J.P.Serre, who gave some fine talks on fi nished groups and the Serre-Swan module associated with the Artin conductor, which I needed for the development of the general fi xed 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 discoververte 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 farreaching 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. "

* *

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 Deligne⁴²⁶ (*), 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)⁴²⁷ (**) - techniques which were to form the basis of all his subsequent work.

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.

⁴²⁶(*) 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").

⁴²⁷(**) 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 SGA₂ 4^{1} - SGA 5") set up by my brilliant ex. As a student, I see four inseparable "maneuvers".

Manoeuvre 1: Discrediting the SGA 4 - SGA 5 mother seminar as a "gangue of nonsense" and others 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 "SGA 4^{\perp} " (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 presentations⁴²⁸ (*). 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 "SGA $4^{\frac{1}{2}}$ ". (This name, however, does not appear to have been

bewildered or surprised, even shocked, no one but me $(169)_1^{429}$ (*)) For details, see notes "Le feu vert" and "Le renversement" (n° s 68, 68'), where the sense of volume calling itself "SGA 4¹" 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

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).

⁴²⁸(*) 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. ...

 $^{^{429}(*)}$ 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° 169₁) 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"⁴³⁰ (**), answering to the unusual name "SGA 4^{1} "⁴³¹ (***). 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 stale 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

p. 850

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_2 ", 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 to⁴³² (*), 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^{1} "] 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 $4^{\frac{1}{2}}$ ", which I₂hadn't even considered. the previous two days.

The same sham of "logical dependence" is clearly suggested in the introduction to SGA 5 by Illusie (169) $_2^{433}$ (**). It is further rendered plausible, for an uninformed reader, by the innumerable references to "SGA 4¹" which the late editors of my⁴³⁴ (***) 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 Deligne⁴³⁵ (*)), which were incorporated without further ado into the text.

⁴³⁰(**) 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).

⁴³¹(***) Subtitle: Cohomologie étale - by Pierre Deligne. ... The subtitle says it all!

⁴³²(*) 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).

 $^{^{433}(**)}$ For details, see the sub-note "Good Samaritans" (n° 169₂) to the present note (n° 169), originally intended as a b. de p. note here.

⁴³⁴(***) (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 4¹", 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.

⁴³⁵(*) 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 $4^{\frac{1}{2}}$ " - 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!)⁴³⁶ (*) - a collaborator a little "confused" around the edges⁴³⁷ (**) it is true, but "collaborator" all the same. . .

As for the pirate-text called "SGA $4\frac{1}{2}$ ", 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 trigo- nometric 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, admittedly⁴³⁸ (***)) to the cohomology formalism \Box 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

fi nitude 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**

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 fi nitude of Rf_* , as well as the biuality 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 fi nitude, 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 "fi nitude" complements, see the sub-note "Le cheval de Troie" (n° 169₃) to this "Maneuvers" note.

- ⁴³⁶(*) 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. . .).
- ⁴³⁷(**) In his summary (a copy of which he sent to me) of "SGA 4¹ "₂ 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"...
- $^{438}(***)$ These are fi nitude 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 fi xed point formulas "modulo" l^n 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 fi ni type scheme, over a fi ni 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 4¹". $_{7}$

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 fi nitude theorems in staggered cohomology, it developed the theorems of

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 fi nitude statement for the functor Rf_{\cdot} the similar statement for Lf^{\dagger} and for <u>*RHom*(., .)</u>), introduced a uniform technique for using the strong form (à la Hironaka) of the resolution of

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^{1} " to the chapter on finiteness theorems), and to rename the biduality theorem in stale 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"...).

b_3 . Episodes of escalation

Note 169(*iii*) \Box (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 4¹", followed by the massacre edition (known as the "Illusie edition") of SGA 5⁴⁴⁰ (*). 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. Berthelot⁴⁴¹ (**). Illusie is responsible for

gique" (like Poincaré). Along with the introduction of the Lf functor[!] (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.

- ⁴⁴⁰(*) (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... ...
- ⁴⁴¹(**) 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

⁴³⁹(*)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 "cohomolo-duality" type.

(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 \Box of Deligne⁴² (*).

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 conjectures⁴⁴³ (**), 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)⁴⁴⁴ (***). 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".

 $(n_{\underline{s}1}^{\circ}65, 166)^{445}$ (****)

The operation culminated in 1977⁴⁴⁶ (*), with the publication (in no particular order) of $_2$ (sic) - SGA $_{p. 855}$ "APG 4 1

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

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...

⁴⁴²(*) 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.

⁴⁴³(**) (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° 169₄) to the present note "Les manoeuvres".

⁴⁴⁴(***)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...

⁴⁴⁵(****) 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 Scientific" (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 thumbsucking 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).

 ⁴⁴⁶(*) (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 article⁴⁴⁷ (**) in which Mac-Pherson proves this conjecture in the analytic context.

p. 856

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_1 to the note "The massacre" (n° 87)⁴⁴⁸ (*).

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)⁴⁴⁹ (**).

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' \Box this 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.

p. 857

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.

⁴⁴⁷(**) Mac Pherson, Chern classes for singular algebraic varieties, Annals of Math. (2) 100, 1974, pp. 423-432.

⁴⁴⁸(*) 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....

⁴⁴⁹(**) 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° 169₁) 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 curve⁴⁵⁰ (*). 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 footnote⁴⁵¹ (**) (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" $(169)^{452}$ (***) published in the same year (1973) in 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 parternity of which was still all too notorious in well-informed circles)⁴⁵³ (*); 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 4^1 - 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 cycle⁴⁵⁴ (**). In this article, my name is not mentioned (except once,

⁴⁵⁰(*) 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.

⁴⁵¹(**) 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... ...

 $^{^{452}(***)}$ see sub-note ""**The**" "Conjecture"" (n° 169₄), from a b. note here.

⁴⁵³(*) 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 4¹ ", 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

[&]quot;La Formule" (n° s 169 -169₅₈) to the present note, as well as the two sub-notes that precede it, "Le cheval de Troie" and ""La" Conjecture" (n° s 169₃, 169).₄

⁴⁵⁴(**) The idea of defining the **homology** of a scheme (or "space" . . .) as its hypercohomology with values in a "complex

 $_{\rm p.\,859}$ by way of a joke of a very particular kind. . .), and no allusion \Box 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 com- plex spaces, for analytically constructible discrete coefficients, reproducing ne varietur the one I had developed (as early as 1963 and especially, in SGA 5 in 1965/66) in the étale schematic context, became su- breptively 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 below⁴⁵⁵ (*).

b_4 . 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 \Box 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 satisfaction⁴⁵⁶ (*).

"the **only significant changes from** the original version concern Lecture II [fi nitude 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

686

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 "coeffi cients", 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.

⁴⁵⁵(*) See "Sharing" notes, n° s 170 (i) - (iii).

⁴⁵⁶(*) As for the **cohomology** variant (just touched on in Verdier's article, which Deligne refrains from quoting), it is

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 "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^{1} "⁴⁵⁷ (**). Certain shapes are still preserved, in the de rigueur "pouce!

excels at constantly **suggesting** the false, without ever (or almost. . . (169)₃⁴⁵⁸ (***)) goes so far as to suggest en \Box clair. My first confrontation with "SGA 4¹" and with the particular form that this style takes there _{p. 861} (that of disdainful depreciation⁴⁵⁹ (*)) 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 dimension⁴⁶⁰ (**). 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 contempt⁴⁶¹ (***).

. The hoard

Note (169(v)) \Box (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.

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 infi nite 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 4^1 " of every turn of the page, and mercever to present their late meter as the humble (and confued) collaborator of his meter.

^{4&}lt;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. . .

 $^{^{457}(**)}$ (March 21) Further reflection in the series of sub-notes grouped under the name "The Formula" (n° s 169₅ to 169₈) has shown me that this impression was wrong, despite "certain forms" that are still retained. ...

⁴⁵⁸(***) On this subject, see the sub-note "Le cheval de Troie" (n° 169₃), taken from a b. de p. note here, which was supposed to explain this "or almost...".

⁴⁵⁹(*) 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...

⁴⁶⁰(**) (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 4¹ - SGA 5₂" 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... ...

⁴⁶¹(***) This sufficiency and contempt can be seen quite clearly in and between the lines of the volume entitled "APG 4¹" (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 time⁴⁶² (*). \Box And what I put into their hands, I knew well

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 addressing⁴⁶³ (*). 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 behalf⁴⁶⁴ (**).

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.

⁴⁶²(*) 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).

⁴⁶³(*) (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**.

⁴⁶⁴(**) 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"⁴⁶⁵ (***), in the two notes "Yin the Servant, \Box 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 texts⁴⁶⁶ (*).

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 left⁴⁶⁷ (**) - as if by a sudden stroke of \Box scie (or p . 865 chainsaw... ⁴⁶⁸(*)).

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 169₁ (March 9)⁴⁶⁹ (**) 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,

⁴⁶⁵(***) 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"?

⁴⁶⁶(*) 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...).

⁴⁶⁷(**) (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).

⁴⁶⁸(*) 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).

⁴⁶⁹(**) 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")⁴⁷⁰ (***). 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".

arithmetic", a synthesis of algebraic geometry, topology \Box and arithmetic⁴⁷¹ (*).). 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^{1} " 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 $4^{\frac{1}{2}}$ " 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

escalade) in the note \Box "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

p. 866

⁴⁷⁰(***) Written in collaboration with J. Dieudonné.

⁴⁷¹(*) 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...⁴⁷²(*).

 \Box (March 16) This sub-note has been given the appropriate name, "Eviction (2)". The (2) sign is a reminder that there are

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 to⁴⁷³ (*). 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

⁴⁷²(*) (April 9) For the rest of the story, see the note "Les Pompes Funèbres - - im Dienst der Wissenschaft" (n° 175).

⁴⁷³(*) 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° 134₁) 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 \Box 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 time⁴⁷⁴ (*).

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 still⁴⁷⁵ (**). Apart from Deligne's work on Weil's conjecture, this \Box was at the same time the beginning of a long stagnation in each of the major themes that had most interested me.

p. 870

p. 869

a stagnation which (apart from the partial "revival" triggered by the philosophy of Mebkhout- the unnamed) continues to this day⁴⁷⁶ (*).

b_7 . Good Samaritans

 $[\]frac{474}{100}$ (*) I begin to realize that this was an illusion, at the end of the note "Le feu vert" (n° 68), p. 260.

⁴⁷⁵(**) 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.

⁴⁷⁶(*) (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_2 (March 13)⁴⁷⁷ (**) 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^{1} "⁴⁷⁸ (***)). 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 $\overset{\square}{I}$ -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 4¹".

For details, see the note "Le massacre" and its sub-note $n^{\circ} 87_2$. 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

⁴⁷⁷(**) 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 169₃ and 169 -169₅₈.

⁴⁷⁸(***) See b. de p. (***) page 841 and (*) page 850.

Note 169_3 (March 10)⁴⁷⁹ (*)In the sub-note (n° 67_1) 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^{1} , 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 analysis⁴⁸⁰ (*).

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"⁴⁸¹ (**). 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 dua-

p. 873 lité étale points"

lité 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^{1} " (a dependence posited by this very name, and by the beautiful procession "SGA 4 - SGA 4^{1} - SGA 5"), and thereby justify the incredible assertion (already quoted and commented on) in his introduction:

"Its existence [of "SGA 4^{1} "], 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 $4^{\frac{1}{2}}$ ", and

⁴⁷⁹(*) 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.

(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".

2

 $^{^{480}}$ (*) 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).₇

⁴⁸¹(**) 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.

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 envisaged⁴⁸² (*). 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 \Box formula of Lefschetz-Verdier⁴⁸³ (*), p.874 which itself played a purely heuristic role⁴⁸⁴ (**). 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 $4^{\frac{1}{2}}$ ", 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 opera- tions** [not to say the "six operations", which must definitely not be named!] transform any constructible bundle into a constructible bundle." (Emphasis added.)

 \Box 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 cohomology⁴⁸⁵ (*). However, I had the pleasure of proving the first such finiteness theorem, and the most crucial of all, for the functor

important, providing **motivation** for the development of "explicit" fi xed point formulae.

⁴⁸²(*) Thus, the "six functors" and the essential formulas concerning them, the most crucial of which is the "duality formula" for a separate morphism of fi ni 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 fi nitude hypotheses on the coeffi cients. 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)!

⁴⁸³(*) (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" (n° s 169₅ - 169₉) to the note "Les manoeuvres". ⁴⁸⁴(**) As I point out below (in the sub-note "Real maths...." (n° 169₅), this formula has been psychologically

⁴⁸⁵(*) 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 stale 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 <u>RHom(., .)</u>RHom(.,.)⁴⁸⁶ (**). Admittedly, this has since become "basic folklore" in staggered cohomology, and is surely part of the "technical digressions" that my brilliant precursor "SGA 4¹" is intended to "make people forget". . .

b₉. "The conjecture

Note 169_4 (March 12)⁴⁸⁷ (***) 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 "gangue 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 variates). 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".

⁴⁸⁶(**) As for the remaining two of the six operations, namely Lf^* and L_{\otimes} it is trivial that they transform construct- coeffi cients.

⁴⁸⁷(***) 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 conjecture⁴⁸⁸ (*).

. The Formula

(a) Real math. ...

Note 169 \Box (March 17) The famous "Weil conjectures", for an algebraic variety *X* defined over unp. 877 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 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

$$L(t) = \frac{P_0(t) P_2(t) - - P_{2n}(t)}{P_1(t) - - P_{2n-1}(t)}$$

where the P_i ($0 \le i \le 2n$, 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 X on the algebraic closure 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 C^{489} (*). The rational function must satisfy a

functional equation, which is equivalent to saying that the roots $_{2n-1}$ are exactly q^n , where $q = p^f$ is of P

the cardinal of the base field *k*, and where ξ_{α} 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 $\overline{X.}$ 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 ξ_{α} 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 \Box 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 P_i are all equal to q_i (an assertion that leads to highly accurate estimates on numbers of

⁴⁸⁸(*) (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 169₅ - 169).9

⁽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)).

⁴⁸⁹(*) 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 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 fins $h^{i,j}$ invariants of Hodge theory.

points of X^{490} (*)).

p. 879

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 intersec- tions into cup-products), makes it possible in an essentially "formal" way to transcribe the classic "Lefschetz fixedpoint formula". By applying this formula to the Frobénius 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 $R = Q_l$, $l \neq p$. 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 Z_l of *l*-adic integers, were in fact ordinary integers, and moreover, independent of the considered prime number I(l/= p = 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. They have also stimulated thinking to also find a **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 *I* (**including** for I = 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

 $[\]frac{490}{490}$ (*) 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 $f : X \to X$ (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, and hence the "biduality theorem"), was entirely irrelevant⁴⁹¹ (*).

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) 1-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 Lef- schetz et rationalité des fonctions *L*, Sém. 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. ...)

 \Box In the Bourbaki paper in question, I confine myself to giving the general statement of the formula for functions *L*

p. 881

"with coefficients in an ordinary 1-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 routine⁴⁹² (*). 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

⁴⁹¹(*) (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° 169₆ and 169₇.)

⁴⁹²(*)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 fi xed 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 stale 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 operation₂ - 1 GA 5" was the desire to appropriate the trace formula alone, and thus, as a "corollary", that of *L* functions.

short. I believe that both "pieces" were too big, and that even today and notwithstanding "SGA $4^{\frac{1}{2}}$ " and Colloque Pervers et tutti quanti, "people" (even those who are not so well informed) "know" that it was not he who created the 1-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 theme⁴⁹³ (*) of the volume entitled "SGA $4^{\frac{1}{2}}$ ", 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 Grothen- dieck (author or director of a rather vague seminar subsequent to the volume "SGA 4^{1} ", 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 others⁴⁹⁴ (**). It reads:

"If we admit the formalism of Q_l -beams. . . **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 $[2]^{495}$ (*)) has (non

⁴⁹³(*) In fact, nowhere in "SGA 4¹ " 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 stale 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 stale and *l-adic* cohomology must already have been present.

⁴⁹⁴(**) 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. . .), $n \circ 75$ ". 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 4^1 " 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.<u>Nielsen-Wecken</u>. A method inspired by Nielsen-Wecken's work can be used to bring 3-2 [the fortrace 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 "Lefschez-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

⁴⁹⁵(*) 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:

"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 Artin⁴⁹⁶ (*), 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**!

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 full⁴⁹⁷ (*).

(b). . and nonsense. .

p. 886

p. 885

Note 169₆ (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

⁴⁹⁶(*) 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...

⁴⁹⁷(*) (March 20) I'll come back to this introduction in yesterday's reflection (Cf. "Les double-sens - ou l'art de l'arnaque", subnote n° 1697.)

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 Introduction⁴⁹⁸ (**), but in terms that remained perhaps a little too "general".

biduality theorem.

If I say that the theorem discovered by Verdier (following the path traced by Lesfchetz) 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 formalism⁵⁰⁰ (*), to explain a general formula of Lefschetz's, for the purposes of

⁴⁹⁸(**) See Introduction 4, "A journey in pursuit of the obvious".

 $^{^{499}(***)}$ On this subject, see the note "Youth snobbery - or the defenders of purity" (n° 27).

⁵⁰⁰(*) 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 variety, everyone would have arrived there infallibly, by

taking a few hours or days, or if need be a few weeks⁵⁰¹ (*). 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 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 new⁵⁰² (**).

The creative moment, the spark that triggers a process of discovery, was here when the **problem w a s 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 Lef- schetz'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 dif- ficult 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

p. 889

p. 888

can \Box signify: releasing "the" right conjecture⁵⁰³ (*); 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 avoided⁵⁰⁴ (**).

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

⁵⁰¹(*) 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!

⁵⁰²(**) 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. ...

⁵⁰³(*) 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 "defi nitive".

⁵⁰⁴(**) 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 \Box precise p. 890 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 all⁵⁰⁵ (*).

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 uninvolving thread (and "conjectural", what's more, not to mention that the local terms "were not calculated") was suspended a certain seminar 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

⁵⁰⁵(*) 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 pronounced⁵⁰⁶ (*). 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 \Box

modesty, precisely among the most brilliant people! Two pages later (as we saw yesterday) the

p. 892

p. 893

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 decoverage **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"⁵⁰⁷ (*). For example, the unscrewing I did for the "function formula

L" as in other circumstances, unscrewings which today pass for "trivial", Dhave 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^{508} (*). It's not because one stage comes after another, or because it happens to be

⁵⁰⁷(*) Readers will note that this is a description of the "yin", "feminine" approach to a diffi culty - 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

⁵⁰⁶(*) (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 definite traces in "unorthodox" cases).

possible approaches the note "The rising sea. . . " $(n^{\circ} 122)$, 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" ($n^{\circ} 152$) which follows the latter.

⁵⁰⁸(*) 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-Wecken⁵⁰⁹ (**)).

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 5H0 (***). \Box Lap The second stage took longer, as it turned out: when it came to working it out with all its generality 511 (*) (given that there are other endomorphisms of a curve than the Frobenius one), there was a whole "carpet" of non-commutative traces "à la Stallinge" 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!

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 credit and comprehensive insurance" (n° 63", 81).

894

in stale 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_1 , between stale cohomology and transcendental cohomology (for fi ni 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....

⁵⁰⁹(**) (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).5

⁵¹⁰(***) The language of derived categories is indispensable in this demonstration. After my departure, and until about the year with the publication of the volume entitled₂SGA 4¹ ", 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).

⁵¹¹(*) (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 subnote "Les double-sens - ou l'art de l'arnaque", n° 169₇.

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. ...

It would seem that these days, and increasingly so, in such a situation (and when you're in a position of

p. 895

p. 896

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

- that's what en est \Box 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 mine⁵¹² (*).

