Part two.

BURIAL (I)

or the Emperor of China's robe

To those who have been my
friends and to the few who
have remained so
and to those who came in great numbers to sing at my
funeral

To the memory of a memorable

Colloquium... and to the entire

Congregation...

9. A) HERITAGE AND INHERITANCE

Contents

13.1. I The student posthumous

13.1.1. Teaching failure (2) - or creation and fatuity

Note 44' [This note is called by section 50 of Chapter **VIII The Lonely Adventure** of Part **(I) Fatuity and Renewal** p. 227]

p. 173 This passage struck a chord with the friend I asked to read this last section, "The weight of a past" [1](*). He wrote to me: "For many of your former students, the aspect, as you say, of the invasive and almost destructive 'boss' has remained strong. Hence the impression you have." (Knowing, I presume, "the impression" that is expressed in certain passages of this section and notes n° s 46,47,50 that complete it.) Earlier he writes: "First of all, I think you did well to leave mathematics for a moment [!], because there was a kind of incomprehension between you and your students (apart from Deligne, of course). They were left a little dumbfounded... . ".

It's the first time I've heard such high praise for my role as "boss" before 1970, going beyond the usual compliments! Further up in the same letter: ". . I understand that your former students [read: those "before 1970"] don't really know what it's like to **create** mathematics, and that you may have had something to do with it. . . It's true that in their time, the problems were all posed... . "[2] (**).

My correspondent probably means that it was **I** who was posing the "problems", and with them the notions of that I had to develop, rather than leaving it up to my students to find them; and that it was in this way that I may have obscured in them the knowledge of what is the essential part of the work of
p. 174 mathematical creation. This also ties in with a impression that emerged from the conversation with two of my post-1970 ex-students, mentioned in a previous note (note (23iv)). It's true that, in the students who came to me, I was above all looking for **collaborators** to develop intuitions and ideas that had already formed within me, to "push at the wheels", in short, of a carriage that was already there, which they therefore didn't have to pull out of a kind of nothingness (as my correspondent had to do). And yet, this is what has always been the most fascinating aspect of mathematical work for me, and the part of the work in which I felt that a "creation" was taking place, the "birth" of something more delicate and more essential than a simple "result".

If I sometimes see some of my students treating this prized thing with disdain, and thus displaying the "snobbery" of which J.H.C. Whitehead spoke (which consists in despising what one "would know how to dis- show")[3] (*), I'm no doubt no stranger to it, in one way or another. The failure of my teaching, glaringly obvious in the period after 1970, is now also apparent to me, in a different and more hidden form, in my teaching in the first period, even though in the conventional sense it appears to have been a complete success! It's something I've already glimpsed from time to time over the last few years, and which I've mentioned in letters to several of my ex-students, without so far having received any real response from any of them.

I don't think it would be accurate, however, to say that the work I proposed to my students, and what they did with me, was purely technical work, of pure routine, unsuited to bringing their personalities into play.

[1](*) (May 10) The friend in question is none other than Zoghman Mebkhout, who has kindly authorized me to lift the anonymity I felt I had to maintain regarding the origin of the letter (April 2, 1984) I quote in this note.

[2](**) (May 10) The above quotation is heavily truncated, out of respect for my correspondent's anonymity. See the following note for a full quotation of the passage from which this quotation is taken, and for comments on its true meaning, which had initially escaped me for lack of more detailed information.

[3](*) See note "Youth snobbery - or the defenders of purity", n° 27 p. 247.

creative faculties. I put at their disposal tangible and sure starting points, between which they were free to choose, and from which they could launch out, as I myself had done before them. I don't think I've ever proposed a subject to a pupil that I wouldn't have enjoyed tackling myself; nor do I think there has been such an arid path in the journey that any of them have taken with me. that I haven't myself gone through others just as arid in the course of my mathematical life, without getting discouraged or rushing through them.
when it was clear that the job had to be done and there was no other way.

Also it seems to me that the failure I see today has more subtle causes than the type p. 175
of themes I proposed, and to what extent these remained nebulous or were, on the contrary, clear-cut. My share in this failure seems to me to be due rather to attitudes of fatuity in my relationship to mathematics; attitudes which I have had occasion to examine in this reflection. If not in the actual work with a particular student, then at least in the atmosphere that surrounded me. Fatuity, even when it is expressed in the most "discreet" way in the world, always goes in the direction of a closure, an insensitivity to the delicate essence of things and their beauty - whether these are "mathematical things", or living people whom we have the power to welcome, encourage, or also to look upon from the height of our grandeur, insensitive to the breath that accompanies us and to its destructive effects on others as on ourselves.

13.1.2. A feeling of injustice and powerlessness

Note 44" [The appearance of this note does not respect the chronological order of writing]

(May 10) Taking advantage of my friend's permission to quote freely from his letters as I see fit, I give here a more complete quotation[4] (*), which places the truncated quote in its true context:

> "It's true that I was very isolated between 75-80, apart from a few rare questions to Verdier. But I don't blame your former students for that period, because nobody really understood the importance of this link [read: between discrete coefficients and continuous coefficients]. That all changed in October 1980, when we discovered the first very important application of this link for semisimple groups, namely the proof of the Kazhdan-Lusztig multiplicity formula, where we made essential use of the category equivalence in question. This equivalence took the name of "Riemann-Hilbert correspondence" without further comment - after all, it's so natural! That's when I realized that your former students don't really know what a mathematical **creation** is, and that perhaps you were partly to blame. I still feel a sense of injustice and powerlessness. It's true that by their time, the problems had all been solved. The number of applications of this theorem is impressive, both in the context of
> the transcendental framework, but always under the name of correspondence.
> of Riemann-Hilbert! I feel my name is unworthy of this result for many p. 176
> people and in particular for your former students. But as you can clearly see from the introductions to my work, it's your "duality" formalism that naturally leads to this result. But like you, I'm not worried about the future of this link between "constructible discrete coefficients" and crystalline coefficients (or holonomic D-Modules). It's clear that it applies in many fields both in the cohomology of spaces and in analysis."

It was this passage from my friend's letter that inspired (in addition to the present note) the later "L'inconnu" (The unknown).

[4](*) See second b. de p. of the previous note. "The failure of teaching (2) - or creation and fatuity", n° 44'.

and the good Lord's theorem". From the terms of this letter, I had no idea (as I explain in its place) that this "feeling of injustice and powerlessness" in my friend was the reaction, not simply to an attitude of blind disdain systematically **minimizing** his contributions (an attitude which has become quite familiar to me, among some of my former students), but to a veritable operation of swindling, consisting in purely and simply **swindling** the paternity of a theorem - key. This situation became clear to me only eight days ago - see the note "L' Iniquité - ou le sens d'un retour" and the following notes (n° s 75 to 80), grouped together under the title "Le Colloque - ou faisceaux de Mebkhout et Perversité".

Note 45 My change of environment and lifestyle has meant that opportunities to meet or make further contact with my old friends have become rare. This has not prevented signs of "distancing" from manifesting themselves in many ways, to a greater or lesser degree from one to another. With others, however, such as Dieudonné, Cartan or Schwartz, and indeed with all the "elders" who had given me such a warm welcome in my early days, I felt nothing of the kind. Apart from these, however, I have the impression that there are very few of my former friends or students in the mathematical world whose relationship with me (whether or not it finds occasion to express itself) has not become divided, "ambivalent", after I withdrew from what was once a common milieu, a common world.

13.2. II orphans

13.2.1. My orphans

Note 46 [This note is called by section 50 of Chapter **VIII The Lonely Adventure** of Part **(I) Fatuity and Renewal** p.]

p. 177 I'd like to take this opportunity to say a few words here about mathematical notions and ideas, of all those I've uncovered, which seem to me (by far) to have the greatest impact $(46\ )_1$[5] (*). First and foremost, there are five closely related key concepts, which I'll briefly review, in order of increasing specificity and richness (and depth).

The first is the idea of a **derived category** in homological algebra (see note 48 on p. 274), and its use in a "catch-all" formalism known as **the** "**six operations formalism**" (i.e. the operations of the "six operations"). $\overset{L}{\otimes}$, Lf^* $Rf_!$, $RHom$, Rf_* , $Lf^!$)) $(46_2\ )$ for the cohomology of the most important types of "spaces" which are have so far been introduced in geometry: "algebraic" spaces (such as schemas, schema- tic multiplicities, etc. . .), analytic spaces (both complex analytic, and rigid-analytic and similar), topological spaces (pending, of course, the context of "moderated spaces" of all kinds, and surely many more besides, such as that of the category (Cat) of small categories, serving as homotopic models. . .).). This formalism covers both discrete and continuous coefficients.

The gradual discovery of this duality formalism and its ubiquity came about through a solitary, obstinate and demanding process of reflection, which continued between 1956 and 1963. It was in the course of this reflection that the notion of derived category gradually emerged, along with an understanding of its proper role in homological algebra.

[5](*) Readers will find in notes n° 46_1 to 46_9 some more technical comments on the concepts reviewed in the present note. On the other hand, independently of the specific **notions** I have introduced, the reader will find some reflections on what I consider to be "the main part" of my work (within the part of my work "fully completed"), in note no.° 88 "La dépouille".

What was still lacking in my vision of the cohomological formalism of "spaces" was an understanding of the link between discrete coefficients and continuous coefficients, beyond the familiar case of "spaces".
local systems and their interpretation in terms of integrable connection modules, or mo- crystals. p. 178
dules. This deep connection, first formulated in the framework of complex analytic spaces, was discovered and established (nearly twenty years later) by Zoghman Mebkhout, in terms of derived categories formed on the one hand using "constructible" discrete coefficients, on the other using the notion of "*D-Module*" or "complex of differential operators" (cf. note 46_3 p.).

For almost ten years, Zoghman Mebkhout pursued his remarkable work in almost total isolation, in the absence of encouragement from those of my former students who were best placed to give it to him, and to back him up with their interest and the experience they had acquired through my contact. This did not prevent him from discovering and proving two key theorems[6] (*) of a new crystalline theory that was being born in the midst of general indifference, both of which (and this was a decidedly bad sign!) expressed in terms of derived categories: one giving the category equivalence between "constructible discrete" coefficients and crystalline coefficients (satisfying certain conditions of "ho- lonomy" and "regularity") (48'), the other being "**the**" crystalline global duality theorem, for the constant application of a smooth complex analytic space (not necessarily compact, which entails difficulties
techniques) to a point. These are deep theorems[7] (**), which throw
a nou veau day on the cohomology of both analytic and schematic spaces (in characteristic zero p. 179
for the moment), and hold out the promise of a far-reaching renewal of the cohomological theory of these spaces. After two unsuccessful applications to the CNRS, they finally earned their author a position as a research fellow (equivalent to an assistant or senior assistant at the University).

No one in these ten years has thought of telling Mebkhout, who is grappling with the considerable technical difficulties of the transcendent context, about the "six-variance formalism", well known to my students[8] (*), but nowhere to be found "on the net". He finally learned of its existence from me last year (in the form of a form that, apparently, is known only to me. . .), when he was kind enough and patient enough to explain to me what he had done; to me, who wasn't so much into cohomology any more . . Nor did anyone think to suggest to him that it might be more "profitable" to start with the context of zero-characteristic schemes, where the difficulties inherent in the transcendental context disappear, and where, on the other hand, the conceptual questions fundamental to the theory appear all the more clearly. No one thought of pointing out to him (or even noticed what was known to me from the time I introduced crystals[9] (**)) that the "*D-Modules*" on spaces (analytic or schematic)

[6](*) (June 7) Mebkhout points out that, in addition to these two theorems, a third must be added, also expressed in terms of derived categories, namely what he has called (somewhat improperly perhaps) the "**biduality theorem**" for *D* -Modules, and which is the most diffi cult of the three. For an overview of Mebkhout's ideas and results and their uses, see Le Dung Trang and Zoghman Mebkhout, Introduction to linear differential Systems, Proc. of Symposia in Pure Mathematics, vol.40 (1983) part.2, pp. 31-63.

[7](**) (May 30) The demonstration of the second theorem runs up against the technical diffi culties usual to the transcendental context, requiring the use of "evesque" techniques, so I guess it can be classed as a "diffi cult" demonstration. That of the first theorem is "obvious" - and profound, using the full force of Hironaka's resolution of singularities.

As I point out in the penultimate paragraph of the note "Solidarity" (n◦ 85), once the theorem has been worked out, "anyone" who is well-informed is capable of proving it. Compare also with J.H.C. Whitehead's observation quoted in the note "Youth snobbery - or the defenders of purity" (n◦ 27). When I wrote this last note, as if under the silent dictation of a secret prescience, I had no idea how far reality would go beyond my timid, groping suggestions!

[8](*) They learned this first-hand in the SGA 4 and SGA 5 seminars, and by text, in R. Hartshorne's "Residues and Duality".

[9](**) (May 30) But I had time to forget it - only to remember it again by virtue of the second encounter with Mebkhout, last year. (See note "Rencontre d'outre-tombe", n◦ 78.

smooth spaces are no more and no less than "**module crystals**" (when we disregard any question of "coherence" for either of them), and that the latter was a catch-all notion that worked just as well for "spaces" with any singularities as it did for smooth spaces (46).4

Given Mebkhout's means (and uncommon courage), it is quite clear to me that, placed in an atmosphere of sympathy, he would have had no difficulty but great pleasure in establishing the complete formalism of the "six variances" in the context of the crystalline cohomology of zero characteristic schemes, while all the ideas essential for such a large-scale program (including his own in addition to those of the others) would have been available to him.

p. 180 of the Sato school and my own) were already, it seemed to me, combined. For someone of his caliber, this was a matter of a few years' work, as was the development of an all-purpose formalism from cohomologie étale was a matter of a few years (1962-1965), as long as the common thread of the six operations was already known (in addition to the two key theorems of base change). It's true that these were years carried by a current of enthusiasm and sympathy from those who were co-actors or witnesses, and not a work against the haughty smugness of those who have everything in hand. ...

This brings me to the second pair of notions I wanted to talk about, that of **schema**, and the closely related one of **topos.** The latter is the more intrinsic version of the notion of **site**, which I first in- troduced to formalize the topological intuition of "localization". (The term "site" was later introduced by Jean Giraud, who also did much to give the notions of site and topos the necessary flexibility). It was the obvious needs of algebraic geometry that led me to introduce diagrams and topos one after the other. This pair of notions potentially contains a vast renewal of algebraic geometry, arithmetic and topology, through a **synthesis of** these "worlds", too long separated, in a common geometric intuition.

The renewal of algebraic geometry and arithmetic through the point of view of diagrams and the language of sites (or of "descent"), and through twelve years of work on the foundations (not counting the work of my students and other willing participants), has been accomplished over the last twenty years: the notion of diagram, and that of spread cohomology of diagrams (if not that of spread topos and that of spread multiplicity) have finally entered the mainstream, and the common heritage.

On the other hand, this vast synthesis, which would also encompass topology, while for the past twenty years ideas have been essential and the main technical tools required seem to me to be gathered and ready[10] (*), is still biding its

p. 181 time. For five years (since my departure from the mathematical scene), the fertile unifying idea and the The notion of topos, a powerful tool for discovery, has been relegated by a certain fashion[11] (*) to the sidelines of serious notions. Even today, few topologists have the slightest inkling of this considerable potential expansion of their science, and of the new resources it offers.

In this renewed vision, the topological spaces, differentiable spaces, etc. ... that the topologist handles daily are, along with schemas (which he has heard of) and topological multiplicities, differentiable spaces, etc. ... .

[10](*) (May 15) These "essential ideas and principal technical means" had been brought together in the vast fresco of the SGA 4 and SGA 5 seminars, between 1963 and 1965. The strange vicissitudes that befell the writing and publication of the SGA 5 part of this fresco, which appeared (in unrecognizable, devastated form) eleven years later (in 1977), give a striking picture of the fate of this vast vision in the hands of "a certain fashion" - or rather, in the hands of some of my students who were the first to introduce it (see note b. on next p.). These vicissitudes and their meaning are gradually revealed in the course of reflection over the last four weeks, continuing in the notes "Le compère", "La table rase", "L'être à part". "The signal", "The renversement", "Le silence", "La solidarité", "La mystifi cation", "Le défunt", "Le massacre", "La dépouille", notes n◦ s 63"', 67, 67', 68, 68' and 84-88.

[11](May 13) Further reflection in the six weeks since these lines were written (end of March), has revealed that this "fashion" was first and foremost established by some of my students - the very ones who were best placed to make a certain vision, ideas and technical means their own, and who chose to appropriate working tools, while disowning both the vision that had given rise to them, and the person in whom this vision had originated.

or schematics (which nobody talks about) are all embodiments of the same type of remarkable geometric objects, the **ringed topos** (46_5), which play the role of "spaces" in which intuitions from topology, algebraic geometry and arithmetic converge into a common geometric vision. The "modular" multiplicities of all kinds that we encounter at every step (provided we have eyes open to see) provide so many striking examples (46_6). Their in-depth study is a first-rate guide to the essential properties of geometric objects (or other objects, if there are objects that are not geometric. . .), whose variation, degeneration and generalization are described by these modular multiplicities. Yet this richness remains ignored, since the notion that allows us to describe it in detail doesn't fit into commonly accepted categories.

Another unexpected aspect brought by this recused synthesis[12] (**), is that the homotopic invariants p. 182 familiar from some of the most common spaces (46_7) (or more precisely, their profinite compactifications) are equipped with unsuspected arithmetic structures, including operations of certain profinite Galois groups. ...

And yet, for almost fifteen years now, it has been part of the "high society" to look down one's nose at anyone who utters the word "topos", unless it's in jest, or they have the excuse of being a logician.) Nor has the yoga of derived categories, to express the homology and cohomology of topological spaces, penetrated among topologists, for whom Kûnneth's formula (for a ring of coefficients that is not a body) still continues to be a system of two spectral sequences (or, at the very least, a kyrielle of short exact sequences), and not a unique canonical isomorphism in a suitable category ; and who still ignore the base change theorems (for a proper morphism or by a smooth morphism, for example), which (in the neighboring framework of stale cohomology) were the crucial turning point for the strong "start" of this cohomology (cf. note 46_8 p. 270). I shouldn't be surprised when the very people who helped develop this yoga have long since forgotten about it, and beat the crap out of anyone who even pretends to want to use it![13] (*).

The fifth notion that is closest to my heart, perhaps more than any other, is that of "**motif**". It differs from the previous four in that "**the**" right notion of pattern (even above a basic body, let alone a basic schema of any kind) has not so far been the subject of a satisfactory definition, even admitting for this purpose all the "reasonable" conjectures one would need. Or rather, visibly, **the** "reasonable conjecture" to be made, as a first step, would be that of p . 183 the **existence of** a theory, satisfying such data and such properties, that it would not be at all difficult

[12](**) (May 13) This synthesis was "rejected" in the first place, both in its spirit and in the key notion that makes it possible, by none other than the very person who was the main user and beneficiary, throughout his work, of the technical means it had enabled me to develop (with the language of diagrams and the construction of a theory of staggered cohomology). It's Pierre Deligne. Because of his exceptional influence (due to his exceptional means), and because of the very special position he occupied in relation to my work, of which he was like an implicit legatee, the discreet and systematic barrage he put up against the main ideas I had introduced (with the exception of the notion of schema and staggered cohomology) was highly effective, surely playing a leading role in the establishment of the "fashion" that **buried** these ideas, reduced for nearly fifteen years to a vegetative life. His work has been deeply marked by this ambiguity, which I first glimpsed in the reflection that continues that of the present note. (See "Refus d'un héritage - ou le prix d'une contradiction", note n° 47 p. 271) This initial perception, vivid but still confused, of this permanent hindrance in Deligne's work after my departure, became clearer and confi rmed in a striking way during all the reflection on this Burial, in which my friend plays the role of principal offi cial.

[13](*) (May 13) In the course of further reflection, it became clear that the situation began to change with the Colloque de Luminy in June 1981: those who had "forgotten" (or rather, buried. . .) these notions began to strut about with them, without however ceasing to
(See notes° s 75 and 81 on this memorable Colloquium).

(and quite fascinating!), for someone in the know[14] (*), to fully explain. In fact, I came very close to doing so, shortly before I "quit maths".

In some respects, the situation resembles that of the "infinitely small" in the heroic days of differential and integral calculus, but with two differences. Firstly, we now have the experience in building sophisticated mathematical theories, and an effective conceptual baggage, that our predecessors lacked. And then, in spite of the resources at our disposal and in the more than twenty years since this visibly essential notion first appeared, no one has deigned (or dared, in spite of those who don't deign. . .) to put their hand to the grindstone and outline a theory of patterns, as our predecessors did for infinitesimal calculus without beating about the bush. However, it's as clear now for patterns as it was for "infinitesimals", that these beasts exist, and that they manifest themselves at every step in algebraic geometry, as long as we're interested in the cohomology of algebraic varieties and families of such varieties, and more particularly in their "arithmetic" properties. Perhaps even more so than for the other four notions I've mentioned, that of motif, which is the

and richest of all, is associated with a multitude of intuitions of all kinds, by no means vague but for□mululable often with perfect precision (even sometimes, if need be, admitting a few
p. 184
motivic premises). For me, the most fascinating of these "motivic" intuitions was that of the "motivic Galois group", which, in a sense, makes it possible to "put a motivic structure" on the Galois groups profi- ned by bodies and schemes of finite type (in the absolute sense). (The technical work required to give precise meaning to this notion, in terms of the "premises" providing a provisional foundation for the notion of motive, was accomplished in Neantro Saavedra's thesis on "Tannakian categories").

The current consensus is a little more nuanced for the notion of pattern than for its three brothers (or sisters) in misfortune (derived categories, duality formalism known as the "six operations", topos), in the sense that it is not treated outright as "bombast"[15] (*). In practice, however, it all boils down to the same thing: as long as there's no way of "defining" a motif and "proving" something, serious people can only refrain from talking about it (with the greatest regret, it's a matter of course, but you're either serious or you're not... .). Of course, we may never be able to construct a theory of motives and "prove" anything about them, as long as we declare that it's not serious even to talk about them!

But the few people in the know (and who make the fashion) know very well that in terms of the premises, which remain secret, many things can be proved. In other words, ever since the notion first appeared in the wake of Weil's conjectures (though proven by Deligne, which makes it

even a good point!), **pattern yoga does** exist. But it has the status of a **secret science**, with admittedly very few initiates[16] (**). It may be "not serious", but it nevertheless allows □those rare initiates to say
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in a host of cohomology situations, "what you'd expect". It thus gives rise to

[14](*) (May 13) I've come to understand that the only person (apart from myself) who to this day responds to the rather peculiar meaning of this "somewhat in the loop" is Pierre Deligne, who for four years has had the advantage of being the day-to-day confidant of my motivic reflections, at the same time as listening to "the little I knew about algebraic geometry". It's true that I've talked about these things to many other colleagues here and there, but none of them has apparently been "plugged in" enough to assimilate an overall vision that had been developing in me over several years, or to take my indications as a starting point for developing a vision and program on their own (as I myself had done from two or three "strong impressions" produced by some of Serre's ideas). Perhaps I'm mistaken, but it seems to me that people interested in the cohomology of algebraic varieties were not psychologically disposed to "take the motifs seriously" for as long as Deligne, who was an authority on cohomology and at the same time the only one supposed to know in depth what these motifs were all about, himself passed them over in silence.

(June 8) Checking this out, it appears that my first motivational reflections date back to the early sixties - so they've been going on for almost a decade.

[15](*) As I mentioned in a previous footnote, the derived categories were exhumed three years ago to great fanfare (without my name being mentioned). The topos and the six operations are still waiting for their time, and the motives

a multitude of intuitions and partial conjectures, which are sometimes accessible after the fact by the means at hand, in the light of the understanding provided by "yoga". Several of Deligne's works are inspired by this yoga[17] (*), notably the one that (if I'm not mistaken) was his first published work, establishing the degeneracy of the Leray spectral sequence for a projective and smooth morphism of algebraic varieties (in null car., for the purposes of demonstration). This result was suggested by considerations of "weight", of an arithmetical nature. These are typically "motivic" considerations, by which I mean they can be formulated in terms of the "geometry" of motives. Deligne proved this statement with Lefschetz-Hodge theory and (if I remember correctly) said nothing about motivation (49), without which no one would have suspected anything so implausible!

In fact, the yoga of motifs was born, first and foremost, from this "yoga of weights" that I inherited from Serre[18] (**). It was he who made me understand the charm of Weil's conjectures (now Deligne's theorem). He explained to me how (modulo a hypothesis of resolution of singularities in the envisaged characteristic) one could, thanks to the yoga of weights, associate "virtual Betti numbers" with every algebraic variety (not necessarily smooth or proper) over any body - something that struck me a lot at the time (46_9). It was this idea, I believe, that was the starting point for my thinking on weights, which continued (in parallel with my tasks of writing foundations) throughout the years of

following years. (This is also the one I took up in the 70s, with the notion of "virtual pattern" on any ba□se diagram, with a view to establishing a formalism of the "six operations" at least for p . 186 patterns.

virtual). Over the years, I've talked to Deligne (as a privileged interlocutor) about this yoga of motives, and to anyone who would listen[19] (*). I certainly didn't want him and others to keep it a secret science, reserved for them alone. (⇒note 47 p. 271)

Note 46_1 At most, I would make an exception for the ideas and points of view introduced with the formulation I had given to the Riemann-Roch theorem (and with the two demonstrations I found of it), as well as for various variants of it. If I remember correctly, such variants appeared in the last lecture of the 1965/66 SGA 5 seminar, which was lost along with various other lectures from the same seminar. The most interesting seems to me to be a variant for constructible discrete coefficients,

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also, except for the little piece unearthed two years ago, with an alternate authorship (see notes n◦ s 51, 52, 59). (May 13)

[16](**) (May 13) I understand now that the "very few insiders" were reduced to the one and only Deligne until 1982. It's true that he revealed this "secret science" through certain important results included in this yoga, revealed as and when he was able to prove them, in order to take credit for them while concealing his source of inspiration, which remained secret. And yet, for fifteen years, no one has come up with a far-reaching theory of patterns, because our times are decidedly far from the bold dynamism of the heroic age of infi nitesimal calculus!

[17](*) (May 13) Having familiarized myself with the bibliography to some extent. I now see that Deligne's entire work is rooted in this yoga. And my bibliographical sampling (as well as other cross-checking) leads me to assume that in Deligne's entire work, the only reference to this source is found in a lapidary line (quoting me in a haleine with Serre) in "Hodge Theory I" in 1970. (See notes n◦ s 78_1 and 78_2 .)

[18](**) What I've got from Serre (early 60s?) is an initial idea or intuition, making me realize that there was something important to understand! This acted as an initial impulse, triggering a process of reflection that continued over the following years, first on a "yoga" of weights and soon on a broader yoga of patterns.

[19](*) (April 10) It seems to me that Deligne was the only one to "hear" - and he was careful to reserve the exclusive privilege of what he heard. It's also true that in writing these final lines, I was "delaying" events: two years ago, there was a partial exhumation of the Yoga des motifs, without any hint of a role I'd played in it! See notes n◦ s 50, 51, 59, prompted by an unforeseen discovery that shed unexpected light (for me at least) on the meaning of the burial that had been going on for twelve years. Until then, I had been vaguely aware of a sort of burial, without taking the time to look more closely. ...

which I don't know whether it has since been made explicit in the literature[20] (**). Note that this also admits a "motivic" variant, which essentially amounts to asserting that the "characteristic classes" (in the Chow ring of a regular scheme Y) associated with constructible χ-adic bundles for different prime numbers χ (prime to residual characteristics), when these bundles come from the same "word "pattern" (e.g. are $R f^{i}_{!}(Z_{\chi})$ for a given $f : X \rightarrow Y$) are all equal.

Note 46_2 This formalism can be seen as a kind of quintessential "**global duality**" formalism in cohomology; in its most "efficient" form, freed of all superfluous assumptions (of smoothness in particular for the "spaces" and applications considered, or of cleanliness for the morphisms' There is a need to to complement it with a **local duality** formalism, in which we distinguish among the "coefficients" admitted p. 187 the objects or "complexes "□ said to be "**dualizing**" (a notion made stable by the operation $Lf^{!}$ "), i.e. those giving rise to a "**biduality theorem**" (in terms of the RHom operation) for coefficients satisfying suitable finiteness conditions (on degrees, and consistency or "constructability" on local cohomology objects). When I speak of the "six-variance formalism"; I hereafter imply this complete duality formalism, both in its "local" and "global" aspects.

A first step towards a deeper understanding of duality in cohomology was the progressive discovery of the six-variance formalism in a first important case, that of Noetherian schemes and cohomology-coherent complexes of modules. A second was the discovery (in the context of spread cohomology of schemes) that this formalism also applied for discrete coefficients. These two extreme cases were sufficient to establish the conviction that this formalism was **ubiquitous** in all geometric situations giving rise to a Poincaré-type "duality" - a conviction that was confirmed by the work (among others) of Verdier, Ramis and Ruget. This conviction will undoubtedly be confirmed for other types of coefficients, once the fifteen-year **blockade** against the development and widespread use of this formalism has been broken down.