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

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

⁵¹²(*) 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).

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 169₆ **bis** (April 10)⁵¹³ (**) 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 \Box 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.

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 farreaching 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 4¹ ". This was set up from scratch to bury₂the 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

. 898

⁵¹³This sub-note is taken from a b. de p. note to the previous sub-note "... and 'non-sense'" (n° 169₆); 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 169₇ (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) suffices⁵¹⁴ (*). 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 "ex- plicit" 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".

p. 899

It is true that we can then formulate them in entirely geometrical terms, without explicit reference to the forma \Box lism of the *L*-functions.

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 4¹". 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 1-adic cohomology.

It remains for me to review the three remaining passages, among the four in "SGA 4^{1} ", 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

 $^{^{514}}$ (*) (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 ζ -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 4^1 , 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 $4^{\frac{1}{2}}$ "⁵¹⁵ (*). When

an allusion is made to a presentation in SGA 5 (generally unspecified), it is clearly specified that itp .900 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^{1} " 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 \Box 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 $4^{\frac{1}{2}}$ " - which surely provided the quidam in question with what he needed.

 $[\]frac{1}{5^{15}}(*)$ 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° 169₃) 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 Frobénius 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 $516\square$

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 waters⁵¹⁷ (*).

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 mor- phism".

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 $4^{\frac{1}{2}}$ "? - and since he knows that SGA 5 contained a complete demonstration, it's awarded once again: the author has indeed referred (somewhat vaguely,

⁵¹⁶(*) 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!

 $^{^{517}(*)}$ 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 4¹".

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 fi xed points not necessarily "transverse"), and generalized the classical "intersection multiplicities" that fi gure 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 coeffi cients, 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 crue the other (e.g., between the local terms defi ned 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 Frobénius 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 \Box the volume "SGA 4¹ " 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 doubt⁵¹⁸ (*)!

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 it⁵¹⁹ (**)), 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. .. ⁵²⁰(***).

The "**this seminar**" is simply brilliant - my thumb-friend is incoincible on that term there. Still, both in the paragraph quoted, and \Box 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 ventured⁵²¹ (*) 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. 903

 $^{^{518}(\}ensuremath{^*})$ See the sub-note from the day before yesterday "Real maths. . . " (n° 168).5

⁵¹⁹(**) Nowhere in the volume entitled "SGA 4¹" did I find a reference to one of the SGA 5 presentations containing either the demonstration of a fi xed 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" !

⁵²⁰(***) 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.

⁵²¹(*) "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).₃

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_B "Calculations of local terms" (p. 139)⁵²² (**).

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 $4^{\frac{1}{2}}$ " that the reader holds in his hands, and on which (if only because of this fact) the later and "confused" seminar SGA 5 depends. . .

As for the last sentence of the passage quoted, beginning with "Other references" (sic), it too is a

p. 905

p. 906

model of its kind, to avoid saying that the vague quidam Grothendieck had given a com- pllete demonstration eleven years \Box 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 co-homological 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 Grothen- dieck 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."

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

⁵²²(**) For the motivation behind Illusie's sudden efforts, see the sub-note "Les félicitations - ou le nouveau style" (n° 169₉), especially pages 916-918.

⁵²³(*) (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^{1} " 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 abracadabratic, 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 1-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 seminar¹ (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

p. 907

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 169₈ (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.

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^{1} ", or "SGA 4^{1} - 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 p. 909 \Box and (there's no doubt in my mind) still is today 524(*). Appropriation-escamotage reflexes,

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

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 4¹") 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 Elle fi nally, who appears to me (up to the moment of the Colloque Pervers in June 1981) as **the** great point of fi xation of the conflict that has been woven in my ex-student around the disavowed master.

⁵²⁴(*) 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 fi ctive, 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 4¹ - 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 "gangue 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.

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 4¹", 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.

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^{1} " 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^{525} (*).

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^{1} " 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^{1}

⁵²⁵(*) 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 fi gures 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° 169₂) 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 Lefschefez-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 "noncommutative"!) "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 formula⁵²⁶ (*)) 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_B, 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 527 (**) 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 thesis⁵²⁸ (*), which for the first time dealt with the case of a cohomological correspondence.

⁵²⁶(*) 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 fi xed points with quel- conque constructible coefficients. On this subject, see the sub-note "Les vraies maths. . . "(n° 169₅, 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").

⁵²⁷(**) 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 "SGA₂4¹" (however earlier it may be) is supposed to "remedy"... All this under the watchful eye of the Congrégation des fi dèles. Congratulations!

⁵²⁸(*) 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_B (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 seminar⁵²⁹ (**)! 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 simplicity⁵³⁰ (***). 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 \Box passed off (along with five or six other packets of exposés 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 Bucur⁵³¹ (*), 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_B -sic, by the improvised "father" Illusie, we read:

"The second part of this talk (III_B), 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 4^{1} 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 $n^{\circ} 5^{532}$ (**)

"to define, at n° 6, **local Lefschetz-Verdier terms** for coho- correspondences.

mological complexes of modules on rings that are not necessarily commutative."

The name surreptitiously given to these "local terms" that I had introduced in 1965 for the purpose of writing the formula

 $^{^{529}(**)}$ 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 4¹"): "*L-adic* eohomology and *L-functions*".

 ⁵³⁰(***) 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 fi gured in Bucur's presentation (based on my oral presentation), the Serre-Swan modules are indeed explicitly involved.

⁵³¹(*) 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.

⁵³²(**) 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)"⁵³³ (*) (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_B ." (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

just⁵³⁴ (**), from the seminary gaily massacred by his "publisher"-fossilizer Illusie.

In the original seminar, the retracted exposé XI, renamed III_B (with $\Box a$ 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_B, 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

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° 169₁.)

⁵³³(*) (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 defi ning 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... ...

⁵³⁴(**) 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

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"⁵³⁵ (*), 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 169₉ (March 22 and April 29) I would like to come back to the confusion between the formula de Lefschetz-Verdier and the **occult** formula, \Box . 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 4¹" as its name suggests saves the day here. . . .) Beautiful work, yes!

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-massacre⁵³⁶ (*), 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-oeil 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 537 (**)),

□ it is said in chiaroscuro that a certain formula in the traces of Lecture XII (which the reader must work out for

himself

p. 916

⁵³⁷(**) 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.

p. 917

⁵³⁵(*) 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.

⁵³⁶(*) See the beginning of the quotation in the previous sub-note "Les prestidigitateurs - ou la formule envolée" (n° 169₈), page.

that the explicit fi xed-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.

p. 918

(in this formula, traces of exposé XII, the author of which will never be clearly named 538 (*))

 \Box - 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 "rabiot" 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

$$T=L, T=L$$

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

⁽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 4^1 - 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_5 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. ...

⁵³⁸(*) For the reader of SGA 5, it's Illusie, author of the brilliant exposé III_B on "local terms", who must appear as the modest father of the never-named formula. For a reader of the volume entitled "SGA 4¹", who hasn't heard of 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 4¹ " - 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 less⁵³⁹ (*).

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 future⁵⁴⁰ (**). 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,

p. 920

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^{1} - SGA 5" operation, we have seen just how far this new, innocent technique can go.

 $^{^{539}(*)}$ 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 fi guring in L and those fi guring 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...

⁵⁴⁰(**) For eloquent examples in this vein, see the few samples of the "1980" style that appear in the note "La maffi a" (n° 171₂), 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° 171₄.)

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**⁵⁴¹ (*). The latter has obviously reserved the lion's share for himself, with the motifs and staggered cohomology, and more specifically-

the 1-adic cohomological tool. The remainder(*) is shared between two other of my co-students.

homo^{\Box} logistes, **J-L. Verdier and P. Berthelot**⁵⁴² (*). 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 an- nexes everything that revolves around the yoga of duality⁵⁴³ (**), 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° 91₁). This is Berthelot's article-survey, which I quote there⁵⁴⁴ (***), 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 yoga⁵⁴⁵ (****).

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).

⁵⁴¹(*) (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. ...

⁵⁴²(*) (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 176₁ to 176₇.

⁵⁴³(**) See footnote on previous page.

 ⁵⁴⁴(***) 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".

⁵⁴⁵(****) on this subject, see the sub-note "Deaf ears" (n° 170(i)bis) which follows this note.

made of some of the ideas of \Box départ⁵⁴⁶ (*)). 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 motifs⁵⁴⁷ (**), 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)⁵⁴⁸ (***) 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 sixty⁵⁴⁹ (****). 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 \Box 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.

⁵⁴⁶(*) 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 0. 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).

⁵⁴⁷(**)See "Silence" (n° 168), especially "... and exhumation" (n° 168(iii)).

⁵⁴⁸(***) 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.

⁵⁴⁹(****) (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

p. 924 unnecessary assumptions such as smoothness)(*). Identifying "the right site" and "the right coefficients" is part of the pro-

gram 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 (a 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 ears⁵⁵¹ (*)...

(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.

⁵⁵¹(*) Having deaf ears doesn't stop the same Berthelot, in the article I quoted in the previous b. de p. note,

 $^{^{550}}$ (*) 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 definition had to be such that the notion of "generic fi bre" became tautological. . .

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*) \Box (February 28)⁵⁵² (*) To situate "Operation Duality", to the dubious benefit of J.L. Verdier, there are

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° 46₂ 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" operation⁵⁵³ (**).

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 \Box around the theme, methods that apply mutatis muntandis to all kinds of other contexts⁵⁵⁴ (*), such as topological spaces, or complex analytic spaces.

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)⁵⁵⁵ (**). 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.

p. 925

⁵⁵²(*) The text of this note was edited and corrected on certain points on May 1 (Lily of the Valley Day).

⁵⁵³(**) (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 "Coeffi cients" chantiers (n° s 3,4).

⁵⁵⁴(*) Of course, in the "other contexts" in question, the original diffi culty 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......

⁵⁵⁵(**) 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.

□ courses of 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 Survive 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

locally compact spaces, in the spirit of the six operations \Box (which always remain unnamed)⁵⁵⁶ (*). But

p. 928

p. 927

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.

⁵⁵⁶(*) On the subject of this rather unusual spirit, see the sub-note "Le patrimoine - ou magouilles et création" (n° 169₆ bis), and also last year's sub-notes (n° s 81₂, 81₃) 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 seminar⁵⁵⁷ (**), 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 idyllic⁵⁵⁸ (***).

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 "lescamotage de my person in the Hodge I, II, III articles⁵⁵⁹ (*), 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"⁵⁶⁰ (**)), 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')⁵⁶¹ (***).

Stage 2 (1976). 1976 saw the publication of the "memorable article" \Box de Verdier in Asterisk⁵⁶² (*), 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.

p. 929

⁵⁵⁷(**) 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° 81₁ to the much-quoted note.

⁵⁵⁸(***) 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".

⁵⁵⁹(*) 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!

⁵⁶⁰(**) See note n° 81.

⁵⁶¹(***) (May 1) See also the sub-note "Eclosion d'une vision - ou l'intrus" (n° 171₁) to the note "L'Apothéose".

⁵⁶²(*) 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, Ill 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 me⁵⁶³ (**) that he only became aware of Verdier's article after the publication of "SGA $4\frac{1}{2}$ " (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^{1} - 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 \Box peine at least to go through it - question of a quarter

hour at the most for someone "in the know" like him⁵⁶⁴ (*). 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....

Stage 3 (1977). In this "SGA 4^{\perp} - 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^{\perp} ", 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

⁵⁶³(**) See "Dotting the i's" (n° 164), part IV 1.

⁵⁶⁴(*) 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 view⁵⁶⁵ (**). 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, $_{p. 932}$ 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 forma- lism, 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.

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 Proceedings⁵⁶⁶ (*). 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^{1} " 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^{1}_{2} ", 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...).

This same article (signed Beilinson-Bernstein-Deligne) also marks the return in force of the sixoperations forma- lism (never named, of course) in the spread context, with the now consa- cerned notations I had introduced in the fifties. As I wrote elsewhere⁵⁶⁷ (*) "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".

⁵⁶⁵(**) As I explained in a previous b. de p. note (note on page), in the text-compendium entitled "SGA 4¹" 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.

⁵⁶⁶(*) 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.

⁵⁶⁷(*) 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 euphorie⁵⁶⁸ (**). 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 article⁵⁶⁹ (***), 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*) \Box (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 Berthelot⁵⁷⁰ (*). 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"⁵⁷¹ (**), 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 mathematics⁵⁷² (***), 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.

⁵⁶⁸(**) 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!

⁵⁶⁹(***) 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.

⁵⁷⁰(*) (May 2) In fact, a fourth "benefi ciary" should be added, whom I discovered only recently, namely Neantro Saavedra, mentioned in a previous b. de p. note (note (*) page 921).

⁵⁷¹(**) (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.

⁵⁷²(***) (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 \Box boycott p.

935

that they had to bear 573 (*).

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 year⁵⁷⁴ (**), 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'auréole⁵⁷⁵ (***). 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. ..." "⁵⁷⁶ (****) "great attention terminology....", "problems. ... in the line he tra- p .936

. . 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 lines⁵⁷⁷ (*)), 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,

⁵⁷³(*) (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).

⁵⁷⁴(**) 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"....

⁵⁷⁵(***) 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".

⁵⁷⁶(****) 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. ...

⁵⁷⁷(*) 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 ("Coeffi cients de De Rham et D - Modules")

18.5.4.1. a.The ancestor

Note 171(*i*) (March 1 and May 2-8⁵⁷⁸ (**)) In each of these four partial "operations" that I have distinguishedmy early burial, it's Deligne who visibly plays the role of conductor (or rather, Grand Officiant at the \Box Obsèques), with the more or less active participation of my other four coho- students.

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 conti- nuator".

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 geometry⁵⁷⁹ (*), had a major impact on my work. were pursued in near-complete solitude, having failed to interest anyone but myself⁵⁸⁰ (**). It was these reflections that \Box lead me to gradually draw out the notion of the derived category,

p. 938

nuclear, in perfect duality with the $H^{n-1}(X, \omega)$ (gesp. the $H^{n-1}(X, \underline{O})$). At the time, I didn't think of applying the same method to the case of vector fi bres (not having realized the very simple algebraic fact that the operatoro⁻ being \underline{O}_X -linear, extends to differentiable differential forms with values in a holomorphic vector fi bre), 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.

 $^{580}(**)$ 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 $\underline{E}_{X} t^{n-i}(X; F, \underline{w})$. 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

⁵⁷⁸(**) (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....",

[&]quot;... 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° 171₁ to 171₄) 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 171₁ to 171₄) 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). last. .).

⁵⁷⁹ (*) My first thoughts on duality were in the context of analytic spaces, and predate those of Serre. Using "evetesque" duality techniques and the Poincaré-Grothendieck lemma on the $\tilde{0}$ -operation (which I had just proved), I proved that if X is a Stein variety, the $H^i(X, O_X)$ (resp. $H^i(X, \omega_X)$) are Fréchet spaces. nuclear, in perfect duality with the $H^{n-i}(X, \omega)$ (resp. the $H^{n-i}(X, O)$). At the time, I didn't think of applying the same

whose objects were presented as natural "coefficients" in the homological and cohomo- logical 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 homologique"⁵⁸¹ (*), albeit in the language of derived categories.

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*, and *Extⁱ* 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 $i: X \to Y$, we can add the pair of adjoint functors Ri_1 , Ri'_1 , 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 quasicoherent bundle, without any smoothness or projectivity assumption on X (keeping only cleanliness, which then seemed essential⁵⁸² (**)). 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 mor- phism $f: X \to Y$ of otherwise arbitrary schemes. It was by means of approximations, on p .939 in the course of several years' work⁵⁸³ (*), 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 functor¹ 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

$$Lf^st$$
 , Rf_st , Rf^st .

To have a fully completed formalism, all that was missing was the description of an Rf functor₁,

Harvard seminar, published in 1966 ("Residues and duality" by R. Hartshorne, Lecture Notes in Mathematics, n° 20, Springer Verlag).

⁵⁸¹(*) In Tohoku Mathematical Journal, 9 (1957), p. 119-221.

⁵⁸²(**) See b. de p. (*) page 940, below.

⁵⁸³(*) 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 ^{me₅₈₄} (*).

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)⁵⁸⁵ (**), those \Box sur la dualité étale in one or two chapters of SGA

p. 941

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_1 , Rf^1 are adjoint), were written

⁵⁸⁴(*) Of course, I had realized that already in the case of an open immersion $f: X \to Y$, where the functor Rf^{t}

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_1 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_1 and to recover the coherent duality theorem for a separate morphism of type fi ni not proper, provided we worked with coeffi cients 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 coeffi cients" 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 coeffi cients" that I had been anticipating for ten years. ...

⁵⁸⁵(**) 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 fi ne developments intimately linked to this formalism: a residue theory (for fi neand 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 fi nally 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 notes⁵⁸⁶ (*). 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 friend⁵⁸⁷ (**). It is here, in this ruin of what was \Box one of the most beautiful seminars I've developed and, along with SGA 4, the most crucial of all p.

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 yoga⁵⁸⁸ (*), 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"⁵⁸⁹ (**), 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 on⁵⁹⁰ (*), and on the other hand constantly solicited by questions on themes often far removed from my own.

coeffi cients), and "Le tour des chantiers - ou outils et vision" (notes n° s 167,178).

⁵⁸⁶(*) 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"...).

 $^{^{587}}_{500}(**)$ On this subject, see the note "The four maneuvers" (n° 169 (ii)), and the sub-notes that follow.

⁵⁸⁸(*) (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 tobelieve 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. ... ⁵⁸⁹(**) 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 suffi sance" (developing the "melody", or the theme with variations, types of

⁵⁹⁰(*) 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 fascinating⁵⁹¹ (**). 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 coho- mology, 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

p. 945

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.

trade and a broad basis for their future work, everything was for the best' \square and for the most

My coherent duality works have never been very popular, it seems to me⁵⁹² (*). 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 comments⁵⁹³ (**). 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** \Box **vision**, childlike in its simplicity and

perfect elegance, which has nonetheless given eloquent proof of its power⁵⁹⁴ (*), remains ignored, the object of the

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.....

⁵⁹¹(**) 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....

⁵⁹²(*) 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....

⁵⁹³(**) For further details and comments, see the sub-note "Unnecessary details", n° 171 (v): in particular part (a), "Packages of a thousand pages...".

⁵⁹⁴(*) 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 men⁵⁹⁵ (**). 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 p. 946 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 coefficients⁵⁹⁶ (*). It doesn't seem not that any of my students could sense this problem 597 (**)' \square with the sole exception of Deligne. He devotes a 947

⁵⁹⁷(**) 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 subnotes 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 coeffi cient.

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

⁵⁹⁵(**) For some initial thoughts on this subject, see the sub-note "Freedom. . . "(n° 171(vii)).

⁵⁹⁶(*) 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)).

⁽May 14) In fact, as early as the 1950s, I knew that Serre's duality theorem could be generalized to the case of a com- plex 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 coeffi cients). 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

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 = 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 operations⁵⁹⁸ (*), 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" C-vector bundles on X^{599} (**), which is defined by transcendental means when the base field is the 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 600 (***), left behind by those who were my students. $\Box A$ common thread, which

p. 948

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 pen⁶⁰¹ (*).

a complex of such stratifiedpromodules, 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.

⁵⁹⁸(*) 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!

⁵⁹⁹(**) 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° 46₃).

⁶⁰⁰(***) (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.