This ubiquity seems to me a **fact of** considerable significance. It made it imperative to feel a profound unity between Poincaré's duality and Serre's duality, which was finally established with the required generality by Mebkhout. This ubiquity makes the "six-variance formalism" one of the fundamental structures in homological algebra for an understanding of cohomological duality phenomena "tous azimuths"[21] (*). The fact that this rather sophisticated structure has not been made explicit in the past (any more than the "right" notion of "triangulated category", of which the Verdier version is still a very provisional and inadequate form) doesn't change a thing; nor does the fact that topologists, and even algebraic geometers who pretend to be interested in cohomology, continue to ignore the very existence of the duality formalism, as well as the language of derived categories on which it is based.

Note 46_3 The point of view of D-Modules and complexes of differential operators was introduced by Sato and developed first by him and his school, in a rather different op□tic (it seemed to me to understand)
p. 188 Mebkhout's approach, which is closer to my own.

The various notions of "**constructibility**" for "discrete" coefficients (in the analytic-complex, analytic-real, piecewise linear contexts) were first teased out by me, it seems, in the late 1950s (and I took them up again a few years later in the context of staggered cohomo- logy). At the time, I raised the question of the stability of this notion of higher direct images for

[20](**) (June 6) I found it again (in a similar form, and under the flattering name of "Deligne-Grothendieck conjecture") in an article by Mac-Pherson published in 1974. For details, see note n◦ 87_1 .

[21](*) The interested reader will find an outline of this formalism in the Appendix to this volume.

a proper morphism of real or complex analytic spaces, and does not know whether this stability has been established in the complex analytic case[22] (*). In the real analytic case, the notion I had envisaged was in fact the wrong one, for want of Hironaka's notion of a real subanalytic set, which possesses the essential liminal property of stability by direct images. As for operations of a local nature such as RHom, it was clear that the argument establishing the stability of constructible coefficients in the framework of excellent schemes of zero characteristic (using the resolution of Hironaka singularities) worked as is in the complex analytic case, and likewise for the biduality theorem (see SGA 5 I). In the piecewise linear framework, natural stabilities and the biduality theorem are "easy exercises", which I had enjoyed doing as a check on the "ubiquity" of the duality formalism, when starting up staggered cohomology (one of whose main surprises had been precisely the discovery of this ubiquity).

Returning to the semi-analytical case, the "right" framework in this direction for stabi- lity theorems (coefficients constructible by the six operations) is obviously that of "moderated spaces" (see Esquisse d'un Programme, par. 5, 6).

Note 46_4 Of course, the D-Modules point of view, combined with the fact that *D* is a coherent ring bundle, brings to light a more hidden notion of "coherence" for moduli crystals than the one I used to work with, and which still makes sense on spaces (analytic or schematic) that are not necessarily smooth. It would only be fair to call it "**M-coherence**" (M as in Mebkhout). It should be

It is therefore obvious to anyone with a clue (and in full possession of their healthy mathematical instincts) that the "right category of coefficients" which generalizes the complexes "of differential operators" p. 189

in the smooth case, must be none other than the "M-coherent" derived category of that of moduli crystals (a crystal complex being called **M-coherent** if its cohomology objects are). This makes reasonable sense without smoothness assumptions, and should encompass both the theory of ordinary "continuous" (coherent) coefficients, and that of "constructible" discrete coefficients (introducing suitable holonomy and regularity assumptions for the latter). If my vision of things is correct, the two new conceptual ingredients of Sato-Mebkhout's theory, compared with the previously known crystalline context, are this notion of M-coherence for moduli crystals, and the holonomy and regularity conditions (of a deeper nature) concerning M-coherent complexes of crystals. With these notions acquired, a first essential task would be to develop the six-variance formalism in the crystalline context, so as to encompass the two special cases (ordinary coherent, discrete) I had developed over twenty years ago (and which some of my ex-cohomology students have long since forgotten in favor of arguably more important tasks... .).

Mebkhout had eventually learned of the existence of the notion of "crystal" by frequenting my writings, and he felt that his point of view should give a good approach to this notion (at least in zero characteristic) - but this suggestion fell on deaf ears. Psychologically, it was hardly conceivable that he would embark on the vast task of laying the foundations, placed as he was in a climate of haughty indifference on the part of the very people who were cohomolo- gical authorities, and best placed to encourage - or discourage. ...

Note 46_5 (May 13) The focus here is on topos ringed by a **local commutative** ring. The idea of describing a "variety" structure in terms of the datum of such a ring bundle on a topological space, a

[22](*) (May 25) Established by J.L. Verdier, see "Les bonnes références" note n° 82.

was first introduced by H. Cartan, and was taken up by Serre in his classic work FAC (Faisceaux algébriques cohérents). It was this work that provided the initial impetus for a reflection that led me to the notion of "schema". What was still missing in Cartan's approach, taken up by Serre, to encompass all the types of "spaces" or "varieties" that have arisen to date, was the notion of topos (i.e. precisely "something" on which the notion of "bundle of sets" makes sense, and possesses the familiar properties).

p. 190 **Note** 46_6 As other remarkable examples of topos that are not ordinary spaces, and for the-

As there doesn't seem to be a satisfactory substitute in terms of "accepted" notions either, I'd like to point out the following: quotient topos of a topological space by a local equivalence relation (e.g. manifolds of varieties, in which case the quotient topos is even a "multiplicity", i.e. is locally a variety); "classifying" topos for just about any kind of mathematical structure (at least those "ex- priming" in terms of finite projective limits).e. is locally a variety); "classifying" topos for just about any kind of mathematical structure (at least those "ex- priming in terms of finite projective limits and inductive limits of any kind"). When we take a "variety" structure (topological, differentiable, real or complex analytic, Nash, etc. . . or even smooth schematic on a given basis) we find in each case a particularly attractive topos, which deserves the name of "universal variety" (of the species under consideration). Its homotopic invariants (and in particular its cohomology, which deserves the name of "classifying cohomology" for the species of variety under consideration) should have been studied and known long ago, but for the moment it's taking no such course. ...

Note 46_7 These are spaces X whose homotopy type is "naturally" described as that of a complex algebraic variety. The latter can then be defined on a subbody K of the complex field, such that K is a finite-type extension of the prime field Q. The profinite Galois group $Gal(\overline{K}/K)$ operates then naturally on the homotopic invariants profinis of X. Often (e.g. when X is an odd-dimensional homotopic sphere) we can take the prime field Q for K.

Note 46_8 (May 13) When I learned my first rudiments of algebraic geometry from Serre's FAC article (which was to "trigger" me in the direction of schemas), the very notion of base change was virtually unknown in algebraic geometry, except in the special case of base body change. With the introduction of the language of schemes, this operation has become probably the most commonly used in algebraic geometry, where it can be introduced at any time. The fact that this operation is still virtually unknown in topology, except in very special cases, strikes me as a typical sign (among many others) of topology's isolation from the ideas and techniques of algebraic geometry, and a tenacious legacy of the inadequate foundations of "geometric" topology.

p. 191 **Note** 46_9 (June 5) Serre's idea was that it should be possible to associate any scheme X of finite type on a body K, integers

$$h^i(X) \ (i \in N)$$

which he calls his "virtual Betti numbers", so that we have :

a) for Y a closed subschema and U the complementary open

$$h^i(X) = h^i(Y) + h^j(U)$$

b) for smooth projective X, we have

$$h(X) = \text{i.th Betti number of } X$$

(defined, for example, via χ-*adic* cohomology, for χ prime to the characteristic of k). If we admit singularity resolution for algebraic schemes over k, then it's immediate that the $h^i(X)$ are uniquely determined by these properties. The **existence of** such a function $X \longrightarrow (h^i(X))_{i\in\mathbb{N}}$ for a fixed k, using the formalism of cohomology with proper support, can essentially be reduced to the case of where the base field is finite. Working in the "Grothendieck group" of finite-dimensional vectors on $\mathbb{Q}_\chi$ on which Gal($\bar{k}$) operates continuously, and taking the χ-*adic* Euler-Poincaré characteristic (with proper support) of X in this group, $h^i(X)$ then designates the virtual rank of the "weight component *i*" of EP(X, $\mathbb{Q}_\chi$), where the notion of weight is that deduced from Weil's conjectures, plus a weak form of singularity resolution. Even without resolution, Serre's idea is realized thanks to the strong form of Weil's conjectures (established by Deligne in "Weil's Conjectures II").

I pursued heuristic reflections along these lines, leading me to a six-operation formalism for "virtual relative schemes", with the base body k replaced by a more or less arbitrary base scheme S - and to various notions of "characteristic classes" for such virtual schemes (of finite presen- tation) on S. Thus, I was led (to simplify matters by returning to the case of a basic body) to consider integer numerical invariants finer than those of Serre, denoted $h^{p,q}(X)$, satisfying properties analogous to a), b) above, and giving back the virtual Betti numbers of Serre by the usual formula

$$h^i(X) = \sum_{p+q=i} h^{p,q}(X)$$

13.2.2. Refusing an inheritance - or the price of a contradiction

Note 47 [This note is a direct continuation of note 46 p.13.2.1]

It should be noted that four of the five notions I have just reviewed (the ones that pass p. 192 for "not serious" things) concern cohomology, and above all, **the cohomology of algebraic schemes and varieties**. In any case, all four were suggested to me by the needs of a cohomological theory of algebraic varieties, first for continuous coefficients, then for discrete ones. In other words, the cohomology of algebraic varieties was my main motivation and a constant leitmotif in my work over the fifteen years from 1955 to 1970.

Remarkably, this is also the theme that Deligne still considers to be his main source of inspiration, if I am to believe what is said on the subject in last year's IHES brochure[23] (*). I read this with some astonishment. Admittedly, I was still "on the scene" and all that, when Deligne (after his fine work on the Ramanuyam conjecture) developed his remarkable extension of Hodge's theory. Above all, for him as for me, it was a first step towards a formal construction of the notion of pattern on the field of complexes - for a start! In the early years after my "turning point" in 1970, I of course also heard of Deligne's proof of Weil's conjectures (which also proved Ramanuyam's conjecture), and in the wake of this, of the "Lefschetz cow theorem" in positive characteristic. I expected nothing less from him! I was even sure that he would

[23](*) (May 12) On the other hand, I've just noticed that nothing in the aforementioned brochure could lead the reader to suspect that my work has anything to do with the cohomology of algebraic varieties, or that of anything else! On this subject, see the note "L'Eloge Funèbre (1) - ou les compliments" (n° 98) written today. The brochure referred to is the one mentioned in the footnote to the note "L'arrachement salutaire", n° 42, and examined a little more closely in the note "L'Eloge Funèbre" mentioned above.

to have proved at the same time the "**standard conjectures**", which I had proposed towards the end of the sixties as a first step towards founding (at least) the notion of a "semisimple" pattern over a body, and translating some of the expected properties of such patterns in terms of χ-adic cohomology properties and algebraic cycle groups. Deligne told me afterwards that his proof of Weil's conjectures would certainly not make it possible to prove the standard (stronger) conjectures, and that he had in fact no how to approach them. That must be about ten years ago now. Since then, I'm not aware of any other really decisive progress that has taken place in understanding the as□pects

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"motivic" (or "arithmetic") cohomology of algebraic varieties. Knowing Deligne's means, I had tacitly concluded that his main interest must have turned to other subjects - hence my astonishment to read that this was not the case.

What seems to me beyond doubt is that, for the last twenty years, it has scarcely been possible to make a major breakthrough in our understanding of the cohomology of algebraic varieties, without also appearing, more or less, as a "Grothendieck follower". Zoghman Mebkhout learned this the hard way, and (to a certain extent) so did Carlos Contou-Carrère, who soon realized that it was in his interest to change his subject (47_1). One of the very first things that cannot be avoided is the development of the famous "six-variance formalism" in contexts of various coefficients, as close as possible to that of patterns (which, for the moment, play the role of a kind of ideal "horizon line"): crystalline coefficients in zero characteristic (in the tradition of the Sato school and Mebkhout, Grothendieck sauce) or *p* (studied above all by Berthelot, Katz, Messing and a whole group of visibly motivated younger researchers), "stratified promodules" à la Deligne, (which appear as a dualized variant, or "pro", of the "ind"-notion of coherent D-module, or D-coherent crystal'), and finally "Hodge-Deligne" coefficients (which seem just as good as motifs, except that their definition is transcendental and restricted to basic schemes of finite type over the field of complexes). . . At the other end of the spectrum is the task of clearing the very notion of pattern from the mists that surround it (and for good reason. . .), and also, if at all possible, tackling such precise questions as "standard conjectures". (For the latter, I had been thinking, among other things, of developing a theory of "intermediate Jacobians" for projective and smooth varieties over a body, as a means perhaps of obtaining the trace positivity formula, which was one of the essential ingredients of the standard conjectures.)

These were tasks and questions that burned in my hands right up to the moment when I "left maths" - burning, juicy things, none of which at any time appeared to me to form a "wall", a stopping point[24] (*). They were an inexhaustible source of inspiration and sub□stance.

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sables something where all you had to do was pull where it stuck out (and it "stuck out" everywhere!) and something would come along, the expected as well as the unexpected. With my limited means, but without being divided in my work, I know just how much can be achieved if you put your mind to it, in a single day, or in a year, or in ten. And I also know, having seen him at work at a time when he was not divided in his work, what Deligne's means are, and what he can do in a day, a week, or a month, when he puts his mind to it. But no one, not even Deligne, can, in the long run, do fruitful work, work of profound renewal, while looking down on the very objects that it is basically a question of probing, as well as the language and a whole arsenal of tools that have been developed for this purpose by such and such a predecessor (and with his assistance what's more, among many others who have put their hand to the dough. . .).) (59).

I'm also thinking of the "Deligne-Mumford" compactification of the modular multiplicity $M_{g,\nu}$ (on Spec Z),

[24](May 25) Yet this is what was kindly suggested in that famous jubilee brochure, by an anonymous pen I think I recognize. On this subject, see the note "L'Eloge Funèbre (2)". which follows "L'Eloge Funèbre (1)" quoted in the previous b. de p. note.

for connected smooth algebraic curves of genus g with v marked points. They were introduced[25] (*) on the occasion of the problem of proving the connectedness of modular spaces $M_{g,v}$ in any characteristic, by a specialization argument from characteristic zero. These objects $M_{g,v}$ seem to me (along with the group $Sl(2)$) the most beautiful, the most fascinating I've encountered in mathematics (47_2). Their very existence, with such perfect properties, seems to me a kind of miracle (perfectly well understood, moreover), of incomparably greater scope than the fact of connectedness that they were intended to demonstrate. For me, they contain the quintessence of what is most essential in algebraic geometry, namely the totality (more or less) of all algebraic curves (over all conceivable basic bodies), which are precisely the ultimate building blocks of all other varieties.

algebraic. But the kind of objects we're talking about, "smooth proper multiplicities on Spec(Z)", eludes still to the "accepted" categories, i.e. those we are **willing** (for reasons we don't care to examine) to "admit". The average person speaks of them by allusion at most, and with an air of apology for appearing to be still making "general nonsense", while we are certainly careful to say "stack" or "field", so as not to utter the taboo word of "topos" or "multiplicity". This is the reason without
no doubt why these unique gems have not been studied or used (as far as I know) since
their introduction over ten years ago, except by myself in seminar notes rest unpublished. At p. 195
Instead, we continue to work either with "coarse" varieties of modules, or with finite coverings of modular multiplicities that have the appearance of being real schemes - both of which, however, are only relatively fallible and lame shadows of those perfect gems from which they originate, and which remain practically banished. ...

Deligne's four works on the Ramanuyam conjecture, on mixed Hodge structures, on the compactification of modular multiplicities (in collaboration with Mumford), and on Weil's conjectures, each constitute a renewal of our knowledge of algebraic varieties, and thus a new starting point. These fundamental works follow each other within a space of a few years (1968-73). For nearly ten years, however, these major milestones have not been the springboards for a new launch into the glimpsed and the unknown, or the means for a more far-reaching renewal. Instead, they have resulted in a morose stagnation (47_3). It's certainly not that the "means" that were there ten years ago, on the part of some and others, have magically disappeared; nor that the beauty of things at our fingertips has suddenly vanished. But it's not enough for the world to be beautiful - we must also deign to rejoice in it. ...

Note 47_1 I'm thinking here of Contou-Carrère's promising start, five or six years ago, on a theory of relative local Jacobians, their links with global Jacobians (known as "generalized Jacobians") for smooth curve schemes and not necessarily proper on any scheme, and with Cartier's theory of commutative formal groups and typical curves. Apart from an encouraging reaction from Cartier, the reception of Contou-Carrère's first note, by those best placed to appreciate it, was so cool that the author refrained from ever publishing the second, which he kept in reserve, and hastened to change subject (without, however, avoiding further mishaps)[26] (*).

I had suggested to him the theme of local and global jacobiennes, as a first step towards a program of local and global jacobiennes.
which dates back to the late 1950s, and which is notably oriented towards a theory of a dualistic "adequate" complex.
lique" in any dimension, formed with local Jacobians (for local rings of dimension p. 196
arbitrary), in analogy with the residual complex of a noetherian scheme (formed with the dualizing modules

[25](*) In Pub. Math. 36, 1969, pp. 75-110. See comments in note n◦ 63_1

[26](*) (June 8) See the sub-note (95_1) to the note "Cercueil 3 - ou les jacobiennes un peu trop relatives", n◦ 95.

of all its local rings). This part of my cohomological duality program found itself (along with others) somewhat relegated to oblivion, during the sixties, due to the influx of other tasks which then appeared more urgent.

Note 47_2 In truth, it is the "Teichmüller tower" into which the family of all these multiplicities fits, and the discrete or profinite paradigm of this tower in terms of fundamental groupoids, that constitutes the richest, most fascinating single object I have encountered in mathematics. The group $S\chi(2, \underline{Z})$, with the "arithmetic" structure of the profinite compactivity of $S\chi(2, Z)$ (consisting in the operation of the Galois group $\mathrm{Gal}(\overline{\mathbb{Q}}/\mathbb{Q})$ on it), can be considered as the main building stone for the "profinite version" of this tower. On this subject, see the indications in "Esquisse d'un Programme" (pending the volume(s) of Réflexions Mathématiques that will be devoted to this theme).

Note 47_3 This observation of "morose stagnation" is not the considered opinion of someone who is well acquainted with the main episodes in the last ten years concerning the cohomolo- gy of algebraic schemes and varieties. This is simply the overall **impression** of an "outsider", which I got from conversations and correspondence with Illusie, Verdier and Mebkhout, among others, in 1982 and 1983. This impression could surely be qualified in many ways. For example, Deligne's work "Conjectures de Weil II", published in 1980, represents substantial new progress, if not a surprise in terms of the main result. It seems that there has also been progress in crystalline cohomology of car. $p > 0$, not to mention the "rush" around intersection cohomology, which has ended up making some people (unwillingly) return to the language of derived categories, even making them remember long-repudiated paternities. ...

13.3. III La Mode - ou la Vie des Hommes illustres

13.3.1. Instinct and fashion - or the law of the strongest

Note 48 [This note is called by note 46 p. 265]

p. 197 As is well known, the theory of derived categories is due to J.L. Verdier. Before he undertook the foundation work I had proposed, I had confined myself to working with derived categories in a heuristic way, with a provisional definition of these categories (which later turned out to be the right one), and with an equally provisional intuition of their essential internal structure (an intuition that turned out to be technically wrong in the intended context, as the "cone mapping" does **not** depend fonctorially on the arrow in a derived category that is supposed to define it, and which defines it only to non-unique isomorphism). The theory of the duality of coherent beams (i.e. the "six variances" formalism in the coherent framework), which I had developed towards the end of the fifties[27] (*), only made sense as a module in the foundations of the notion of derived category, as Verdier subsequently did.

The text of Verdier's thesis (passed only in 1967), some twenty pages long, seems to me the best introduction to the language of derived categories written to date, placing this language in the context of its essential uses (many of which are due to Verdier himself). It was only the introduction to a work in progress, which ended up being written later. I can pride myself on being, if not the only one, at least one of the very few people who can testify to having held this work in their hands,

[27](*) It still lacked an Rf operation$_!$ (cohomology with proper support) for a non proper morphism, which was introduced six or seven years later by Deligne, thanks to his introduction of the context of coherent promodules, which seems to me to be an important new idea (successfully taken up in his theory of stratified promodules).

which is supposed to establish the validity of the title of Doctor of Science awarded to its author on the basis of the introduction alone! This work is (or was - I don't know if a copy still exists somewhere... .) the only text, to date, that presents systematic foundations of homological algebra from the point of view of derived categories.

□Perhaps I'm the only one to regret that neither the introductory text nor the foundations themselves have been p. 198 published[28] (*), so that the technical baggage essential for using the language of derived categories is scattered in three different places in the literature[29] (**). This absence of a systematic reference text of comparable weight to the classic Cartan-Eilenberg book seems to me both a **cause and a** typical **sign** of the disaffection that struck the formalism of derived categories after my departure from the mathematical scene in 1970.

It's true that as early as 1968, it had become clear (in connection with the needs of a cohomological theory of traces, developed in SGA 5) that the notion of derived category in its primitive form, and the corresponding notion of triangulated category, were insufficient for certain needs, and that further groundwork remained to be done. A useful but still modest step in this direction was taken (mainly for the purposes of the trace cause) by Illusie, with the introduction in his thesis of "filtered derived categories". It would seem that my departure in 1970 was the signal for a sudden and definitive halt to any re-flexion on the foundations of homological algebra, as well as on those, intimately linked, of a theory of patterns (48_1). However, as far as the former were concerned, all the essential ideas for en- vergure foundations seemed to have been acquired in the years before my departure (48_2). (Including the key idea of the "derivator", or "machine for making derived categories", which seems to be the common richer object underlying the triangulated categories we've encountered so far, an idea that would eventually be developed to some extent in a non-additive framework, almost twenty years later, in a chapter in volume 2 of The Pursuit of Fields). What's more, much of the groundwork to be done had already been laid by Verdier, Hartshorne, Deligne and Illusie, work that could be used as it stood for a synthesis taking up the ideas acquired in the broader perspective of derivation.

□It's true that this disaffection in the past fifteen years[30] (*) for the very notion of category p. 199
This derivative approach, which for some is akin to a disavowal of the past, is in line with a certain fashion, which tends to look with disdain on any fundamental reflection, however urgent[31] (**). On the other hand, it's quite clear to me that the development of staggered cohomology, which "everyone" uses today without looking twice (if only implicitly via Weil's conjectures. . .), could not have taken place without the conceptual baggage represented by the derived categories, the six operations, and the language of sites and topos (first developed precisely for this purpose), not to mention SGA 1 and SGA 2. And it's just as clear that the stagnation we can see today in the cohomological theory of algebraic varieties could not have arisen, let alone taken root, if some of my students had known how to follow their healthy mathematical instincts during those years, rather than a fashion they were among the first to introduce, and which has long since become law with their support.

[28](* (May 25) After these lines were written, I discovered that the first embryo of Verdier's thesis, dating from 1963 (four years before the defense), was published in 1967. See notes "Le compère" and "Thèse à crédit et assurance tous risques", n◦ 63"'and 81.

[29](** These places are: Hartshorne's well-known seminar on coherent duality, containing the only part published to date of the duality theory I had developed in the second half of the '50s; one or two Deligne papers in SGA 4; one or two chapters of Illusie's voluminous thesis.

[30](*) (May 24) these "fifteen years" should be qualified - see note no.◦ 47_3 , as well as the more detailed note "Thèse à crédit et assurance tous risques", no.◦ 81.

[31](**) (May 25) For a reflection on the forces at work in the emergence and persistence of this fashion, see the note "Le Fossoyeur - ou la Congrégation toute entière", n◦ 97.

Note 48_1 The same can be said (with certain reservations) of my entire program on the foundations of algebraic geometry, of which only a small part was completed: it came to a screeching halt with my departure. The stoppage hit me particularly hard in the duality program, which I considered particularly juicy. Zoghman Mebkhout's work, pursued against all odds, is nevertheless in line with this program (renewed by the contribution of unforeseen ideas). The same is true of Carlos Contou-Carrère's 1976 work (mentioned in note (47_1) p. 273) - which he prudently suspended sine die. There was also work on duality in the fppf cohomology of surfaces (Milne). That's all I know about it.

It's true that I never thought of writing an outline of the long-term work program that I had set myself.
for me in the years between 1955 and 1970, as I did for the last twelve years, with Esquisse d'un Programme. The reason for this, I believe, is simply that there was never an opportunity to do so.
sion (like now my application to join the CNRS) to motivate such exposure work.

p. 200 The letters to Larry Breen (from 1975), reproduced in the appendix to Chap I of the History of Models (Mathematical Reflections 2), give some indication of certain theories (notably duality) on my pre-1970 agenda, theories that are still waiting for arms to enter the common heritage.

Note 48_2 The same is true for the theory of motives, except that it is likely to remain conjectural for some time to come.

13.3.2. The service stranger and the good God theorem

Note $48^{'}$ [This note is called by note 46 p.]

While it is customary to call the key theorems of a theory by the names of those who have done the work of identifying and establishing them, it would seem that Zoghman Mebkhout's name was deemed unworthy of this fundamental theorem, the culmination of four years of obstinate and solitary work (1975-79), against the fashion of the day and the disdain of his elders. The latter, on the day when the theorem's significance could no longer be ignored, took pleasure in calling it the "Riemann-Hilbert theorem", and I trust (although neither Riemann nor Hilbert would surely have asked for so much. . .) that they had excellent reasons for doing so. After all (once the feeling of a need - that of an understanding of the precise relationships between general discrete coefficients and continuous coefficients, appeared against the general indifference, that it was refined and specified by delicate and patient work, that, after successive stages, the right statement was finally found, that it was written down in black and white and proved, and when finally this theorem, the fruit of solitude, proved itself where it was least expected - after all that) this theorem appears so obvious (not to say "trivial", for those who "would have known how to prove it" . .) that there's really no point in burdening our memory with the name of a vague stranger on duty!

Encouraged by this precedent, I propose to call any theorem "Adam and Eve's theorem".
fundamental to a theory, or even to go back even further and give honor where honor is due, by simply calling it "**God's theorem**"[32] (*).

As far as I know, apart from myself, Deligne was the only one before Mebkhout to feel the interest he

p. 201 There was a need to understand the relationships between discrete coefficients and continuous coefficients in a framework broader than that of stratified modules, so as to be able to interpret any "construc- tible" coefficients in "continuous" terms. The first attempt in this direction was the subject of a seminar (still unpublished) by De-

[32](*) In my life as a mathematician, I've never had the pleasure of inspiring, or even encouraging, a student to write a thesis containing a "God's theorem" - at least not one of comparable depth and scope.

line at the IHES in 1968 or '69, where he introduces the "stratified promodule" point of view and gives a comparison theorem (over the field of complexes) for transcendental discrete cohomology and the associated De Rham-type cohomology, which still makes sense for schemes of finite type, over any base field of zero square. (Apparently, he was still unaware at that time of the remarkable result of his distant predecessors Riemann and Hilbert... .) Even more than Verdier[33] (*) or Berthelot[34] (**), Deligne was in a particularly good position to appreciate the interest of the direction in which Mebkhout's research was heading in 1975, and subsequently the interest of Mebkhout's results, in particular the "theorem of the good God", which gives a more delicate and deeper apprehension of discrete coefficients in terms of continuous coeffi- cients, than the one he himself had worked out. However, this did not prevent Mebkhout from continuing his work in painful moral isolation, and the credit he deserves (all the more so, I would say) for his pioneering work remains unacknowledged even today, five years after[35] (***).

13.3.3. Canned weight and twelve years of secret

Note 49 [This note is called by note 46 p.]

Verification done (in Publications Mathématiques 35, 1968), I note that towards the end of the article "Théo- p . 202 rème de Lefschetz et critères de dégénérescence de suites spectrales", three lines allude to "weighty considerations" which had led me to conjecture (in a slightly less general form) the main re-sult of the work. I doubt that this sybilline allusion could have been useful to anyone, nor understood at the time by anyone other than Serre or myself, who were already aware of it anyway[36] (*).

In this connection, I'd like to point out that a very precise "yoga of weights", including the behavior of weights for operations such as $R f^i_÷$ and $R f^i_!$, was well known to me (and therefore to Deligne) as early as the late sixties, in the wake of Weil's conjectures. Some of this yoga is finally established (in the context of *l-adic* coefficient bundles, until it is in the more natural setting of motives) in Deligne's work "Conjectures de Weil II" (Publications Mathématiques 1980). Unless I'm mistaken, in the twelve years or so between the two moments[37] (**), there has been no trace in the literature of an exposé, however succinct and partial, of the yoga of weights (still entirely conjectural), which for all that time remained the exclusive privilege of a few (two or three?) initiates[38] (***). However, this yoga constitutes an essential first key to understanding the properties of weight.

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[33](*) It would seem that Verdier, as official thesis supervisor for Zoghman Mebkhout's thesis (and who in this capacity even "granted him some discussions"), was the main person involved (apart from Mebkhout himself) in the concealment that took place around the authorship of this fundamental theorem, and of the credit due to his "pupil" in the renewal of the cohomological theory of algebraic varieties by the point of view of *D* -Modules developed by Mebkhout. However, I'm not aware that he was more moved by this than Deligne.

[34](**) (May 25) In writing these lines, I refrained (with some hesitation) from including the name of my friend Luc Illusie in this list of my students who would have been "best placed" to give Zoghman Mebkhout the encouragement that should have been self-evident. I didn't notice a certain uneasiness within me, which might have taught me that I was giving a helping hand to someone I care about, in order to relieve him of a responsibility that falls to him and to my other "cohomology students".

[35](***) (May 25) In fact, Deligne and Verdier themselves were the first to do this. On this subject, see the note "L'Iniquité - ou le sens d'un retour", n◦ 75.

[36](*) (April 29) For a closer look at this article, which is instructive in more ways than one, see the note "Eviction" (n◦ 63).

[37](**) (April 19) I note on a list of Deligne's publications that I have just received and read with interest, that it mentions of "weights" as early as 1974, in a paper presented by Deligne at the Vancouver Congress - that's six years of "secrecy around weights" instead of twelve. Yet this secret seems to me to be inseparable from the similar secret surrounding the motifs (during the twelve years 1970-1982). The meaning of this secrecy has just become clearer in the course of today's reflection, in the long double-note that follows n◦ 51-52).

[38](***) (May 25) It would seem, from all the information that has come to light in the course of our discussions, that these "two or more three insiders" are limited to the one and only Deligne, who seems to have taken great care to reserve for himself the exclusive benefit of the

"In other words, it was a **way of** recognizing oneself in a given situation and of making predictions with a reliability that had never been seen before,

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At the same time, it represented one of the most urgent and fascinating **tasks** facing the cohomological theory of algebraic varieties. The fact that this yoga remained virtually ignored until it was finally established (in certain important aspects at least), seems to me a particularly striking example of **the information-blocking** role often played by the very people whose privileged position and functions are supposed to ensure its wide dissemination[39] (*).

13.3.4. You can't stop progress !

Note 50 [This note is called by section 50 of chapter **VIII The solitary adventure** of part **(I) Fatuity and Renewal** p.]

My first experiences in this direction were the unexpected fruits of my unsuccessful efforts to get Yves Ladegaillerie's thesis on isotropy theorems on surfaces published - a work as good as any of the eleven state doctorate works ("pre-1970", it's true!) for which I had been the "boss". As I recall, these efforts continued for a good year or more, and involved many of my former friends (not to mention one of my former students, as it happens)[40] (**). The main episodes still seem like vaudeville to me today!

It was also my first encounter with a certain new spirit and mores (now commonplace in the circle of my old friends), which I've already alluded to here and there in the course of my reflection. It was during that year (1976) that I learned for the first time, but not for the last, that it is nowadays considered unserious (at least on the part of the first-timer. . .) to actually demonstrate delicate things that everyone uses and that I'm sure you'll agree with me.

predecessors were always content to admit (in this case, the non-existence of wild phenomena in surface topology)[41] (***). Or to demonstrate a result that encompasses as special cases

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or corollaries of several well-known deep theorems (which obviously shows that the so-called

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possession of this yoga, which he had inherited from me, until 1974 (see previous p. b. note)) when the time was ripe to be able to present it as his own ideas, without any reference to me or Serre (see notes n° s 78_1, 78_2.

(April 18, 1985) Since these lines were written, I have also had occasion to become acquainted with Deligne's paper "Théorie de Hodge I" at the Congrès Int. Math, Nice (1970) (Actes, t.1, p. 425-430). Contrary to what I had reason to believe from the fragmentary information in my possession, this article sets out a substantial part of weight yoga as early as 1970. As to the origin of these ideas, it confines itself to a sibylline and perfunctory mention of an article by Serre (a foreigner, by the way).

to the question), and "Grothendieck's conjectural theory of motives". (Compare notes n° s $78_1'$, $78_2'$.) The question

of the behavior of the notion of weight by operations such as $R f^i_!$ and $R f^i_*$ is not even mentioned, nor is it important.

will not be until the quoted article "Weil's Conjecture II" from 1980, where my name is not mentioned in connection with the theorem.

of this work, any more than is Serre's or mine in the paper "Weights in the cohomology of algebraic varieties" mentioned in the previous b. de p. note (from a year ago to the day).

[39](*) On this subject, see also sections 32 and 33, "The mathematician's ethic" and "The note - or the new ethic (1)", as well as the two related notes, "Deontological consensus and information control" and "Youth snobbery, or the defenders of purity", n° s 25,27.

[40](**) On this subject, see the note "Coffin 2 - or cut to length", n° 94.

[41](***) See also the episode "The note - or the new ethic" (section 33). This famous "note" had precisely the wrong idea.

to make explicit notions and statements that had hitherto been left in the dark, yet which I have implicitly used to establish results that bear my name and that everyone has been shamelessly using for almost twenty-five years (something, incidentally, that the two illustrious colleagues knew perfectly well). (June 8) For further details, see note "Coffin 4

- or topos without flowers or wreaths" (n° 96). The "results that bear my name" are results on the generation and fi nished presentation of certain fundamental global and local profi nis, "demonstrated" in SGA 1, among others.

by descent techniques that remain heuristic in the absence of careful theoretical justification, accomplished in Olivier Leroy's (apparently "unpublishable") work on Van Kampen-type theorems for fundamental topos groups.

new can only be a special case or an easy consequence of known results). Or to take the trouble only, when stating a result or describing a situation in terms of another, to carefully formulate the natural hypotheses (a sign of regrettable bombast), rather than limiting oneself to some case in point to the liking of the high-flying person issuing the opinion. (Just last year, I saw Contou-Carrère reproached for not having confined himself in his thesis to a basic body instead of a general scheme - while conceding the mitigating circumstance that it was surely at the insistence of his boss of circumstance that he had had to do so. The person who expressed himself in this way was, however, sufficiently familiar with the subject to know that, even if we limit ourselves to the body of complexes, the necessities of demonstration force us to introduce general basic diagrams. ...).)

The excesses of a certain fashion today go so far as to disgrace not only careful demonstrations (or even demonstrations at all), but often even formal statements and definitions. Given the price of paper and the longevity of the gorged reader, it will soon be out of the question to bother with such costly luxuries! Extrapolating from current trends, we can predict the moment when a publication will no longer need to spell out definitions or statements, but will simply name them with code words, leaving it to the indefatigable and genial reader to fill in the blanks according to his or her own insights. The referee's task will be made all the easier, as all he'll have to do is look in the "Who is Who" directory to see if the author is known to be credible (in any case, no one could contradict the blanks and dotted lines that make up the brilliant article), or if he's an unavowable unknown who will be (as is already the case today and has been for a long time) automatically ejected. ...

10. B) STONE AND MOTIFS

Contents

14.1. IV Motives (burial of a birth)

14.1.1. Souvenir d'un rêve - or the birth of motifs. . .

Note 51 [This note is called note 46 p.]

p. 205 (April 19) Since these lines (which end the note "My orphans", n° 46) were written, it is less than of a month, I've noticed that they're a bit behind the times! I've just received "Hodge Cycles, Motives and Shimura Varieties" (LN 900), by Pierre Deligne, James S. Milne, Arthur Ogus and Kuang-Yen Shih, which

Deligne was kind enough to send me, along with a list of his publications. This collection of six texts, published in 1982, represents an interesting development since 1970, with the mention of motives in the title and a presence of this notion in the text, albeit still modest, especially via the notion of "motivic Galois group". Of course, we're still a long way from the overall picture of a theory of motives, which for the past fifteen or twenty years has been awaiting the bold mathematician who will be willing to "paint it, vast enough...".

to serve as inspiration, Ariadne's thread and horizon line for one or more generations of arithmetician geometers, who will have the privilege of establishing their validity (or at any rate of discovering the final word on the reality of the motifs. . .) (53).

It's also since 1982[1] (*), it would seem, that the tide of fashion has begun to turn, more or less, towards derived categories; Zoghman Mebkhout (in a perhaps somewhat euphoric flight of fancy) already sees them on the verge of "invading all areas of mathematics". If their usefulness, which simple mathematical instinct (for a well-informed person) made quite obvious in the early sixties, is only just beginning to be recognized now, it is (it seems to me) mainly thanks to the solitary efforts of Mebkhout, who for seven years stuck to the thankless task of wiping the slate clean, with the courage of one who trusts his instinct alone, against a tyrannical fashion... ...

It's remarkable to read this first publication, which dedicates (twelve years after my departure from the mathematical scene) a modest re-entry of the notion of motif into the areopagus of accepted mathematical notions, nothing could lead the uninformed reader to suspect that my modest person was in any way associated with the birth of this long-taboo notion, and with the unfolding of a rich and precise "yoga", which (in a very fragmentary form) appears there as if it had emerged from nothing, without allusion to any paternity (51).1 p. 206

When, just three weeks ago, I wrote in a page or two about the yoga of patterns - even one of my "orphans", and one that was closer to my heart than any other - I must have been way off the mark! No doubt I was dreaming, as I seemed to recall years of gestation of a vision, tenuous and elusive at first, and growing richer and more precise as the months and years went by, in an obstinate effort to try and grasp the common "motif", the common quintessence, of which the many cohomological theories then known (54) were so many different incarnations, each speaking to us in its own language about the nature of the "motif" of which it was one of the directly tangible manifestations. No doubt I'm still dreaming, remembering the strong impression made on me by such an intuition of Serre's, who had been led to see a Galois profinite group, an object that seemed to be essentially discrete in nature (or, at least, reducible to the Galois profinite group).

tautologically to simple systems of **finite** groups), as giving rise to an immense projective system of **analytic** l-adic groups, or even **algebraic** groups on Q_l (by moving to suitable algebraic envelopes), which even had a tendency to be reductive - with the introduction of any

the arsenal of intuitions and methods (à la Lie) of analytic and algebraic groups. This construction made sense for any prime number *l*, and I felt (or I dream I felt. . .) that there was a mystery to be probed, about the relationship of these algebraic groups for different primes; that they all had to originate from the same projective system of algebraic groups on the only natural common subbody of all its bodies

the Q body, the "absolute" body of zero characteristic. And since I like to dream, I continue to dream that I remember entering this glimpsed mystery, through a work that was surely only a dream since I wasn't "demonstrating" anything; that I ended up understanding how the notion of motif provided the key to understanding this mystery - how, by the very fact of the presence of a category (here that of "smooth" patterns on a given basic scheme for example, patterns on a given basic body), p. 207 having internal structures similar to those found on the category of linear representations of an algebraic pro-group over a *k-field* (the charm of the notion of algebraic pro-group having been revealed to me previously by Serre as well), we can indeed reconstitute such a pro-group (as soon as we have a suitable "fiber functor"), and interpret the "abstract" category as the category of its

[1](*) (May 25) I delay again, this time by a year - the turning point comes in June 1981 with the Colloque de Luminy, see the note "L'Iniquité - ou le sens d'un retour", n◦ 75.

linear representations.

This approach to a "motivic Galois theory" was inspired by the approach I had found, years before, to describe the fundamental group of a topological space or scheme (or even of any topos - but now I feel I'm going to offend delicate ears that "topos don't amuse". . .), in terms of the category of spreadable coverings on the "space" under consideration, and the fiber functors on it. And the very language of "**motivic Galois groups**" (which I might just as well have called motivic "fundamental groups", the two kinds of intuition being for me the same thing, since the late fifties... .), and that of "fiber functors" (which correspond exactly to the "manifest embodiments" referred to above, i.e. to the various "cohomological theories" that apply to a given category of patterns) - this language was designed to express the profound nature of these groups, and to suggest their immediate links with Galois groups and with ordinary fundamental groups.

I still remember the pleasure and wonder, in this game with fiber functors, and with the torsors under Galois groups that make it possible to pass from one to the other by "twisting", of finding in a particularly concrete and fascinating situation the whole arsenal of notions of noncommutative cohomology developed in Giraud's book, with the sheaf of fiber functors (here above the étale topos, or better still, Q's fpqc topos - non-trivial and interesting topos if ever there was one!), with the "link" (in groups or algebraic pro-groups) that link this sheaf, and the avatars of this link, realized by various algebraic groups or pro-groups, corresponding to the various "sections" of the sheaf, i.e. the various cohomological functors. The various complex points (for example) of a scheme of zero characteristic gave rise (via the corresponding Hodge functors) to as many sections of the sheaf, and to torsors of passage from one to the other, these torsors and the pro-groups operating on them being provided with struc- remarkable algebraic-geometric patterns, expressing the specific structures of Hodge cohomology - but here□I'm anticipating another part of the motives dream. ... Those were the days when those who today make the
p. 208 mode hadn't yet declared that topos, sheaves and the like didn't amuse them and that it was therefore bullshit to talk about them (I wouldn't have minded recognizing topos and sheaves where they were. . .). And now, twelve years on, the same people are pretending to discover and teach that sheaves (or even topos) do indeed have something to do with the cohomology of algebraic varieties, or even with the periods of abelian integrals... .

I could evoke here the dream of another memory (or the memory of another dream. . .) around the dream of motifs, also born of a "strong impression" (I'm definitely in full subjectivity!) made on me by some of Serre's comments on a certain "philosophy" behind Weil's conjectures. Their translation into cohomological terms, for l-adic coefficients with variable *l*, made me suspect remarkable structures on the corresponding coho- mologies - the "filtration by weights" structure[2] (*). Surely the "motif" common to the various l-adic cohomologies was to be the ultimate support of this essential arithmetic structure, which then took on a geometric aspect, that of a remarkable structure on the geometric object "motif". To speak of "work" (when, of course, it was still a matter of guessing games, no more and no less) when it was a matter of "guessing" (with only the inner coherence of a vision being formed as a guide, using scattered elements known or conjectured here and there... .), on the specific structure of the various cohomological "avatars" of a motif, how the filtration of weights was translated[3] (**), starting with Hodge's avatar (at a time when the

[2](*) (January 24 1985) For a rectification of this distorted memory, see note no.° 164 (I4), and sub-note no.° 164_1 , giving details of the "yoga of the weights" fi liation.

[3](**) (February 28, 1985) There's a slight confusion in my mind here. It is, in fact, the fi ltration closely linked by the

Hodge-Deligne theory had not yet seen the light of day, and for good reason... . [4](***)). This allowed me (in my dream) to see Tate's conjecture on algebraic cycles (yet another "strong impression" that inspired the Dreamer in his dream of motives!) and Hodge's (55) competing in the same vast picture, and
to come up with two or three conjectures of the same kind, which I've mentioned to some people who must have forgotten them, because I've never used them.
I never heard of them again, nor of "standard conjectures". Anyway, they were just p. 209
conjectures (and unpublished ones at that. . .). One of these did not concern a particular cohomological theory, but gave a direct interpretation of the filtration of weights on the motivic cohomology of a nonsingular projective variety over a body, in terms of the geometric filtration of this variety itself by closed subsets of given codimension (the codimension playing the role of the "weight")[5] (*).

And then there was the work (I should put quotation marks on "work", but I can't get used to it!) of "guessing" the behavior of the weights through the six operations (since lost. . .). Here again, I never had the impression of inventing, but always of discovering - or rather of listening to what things were saying to me, when I took the trouble to listen to them with pen in hand. What they said had a peremptory precision that could not be mistaken.

Then there was a third "pattern-dream", which was like the marriage of the two previous dreams - when it came to interpreting, in terms of structures on motivic Galois groups and on the torsors under these groups that serve to "twist" a fiber functor to obtain (canonically) any other fiber functor[6] (**), the various additional structures with which the category of patterns is equipped, one of the very first of which is precisely that of filtration by weights. I seem to remember that there was never any question of riddles, but rather of mathematical translations in due form. These were all brand-new "exercises" on linear representations of algebraic groups, which I enjoyed doing for days and weeks on end, feeling that I was getting closer and closer to a mystery that had fascinated me for years! Perhaps the most subtle notion I had to grasp and formulate in terms of representations was that of the "polarization" of a motif, taking my inspiration from Hodge's theory and trying to decant what still made sense in the motivic context. This was a reflection
which must have been around the time of my reflection on a formulation of the "standard conjectures", inspired one and the other by Serre's idea (still him!) of a "Kählerian" analogue of Weil's conjectures. In such a p. 210
situation, when things themselves tell us what their hidden nature is, and by what means we can most delicately and faithfully express it, and yet many essential facts seem beyond the immediate reach of demonstration, simple instinct tells us to simply write down in black and white what things insistently tell us, and all the more clearly as we take the trouble to write under their dictation! There's no need to worry about demonstrations or complete constructions - to bother with such requirements at this stage of the work would be tantamount to denying ourselves access to the most delicate, most essential stage of a vast work of discovery - that of the birth of a vision, taking shape and substance out of an apparent nothingness. The simple act of **writing**, **naming**, **describing**
- if only to describe elusive intuitions or simple "suspicions" reluctant to take shape
-has a **creative power**. This is the instrument of the passion to know, when it is invested in things that the intellect can apprehend. In the process of discovering these things, this work is the creative stage above all others, which always precedes the demonstration and gives us the means to do so - or, to put it another way, to discover them.

"levels".

[4](***) This was at a time when the young Deligne had probably never heard the word "schema" in a mathematical context, nor the word "cohomology". (He became acquainted with these notions through my contact, from 1965 onwards).

[5](*) (February 28, 1985) This is in fact fi ltration by "levels" (see previous footnote).

[6](**) Just as the fundamental groups π_1 (s), π_1 (y) of some "space" X at two "points" x and y reduce from each other by "twisting" by the torsor π_1 (x, y) classes of paths from x to y. . .

to put it better, without which the question of "demonstrating" something doesn't even arise, before anything that touches on the essential has been formulated and seen. By the sheer virtue of an effort to formulate, what was shapeless takes shape, lends itself to examination, decanting that which is visibly false from that which is possible, and that above all which accords so perfectly with the totality of things known, or guessed at, that it in turn becomes a tangible and reliable element of the vision in the making. This vision becomes richer and more precise as the work of formulation progresses. Only ten suspected things, none of which (let's say Hodge's conjecture) leads to conviction, but which mutually clarify and complete each other and seem to contribute to the same still mysterious harmony, acquire in this harmony the force of vision. Even though all ten would eventually prove false, the work that has led to this provisional vision has not been done in vain, and the harmony it has given us a glimpse of and enabled us to penetrate is not an illusion, but a reality, calling us to know it. Through this work alone, we have been able to enter in intimate contact with this reality, this hidden and perfect harmony. When we know that things have
p. 211 reason to be what they are' that our vocation is to know them, not to dominate them, then the day when a error erupts is a day of exultation (56) - just as much as the day when a demonstration teaches us beyond any doubt that something we imagined was indeed the faithful and true expression of reality itself.

In either case, such a discovery comes as a reward for work, and could not have happened without it. But whereas it would only come at the end of years of effort, or even if we never learned the final word, reserved for others after us, work is its own reward, rich in every moment of what this very moment reveals to us.

Note 51_1 (June 5) Zoghman Mebkhout has just drawn my attention, however, to a mention of "Grothendieck's motives" made on page 261 of the volume quoted, in an article by Deligne that "takes up and completes a letter to Langlands". It reads: "I will not be referring to Grothendieck's motives, as he defined them in terms of algebraic cycles, but to **absolute Hodge motives**, similarly defined in terms of absolute Hodges cycles". Grothendieck's motives" (not underlined) are named here, not as a source of inspiration, but to distance ourselves from them and insist that we're talking about **something else** (which we take care to underline). This distancing is all the more remarkable, given that the validity of Hodge's conjecture (a conjecture known to Deligne, I suppose, as it is to every reader of his article-letter, starting with his original addressee Langlands) would imply that the two notions are **identical**! !

Of course, as early as 1964, when I developed the notion of motivic Galois group, it was well known to me that a notion of "Hodge pattern" could be developed on the same model, with a corresponding notion of "motivic Galois-Hodge group", which was introduced independently by Tate (I can't say whether it was before or after) and then given the name of Hodge-Tate group (associated with a Hodge structure). The crude swindle (which doesn't seem to bother anyone, coming from such a prestigious figure) consists in purely and simply swallowing the paternity of a new and profound notion, that of motif, and of the whole rich fabric of intuitions I had developed around this notion, under the derisory pretext that the technical approach taken towards this notion (via absolute Hodge cycles, instead of of algebraic cycles) is (perhaps, if Hodge's conjecture is wrong) different from the one I had (very tentatively) adopted. This yoga, which I had been developing over a period of nearly ten years, has been
p. 212 the main source of inspiration in Deligne's work since its inception in 1968. Its fruitfulness and power as a tool for discovery were clear long before I left in 1970, and its identity is independent of any technical approach taken to establish the validity of this or that limited part of Deligne's work.

this yoga. Deligne had the merit of identifying two such approaches, independently of any conjecture. On the other hand, he was not honest enough to name his source of inspiration, endeavoring as early as 1968 to hide it from public view so as to reserve the exclusive benefit for himself, until he (tacitly) claimed credit for it in 1982.

14.1.2. Burial - or the New Father

Note 52 Returning to the dream of the motifs, I seem to remember that I dreamt it aloud. Admittedly, dreaming is by its very nature solitary work - but the twists and turns of this tenacious work, which went on for years on the sidelines of a vast task of writing foundations that absorbed most of my time - these twists and turns had a day-to-day witness, much closer than Serre, who confined himself to following things from afar. ... [7](*). I could have added that I even told him what I didn't "know" in the common sense of the term - those mathematical "dreams" (on the theme of patterns as on others) which always found in him an attentive ear and an alert mind, like mine, eager to understand.

It's true that when I wrote that Pierre Deligne may have been "a bit of a pupil", this is still a subjective impression (57), not corroborated (as far as I know) by any written or at least printed record, which could lead anyone to suspect that Deligne might have learned something from me - whereas it's a pleasure for me to remember that I never spoke mathematics with him without learning something. (And even when I stopped talking mathematics with him, I continued to learn more difficult and perhaps more important things from him, including on the very day I'm writing this.
these lines...).

Having been informed lately by a third party, who had guessed (one wonders how!) p. 213
that I might be interested in the matter, of the existence of a text by Deligne and others in which motives or at least "Tanakian categories" were discussed, and having mentioned this to Deligne, he expressed his sincere surprise that I should be interested in such things. Looking through the copy he was kind enough to send me, however, I can see that his surprise was perfectly justified. Clearly, I'm a complete stranger to the subject at hand. At most, the introduction hints in passing that certain "standard conjectures" (which I had made at the time, one wonders why) would have a consequence for the structure of the category of patterns over a body. ... The reader curious to know more would be at a loss, for throughout this book he will find no details or references to these conjectures, which are no longer mentioned; nor any mention of the one and only published text in which I explain the construction of a category of patterns over a body in terms of the standard conjectures; nor of the only other published text from before 1970 in which patterns are discussed, by Demazure (in a Bourbaki Seminar, if I remember correctly), which followed my principle of ad hoc construction, in a slightly different perspective. ... [8](*).

[7](*) (May 25) The beginnings of my thinking on motives, however, predate Deligne's appearance. My manuscript notes on Galois motivic theory date from 1964.

[8](*) After verification, I note that apart from a few pages on standard conjectures (Algebraic Geometry, Bombay, 1968, Oxford Univ. Press (1969) pp. 193-199), there is no published mathematical text by me in which motives are mentioned. In Demazure's talk (Séminaire Bourbaki n◦ 365, 1969/70), following Manin's talk in Russian, mention is made of talks on
I had done at the IHES in 1967, and which (I suppose) constituted a first overall sketch of a vision of the
motives. An account of the standard conjectures and their relation to the Weil conjectures, more detailed than the announcement at the Bombay congress, is given by Kleiman (Algebraic Cycles and the Weil conjectures, in Dix exposés sur la cohomologie des schémas, Masson-North Holland, 1968, pp. 359-386). I was not aware of any reflection on the standard conjectures, in particular towards a demonstration of them, outside my own before 1970. The deliberate intention to ignore these key conjectures (which I

p. 214 Even Neantro Saavedra, who had the good fortune to be one of my "pre-1970 students", was duly cited. He had done a thesis with me on what I called, I believe, "rigid tensorial categories", and which he called "Tannakian categories". One still wonders by what miraculous chance Saavedra had been able to foresee the needs of Deligne's theory of patterns, which was to blossom ten years later! In fact, in his thesis he does exactly **the** work that technically constitutes the key to a motivic Galois theory, just as J.L. Verdier's thesis was in principle **the** work that technically constitutes the key to a formalism of the six operations in cohomology. One difference (among others) in Saavedra's honor is that he took the trouble to publish his work; admittedly, he hadn't had the pen of Hartshorne, Deligne and Illusie combined to dispense with such a formality. Yet, ten years later, Saavedra's thesis is reproduced ab ovo and practically in toto in the remarkable collection, this time by Deligne and Milne. This may not have been essential, if it was merely a matter of correcting two particular points in Saavedra's work (58). But there is a reason for everything, and I think I can see why Deligne himself took the trouble[9] (*), even though it runs counter to his own extremely exacting standards for publication, which he is known to apply with exemplary rigor when it comes to others... . [10](**).

As for the authorship of the notions and motivic yoga themselves, for an uninformed reader (and informed readers are becoming rare and will eventually die their own death. . .) this authorship cannot be in the slightest doubt - without any need here to go bothering distant Hilbert and Riemann, let alone the good Lord.) there can be no doubt whatsoever as to their authorship - and there's no need to go bothering distant Hilbert and Riemann, let alone the good Lord. If the prestigious author, whose beautiful result on absolute Hodge cycles on abelian varieties appears as the starting point, and birth to say the least, of the theory of motives, does not
p. 215 breathe a word of its paternity, it is there a modesty which honors him and in perfect agreement with the customs and the ethics of the profession, which dictate that we leave it to others (if need be) to give honor where honor is obviously due: to the legitimate Father... .

Note 53 Touched by the vicissitudes of this orphan, and doubting that another will do the work whose need and scope I am apparently the only one, even today, to feel, I presume that the "bold mathematician" in question will be none other than myself, once I have completed the Poursuite des Champs (which I anticipate will occupy me for another year or so).

Note 54 Since then, two new cohomological theories have appeared for algebraic varieties (apart from the Hodge-Deligne theory, a natural extension, in the "motivic" spirit, of the Hodge cohomology): Deligne's theory of "stratified promodules" and, above all, that of crystals, a "*D-Modules*" version à la Sato-Mebkhout, with the new light provided by the theorem of the good God (aka Mebkhout) mentioned earlier. This approach to constructible discrete coefficients is likely to replace Deligne's earlier version, as it is probably better suited to expressing relations with De Rham's cohomology. Moreover, these new theories do not provide new functor-fibers on the category of smooth patterns on a given scheme, but rather (modulo a more thorough groundwork than has been done so far) a way of precisely apprehending the "Hodge" embodiment of a (not necessarily smooth) pattern on a finite-type scheme over the body

said, in my Bombay sketch, that I considered them, along with the resolution of singularities of excellent schemes, to be the most important open problem in algebraic geometry), seems to me to have a lot to do with the impression of stagnation that the cohomological theory of algebraic varieties gives me, from the echoes that have come back to me.

[9](*) On this subject, see the reflections in the note "La table rase", n◦ 67.

[10](**) (June 8) And even more so, when it comes to works that bear the mark of my influence - on this subject, see the episode entitled "The note - I'm not the only one! or the new ethics". Section 33.

of complexes, or the "De Rham" incarnation on a finite-type scheme over a body of zero characteristic. It is likely, moreover, that the (apparently still unwritten) theory of Hodge-Deligne coefficients

on a finite-type scheme on C, will eventually appear as contained in the (equally unwritten) theory crystalline coefficients à la Sato-Mebkhout (with an additional filtration datum), or more precisely as a kind of intersection of the latter with the theory of constructible discrete coefficients

Q-vectorial ... . As for elucidating the relationship between Mebkhout's crystalline theory and that of the developed in positive characteristic by Berthelot and others, this is a task Mebkhout has been feeling since before 1978, in a climate of general indifference, and which seems to me to be one of the most fascinating that im- mediately arises for our understanding of "the" cohomology (unique and indivisible, motivic knowledge!) of algebraic varieties.

Note 55 I may have been dreaming, but my dream about the relationship between Hodge's patterns and structures made mep . 216
to put my finger, without even doing so on purpose, on an inconsistency in the "generalized" Hodge conjecture as originally formulated by Hodge, and to replace it with a rectified version which for the
(I'd wager) must be no more or less false than the "usual" Hodge conjecture on algebraic cycles.

14.1.3. Prelude to a massacre

Note 56 I'm thinking in particular, in the context of the cohomology of algebraic varieties, of Griffiths' discovery of the falsity of a seductive idea we'd long had about algebraic cycles, namely that a cycle homologically equivalent to zero had a multiple that was algebraically equivalent to zero. This discovery of a brand-new phenomenon struck me enough for me to spend a week trying to grasp Griffiths' example, transposing his construction into a new one.

(which was transcendental, on the C body) into a construction "as general as possible", and valid in particular- on bodies of any characteristic. The extension wasn't entirely obvious, with (if I remember correctly) Leray spectral sequences and Lefschetz's theorem.

(June 16) This reflection had been the occasion for me to develop, in the étale context, the cohomological theory of "Lefschetz brushes". My notes on this subject are developed in the SGA 7 II seminar (by P. Deligne and N. Katz) in lectures XVII, XVIII, XX by N. Katz (who takes care to refer to these notes, which he followed closely). In the introduction to the volume by P. Deligne, on the other hand, where it is stated that the key results of the volume are talks XV (Picard-Lefschetz formulas in stale cohomology) and XVIII (Lefschetz brush theory), the author is careful not to point out that I had anything to do with this "key theory" of Lefschetz brushes. Reading the introduction gives the impression that I have nothing to do with the themes developed in the volume.