⁶⁰¹(*) (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, Meb- khout's essential new ingredient is the microlocal analysis of Sato and his school. More precisely, Mebkhout borrows from them the notion of Dmodule 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 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 cohomology⁶⁰² (*). 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 $\underline{Cons}^*(X, \mathbb{C})$ the full subcategory of the derived category $D^*(X, \mathbb{C}_X)$ formed by the complexes of C-vector bundles on *X* with analytically constructible cohomology, and finally by $\underline{Coh}^*(X) = D^*_{coh}(X, \)$ the full subcategory of the derived category $D^*(X, \underline{O} \ X)$, formed by the complexes \underline{O}_X

of \underline{O}_X -Modules with coherent cohomology. Mebkhout highlights fundamental functions

$$\underbrace{Cons_{coh}^{*}(X, UC)}_{\stackrel{Np}{\xrightarrow{}} M} \underbrace{Coh}_{\substack{Np}{\xrightarrow{}} ppp} pp} \underbrace{Cris^{*}(X)}_{\stackrel{Np}{\xrightarrow{}} (X)}$$
(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 nature⁶⁰³ (**). It is **fully faithful**, and its essential image is the full subcategory by <u>*Cris*</u>^{*}_{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 \Box by Mebkhout⁶⁰⁴ (*), drawing inspiration above all (he tells me) from

my p . 950

comparison theorem between algebraic De Rham cohomology and analytic De Rham cohomology

(May 19) See also the sub-note "Dead pages" (n° 171(xii)).

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" . .

 $^{^{602}}$ (*) 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).

⁶⁰³(**) For an "explicit" description of a closely related functor M_{∞} , in the context of D^{∞} -Modules, see sub.

note already quoted n° 171 (ix), part (b); "La formule du bon Dieu".

⁶⁰⁴(*) 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"⁶⁰⁵ (**)) is described as a quasi-inverse functor of the functor

 $m: \underline{Cris}^*(X)_{\text{hol.rég.}} \rightarrow \underline{Cons}^*(X, C)$,

defined by

p. 951

p. 952

Π

 $m: F \longrightarrow DR(F) = \underline{RHom}_{D}(O_X, F),$

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")⁶⁰⁶ (*). 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 607 (**)! (To do so, we would have had to remember a

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... $^{608}(***)$) \Box Moreover, the

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

$$Ri^{\mathrm{mer}}_{*}(C_U) \to Ri_{*}(C_U)$$

of the "meromorphic direct image" of the restriction C_U from C to U, to the ordinary direct image, induces a quasiisomorphism 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).

- ⁶⁰⁵(**) 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 mystifi cation of the Colloque Pervers, or even of "L'Enterrement dans toute sa splendeur".
- ⁶⁰⁶(*) On this subject, see the aforementioned note "Les cinq photos (cristaux et *D* -Modules)" n° 171 (ix), part (a), "L'album "coeffi cients de De Rham" ".

⁶⁰⁷(**) (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_{∞} , M_{∞} relating to D^{∞} -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, (δ, Δ) and $(\delta_{\infty}, \Delta_{\infty})$, countervariant themselves, and more convenient in certain respects (cf. note cited above). These are the four "**Meckbhout contractors**".

⁶⁰⁸(***) (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 <u>RHom is</u> doing maths... . He didn't let it shake him, any more than I did when I introduced (in 1955) the *Extⁱ* 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 609 (*), 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 <u>*Cons*</u> category^{*} (X, C) (of "topological" nature) can be interpreted, thanks to the Mebkhout functor M, as a subcategory full <u>*Cris*</u> 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 sense⁶¹⁰ (*) The full sub-category in question, defined by

⁶⁰⁹(*) (May 5) In his thesis, Mebkhout states and proves the corresponding equivalence theorem for D^{∞} -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 "Eclosion d'une vision - ou l'intrus" (n° 171₁). By 1976, Mebkhout had come to the conviction that the two functors m, m_{∞} (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_{∞} . The result that fi gures in his thesis, concerning m_{∞} , is from 1978. By this time, he had all the ingredients for the demonstration (analogous, but with diffi culty 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_{∞} , 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).

analytically constructible, and regular holonomous D-complexes, or holonomous D-complexes.^{∞}

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 efficacy. ...

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 diffi culty he had to overcome was above all psychological, rather than technical: working against the grain, and entirely reduced to his lights alone... ...

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 maffi a" n° 171₂), nowhere in the literature, apart from Zoghman Mebkhout's work alone, is there any mention of the *m* or *m* functor_∞ or of a duality "philosophy", relating discrete coefficients precisely.

⁶¹⁰(*) 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**"⁶¹¹ (**), denoted by DRM^* (X) (or Meb^* (X)), or DRM^* (X/k) (or Meb^* (X/k) \Box in the schematic framework, when X is a finite-type scheme over a body k with zero characteristic⁶¹² (*).

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-

<u>*Cris* jets</u>^{*} $_{coh}(X)$ 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 \underline{O}_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)⁶¹³(**). That said, the two functors $\Box M$ and N are compatible with natural dualistic functors 614 (*).

⁶¹¹(**) 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 fin 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".

 $^{^{612}}$ (*) 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" $R\Gamma_{cris}$, 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...

⁶¹³(**) (May 5) The extension of my results on biduality, and on the stability of constructibility by the <u>RHom</u> 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 <u>*RHom*</u> 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^{615} (**)

$$R\Gamma_{cris}(F)_{!}, R\Gamma_{cris}(F)'$$

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)

$$H^{i}_{cries}(X, F) \times H^{-i}(X, F') \to C$$

(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"⁶¹⁶ (*).

⁶¹⁴(*) 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 $m = (G' \rightarrow DR(G) = \underline{RHom}_D(O_X, G))$, 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

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. ...

 $^{615}(**)$ I remind you (cf. "The five photos", n° 171(ix)) that the crystalline ("absolute") cohomology of F on X is defined as follows

 $R\Gamma_{cris}(F) = \underline{RHom_D(O_X, F)} R\Gamma(\underline{RHom_D(O_X, F)} = R\Gamma(DR(F)).$

On the other hand, the index ! designates the cohomology (crystalline in this case) with its own supports, i.e..

$$\mathbf{R}\Gamma_{!}(F) \stackrel{\text{dum}}{=} \mathbf{R}\Gamma_{!} \underline{RHom}_{D} (O_{X}, F).$$

⁶¹⁶(*) 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...).

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 $\underline{RHom}(F, G)$ 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....

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 bidua- lity) formalism for De Rham - Mebkhout coefficients on finite-type schemes over a characteristic body.

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 \Box 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.

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 coefficients⁶¹⁷ (*), described in the theorème du bon Dieu (aka Mebkhout the never-named. . .). Thus, it was almost twenty years after the creation of the cohomo logique 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

⁶¹⁷(*) (May 7) Precisely, to a holonomic D-Module (complex reduced to degree zero) the good-god functor associated in general a constructible complex of 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 $i := X \setminus Y' \to X$, and the sub-bundle of $i \cdot (O_U)$ 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 $Ri_{\star}(C_U)$, 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 C-vector bundles and certain stratified $Coh(\underline{O}_X)$ proobjects (the category of coherent Modules on \underline{O}_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 coeffi cients - 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. By \Box contrary, 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 equiva- lence (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.... ${}^{618}(*)$ 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 \Box de him ${}^{619}(*)$ - still he com- p .960

⁶¹⁸(*) (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° 171₄), especially page 1091.

⁶¹⁹(*) (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° 172₂ (e)).

munique in October the following year⁶²⁰ (**) 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 conjecture⁶²¹ (***) 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"⁶²² (****).

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".

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 \Box 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 purpose⁶²³ (*). 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 Colloquium⁶²⁴ (**) about the theorem he had modestly called Riemann's and

For further details, see the sub-note already quoted "La maffi a" (n° 171₂) parts (c) and (d).

⁶²⁰(**) (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° 171₂), part (d), "La Répétition Générale".

⁶²¹(***) The same conjecture is demonstrated, independently and nevertheless with a remarkable set, at the same time (at

⁶²²(****) 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.

^{623(*)} On this instructive episode, see the sub-note "La maffi a" (n° 1712), part (d) "La Répétition Générale (avant Apothéose)".

⁶²⁴(**) (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.

p. 962

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)⁶²⁵ (*).

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 coeffi cients (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.

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 jacobiennes 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).

⁶²⁵(*) (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 Bry- linski 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° 171₄). 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 mystifi cation-escroquerie of the Colloque Pervers - not to mention Hotta's thirteen!

p. 963 □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 Verdier⁶²⁶ (*)) 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 correspon dance, which the lead author

p. 964 little room for doubt as to the true authorship of this conception database, which the lead author and presenter-prestidigitator⁶²⁷ (*), 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 Mebkhout⁶²⁸ (**).

a1. Unnecessary details

Note 171(*v*)⁶²⁹

(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".

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).

⁶²⁶(*) This is the implicit "opinion" clearly expressed in the above-mentioned Introduction to the Colloquium, signed by Teissier and Verdier.

⁶²⁷(*) 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").

⁶²⁸(**) (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 "Eclosion d'une vision - ou l'intrus", "La maffi a", "Les racines", "Carte blanche pour le pillage" (n° 171(i) to 171(iv))

⁶²⁹(***) 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. . .)⁶³⁰ (*), 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)⁶³¹ (**). 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).

 $[\]frac{630}{(*)}$ (May 27) For a further reflection on the evocation of Serre, see part (c) of this note.

⁶³¹(**) (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 is irresistibly suggested by this formalism) and technical (in the sense that formalism also gives the

p. 967

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- 1970⁶³² (*). 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 and⁶³³ (**) patterns.

p. 968

 \Box 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 Eve⁶³⁴ (*)) 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).

 $^{^{632}}$ (*) (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_1 (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 coeffi cients" 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).

⁶³³(**) (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 coeffi cients 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.).

⁶³⁴(*) 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)⁶³⁵ (**) The way I express myself about Serre came about spontaneously, and stems from a perception of things, the pourtant, which must have formed in me over the last few weeks or months. There has been 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"!

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

p. 970

 $[\]frac{635}{(**)}$ 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 "blinders", 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 tran- chased, "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"⁶³⁶ (*), 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).

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**, \Box raide and cold, for impeccable execution

of "snatch-and-grab" tasks, tasks at public $\operatorname{auction}^{637}(*)$ - 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 incommo- der us; indeed, to worry us, when they call us into question ourselves. I know that Serre, no more than

⁶³⁶(*) 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.....

[&]quot;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.

⁶³⁷(*) 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 offi cial 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 $^{.972}$ 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.

 \Box 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 forest⁶³⁸ (*).

But that hasn't stopped me from clearing, planting and replanting, pruning and replanting and replanting and replanting.

⁶³⁸(*) (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 alone⁶³⁹ (**).

a2. Crazy questions

Note 171(vi) (May 5)⁶⁴⁰ (***) 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 \Box 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 course⁶⁴¹ (*)'. 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

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 <u>*RHom*</u>_{O_x} (-, <u> O_x </u>), 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

⁶³⁹(**) (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.

 ⁶⁴⁰(***)This note is taken from a note by b. de p. to the note "L'ancêtre" (n° 171 (i)) - see note (*) on page 946.
 ⁶⁴¹(*) It's also clear, when the base body was C, that we wanted a category equivalent to that of 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 subschema (not necessarily a C-vector system), a category equivalent to that of 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. ...

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 coeffi cients and De Rham - Deligne coeffi cients, 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 caracté \Box ristique p . 975. Better late than never.... ⁶⁴²(*).

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"⁶⁴³ (**) (NB in the language of D-Modules, this is the extension of the Ring of scalars $\underline{O}_X \rightarrow D_X$, for smooth X at least. ...) - and even if I had seen it, the crystal obtained (already for $F = \underline{O}_X$, 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 evoesque 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 duality⁶⁴⁴ (*) 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. . .

(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 cohomolo- gy. Technically speaking, this was pretty much the equivalent of the algebraic version of the

⁶⁴²(*) (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 coeffi cients, at least for a finitype scheme on *Spec* Z, (which appears to me as the 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 fi ni type schemes over *Spec*(Z), must also give the right De Rham coeffi cients (Mebkhout or Deligne style, your choice) for fi ni type schemes over any body (of zero characteristic, or

no). I intend to outline this approach in the "Coeffi cients de De Rham" section of volume 3 of Réflexions, among other "technical digressions" that my students can come and copy at their leisure. ...

⁶⁴³(**) (May 26) It may be better to take the enveloping "co-crystal" (see note 171 (ix) part B, for allusions to the notion of cocrystal). I'll no doubt come back to this question in the presentation promised in the previous b. de p. note.

⁶⁴⁴(*) 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.

What was missing, then, was a good notion of "quasi-isomorphism" for a (differential) morphism between complexes of differential operators, \Box 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 setting⁶⁴⁵ (*)). 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 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.

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 \Box well be cleared up one day

p. 978 arour suspe was t

- 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"

 $^{^{645}}$ (*) 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 ho- momorphism 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 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. . . $^{646}(*)$

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 \Box telle dissatisfaction so vague and so elusive and very real p . 979 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)⁶⁴⁷ (*) 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 semailles⁶⁴⁸ (**). And yet, in my relationship with mathematics at least, I think I can say that throughout my life I have maintained a good-natured simplicity⁶⁴⁹ (***), while at the same time

 $^{^{646}(\}ast)$ On this subject, see the reflection in the sub-note ". . and hindrance" (n° 171 (viii)).

⁶⁴⁷(*) This sub-note is taken from a b. de p. note to the note "L'ancêtre" (n° 171 (i)) - see note (**) on page 945.

⁶⁴⁸(**) (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 Renouvellement. 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 egotic 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. . .

⁶⁴⁹(***) (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 Renouvellement, 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

p. 981

than a fidelity to my original nature \Box . 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 flair⁶⁵⁰ (*).

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^{651} (**), 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" $(n^{\circ} 97)^{652}$ (***), in the particular context of \square ! 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"⁶⁵³ (*); 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 me⁶⁵⁴ (**). 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

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.

 $^{^{650}(*)}$ See previous b. de p. note for reservations on this subject.

⁶⁵¹(**) (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 Renouvellement ("Work and Discovery" and "The Dream and the Dreamer"), sections 1 to 8.

⁶⁵²(***) 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.

⁶⁵³(*) 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).

 $^{^{654}(**)}$ 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.

 \Box When it came to other people's work, which I was in a position to understand and thereby appreciate or judge

(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 Outline⁶⁵⁵ (*). 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 geometer⁶⁵⁶ (**).

 \Box 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

⁶⁵⁵(*) These "Comments" are announced in "Compass and Luggage" (Intr. 3).

⁶⁵⁶(**) By way of example (among many others), I'd like to point out the principle of reducing statements about relative schematic situations "of fi nite 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)⁶⁵⁷ (*) 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 category⁶⁵⁸ (**). The coefficients it introduces (via a condition of_{\Box}

p. 984

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 de- scribe, "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 coho- mology of algebraic varieties. Eight years later, he left it to someone else to come up with a somewhat dual and more penetrating approach⁶⁵⁹ (*), which would immediately⁶⁶⁰ (**) 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

p. 985

"explanation" simply doesn't hold water! Surely it's not because an approach to a neuralgic question would be "too simple",¹ 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 question⁶⁶¹ (*)!

⁶⁵⁹(*) I have no doubt that if Deligne hadn't dropped De Rham's coeffi cient theme (which he inherited from

⁶⁵⁷(*) This note is taken from a b. de p. note to the note "The ancestor" (n° 171 (i)), see note (*) on page 947.

⁶⁵⁸(**) This is the category (noted as <u>*Cons*</u>^{*} (*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 $D^*(X, C_X)$.

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.

⁶⁶⁰(**) 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. . .).

⁶⁶¹(*) 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 yoga⁶⁶² (**) (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 yoga (**) (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 it⁶⁶³ (***)), 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' (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. . . "⁶⁶⁴ (*). 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... ...

cohomology students, whom I have known to be gifted with fi ne intuition, see sub-note n° 91₂ to the note "Les cohéritiers... ". ⁶⁶³(**) This seems to be clear from Serre's report on Deligne's work, quoted in sub-note no.° 165₁ on the note "Requiem pour vague squelette" (notably p. 813). For an explanation of this fi liation, see "Les points sur les i" (note

 n° 164), I 4 (in particular p. 793), and its sub-note n° 164₁.

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 diffi cult**?" (Emphasis added).

of being closer to geometric intuition, and Mebkhout's being technically simpler (avoiding recourse to pro-objects), and in various respects deeper.

⁶⁶²(**) 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 topossic 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

⁶⁶⁴(*) 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...):

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 diffi culty". These connections make me realize that in my friend Pierre's stereotypical "c'est trop diffi cile....", 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...).

p. 987

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 \Box 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 occasion⁶⁶⁵ (*). 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 egotic 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 pursued⁶⁶⁶ (**).

b1. The five photos (crystals and D-

Modules) Note 171(ix) ⁶⁶⁷

"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 coeffi cient probléma- tics, and of a formalism of the six operations (and biduality) to be established for such coeffi cients(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 nonintellectual 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.

⁶⁶⁵(*) 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).

⁶⁶⁶(**) 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.

 $^{^{667}(*)}$ 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.

(a) The "De Rham coefficients" album \Box (May 4 and May 19-20) I remind you that for an analy-In complex smooth dynamics, D_X (or simply D) refers to the ring bundle (or, more precisely, 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 \underline{O}_X -Modules, where we take as

morphisms, not just \underline{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 contrafonctor⁶⁶⁸ (**)

$$F' \to \underline{Hom}_{O_{Y}} (F, D_d \xrightarrow{\sim} \underline{Op \ diff}(F, \underline{O})_X$$
(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 \Box of D on the left on itself (which make the second member) .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, "commutating 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:

$$C \rightarrow \underline{Hom}_D(C, \underline{O})_X$$
 (2)

This is a contravariant functor, which might seem to provide "the" natural candidate for a quasiinverse functor of (1). The trouble is that this bundle (2) is not naturally provided with an <u>O</u> structure_X -Module, but only with a C structure_X -Module (where (C_X is the constant bundle on X defined by the field of complexes C). When C comes from a locally free <u>O</u>_X -Module F by the contrafonctor (1), then (2) is canonically isomorphic to the 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"⁶⁶⁹ (*)) 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 differ- rential 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 Mebkhout⁶⁷⁰ (**) represents a **crucial relaxation**: \Box it's thanks to this point of view ⁹⁹⁰

⁶⁶⁸(**) The isomorphism written here is $u' \to \varepsilon \circ u$, where $\varepsilon : D \to \underline{O}_X$ is the "augmentation" $\theta \to \theta(1)$.

⁶⁶⁹(*) In Tohoku Mathematical Journal, 9 (1957) p. 121-138.

 $^{^{670}(**)}$ (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 stale cohomology, which was (in 1963) **the** first major theorem to kick-start stale cohomology, leading in the space of a few weeks to a situation of almost complete "mastery" of the stale 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"⁶⁷¹ (*)), is that formed by the complexes of D-Modules with **coherent** cohomology bundles (as D-Modules), seen as a full subcategory.

$$D^*_{coh}(X, \quad) \text{ or } \underbrace{Cris^*}_{Ch}(X) \tag{3}$$

of the usual derived category $D^*(X, D_X)$. If we restrict ourselves to complexes with bounded cohomology (for-

mating the full subcategory $\underline{Cris}^{b}(X)$, 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.⁶⁷²

p. 991

 \Box When working with derived categories, it is of course necessary to replace the fundamental functors

(1) and (2) by the total derived functors

$$F \to \underline{RHom}_{\underline{O}_{X}}(F, D_{X}), C \to \underline{RHom}_{D}(C, \underline{O}_{X}).$$
 (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) :

$$F \to D \otimes_{\underline{O}_{\mathsf{X}}} F, \tag{5}$$

(total tensor product), where in the tensor product we still use the <u>O</u> 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

 $[\]overline{}^{671}(*)$ This is the formalism of the six operations and biduality, which I developed in the coherent framework in the second half of the 1950s.