The long SGA 7 seminar, which in 1967-69 followed on from the SGA 1 to SGA 6 seminars developed at my instigation between 1960 and 1967, was conducted jointly by Deligne and myself, who had kicked things off with a systematic theory of evanescent cycle groups. The presentations by
volunteers, the two seminar volumes (SGA7 I and SGA 7 II) have not yet been published.
were not published until 1973, in the care of Deligne. While it had been understood at the time of the seminar that p. 217

After I left, Deligne informed me of his desire (which seemed strange to me) for the seminar to be **split in two**, with Part I presented as directed by me, and Part II by him and Katz. I now perceive in it an "operation" that prefigures the "SGA 4 operation[1] " aimed (among other things) at bringing to light[2] the whole series of foundations SGA 1 to SGA 7, which in his mind and its conception was inseparable from my person, as was the EGA series of Eléments de Géométrie Algébrique, as a collection of all-purpose texts in which my person would play only an episodic, even superfluous role. This tendency is very clear, even brutal, in the SGA 4 volume$\frac{1}{2}$ and above all in the massacre of the SGA 5 seminar, to which this volume is indissolubly linked. On this subject, see, among others, the notes "La table rase" and "Le massacre", n° s 67 and 87, and especially "La dépouille. .. . "(n° 88).

(June 17) The overall conception of the SGA 7 seminar (in which I made no distinction between parts "I" and "II", and still don't) was due to me, and Deligne had made important contributions (reported in my report on Deligne's work, written in 1969, see n° s l3, 14 of this report), the most crucial for the purposes of the seminar being the Picard-Lefschetz formula, proved by a specialization argument from the transcendental case already known. The split of the seminar into two parts was unjustified both mathematically and in terms of the respective contributions - there are substantial contributions from both Deligne and myself in each of the two "pieces" of SGA 7.

Of course, I would have been delighted if Deligne had continued the series of APG foundations I had inaugurated.

- which was nowhere near the end of its run! This "SGA 7 operation" is by no means a continuation, but I feel it is a sort of brutal "saw" (or chainsaw. . .), **bringing** the SGA series to an **end** with a volume that ostentatiously distances itself from my person, even though it is linked to my work and bears its mark just as much as the others. While my person is concealed as far as possible, the tone towards my work is not yet that of the barely disguised contempt of the "SGA 4 operation$\frac{1}{2}$ ", which represents an even more brutal saw cut in the unity of the SGA 4 and 5 seminar, and the means and pretext for the ruthless ransacking of the unpublished SGA 5 part of it, the torn-off pieces of which are shared equally between Deligne and Verdier. ...

p. 218 **Note** 57 I hasten to add that the same remark applies to the other mathematician of great means of whom I ventured to say (in note no.° 19) that he was "a bit of a pupil", ten years after Deligne.

Note 58 This reminds me that Notes readings (which had published six or seven "pre-1970" doc- torat theses done with me) never wanted to publish Yves Ladegaillerie's "post-1970" one (reason: they don't publish theses!). On the other hand, they did publish Saavedra's thesis a second time. ... Incidentally, I had told Deligne about Ladegaillerie's beautiful isotopy result, which was being rejected everywhere (with the further secret hope that he would lend his help to publish it) - but didn't seem to interest him (reason: his incompetence in surface topology. . .).

Curtain. . .

14.1.4. The new ethics (2) - or the fair d'empoigne

Note 59 (April 20) In the few weeks since these lines were written, which note a contradiction and its price, I was surprised to discover that the person concerned had already, two years ago, found a very simple way of "resolving" the said contradiction - all it took was a little thought! We could call it "the early burial method" (which the reader can read about in the double note (50) (51), written yesterday in the fresh emotion of the discovery). I'm sorry to say that the unexpected reappearance of the anticipated deceased on the famous "mathematical stage" (which sometimes looks rather like a jostling match. . .) risks introducing technical complications for the smooth application of this brilliant method! In a previous note ("Deontological consensus - and control of information", n° 6) I felt (a little)

still confused) that the most universally accepted rule of ethics in the scientific profession "remained a dead letter" in the absence of respect, by those in control of scientific information, for every scientist's right to make his or her ideas and results known. At about this time, I also took the trouble to describe in some detail a case in point where, for me, the disregard for this right was flagrant, and where I also felt that this disregard was bordering on disregard for
the primary rule, on which there is general consensus. (See "The note - or the new ethic", section 30).

It's not the only time I've felt this very particular uneasiness, when I saw the **spirit of** this p. 219
The first rule was scorned, while the person who made it was "thumbed" both by his position (above suspicion!) and his means, and by the casualness of his form. I try to pinpoint this malaise in the note ("the snobbery of youth - or the defenders of purity") that relates to the quoted section. When one allows oneself to despise the "obvious" things I'm talking about there, and in the same spirit also (might I add now) the (perhaps profound) things that are neither demonstrated nor patented as published "conjectures" known to all, one might as well (given the little l) consider them common property (trivial, it goes without saying)[11] (*), and therefore also, when the time comes, as "one's own" with the greatest casualness and the best conscience in the world - it being understood, of course, that one wouldn't dream of appropriating a ten-page or one-hundred-page (or just ten-line) muscular demonstration that establishes a result "that one hasn't been able to demonstrate" (59). I didn't think I had such a good feeling or such a good word to say (on the subject of "dead letter"), since I'v e just seen the undecided "limit" of the case cited above blithely crossed - and surely crossed with the best conscience in the world, **given how little: a dream**, and what's more, not even demonstrated (nor, above all, **published** . . .).[12] (**)

Fortunately, I've got a bit of a backbone - when I need to, I can express what I feel as best I can, and that's not all. that I want to say, I've acquired (rightly or wrongly) credibility, and thus a chance to be listened to when I have something to say, or to publish it if I feel the need. On the other hand, I realize more vividly what "feeling of injustice and powerlessness" of one who is wronged without recourse, when he feels his hands and feet tied p. 220
to the arbitrariness of "those who have everything in their hands" - and use it as they see fit.

It's true that in my life as a mathematician, I've sometimes behaved badly with an equally good conscience, and I've had the opportunity in my reflection to talk about cases that this has brought to the surface from the mists of oblivion and ambiguity never examined. By probing them, I finally understood that I shouldn't be surprised if today (and for a long time now) the pupil has blithely surpassed the master, nor should I disown anyone to whom I have sympathy or affection. But it's healthy, for me as for everyone else, to call a spade a spade, whether that spade belongs to my house or to someone else's.

14.1.5. Appropriation and contempt

Note! 59' (June 8) I'm no longer convinced, as far as my friend Pierre Deligne is concerned, having had the opportunity to note that he has finally slipped into the game of "tacit paternity" with regard to the l-adic cohomolo- gic tool, i.e. what I call "mastery" of spread cohomology. There has been a remarkable evolution between the "SGA 4 operation $\frac{1}{2}$ " (where my name is still pronounced, but with an affectation of disinterested disdain) and the "SGA 4 operation" (where my name is still pronounced, but with an affectation of disinterested disdain).

[11](*) Such was the fate of "Le théorème du bon Dieu" (alias Mebkhout).
(June 8) And, as in pattern yoga, we take care to cleverly create the appearance of authorship, without ever saying so outright! See on this subject (in the case in point) the note "Le Prestidigitateur" n◦ 75", and for the brilliant general method or style, the note "Pouce!" n◦ 77, as well as the note that follows "Appropriation and contempt", n◦ 59'.
[12](**) It would be wrong to be embarrassed, as the event seems to show that the general consensus these days is that the something quite normal - at least from someone of such high standing! What we call "good conscience" is no more, no less,

than a feeling of agreement with the prevailing consensus in the milieu to which one belongs.

(See the notes "La table rase" and "L'être à part" for the initial phase, and the notes "L' Eloge Funèbre (1), (2)" for the final phase).

Intermediary phases in this escalation include the "memorable article" on so-called "perverse beams" in 1981 (see the notes "L'Iniquité - ou le sens d'un retour" and "Pouce!", n° 75 and 77), and the exhumation of the motifs in LN 900 the following year (the Eloge Funèbre taking place the following year, in 1983). In all these cases and others of lesser scope that I've observed, the inner attitude and "method" that enables Deligne to appropriate the credit of others' ideas with a clear conscience is that of **contempt** (which remains partially tacit, so "little" in fact that it's not even worth talking about, when you're going to use it to do really powerful things - Weil's conjectures, the theory of so-called "per- vers" beams. . . Once the operation has been completed and appropriation accepted by all, it's always

time to put things right and strut modestly with what has been appropriated. The same contribution is the object of casual scorn, as long as it still seems tainted with the name of one of those it is intended to bury, and

p. 221 has been taken up when it has been appropriated by himself (*l-adic* cohomology, motifs, while waiting for Mebkhout's yoga) or by some good buddy (yoga of derived categories, yoga of duality, appropriated by Verdier with Deligne's active encouragement).

14.2. V My friend Pierre

14.2.1. The child

p. 223 **Note** 60 (April 21) To take up this dream of a memory, which is not just the memory of birth

a vision... I remember well (even though I've forgotten so much!) the pleasure I took in talking with him, who soon became a confidant of everything that intrigued me, or that enlightened and delighted me day by day in my love affair with mathematics, than he had ever been a "pupil". His ever-awakening interest, the ease with which he took in everything ("as if he'd always known. . . ") were a constant source of enchantment for me. He was a perfect listener, driven by the same thirst for understanding that animated him as me - a highly awakened listener, a sign of communion. His comments always met my own intuitions or reservations, when they didn't throw some unexpected light on the reality I was trying to pin down through the mists that still surrounded it. As I've said elsewhere, he often had the answers to the questions I raised, often on the spot, or would elaborate on them in the days or weeks that followed. In other words, the listening was shared, when he in turn explained to me the answers he had found, i.e. quite simply the reason for things, which always appeared with that perfect naturalness, with that same ease that had often enchanted me with some of my elders like Schwartz and Serre (and also, with Cartier). It was this same simplicity, this same "obviousness" that I had always pursued in my understanding of mathematical things. Without having to say it, it was clear that with this approach and these high standards, he and I were "from the same family".

I sensed from the moment we met that his "means", as they say, were of a very rare quality, far beyond the modest means at my disposal, even though we were on the same wavelength in terms of our passion for understanding and our demand for understanding mathematical things. I also had the vague feeling, although I couldn't put it into words at the time, that this "strength" I saw in him (and which I also sensed in him) was the same as the "strength" I saw in myself.

The ability to "see" obvious things that nobody else could, was the strength of childhood, the innocence of a child's eyes. There was something of the child in him, far more apparent than in any other mathematician I've known, and surely not by chance. He told me
that one day, when he was still in high school I think, he amused himself by checking the multiplication table (and along the way and by force of circumstance, the addition table too), for numbers from 1 to 9, in terms p. 224
definitions. He certainly wasn't expecting any surprises - if there was any surprise (a pleasant one, as always. . .), it was that the demonstration could be done nicely and completely in just a few pages, the story of perhaps half an hour. When he laughed and told me the story, I could feel that it had been half an hour well spent - and that's something I understand even better today than I did then. This little story struck me, even impressed me (though I don't think I let it show) - I sensed in it the sign of an **inner autonomy**, a freedom from received knowledge, which had also been present in my relationship with mathematics in my childhood, from the very first contacts (69)[13] (*).

This relationship of privileged interlocutor for each other, when we saw each other practically every day I believe[14] (**), continued over a period of five years, from 1965 (if I remember correctly) to 1969 inclusive. I still remember the pleasure I had, in that year, in writing a detailed report on his work, when I was proposing to co-opt him as a professor in the institution where I had worked since its foundation (in 1958), and where most of my mathematical work was accomplished. I no longer have a copy of this report (64), in which I reviewed, I believe, a good dozen of my friend's works, almost all unpublished at the time (many have remained so), and most if not all of which, in my opinion, carried the weight of the main substance of a good state doctorate thesis. I was prouder and happier to present this eloquent report than I would have been had I been presenting a report on my own work (something I've only done twice in my life, and each time under compulsion. . .).). Many of these works were answers to questions I had raised (the only one published among them being the work already mentioned
on the degeneracy of the spectral Leray sequence for a clean and smooth morphism of schemes (63)). The two most important parcontre were answers to questions Deligne himself had asked, and he was p. 225
It was clear that their scope was of a completely different order than a "good state doctorate thesis". These were his work on the Ramanuyam conjecture (published in the Bourbaki seminar), and the work on mixed Hodge structures, also known as "Hodge-Deligne theory".

It's a strange thing, and one I was far from suspecting when I wrote this sparkling report, that I was to leave less than a year later this institution where I was about to have my young and impressive friend co-opted, and where I intended to end my days. And (now that I've put these two double episodes together) it's another strange thing, and no more surely the effect of mere "chance", that this same (now less young!) friend announced to me a month or two ago his own departure from this same institution, when it had also been a year since I had resumed regular mathematical activity, in the sense of a kind of unexpected "re-entry" onto the mathematical scene (if not into the "great world". . .).

On more than one occasion in Récoltes et semailles, I've spoken of my departure - of this "salutary uprooting" - and even more of the "awakening" that closely followed it, and which made this episode a crucial turning point in my life.

[13](*) Incidentally, it seems to me that this freedom has never entirely disappeared during my life as a mathematician, and that it is once again present as it was in my childhood. Two or three years ago, I revisited the little episode of the multiplication table for my friend. I felt he was embarrassed by this evocation of a childhood memory, which no longer visibly corresponded to his self-image. I wasn't really surprised by his embarrassment, but saddened to see something that I knew well, but still found hard to admit, confirmed once again... ...

[14](**) This was the case at least as long as I lived in Bures, where he was housed in a studio at the IHES. From 1967 onwards (when I moved to Massy), I think we still saw each other once or twice a week, at least as long as I remained involved in mathematics.

my life. In the intense years that followed, the world of mathematicians, with those I had loved in it, and that very thing that had fascinated me most in mathematics itself, became very distant - as if drowned in the mists of memory of another "myself", who would have died ages ago... .

But both before this episode, and in the years that followed this first major turning point, I knew that the man who had been (a little[15] (*)) my pupil and (a lot of) my confidant and friend, had only to follow the spontaneous impulse within him of a child who plays and wants to know, to discover and bring to light new and unsuspected worlds, and to fathom them and know their intimate nature - and thereby reveal them to his fellow creatures as well as to himself. So, if after my departure (with no spirit of return!) I saw "a bold and inspired mathematician" sketch out (for a start. . .) the vast picture I had glimpsed, and of which I had still only drawn a series of partial and provisional sketches, it was indeed he - who had everything in his power to make the world a better place.

p. 226 hands to do it! Brushing up this first large-scale picture, a "master builder" bringing together in a common vision the essentials of what was known and guessed about the cohomology of varieties.

For someone in whom such an overall vision was already ready to emerge from the mists of the as yet unwritten, this algebraic work was the work of a few months, not even years. (It would have to be taken up again and deepened over the years - or generations, if generations were needed - until the final word on the reality of the motifs was fully understood and established). And I had no doubt that this work, which used to "burn in my hands", would be done any moment now, and at least over the next two or three years, while it was still hot. After my departure, there was certainly only one person left who was called upon, by his very impulse of knowledge, to do this burning and fascinating work. Once the "maître d'oeuvre" had been written and tested, and the construction of the work more or less completed, I would leave it to others to continue this work, however fascinating it may be, and embark on other adventures, in this world of mathematical things, where every bend in the road reveals the promise of a new, limitless world, provided we have open, new eyes to see... .

At a time when my life was still taking place in the warm scientific incubator that isolated it from the noises of the world, and when Deligne was developing his extension of Hodge's theory (this must have been in 1968 or '69), it was a matter of course between us that this work was a very first step towards realizing, testing and refining a certain **part** of this "tableau des motifs", which had never been put down in black and white in its entirety[16] (*). In the years following my departure from the "étuve", at a time when mathematics was a long way off for me, it came as no surprise to learn that Weil's conjectures had finally been demonstrated. (If there was any surprise, it was that the "standard conjectures" had not been demonstrated in the same breath, even though they had been developed precisely with a view to an approach to Weil's conjectures.

p. 227 Weil, at the same time as a means of establishing at least a theory of semisimple patterns on a body[17] (**).) I was well aware that neither by this first draft towards a general theory of coefficients to the

Hodge, nor by this demonstration of certain key conjectures (among a number of others that are more or less well known) he had yet reached his full potential - indeed, he was far from it. And I waited without impatience, while most of my attention was absorbed elsewhere. (-> 61)

[15](*) For the meaning of this scruple in me to consider the (too!) brilliant Deligne as one of my students, see the note "L'être à part" (n° 67).

[16](*) That this Hodge-Deligne theory never (as far as I know) went beyond this first draft, that it never expanded into a theory of "Hodge-Deligne coeffi cients" (and the "six operations" on them) above the type schemes fi nished on the field of complexes, is inseparable from this other strange fact: that this vast "tableau des motifs" has never been painted, and that its very existence has been carefully hushed up to this very day. ...

[17](**) It's only in recent years that I've become vaguely aware (but more precisely lately!) that the "standard conjectures", as much as the very notion of pattern for which they provided a first "constructive" approach, had been **buried**, for reasons that are now particularly clear to me. (Compare also the previous footnote).

14.2.2. The funeral

Note 61 I had been privileged to see the first flowering of a child's impulse, bearing the promise of a vast deployment. Over the next fifteen years, I came to realize that this promise was constantly being deferred. There was this delicate thing in him that I had been able to sense and recognize (at a time when I was insensitive to so many things!), a thing that is of an entirely different nature from cerebral power (which crushes as well as penetrates. . .) - a thing that is essential above all for any truly creative work. I'd sensed it in others at times, but in no mathematician I'd ever known had it manifested itself with comparable force. And I expected (as a matter of course) that this thing would continue to blossom in him and transform itself, and express itself effortlessly in a unique work, of which I would have been a modest precursor. But strangely enough (and surely there's a deep and simple connection between so many "strange things") - I've seen this "delicate thing", this "strength" that's neither muscle nor brain, gradually fade away over the years, as if **buried** under successive layers, and thicker and thicker - layers of **something else** I know only too well - the most common thing in the world! It's not necessarily a bad match for brain power, consummate experience or a trained flair in a particular discipline, which can force the admiration of some and the fear of others, or both, through the accumulation of works, brilliant perhaps and surely having their strength and beauty. But **that**'s not what I had in mind when I spoke of "unfolding" or "blossoming". The blossoming I had in mind is the fruit of an innocence, eager to know and always ready to rejoice in the beauty of the small and great things of this inexhaustible world, or of such and such a part of this world (such as the vast world of mathematical things. . .). It is he who alone has the power of profound renewal, whether of self, or of knowledge of the things of this world. It is this power that has been fully realized, it seems to me, in the modest person of a Riemann[18] (*). This true fulfillment is foreign to contempt: to the disregard of p. 228 others (those we feel far below us...), or that of things too "small" or too obvious for us to deign to take an interest in, or of those we feel fall short of our legitimate expectations; or the contempt of such a **dream** perhaps, speaking to us insistently about the things we profess to love... . It is foreign to contempt, just as it is foreign to the fatuity that feeds it.

Certainly, with his impressive "means", but even more so with that delicate thing that impresses no one and **creates**, the "pupil" was destined to far surpass the "master". I had no doubt that, in the years following my departure from this place where I had witnessed such a beautiful flight, Deligne would give his full measure in the deployment of a vast and profound work, of which I would have been one of the precursors. The echoes of such a work would not fail to reach me over the years, while I myself, in the pursuit of other quests far from mathematics, could only imperfectly appreciate the full scope and beauty of the new worlds he was about to discover.

But the pupil cannot surpass the teacher by **disowning** him in his innermost self, by secretly striving, before himself and before others, to erase all trace of what he has contributed (whether the contribution was for the better, or for the worse. . .) - any more than the son can truly surpass the father by disowning him. This is something I've learned above all through my relationship with my children, but also (later on) through my relationship with some of my former pupils; and above all with the one, of all people, whom I've always been scrupulous about calling "pupil", having sensed from the moment I met him that I had to learn from him.

[18](*) The work of Riemann (1826-1866) is contained in a modest volume of around ten works (it's true that he died in his forties), most of which contain simple, essential ideas that profoundly renewed the mathematics of his time.

of him, as much as he of me[19] (*). But it was only almost ten years after that meeting, after 1975 and especially since I've been meditating on the meaning of what I experience and witness, that I began to sense this **hindrance** in the man who continued to be dear to me. And I also felt, obscurely, that this secret disavowal of my person and of a role I had played in crucial years of his life, was too,
more profoundly, a disavowal **of itself**, (It is so, no doubt, whenever we disavow and want to erase something that has well and truly taken place, and whose fruit is ours to gather. ...).

p. 229 However, lacking any kind of "connection" to "what was being done in maths", and to what he was doing himself[20] (*), I never realized, until I thought about it a few weeks ago, how much this hindrance also weighed on the very thing in which he had invested his all: his mathematical work. Certainly, more than once in the last eight or nine years, I've seen simple common sense or a mathematician's healthy instincts wiped out by a deliberate gesture of disdain (towards me) or contempt (towards others whom it was in his power to discourage) (66). Indeed, he was not the only one of my former students, with or without quotation marks, in whom I witnessed such attitudes towards people I cared about (or towards others). But in no other case have I been so painfully affected. More than once in the course of my reflections over the past two months, I've alluded to this experience, "the most bitter I've ever had in my life as a mathematician" - and I've also said what it ultimately taught me, at the end of this Harvest and Sow reflection. This sorrow was so vivid, it taught me something so far-reaching about a person who was still dear to me (while I continued to evade what it also taught me about myself and my past. . .), that the question of its impact on his mathematical "creativity", or even on that of the person who had been discouraged or humiliated, became entirely secondary, not to say derisory.

The note "Refus d'un héritage - ou le prix d'une contradiction" (Refusal of an inheritance - or the price of a contradiction) is the first written reflection in which I took stock of what had come back to me in bits and pieces, here and there, over the years, both on the "state of art", and on the work of the man I had known so well and so little. It was also the first time I had seen, in a single glance, the full "**price**", or the full weight, in his work as a mathematician, of the refusal he had carried within him for more than fifteen years. In writing this note, however, I was "delaying", since for two years already (and without "anyone" seeing fit to inform me), the reasons had been brought out of the secrecy in which they had been kept.
for twelve years. ... And today, as I write this final stage (I believe) of my reflection on my mathematical past, two days after having read in large lignes this memorable volume which

p. 230 the perception of this crushing weight has become striking. It's the weight that, day after day and through a hundred detours, those who are made to fly enjoy dragging along - a flight that's supple and light, joyful and intrepid in its pursuit of the unknown, for its own joy and that of the wind that carries it. ... [21](*)

If he doesn't steal, and if he's content to be a man admired and feared, accumulating proof of his superiority over others, I have nothing to worry about, If he drags the weights he likes to drag, surely he finds some

[19](*) (June 14) On the subject of my deliberate and persistent intention to minimize what I had to contribute, and to deny the reality of a master-pupil relationship, see the note "L'être à part", n° 67 . Clearly, there's no comparison between what my friend learned from me ("as if he'd always known", of course!), and what I learned from him. It would undoubtedly have been
otherwise, had I continued my intense mathematical investment to this day, and maintained regular mathematical contact between us.

[20](*) Since 1970, I've received four offprints by Deligne, which (like most of the offprints I still receive) I skimmed through on the spot. It wasn't enough to give me an idea of a mathematical work, even in outline or through its main themes.

[21](*) I don't mean to suggest that it's the privilege of a few exceptional beings to be called upon to "fly" and discover the world - surely we're all called upon by birth! However, this capacity rarely finds the opportunity to blossom, even if only in a very limited direction (such as mathematical work). But in one person I've seen such an ability (in the "mathematical" direction) preserved as if by miracle, only to regress over the years.

satisfactions - just as I myself have taken pleasure in dragging along weights, and continue today to drag along those I haven't yet been able to part with along the way. Of what I had to offer, the best and the worst, he took what he liked. I don't have to worry about his choices, which are his alone; nor do I have to decide here whether they are the best or the worst (62). What's "best" for one person is "worst" for another, or sometimes for the same person (as long as he changes, which is admittedly not very common. . .).

But the choices we make, and the actions that express them (even though our words often deny them), we make at our peril. While they often bring us the expected gratifications (which we receive as "the best"), these very gratifications sometimes end up having setbacks (which we reject as "the worst", and often as an outrage). When we finally understand that setbacks are not an outrage, we often regard them as a price to be paid, which we pay with reluctance. Sometimes, however, we come to understand that such setbacks are something other than ruthless cashiers, to whom we have to pay for the good time we've had, whether we like it or not. That they are patient and obstinate messengers, who never tire of coming back to bring us the same message over and over again - an unwelcome message, to be sure, and one that is constantly rejected.
- because even more than the setback itself, it's its humble message, always rejected, that appears to us as "the worst": worse than a thousand setbacks, often worse than a thousand deaths and the destruction of the entire universe, for which we no longer care. ...

On the day at last when it pleases us to welcome the message, eyes suddenly open and see: what was p. 231
and the crushing weight from which we are suddenly relieved is the very thing we were clinging to only yesterday, as "the best".

14.2.3. The event

Note 62 (April 21) People will tell me that if I have nothing to worry about, then why am I going on for pages and pages about a personal relationship that concerns only me and the person concerned!

If I felt the need to reflect retrospectively on certain important aspects of a relationship, it was under the impact of a specific event that affected me closely (even though I learned of it two years late). This event, on the other hand, is in the public domain, in an even more obvious way than the behaviours and routine acts of prominent mathematicians (such as Deligne, or myself) towards others of lesser renown or beginners (although their effect on the lives of others is often of a quite different scope than in the present case). The event in question (i.e. the publication of the "memorable volume" of Lecture Notes LN 900, a.k.a. the "funeral volume"), like everything surrounding it, struck me as **unhealthy**, rightly or wrongly. I felt it was healthy for everyone, starting with the "interested party" himself, to give a detailed account of some of the ins and outs, getting to the bottom of things as I see them today.

With this account and these thoughts, I'm not trying to convince anyone of anything (far too tiresome, and moreover hopeless!)[22] (*), but simply to understand the events and situations in which I found myself involved. If they inspire others to think beyond the usual clichés, this testimony will not be published in vain.

[22](*) (May 25) If I felt the need here to repeat to myself that it was "far too tiring" and "hopeless" to try to convince, it's undoubtedly because somewhere inside me, the intention to convince was nonetheless well and truly present, and also perceived. The entire period of reflection between April 19 (when I learned of the "memorable volume" LN 900) and April 30 was marked by a state of inner tension, and also division, in the face of the impact of an entirely unexpected "event", which I was trying as best I could to reconcile.
to assimilate the message. This tension was finally resolved with the note "Le retour des choses" (n° 73) of April 30, when in fact reflection had just returned to my own person, to immediately provide me with the obvious key to this message.

14.2.4. Eviction

p. 232 **Note** 63 (April 22)[23] This article (*) appeared in Publications Mathématiques in 1968, i.e. two years later. before I left the world of mathematicians. Its starting point had been a conjecture I'd told Deligne about, of a property of degeneracy of spectral sequences which at the time might have seemed quite incredible, but which nonetheless became plausible by "arithmetic" means, as a consequence of Weil's conjectures. This motivation was of great interest in its own right, as it showed how much could be gained from a "yoga of weights" implicit in Weil's conjectures (a yoga first glimpsed by Serre, in certain important aspects). Since then, I've routinely applied it to all sorts of analogous situations, to draw conclusions of a "geometric" nature (for the cohomology of algebraic varieties) from "arithmetic" arguments. These remained heuristic as long as Weil's conjectures were not established, but were nonetheless highly probative, and represented a first-rate **means of discovery**. Deligne's "geometrical" demonstration for the particular conjecture in question, using Lefschetz's theorem (established then in null car. only), had interest in an entirely different direction, in addition to the first merit of not depending on any conjecture. The link indicated by the two approaches between two seemingly unrelated things - Weil's conjectures (and the yoga of weights that represented the most fascinating aspect of them for me at the time), on the one hand, and Lefschetz's theorem, on the other - was in itself highly instructive.

The interesting thing here, for my own present purposes, and which only became clear to me today, is that the reader of this article will have very little chance of suspecting that I had anything to do with the initial motivation of the main result, and no chance at all of learning from this article what that motivation had been. (See also the beginning of note (49). The **spontaneous** approach (including, I'm sure, on the part of the author himself) to the exposition of a result like this would have been to start with the (admittedly striking) conjecture, to indicate the first, equally striking, reason found for it, which was
a good opportunity to finally "sell" this famous yoga of the weights, of much greater scope in itself
p. 233 that the main result of the work[24] (*); then to follow with the "Lefschetz theorem" point of view[25] (**) which proved the initial conjecture under slightly more general conditions (any basic scheme, not necessarily clean and smooth over a body), but in characteristic zero only. On the other hand, the exposition that follows begins with generalities of homological algebra (as pretty as can be, and presented with the author's customary elegance), generalities that he, like everyone else, must have since forgotten, axiomatization-style, of Lefschetz's theorem. The main result (the only one everyone remembers, of course) appears as cor. *X* towards the middle of the article, while in "remark 2.9" some

[23](*) This is Deligne's article on the degeneracy of spectral sequences and Lefschetz's theorem (Publications Mathéma- tiques 35, 1968) cited in the note "Poids en conserve et douze ans de secret", n◦ 49).