⁶⁷²(**) (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).

⁶⁷³(*) It is known that *D* is flat as an \underline{O}_X -Module on the right or left (can be seen immediately on the canonical fi ltration 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

$$\underline{Cris}^*_{coh}(X) \to D^*(X, \mathbb{C})_X,$$

also admits an important covariant counterpart, given by

$$C \to \underline{RHom}_D(O_X, C) = \overset{\text{din}}{D}R(C) (\text{"De Rham complex" associated with C}),$$
 (6)

where the second member is indeed explained by a De Rham complex, thanks to the canonical Spencer resolution of \underline{O}_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 $C \rightarrow \underline{Hom}_D (O_X, C)$, 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

 $R\Gamma_X(C)$ ' $RHom_D(X; D, C)$

but the functor (familiar to me as a total crystal cohomology functor) derived totally from the func-

horizontal sections (global)" teur $C' \rightarrow \underline{Hom}_D$ ($\underline{O}_X C$); I denote this total derivative $R^{\Gamma cris}(C)$, so that p. 992 we have tautological isomorphisms

$$R\Gamma_{cris}(C) \stackrel{dfn}{=} RHom_D(\underline{O}_X, C) ' R\Gamma_X(DR(C)), \qquad (7)$$

i.e. the crystalline cohomology of C on X is obtained by taking the ordinary (global) cohomology of the associated De Rham complex.

In <u>*Cris**</u> $_{coh}(X)$ 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):

$$D: \underline{Cries}^{*}_{coh}(X) \approx {}^{*}(X).$$

$$(8)$$

$$Cris_{coh}(X) \xrightarrow{} Cris_{coh}(X)$$

It is an anti-equivalence, essentially involutive (i.e. we have a biduality isomorphism, functorial in C:

$$C \bullet D(D(X))). \tag{9}$$

(9). This functor transforms (by composition) the contrafunctors (1) and (2) into cova- riant 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)

RHom_D(
$$O_X$$
, C) $\stackrel{\bullet}{}$ *RHom_b*(C' , Q_X), and vice versa. (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 $\underline{Hom}_{\mathcal{O}_X}$ (*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,

$$F \rightarrow \underline{Hom}_{\underline{O}_{X}}(F, \underline{\omega}_{X}) \quad F \otimes_{\underline{O}_{X}} \omega_{X}(F \text{ an } \underline{O}_{X} \text{ -Module loc. lib. of finite type}),$$
(11)

where $\underline{\omega}_X$ denotes the "dualizing module" of differential forms of maximum degree on X. Note that De Rham's functor

$$DR: D^*_{colo}(X, D \rightarrow D^*(X, \mathbb{C})),$$

does not in general commute to dualizing functors (taking in the second category the <u>*RHom*</u> 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

$$\underline{Cris}^{*}(X)_{Ho} \xrightarrow{} \underline{Cons}^{*}(X, \mathbb{C}) (' \rightarrow D^{*}(X, \mathbb{C}))$$

p. 993 there is commutation to dualizing functors. I do not "recall" here the condi tion 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 $Cons^*$ (*X*, C). Introducing the notion of Mebkhout **regularity**, also local and "algebraic" in nature⁶⁷⁴ (*), we find the "God's functor" (aka Mebkhout) $m: Cris^*(X)$ (12)

hol rég
$$\rightarrow \underline{Cons}$$

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)

$$M: \underline{Cons}^{*}(X,) \xrightarrow{\sim} {}^{*}(X) \xrightarrow{'} Cris^{*}(X)$$

$$\xrightarrow{-} \underline{Cris}^{hol reg} \underline{\qquad}_{coh}^{(13)}$$

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^* $_{cons}(X, D) = \underline{Cris}^*_{coh}(X)$, 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" condition⁶⁷⁵ (**), playing the role of "topological" point of view.

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

⁶⁷⁴(*) 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 coeffi cients ("Mebkhout" style or "Deligne" style), in volume 3 of Réflexions.

⁶⁷⁵(**) 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 $i_1(F)$, where $i : Y \to X$ 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 fi ni (or "local system of 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 "ana- lytic" 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 " 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)⁶⁷⁶ (*).

Photo 1 has the advantage of making sense when *X* is no longer assumed to be smooth, but is a complex ana-lytic 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 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 topo \Box logy). 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.

p. 995

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 <u>Cons</u>^{*} (X, C) can be interpreted as the full subcategory of <u>Cons</u>^{*} (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

⁶⁷⁶(*) 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 <u>Cris*</u>(X), <u>coh</u> 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⊔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 ξ on an open U of X, associates a " ξ -derivation" θ_{ξ} of F, linearly in ξ , and compatible with the "hook" operation of derivations⁶⁷⁷ (*). We can say that this is a structure of a "differential" nature on F, of order 1.

Since we're in characteristic zero⁶⁷⁸ (**), this structure can also be interpreted as a richer struc- ture, 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 \Box of an isomorphism, above the formal completion of *XxX* along the diagonal,

between the two inverse images of F (by the two canonical projections pr1 and pr2), 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

⁶⁷⁸(**) 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 $Xx_S X$ along the diagonal, by the formal completion "with powers S divided". This also leads/ for an <u>O-bundle_x</u> -Module F on X, to replace the pro-bundle P(F) of its "principal parts" of order infi ni", by "principal parts with divided powers (of order infi ni)". On the dual side, this amounts to replacing the D-

 $[\]overline{^{677}}(*)$ Of course, a compatibility condition is also required for the restriction to an open.

ring 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 coeffi cients, which will extend Mebkhout's from the coeffi cients to the coeffi cients.

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 site⁶⁷⁹ (*), ringed in **com-mutative local rings**, namely the "crystalline site" formed by the thickenings U' of the various *U*-openings of *X* (the bundle

structural crystalline being simply $U \to \Gamma(U, O)$. 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 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 crystalline⁶⁸⁰ (**).

Thus we have a perfect dictionary, explained at length in my 1966 talks already cited⁶⁸¹ (*), p. 998 between four object types on *X*, or four structure types on an O_X -Module:

$$\Box D-Modules$$

$$\Box O_X - Integratable connection modules$$

$$Stratified modules (infinitesimal descent data of infinite order)$$

$$\Box O arrestals - Modules$$
(Cr)

O crystals_{*X*} -<u>M</u>odules

This dictionary is valid without any coherence or quasicoherence restrictions on F. Note, however, that if we compare the extreme terms

D-Modules $\Leftrightarrow \underline{O}_{crystals_X}$ -Modules

the natural notions of "coherence" in either context **do not correspond**. The crystalline struc- tural 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 nature⁶⁸² (**), 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 prerather, 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 cry-

rate of \underline{O}_X -Modules, as in the smooth case⁶⁸³ (***).

⁶⁷⁹(*) 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]).

⁶⁸⁰(**) 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. . .).

⁶⁸¹(*) See the presentation [Crystals], cited in the first footnote to this sub-note (note (*) on page 988).

 $^{^{682}(**)}$ In fact, it's the *D* -coherence, of course (which had escaped me in the sixties) that is the important notion of fi nitude here. $^{683}(***)$ this assertion was made hastily, and is false as it stands. For it to be true, the "site" must be replaced by "site".

 \Box confess that, because I haven't given it much thought, I can't quite visualize the exact relationship yet, for X

plunged

p. 999

p. 1000

in X' smooth (let's say), between crystals on X and crystals on X' (and this even when X itself is smooth)⁶⁸⁴ (*). 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 $f: X \to X'$ is a morphism between analytic spaceswe deduce

$$_{fcris}: _{Xcris} \rightarrow X' \quad _{crie};$$

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 $X' \rightarrow X'$, 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 $D^*(X_{cris}, O)$

and F its image by the total derivative functor of f_{cris^*} , and further assuming X smooth, the condition that F is 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 <u>Cons</u>* (X, C) ne varietur, and there's the category (which for the moment remains hypothetical, and which as it stands I still can't see⁶⁸⁵ (*)) of "De Rham - Mebkhout" coefficients DRM^* (X), for which I've just hazarded a principle of definition. The <u>Cons</u> category* (X, 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 modules⁶⁸⁶ (**).

772

crystalline", formed by all infi nitesimal 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 stratific cation, 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 Rhamtype coeffi cients 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)).

⁶⁸⁴(*) (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.

⁶⁸⁵(*) I allude below to a "fifth photo", which is much clearer for me right now, to capture De Rham's "good" coeffi cients 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.

 $^{^{686}(**)}$ 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 with⁶⁸⁷ (***). A fortiori, this functor does not commute "to the six operations". The in-

tuition which \Box (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**⁶⁸⁸ (*) that I've been keeping in reserve: it's Deligne's, with stratified pro-modules⁶⁸⁹ (**). 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 (" \underline{O}_X -duality") on \underline{O}_X -Modules (at least, initially, for smooth *X*) - I've already alluded to this in a previous footnote⁶⁹⁰ (***). I have so I have the impression of being on solid, familiar ground, where I can recognize myself,

so that we the impression of being on solid, raining ground, where I can recognize myself,

as soon as I get the chance. I even thought of sketching \Box (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 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").

$$M: \underline{Cons}^* (X, \mathbb{C}) \to \underline{Cris}^* (X \qquad \dim_{coh} D) (= D^*_{coh} (X, D)$$
(1)

$$\begin{array}{ccc} \mathsf{R}\Gamma^{alg} (\underline{O} & \overset{\bullet}{\mathsf{R}}\Gamma^{alg} & \overset{L}{} & {}^{alg} \\ & & & & \\ & & & \\ & & & & \\ & & & & \\ & & & \\ & & & \\ & & & \\ & & & \\ & & & \\ & & & \\ & & & \\ & & & \\ & & & & \\ & & & \\ & & & \\ & & & \\ & & & \\ & & & \\ & & & \\ & & & \\ & & & \\$$

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....

⁶⁹⁰(***) This "previous b. de p. note" has since been transformed into part (c) of the present "Five photos" note.

module crystals as special cases of crystalline modules.

 $^{^{687}(***)}$ 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...):

⁶⁸⁸(*) 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 coeffi cients" alone, and the second crystalline coeffi cients in general.

⁶⁸⁹(**) 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... ...

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 <u>Cris</u>^{*}_{coh}(X),

$$m = DR/DRM^*(X) : DRM^*(X) \xrightarrow{\text{dfn}} Cris^*(X) \xrightarrow{\text{holrég}} \rightarrow Cons^*(X, C)$$
(2)

which turns out to be an equivalence ("God's theorem"). In fact, Mebkhout obtains a remarkable direct description of the function M_{∞} , deduced from the functor M by the "scalar extension" functor i through the **Ring homomorphism**

where D^{∞} (or D^{∞}) 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 \underline{O}_X , seen as a bundle of complex topological vector spaces. It is known that D^{∞} is faithfully flat on the left and right on D, so that the total derived functor of the Ring extension functor

$$i: \underline{Cris}^* (X) = D(X, D) \to D(X, D) \to D(X, D^{\infty}) \stackrel{\text{dfn}}{=} \underline{Cris}_{\infty} (X)$$

$$(4)$$

is explained by an ordinary tensor product. Note that we don't know whether the D-ring^{∞} is coherent, but apparently we don't need to. We define the full subcategory

$$\underline{Cris}^*(X)_{HO} \xrightarrow{} \underline{Cris}^*(X)$$

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 \Box (De Rham -Mebkhout", and this determines C on all X to within a single isomorphism...) Consider the functor $M_{\infty} = i$ *M*, fitting into the commutative diagram

DRM* (X)

°,,,,M

 $= Cris^*(X)_{holrég}$

It turns out (or rather, the unknown worker proves...) that the functor M_{∞} is also a category equivalence (so *i* is too). It can also be obtained as a quasi-inverse of the functor m_{∞} , of the "De Rham" type analogous to m, defined on <u>Cris</u>^{*} (X)_{hoL}. To describe the functor M_{∞} , it is more convenient to describe the contractor

$$\Delta_{\infty} \stackrel{\text{dun}}{=} M_{\infty} D = D M_{\infty\infty} = i(MD) = i(DM) , \qquad (6)$$

where D denotes the dualizing functor already mentioned, in <u>Cons</u>^{*} or DRM^{*}, and D_{∞} , the dualizing functor simi-(X)). (NB The three functors involved laire that exists in $Cris^*(X)$ $_{HO}$ (and even in <u>Cris</u>*

in (5) commute to dualistic functors). The quasi-inverse δ_{∞} of Δ_{∞} is therefore given by the formula

(5)

similar to (6)

$$\delta_{\infty} \stackrel{\text{dff}}{=} Dm_{\infty} = m \, D_{\infty\infty} \tag{7}$$

We then find the Mebkhout expression of Δ_{∞} , δ_{∞} by the following two remarkably symmetrical formulas:

$$\begin{pmatrix} \Delta_{\infty} (F) = \underline{RHom}_{C} (F, \underline{O}) \\ \lambda_{X} \\ \delta_{\infty} (C) = \underline{RHom}_{D} (C, \underline{O}) \\ \lambda_{X} \end{cases}$$

$$(8)$$

Note that in the first of these formulas, the second member inherits a D^{∞} -structure, thanks to the operations of D^{∞} ; on the second argument \underline{O} , while in the second formula, the second member is interpreted simply as a complex of C-vector bundles. The second of these formulas, put there "for the record", is essentially tautological, and simply states that the functor δ_{∞} associates with the D complex^{∞} -Modules C the complex of differential operators (of infinite order) "adjoint" to that associated with C (by De Rham's DR functor_{∞}) - this complex being interpreted as a complex of C-vector bundles. (That a complex with constructible cohomology bundles can be found in this way is equivalent to Kashiwara's constructibility theorem).

 \Box (It is a profound theorem, on the other hand, that the first functor Δ_{∞} transforms constructible bundles into (complexes of) D^{∞} -Modules that are holonomic. The finiteness theorem alone implied by this result⁶⁹¹ (*) (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 <u>*Cons*</u>^{*}, or in

the Cris category
$$(X)_{HO}$$
 - except that the "dualizing" contrafractors (expressed in both cases as

as a <u>*RHom*</u>_{∞} (-, <u>*O*</u>_{*X*})) connect two **different** categories. It was this formal analogy that led Mebkhout to call the theorem that affirms isomorphism

the "**biduality theorem**" for *D-complexes*^{∞} -Modules (a potentially confusing terminology). This relation, plus the fact that the functor δ_{∞} is fully faithful (or, more precisely, that Δ_{∞} 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^{∞} -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 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^{∞} -Modules is a quasiisomorphism 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)⁶⁹² (**).

⁶⁹¹(*) This fi nitude result implies, for example, that locally on *X*, the complex $R \underline{Hom}_{\infty}(F, \underline{O}_X)$ is isomorphic (in the derived category) to a complex of D^{∞} -Modules that is locally free of type fi ni 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.

⁶⁹²(**) (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^{∞} -Modules is quasi-zero, so is the latter (and so are the *D*-Modules).
$_{p.1005}$ \Box (Mebkhout's biduality theorem is in some ways "half" of God's theorem

(for D^{∞} -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 op- tic 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-Kawai⁶⁹³ (*). It was renamed "reconstruction theorem" for the occasion, and without the slightest al- lusion to a certain Zoghman Mebkhout. Incidentally, it was also the memorable year of the Colloque Pervers - the glorious year when a certain "new style"⁶⁹⁴ (**) conquered with aplomb (and without encountering the slightest resistance...), that part of mathematics, of all places, where I used to feel at home.

^{p. 1006} (c) The fifth photo (in "pro" \Box (May 21) The "biduality theorem" (9) is from 1977. To proumode)

ver the other half of the "God's theorem" for D^{∞} -Modules, which therefore amounted to proving that the functor δ_{∞} is essentially surjective, a first difficulty was to prove that for F in <u>Cons</u>^{*}, and by defining the D complex^{∞} -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^{695} (*). This idea had been deve- loped 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

⁶⁹³(*) 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-Pacifi que".

⁶⁹⁴(**) 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).9

⁶⁹⁵(*) 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

 $J \rightarrow Hom_{\mathbb{C}}(F, J)$

on the category of \underline{O}_X -Module coherents 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 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). We see (returning to the general case) that the pro-beam (F_i) is equipped with a canonical stratification 696 (**). Deligne's idea is that the "**functor of** \Box (**Deligne**" from category p ...1007 of constructible 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 697 (*)), in purely algebraic terms too. This is the category I'll call

$$DRD^*(X) \text{ or } Del^*(X), \tag{10}$$

which constitutes the "**fifth photo**", which I didn't want to explain yesterday⁶⁹⁸ (**). 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 <u>*RHom_C*</u> (*F*, <u>*O*_X</u>) should be very naturally definable as an inductive limit (in a suitable sense) of <u>*RHom_{O_X*</u> (F_{i} , *O_X*) and in particular (passing to cohomology bundles), we describe canonical arrows</u>}

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 \Box (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^{∞} -Module deduced from the first by extension of the scalars⁶⁹⁹ (*) - 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).

⁶⁹⁶(**) 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 "fi brished 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.

⁶⁹⁷(*) "Tautological" at least in terms of the already familiar dictionary (first developed by Deligne) between bundles of locally constant 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.

⁶⁹⁸(**) 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.

⁶⁹⁹(*) 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

$$\lim_{\stackrel{\longrightarrow}{n}} \underline{Ext}^d (\underline{O}_{X_n} \underline{O}_X),$$

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 $\underline{O}_{X,x}$, 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 sixties⁷⁰⁰ (**). In any case, the main theme of Mebkhout's work between 1972 and 1976 was to study arrow (11) in this crucial case.

$$\lim_{n \to \infty} \underbrace{Ext^{d}}_{\text{ox}} (\underline{O}_{\text{xn}}) \xrightarrow{H^{d}}_{=} (\underline{O}_{\text{Y}})_{X \ alg} \xrightarrow{- \to} \underbrace{H^{d}}_{Y} (\underline{O}_{\text{Y}}).$$
(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,

р. 1009 🛛 🖡

the analogous statement for (11) must be an immediate consequence by unscrewing⁷⁰¹ (**)' \Box (including in the case where *F*, instead of being a constructible C-vector bundle, is a complex in <u>Cons</u>* (*X*, C). The only grain of salt, apart from the Deligne functor construction, is in the definition of the <u>RHom_{Ox}</u> 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, \underline{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" goodgod functor M (as opposed to the "transcendental" good-god functor M_{∞}), or rather of the associated contrafunctor Δ and its quasi-inverse δ

$$\Delta = MD = DM, \qquad \delta = mD = Dm, \tag{13}$$

by a double-formula that paraphrases (8). But to write it, using Deligne's equivalence

$$Del: \underline{Cons^*}(X, \mathbb{C}) \xrightarrow{\approx} DRD^*(X) \tag{14}$$

we'll look instead at the corresponding functors Δ^{\uparrow} , δ^{\uparrow} between $DRD^*(X)$ and $DRM^*(X)$, where the \uparrow 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"

⁷⁰⁰(**) 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*.

 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):

$$\begin{aligned} \mathbf{A}^{(C')} &= \underline{RHom}_{\mathcal{O}_{X}}(C', \underline{\mathcal{O}_{X}}) \\ \delta^{(C)} &= \underline{RHom}_{\mathcal{O}_{X}}(C, \underline{\mathcal{O}_{X}}). \end{aligned}$$
(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 thing⁷⁰² (*), 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{O}_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. . .).

 \Box (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{O} 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^i , the functor $F \rightarrow \underline{Hom}_{Q_X}(F, D_d)$ (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^i , 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 <u>RHom</u> reduces to <u>Hom</u>. (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 $P^{\infty}(L^i) \rightarrow Q$ correspond exactly, just like the linear homomorphisms $L^i \rightarrow D$, to the differential operators $L^i \rightarrow Q_X$, using respectively the "universal" differential operator (of infinite order) $L^i \rightarrow P^{\infty}(L^i)$, and the "augmentation" D_X data $\rightarrow Q$

by $\theta \to \theta(1)$.). Since at least locally on *X*, any object of <u>*Cris*</u>^{*} $_{coh}(X)$, (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 \Box (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-

 $[\]overline{}^{702}(*)$ See b. de p. note (**) on page 1006, about this translation.

are worth⁷⁰³ (*) to Mebkhout's theorem mentioned above (dating back to 1976, even before the Good God theorem was demonstrated), concerning the D-coherence of <u>*H*</u> beams^{*d*}_{*Y*}(<u>*O*</u>)_{*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 consequence⁷⁰³ (*) 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 probeams of P^{∞} (*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 $Z \subseteq Y \subseteq X$ 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

^{p. 1012} smooth) is "regular" in the vicinity of Z, in the sense of Deligne⁷⁰⁴ (**). (NB. The associated pro-beam is obtained by growing the \Box (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....)

(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" <u>Cons</u>* (*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 (procoherent) pro-Modules⁷⁰⁵ (*). 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 $U' \to U'$ of thickenings, cannot even be interpreted as pro-beams on the crystal site (or what amounts to the same thing, on the topos

⁷⁰³(*) (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.

 $^{^{704}(**)}$ 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 fi bres with integrable connection on *Y* - *Z*, provided with a "meromorphic structure" along *Z*, and with regular connection along *Z*. This meromorphic structure (implying the possi-bility 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. ...

⁷⁰⁵(*) (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, when $\underline{Del^*}(X)$ is a challenge, as Deligne does, without recourse to the crystalline site. It is perhaps even for this reason that

that he fi nally 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 profiled 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 \Box (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)⁷⁰⁶ (*). 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 $D^*(X_{cris}, O)$, defined_nby 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... ...

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** <u>*RHom*_{$O_X}</u>, without bothering with pro-questions (but simply taking care to transport stratifications...). A first test along these lines would be as follows: if <math>u: C_1 \rightarrow C_2$ is</u></sub>

a morphism of D-Module complexes with coherent cohomology, such that its image by the naive dualizing functor <u>*RHom*</u>_{O_X} (-, <u> O_X </u>) 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 $\lim_{t\to -}$). Mebkhout will surely be able to tell me....

 \Box ((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...

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 <u>Cris</u>^{*} 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".

$$L^{\cdot} \rightarrow P^{\infty}(L^{\cdot}),$$

p. 1014

⁷⁰⁶(*) 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

p. 1015

$$\underline{H}^{i}(L^{\cdot}) \longrightarrow \underline{H}^{i}(P^{\infty}(L^{\cdot})) \quad (i \in \mathbb{Z})$$
(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, \Box (going from category $D^b (\sum_{cont} D)$ (say), to a crystalline idon category, such that $D^b (X = Q = x)$ is itself fully faithful.

such that $D^b(X_{cris}, O_{X_{rie}})$, 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"...):

$$J: D^*_{coh}(X, D) \to D^*(X_{cris.} \times_{cris}).$$

$$(17)$$

$$\underline{O}$$

The crucial question here is whether this functor is fully faithful. Only then is the notation

<u>*Cris*</u>^{*} $_{coh}(X)$ 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

$$\Gamma(X, D_X) \xrightarrow{\sim} Hom (J(D), J(D)), Ext^i \underbrace{O_X_s^{crie}}_{S}(X_{cris}; J(D), J(D)) = 0 \text{ for } i > 0.$$
(18)

(for affine *X*, resp. Stein). I haven't taken the time to check it⁷⁰⁷ (*), but have little doubt that it's true. I demonstrated something very similar, it seems to me, in [Crystals] (in 1966)⁷⁰⁸ (**).

⁷⁰⁷(*) 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....

⁷⁰⁸(**) This is the result I've already alluded to elsewhere, that for a complex of differential operators L' on a scheme

p. 1016

 \Box (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 topossical 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...).

$$J^{o}: D \xrightarrow{*}_{coh}(X, D)^{opp} \to D^{*}(X_{cris}, \operatorname{xcris}).$$

$$\underbrace{O}$$
(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^{c} , its image by (19) is obtained by looking at $P^{\infty}(L^{c})$ (the "formalization" of the complex L^{c}) 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 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

$$J^{o}(C) \to \underline{RHom}_{O \times cris} (J(C), \underline{O}_{\times cris}) \text{ (where } J \text{ is defined in (17))}$$
(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 \Box (

$$\underline{Ext}^{i}_{\underline{O}_{\mathsf{X}_{\mathsf{cries}}}}(J(D, \ \underline{O}_{X_{\mathsf{cris}}}) = 0 \text{ for } i > 0,$$
(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

$$\Gamma(X, D) ' Hom(P, P) , Ext^{i}_{\underline{O}_{\mathsf{Cris}}}(X_{cris}; P, P) = 0 \text{ for } i > 0 , \qquad (18.1)$$

where

$$P = P^{\infty} (O)_{X cris},$$

$$Hom(C, C) \rightarrow Hom(G^{\circ}(C), G^{\circ}(C))$$

is bijective, in the case where $C = \underline{O}_X$ (which is not bad at all, and allows every hope...).

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

which is a remarkable Crystalline Algebra on X. We assume here (for the nullity of Ext^{t} 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 interpreta tion 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 (X) and the Chow Ch group (X), or the Grothendieck ring (I apologize for the oddity...) K'(X) and the Grothendieck group $K_{i}(X)$ (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 Cris(X) of the crystals of Modules on X, provided with a structure

"by the tensor product, and the <u>*Cris*</u> (X) of module co-crystals, on which the former "operates" by a capproduct, 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"⁷⁰⁹ (*).

⁷⁰⁹(*) 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 \Box (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. ...

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 cohomolo- gical theme), but also in a technical sense.

Once we interpret constructible C-vector bundles on (smooth) X, either in terms of stratified procoherent Mo- dules (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 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 \underline{O}_X essentially) is **fully faithful** to the category $D_{coh}(X) = \underline{Cris}^*_{coh}(X)$, to the desired coefficient category \underline{Coeff}^* selected by the D_X

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. \Box (The *Coeff** of holonomic "coefficients" is in the <u>Cris</u> category image* _{coh}(X) by the _{p. 1020} (fully faithful) "Mebkhout-Grothendieck" functor. (NB. Morally, this functor is the of Mebkhout, but watched on <u>Cris</u>* _{coh}(X) in its entirety, and "revised and corrected by the ancestor", so that the goal is in <u>Coeff</u>* which has a purely algebraic meaning. ...).
- 2. Characterize the inverse image of $Coeff_{HOL}^*$ and $Coeff_{HOL reg}^*$ 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 *Coeff*^{*} back to the condition of regularity for a "local system" (e.g. fiber with integrable connection) in the vicinity of a divisor

crystals...

p. 1019

singular.

- 2. Microlocal" or "Japanese" aspect, expressed directly in terms of dif- ferential 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

<u>Coeff</u>_{hol rég}. If we can even find one for <u>Coeff</u>_{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 \Box (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 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 *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)⁷¹⁰ (*). 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

23)

Note 171(*x*)

 $[\]Box$ (May 5 and May ⁷¹¹(*) Mebkhout's philosophy, developed between 1972 and 1980, can be summarized as follows

⁷¹⁰(*) 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 $foresee^{712}$ (**).

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 of infinite-order differential operators on $X^{'15}$ (***), due to the fact that they \Box (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 <u>Ext</u> bundlesⁱ. This led Mebkhout, by analogy, to introduce an "algebraic part" of cohomology and a canonical arrow

$$\underline{\underline{H}}_{Y}^{i}(\underline{O}) \underset{x \ aLg}{\text{dfn}} = \lim_{\underline{n}} \underbrace{Ext^{i}(\underline{O} \quad \underline{O})}_{O_{X}} \xrightarrow{\underline{O}} \rightarrow \underline{\underline{H}}^{i}(\underline{O} \quad \underset{Y \quad X}{\text{dfn}} \underbrace{Ext^{i}(\underline{C}, \underline{O})}_{X}), \qquad (1)$$

where X_n denotes the nth 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 Q_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

$$D \dashrightarrow D^{\infty}$$

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

⁷¹²(**) 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

substitute for algebraically constructible C-vector coefficients, making sense for relative characteristic schemes

 $^{^{711}(*)}$ 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.

complexes of free Modules of type fi ni with bounded degrees), and dual to each other in the usual sense for perfect complexes. In the case where S = 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.

[&]quot;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 coeffi cients" (or De Rham coeffi cients). 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 coeffi cients".

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).

⁷¹³(***) 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^{∞} -Module (but perhaps not coherent, since we don't know if D^{∞} 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 to⁷¹⁴ (*), with strokes resolution of singularities.

 $_{p.\ 1024}$ \Box (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**⁷¹⁵ (*) (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[°] -*Modules* RHom_D \sim (C_Y, <u>O</u>) = C, by the extraordinary inversion formula:

$$C_Y = \underline{RHom}_{D^{\infty}}(C, \underline{O}_X).$$
⁽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^{∞} -Modules, and the double inversion (or "biduality") formula mentioned earlier⁷¹⁶ (**). This is also the wonderfully simple solution to the problem of the relationship between discrete (analytically constructible) coefficients and "continuous" coefficients.

 \Box (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

⁷¹⁴(*) 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).

⁷¹⁵(*) Mebkhout's original (transcendental) definition of regularity is recalled in the note "L'oeuvre. ... "(n° 171 (ii)), b. de p.(*) page 950.

⁷¹⁶(**) 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 theorem⁷¹⁷ (*). 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 dis- cret coefficients⁷¹⁸ (**), 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 thesis⁷¹⁹ (***)), 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 \Box lant to my contact²⁰ (*).

Mebkhout had great difficulty getting this theorem published, as it smacked of "grothendiecke- ries". (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 1982⁷²¹ (**).) I think that was his favorite theme, when he was giving lectures on D-Modules phi- losophy, 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 establishment⁷²² (***). That's a comforting thought. It

⁷²¹(**) 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,

 $[\]overline{717}(*)$ This statement is reiterated in the note "L'oeuvre. . . "(n° 171 (ii)).

⁷¹⁸(**) 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.

 $^{^{719}(***)}$ See the note "Rencontres d'outre-tombe" (n° 78).

⁷²⁰(*) 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 "coeffi cients" of all kinds, which also included a formalism of "Hodge coeffi cients". 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 coeffi cients". 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).)

represent a sketch of the philosophy brought by Mebkhout, at a time when he was the only one to be its depositary and advocate.

⁷²²(***) (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 suffi ciency, for whom "nothing is beautiful enough for her to deign to rejoice". I don't know if these dispositions are the rule throughout the scientifi c 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... ...

p. 1027

□(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 com- plex of differential operators", viewed as a complex of C-vector bundles with constructible cohomology), for the natural dualistic functor I had introduced on these⁷²³ (*). 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"⁷²⁴ (**). 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).....

p. 1028

 \Box (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^{∞} -Modules" aspect: the fact that the restriction m_{∞} (where "Mebkhout functor") of the "associated De Rham complex" functor to holonomic D^{∞} -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 δ_{∞} , given by

$$C \to \underline{RHom}_D(C, \underline{O})_X$$
 (3)

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 coeffi cients) also fi gures 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.

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. ...

⁷²³(*) 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.)

⁷²⁴(**) 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.

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

$$F \rightarrow \underline{RHom}_{C_X}(F, \underline{O})_X$$
 (4)

Following on from this, Mebkhout also proves a **reciprocal of** Kashiwara's constructibility theorem: if a complex of D^{∞} -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*-complexes^{∞}</sup> -Modules, where there is no question of regularity, so this implies that in the \Box (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 elsewhere⁷²⁵ (*), 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 key⁷²⁶ (**). 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^{∞} -theory already written, in order to demonstrate the theorem required in D-theory. It seems that the one and only demonstration published to date⁷²⁷ (***) 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 \Box (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.

⁷²⁵(*) See b. de p. (*) p. 952 at "L'oeuvre. . . " (n° 171 (ii))

⁷²⁶(**) As we saw earlier, there is one - and I'll come back to this point a little further down.

⁷²⁷(***) 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 varieties⁷²⁸ (*).

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 time⁷²⁹ (**). 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 \Box (and "break through", had only to follow the ready-made path of the "new style"⁷³⁰ (*), 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)⁷³¹ (**) 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 operation⁷² (***). To hope for a six-operation formalism, we must (therefore work with a category)

operation⁷⁵² (***). p. 1032

p. 1031

 $[\]frac{728}{728}$ (*) As I've already pointed out, in the algebraic context, when we wish to paraphrase discrete al-gebraically 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 infi ni.

⁷²⁹(**) 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"" (n° s 169₁ and 175). (See also p. 899, paragraph 3, in the note "Les double-sens - ou l'art de l'arnaque", n° 169₇.)

⁷³⁰(*) 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° 169₉.

⁷³¹(**) 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).

⁷³²(***) (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{O}_X) and by the notion of inverse image, and that the **countervariant** good-god functor δ commutes to it.

even larger than $\underline{\operatorname{Cris}}^*(X)$, 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 moduli⁷³³ (*). 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 case⁷³⁴ (**).

 \Box (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 much⁷³⁵ (***). 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.

 $Rf_* (\underline{RHom}(F, Rf^! (G))) \approx \underline{RHom}(Rf_! (F), G)$

⁽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^b (X) does not admit an "internal Hom" operation (playing the role of <u>*RHom*</u>), and for $f : X \to Y$, 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 $f: X \to Y$ of separate schemes of type fi ni on a

body K of zero (let's say) car., 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_1 .

⁷³³(*) This assertion should be rephrased in terms of a "dual theory with six operations", see b. de p. note. above.

⁷³⁴(**) 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) :

⁷³⁵(***) (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)⁷³⁶ (*) Mebkhout told me that until I mentioned it to him when we met two years ago⁷³⁷ (**), 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.

p. 1035

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, \Box (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^t (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 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 things⁷³⁸ (*), pages that I felt strongly about but for which I didn't know how much he was

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.

⁷³⁶(*) (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.

 $^{^{737}(**)}$ This meeting is mentioned in the note "Rencontres d'outre-tombe" (n° 78).

⁷³⁸(*) 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 \Box (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_1 (April 15)⁷³⁹ (*) Taking advantage of the recent visit to my home of my co-buried Zoghamn 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"⁷⁴⁰ (**) J.L. Verdier on three occasions. The

 ⁷³⁹(*) (May 30) The three notes that follow (n° s 171₁ to 171₃) 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.

⁷⁴⁰(**) (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 \Box (is today. As it happens 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"⁷⁴¹ (*), 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)⁷⁴² (**). 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 forma- lisms 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 \Box (tacit and rigorous on derived categories, which

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^{743} (*), 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

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...).

⁷⁴¹(*) 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° s 82, 83), and more briefly, in the note "Episodes of an escalation" (n° 169 (iii)), with episode 3.

⁷⁴²(**) 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 fi nitude 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.

⁷⁴³(*) 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 $f: X \to Y$, in the form (for example) of an adjunction statement between two functions $Rf_!$ 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"⁷⁴⁴ (**)) provides a "common denominator" to "cap" the \Box (phenomena (of duality, in particular) in discrete cohomology, and in cohomology coherent. With this momentum, encouraged by someone "in the know" and equipped with a modicum of mathematical instinct⁷⁴⁵ (*) 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 it⁷⁴⁶ (**) - 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 result⁷⁴⁷ (***) - no doubt that was the reason

 ⁷⁴⁴(**) 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° 46₄ (p. 188) (x). On the subject of "blocking healthy faculties" against links

See the note "La mystifi cation" (n° 85', p. 350-351).

⁽x) (May 24) See also note "The five photos (crystals and D -Modules)" (n° 171 (ix)).

⁷⁴⁵(*) 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. $^{746}(**)$ (June 5) On this subject, see the note "The ancestor" (n° 171 (i)), particularly the b. de p. note (*) on page 946.

⁷⁴⁷(***) (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.

platonic - 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,

 $_{p.1040}$ too much "grothendieckerie" of the \Box (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" $^{\prime 48}$ (*). . .), and on the "latest cry" (intersection cohomology), then \Box that the **vision**

p. 1041

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 subject⁷⁴⁹ (*), 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

quite the contrary."

⁷⁴⁸(*) 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 prestidigi- tateur" (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 maf- fi a", n° 171₂), 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.

⁷⁴⁹(*) 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° 47₃ 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 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 "to- pological", 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"⁷⁵⁰ (*), I'll call these objets "De Rham coefficients - \Box Mebkhout"⁷⁵¹ (*). If *X* is a smooth analytic space⁷⁵² (**), on the one hand there are ¹⁰⁴³

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 fi le in the edifi cation 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

the cohomological approach I was developing, valid in principle for all kinds of "coeffi cients" other than "*l-adic* coeffi cients". 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 coeffi cients" that went beyond the particular context of the seminar's main subject.

p. 1042

p.

⁷⁵⁰(*) This idea of various "types of coeffi cients", each of which presented itself to me as a particular incarnation of the forma- lism of the six operations (and biduality), more or less encircling the fi nest "type of coeffi cients" 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 suffi sance" 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.

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

See also the note "Dead pages" (n° 171 (xii)), and also "Useless details" (n° 171 (v)), part b) ("Machines for doing nothing. . . ").

 $\underline{Cons}^*(X)$ ("topological" aspect), that of complexes of with coherent cohomology bundles⁷⁵³ (***),

p. 1044

p. 1045

generalizing complexes of infinite-order differential operators, which I <u>denoteDRM</u>_{∞} (*X*) (transcendental **"analytic" aspect**), and finally the category of D^{∞} -complexes with co-herent cohomology bundles, generalizing complexes of ordinary (finite-order) differential operators, which I denote <u>DRM</u>^{*} (*X*) (**"algebraic" aspect**). There is a tautological extension functor for scalars of the coherent Ring D_X to Ring D^{∞}

 $i: \underline{\mathsf{DRM}}^*(X) \to \underline{\mathsf{DRM}}^*(X)$

inserted in a functor diagram (essentially commutative) :

$$DRM^{*}(X) \underbrace{i}_{DRM^{*}(X)} DRM^{*}(X)$$

$$(1)$$

$$\int_{a} \sum_{x,y} Z \qquad (1)$$

$$\underbrace{Cons^{*}(X)}_{Cons^{*}(X)}$$

where the oblique arrows are the "associated De Rham complex" arrows⁷⁵⁴ (*), which is none other than <u>*RHom*</u>_D (Sp_{*}, .)

where $D = D_X$ or D_X^{∞} , and where Sp_* is the "Spencer resolution" of \underline{O}_X by locally free D-Modules ⁷⁵⁴(*).

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 1975⁷⁵⁵ (**), 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 categories⁷⁵⁶ (***), 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 \Box dressing a table of a dozen typical examples (reproduced in his expository article with The Dung Trang⁷⁵⁷ (*)) of constructible C-vector bundles that can be called "elementary", which

 ⁷⁵¹(*) (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 coeffi cients" (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 coeffi cients" (or simply, "de Mebkhout"), "of infi ni order" and "of fi ni order" respectively.

⁷⁵²(**) (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 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*[∞] -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.

 ⁷⁵³(***) 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").

 $^{^{754}}$ (*) (May 24) See the note already quoted "The five photos. . . " (n° 171 (ix)), part (a).

⁷⁵⁵(**) Masaki Kashiwara, On the maximally overdetermined System of linear differential equations, I Publ. RIMS, Kyoto university 10 (1975), 563-579.

⁷⁵⁶(***) 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.