[24](*) It was yoga itself that remained a secret (I think) for the next six years!

(June 7) And (as it has since appeared) which was then presented by Deligne "on his own account", without any allusion either to Serre or to me. (See notes n◦ 78_1 , 78).$_2$

[25](**) (June 17) The idea of using Lefschetz's theorem ("Vache") to demonstrate a degeneracy of spectral sequences is a good one. due to Blanchard, who only obtained the degeneracy theorem under the draconian assumption (rarely verified) that the local system formed by the rational cohomology of fi bres is trivial. I was familiar with Blanchard's work, and mentioned it to Deligne, who thus drew on Blanchard's idea for his demonstration, even though he hadn't read the article. Serre, who remembered Blanchard's demonstration better than I did, pointed out to Deligne that his demonstration was in fact an easy adaptation of Blanchard's. This is what Deligne points out in his remark 2.10. This remark, in which he quotes Serre, is written in such a way as to give the impression that he only learned of Blanchard's idea after the fact, which is in no way the case. The two main **sources** for his article have therefore been overlooked: on the one hand, the arithmetical **motivation,** which made it possible to foresee a considerable strengthening of Blanchard's result, and on the other, Blanchard's **demonstration idea**, which he elegantly adapts to obtain a result that Blanchard had probably not dared to hope for, and for that reason had not even tried to "get" by his method.

Somewhere near the end (the reader doesn't quite know why) the word "weight" and my name are pronounced. ...

I can't remember what impression the article made on me when it first appeared - as I was in the know, I must have just glanced at it. I must surely have sensed an intention to "distance myself", but also felt that it was only natural that my friend should be concerned about not
risk appearing as a disciple (or "foal") of a "master"[26] (***), It is true that if there had been in him
the quiet assurance in his own strength, he would have had no hesitation in writing a work of greater scope p. 234
greater and more useful for everyone (including surely for himself), without fear of not being seen for what he is... . (65).

The situation was somewhat analogous with the publication of his first large-scale work the following year, on mixed Hodge theory. (At the time, I regarded this work as comparable in scope to Hodge theory itself, seeing it as the starting point for a theory of "Hodge- Deligne coefficients", which unfortunately never saw the light of day. ...) As I said, it was obvious to both him and me that this work had its "motivation" in the yoga of patterns I had arrived at over the preceding years - it was a first approach towards a tangible realization of this yoga. To emphasize such a link in his work, it seemed to me (and it must have seemed to me then too), would at once have given his work an even wider scope than it already had on its own merits. At the same time, it was yet another opportunity to draw the reader's attention to the reality of patterns, perceptible at every step behind that of Hodge's structures (63).$_1$

It's only with hindsight that these omissions take on their full meaning, against the backdrop of six years of silence on weight yoga[27] , twelve years of silence (not to say, banning) on[28] patterns, the unusual re-entry of these in the LN 900 volume-burial, stagnation in Hodge- Deligne theory after a dazzling start... . But no one can do great things in a bogeyman's mood!

In any case, had I been more mature when I left IHES in 1970, it would have been clear to me from that moment on that there was a profound ambiguity with regard to me in the man who, over the past five years, had been my closest friend. Moreover, behind the amiable facade of good company within the same hushed institution, my departure was ultimately to my advantage.
everyone, for reasons that I think I can discern with hindsight, and which were not the same for everyone.
Visiblement this departure suited my young friend, recently settled in the place, and to whom he would have p. 235
all it took was a show of solidarity with me (in the face of the hesitant indifference of the other three permanent colleagues) to turn around an indecisive situation. If I didn't understand what was going on at the time, it was because I really didn't want to understand things that were quite clear and even eloquent! It was as if, often in the course of my life, there was an anguish in me (never called by that name!) that signalled a "take-off" between a reality that was tangible and simple, and an image of reality that I didn't want to understand.

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[26](***) (May 26) About this attitude of mine, see the note that follows this one, "Ascension" (n◦ 63').
(June 8) Drawing a parallel with his own style of appropriating other people's ideas, which I see here
the first typical example, I realize that my friend's motivation was in no way to preserve an "autonomy" in relation to a prestigious "master", but rather to conceal the role of other people's ideas in the genesis of his own.
We're also looking forward to appropriating these ideas from others (at a later date). (On this subject, see the two notes "Le Prestidigitateur" and "Appropriation et mépris", n◦ 75" and 59'.) Regarding my share of responsibility for the unfettered development of this propensity in my friend, see the two notes "The Ascent" and "Ambiguity", as well as "The Being Apart" (n◦ 63',
63", 67'), in which the role of my complacency towards the brilliant young man Deligne becomes apparent.

[27](*) (April 19, 1985) For corrections to "six years" and "twelve years", see b. de p. note (***) p. 302 (part dated April 18, 1985), for weights.

[28](*) (April 19, 1985) For corrections to "six years" and "twelve years", see sub-note "Pre-exhumation" (n◦ 168_1), for reasons.

the image of my role in the institution I was leaving, and perhaps even more so, the image of my relationship with my friend. It was this refusal to acknowledge an irrefutable reality, and the anguish of this contradiction to which I was clinging, that made the episode of this "salutary uprooting" so painful at the time[29] (*).

To tell the truth, since I'd never written anything about this relationship (apart from the beginnings of a few episodic letters to my friend, none of which got any response. . .), I hadn't realized before that the first signs (discreet, of course, but which can't be mistaken) of ambi- valence in my friend's relationship with me go back at least to 1968.), I hadn't yet realized that the first signs (discreet, admittedly, but unmistakable) of ambivalence in my friend's relationship with me date back at least to 1968, two years before "Le grand tournant". It was a time when the relationship seemed perfect, a mathematically unclouded communion, in the context of a simple, affectionate friendship. It's all very well to mock the beautiful "tartines" about innocence, the creative child and so on!

Yet I know that this communion was a **reality**, by no means an illusion; just as this "delicate thing" was a reality - this creative force, of which the work that followed gives only a pale re- flet. "Innocence" and "conflict" are two tangible realities, recognizable to the slightest awakened perception, by no means concepts; and they seem to me by nature foreign to each other, one excluding the other. Yet there's no doubt that these two realities coexisted in my friend's relationship with me, at different times of the year.

p. 236 different levels[30] (**). It doesn't seem that at the time I'm talking about here, "conflict" interfered with mathematical creativity - at least not in the work done in solitude, or the work done in interviews face-to-face. It's also true that in the two articles I've just mentioned, which after all are among the most tangible fruits of this work, the imprint of "conflict" is already clearly visible. And with the benefit of fifteen years' hindsight and the reflection of days and weeks gone by, I can see that this imprint (however discreet it may be) strikingly prefigures the particular form that this gradual grip of conflict on the initial impetus was to take, stripping it over the years of its rarest essence - that which makes for great destinies(*).

Note 63_1 (May 26) Compare also with the remark in footnote[31] (*) at the end of note 60, noting the "blockage" of the natural development of Hodge-Deligne theory, as a result of attitudes of rejection towards certain key ideas introduced by me (here, the six operations - to which the motives are indissolubly linked), of the same nature as that examined here, apparent therefore from the publication of Hodge Theory I and II.

The same attitude, striving as far as possible (if not beyond!) to erase all traces of my influence, can be found in the work (already mentioned in note n° 47) written in collaboration with Mumford, on Mumford-Deligne compactifications of modular multiplicities. (This work also predates my departure.) The work uses a principle of passing topological results on

[29](*) See note no.° 42.

[30](**) On two or three other occasions, I have witnessed such coexistence in the same person at a given time, y including myself at times.

[31](*) Such lofty lyricism has made me lose touch with down-to-earth realities. If I describe this "imprint" as "discreet", it's because I'm wrapped up in a layer myself, and I find it hard to separate myself from the blinders I still hold dear! Having finally got rid of them, I realize that the "imprint" in question is a crude concealment, which I didn't want to see because of a certain complacency in myself, which I clearly realize in the note of June 1 "Ambiguity",

n° 63". As for the "grip of conflict on the initial impetus" of my brilliant young friend, I speak of it almost as a regrettable fatality of which the poor man would be the unwitting victim, losing in the process, alas, the benefit of "great destiny". Yet he is responsible for his own destiny, just as I am for mine. If, even before my departure, he chose the role of his master's gravedigger (for a start), and if circumstances (including the spirit of the times) were conducive to this choice, granting him the role of Big Boss to whom all blows are permitted, he also chose to taste to the dregs the privileges that prestige and power can give, including that of crushing (discreetly) and despoiling. You can't have it all, and it's in the nature of things that by this choice (in which he's in good company) he loses the benefit of more delicate and less sought-after things. ... (Undated footnote, early June).

the body C (known by transcendental means) to results in car. $p > 0$, which I had introduced in the late fifties' for fundamental group theory. By the early sixties, I had suggested p. 237
to use this method to prove the connectedness of modular varieties in any characteristic[32] (*). However, this idea ran up against technical difficulties that had stopped Mumford, and which were elegantly overcome in their work by the introduction of modular **multiplicities,** and a "compactification" of these that has perfect properties. The very idea of modular multiplicities can be found, "between the lines" at least, in my "Teichmüller" talks at the Cartan seminar, given at a time when the language of sites and topos did not yet exist. The very language used by Deligne ("algebraic stack") where there was a whole language of sites, topos, multiplicities tailor-made to express this kind of situation, shows quite clearly (with hindsight and in the light of much larger later "operations") the intention to erase the provenance of some of the main ideas implemented in this brilliant work. It was surely this attitude (as I first sensed in the note "Refus d'un héritage - ou le prix d'une contradiction", n° 47) that had a "chainsaw effect", cutting short further reflection on modular multiplicities, which nonetheless appear to me to be among the most beautiful and fundamental of all "concrete" mathematical objects identified to date.

In passing, I'd like to point out that the arguments I introduced in the late 1950s make it possible (thanks to Mumford-Deligne compactification) not only to prove the connectedness of modular multiplicities in any characteristic, but also to determine their "*p-first* fundamental group", as the "*p-first* profinite compactification" of the ordinary Teichmüller group.

14.2.5. The ascent

Note 63' (May 10) With the additional hindsight of less than three weeks, I now realize that this attitude, which was intended to be "understanding" in relation to this "quite natural" intention to distance oneself, was in reality a lack of clear-sightedness and complacency towards my brilliant young friend. If I had relied on my healthy faculties of perception, instead of letting myself be dazzled and given the lie to vague clichés posing as "understanding" or even "generosity" ("I'm not going to give him a hard time just because he doesn't put my name up. . . ."), I would have realized that my friend's attitude was one of "understanding" and "generosity". "), I would have realized then what I realize now, sixteen years later. I could call it a lack of probity.

vis-à-vis the reader, vis-à-vis myself and vis-à-vis himself. Seeing things simply and unafraid to call them by name, I would have been able to talk about them simply, as I am now, and my p. 238
friend had the opportunity to learn from them - or at least he would have understood that even with the means at his disposal, his elders (or at least one of them) expected him to show the same probity in his work as they did themselves. So I can see that on that occasion, before my departure from the mathematical scene, at a time when I was by no means "out of the game" and undoubtedly exercised a certain moral ascendancy over my young friend, I failed to live up to my responsibility towards him, through the **laxity** I displayed at the time[33] (*). This was confirmed by the publication of "Hodge Theory II", Deligne's thesis work, in which he makes no reference to my motives or to me. It's true that, even then, the mathematics and the very person of my friend were very far away and appeared to me as if through a fog!

[32](*) (September 1984) verified, this circumstance is indeed mentioned in the introduction to the work cited (p. 75).

[33](*) (May 28) The word "complacency" here better expresses the nature of my attitude, than the somewhat elusive word "laxity". This complacency in my relationship with my young and ; brilliant friend became clearer to me in yesterday's reflection, see the note "L'être à part", n° 67'.

In the light of what I've seen of my friend's development, both spiritual and mathematical (and the two aspects are closely intertwined), I can see that when I first met him and was impressed by his intellectual means, his acuity of vision and his liveliness of understanding in mathematics, I could discern no lack of maturity in him ; nor (subsequently) the effects of his meteoric social rise, in the space of just four years, from unknown student to mathematical star and tenured professor, vested with considerable privilege and power, at an already prestigious institution. I have no regrets about facilitating his ascent and speeding it up - but I can see that it was due to a lack of discernment and maturity on my part. The "favor" I did him was not a favor. It won't have been a "service", at least until my friend himself has completed this harvest, which he prepared with my carefree assistance.

14.2.6. Ambiguity

Note 63" (June 1) In the three weeks since this observation of "laxity" (or "complacency", to use the more appropriate expression that has appeared in the meantime) in my relationship with my friend Pierre, I have had the opportunity in my reflection to realize more clearly a certain lack of rigor, a complacency in myself. They manifested themselves in my relationship, first of all, with the one I more than

but also to other mathematicians for whom I was the eldest. What I have detected justifiably so far in this
p. 239 sense has been expressed by a certain ambiguity in me, and without
I'm sure I was also a pupil, in situations where my pupil took on ideas and methods he'd learned from me, or even a detailed master builder of a whole body of work he was doing, without clearly indicating its source or even alluding to it. Such situations were quite common in the sixties, after my departure and right up to the last few years. It seems to me that in all these situations, at some level I sensed the ambiguity, which was expressed by a shadow of unease, never examined until these very last days. The motivation that made me play along with a certain connivance, and that made me pass over this malaise without ever paying attention to it, was to **conform to** a certain image I had of myself, and of what so-called "generosity" should be. True generosity is not born of conformism, of a concern to be (and appear, to oneself and others) "generous". The repressed discomfort was always a clear sign that this "generosity" was fake, that it was an **attitude,** not the spontaneous, unreserved gift of true generosity.

In this malaise, I see two components of different origins. One comes from the "boss", the "me" who remains frustrated, because he hasn't been able to have it both ways: taking credit for a job he knows he's done (to a greater or lesser extent), and at the same time living up to a certain brand image, which includes (among many other things) the eponymous label of "generosity". The other component comes from
"the child", of the one in me who is not fooled by attitudes and facades, and who has the simplicity to feel what this situation has of false[34] (*). Not only false towards myself, but also vis-à-vis the other.

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[34](*) (June 5) When I say here that the discomfort comes (in part) from "the child", it's a way of speaking that gives a false image of reality. It's not the candid perception of a false situation that creates discomfort. The discomfort is the sign of **resistance** against this perception, of a take-off between the reality actually perceived at a certain level (in this case, that of a false situation), and an **image** of reality to which I cling (in this case, that I'm being "generous" and that I couldn't do less!), to the benefit of which **I dismiss**, I repress the unwelcome perception. In this case, as soon as I abandon resistance and allow the perception to appear in the field of conscious gaze, the "discomfort" has ceased, along with the false situation. I was going to add "assuming it's a false situation involving my present, and not a situation in the past". But on reflection, I realize that these false situations "from the past", of which I have just spoken, have remained present as such until today, or at least until the reflection of three days ago,

In short, my "generosity" consisted in entering into a game where the other presents as his own ideas that come from others, and thus gives an image of himself and of a certain reality, which he and I both know is false. So we're in this together, in what we might call a "cheating" game, in which both he and I have had our share. It's a "cheat" at least according to the consensus that prevailed "in my day", and which, it seems to me, is still being paid lip service to today. Surely I would not have entered into such a game if it had been a question of someone else's ideas being used as if they had been found by my "protégé"[35] (*). However, the fact that I tacitly agree to my own ideas being presented as someone else's does not, it seems to me, change the essential nature of the thing - the only difference is that in this case there are two of us cheating, instead of just one. And even apart from this aspect concerning myself (that I myself am taking part in cheating, in behavior that goes against the very consensus I claim to adhere to), it's quite clear that there's no generosity in encouraging others to cheat (even if it looks like we're doing it at our own expense - which is in no way the case), or at the very least to adopt an attitude of ambiguity towards a consensus to which they too pretend to adhere, while at the same time violating it. True generosity is by nature beneficial to all, starting with the person in whom it manifests itself and the person to whom it is addressed. My ambiguous attitude, arousing or encouraging ambiguity in others, and allowing myself to pose as "generous" when, logically, the other must appear to be a bit of a cheat (and in fact we both are) - this attitude is a benefit neither to me nor to the other.

All I had to do was examine the matter and the obvious would become apparent, without even having to refer to an ex- perience, a "lesson from events". Yet it was events that brought me to this point. examination, finally making me discover something obvious that I was just as capable of discovering thirty years ago,

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before another student appeared on the horizon to learn a trade with me, and to imbibe a certain spirit in the exercise of that trade. I've had occasion to talk about the "rigor" in the work itself, which I believe I demonstrated (see the section "Rigor and thoroughness", n° 26). But today I've also noticed, outside the "work" itself, an absence of rigor, expressed in the ambiguity and complacency I mentioned earlier. It seems to me that this ambiguity in me was not communicated to me by any of my elders, all of whom (I believe) were as demanding of me as they were of themselves. Beyond the ambiguity of the particular attitude, I detect an ambiguity in my own person, which I had occasion to mention more than once in the first part of Récoltes et Semailles. This ambiguity began to be resolved with the discovery of meditation in 1976, although some of the signs of this ambiguity, expressed in attitudes and behaviour that have become habitual (notably in my relationship with my students) must have persisted to the present day.

Clearly, this ambiguity within me has found fertile ground in some of my students. What was done by tacit agreement has even become, it seems, a fundamental note in the mores of the mathematical "big world" today, where fishing in troubled waters (with or without the agreement of the "interested party"), or even plundering (when the one who allows himself to do so is part of the intangible elite), seems to have become such a common practice that nobody seems to be surprised by it anymore, even though everyone is careful not to talk about it. The "boss" in me

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by the very fact of never having been examined and thus resolved. I remained a prisoner to the point of mechanically reproducing the same situations as soon as the opportunity arose. The knowledge of my meditation "power" (which I mentioned in the section "Desire and meditation", n° 36) was of no use to me, as I was unable to pay day-to-day attention to the situations in which I found myself.

I'm involved, and in the incessant game of perception and "sorting" of perceptions, this game of the child and the boss silencing him. ...

[35](*) This expression "my protégé", used by one of my former pupils to refer to one of my current pupils who had just done some great things in mathematics, made me cringe. And yet, the ambiguous situation I'm now examining, on balance, establishes a false relationship in which one of the two protagonists does indeed act as the other's "protégé".

would like to stand out, to denounce, to take offense - and yet in doing so, I'm only perpetuating the same ambiguity in myself that I can now see has proliferated.

14.2.7.

Note 63''' (April 24)[36] (*) Flipping through an offprint of Mebkhout that I had just received two days ago, I came across a reference to a work by J.L. Verdier entitled "Catégories Dérivées, Etat 0" published in SGA $4\frac{1}{2}$(Lecture Notes n° 569, pp. 262-311). I apologize for not realizing this earlier.
publication, having never before had the honor of holding this volume in my hands, of which neither
p. 242 Verdier nor Deligne (who is the author) have seen fit to send me a copy, on its publication or later.
I don't know whether C. Chevalley and R. Godement, who with me formed the jury that awarded J.L. Verdier the title of "docteur es sciences" on the strength of a 17-page introduction (still unpublished), were themselves entitled, ten years later, to receive "L'état 0" (50 pages this time) of this "thesis" like no other! I seem to remember once holding in my hands a serious work of foundations of some hundred pages, which could reasonably pass for a good doctoral thesis, and which corresponded roughly to the work of foundations I had proposed to Verdier around 1960 - except that it had already become clear by then that the framework of "triangulated categories" developed by him (to express the internal structure of derived categories) was insufficient.

Needless to say, my name is nowhere to be found in this "State 0" of a thesis. Indeed, one wonders what it would have to do with it. It's well known that the derived categories were introduced by Verdier, to enable him to develop the so-called "Poincaré-Verdier" duality of topological spaces, and the so-called "Serre-Verdier" duality of analytic spaces, while waiting for a vague unknown in the service[37] (*) to develop a synthesis of the two on his behalf, appropriately called (the Elève Inconnu could do no less!) the "Poincaré-Serre-Verdier duality". After all that, all I had to do was follow suit and make the necessary adaptations to develop the Poincaré-Verdier duality and the Serre-Verdier duality within the very specific framework, my faith, of the coherent cohomology of schemes... .

I've only just become aware (libraries are useful!) of SGA $4\frac{1}{2}$[38] (**), in which I've again been honored to be listed as Deligne's co-author, or rather "collaborator" (sic) (without seeing fit to inform me, let alone consult me). This was obviously a precursor to the memorable "volume enterrement" published five years later, which I had the pleasure of reading a few days ago (see notes n° 50, 51 and following, inspired by the event). But I didn't get to hold the pre-burial volume in my hands, with this piece of evidence of a phantom thesis that doesn't say its name
name, to understand as early as last year that the next state of this "thesis" would never be written by anyone but myself. And so I set to work on La **Poursuite des Champs**, where
p. 243 seventeen years ago.

[36](*) This note comes from a footnote to "L'instinct et la mode - ou la loi du plus fort" (n° 48) - in which I stated that Verdier's work on derived categories had never been published, without realizing that a "Etat 0" of his thesis had appeared in 1977. For an overview of Verdier's strange twists and turns in relation to the theory that was supposed to constitute his thesis work, see the note "Thèse à crédit et assurance tous risques", n° 81.

[37](*) See the note "L'inconnu de service et le théorème du bon Dieu" for some information on this dubious character. (note n° 48').

[38](**) see note on this volume, "La table rase", n° 67.

14.2.8. The investiture

Note 64 (April 25) Yesterday, however, I found a copy in my office at the University. In fact, it's two consecutive reports, written a year apart in April (?) 1968 and April 1969. In seventeen pages, I review fifteen pieces of work carried out over three years of scientific activity at IHES. These included work on Ramanuyam's conjecture, the compactification of modular sites, and the extension of Hodge's theory. The body of work reviewed in this report (if only the works I have just named) bears witness to a prodigious creativity, unfolding with perfect ease, as if at play. Leaving aside the demonstration of Weil's conjectures, still in the wake of this first plunge into the unknown, it seems to me that the subsequent work gives only a pale image of this unique flight of a young mind with exceptional means, and also benefiting from exceptional conditions for its blossoming. Yet something about these "exceptional conditions" must have nourished this other force, foreign to the drive for knowledge, which ended up taking over and supplanting it, diverting and absorbing the initial impulse. And obviously, this "something" was also linked to me... . [39](*)

This short report with commentary (which I intend to include as an appendix to the present volume) seems to me to be interesting in more ways than one, including from a mathematical point of view (although some of the work reviewed remains unpublished to this day). In several places in the report, I anticipate that such work, which Deligne had confined himself to outlining and dealing with the crucial points, will be developed further by
future students. These students never appeared, given the changes that subsequently took place in
its relationship to the common man[40] (**) Of the ideas I review, the only one to my connaissance p. 244
that was developed by someone else (who would thus appear to be a pupil of Deligne) was the theory of cohomological descent, developed by Saint Donat in SGA 4 (so still in the period of the initial impetus), a theory that has since become one of the most commonly used tools in the cohomological arsenal.

Amusingly and characteristically, for three of the four works that have since been the subject of articles by Deligne[41] (*), I take touching care to make clear, in passing, the relationship of these works to ideas I had introduced and questions I had raised - as if to pre-empt, one would say, the silence the author was going to make about them in his articles (none of which had appeared, or even, I believe, been written, at the time I reported).

14.2.9. The node

Note 65 (April 26) It's clear, too, that keeping a large-scale "yoga" (that of weights, and beyond that, that of motives), about which I had spoken here and there to others than him, but which he was the only one to know about, is not a good idea.

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[39](*) (May 26) On the subject of a certain complacency within me that gave rise to this "something", see the note (two weeks later than the present one) "L'ascension" (n◦ 63').

[40](**) In the days when I worked with him regularly at IHES (in my seminar in particular), Deligne's relations with the other
The kindness of the mathematicians, especially the young researchers (often beginners) who came to the seminar, was unmistakable. I noticed the same openness to other people's thoughts, even if they were awkward or confused, as in our mathematical tête-à-tête. He had that ability to follow the thoughts of others in their images and language, which I've always lacked, and which (it seems to me) predisposed him much more than me to the role of "master", able to stimulate the blossoming of a vocation, a creativity in others.

[41](*) The only one of the four works in question not directly influenced by me is the one on Ra- manuyam's conjecture, deducing it from Weil's conjectures. It takes place in a research direction (that of modular forms) that constituted one of the most serious "holes" in my mathematical culture. The other three works are those on the degeneracy of the
cence of Leray's spectral suite, on Hodge-Deligne theory, and on modular multiplicities (in collaboration with Mumford), discussed in the note "Eviction" (n◦ 63) and in sub-note n◦ 63_1 .

to have assimilated it intimately and to grasp its full scope, conferred on him an additional "superiority", as the exclusive possessor of an incomparable instrument of discovery for an understanding of the cohomology of algebraic varieties. However, I don't think that this temptation played a decisive role, at a time when I was still very much present and active in the mathematical world, and when there was nothing to foreshadow my departure sine die. She must have appeared with or after my departure, which was an unhoped-for "opportunity" to seize an inheritance (which was rightfully hers!), hiding both the inheritance and its provenance. It is here that I see once again, in an extreme and particularly striking case, the crux of a profound contradiction, which goes far beyond any specific case. I want to bet on the ignorance, the disdain, the deep-seated doubt that surrounds the creative force that lies within our own person - that heritage. unique and of greater value than anything a person could ever pass on. It is this ignorance, this insidious alienation from what is most precious, most rare within us, that makes it possible for us to
p. 245 envy the strength perceived in others, and covet for ourselves the fruits and outward signs of this strength in others that we have forgotten in ourselves. As soon as this envy, this desire to **supplant,** takes root and finds an opportunity to proliferate, as soon as it channels the energy available for creative fulfillment, this alienation within us deepens, settles in permanently. The closer we come to the coveted "goal" of supplanting, crowding out, dazzling, the more we distance ourselves from and cut ourselves off from this delicate force within us, and clip the wings of our own creative impulse. In our tenacious effort to rise, we have long forgotten that we are meant to fly.

In his relationship with me, from the day we met, I felt my friend was perfectly at ease, without any sign that he was in the least impressed or dazzled by my reputation or person, or that there was any unspoken doubt in him, whether about his gifts or faculties in the mathematical field, or about anything else. It's also true, it seems to me, that he had received a friendly and affectionate welcome from me and my environment, including my family, which was likely to put him at ease. But the simple, seemingly unproblematic naturalness that drew me to him as it drew others to me, had surely not waited for this encounter to appear and blossom. The impression he gave off, which made him so endearing, was one of harmonious balance, where his penchant for mathematics was in no way a devouring goddess. Next to him, I was a bit of an unrepentant "polard", not to say a "thick brute" - and I remember his discreet astonishment at my lack of deep contact with nature around me and the rhythm of the seasons, which I passed through without seeing anything, as much as saying... .

Yet this profound "doubt", which I would have been incapable of perceiving then (or perhaps even today, in similar circumstances), must have been present in my friend long before we met. Looking back, I can see the first unambiguous sign of this as early as 1968, and even clearer signs in the years that followed[42] (*). These are "indirect" signs, however - none of the ones I've been able to

First-hand observation doesn't come in the form of doubt, a lack of assurance - rather, and increasingly over the years by what may seem the opposite: a smugness, a propos deliberate disdain, even contempt. But such an "opposite" reveals its opposite, with which it forms a pair and of which it is the
p. 246 shadow.

I also heard through an intermediary that, for a prestigious (and notoriously awkward) mathematician whom he had never had the opportunity to meet on a personal level, he would have been in great tension at the prospect of a meeting, in a sort of irrational fear of not being considered by the great man as worthy of his own greatness. This testimony was so contrary to what I myself had seen in my young friend, that I found it hard to believe (this was in 1973). In retrospect, it

[42](*) (May 10) In fact, another "very clear" sign dates back to 1966, see footnote (*) to note no.° 82 (p. 329).

However, the signs of division that I'm aware of elsewhere all point in the same direction.

This division, and the role I played as a sort of fixer of a conflict that was undoubtedly diffuse before our meeting, would probably have remained hidden in the usual circumstances of the evolution of a relationship with someone who was (in one sense or another) a "master", or at least someone who transmits or confides. In this way, my departure will have **revealed** a conflict unknown to all, and which perhaps only I know about.

And my "return" today is a second, more untimely revelation. I can't imagine what it will reveal to me, beyond what it has already taught me about my own past and present, and about the people I have loved and to whom I am still linked today. Nor what it will reveal to the person who, for the past week, has been at the center of this final stage of my reflection, which I called last month (and I didn't think I was saying it so well. . .) "**the weight of a past**".

14.2.10. Two revolving

Note 66 (April 25) This deliberate disdain and antagonism in my friend Pierre's relationship with me has been confined exclusively to the mathematical and professional level. The personal relationship has remained to this day one of affection and friendly respect, manifested more than once by delicate attentions that have touched me, surely signs of genuine feelings without ulterior motive.

In the intense years that followed my departure from the IHES, this episode faded into oblivion, as did the long misunderstood teaching it had given me. So, for more than ten years, my friend remained for me (as a matter of course) my privileged interlocutor in mathematics.
tics; or to be more precise, between 1970 and 1981 he was the only interlocutor (apart from one episode) with whom I consider addressing me during periods of my sporadic mathematical ac□tivity, when the need for a p. 247
the interlocutor was felt.