⁷⁵⁷(*) 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^{∞} ("**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_{∞} 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 quel ques jours, ou enp______. 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"⁷⁵⁸ (*), 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 equiva- lences 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_{∞} , in March 1978. He told me that psychologically, my comparison theorem for co

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.

⁷⁵⁸(*) (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 coeffi cients", which foresaw (and in a very clear way at least since 1966) a "theory of De Rham coeffi cients" (intimately linked to my crystalline ideas), having the same essential formal properties as the theory of *l-adic* coeffi cients, 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 coeffi cients 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)).

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

^{p. 1047} "generalization" of my comparison theorem. From this moment on, he also knows 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_{∞} , which was his favorite⁷⁵⁹ (*). 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 equivalence⁷⁶⁰ (**). 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.

* *

p. 1048 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

⁷⁶⁰(**) 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_{∞}) 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_{∞} and takes up his thesis), "Une équivalence de catégories" and "Une autre équivalence de catégories", in Compositio Mathematica 51 (1984), pp. 51-62.

an equivalence between the category of regular holonomic D_X -Modules, and that of holonomic D^{∞} -Modules. Incidentally, Mebkhout's fi nal result is considerably stronger, even when applied to **modules** (instead of

complex of modules), as it simultaneously displays the canonical arrows

$$Ext^{n}_{DX}(M, N) \rightarrow Ext^{n}_{D^{\infty}_{Y}}(M_{\infty}, N)_{\infty}$$

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."

⁷⁵⁹(*) (May 24) Another, perhaps stronger, reason is that in the case of the D^{∞} -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".

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_{∞}) 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_{∞}), 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

(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 note⁷⁶¹ (*), 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, \Box of listening in relation to a p. 1049

certain **spirit** in his writings⁷⁶² (*). 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 171₂ (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

⁷⁶²(*) One may wonder (or ask me) what is this famous "spirit" so particular to my writings, which would have inspired my

⁷⁶¹(*) See note ". . and the bargain" (n° 171 (iii)).

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 fi liation 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 $simply \Box from SGA$'s first paper.

5 (the same one referred to in a strangely-named volume "SGA 4^{1} " by : "various supplements are given in SGA 5 I"⁷⁶³ (*)!). 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 themes⁷⁶⁴ (**) 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 fibers⁷⁶⁵ (***). 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 $\lim_{n \to \infty} \frac{1}{2} (****)$. 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...).

p. 1050

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 sparingly⁷⁶⁷ (*), 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 seminar⁷⁶⁸ (**). We may be of the

⁷⁶⁶(****) (May 30) And all the while kindly calling him a smoker to boot. ...

 $^{^{763}}$ (*) 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).₃

⁷⁶⁴(**) These are the "three themes": constructibility, biduality for constructible bundles, cohomology (and homology) class. associated with a cycle.

⁷⁶⁵(***) see for details the beginning of the note "L'éviction" (n° 63), and the b. de p. note (**) on page 233 of this note.

⁷⁶⁷(*) 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." "

 $^{^{768}(**)}$ 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. ... $^{769}(***)$ I think it was easier for Zoghman to p . 1052

saying that a Japanese he'd never seen⁷⁷⁰ (*) 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 mathematics⁷⁷¹ (**). Life was good for Zoghman that day - but not for long.

 \Box The same year, in September 1979, he took part in the Colloque des Houches⁷⁷² (*), where he gave a talk p. 1053

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

⁷⁷⁰(*) (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 mystification of the Colloque Pervers, in which Kashiwara was used as a convenient "pawn" to prepare the ground - and then be dropped....

- ⁷⁷¹(**) (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° 174₄), page 1091 (and in particular the b. de p. note (**) on the same page).
- ⁷⁷²(*) 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."

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. . .).

⁷⁶⁹(***) (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

from Mebkhout's philosophy of duality, directly inspired by my work

"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 school⁷⁷³ (**)), this theorem asserted that on a complex (smooth) analytic variety, the "change of scalars" functor from *D* to D^{∞} 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 since⁷⁷⁴ (***).

Mebkhout, like all the other listeners, was a bit overwhelmed. This theorem, presented as

sensational and where nobody quite understood what it was all about, yet had a familiar "je ne sais 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 sin- gularities" were. But Mebkhout, 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-Module⁷⁷⁵ (*). 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^{∞} -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.

p. 1055

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 conference⁷⁷⁶ (**) with arrows and diagrams (the kind of 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

806

⁷⁷³(**) (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 Pacifi c, into a method of systematic mystifi cation and appropriation to befuddlement.

⁷⁷⁴(***) M. Kashiwara, T. Kawai, On holonomic Systems of microdifferential equations III, System with regular singularities, Pub. RIMS 15, 813-979 (1981).

⁷⁷⁵(*) For Mebkhout'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).

⁷⁷⁶(**) (June 4) In fact, Mebkhout 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 dreaming⁷⁷⁷ (*). . . 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^{778} (**), he announces **as his own** this 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)⁷⁷⁹ (*).

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... ...

 $[\]overline{777}(*)$ (June 4) Mebkhout writes to me along these lines (April 22):

[&]quot;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."

⁷⁷⁸(**) (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° 171₄.

⁷⁷⁹(*) 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 whole⁷⁸⁰ (**) world would be wrong to shy away,

while the aforementioned 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 all⁷⁸¹ (*). 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 \Box 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 Singulari- tés" 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").

p. 1058

⁷⁸⁰(**) (June 4) For a "défi lé" of the actors who participated directly and actively in the mystifi cation-scam surrounding Zoghman Mebkhout's work (or at least, those of whom I was aware), see the note "La maffi a" (n° 171), part (f) "Le défi lé des acteurs - ou la maffi a". 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 maffi a", part (c1) "Les mémoires défaillantes - ou la Nouvelle Histoire") - and fourteen! (Not counting an anonymous referee - and fifteen...)

⁷⁸¹(*) (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 Inventions 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 achievements⁷⁸² (*).

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.

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 Mebkhout⁷⁸³ (*). 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 holono-mic Systems and constructible sheaves.**"

(emphasis added). There is not the slightest reference or explanation about this famous "correspondence" 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 necessary 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-**

p. 1059

⁷⁸²(*) 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"...

⁷⁸³(*) 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 Bry- linski. It would already be scandalous, on a mere **presumption**⁷⁸⁴ (*) of anteriority of □ results obtained independently-Such practices obviously open the door (and have long opened the door . . .) to the most serious abuses⁷⁸⁵

(*). But there's more. The "theorem

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 maffi a". 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 conductive fi le, 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 Kashi- wara.

⁷⁸⁵(*) 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 Inven- tiones, he is directly responsible for this swindle, regardless of the fact (of which he could not have been aware) that he was involved.

⁷⁸⁴(*) (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.

de reconstruction" he cites (and which \Box is also plundered in Mebkhout's thesis⁷⁸⁶ (*), where it appears under thep

(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 implies⁷⁸⁷ (**). As far as I'm concerned, the referee's bad faith in relying on the conni-

vence 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 contributes⁷⁸⁸ (***). 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 1981⁷⁸⁹ (****), 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 solitude⁷⁹⁰ (*).

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 angle⁷⁹¹ (**), 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 give⁷⁹² (***). After my departure, this work became the target and prey \Box de the covetousness of those

p. 1064

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 maffi a"), entitled "Les mémoires défaillantes - ou la Nouvelle Histoire".

⁷⁸⁶(*) 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.

⁷⁸⁷(**) 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).

⁷⁸⁸(***) (May 30) And to which R. Remmert, as publisher of Les Inventions, gives his unreserved support. ...

⁷⁸⁹(****) (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 "coeffi cients", in cohomology of varieties and spaces of all kinds.

⁷⁹⁰(*) 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)).

⁷⁹¹(**) (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",

⁷⁹²(***) 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 (note⁷⁹³ (*) 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 Archimedian Analysis" by

S. Bosch, U. Guntzer and R. Remmert⁷⁹⁴ (**). 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 \Box notions of "varieties" (analytic, in this case).

p. 1065

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.

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 Renouvellement (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".

 $^{^{793}(\}ast)$ See the note "Les félicitations - ou le nouveau style", n° 169 $_9$.

⁷⁹⁴(**) 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 theory⁷⁹⁵ (*), 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 cohomo-logy, 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 $R f^{i}_{*}$, for a mor-

phism of rigid-analytic spaces - problems solved by Kiehl in the years that followed⁷⁹⁶

⁷⁹⁷. But it is true that following \Box the wind that blows these days, it is considered a thing of no importance, p. 1067

⁷⁹⁵(*) 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...

⁷⁹⁶(**) 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 fi ni diagrams on a body exhaust the intuition associated with the word "diagram". A leading fi ltowards 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 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.

⁷⁹⁷(***) (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 **fi nite** 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 ("impos- sible") 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" va- rities (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....

p. 1068

 \Box 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 fifties⁷⁹⁸ (*), 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 problem⁷⁹⁹ (*), 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^{800} (**).

⁷⁹⁸(*) There's an obvious misunderstanding here about what I meant in the first part of Récoltes et Semailles, "Fatuité et Renouvel- lement". 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.

 $^{^{799}(*)}$ (May 25) It's possible that Deligne had long since lost the sense for this "crucial" character. See note ". . et entrave" (n° 171 (viii)').

 ⁸⁰⁰(**) 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

Brylinski-Kashiwara on the one hand, Beilinson-Bernstein on the other, proving the famous conjecture using were received, 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 once⁸⁰¹ (*).

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

⁸⁰¹(*) 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 indeed⁸⁰² (*), 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] ... "⁸⁰³ (**)' "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^{804} (*), 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... ...

(May 25) On the subject of this "new style", see the note "Les félicitations - ou le nouveau style" (n° 169).9

. 1073

⁸⁰²(*) (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.

⁸⁰³(**) 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 4^1 - SGA 5" operation, which consisted in recalling (in passing) "the construction exposed" 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...".

⁸⁰⁴(*) 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-tested⁸⁰⁵ (*) 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 scenes⁸⁰⁶ (**). 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'Enterrement⁸⁰⁷ (***), and which is strikingly prefigured in

p. 1075

p. 1074

the 1968 article by my most brilliantly gifted student, the same Pierre Deligne⁸⁰⁸ (****). And this circumstance also suffices to remind in y 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 ridiculous⁸⁰⁹ (*). 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.

 $^{808}(****)$ See the beginning of the note "Eviction" (n° 63).

⁸⁰⁵(*) For other examples of this "dilution by assimilation" method, see the sub-note "Les vraies maths. . . "n° 169₅), p. b. note (*) page 885.

⁸⁰⁶(**) (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.

⁸⁰⁷(***) On the subject of this style, see the end of the note already quoted "Les félicitations - ou le nouveau style", n° 1699.

⁸⁰⁹(*) 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").

□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... . "⁸¹⁰ (*). 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 ar- ticle, 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 deal⁸¹¹ (**), tacit if not expressly Beilinson etp

Bernstein's Kazhdan-Lusztig conjecture and (for good measure, given that there was already Brylinski-Kashiwara

⁸¹¹(**) 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 4¹ - 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

 $^{^{810}(*)}$ On this subject, see the aforementioned note "Le prestidigitateur" (n° 75).

Formule", n° s 169₅ - 169₉.) 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 no.° 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"⁸¹³ (**). 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 ago⁸¹⁴ (***), 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 \Box , it was **M. Kashiwara, R. Hotta**⁸¹⁵ (*), **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**⁸¹⁶ (**), 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 sem \Box blerait 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,⁸¹⁷ (*) **A. Beilinson** and **J. Bernstein** (whose work I've just done in this very room).

820

⁸¹²(*) See "Perversity", n° 76,

⁸¹³(**) 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.

 $^{^{814}(***)}$ See note of the same name dated March 22, n° 173.

⁸¹⁵(*) An attentive reader may be surprised not to find in this "actors' challenge" (in the swindle-mystifi cation 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.

⁸¹⁶(**) 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 fi lm about the reign".

throughout the present peregrination through the misadventures of the vague stranger on duty....

⁸¹⁷(*) (May 25) This "enfi n" turned out to be premature - other gang members have since come to my attention. See

pa ni ed m e ag ai n, st ep b y st ep 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 171₃ (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 \Box 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 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).

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 défaillantes - 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 them⁸¹⁸ (*).

□ I can afford, in short, to be "alone against all" - say what I have to say, and go my own way.

p. 1081

(May 25)⁸¹⁹ (*) It doesn't take much imagination to understand the frustration of Mebkhout, who suddenly feels "swept away"⁸²⁰ (**) 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

 \Box 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 Colloquium⁸²¹ (*). 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.

⁸¹⁹(*) The following two pages are taken from what was originally intended as a b. de p. note to the note "... et l'aubaine" (n°

"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.

⁸²¹(*) 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

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.

⁸¹⁸(*) 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).

^{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.

⁸²⁰(**) 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:

⁻ 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.

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 **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 information⁸²² (*). 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 $171_4 \square$ (June 2) It's been two months since I had the satisfaction of putting the "final touch" under p. 1084 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 I' Enterrement! It's the reissue, more or less of what happened around this time last year - when I was still

⁸²²(*) (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 exhaustion⁸²³ (*). 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, "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.

⁸²³(*) 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. $II\square$ 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. ... $^{824}(*)$

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 \Box . It was **because of ''his'' people**: those of the "little family"⁸²⁵ (*) - 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... .).

 $^{^{824}}$ (*) This is the image already featured in the note "L'enfant et la mer - ou foi et doute" (n° 103).

⁸²⁵(*) (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 "Eclosion 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).

 \Box 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.

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 Houches⁸²⁶ (*)! 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 question⁸²⁷ (**). 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 sudden⁸²⁸ (***)! 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 \Box 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.

⁸²⁶(*) On the Colloque des Houches and the Goulaouic-Schwartz seminar episode, see the note "La maffi a" (n° 171), section (b) "Premiers ennuis - ou les caïds d'outre-Pacifi que".

⁸²⁷(**) 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.

⁸²⁸(***) 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 Gaulaouic-Schwartz⁸²⁹ (*), or among all those prestigious people who "do" cohomology of algebraic varieties.

 \Box 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.

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

⁸²⁹(*) 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!

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

p. 1092

Colloque d'Analyse p-adique in Rennes, in July 1978. He then explained \Box "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's⁸³⁰ (*). 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 this⁸³¹ (**). 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^{832} (***). 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... ...

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 \Box 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 \Box (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

⁸³⁰(*) This theorem is discussed in the two notes "L'oeuvre. . . " and "Three milestones - or innocence" (n° 171 (ii), (x)).

⁸³¹(**) 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 mystifi cation" (n° 85'), in particular my comments on Illusie, page 351.

⁸³²(***) On the Colloque des Houches and the Kashiwara scam at the Goulaouic-Schwartz seminar, see the note "La maffi a" (n° 171), part (b), "Premiers ennuis - ou les caïds d'outre-pacifi que".

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 years⁸³³ (*). 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 indepth 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

 \Box 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 moeurs 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.

⁸³³(*) I express myself along these lines in note no.^o 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.

UNIVERSITE DES SCIENCES ET TECHNIQUES DU LANGUEDOC

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 ¹'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 ⁸³⁴(*)). 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

⁸³⁴(*) 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.

UNIVERSITY OF SCIENCE & TECHNOLOGY

DU LANGUEDOC

Thursday, May 30, 1985

p. 1097

MATHEMATICS

□Madame J. CHARLES "responsible for the premises at the Institut de Mathématiques". to Mr A. GROTHENDIECK, Professor of Mathematics.

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.

LANGUEDOC UNIVERSITY OF SCIENCE AND

TECHNOLOGY

Institute of Mathematics

□ MATHEMATICS INSTITUTE

p. 1099

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 HU-BERT COULIN "M. LEFRANC, M. LOUPIAS, Mme MEDEN, M. MOLINO, MmePIERROT, M. PIN-CHARD, M. SAINT PIERRE, MIe 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

p. 1100 UNIVERSITE DES SCIENCES ET TECHNIQUES DU LANGUEDOC

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 \square 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.

p. 1101

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 \Box (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 point⁸³⁵ (*).

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.

p. 1103

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..... $^{836}(*)$), 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 $4^{\frac{1}{2}}$ "). 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 others⁸³⁷ (**). . . At times it seemed as if I was never going to finish - and then no, it ended up converging⁸³⁸ (***). 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.

⁸³⁵(*) 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).

 $^{^{836}(*)}$ See note of the same name, n° 77'.

 $^{^{837}(**)}$ (June 1) Which have since become six. . .

⁸³⁸(***) (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 \Box "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 called⁸³⁹ (*) "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 1970⁸⁴⁰ (**). 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 \Box ter p.1105 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 ideas⁸⁴¹ (*) that he or she has taken from

⁸³⁹(*) In the note "Le Fossoyeur - ou la Congrégation toute entière", n° 97.

⁸⁴⁰(**) It is true, however, that even some of the ideas and techniques that had already come into "everyday" use (at the very

At 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.

⁸⁴¹(*) 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",

p. 1106

 \Box 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 respect⁸⁴² (*), 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 \Box 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

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° 106₁ of the note "Muscle and gut (yang buries yin (1))", n° 106), the "value" of a conjectural statement depends neither on its presumed diffi culty, 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 scientifi c ethics. This is why an honest scientifi c 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 qualifi catives of the same water).

⁸⁴²(*) 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 pro- bllemly difficult problems. In a way, I knew this all along - but I underestimated the "ethical" aspect, or **collective**⁸⁴³ (*), 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...).

18.5.6. (5) The family album

Note 173^{844} (*)

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

⁸⁴³(*) 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).

⁸⁴⁴(*) 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-deceased⁸⁴⁵ (**) 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. 1110

□ terms from a certain advertising placard⁸⁴⁶ (*)), 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 twelve⁸⁴⁷ (**). 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^{1} ", 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^{1} " (as a current reference acronym), and also "Cohomology".

Etale" - by P. Deligne, with the "collaboration" (among others and in addition to L. Illusie and J.L. Verdier) of A. Grothendieck⁸⁴⁸ (***). The second text is the Proceedings of the Colloque de Luminy of June 1981, and

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

⁸⁴⁵(**) 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.

⁸⁴⁶(*) 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.

⁸⁴⁷(**) 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!

⁸⁴⁸(***) 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 opprobrize 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 fiveor 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 $p_{\perp 1111}$ 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 GmbV side, there are **K.F. Springer** (one of the company's copublishers), **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), 171₂, 171).4

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 " $^{cavalier \square servant}$ "), were p. 1112 reconstructed page by page in the suite of notes "Le sixième clou (au cercueil)" (n° s 176₁ à 176₇), 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 nineteen⁸⁴⁹ (*) 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),

p. 1113

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 \Box 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"⁸⁵⁰ (*): 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 "vin" or "feminine" side, the result was an astonishing "Verflachung", a "flattening", a "drying out" of the mathematical work⁸⁵¹ (**). 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.

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). ⁸⁵¹(**) 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)).

⁸⁴⁹(*) Twenty, including the famous anonymous referrer.

⁸⁵⁰(*) 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

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 m e n t i o n e d, 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 \Box 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 mv 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 them⁸⁵² (*), and for more than one I can even add that their complete good faith and honesty are for me above suspicion. \Box 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 students⁸⁵³ (*). 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

⁸⁵²(*) (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 Colloque, 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.

⁸⁵³(*) 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 $my \Box$ 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 $list^{854}$ (*) 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 \Box this one! I

P. 1117 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

⁸⁵⁴(*) 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 name⁸⁵⁵ (*). 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 \Box 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 me⁸⁵⁶ (*), 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 temperaments⁸⁵⁷ (**) - 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 \Box 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

⁸⁵⁵(*) 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)).

⁸⁵⁶(*) Between 1965 and 1969, while the relationship between Serre and me remained close, it was Deligne who took on the role. 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.