It was also to him, as the mathematician closest to me, that I turned just as spon- taneously on the first occasions (between 1975 and 1978) when I had to ask for assistance, surety or support for students working with me. The first of these occasions was the defense of Mrs. Sinh's thesis in 1975, which she had prepared in Vietnam under exceptionally difficult conditions. He was the first person I contacted to sit on the thesis jury. He declined, suggesting that it could only be a bogus thesis, and that he had no intention of endorsing it. (I did, however, have the skill to circumvent the good faith of Cartan, Schwartz, Deny and Zisman to lend me a hand in this deception - and the defense took place in an atmosphere of interest and warm sympathy). It took three or four similar experiences over the next three years before I finally understood that my prestigious and influential friend was deliberately antagonistic towards my "post-1970" students, as well as towards work that bore only the stamp of my influence (at least that undertaken "after 1970"). I don't know whether the attitudes of overt contempt that I witnessed on several of these occasions are also to be found to a greater or lesser extent in his relationship with other mathematicians whom he considers to be far below him. The very spirit of a certain elitism he prides himself on professing would lead me to suppose so. The fact remains that since 1978 I have refrained from addressing him on any matter whatsoever. This has not prevented his power to discourage from manifesting itself effectively.

It was also around the same year that the first signs, discreet at first, of an attitude of disdain towards my own mathematical activity appeared. The first occasion had been my reflection on cellular maps, after a discovery about them that had flabbergasted me (see Esquisse d'un Programme, par. 3: "Bodies of numbers associated with a child's drawing"). This discovery (admittedly "trivial", and which had nothing to move or even interest my prestigious friend) was the starting point and first material for that other mathematical **dream,** of comparable dimensions to that of the motifs, which began to take shape only three years later (January-June 1981), with "La Longue Marche à travers la théorie de Galois". These notes and others from the same period (some two thousand handwritten pages) constitute a very early version of his work.

p. 248 tour through this "new continent" that a trivial remark on a child's drawing had given me a glimpse of.

In the course of this intense work, I wrote two or three times to my friend, to tell him about some of my ideas, and occasionally ask him questions of a technical nature. When it pleased him to speak to my questions, his comments were always as clear and as pertinent, and bore witness to the same "means" that had impressed me even at his young age. But a smugness had dulled the eagerness to understand that had enchanted me then, and the ability to apprehend great things through "small" things, as well as to apprehend or conceive great designs, by listening to each other. This ability is not a matter of intellect, of simple "efficiency", or of "mastery" of an already established discipline or known techniques. It's a reflection, at the level of the intellect, of something of an entirely different essence - of the child's **gift of wonder.** This gift in him seemed extinguished, as if it had never been. It was so at least in his relationship with me, after it had been so first in his relationship with my "later" students. He had become an important man, and his approach to mathematics had become neither more nor less than that "sporting" attitude which I first examined only a month or two ago, and to which I myself was by no means a stranger. ...

Perhaps I would have been able to come to terms with the obvious absence of this communion in a shared passion, this deep bond that had once bound us together. I would no doubt have been content to submit (when the opportunity arose) more or less technical questions or simple requests for information to my friend's astuteness and vast knowledge of the world of mathematical things. But in that year (1981) the signs of this disdainful affection suddenly became so brutal[43] (*), that I lost all interest in communicating with him again on mathematical matters, even occasionally. (⇒ 67)

14.2.11. The table shaved

Note 67 (April 26) It was while I was writing the preceding lines yesterday that I made the connection between this new turning point in our relations and the publication in 1982 (practically at the same time as this dramatic turnaround) of the "remarkable volume" of Lecture Notes, which consecrated my mathematical funeral without flowers or wreaths! At a time when I had been declared mathematically "dead", it was a kind of grace for my friend to continue answering mathematical questions here and there, which, in the end, were still valid,

p. 249 were no longer relevant...

Essaying to tune into the meaning of events, I get the feeling that this isn't a ha- either.

sard si la première apparition d'un dédain, d'un désintérêt mathématique (vis à vis de choses, de plus, dont son "sain instinct" mathématique devait lui dire qu'elles étaient brûantes et ju juuuses), dans sa relation à ma propre personne tout au moins, se place à peu près le moment de la parution du volume de pré-enterrement SGA $4\frac{1}{2}$, cinq ans avant[44] (*). The circumstances surrounding the publication of this volume already bear witness

[43](*) (May 28) For new insights into this second turning point, see also the note "La Perversité", n◦ 76.

[44](*) On this subject, see the note "Le compère" (n◦ 63''') of the day before this one.

(June 5) The reflections of this note are taken up in this note and the three that follow ("La table rase", "L'être à part", "Le feu vert", "Le renversement"), which hint at the meaning of "operation SGA $4\frac{1}{2}$ " and its link to the "dismantling" of the mother seminary SGA 5. This reflection is taken up again in the "My students" procession, and in particular in the continuation "My students (1)-(7)", where little by little the picture emerges of a veritable massacre of the seminary where my cohomology students learned their trade.

The fact that I was introduced as Deligne's "collaborator", without consulting me or even informing me, and without sending me a copy, is in itself more eloquent. The mere fact of introducing me as Deligne's "collaborator", without deigning to consult me or even inform me, and without even sending me a copy, seems to me in itself more eloquent than a speech. Not to mention the fact that Deligne's book was essentially intended to make more accessible to a wider public the work I had developed over fifteen years earlier, at a time when I had not yet heard the name of my brilliant friend! This disdain, and later arrogance, must have been fuelled, on the one hand, by my absenteeism, which meant that I wasn't aware of anything and was, in fact, "cashing in" without knowing it; but also by a certain climate, which meant that this kind of misunderstanding could "pass", without apparently eliciting the slightest comment. The fact remains that I have not received a single echo from anyone (particularly among the many friends I had thought I still had in the world of mathematicians) about this volume, nor about the burial volume it has prepared.

In the introduction, the author doesn't beat about the bush to set the scene. The aim of the volume is to spare the non-expert "recourse to the lengthy presentations of SGA 4 and SGA 5", "to prune out unnecessary details", "to enable the user to forget SGA 5, which can be considered as a series of digressions, some of them very interesting" (how nice of these "digressions"!). The existence of SGA $4\frac{1}{2}$ "will soon make it possible to publish SGA 5 as it stands" - a mysterious assertion, since one wonders how this publication (of something one is advised to forget), which had already dragged on for a dozen years, and which presented a perfectly coherent set of results (and which had not waited for Deligne to be identified and proven) could be subordinated to the existence of SGA $4\frac{1}{2}$ (*). p. 250

In asking this question, I also see a simple answer, and a possible explanation for the vicissitudes of this poor seminar SGA 5 (68), (which I had developed at length in 1965/66, eleven years before the publication of Deligne's volume SGA $4\frac{1}{2}$)[45] (*). The first hint of this can already be seen when it is stated (page 2) that in the original version of SGA 5 "the Lefschetz-Verdier formula was established only conjecturally" (which is harsh for Verdier, who is supposed to have been able to prove his theorem, which predates SGA 5[46] (**)) and that "moreover, the local terms were not calculated". This may seem an unfortunate omission for the non-expert reader (for whom this volume is primarily intended). Readers with a bit of experience know that the said local terms are still not "calculated" today, and that the brilliant and peremptory author himself would be at a loss if asked what he meant in this case (in the general case) by "calculating"[47] (***) (but apparently nobody thought of asking him this indiscreet question).

An ambiguous sentence "this seminar (?) contains another demonstration, it completes, in the case particular Frobénius morphism", seems to suggest that SGA 5 does not give (as one might have expected, for p. 251

In all this, there's a casual contempt, of which the "discreet disdain" (which I saw appear around the same time), in my friend's relationship with me, was only a very pale reflection.

Another association came to me a week or two ago, for the moment of that "first turning point" in my friend's relationship with me, at the end of 1977 or during 1978. It was in 1978 that my friend got his well-deserved "medal" (for proving Weil's conjecture). The way in which this new title (linked to the demonstration of a conjecture "of proverbial diffi culty") was internalized by my friend, is strikingly apparent in the Funeral Eulogy (concerning my late self) and its counter- Eulogy (concerning my late self).

part (concerning his own), published admittedly only five years later on a "grand occasion". See note "L'Eloge Funèbre (1) - ou les compliments", n° 104.

[45](*) See a footnote (April 28) to the note "Le feu vert" (n° 68) for an elucidation of this "mystery".

[46](**) (June 10) For further details, see sub-note no.° (87) to "The massacre" note no.° 87.

[47](***) (June 10) In the general Lefschetz-Verdier formula, for a cohomological correspondence between a bundle of coeffi cients and itself, the "local terms" (corresponding to the related components of the set of fi xed points) are unambiguously defi ned by the very fact of writing the formula. The question of "calculating" these local terms only takes on a precise meaning in special cases, one of the simplest of which is that of the Frobenius morphism, where they are given simply by the ordinary traces of the endomorphisms induced on the fi bres at these points. This formula was fully demonstrated in the oral seminar as a special case of a much more general one.

a volume of digressions!), at the end of the ends, a complete demonstration of the main "result" it announces, a trace formula implying the rationality of L functions à la Weil; fortunately, "this seminar" comes to save, better late than never, a very compromised situation. ...

On page 4, we learn that the aim of the "Arcata" lectures was "to give demonstrations of the fundamental theorems in stale cohomology, rid of the gangue of nonsense[48] (*) that surrounds them in SGA 4". He has the charity not to dwell on this regrettable nonsense that is rife in SGA 4 (such as topos and other similar horrors - the reader can flatter himself that he has escaped it by the providential appearance of this brilliant volume, finally making a clean sweep of the regrettable "gangue" that had preceded it. ...) (67) ($67'_1$).

As I've just gone through the introduction to the volume and the introductions to its various chapters, I've re- produced the assessments and declarations of intent that seem to me to most clearly announce the color, among two or three others (style: digressions, admittedly, but "very interesting") that seem to me intended above all to "pass the pill" (which has indeed passed without a problem). For example, the author is honest enough to state at the outset that "for complete results and detailed demonstrations, SGA 4 remains indispen- sable". This volume, however ambiguous in spirit and motivation, is not a scam[49] (**). Its role seems to me to be more that of a sounding board, obviously conclusive: there was really no need to bother!

There's a kind of **escalation in absurdity** (seemingly unnoticed by all!) from one volume to the next.
p. 252 prepares (APG $4\frac{1}{2}$, and LN 900). In both cases, we see a man of impressive means, made for discovering and exploring and probing vast worlds, set about "redoing" the work of a predecessor, first myself, then a former pupil of mine (Saavedra), when in so doing he had nothing essential to contribute to the work of these predecessors, which had been done with care and by getting to the bottom of things. (What he contributed in total could be set out in some twenty or thirty pages, it seems to me.). In the first case, the reason given was plausible: to give the non-expert user tear-free access to[50] (*), without having to rely on the voluminous SGA 4 and SGA 5 seminars. (It's the first time, however, that we've seen the author show such concern for the common man, taking precedence here over the pleasure of doing math. ...) The second time, the work consisted almost entirely of **copying** the thesis Saavedra had done with me! This thesis was a perfect reference, and the fact that the demonstration of one statement in it was false, and that another statement contained an unnecessary hypothesis, was surely no reason to rewrite the whole article. Of course, no "reason" was given for such a strange thing.

[48](*) In my day, the term "general non-sense" did not have a pejorative connotation, but rather a slightly jokey, good-natured one. It's no coincidence that the adjective "general" has been "forgotten" here, to mean "non-sensé", which in good French means neither more nor less than non-sens, and suggests the idea of bombast, of "bullshit".

[49](**) (May 26) See, however, the following day's note, "Le renversement" (n◦ 68'), where I go back over this impression, which turns out to have been hasty= In further reflection, a large-scale "SGA $4\frac{1}{2}$ - SGA 5" operation was gradually revealed, for the "benefi œ" mainly of Deligne, with the help or tacit agreement of all my "cohomologist" students, "The honesty" that I believe I can observe (on the strength of the statement, in line 7 of the introduction, which has just been quoted), plays here the role of
the "punch line" designed to give the impression of a "thumb". My friend used this style as early as 1968 (see "Poids en conserve et douze ans de secret", and "L'éviction", notes n◦ 49 and 63). See also the notes "Pouce!" and "La robe de l'Empereur de Chine", n◦ 77 and 77' .

[50](*) (June 10) When I wrote this note, I had only just "landed" and hadn't yet grasped the true meaning of "operation APG $4\frac{1}{2}$ " (and its link with the vicissitudes of SGA 5, of which I had only just had a sudden foreknowledge). I've since realized that the motley collection of texts published under the misleading name of SGA $4\frac{1}{2}$ (see the note "Le renversement", n◦ 68') is in no way intended as a popularization ("without tears") of the SGA 4 and SGA 5 seminar (which forms the core of my "SGA"). published mathematical work), but that it represents a manoeuvre to replace it (acting as a precursor that's a bit muddy around the edges), and to appear as the true masterwork on stale cohomology, which would be due to
to Deligne. For a striking formulation (by an anonymous writer) of such an imposture, six years after the "coup de sonde" named SGA $4\frac{1}{2}$, see "L'Eloge Funèbre (1) - ou les compliments" (note n◦ 104).

Yet I didn't have to hold SGA $4\frac{1}{2}$ in my hands to feel the meaning of this seemingly absurd thing: Deligne "redoing" Saavedra's thesis, ten years later! It's surely the same as the meaning of that scarcely less absurd thing that had prepared it: Deligne doing (twelve years later) a "digest" (a little condescending around the edges), of a certain part of Grothendieck's published work. This is precisely the part of Grothendieck's work he can't pretend to do without, if he continues to be interested in the cohomology of algebraic varieties (from which he can't seem to detach himself). And Saavedra's thesis is the work of all, published and bearing the mark of my influence, which he can in no way do without, if he wants to take up "on his own account" the notion of motivic Galois group that I had 'developed, and finally exploit p. 253
(fifteen years later!) this obviously crucial notion. First with SGA $4\frac{1}{2}$, and five years later with the landmark Milne-Deligne (alias Saavedra) article in LN 900, my friend indulged in an illusory sense of liberation from something he surely felt a painful obligation to do: to have to constantly refer to the very person he was trying to supplant and deny, or even to such and such another who referred to him.

To arrive at this intimate conviction about the common meaning of these two "absurd" acts, I didn't need to go through all my prolific friend's (fifty-one) publications, a list of which I received (for the first time) about ten days ago. To tell the truth, I haven't even thought of going back through the four offprints in my possession[51] (*), to seek confirmation of what I think I know. If, in the future, I consult any of my friend's works, it will be to find something other than what I already know. I'm sure I'll then have the pleasure of learning beautiful mathematical things, which I used to have the even greater pleasure of learning in person from him!

Note 67_1 (1) (June 14) I found two other micro-crooks (of detail) in SGA $4\frac{1}{2}$. One is in the "Breadcrumb trail for SGA 4, SGA $4\frac{1}{2}$, SGA 5" (admire the suggestive sequence!), where the author writes (p. 2) that to establish in stale cohomology a "duality formalism analogous to that of coherent duality. ... Grothendieck used the resolution of singularities and the purity conjecture", giving the impression that this formalism was ultimately established only by him, Deligne, in the case (sufficient for many applications) of finite-type schemes on a regular scheme of dimension 0 or 1 (see same paragraph). He knows full well that the formalism of the six variances (i.e. the theory of global duality) was established by me without any "conjecture", and that his restriction is only founded for the biduality (or "local duality") theorem - which, by the way, becomes in SGA 5 (under Illusie's pen) "Deligne's theorem"!

In addition, on page 100 there is a section entitled "The Nielsen-Wecken method", which is the method of which I introduced into algebraic geometry to prove a Nielsen-Wecken-type formula, proven by these authors (in the transcendental context) by a technique of triangulations unusable in the context p. 254
algebraic. Deligne learned about this method (as well as the names of Messrs Nielsen and Wecken, whose fine German article he didn't need to read!) from me, in the SGA 5 seminar of "technical digressions", which SGA $4\frac{1}{2}$ is designed to make us forget! In this section, neither SGA 5 nor I are alluded to, and the reader has the choice, for the authorship of this method, between Nielsen-Wecken (if he's very misinformed) and the brilliant, modest author of the volume.

Interestingly, in this entire volume, Verdier's "Woodshoie" proof for a trace formula including the case I needed (for Frobénius morphisms) is not mentioned. This demonstration (apparently forgotten, in favor of the more general method developed in SGA 5) was the missing link to fully justify my cohomological interpretation of the functions

[51](*) Not counting the works in the IHES Mathematical Publications, which director Nico Kuiper has been kind enough to send me for nearly fifteen years.

L. Clearly, there was an agreement (tacit, no doubt) between Deligne and Verdier - Verdier giving Deligne credit for the trace formula for Weil's conjectures, in return for the part of SGA 5 he had taken over on his own account the previous year (1976). (See note "Les bonnes références" n° 82.) Other compensation: the appearance in SGA $4\frac{1}{2}$ of the "Etat 0" of derived and triangulated categories, from which my name is equally absent. Four years later, under Deligne's pen, the duality of algebraic geometry took on the name of "Verdier's duality" - Verdier had not done a bad job! (See the end of note n° 75 "L' Iniquité - ou Le sens d'un retour").

14.2.12. The being at part

Note 67' (May 27)[52] (*) The passages quoted, like all the circumstances surrounding the publication of this remarkable volume, SGA $4\frac{1}{2}$, testify to my friend's deliberate intention to divert and scorn the central part of my work, represented by the two interrelated seminars SGA 4 and SGA 5. Not the least of these "circumstances", which came to light in the course of reflection from April 24 (see the note "Le compère", n° 63) to May 18 (see the notes "La dépouille. . .", ". . . et le corps", n° 88, 89), was the ransacking of the original SGA 5 seminar, which took the form of the 1977 edition-massacre (see in particular the note "Le massacre", n° 87).

This deliberate attempt at derision on my friend's part takes on its full meaning if we remember that the SGA 5 oral seminar represented the young homme Deligne's first contact with schemas, cohomological
p. 255 techniques
and in particular the duality formalism, and with χ-*adic* cohomology, when he arrived at the IHES in 1965 at the age of 21, with the specific aim of learning "algebraic geometry" with me. It was in this oral seminar, and in the notes of the SGA 4 seminar that had taken place two years before, that he had the privilege of learning first-hand the ideas and techniques that have dominated his work to this day[53] (*).

This essential aspect of the context of the "SGA $4\frac{1}{2}$ - SGA 5 operation", and beyond it, of the relationship between the company and its customers.
even from my friend Pierre to myself, was clearly not present when I wrote the previous note ("La table rase (1)", n° 67), nor in the part of the reflection on the Burial that precedes it. The memory of this "young man Deligne", arriving at the SGA 5 seminar where he still had everything to learn and where he did indeed (and very quickly) learn a lot, only came back in the last stages of the reflection, as if against my will. My deliberate intention, from the very year of young Deligne's appearance in my mathematical "microcosm", not to count him among my students (as if by doing so I would have failed in my obligation of modesty towards such a brilliantly gifted person), made me downplay, or to put it more accurately, totally ignore until these very last weeks, a reality that is nonetheless obvious and tangible, and which is commonly expressed by the double appellation (which I objected to) of "teacher-student"[54] (**). I was happy to forget, to ignore, that there had indeed been a "transmission" of something from me to him, something that for me as for him had great **value**, in a sense that was surely quite different for him and for me. What I was passing on, in those four years of close mathematical contact between him and me, was something in which I had put the best of myself, something nourished by my own experience.

[52](*) This note is a footnote to the previous note "La table rase", of which it is a complement, written one month later to the day.

[53](*) A similar comment can be made for each of my other cohomologist students Verdier, Illusie, Berthelot, Jouanolou - see the note "Solidarity", and the four notes that follow it (notes n° 85 to 89).

[54](**) (June 14) This deliberateness is quite apparent in the way I finally resolve to talk about him (as if in so doing I was violating an obligation of reserve or modesty towards someone who liked to distance himself from me... .) four months ago, in the note "Jesus and the twelve apostles" n° 19.

of my strength and my love - something that (I think) I gave without reserve and without really measuring or even, perhaps, feeling the price.

Surely, what I was giving was fodder for a passion to know in him in tune with the one that ani- me. p. 256
mait - and to **something else** too, which I didn't feel until much later, and without yet linking it to this "transmission" that had taken place and which I was happy to ignore. To put it another way, what I gave was **also** received, at another level that remained hidden from me, not as tools to fathom a fascinating and inexhaustible Unknown, but as **instruments** to supplant (at first), and later to establish a domination, a ruthless "superiority" over others.

Without even taking into account what came back to the "child" in my friend, eager to discover, and what came back to the "boss" in him, eager to supplant, dominate (or even crush), but from the more superficial point of view of the part certain ideas play in a work, techniques, tools - it's been an unexpected discovery over the past six weeks to what extent my friend's work, which took off the year we met, has been nourished to this day by what I'd passed on to him. I had imagined, when I left the mathematical scene fifteen years ago, that "the little" I had given my friend-non-student (a "little" whose role in his impressive initial impetus I could see clearly) would be a springboard for a flight that would take him far beyond his starting point, **away from** my work and my person. What happened, however, was that my friend remained attached to this point of departure to this day, **attached** to the very work that was simultaneously to be disowned, derided or forgotten, and "used". It's a typical case of a conflicted link to one's father or mother, which indefinitely holds one in the orbit of those he or she is destined to leave and surpass, the one who takes pleasure in cultivating this conflict within him or herself, instead of launching out to meet the world. ...

I can see today that by my deliberate intention to treat my young friend as a "being apart", and not simply as one of my students who seemed to have more means than the others - and by my deliberate intention to minimize or forget in my relationship with him the price of what I was transmitting (and the **power** that I was thereby placing in his young hands .) - through these attitudes within myself, I was unwittingly feeding a fatuity and a conflict within him, both of which remained hidden from me. At the same time, I was entering into a certain game - or rather, there was a game between the two of us in perfect harmony, and I'd be hard pressed to say who "started it" (assuming the question makes any sense): myself out of "modesty", claiming that my young friend was far too bright to be anyone's pupil, and that the little I'd been able to bring him wasn't really worth the trouble.
to talk about it - and himself distancing himself (even before I left) from my person and my work, disowning (under my complaisant eye) the soil that had well and truly nurtured it. p. 257

It's only by writing this note that I'm finally seeing clearly this game, of which a diffuse perception had only been present for a week or two. And I also see that this "modesty" or "humility" in me was a false modesty, a false humility: a lack of simplicity, to see things sim- ply for what they are. In this game, there was complacency towards my young friend - seed that proliferated a hundredfold! - and, more subtly, an indulgence in myself, by making a kind of pedestal of a "privileged relationship", extraordinary and all[55] (*). (Just as any lack of simplicity, perhaps, is basically a self-indulgence... .)

[55](*) Compare with the note of May 10 "L'ascension" (n° 63') where for the first time I perceive this ingredient of complacency in what was my relationship with my friend Pierre. This perception had remained isolated and fragmentary until now, when it has been
which is the subject of this note "L'être à part".

14.2.13. The green light

Note 68 (April 27) To tell the truth, I've never given much thought to the meaning behind the strange vicissitudes of the SGA 5 seminar. The oral proceedings in 1965/66 had not given rise to any particular difficulties, whereas the drafting by successive and often failing volunteers dragged on for **eleven years**[56] (**)! It was in 1976 that Illusie finally took matters into its own hands, writing up what was left over and publishing the whole thing. Today is the first time (after almost twenty years since that seminar) that I realize "there's something to understand". Maybe I'm the only one...

The first idea that comes to mind is that among the seminar's more or less active listeners, who were also more or less familiar with the previous seminars SGA 1 to SGA 4, there must have been a phenomenon of **satu- ration in** relation to the tide of "grothendieckeries", breaking over them like a sort of tidal wave without reply[57] (***). Clearly, some of the editors lacked faith, and must not have sensed this very well.
where it was all going, and why on earth I'd been so stubborn, for a whole year, to want to turn it around and
p. 258 around until I'd completely mastered the es sential formal properties of the
cohomology, and the whole arsenal of new notions associated with it. The fact that no trace remains of either the seminar's final lecture, setting out open problems and conjectures (never published to my knowledge), or the introductory lecture reviewing Euler-Poincaré and Lefschetz-type formulas in various contexts, is a particularly eloquent sign of a general disaffection. I don't recall perceiving this disaffection at the time (or even afterwards, until today[58] (*)), as I was so engrossed in my tasks at the time.

The fate of SGA 5, which originally had as strong a **unity as** any of my other seminars, and which was gradually **dismantled** (68) in the eleven years that followed without being written up, could have shown me that the great projects I had so doggedly pursued, and for which I had for some years found arms to assist me, had by no means become a joint undertaking, but remained personal to me. My program gave rise here and there to occasional collaborations, but failed to become a driving force in any of my students at the time - a force that would have inspired him to work on a longer-term, more far-reaching project than the one he had pursued with me in his thesis, whose main role in his life would have been to help him learn the mathematical profession he had chosen.

The only one, it seems to me, to have grasped as a whole (if not made his own) a certain overall vision, going beyond the framework of a particular "collaboration" on such and such a question or for the development of such and such a particular tool, was Deligne. This is why I must surely have seen in him (without ever having to formulate it) much more a designated "heir" than a "pupil". The term "heir" here better captures what I'm trying to express than the term "continuator", which came to mind at first, but which might suggest the idea of a "successor".
of a work that would be limited by a received inheritance. On the contrary, I felt this "inheritance" to be a simple
p. 259 **contribution** I was in a position to make for the deployment of a personal vision, which would be nourished

[56](**) Writing the whole seminar, based on my detailed notes for the oral presentations, would have taken me just a few months.

[57](***) This goes hand in hand with the impression of students who remained "a little dumbfounded", expressed in the letter quoted in the note "Teaching failure (2) - or creation and fatuity" (n° 44').

[58](*) (May 26) After getting back into the swing of things at the SGA 5 seminar, I remembered an impression I'd had for some time. of unease I'd had, when I leafed through (it must have been 1977, the year of its publication) the copy of the published seminar I'd just received. This impression of "mutilation" (which then remained in a diffuse, informal form) was due above all, perhaps even entirely (I must not have spent much time looking more closely, although it would have been well worth it. . .), to the absence of the introductory and final presentations, and above all (I think) to the casualness with which this absence was announced, as something almost taken for granted - why on earth would anyone have bothered to include them! At some level, I must have "sensed something", which I only took the trouble to let rise and examine this month (almost seven years later!), in the note "Le massacre" and in the two notes "La dépouille. .. . ", ". . and the body" that follow it.

of many other contributions (as was indeed the case even before my departure), and which was destined to effortlessly surpass all that had preceded and nourished it.

Returning to the sad fate of SGA 5, the thought that occurred to me yesterday was that this fate was perhaps not unconnected with the ambiguity of Deligne's relationship with me and my work, particularly given the as- cendant that his strong mathematical personality could not fail to exert on all my students[59] (*). I'm sure he must have found some inner satisfaction in the vicissitudes that affected the notes of this seminar, stripped of what made up the unity and impetus of the oral seminar. On reflection, however, it is clear that the primary and es- sential cause of these vicissitudes does **not lie** in the dispositions of a single participant. Without yet clearly discerning this cause, there is no doubt that it concerns above all myself **and** the people who pretended in 65/66 to take charge of editing the seminar. Surely it lies in their relationship to my person, or perhaps also in their relationship to a certain way of doing mathematics (or a certain program, or a certain vision of things) that I embodied for them. The fate of SGA 5 now seems to me to be an eloquent and tenacious **revelation of** something I've never yet taken the trouble to examine, for want of even realizing it, and which even now I'm only glimpsing[60] (**). Perhaps these lines will encourage some of the protagonists of this collective misadventure to share their own impressions with me.

Perhaps there is a lesson, however (at least a provisional one) that I can draw from the episode right now p. 260
SGA 5, which first prefigured, and then illustrated, this spectacular **halt** after my departure, on almost the entire line, from the famous "program" in which I was embarked. Contrary to what I must have more or less believed in the euphoric sixties (happy as I was to have finally found some goodwill to back me up!), it seems to me today that the concretization of a vast per- sonal vision through tenacious and meticulous work cannot be in the nature of an adventure or a **collective** undertaking. Or rather, if there is such a thing as a "collective undertaking", it's not one that would be achieved through ten or twenty (or even thirty) years' work around a single person. If the vision is to become a common heritage for all, it will be embodied here and there under the pressure of needs alone, through the day-to-day work of this or that other person who may only know the predecessor by name (and even then!), whose vision had been too vast for his arms alone to be enough to bring it to fruition[61] (*)

[59](*) (April 28) An eloquent sign of this ascendancy is that SGA 5 was only published when Deligne saw fit to signal to Illusie to take an active interest in it - in other words, at the **precise moment** when he himself needed it as the basic text for his "digest" SGA $4\frac{1}{2}$, destined to replace it. (See the end of the introduction to SGA 5, written by Illusie.) This sheds light on and gives full meaning to this statement (which I still described as "mysterious" only yesterday)

in the "Table rase" note (note n◦ 67)), that "the existence of SGA $4\frac{1}{2}$ will soon enable us to publish SGA 5 as is". The "tel: quel" here is a touch of humor that I was probably the only one to sense (as early as the day before yesterday), and to appreciate at its true value! (Seen on

"dismantling" that the published version represents in relation to the original seminar).