⁸⁵⁷(**) 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 1970⁸⁵⁸ (*). 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 antagonism⁸⁵⁹ (**), on either side. There were frictions,

p. 1120

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"⁸⁶⁰ (*), published in 1980, so **after** the incredible "SGA 4 operation¹ - SGA 5", I was

⁸⁵⁸(*) 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).

⁸⁵⁹(**) 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 tomy 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": fi ction 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 of 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!

⁸⁶⁰(*) 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, \Box with the help of Mr. Artin⁸⁶¹ (*). It is visible in more ways than one, that Milne has only occasionally read in SGA 4 and SGA 5^{862} (**), 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^{1} ") to present, in his introduction, Les textes originaux SGA 4 et SGA 5^{863} (***) 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....

□ I think it was last year that I first took a quick look at this presentation

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

p. 1122

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.

⁸⁶¹(*) 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.

⁸⁶²(**) 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 1698, 169).9

⁸⁶³(***) As far as the published version of SGA 5 is concerned, which (thanks to the "care" of the publisher-sic Illusie) only represents a a defi ned ruin of the original seminar, Milne apologizes for finding it "diffi cult to access", Le bon samaritain 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 though⁸⁶⁴ (*), when I came back to this text by Serre from 1974, against the backdrop of a yearlong 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, \Box à la lu-

l'Enterrement⁸⁶⁵ (*). 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**.

p. 1124

⁸⁶⁴(*) In fact, it was only the third time I had this text in my hands that it "clicked".

⁸⁶⁵(*) 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 students⁸⁶⁶ (*), 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 speak⁸⁶⁷ (**) - the burial of a had been linked from its very origins, and more closely than anyone else in the world. And it's 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-⁸⁶⁸ (*). This eliminates from the deeper motivations the usual "parasitic" components of antagonism "by compensation"⁸⁶⁹ (**). As I pointed out earlier, I can only detect I a relationp

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 fi xed 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".

⁸⁶⁸(*) 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.

⁸⁶⁹(**) I've already alluded two or three times, here and there, to this (apparent) "causeless antagonism", notably in the note

. 1126

⁸⁶⁶(*) 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.

⁸⁶⁷(**) 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 "fi xed point formula" (which I had admitted there) had still not been demonstrated in the literature.

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 funeral⁸⁷⁰ (*).

p. 1127

 \Box 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.

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.

⁸⁷⁰(*) 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 fiendishhindrance to going round in circles in the hushed purr of a beautiful

April 3 (below) "Le messager (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.

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**⁸⁷¹ (*). 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 egotic 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' \square without interfering with my love affair with lady mathematics, or (more generally) with my p. 1129

spontaneous adventure of discovery, whether mathematical or otherwise⁸⁷² (*).

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.

funeral ceremony ...

⁸⁷¹(*) 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. zinc. 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".

⁸⁷²(*) I discuss the role of this repressive force in my own life in the note "Le Superpère (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 \Box 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"⁸⁷³ (*).

p. 1131

d. L'Entrerrement - or the natural slope

 \Box (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

⁸⁷³(*) 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 heaviness⁸⁷⁴ (*), 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 recently⁸⁷⁵ (**). 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 \Box - 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 C by a first factor Z, to find 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 construction⁸⁷⁶.

⁸⁷⁴(*) 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.

⁸⁷⁵(**) see note "La maffi a" (n° 171), part (c), "Les mémoires défaillantes - ou la Nouvelle Histoire".

⁸⁷⁶(*) (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 fi lm 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,

a "good notion" of analytic variety, smarter than the one we knew and close (by "connection" type varieties. properties) to real or complex, or even, algebraic analytic But then again,

p. 1133

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. ...

At the end of the job, I knew for sure: modulo additional technical work, which I wasn't

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 desiderate 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 notes⁸⁷⁷ (*).

□ 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).

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...

⁸⁷⁷(*) 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.

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_1) "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

Funeral⁸⁷⁸ (*), 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

p. 1137

p. 1136

everything he fallait! I could hardly believe my ears at the time⁸⁷⁹ (*) - 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, "ridige-analytical spaces", was me.

⁸⁷⁸(*) 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

[&]quot;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...

⁸⁷⁹(*) 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 fi ber 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 \Box into the world and here he is frolicking and in good shape, someone

p. 1138

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 Remmert⁸⁸⁰ (*)). - and that in 1972, when I wrote the Esquisse Thématique in which I alluded to it⁸⁸¹ (**), 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.

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!

"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

That's Grothendieck all over again!

⁸⁸⁰(*) 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......

⁸⁸¹(**) 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:

[&]quot;**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 fi bres 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 car. p > 0, and cohomology of local systems on rigid-analytic varieties in zero characteristic."

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 **indecency** - 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 \Box peu pressured man, taken to task by a lump who stubbornly refuses to

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)⁸⁸² (*), 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, Saavedra⁸⁸³ (**)

- 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 once⁸⁸⁴ (***). "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

⁸⁸²(*) This is the group of notes (n° s 176₁ à 176₇) 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

[&]quot;tannakiennes" for the occasion) - scam set up by a Deligne and (initially) through the "pawn" Saavedra interposed. ...

⁸⁸³(**) This is the famous book "Tannakian Categories" (sic) by the same Neantro Saavedra Rivano, published in Lecture Notes 265 (1972), Springer Verlag.

⁸⁸⁴(***) 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.

 \Box 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 time⁸⁸⁵ (*). 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

⁸⁸⁵(*) 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

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 me⁸⁸⁶ (*), 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's⁸⁸⁷ (**). This is all in line with what I wrote three weeks \Box 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 corruption⁸⁸⁸ (*).

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 \Box temps to empty. And then there was the usual procrastination. That

p. 1144 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.

⁸⁸⁶(*) 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 Renouvellement", 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).

⁸⁸⁷(**) 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......

⁸⁸⁸(*) 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° 171₄, 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 Renouvellement, 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.

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 firstrate "revealer" that is Burial. It was this contradiction

p. 1146 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 ac- quiescence") 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 Renouvellement, with the section "La mathématique sportive" followed by "Fini 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 ^{en□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.

p. 1147

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 \Box (March 22)⁸⁸⁹ (*) 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 vin and vang⁸⁹⁰ (**). 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^{1} " and the - \Box Illusie edition of SGA 5^{891} (*). 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 com- pliant eye of the "whole Congregation"), it seems that there's been a lull for four years. Whereas

⁸⁸⁹(*) (June 14) This note follows on from part a. ("A deceased well surrounded") of the previous note, written on the same day.

⁸⁹⁰(**) This is the reflection forming the major part of the third part of Récoltes et Semailles, with notes n° s 104 to 162".

⁸⁹¹(*) (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 176₁ - 176₇.

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. ... ⁸⁹²(**). It was also the year in which Deligne learned of the "theorem of the good Lord" (alias Mebkhout)⁸⁹³ (***), 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

p. 1150

of the "history"-sic-of crystalline cohomology), appear to me afterwards \Box 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 earlier⁸⁹⁴ (*)) 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^{1} - 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 GmbV (Heidelberg), a well-known publisher of books and scientific periodicals, ho-

⁸⁹²(**) 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 offi cial co-paternity... ...

⁸⁹³(***) (June 1) In fact, this episode took place the previous year, in June 1979, at the Bourbaki seminar.

⁸⁹⁴(*) In the "Jewels" note, n° 170 (iii).

norant d'ailleurs de la devise "Im Dienste der wissenschaft" - au service de la science⁸⁹⁵ (**). 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 \Box 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 "Grundleh- ren" series (der Mathematik und ihrer Grenzgebiete), where three books (including the reprint of EGA I) were published by myself⁸⁹⁶ (*).

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"⁸⁹⁷ (**); and perhaps even more than that, that the spirit I felt \Box 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 question⁸⁹⁸ (*). 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.

For details of the "Tannakian categories" operation, see the "Sixth nail (in the coffin)" suite of notes, n° s 176₁ - 176₇.

⁸⁹⁵(**) (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".

⁸⁹⁶(*) The other two books are Jean Giraud's and Monique Hakim's theses (on the formalism of fields and non-commutative 1cohomology, and on relative schemes on general ring topos).

⁸⁹⁷(**) 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 Birkhâuser 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).

⁸⁹⁸(*) (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

on monodromy groups, 1967/69; the volume entitled "SGA 4^{1} " (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 series⁸⁹⁹ (**). The first four volumes were published when Dr. K. Peters was in charge of Lecture Notes⁹⁰⁰ (***), 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 4¹ ", thus presenting themselves under the acronym "SGA", which I believe is not

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 ima- gine) 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^{1} ", 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^{1} " 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 \Box 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

⁸⁹⁹(**) These are volumes n° s 288, 340, 569, 589, 900.

^{900(***)} 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° 169₁) 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 much⁹⁰¹ (*) - 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 \Box ("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.

⁹⁰¹(*) In this famous "Ariadne's thread" (through SGA 4 etc.) in the volume entitled "SGA 4¹/₂", 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 4¹"...). On this subject, see my comments in the note "Les double-sens - ou l'art de l'arnaque" (n° 169₇), 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 he runs, and in any case not their way of conceiving their profession, mobilizing lawyers and by making them mobilize their own^{902} (*). 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**⁹⁰³ (**). 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

p. 1157

feel

p. 1156

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.

⁹⁰²(*) 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...).

⁹⁰³(**) 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_{1}^{904} (*) (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° 168₁). 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^{905} (**). 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 \Box 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 1984⁹⁰⁶ (*). 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 trace⁹⁰⁷ (**) - 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... ...

⁹⁰⁴(*) (June 16) The following group of notes (n° s 176₁ to 176₇), 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° 176₇), 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 "fi nal 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"....

⁹⁰⁵(**) 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).

 $^{^{906}(*)}$ On this subject, see the note "Souvenir d'un rêve - ou la naissance des motifs", n° 51.

⁹⁰⁷(**) 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

p. 1159 Note 176_2 \Box 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 week⁹⁰⁸ (*). 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 of⁹⁰⁹ (**). The burial was already well underway, since n o n e 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)⁹¹⁰ (***).

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 is⁹¹¹ (*)) 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 assi- milation 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 me⁹¹² (**). Of course, the subject of the thesis itself

⁹⁰⁸(*) (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.

⁹⁰⁹(**) 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° 176₇), of which I will say no more here. . .

 $^{^{910}(***)}$ For a correction, see the note quoted in the previous b. de p. note.

⁹¹¹(*) 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.

⁹¹²(**) 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"⁹¹³ (***), 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 = 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 \Box complete demonstration (from my first thoughts on the motivic Galois group, on p. 1161 1964/65)⁹¹⁴ (*). 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 Z (because of the applications to pattern theory).

Of all the students I had before I left, Saavedra, the latest to arrive⁹¹⁵ (**), 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 R and a list of such categories that are "polarizable", applications to pattern categories and numerous variants) required somewhat "all-round" knowledge⁹¹⁶ (***), 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 me realize that Saavedra has done a really impressive job and in p. 1162

in record time⁹¹⁷ (*). 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...

(See the note following "He who knows how to wait... ", n° 176₃.)

⁹¹⁴(*) I didn't want to take the time to check this in my notes on the motivic Galois group (or rather, on what

⁹¹⁷(*) For more information on this "record", and its (obvious) explanation, see the note "Monsieur Verdoux

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...

⁹¹³(***) 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.

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".

⁹¹⁵(**) 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.

⁹¹⁶(***) Above all, it required a thorough knowledge of the structure theory of reductive algebraic groups, of their classifi - cation 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.

⁻ or the cavalier servant" (n° 176).5

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 cri- teria 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 "doc- teur" 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 introduction⁹¹⁸ (**), 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 \Box references in form are necessary, and none are available that are not from my pen.

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 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 one⁹¹⁹ (*)). But my name doesn't appear either where he introduces and develops these notions (in Chapter VI devoted to

⁹¹⁸(**) 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 fi ltrations on fi bres 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° 176₃), and especially in the note already quoted "Monsieur Verdoux - ou le cavalier servante (n° 176₅) and the one that follows it "Les basses besognes" (n° 176).6

⁹¹⁹(*) 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 \Box the nearest orphanage, I've well recognized a **style** that I know all too well, from the year I've been touring l'Enterrement. ...

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"⁹²⁰ (*). 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... . "⁹²¹ (**). 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 $176_3 \square$ 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 episodes⁹²² (*) 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!

⁹²⁰(*) (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). (*) 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). (*) 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). It is beilinson, whom Mebkhout found "more honest" than Bernstein (see page 1072). It is below the beilinson of the set of Beilinson, whom Mebkhout found "more honest" than Bernstein (see page 1072). It is below the beilinson of Beilinson, whom Mebkhout found "more honest" than Bernstein (see page 1072). It is below the beilinson of Beilinson, whom Mebkhout found "more honest" than Bernstein (see page 1072). It is below the below t

⁹²¹(**) 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....

⁹²²(*) 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 thanks⁹²³ (**). 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 himself⁹²⁴ (***).

If the name "SGA 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 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 scheme⁹²⁵ (*), 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 equivalence⁹²⁶ (**).

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 \Box non-commutative cohomological algebra in dimension ≤ 2 in the sixties, blows

⁹²³(**) On the "mathematical" side of things, these people are (in order of appearance) myself (out of alphabetical order, that was nice), Berthelot and Deligne.

⁹²⁴(***) 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".

⁹²⁵(*) "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, $Gal(K^-/K)$.

⁹²⁶(**) 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 links⁹²⁷ (*), there was no shortage of sniggers. It's the kind of thing that Deligne and co. have been calling a "gangue of nonsense" for a long time now. These sniggers didn't bother me⁹²⁸ (**), I knew where I was going - and it was with "rapture" (as I write elsewhere) but no real surprise, that I saw this "gangue" 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 "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. . .)⁹²⁹ (***) - 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 that 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 him⁹³⁰ (*), 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 me⁹³¹ (**).

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

⁹²⁷(*) 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... .).

⁹²⁹(***) 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 171₂ (e), the latter being part of the long note "La maffi a" n° 171).₂

⁹³⁰(*) 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).5

⁹³¹(**) 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° 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

p. 1169

leaves no room for doubt on this \Box subject⁹³² (*). 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 one⁹³³ (**)) 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

p. 1170

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 \Box 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".

⁹³²(*) 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).

 ⁹³³(**) 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 176₄ (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 thesis⁹³⁴ (*) 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 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 say⁹³⁵ (*)) to thanks for their "help and advice which they have generously given during this work"...

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 Id 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 \Box dentially and by the greatest of chance, or whether the future true p.

father gently made him understand that this result of a stranger, who was hanging around without a father worthy of the name

 $^{^{934}}$ (*) See the notes cited in the penultimate b. de p. note.

⁹³⁵(*) 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 at⁹³⁶ (*). 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^{937} (**), 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 - w h at 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 $else^{938}$ (*), 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 IV, 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.

⁹³⁶(*) (June 16) It would appear that the initiative for the pick-pocket operations on Mebkhout's work did indeed fall to the entrepris Kashiwara, and this as early as 1978, just a few months after Mebkhout had sent him Chapter III of his thesis, which he had just completed. See "La maffi a" part (b) ("Premiers ennuis - ou les caïds d'outre-Pacifi que"), b. de p. (*) p. 1060.

⁹³⁷(**) (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-Schwatz 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).

⁹³⁸(*) (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 operation⁹³⁹ (**) was the appropriation of a certain **fixed-point formula**, of the fact t h a t_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 \exists question was tainted by the **same p** . 1174 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 cru- cial), 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 conjecture⁹⁴⁰ (*). 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... . ⁹⁴¹(**)), 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 ultrageneral 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 the défunt (that of ") 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 find⁹⁴² (*). 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.

⁹⁴¹(**) These two formulas are in fact each an immediate corollary of the other. As my authorship of one of them (the fonctions *L*) was notorious, Deligne managed (in the memorable text entitled "SGA 4¹") 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. infi nitely 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).₅₉
⁹⁴²(*) See on this subject the note "Les prestidigitateurs - ou la formule envolée" (n° 169₈) - and also the b. de p. note (**) page

1121 to the note "The family album, showing the extent to which the escamotage-envolage efforts of good Samaritans Deligne and

Illusie were a great success.

⁹³⁹(**) On this subject, see the group of notes entitled "La formule" (n° s 169 -169₅₉). This initial aim was later considerably broadened - see in particular the notes "L'Eloge Funèbre (1) - ou les compliments" (n° 104) and the note "Les joyaux" (n° 170 (iii)).

 $^{^{940}}$ (*) This is, of course, Weil's conjecture. On this subject, see the note "La Conjecture" (n° 169).4
18.5.9.5. e. Monsieur Verdoux - or the cavalier servant

Note 176₅ 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 nil⁹⁴³ (**). After that, I heard nothing from him, as far as I can remember⁹⁴⁴ (***). I

p. 1176

p. 1177

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 mean⁹⁴⁵ (*)), 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)⁹⁴⁶ (**) - 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^{947} (***) by the time I met Saavedra) where base ring k = 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

⁹⁴³(**) 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.

⁹⁴⁴(***) My memory betrays me a tad here - see note n° 1767 for unexpected revelations on this subject.

⁹⁴⁵(*) 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 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....

⁹⁴⁶(**) Loc. cit. page 160 (I'm not making this up!).

⁹⁴⁷(***) 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 judge⁹⁴⁸ (*). 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 scene⁹⁴⁹ (**).

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 the⁹⁵⁰ 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 subject⁹⁵¹ (**), 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

 ⁹⁴⁸(*) The composition of this lamentable jury will be revealed (to the reader who has resisted until then) in the final note 1767 of "The Sixth Nail" to my coffin. ...

⁹⁴⁹(**) 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.

⁹⁵⁰(*) Apart from the results on fi ltrations of fi ber functors, which are more technical and harder to compress into a single, striking statement.

⁹⁵¹(**) 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

p. 1179

of two years, a mathematical culture that may rightly seem impressive: structural theory of algebraic groups, both on general bodies and on the \Box corps of reals, theory of stranded schemes of 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

p. 1180

⊗-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 Mathéma- ticiens, 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!⁹⁵² (*)) - 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 Ormails⁹⁵³ (**), 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.

18.5.9.6. f. Lowly jobs

Note 176₆ 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

⁹⁵²(*) On this subject, see the note entitled (appropriately enough) "Les bonnes références", n° 82.

⁹⁵³(**) "Les Ormails" is the name of the residential part of the IHES (Institut des Hautes Etudes Scientifi ques), 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.

p. 1182 by

by copying on 128 pages \Box 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 car. p > 0, there is no fiber functor "rational over Q)" (or even over 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").

p. 1183 "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**⁹⁵⁴ (*).

⁹⁵⁴(*) 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 176₇ (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_1 à 176_6 , 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^{\circ} 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 $_{p.\,1184}$ 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, however⁹⁵⁵ (*), 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_1 to 171_4) that now form the end of the Apotheosis.

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

⁹⁵⁵(*) 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.

 $_{p.\ 1185}$ It's not that I didn't want to, of course. \Box 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...

 \Box 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 work⁹⁵⁶ (*).

 \Box In either case, I have to admit that my responsibility is engaged to the same extent.

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 (lighthearted) 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 $alone^{957}$ (*)), 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 blessing⁹⁵⁸ (**), gives the thing a \Box inhabitual dimension - that of the corruption of an entire without making it honorable, however brilliant it may be, or removing one iota of its indecency.

p. 1188

p. 1187

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.

⁹⁵⁶(*) 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.

⁹⁵⁷(*) On this subject, see the note "Brothers and spouses - or the double signature" (n° 134), especially pages 614-615.

⁹⁵⁸(**) For this most explicit blessing, see the note "L'album de famille", part d. ("l'Enterrement - ou la pente naturelle").