[60](**) (May 26) This is the very "something" referred to in the penultimate footnote, which has come to the surface in the course of reflection over the past few weeks, and especially since the moment (May 12) when I took the trouble, for the first time since its publication, to take a closer look at what had become of "a splendid seminar" in the hands of my cohomology students, in the massacre-edition that had been published, for the first time since its publication in 1977, to take a closer look at what had become of "a splendid seminar" in the hands of my cohomology students, in the massacre edition that was made eleven years later.

[61](*) (April 28) Perhaps "my arms alone" would have been enough to carry out the vast program of work I had in mind towards the end of the sixties, but only if I had made myself the exclusive servant of that program for the next twenty or thirty years. Today, I'm glad I didn't follow that path, which could have been mine, but whose pitfalls and dangers I now clearly see.

14.2.14. The reversal

Note! 68 (April 28) As an example (among many others[62] (**)) of this dismantling, I thought back to the fate of one of the key presentations in SGA 5, which ended up being written by none other than Deligne (who I believe had taken charge of it as early as 1965, to "keep" his commitment eleven years later. . .) according to my oral presentation, only to be incorporated without further ado into SGA 4$\frac{1}{2}$! This is the formalism of the cohomology class associated with an algebraic cycle on a regular scheme, which develops with ease by passing to "sup- ports" cohomology in the support of the cycle under consideration. Like almost all constructions in stale cohomology (useful also in many other contexts, where they have become common practice), I had developed this one at the end of the fifties in the framework of coherent cohomology (here, Hodge and
p. 261 De Rham, which, in the context of "abstract" algebraic geometry, are first studied in one of my early Bourbaki lectures). It is so natural that it self-evidently implies the compatibility usual with cup-products[63] (*).

As I write these lines, I realize that the sleight of hand used to include this crucial talk in SGA 4$\frac{1}{2}$ has led to the brilliant result that Deligne, who did take part in the SGA 5 seminar in 65/66[64] (**), **does not appear** on the cover as one of my "collaborators" (something that had already struck me yesterday, while leafing through the published volume Lecture Notes n° 589) and that I am the one entitled (eleven years after the seminar) to be listed as Deligne's "collaborator". It's quite a **reversal of** fortune, I must say! At the time of publication of SGA 4$\frac{1}{2}$, to which I had unknowingly contributed, I had stopped all public mathematical activity for seven years - so much so, in fact, that I never bothered with the publication of poor SGA 5, which for me was part of a past I'd left behind... .

(April 30) As for SGA 5, it now appears to be a rather motley collection of texts with no tail.
or head (they got lost along the way!), and which only "stand up" with reference to the text APG 4$\frac{1}{2}$. Remarkably, and something I've only just noticed, the very name SGA 4$\frac{1}{2}$ actually suggests
p. 262 that this text **precedes SGA 5, which exists only by reference to it**[65] (***). If the author of this text had been less ambiguous[66] (*), and for sentimental reasons wished to insert his "digest" ("plus a few new results") into the SGA series in which he had played his part, the obvious name would have been SGA 5 1/5.

I see this as a second sleight of hand, which makes me realize that Deligne's share of SGA 5's fate is heavier than I thought even three days ago. It also brings me back to the feeling

[62](**) (May 28) I didn't get round to this "dismantling" until the May 12 reflection, in the note (more appropriately named) "The Massacre" (n° 87).

[63](*) (May 28) In the coherent framework, see my Bourbaki lecture no.° 49 (May 1957), § 40 In the note "Les bonnes références" (no.° 82) of May 8, I discovered that these ideas, as well as those I had developed in the same SSA 5 seminar for the of homology associated with cycles (and many others) were taken up by J.L. Verdier, without a word about the existence of an SGA 5 seminar or about myself. This operation took place in 1976, a year before the "SGA 4 operation$\frac{1}{2}$ " (with which it seems to me to be closely associated), and in full view of all the ex-auditors and participants of the 1965/66 SGA 5 mother seminar.

[64](**) (May 28) And it was even there that he first heard of the things he so brilliantly exposes in the SGA 4 pirate-volume$\frac{1}{2}$! On this subject, see yesterday's note "L'être à part" (n° 67')). Compared to the methods used by his friend Verdier the year before, and to those he himself has used on other occasions, my friend here nevertheless stays below the line patent plundering, since he presents me as the author of the paper on cycles (with, admittedly, the brilliant result of being able to present me as his collaborator), and doesn't even pretend to be unaware that I've had something to do with the theory of stellar cohomology, the formula of traces, and so on. For a decisive step in this direction, see however, the note "L'Eloge Funèbre (1) - ou les compliments" (n° 104).

[65](***) (May 28) For a deeper meaning of this "violent insertion" of SGA 4$\frac{1}{2}$ between the two indissoluble parts SGA 4 and SGA 5 at one end, forming the heart of my written work, see the note "La dépouille... . " (n° 88).

[66](*) (May 28) The expression "ambiguous provisions" is definitely an understatement here!

expressed the day before, that SGA $4\frac{1}{2}$ was not a scam operation. If apparently nobody (starting with Illusie, whose good faith is certainly not in question[67] (**)) noticed the "operation", this is undoubtedly due to this "ascendancy" that I've already noticed, and also I think to the charm of my friend's person, both of which place him above suspicion!

14.2.15. Squaring the circle

Note 69 (April 27) Around the age of eleven or twelve, when I was interned in the Rieucros concentration camp (near Mende), I discovered compass drawing games, enchanted in particular by the six-pointed rosettes obtained by dividing the circumference into six equal parts using the opening of the compass. compass transferred to the circumference six times, causing it to fall right back on the starting point. This experimental finding had convinced me that the length of the circumference was exactly equal to **sixp** . 263 times that of the shelf. When, later on (at the lycée in Mende, I think, where I ended up going), I saw in a textbook that the relationship was supposed to be much more complicated, that we had $l = 2\pi R$ with $\pi = 3.14 \ldots$... I was convinced that the book was wrong, that the authors of the book (and no doubt those who had preceded them since antiquity!) must never have drawn this very simple line, which clearly showed that we simply had $\pi = 3$. Typically, I realized my error (which consisted in confusing the length of an arc with the length of the rope that joins the ends) when I expressed my astonishment at my predecessors' ignorance to someone else (an inmate, Maria, who had given me some voluntary private lessons in maths and French), just as I was about to show her why we should have $l = 6R$.

The confidence a child can have in his or her own abilities, rather than taking things learned at school or read in books at face value, is a precious thing. Yet it is constantly discouraged by those around us. Many will see in the experience I relate here an example of childish presumption, which had to bow to received knowledge - with the facts finally revealing a certain ridiculousness. As I experienced this episode, however, there was no sense of disappointment or ridicule, but rather that of a new discovery (after the one I had hastily interpreted by the false formula $\pi = 3$): that of an error, and at the same time that we should have $\pi > 3$, because obviously the length of an arc is **greater** than that of the string joining the two ends. This inequality was well in line with the rejected formula $\pi = 3.14 \ldots$ which, as it turned out, looked reasonable, while at the same time I had to admit that there might be some not-so-idiots out there who had looked into the matter At that point, my curiosity was satisfied, and I don't remember wanting to find out more about the ins and outs of this all-important number,

[67](**) It's high time we took this opportunity to thank Luc Illusie for the care and self-sacrifice with which he saw to the successful completion of a number of distressed presentations and the publication of the "package"; and this under conditions that were certainly not the least encouraging, not least of which was my total absenteeism!

(May 26) In the light of subsequent reflections, pursued in notes n° 84 to 89 and especially in the note "The Massacre", these thanks lavished on Illusie take on an enormous and unforeseen comic dimension, which I was far from expecting. I had no idea when I wrote these lines! It's true that I wrote them against a certain reluctance on my part, which expressed itself in particular by "forgetting" the (already planned) acknowledgements in the "main" text of the note, so that I had to "make up for it" with a footnote. This reluctance was undoubtedly due to the unease I had already felt from the first time I held in my hands this volume called SGA 5 (and which I didn't have the opportunity to hold in my hands again, I believe, until the last few weeks), an unease I mentioned in the footnote (dated today, May 26) to the previous note "The Signal". This inattention illustrates the importance, in meditation, of vigilant attention to what's happening within oneself at the very moment. In the absence of such vigilance, the reflection here remained below the level of meditation, on a superficial level - whereas attention to this reticence would have led me to probe its origins, and thus also to take a closer look at what had become of this fine seminar (something I didn't do until two weeks later).

you had to believe that they were sending him a letter[68] (*)

p. 264 This experience was undoubtedly one of the very first that taught me a certain caution, when my own insights seem to contradict generally accepted knowledge: that such a situation may merit careful consideration. Prudence, which is a fruit of experience, marries and complements (without altering) the spontaneous confidence in one's own ability to know and discover, and the assurance given by the original knowledge of this power within us.

14.2.16. funeral

Note 70 (April 28) Thinking back last night to this story about the cover of SGA $4\frac{1}{2}$, where I unknowingly appear as a "collaborator" of my illustrious ex-student, the thing seemed so incredible that a doubt came to me if I wasn't being betrayed by my memory, and hadn't indeed been consulted and would have given my agreement without thinking too much about it. But this assumption goes so far against the grain of the attitude that was mine until last year, namely that there was no question of my publishing maths again (and even less so, not as a "collaborator" with someone, and of someone whose relationship with me already seemed to me to be fraught with profound ambiguity) - that it is even more "incredible" than what it was supposed to "explain", and which in the end has nothing mysterious or inexplicable for me! As a matter of conscience, I checked my friend's letters between 1976 and the present day (there aren't many of them, and it was a quick job), without finding, of course, any allusion to the publication of SGA $4\frac{1}{2}$. I did write a few lines to the person concerned himself, to ask him if he could give me some explanations about this "hoax", which I didn't really appreciate... . [69](*)

When, in my reflection three days ago, I mentioned the turning point that took place three years ago in my relationship with my friend Pierre, when I lost interest in continuing to communicate with him on ma-
p. 265 thematically (see "Two turning points", note (66)), I remembered a certain impression that had been strongly present at the time. To put it in context, I should first point out that, during the ten years that had elapsed, while my friend had played for me the role of practically the one and only mathematical interlocutor, I had expected (as much a matter of course as the role I was making him play) that he would **relay** the mathematical thoughts and ideas I shared with him, and in turn communicate them to mathematicians who might be interested in them. As I've explained elsewhere (see section 50, "The weight of a past"), it was the feeling of having such a relay interlocutor that gave my sporadic periods of mathematical activity a deeper meaning than that of satisfying a craving, by linking them to a collective adventure that went beyond my own person. It was this feeling, no doubt, that

[68](*) (April 28) The above evocation brought back other memories, which show that this famous number π intrigued me more than I first thought I remembered. The approximate value 344/133, found in a book (perhaps the same one), had struck me - it was so pretty that I could hardly believe it was only approximate! Knowing only fractional numbers at the time, I was intrigued by what the numerator and denominator of the irreducible fraction expressing π might look like - they must have been quite remarkable numbers! Needless to say, I didn't get very far with these childish musings on squaring the circle.

[69](*) (May 26) My friend kindly honored me with a reply, which dispelled the last trace of doubt. He had indeed listed me as a "collaborator" because of the presentation of SGA 5 that he had written and included in SGA $4\frac{1}{2}$ - and he hadn't thought it necessary to ask for my agreement to this transfer, or to be listed as a "collaborator", nor had he thought it necessary to send me a copy of this volume to which I had collaborated so well, given that "I hadn't done maths for seven years".

(June 5) I have just received (better late than never!) a letter (dated May 30) from Contou-Carrère, replying to a letter of April 14 in which I asked him (as a matter of conscience) if he had ever seen a copy of SGA $4\frac{1}{2}$ among my books. It would seem that there was such a copy, which Contou-Carrère had kept with him (unless he bought it and no longer remembers?). On the other hand, Deligne's reply seems to confirm that he hadn't thought it worth sending a copy: "It might indeed have been a good idea to send you a copy of 4 1/2; I doubtless thought that you wouldn't have seen the point" (letter of May 15).

The fact that, for so long, I felt no desire to publish what I found, and even less regret at having withdrawn from the mathematical scene (such regret, incidentally, never appeared, and I "reappeared" on the said "scene" without any deliberate intention, and before I even realized it!)

I can't say to what extent my friend lived up to this expectation - it's possible that he played the expected role as long as he maintained that mathematical availability, driven by curiosity and affectionate sympathy at the same time, which had made possible and quite natural this exceptional role he played in my relationship to the world of mathematicians (and also, to some extent, in my relationship to mathematics itself). When I asked myself the previous question, a day or two ago, I received (as if in immediate partial response!) a letter from Larry Breen, sending me copies of various correspondence from 1974 and 1975, including two lines from Deligne from 1974, accompanying a copy of a letter (which I had just written to him about Picard's field formalism), which asked his opinion about my letter. In it, he refers to me as "the master", in which I think I sense a half-pleasant, half-affectionate intonation. I can't recall any other occasion when I heard from others about things I'd told my friend since I left in 1970. It's quite possible that there have been and that I've forgotten, not to mention the fact that even during the episodes of my mathematical activity, he was relatively uninvolved.
I rarely felt the need to consult my friend, and until 1977 or 1978 the reflections I made to him
on occasion were limited in scope. So there wasn't much to "relay", to pro□prementally speaking, p. 266
until this moment[70] (*).

Things changed in 1977, when for the first time since the sixties, I was strongly "hooked" on an exceptionally rich substance. This was the beginning of my reflections on maps, and one thing leading to another (around the same time), on a new approach to regular polyhedra (see Esquisse d'un Programme, par. 3 and 4). By this time, too, it was clear to me that the facts I had just put my finger on opened up unsuspected vistas, comparable in breadth and depth to those I had glimpsed (and more than glimpsed, subsequently) with the birth of the notion of pattern.

It's strange that, on this occasion, I was still addressing my friend with the expectation that he would echo these things that had amazed me and what they were making me glimpse - whereas the total silence that for seven or eight years already had surrounded the very name "motif" was eloquent enough to teach me that my expectation was illusory! This astonishing lack of discernment illustrates the deliberate intention I had (even after discovering meditation a year or two earlier) to pay no attention to my relationship with mathematics or mathematicians, supposedly part of the distant past.

[70](*) I could make an exception for my first thoughts on a theory of unscrewing stratified structures, which I must have mentioned to Deligne in the early '70s. He had greeted my expectations on this subject with indulgent sympathy, rather like that accorded to a grown-up child who doubts nothing. (These were dispositions he often had in his relationship with me, and which were surely often well-founded!) My friend's scepticism, motivated by his knowledge of certain phenomena of savagery of which I was unaware, didn't convince me - rather, the facts he was pointing out made me suspect from that moment on that the context of "topological spaces", commonly adopted for "doing topology", was inadequate for flexibly expressing certain topological intuitions I felt essential, such as that of "tubular neighbourhood". Over the next ten years, I had little occasion to return to these reflections, and I had to forget my "suspicions" for a while, which were brought up to date again (and then became an intimate conviction) by my reflections of December 81.
- January '82, stimulated by the need for a theory of "unscrewing" the "Teichmüller tower". (Compare Esquisse d'un Programme, par. 5, 6).

(June 5) As another exception, I could count my reflections on virtual relative patterns and virtual patterns (above a general basic pattern), which I seem to remember sharing with Deligne. As these were things closely related to a yoga he had decided to bury (until the time of exhumation in 1982), it's not surprising that he didn't make

For some hints, see note no.◦ 46_9 .

p. 267 and well out of date! My first thought was along these lines [71] (*) takes place precisely in 1981, the year the second "turning point" in my relationship with my friend, which I've had occasion to mention. But even in this meditation, which lasted for several months, the relationship with the other mathematicians was barely touched upon, and the relationship with the one among them who had been undoubtedly the closest of all (at least in terms of our shared passion) was not even touched upon, as far as I can remember. It would have been very useful!

Looking back and reflecting on it now, it's clear that what happened at that moment, which surprised and frustrated me so much (the sudden appearance of a discreet disdain, where I had expected to share the still fresh joy of a discovery that had made a deep impression on me), was indeed what had to happen. It was precisely the **scope of** what I had to communicate, which had motivated my expectation of an interest in tune with my own, that was to arouse in my friend, for the first time in his relationship with me, the reflex of **discouragement**. This reflex must have been all the stronger, given that I was already "pre-buried" by the publication of SGA $4\frac{1}{2}$. When I returned to the fray three years later, as my friend (armed with his beautiful theorem on absolute Hodge cycles) was about to take care of the burial in due form, with the "memorable volume" published the following year[72] (**), this same reflex came into play, but with a completely different brutality. (This episode put an end to communication on a mathematical level, but without "discouraging" me... .)

In both cases, the disinterest was obviously sincere, as it had been in other cases, when it had been expressed towards others than myself. It wasn't the first time I'd seen in him (or in others) forces alien to the thirst for knowledge neutralize it, and take the place of the mathematician's flair.

It was on these two occasions, in 1978 and again in 1981, that I first glimpsed, as if in a flash, the "**price" of** this contradiction in my friend that had been known to me for many years, but whose significance, as a hindrance and limitation in his work and in his understanding of mathematical things, had never been clear to me until then. But it was only in the course of the meditation that I pursued for the past month, on the sense of a certain **burial** that had been taking place insidiously since I left, that this ported ended up gradually appearing in full view.

p. 268 On an obvious level, the funeral I've discovered over the last few days and weeks, which I'd been anticipating for several years but never thought of attributing a particular role to anyone, has been first and foremost the funeral of **my mathematical work**, and through it and above all, of **myself**. The best placed of all, of course, to put a hand to this burial (which many others, in their heart of hearts, were calling for), and to preside over the anonymous obsequies, was the friend who, in the eyes of all, had once been the legitimate heir. If he presided over the funeral, he was certainly not alone! But at a deeper level, the one my friend was so discreetly burying, throughout those twelve long years, was none other than **himself**; that thing in him, rather that impresses no one, a delicate and elusive thing like the fragrance of a flower or a fruit, and that has no price. (⇒ 71)

14.2.17. The tomb

Note 71 But following the thread of associations, I've strayed from my purpose, which was to evoke a certain "strong impression", the memory of which has come back to me insistently over the last three days. This impression occurred at the time of the "turning point" in my relationship with my friend, when I was confronted with signs of

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[71](*) On this subject, see "The troublemaker boss - or the pressure cooker" (s. 43).

[72](**) This is the Lecture Notes 900 volume, see note "Souvenirs d'un rêve - ou naissance des motifs" (n° 51).

(at once muted and brutally obvious) of a kind of deliberate contempt - signs that made me put an end to our relationship on a mathematical level. I then understood that the moment had arrived when I had nothing more to expect from the continuation of such a relationship, and the "decision" was made on its own, without division or regret, as the first fruit of this late (and very partial) understanding.

There was no anger in me, and even less bitterness (I don't remember during the course of our relationship feeling any movement of anger towards my friend, nor any bitterness, except at the time of my departure from the IHES, when he was not the only one to be included in it). But there was a sadness, as I turned that page in my relationship with someone who continued to be dear to me, when the strongest bond that had attached me to him had dried up and perished. And like a sting that stayed with me in the years that followed, there also remained this unresolved frustration, of the joy I had brought to share with him, to the one who seemed closest and best placed to share it, and which had come up against the closed doors of complacency. This frustration has finally been resolved, it seems to me, through the meditation I'm currently pursuing. Just today, this one has come back to show me that what was happening to me was what was supposed to happen' and that the person primarily responsible for this frustration is no one else p. 269 that I myself, who had seen fit to indulge in an illusory image of a certain reality, rather than use my healthy faculties and look at this reality with awakened eyes

It was against the backdrop of this sadness, and also of this frustrated expectation, that this strange impression appeared, which came not as the fruit or outcome of reflection (which didn't happen then), but as an immediate and irrefutable intuition. It was that everything I could say to my friend on a mathematical level, and everything I'd been saying to him for years, I was entrusting or had entrusted to a **tomb.** While I never mentioned this impression to anyone, nor did I write it down in black and white in the course of any subsequent reflection, I do remember that it was this image of a **tomb** that was then present, and the very word that expresses it (in French), and which I have just written down. This "impression" or image must have arisen, at that moment, as the visual expression (so to speak) of some understanding that, at some level, must have been forming and present for a long time, as the fruit of a whole set of perceptions that must have taken place over months and years, without attention retaining them or memory registering them; perceptions that were simple and obvious, no doubt, but which I hadn't "retained" because they seemed undesirable to someone inside me who often has the power to sort them out as he pleases. . . Neither at the time, nor since, has this peremptory image been associated with any precise, tangible recollection of an "event" in line with this image, and which could have given rise to it in me - the memory of this sudden image must have crossed my mind only rarely afterwards, and today is the first time I've dwelt on it in the slightest.

If no memory or association arose at the time, it's surely because I didn't have the minimum availability to welcome it. Strangely enough, at the time I was engaged (if I place the[73] (*) moment correctly) in a meditation on my relationship with mathematics, without this episode, which spoke to me strongly enough, after all, of a certain past through a present, making me think of interrupting the "thread" of my reflection, to include a reflection on the ins and outs of what had just happened and which was not without consequence in my life.

The first (and, to be honest, the only) association that arose even now (having just evoked p. 270 and to say that on the spot it had appeared disjointed from any memory or association. . .) was the fate that had befallen my "dream" of patterns - the mathematical vision of all that had been dear to me,

[73](*) (June 11) Cross-checking confirms this to be the case. This "second turning point" occurred in the second half of 1981.

in my mathematical past. If that past perhaps still had some secret hold over me, it was through that dream - and that secret hold (which I think I glimpse as I write these lines) itself had the force, beyond words, of the dream. If, as the legacy of a past investment, a passionate investment in mathematics, an unspoken, deep-seated frustration had arisen over the past ten years, it was indeed that of seeing a deathly silence surround those things which for me were alive, and which I had entrusted to my friend as living, vigorous things, ready to leap into the light of day! With me gone, it was he and no one else who had the power and vocation to watch over this blossoming, to make available to everyone what he alone (with me) could intimately feel. And without ever saying it to myself in these or any other terms - without ever stopping (as far as I can remember) even for a thought about what I had left behind - somewhere inside me I must have realized, over the years, that this dream that was always dear to me, I had entrusted to a "tomb".

And then, with this evocation and the first association it arouses in me, I see a flood of other associations appearing in its wake, revealing to me that I have indeed touched a nerve center - the point of all, perhaps, through which the (long-ignored) weight of my mathematical past is exerted.

But this is not the place, it seems to me, to follow these associations, as this "final" stage of my reflection is already getting long in the tooth. It seems to me that I've said enough in this reflection about my friend Pierre as well as about motives - and surely too much for many people's taste! And I think it's time, as far as these notes are concerned, to bring them to a close, with a sort of **assessment** of what this reflection on a double funeral is teaching me, for the time being.

14.3. VI The return of things - or the Unanimous Agreement

14.3.1. One foot in the merry-go-round

Note 72 (April 29) . . . .

p. 271 It seems to me that most of the descriptive and decanting work that had to be done, on the subject that occupe, is complete, as far as the "partial images" about a certain situation are concerned. (It's obvious that these notes, 'intended for publication, only give an abridgment of the actual work, while it's out of the question here to spell out in detail all the elements that contribute to the formation of this or that partial "image". . . .) Surely, through this same work, a certain overall image could not fail to take shape, still vague, and waiting to be formulated to take shape and life and tell me what it has to say. From yesterday's reflection, I feel it's ready to blossom, urging me to give it a voice.

To tell the truth, what yesterday's reflection (which I've just reread) taught me most of all **concerns none other than myself**. It's with a certain relief that I see the reflection returning to the firm ground of a reflection on myself, whereas for the past week it has often given me the feeling of involving someone else's person more than my own. Yesterday's reflection finally revealed to me something that is surely quite obvious: the strength of my attachment to a certain past, to my "mathematician past", and the particular role played in it by this famous "dream" of motives.

Once the point is finally made, its obviousness is obvious - the most recent and clearest sign perhaps being the emotion triggered by the discovery (two years later) of a certain "event", of this

[74](*) I thought it wise to spare the reader a good page of considerations on meditation in general, which were a way of beating around the bush - a sign of the resistance to getting to the heart of the matter.

the "furtive" (and belated) re-entry of motifs into the mathematical menagerie, under the guidance of my former "pupil" and friend! This emotion was immediately translated into the resumption of a reflection that seemed to be over, - a resumption that materialized as dry as a fifty-page stream of retrospective reflections! As a result (and I've already realized this several times during this untimely resumption), it would seem that that I'm not yet as "off the merry-go-round" as I thought I was a month or two ago, in my exultation. of the end of a stage and the feeling of liberation (by no means illusory) that this stage had brought me - avec p. 272 the teaching that "I wasn't better than the others", and that "I shouldn't be surprised if the pupil surpassed the teacher"[75] (*). Yet this teaching didn't stop me from being surprised - it was enough for the "pupil" to overtake me in a direction I hadn't anticipated at all! But if the teaching didn't prevent me from being "astonished", it was nonetheless invaluable to me on more than one occasion in the course of the past reflection, to save me from the usual pitfalls (or at least **some of them**).

To come back to the strength of this "hold", to the strength of my attachment to this dream of motifs, it has already appeared in many other places in the present volume, whether in Récoltes et Semailles (where motifs are mentioned several times and in quite eloquent terms), or in the Esquisse d'un Programme (where, "objectively", motifs had nothing to do with it), or in the Esquisse Thématique (where motifs are a bit like unhatched eggs in a flock of vigorous chicks). In the latter text, which dates back twelve years and is obviously written in distant dispositions, this last paragraph on motifs is the only one, it seems to me, where we suddenly feel a warmth passing through... ...

The remarkable thing is that this attachment never occurred to me in the fourteen years since I left, until yesterday, when I finally glimpsed the obvious, and finally formulated it for myself today. During the meditation of almost three years ago (July to December 1981), I came to realize the first obvious fact: that I still had a passion for mathematics, which had expressed itself eloquently over the years. But my attachment to a past, as far as I can remember, went unnoticed at the time, and has remained so to this day.

I must have begun to glimpse it, however, with the reflection "The weight of a past", which came to me as a matter of conscience at a time when the meditation on my past as a mathematician seemed already complete (except that I hadn't yet been able to perceive the **weight** of that past!). In fact, as I wrote it, I sensed that I was still on the surface of things, without really penetrating them. The notes I had to add later (first
(46) (47)) then led me in a direction that for a good while distanced me from myself, focusing my attention on a mathematical work (and on those aspects of it that seemed most "important" to me), then on the vicissitudes of that work and the role of others in them, rather than on myself.

I just reread this reflection "The Weight of a Past" (s. 50). Towards the end of it, I begin to p. 273
Indeed, I glimpse that the "tipping force" (towards a mathematical investment other than episodic) could be due to an "attachment to the past" (as a mathematician), but rather to "the past of these last ten years, the past "after 1970", and not the past of things already written in black and white, of things done, those before 1970". A few lines later, however, I recall, but only "in passing", that in the "vast program I had before my eyes at the time ... only a small part of it has been realized". In writing these lines, I must have been thinking above all of those parts of the "vast program" that were immediately realizable, whose motivating force (!) was nevertheless nowhere near that represented by the "dream of motives". (Its justification (but by no means its formulation) appeared then as one of the great tasks "on the horizon". . .)

[75](*) See "No more merry-go-round!", n◦ 41.

It's clear that my attachment to the "motive dream" is (as I'm sure all attachments are) primarily (if not exclusively) egotistical. It's the desire not only to **contribute** to a collective work, but also to have that contribution **recognized**. Assuming that the "vast picture of motives" had indeed been painted to the full extent I saw it since the late sixties, but that my part in the blossoming of this vision had been silenced, my displeasure would no doubt have been no less (and perhaps greater?) than the displeasure I felt when I came across the "memorable volume" (in which I see certain notions and ideas that I had identified and brought to light, but (so I felt, at least) deprived of the breath and intense life that had so fascinated me in them)[76] (*).

Until this egotistical desire to see things from my distant or more recent mathematical past "recognized" is consumed, it's probably premature to claim that I'm "off the merry-go-round". The mathematical "merry-go-round" no longer **contains** me, as it once did some of my friends. But I've certainly still got a foot in it, and I suspect it'll stay there as long as I keep on doing maths!

14.3.2. The return of things (or a foot in the flat)

Note73 (April 30) I've been thinking about the fate of the SGA 5 seminar, and how this fate was linked to the publication of SGA 4[1] . A situation that had been confusing, and which I've only examined in the last few months.

p. 274 days and glimpses in passing,[2] is now very clear to me. I've just added a note footnote[77] (*) on this subject to my thoughts of three days ago (see "The signal", note (68)), and it seems to me that with the comments I had already made there before yesterday (also in footnotes) and with the thoughts of the day before ("Table rase", note (67)), I have expressed myself clearly enough for there to be no point in making yet another overall summary of a situation which now appears eloquently enough[78] (**).