18.5. THE FOUR OPERATIONS (on a skin)

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!)⁹⁶⁰ (**). 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. Se. 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!

p. 1189

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 sheets⁹⁶¹ (*), 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 situation⁹⁶² (**)). 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 me⁹⁶³ (***).

 $^{962}(**)$ See the aforementioned section "The student and the program", n° 25.

⁹⁵⁹(*) This thesis was discussed in sub-note n° 85₁ (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).

⁹⁶⁰(**) 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).

⁹⁶¹(*) 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... ...

 $^{^{963}(***)}$ 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

18.5. THE FOUR OPERATIONS (on a skin)

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 \Box le 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.

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 964 (*), 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

there ...

⁹⁶⁴(*) 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 me⁹⁶⁵ (**), 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 author⁹⁶⁶ (***) sets out, with maestria

p. 1192

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 ways⁹⁶⁷ (*). 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 thesis⁹⁶⁸ (**) 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 (***)) unexpectedly pretending to "sink" him \Box without warning.

p. 1193

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.

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). ...

⁹⁶⁷(*) 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.

⁹⁶⁵(**) 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° 164₁ (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.

⁹⁶⁶(***) This, at least, is the conviction I arrived at in my penultimate note "Monsieur Verdoux - ou le cavalier servant" (n° 176).5

⁹⁶⁸(**) 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!

⁹⁶⁹(***) 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...

18.5. THE FOUR OPERATIONS (on a skin)

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 itself⁹⁷⁰ (*). 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 wellfounded... .).

END OF THE "FOUR OPERATIONS (ON A BODY)" SECTION

⁹⁷⁰(*) 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-car. body, say) for it to be "algebraic". I found **twelve** variants, in all, of the Hodge and Tate conjectures⁹⁷¹ (*). 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 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 sixties⁹⁷² (**).

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

p. 1195

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 \Box 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 Réflexions⁹⁷³ (*) 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... ...

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

⁹⁷¹(*) (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 suffi sance", 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...).

⁹⁷²(**) This question of the "mysterious functor", establishing the "missing link" between crystalline cohomology in car. p (via the notion of F -cristal fi ltré, F as "Frobénius"), and p-adic cohomology in car. 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. ...

⁹⁷³(*) 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 Reflexions.

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"⁹⁷⁴ (**)!

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 $myself^{975}$ (*)...

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 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

Cortège X or "Funeral Van" (with the Gravedigger), notes 93-97.

⁹⁷⁴(**) (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.

⁹⁷⁵(*) 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
Contact and Weally (with the Control in the section of the section) and the section of the section of the section.

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 furnishing⁹⁷⁶ (*). 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 (**), I ended up relying on the services of one of my friends.

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 finitetype 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 encountered⁹⁷⁸ (*)), 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

 $^{^{976}(*)}$ On the subject of the impulse within me to "build houses" (mathematics), see the note "Yin the servant, and the new masters" (n° 135).

⁹⁷⁷(**) 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.

⁹⁷⁸(*) 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 \Box 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 explained⁹⁷⁹ (*).

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 \Box 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 it⁹⁸⁰ (*)!), 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

 ⁹⁷⁹(*) 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).

⁹⁸⁰(*) 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" (**) to the explicit description of "the" category of motifs sur un

p. 1201

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 conjectures⁹⁸² (*).

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 coeffi cient problem bequeathed by the disowned master. . .) that will be best suited to developing the sixvariance 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)).

(where *Hom* will therefore be modules of type fi ni 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 it is true that there exist (over a fi ni 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 fi ni 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 $Hom(T^{i}, H)$ (*in dimension 2i*), where *T* is Tate's object, and H_{rr} is the hypothetical functor considered. This is why the construction of this

⁹⁸¹(**) 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 defi nition of the "De Rham-Mebkhout coeffi cients" (initially without fi ltrations or *F* -structures) above, say, a smooth scheme on the absolute basis Z. This defi nition should at the same time provide the key of "the" correct defi nition of general crystalline coeffi cients 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 coeffi cients (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 <u>*RHom*(., Q_X)</u> with respect to the structural bundle of the scheme under consideration, which transforms D_X -Modules of fi ni type (which can be considered as " Q_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, ho- lonomy, regularity) for "good coeffi cients", which Deligne lacked. The advantage of Deligne's point of view is that it provides an equivalence (instead of an anti-equivalence) with the coeffi cients 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 coeffi cients 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.

⁹⁸²(*) 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.

I have no doubt that this construction is correct. The work that remains to be done, no doubt clearlyp .1202 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 conjectures⁹⁸³ (*) 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^{984} (**). This time I'm finally going to do it!

* *

Project 1: Topos. I mention them here mainly for the record, having expressed myself quite circumspectly-

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^{1} "), 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 "faisceautique" 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 l-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

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.

⁹⁸³(*) 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 "defi nitive" 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).

⁹⁸⁴(**) 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 his⁹⁸⁵ (*) 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 cohomo- logical 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^{986} (**).

p. 1205

 \Box I now see the two currents coming together in a new discipline, which I have proposed elsewhere⁹⁸⁷ (*)

to call it **topological algebra**, a synthesis of traditional homological algebra (derived catego- ries 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" na- ture 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 coli- mites 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.

 $^{^{985}}$ (*) 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)).

 $^{^{986}(**)}$ 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.)

⁹⁸⁷(*) See sub-note no.[°] 136₁ to the note "Yin the Servant - or generosity" (especially p. 638).

right in line with them.

Two years ago, with my letter to Daniel Quillen⁹⁸⁸ (**), 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 operations⁹⁸⁹ (*).

We can see that this is not strictly speaking a "derelict site" (since the for- malization 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 exceptions⁹⁹⁰ (**)) 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 Harvest and Semesh⁹⁹¹ (***), I intend to include a short sketch of the essential features of the "six operations" formalism. Thanks to the care of my , 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)⁹⁹² (*), 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.

useless. ... " (n° 170 (v)), part (b) ("Machines for doing nothing. . . ").

⁹⁸⁸(**) For more on this letter, see "The weight of a past" (n° 50, page 136, 2nd paragraph).

⁹⁸⁹(*) (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)).

 $^{^{990}(^{**})}$ 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 car. 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.

⁹⁹¹(***) I would remind you that this is volume 3 of Réflexions, which in principle also contains the last part of Récoltes et Semailles. ⁹⁹²(*) These are also the two seminars, as if by chance, that the text which presents itself as "central" and is called (oh irony!) "SGA 4¹" recommends not to read. ... (May 29) For the scope of the six operations vision, see the note "Details".

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 conjectures⁹⁹³ (**). It was at the heart of my interest in cohomology throughout the 1960s.

It was clearly posed, with all the necessary generality and precision, for the main types of coefficients then glimpsed²⁴⁴ (***). I'm speaking about this obviously crucial problem \Box for

p. 1208

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 = C, field of complexes) Betti or Hodge cohomology.

I don't think it's an exaggeration to say that this problematic contains in its germ⁹⁹⁵ (*), both the "Hodge-Deligne theory" "in full maturity" which is still waiting to emerge, and the "De Rham-Mebkhout coefficient theory" which is also waiting⁹⁹⁶ (**); and it's for one and the same reason that both

⁹⁹³(**) 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)).

⁹⁹⁴(***) It doesn't seem that any new types of coeffi cient have appeared, compared to those I had already foreseen in the second half of the sixties.

⁹⁹⁵(*) 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.

⁹⁹⁶(**) 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 defi nes a "good" category of coeffi cients over the spec(C) scheme only, and the functors Rf_1 , Rf_* in the case of the structural morphism $X \rightarrow Spec(C)$ of a separate typed fi ni scheme over C, and for constant coeffi cients (alas!) over X. Mebkhout defi nes a "good" category of coeffi cients, valid in principle for any separate X of type

fi ni on a field of zero characteristic K - but he doesn't go so far as to define functors Rf_1 and Rf_2 for a morphism $f: X \to Y$ of such schemes on K, and to develop a duality theorem for Rf_1 , and Lf^2 (except for Y = 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.

p. 1209

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 coefficients⁹⁹⁷ (*), on a more or less arbitrary base X: l-adic coefficients (**any** prime number),

the **De Rham-Mebkhout** coefficients⁹⁹⁸ (**) (of particular interest for *X* of finite type on a base scheme) *S*, the most \Box important cases being those where *S* is the spectrum of the rings Z, Q, or C), finally the coefficients of p. 1210

Betti (for X of finite type on 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 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 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

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.

⁹⁹⁷(*) If I speak of "fundamental" types of coeffi cient, it is to suggest that all the other important types of coeffi cient 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 enri- chments envisaged on De Rham-Mebkhout coeffi cients, there is (in addition to "fi ltration by weights", which seems "internal" to the category of coeffi cients envisaged), a "De Rham fi ltration" 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 fi ltration to satisfy. I intend to return to these questions in greater detail in "Les motifs mes amours" (in Vol. 3 of Réflexions).

⁹⁹⁸(**) I recall that for this type of De Rham-Mebkhout coeffi cient, 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 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... .

p. 1211

□ 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 exstudent 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

_{p. 1212} (or, more precisely and \Box more generally, "pro-algebraic sheaves" on *k*). He completed this task

in his thesis, published in Lecture Notes in 1972 (LN 265)⁹⁹⁹ (*). I had taken this dictionary a step further

⁹⁹⁹(*) (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 thesis¹⁰⁰⁰ (**), 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 at¹⁰⁰¹ (***), was the determination of the group of Galois of said finite field, which must be commutative (being generated topologically by the element _{p. 1213} of Frobenius), and is in fact an extension of 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 functors¹⁰⁰³ (**).

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. ¹⁰⁰⁴(***). This is how mathematics is done in the 1980s, after many

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" operation¹

SGA 5" done in the same inimitable style), see the suite of notes "The sixth nail in the coffin" (n° s 176₁ to 176).₇

¹⁰⁰⁰(**) (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".

¹⁰⁰¹(***) (May 10) I note that this determination, too, fi gures 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. ...

¹⁰⁰²(*) 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. ¹⁰⁰³(**) The crucial point is that this class becomes zero (thanks to the existence of the "*l-adic* cohomology" fi bres functors) in

all places $l \neq p = car.k$, and the existence of the crystalline functor-fi bre gives us sufficient information about the fate of this

class in the missing place p.

 $^{^{1004}(***)}$ 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 \Box antecedents in the 1970s¹⁰⁰⁵ (*).

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

$$\pi_{1}^{\mathrm{mot}}(X) \to \pi_{1}^{\mathrm{mot}}(\mathrm{Spec}(k))$$

(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 previously¹⁰⁰⁶ (**). The formal description of this "homotopy type"¹⁰⁰⁷ (***),

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 \Box (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^{1008} (*). 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"¹⁰⁰⁹ (**) (and which we can hope will make true various conjectures of the Hodge type) would be sufficient to build such a theory.

the same brilliant author. On this subject, see the sub-note ". . . and nonsense" ($n^{\circ} 169_6$) to the note "Les manoeuvres" ($n^{\circ} 169$), p. 891.

^{1005(*)} And even as far back as the sixties - see the note "L'éviction" (n° 63).

 $^{^{1006}(**)}$ In the note "Requiem pour vague squelette" (n° 165).

¹⁰⁰⁷(***) 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 Spec(C) topos fpqc associated with the sheaf (on this topos fpqc) of functor-fi bers 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-fi bre, the corresponding homotopy-fi bre type must be canonically isomorphic to the homotopy Q-type (neglecting torsion phenomena. . .) associated transcendently with $X \otimes_K C$, at least when $X \otimes_K C$ is 1-connected.

¹⁰⁰⁸ (*) Algebraic Geometry, Bombay 1968, Oxford University Press (1969).

¹⁰⁰⁹(**) 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 nonalgebraic 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.

 \Box This does not prevent the standard conjectures and their variants on the one hand, and those of Hodge, Tate and their p

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 itself¹⁰¹⁰ (*).

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 \Box most burning", with a view to building this "geometry p. 1217 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 framework¹⁰¹¹ (*). Egale-

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.

¹⁰¹⁰(*) 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_5 , 169_6) to the note "Les manoeuvres".

¹⁰¹¹(*) This conjecture is spelled out for the first time, it seems, in sub-note n° 87₁ 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 "arithme- tical" 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 Serre¹⁰¹² (**), in the category of unipotent algebraic groups with near radicial isogeny, over a *k-body* of car. 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 \Box 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 Raynaud¹⁰¹³ (*). 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

¹⁰¹²(**) 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.

 $^{^{1013}(*)}$ See note "Cercueil 3 - ou les jacobiennes un peu trop relatives" (n° 95), and sub-note n° 95₁, 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 ment 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.

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 know¹⁰¹⁴ (*), 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.

¹⁰¹⁴(*) I first wrote about the "mathematical community" in the first part of Harvest and Sowing, in the section "The 'mathematical community': fi ction 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 fi ctive 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

[&]quot;(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 "fi ctive" than I am myself, who feels part of it, more lucidly than I once did. The "fi ction" 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?

This degradation culminates in operations such as ""SGA 4_2 " - 1 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 days¹⁰¹⁵ (*). 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

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 \Box tendency to minimize what was

p. 1222

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.

p. 1221

¹⁰¹⁵(*) 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)₁

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 $\Box d^{\text{in}}$ the easygoing attitude of scientists towards the seductions of military money (not to To speak only of this aspect) and the conveniences it offers, I detect a lack of self-respect, on both an individual and collective level¹⁰¹⁶ (*). 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 Officiant¹⁰¹⁷ (**). 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

¹⁰¹⁶(*) 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!

¹⁰¹⁷(**) (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.

p. 1224

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 \Box 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'Enterre- ment. 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 affection¹⁰¹⁸ (*).

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 \Box une fin, or at least 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 Semailles¹⁰¹⁹ (*). These notes, as well as a reflection,

¹⁰¹⁸(*) These are letters from D. Mumford, I.M. Gelfand and J. Murre.

¹⁰¹⁹(*) 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)¹⁰²⁰ (**). 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" (n⁸4), without \Box 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!¹⁰²¹ (*). 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 students¹⁰²² (**). 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

It may be less apparent in later parts, but it's no less present.

¹⁰²⁰(**) (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.

¹⁰²¹(*) (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 fi lial piety" (n° 176).

¹⁰²²(**) 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).

p. 1227

p. 1228

This (seemingly relatively rational) "cause", which is my "dissidence", seems to me to have nothing in common, however, with the breath of violence that 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 revenge¹⁰²³ (*), 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

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 hidden¹⁰²⁴ (*) has begun to unravel into more incidental things.

¹⁰²³(*) 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... ...

¹⁰²⁴(*) 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 \Box "private" life. This is not the

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 myself¹⁰²⁵ (*).

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).

 \Box "... 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

¹⁰²⁵(*) 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 your¹⁰²⁶ (*) 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 you¹⁰²⁶ (*) 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", 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

p. 1231

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

916

¹⁰²⁶(*) 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, p. 1232 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 advantages¹⁰²⁷ (*). 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"¹⁰²⁸ (**) 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, 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.

^{1027(*)} This is the ambiguity referred to in a previous b. de p. note (note (*) on p. 1219).

¹⁰²⁸(**) 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 Renouvellement, I occasionally refer to them as "parts" of R and S (not to be confused with the five parts of "Fatuité et Renouvellement" 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 $is \square Récoltes$ 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 reflection¹⁰²⁹ (*).

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.

 \Box the lonely child who can attract an adventure no one else in the world wants, and an acquaintance,

p. 1234

¹⁰²⁹(*) (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 1.236 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 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

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 □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 cruder¹⁰³⁰ (*) 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 \Box 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.

¹⁰³⁰(*) 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 infi me 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 Burial¹⁰³¹ (*). 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 \Box épisodedisease¹⁰³² (*).

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 me¹⁰³³ (**)! 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 \Box brilliant friend Pierre. This reflection is deepened in p.

1240

note "L'ambiguïté" (n° 63") of June 1, in which she focuses on my relationships with my students in general. This is where

past" (n° 50) from Fatuity and Renewal, the section on which these notes are intended to comment.

¹⁰³³(**) 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).

¹⁰³¹(*) 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,

¹⁰³²(*) 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).

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° 169₁) (in the second part of the sub-note, dated March 16). In it, Inote that the eviction of my person from the SGA seminar (which represents the sum total of ten years' investment in my life)¹⁰³⁴ (*), 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 end¹⁰³⁵ (**). 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

¹⁰³⁴(*) For the latest episode in this eviction, see the note "Les Pompes Funèbres - "im Dienste der Wissenschaft" (n° 175).

¹⁰³⁵(**) 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"¹⁰³⁶ (*): 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 least¹⁰³⁷ (**) 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 still¹⁰³⁸ (***). (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....)

 \Box Very quickly and without having sought it or foreseen it, it is "the conflict" in human life and in the person that _p.

1242

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 me¹⁰³⁹ (*). 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 others¹⁰⁴⁰ (**). 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 $I \Box$ ve learned (or at least **begun** to learn), in the course of the entire Harvesting and sowing.

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"¹⁰⁴¹ (*). 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.

p. 1243

¹⁰³⁶(*) 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.

 $^{^{1037}(\}ast\ast)$ In the note "Le renversement (3) - ou yin enterre yang" (n° 137).

¹⁰³⁸(***) At the beginning of the note "Patte de velours - ou les sourires" (n° 137).

¹⁰³⁹(*) This "interpellation" began to be perceived especially since my long meditation on my parents, which continued between August 1979 and March 1980.

¹⁰⁴⁰(**) This observation constitutes the high point of the reflection pursued in the note "Without hatred and without mercy" (n° 157). ¹⁰⁴¹(*) 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 subjecting others, and without there

p. 1244

think twice about this "beyond comprehension" violence¹⁰⁴² (*). 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,

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,

¹⁰⁴²(*)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 \Box 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 least¹⁰⁴³ (*), 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 1044 (*).

p. 1246

As I make this unexpected observation about my approach to mathematics, in the note "La mer qui monte. ..." $(n^{\circ} 122)^{1045}$ (**), 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

¹⁰⁴³(*) As I've had occasion to say over and over again in the course of R and S, one of the two strongest egotic 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.)

¹⁰⁴⁴(*) 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.

 $^{^{1045}(\}ast\ast)$ See also the later note "The arrow and the wave" (n $^{\circ}$ 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.

p. 1247

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 nonegotistical 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 Sowing¹⁰⁴⁶ (*), which has not yet been completed. It began as

¹⁰⁴⁶(*) (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 ", 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"¹⁰⁴⁷ (*)), 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 "**l'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 Renouvelle- ment", "L' Enterrement (I) - ou la robe de l' Empereur de Chine", and "L' Enterrement (III) - ou les Quatres Opérations"¹⁰⁴⁸ (**). 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 fray¹⁰⁴⁹ (*). 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... . ¹⁰⁵⁰(**)), 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.

p.

¹⁰⁴⁷(*) Thinking of writing "enlisement", 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.

¹⁰⁴⁸(**) 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).₇

¹⁰⁴⁹(*) 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"...).

¹⁰⁵⁰(**) 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

I'm also grateful to Pierre Deligne, who came to see me and get to know me better. \Box sance (last October) of the then-written part of L'Enterrement, and to let me know about his (*). 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 ways¹⁰⁵² (**). 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".

It's true that, in a far more acute way than last year (when the "second wind" of reflection

p. 1251 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 smugness¹⁰⁵³ (*)). If it's true, as it seemed to me at times, that to understand the Burial is also to "understand the

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"?

¹⁰⁵¹(*) 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).

¹⁰⁵²(**) These "disturbances" are alluded to here and there in the notes of recent months. See, in particular, the note "Le messager (2)" (n° 181).

¹⁰⁵³(*) (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!

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 \Box 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**".