Having reached this point, it's important to note that the first and foremost person responsible for the "sad fate" that befell SGA 5, and for the use that was made of a situation of abandonment, is none other than myself. If the various "volunteers" (who took on editorial work they didn't really want to do) were clearly not in tune with themselves, neither was I, who stubbornly refused to heed the lesson of a situation that spoke for itself, After all, three whole years elapsed between the end of the oral seminar and my departure from the world of mathematics (which immediately translated into a virtually total lack of interest in my published work over the following fourteen years). It's true that during those three years I was fully occupied with my other tasks, including continuing the SGA seminar (with SGA 6 and SGA 7), writing the EGA, reflecting on the often juicy questions arising from day to day, and among these, the gradual maturation of an overall vision of patterns. . . Taken up with these tasks, I chose to turn a blind eye to the fate of a past seminar, which (together with SGA 4 from the previous year) constituted the most profound mathematical contribution I've been able to make, in terms of fully accomplished work I mean, and also the one with undoubtedly the widest scope.

[76](*) (June 14) This "displeasure" is due above all, it seems to me, to this impression of impudence, of deliberate disregard for a link that one affects to ignore, to hold as negligible. The situation is quite different when ideas or results you've discovered are rediscovered by others, which happens quite often.

[77](*) This prohibitively long footnote has been turned into a separate footnote, "Le renversement" (n◦ 68').

[78](**) I return to this subject on May 9 and the following days, see notes n◦ s 84-89.

The situation could only deteriorate further after my unavoidable departure, allowing the most prestigious of my ex-students to carry out the ingenious operation of inserting his famous SGA 4[1/2] between the gangue of nonsense and superfluous details of SGA 4 and SGA 5, and doing me the honor of promoting me to collaborator on what is presented as the central key-text, destined (as he says with that candor that makes up his charm) to do the following charitably "forget" the heavy gangue that surrounds it. ...

In short, the choices I made, from before my departure and through my departure, implied consequences p. 275 for the fate of my published work, or (in the case of SGA 5) work awaiting publication, as well as for the part of my "work" that remained in the state of a dream - an **unpublished** dream, that is. I don't regret my choices, and it's not my place to complain, when I see today certain consequences of those choices that are not to my liking! It is my responsibility, however, to examine these consequences (and all the more so when I don't like them!), to get an overall picture of the facts[79] (*) (which I've done), and to learn from them what I can. That's what I've still got to do, and today's reflection will perhaps be, at the very least, a first step in that direction. A number of things have come together for me over the last few days, and I'd like to start by putting them in black and white.

The main force, the "drive" behind my investment in my pupils in general, in the first period of the sixties, was the desire to **find** "**arms"** to carry out "**tasks"** that my instinct told me were urgent and important (at least from my own mathematical point of view). This "importance" was certainly not purely subjective, it was not simply a matter of "taste and color", and often (I think) the student who took on such a task that I proposed to him felt that it "fit the bill", and also, perhaps, what its place might be within a larger scheme of things.

Yet, as far as this "drive" was concerned, this motivating force within me that pushed me towards completing the tasks, it wasn't some "objective" importance that was at stake - whereas the "importance" of Fermat's conjecture, Riemann's hypothesis or Poincaré's hypothesis left me perfectly cold, that I didn't really "feel" them. What distinguished these tasks from all others, in my relationship to them, was that they were **my** tasks; those I had felt, and made my own. I knew that having felt them had been the culmination of a delicate and profound work, a creative work, which had made it possible to identify the crucial notions and problems that were the subject of this or that task. They were, and no doubt still are (to a large extent), a part of who I am. The link that bound me (or still binds me today) to them, was by no means clear-cut, when I entrusted such and such a task to a pupil - well on the contrary, this link was acquiring new life, new vigor! This link didn't have to be said (and I'm "saying" it here, don't p. 276 if only to myself, for the first time). This link was as obvious to the student who had chosen to work with me, and on the task of his choice, as it was to me, and also (I'm convinced) to anyone else. It's the deep bond between the person who conceived a thing, and that thing - and which is not altered, but (it seems to me) strengthened by those who, after him, also make that thing "theirs" and bring the best of themselves to it It's a bond I've never examined closely. It seems to me deeply rooted in the nature of the "I", and universal in nature, It's a link that we sometimes affect to ignore, as if we were above such pettiness - it's even possible that I've sometimes entered into such an affectation[80] (*). But the few times in recent years (or in recent days and weeks) when I've been confronted with an attitude in others that affects to ignore this link (of which they are aware) that links me to such and such a task that has

[79](*) (May 28) Read here "facts known to me". On the following day, entirely unexpected new facts rekindled my thoughts on the Burial and led me to triple the volume of my notes on the subject.

[80](*) What's certain is that I was following the "bon ton", which consists in ignoring this kind of thing, contrary to the de rigueur images! (May 30) On this link, see the note ". . and the body", n° 89.

has been accomplished (by another, or by myself) or merely designated, I am touched in a sensitive place. We can call this place "vanity" or "fatuity" and put other names to it - and I don't claim that these terms are out of place here, but whatever we call it, I'm not ashamed to talk about it or to be as I am, and I know that the thing I'm talking about is the most universal thing in the world! No doubt this attachment to "one's work" is not as strong from one person to another. In my life, where "Doing" has been the constant focal point of my great investments of energy since childhood, this link has been strong and remains so today.

I can therefore say that the main force driving my relationship with my students was that I saw in them welcome "arms" for the accomplishment of "my" tasks. The formulation may sound cynical, but it simply expresses an obvious reality, surely felt by my students as well as myself. The fact that they were "my" tasks in no way prevented them from also doing "their" tasks - and it was this 'identification in them with their task that mobilized in them the energy necessary for their accomplishment; just as identification with this same task had mobilized in me the energy that had brought it into being and taken shape, and continued to mobilize the energy I continued to invest in the subject. This energy was essential if I was even to "function" as the "master", i.e. as the elder who teaches a craft (which is also an art),
and which cannot be done without mobilizing considerable energy. Never in my teaching past have I felt a contradiction in the fact that the same task was profoundly "his" for the student who was working with me, while remaining just as deeply "mine". I don't believe that this situation is in the least conflictual, p. 277 nor that it has ever given rise to the desire for conflict[81] (*). In this situation of simultaneous investment in, and identification with, the same task, both the student and myself found (it seems to me) our account, in a working relationship that was perfectly clear, and which in itself (it still seems to me) contained no conflictual elements. On the personal level, on the other hand, the relationship remained superficial - which in no way prevented it from being cordial, even friendly and sometimes even affectionate.

The investment in my tasks, and **through them** in my students-collaborators for these tasks, was (as I said) of an egotistical nature (like any investment, no doubt). Surely, carrying out these tasks was above all, for the "I", a means of enlarging itself, through the realization of an overall work of vast proportions that "my arms alone" would not have been able to bring to fruition. From a certain point in my life as a mathematician, there was this constant ambiguity of a cohabitation, a close interpenetration between **the** "**child"** and his thirst for knowledge and discovery, his wonder at things seen and those examined closely, and, on the other hand, the **ego**, the "**boss"**, rejoicing in his works, eager to expand and increase his glory by multiplying his works, or by the dogged, relentless pursuit of an overall construction of grandiose proportions! In this ambiguity, I see a division that continues to weigh on my life and leave a deep mark on it - a division that perhaps will remain as long as I live. Such a division is certainly not unique to me, but perhaps in my life, filled with the "best" as well as the "worst", this division has taken more extreme forms than in others.

So I can say that for this invasive, eager-to-expand "I" (who wasn't alone in this respect)
my pupils were first and foremost welcome "collaborators", not to say "instruments" - welcome "arms" for the construction of an imposing work that would say "my" glory![82] (**)

[81](*) If, encouraged by a certain context, one of my students has wanted to take over a role that had been mine, in a work done with me, this was done at a time when he had long since ceased to be a student.

[82](**) I wrote this sentence with some hesitation, weighing up my words in the full knowledge that it could be seized upon as a sort of cynical admission of the horrible Mandarin finally throwing off the mask! But I'm well aware that I won't prevent anyone who wants to drown an embarrassing fish from doing so at their leisure. It won't stop me from pursuing my aim of discovering and stating the obvious, including the humble truth written above, which will only surprise those who have never bothered to look.

This is something, it seems to me, that became quite clear already in the course of my meditation three p. 278
years on my relationship to mathematics (and beyond that, to "doing" in general), even if I sometimes forgot about it afterwards. It's the thing that's been on my mind these last few days, to make the connection with this other remarkable fact: that it was precisely by one of my students (with quotation marks, mind you!) from that time, and the one who was closest to me of all, and the only one to "feel" effortlessly and in their entirety these great designs within me that seemed to push me relentlessly to realize them - that it was he of all people who, after I left (and in his own heart, no doubt even before.

«.) a mis en oeuvre au cours des ans cet **Enterrement** aux dimensions de l' Oeuvre (les majuscules ici ne sont pas de trop !), et qui a finalement "présidé aux Obsèques" (avec une majuscule de plus, pour faire bon poids !). What's striking about this situation is the enormous, irresistible, **Ubuesque comedy of** the whole thing! I must have sensed this comedy in a vague way over the last few days, but it only revealed itself to me in its true nature at this very moment, when I placed the last capital letter over my solemn funeral - in a sudden, irresistible burst of laughter! It was precisely **laughter** that had been lacking until now in this so-called "final" stage of reflection, where the dominant note was rather the pained air of the "Monsieur bien" disappointed in his legitimate expectations (or even abominably deceived), when the pained air gave way to sarcastic, well-sent comments (one is used to expressing oneself, or one isn't!). I definitely feel I'm on the right track again, after this long digression (that word reminds me of something. . .) in the sad tones.

And just now I've come up with a name for this "note" (I'm not sure what it's a note to, but whatever.) that it's time to close. It will be "**Le retour des choses**". (⇒ 74)

14.3.3. The agreement

Note 74 I finally feel - phew! - that I'm nearing the end of this "final stage", which has stretched over twelve days, each of which (as in the past) was presented as "the last". Perhaps the final word was spoken just a few minutes ago. My (symbolic) funeral was a **return of things**, a harvest of seeds
made by my own hands. (And my burial in chair and bone, if I have this happiness of dying leaving p. 279
behind me living men and women who can bury me, will also be a return to something I left at birth... . [83](*).) It seems to me that whatever remains to be added will be little more than an **epilogue**.

The famous "dearest pupil of them all" was not the only one of my dear pupils to bury me with gusto, and those who did indeed put their hand to the dough may not be the only ones among them, present at the funeral without displeasure! But I don't really care who it was! (Knowing more about it, if nothing else, won't tell me anything more). I've finally understood this "return of things", and having understood it, I'm reaping the benefits.

Yet I haven't yet extracted all the substance this benefit has in store for me. It's not yet clear to me exactly **what it was about** me that made certain ex-students take advantage of the funeral and burial. Is it only this "greed" of which I spoke, which (it seems to me) does not

itself.

[83](*) (May 28) This sudden association with my own death presented itself forcefully. I was tempted to dismiss it, and then to suppress this unexpected parenthesis, which seemed to come like hair on the soup. I refrained from doing so, out of a kind of respect. Strangely enough, the next day I learned that on the evening of April 30, when I was continuing my reflection, in the commune where I live, the (seriously ill) sister of a friend had died. I saw Denise for the first time, and on her deathbed, the very same day. The following day, May 2, I joined my friend and many other living men and women to lay her to rest on a beautiful spring day...

which they had accommodated without difficulty (and probably without even noticing it, at least not on a conscious level) when they first started working with me? Was it then the "occasion" (my departure, etc.) that "made the thief", and **revealed a general propensity**, in them as in the "pupil of all", to bury his "master" or his "father", when the circumstances were propitious? Perhaps I was more "master" (or more "father". . .) than nature, and this circumstance played a part in triggering this "burial syndrome" with a vengeance! For the moment, I don't know! Perhaps the echoes I gather (I hope) will enable me to see more clearly, and to better assimilate the unexpected food before which I am now sitting.

There weren't any students to discreetly take part in the funeral and burial, even though no non-ex-students were in a position (as far as I know) to play a prominent role. Clearly, many of my old friends found this to their liking. The whole thing doesn't seem too mysterious to me.

As I've had occasion to say in passing, more than once I've witnessed the deep malaise created in
p. 280 my friends of yesteryear by my untimely departure from the mathematical scene. It's the malaise that arises from anything that obscurely feels like a **provocation** to profound questioning, to renewal. In this particular case, it was natural that this unease among mathematicians should be strongest among my friends, those who had known me, and who could feel the full force of the investment I had made in the values that are still theirs; not to mention the fact that each of these friends has made, and continues to make, an investment of comparable strength in these values, and in the substantial "returns" they offer. I had already had ample opportunity to observe such unease among other scientists, right from the start of the Survivor period. But that didn't stop me from being surprised every time I saw unequivocal signs of distancing, and sometimes even enmity, among one of my old friends, to whom I continued to feel the same sympathy. What must have made my "abandonment" particularly intolerable for some of them was precisely the fact that I was supposed to be one of the "best" of them, surely the last one they would have suspected of playing such a trick on them! (And I did indeed sometimes sense a tone of **resentment** in such of my old friends in the mathematical world.) It's only natural, then, that they should find it in their hearts to see that all this "grothendieckery" was, after all, a lot of paper for very little money, etc. etc. A single person, no matter how prestigious, is not enough to make a fashion - it's even more important that the fashion you want to launch responds to an expectation, a secret desire, in many others, before it becomes a consensus and the law[84] (*).

In the fourteen years since my departure, I have perhaps tended to underestimate the unease it has created in the "wider world" - even though, for me, my departure in June 1970 came so naturally that there was not even a "decision" to be made: new tasks had taken over from the old ones overnight, and the latter had suddenly receded into the background as if from the distant past! (It's also true that I didn't experience such unease among my colleagues at the University of Montpellier, who form a completely different milieu from the one I'd left). Perhaps I also underestimate the role that such unease may have played among my ex-students "before 1970", including

many of them belong to this same milieu, and "go all out" in their mathematical investment. It's possible that this uneasiness played a role no less strongly in them than in the other friends I thought I had
in the same environment. In any case, each situation (between me and one of my former friends or students) is unique and different from all the others, and any general assumptions I may make are of very limited and
p. 281 provisional scope.

Returning once again to the more solid ground of the case in point, I am struck by the fact that the two ex-students

[84](*) (May 28) See in the same vein the note of May 14, "Le Fossoyeur - ou la Congrégation toute entière", n◦ 97.

whose active participation in my beloved master's funeral I was able to witness, were also the very ones who had first drawn my attention to themselves by their contemptuous, discouraging attitudes: towards younger mathematicians who were "post-1970 students", where the influence of my ideas and approach to mathematics was clearly visible. This coincidence certainly came as no surprise (although, of course, events surprised me at every turn!). Another interesting coincidence is that both were among those with whom the personal relationship was the most friendly and even affectionate (and for one, this relationship has continued, and in this tone, to this day). This is in line with the general observation that it's the closest relationships that have the greatest virtue of attracting and fixing the forces of conflict.

Yet another coincidence struck me. Among all the students I've had over the past twenty-five years, there are two who stand out for me, both for their exceptional "means", and for their investment in mathematics commensurate with these means. (An investment of a strength comparable to that which I myself made during twenty-five years of my life). For both of them, moreover, I made a point of counting them among my pupils, even though it's true that they both learned things from me that were useful to them[85] (*). It was in the nature of things that both of them would discover their own tasks, without my having to suggest any of those I had (or have) in mind.
reserve - and the thesis work of both was carried out independently of me[86] (**).
So many□points in common! As a point of dissimilarity, I would say that the youngest (unless I'm mistaken) of the p.

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two is today "at the pinnacle of honors" (whose detailed enumeration I'll spare the reader, and to the known modesty of the person concerned), and that he is one of the most influential mathematicians, that is to say, also one of the most powerful; the other is for the moment a delegated assistant, in a position that the incumbent will take over next year. There are other points of dissimilarity, which explain to some extent this difference in fortunes - just as there are other points of similarity on which it is pointless to dwell here. Except for the fact that, of all the students I've had, it was with both of them that my personal relationship was also the closest and friendliest, while a common passion had from the outset created a strong bond between each of them and me. The **coincidence** I'd like to mention now is that, as far as I know, they were the only students too (in quotes, that's a given!), who vis-à-vis the "big world" did their utmost to minimize or erase, as far as possible, this very simple and obvious link to me.

It's a truly striking coincidence, the meaning of which still eludes me at the time of writing. For both, I could invoke reasons of conjuncture, different from one to the other. And it is quite possible and even probable that for both of them, at a certain level which is probably no longer that of fully conscious intentions, such a reason (of fatuity for one, of prudence for the other) came into play. I doubt, however, that this ready-made explanation will provide an understanding of the matter, in either case.

[85](*) (May 28) That's an understatement, as I later found out to my chagrin! See yesterday's note "L'être à part", n◦ 67'.
[86](**) (May 28) That's not quite true. Both of them made essential use in their work of tools that I had
and which they learned from me. Beyond this role, Hodge-Deligne's theory in the work that constitutes his thesis (Hodge Theory II, Publications Mathématiques n◦ 40, 1972, p. 5-57) stems directly from the yoga of motives that he got from me - "mixed Hodge structures" being the "obvious" answer to the (also "obvious") question: "What is the difference between the two?
in the perspective of motives) to "translate" in terms of Hodge "structures" ("in a suitable sense") the notion of motive not necessarily semi-simple on the field of complexes. Beyond a brilliantly conducted "translation exercise", there are of course original and profound ideas in this work that are "independent of my person". But it is also clear that the Hodge-Deligne theory would not exist today (nor, doubtless, almost all of the work of Deligne or any of my other students) if they had not had access to the ideas and tools that I introduced into mathematics, and which they learned from me.

the other. Surely, deeper still, other forces must have been at work, the real ones, behind the familiar appearances of fatuity or pusillanimity. Surely, these acts that express them have something important to say to each other. But surely, too, the appearance of the same acts in two such different people, as if they'd given each other the word (something certainly unthinkable, given the difference in their personalities), is a sign that they're on the same wavelength.

p. 283 fortunes!), also has something important to say to me, and about none other than myself. Could this be nothing more or less than a reproduction of the eternal **rejection of the father**? The latter, however, has l'embarras du choix among the avenues open to him to express himself! Or is it because that sure instinct of the unconscious, which makes it touch "just right" in the most sensitive or vulnerable places (when it comes to "touching"), has meant that both have fallen on the **same** spot? I'd actually be inclined to think so. But that's an inferred thing, not a seen thing, whereas lacking eyes with the gift of seeing clearly and deeply, I feel a bit like a blind man groping around in the dark, trying as best he can to "see" with his hands or his ears or his epidermis, which aren't really made for seeing.

So as not to close on this note of **perplexity** (prejudicial to my reputation), but on a note that would be pleasing to a benevolent and hypothetical reader, I will only say the concluding name, which appeared earlier, and which seems to me to express well the content common to the various considerations of this **epilogue** (to a reflection on a funeral), namely:

The Unanimous Agreement!

11. C) THE BEAUTIFUL WORLD

Contents

11.1. VII The Colloquium - or Mebkhout and Perversity bundles

11.1.1. Iniquity or the meaning of a return

p. 285 **Note** 75 (May 2) I'm definitely not done learning! I've just read two texts, which shed unforeseen light (for me at least) on the "escamotage" (of Meb- khout's work) already mentioned ("L'inconnu de service et le théorème du bon Dieu", note (48)). It concerns the role played by the two illustrious colleagues and former students whose disdainful indifference to Zoghman Mebkhout I noted, without however questioning their professional bona fides. Both texts are part of the Proceedings of the **Luminy Colloquium** (July 6-11, 1981) entitled **Analyse et topologie sur les espaces singuliers**, published in Astérisque n° 100 (1982).

The first of these texts is the introduction to the Colloquium, signed by **B.Teissier** and **J.L. Verdier** (the same man who acted as Z. Mebkhout's official thesis director). This one-and-a-half-page text begins with an explanation of a certain "Riemann-Hilbert correspondence", which is clearly destined to play a leading role in the Colloquium (and which is none other than the "theorem of the good Lord" alias Mebkhout). In this correspondence (and this is what gives it its charm and depth, and necessitates the introduction of derived categories), a regular holonomic **module** (i.e., a regular holonomic complex reduced to degree zero) is associated with a constructible complex of C-vector bundles, which can be characterized (it is said) by purely topological properties that make sense for constructible complexes of stale bundles over a not necessarily smooth variety defined over any body.
This, it is explained, is the starting point for the Colloquium's "main theme", "**perversity, intersection complex, purity**" - the (complex □ of) so-called "**perverse**" beams[1] (*) being none other than thosep . 286
which, "morally", correspond ("à la Mebkhout") to the simplest complexes of regular holonomic differential operators, expressed using a single D-Module.

The second text is part[2] (**) of the long article by **A.A. Beilinson, J. Bernstein and P. Deligne** on perverse bundles, referred to in the introduction as the central work of the Colloquium. As can be seen from the table of contents and the other pages at my disposal, this paper marks the sudden re-entry of derived and triangulated categories into the public arena, in the wake of Mebkhout's obscure work and the famous "Riemann-Hilbert" theorem.

Incredibly, in both texts, Z. Mebkhout's name is absent. Mebkhout is absent, just as he is absent from the bibliography. I should point out that not only was J.L. Verdier perfectly aware of Mebkhout's work (and with good reason!), but so was Deligne (and it would be difficult even to imagine that it could be otherwise, for someone so well-informed about current mathematical events, and when it's about the subject that touches him most closely[3] (***)).

I don't know what happened to B. Teissier[4] (****) and the other participants in the Colloque de Luminy, notably the two co-authors with Deligne of the article cited[5] (****). It seems that none of the participants was so curious to know the authorship of the ideas and the key theorem that had had the virtue of mobilizing them.

[1](*) (May 4) See note no. ◦ 76, "Perversity", on this strange application.
[2](**) (May 4) I've since received the full article, which confirms what the part I had already shown me.
[3](***) In particular, Mebkhout's work and his "theorem of the good God" represent a decisive advance on Deligne's earlier work (from 1969), which he refrained from publishing. On this subject, see note n◦ 48' already quoted.
[4](****) (June 12) B. Teissier had long been interested in Mebkhout's work, and had thus been one of the very few to
to have an encouraging attitude towards him. He was therefore perfectly aware of the scam, to which he knowingly lent his support. He justified himself to Mebkhout by assuring him that, in any case, he "couldn't have done anything about it".
[5](*****) (May 28) I have since learned that A.A. Beillinson and J. Bernstein were informed of Mebkhout's results by P. Deligne (in October 1980) and by Mebkhout (in detail in November 1980, at a conference in Moscow). These two authors made essential use of the God's theorem in their demonstration of a famous conjecture known as the Kazhdan-Lusztig conjecture, even before the Colloque de Luminy in June 1981 - Compare the quotation from Zoghman Mebkhout's letter in
the note "A feeling of injustice and powerlessness" (note n◦ 44").

(June 3) For further details on the solidarity of all Colloquium participants, see the following note "The Colloquium", n◦ 75'.

I assume that it was taken for granted, a little (a lot) like in the volume of the lecture Notes LN 900 which, the following year, was to consecrate the re-entry of motifs on this same "public square"[6] (******); that paternity belonged to the most brilliant among the brilliant mathematicians who had taken the initiative of the Colloquium and

p. 287 had animated it. What everyone knew for sure was that it was neither Riemann nor Hilbert, otherwise the brilliant Colloquium would have taken place in 1900 and not in 1981, two years after the pupil's thesis defense.

Unknown by Jean-Louis Verdier.

The kind of operation I've witnessed here is perhaps now commonplace[7] (*) and perfectly acceptable, as long as it's carried out by mathematicians who are at the top of their game, and the one who pays the price is a vague unknown (even though he's been kindly invited to join in the fun). The fact that one of these men is a great mathematician, both in terms of his means and his work (which puts him above suspicion from the outset), doesn't change the nature of the matter. Surely I'm old-fashioned - in my day, this kind of operation was called a **swindle**, and this one strikes me as a **disgrace** to the generation of mathematicians who tolerate it.

The brilliance of genius takes nothing away from such a disgrace. It adds an unprecedented dimension, perhaps unique in

p. 288 the history of our science[8] (**). Behind the apparent absurdity and gratuitousness of the act (carried out by someone whom fate has blessed beyond measure, yet who delights in plundering. . .), we can glimpse the action of forces other than the mere desire to shine, or the gratuitous desire to humiliate or despair those who feel defenceless and voiceless.), the action of forces other than the mere desire to shine, or the gratuitous desire to humiliate or despair those who feel defenseless and voiceless.

Since I'm definitely in the middle of a "tableau de moeurs", I'd like to point out (almost as a matter of course) that my name is equally absent from the quoted texts. Yet I was pleased to note that there is not a single page of the quoted article (among those in my possession[9] (*)) that is not deeply rooted in my work and bears its mark, right down to the notations I introduced, and the names used for the notions that come into play at every step - which are the names I gave them when I first became acquainted with them before they were named. There are, of course, some minor adjustments - for example, the biduality theorem that I had worked out in the fifties[10] (**) has been renamed "Verdier duality" for the occasion, still the same Verdier, there's no mistake. ... [11](***). However, it has not been possible for my name not to appear at least implicitly, through occasional references to texts that are still irreplaceable (despite SGA $4\frac{1}{2}$, which is not quite sufficient for its purpose), namely EGA and SGA. (In the explanation of the acronym SGA = Séminaire de Géométrie Algébrique du Bois Marie, my name of course does not appear, but in EGA, honest or not, the full designation is given, with the names of the authors including mine... .) Another detail that struck me, and which testifies to the obsessive strength of the burial syndrome (in someone who, however, has no obsessive "profile" whatsoever): the two references I saw to SGA make a point of explaining each time especially "Mr. Artin's theorem in SGA 4. ... . ", lest the misguided reader get the idea that said theorem might be due to the carefully non

[6](******) See notes n◦ s 51,52,59.

[7](*) I'm thinking of two other "operations" along the same lines, which took shape with the publication of LN 900 (see previous b. de p. note) and APG $4\frac{1}{2}$ five years earlier (see notes n◦ s 67, 67', 68, 68').

(May 9) For a third such operation, closely related to the previous ones, see the "Good references" note (n◦ 82). on another "memorable article", this time by J.L. Verdier.

[8](**)Nor have I ever heard of such a thing in the history of any other science or art than mathematics.

[9](*) (May 4) And the others too, of which I have since become aware.

[10](**) The same goes for the theory of dualité étale, which becomes "dualité de Verdier" under the pen of his generous friend Deligne!

[11](***) (May 5) Compare notes n◦ s 48', 63" . Throughout this long Burial, which has been going on for nearly fifteen years, and throughout the discovery that the principal "anticipated deceased" has just made over the past month, J.L. Verdier seems decidedly inseparable from his prestigious friend, who lavishes him with the wreaths of flowers that are de

rigueur on this mournful occasion.

named, when it is quite clear that the presentation was indeed made, thank God, by a named author! (77)

It's all fair game in today's "beau monde", it seems. Without indulging myself (and p. 289 it's not meant for that. ...) this guéguère is not really detrimental to the anticipated deceased, whose symbolic remains are thus left to the vagaries of this fairground, which I have been discovering with wonder for barely two weeks. It doesn't gnaw at my life with the feeling of **iniquity** suffered in impotence. It hasn't broken the joy and impetus that carry me to the encounter with mathematical things and those of the world around me, nor has it burned the delicate beauty of these things in me. I can consider myself happy, and I **am**... .

And I'm happy too about my unexpected "return", the meaning of which had escaped me. If it were to teach me only what I have learned in these past days, this return will not have been in vain, as it has already fulfilled me (⇒ 76).

11.1.2. The symposium

Note 75 (June 3) I have received details of the other participants in the colloquium, which dispel all doubts. Although no talk by Mebkhout had been scheduled in the Colloquium's official program, Verdier was obliged to ask him on the spot and in extremis to give a talk, to make up for the shortcomings of one of the official talks (which had been entrusted to Brylinsk'i, who knew little about D-Module theory). Mebkhout was thus able to set out his ideas and results, and in particular the Good God Theorem, in such a way as to leave no doubt as to the authorship of this theorem, and of the philosophy that goes with it, which had led to the spectacular revival of the cohomology of algebraic varieties, culminating in this Col- loque. So, **all the participants in the colloquium were made aware of this paternity**, through this presentation. I also assume that all of them, without exception, have since been acquainted with the Colloquium Proceedings, and in particular with the Introduction and the cited article by Beilinson, Bernstein and Deligne. Not a single one, apparently, found anything wrong with it - or if they did, they didn't let on. Zoghman Mebkhout received no such feedback. So, all the Colloquium's participants can justifiably be considered to be in solidarity with the mystification that took place during the Colloquium.

This collective mystification was already clear at the Colloquium, since no one found anything wrong with the fact that, in Deligne's oral presentation on so-called "perverse" beams, the name of Mebkhout is not pronounced. The speaker confined himself to stating the good Lord's theorem, saying that he wasn't going to demonstrate this in his talk. He made it clear, moreover (with the modesty with which he p. 290 is accustomed to) that "there was no merit" in guessing the extraordinary and a priori unpredictable properties of the beams he calls "perverse", obviously suggested by the "Riemann-Hilbert correspondence" he had just mentioned[12] (*). Everyone found it normal that he should refrain from naming the person who had had the "merit" of discovering this providential correspondence, and that he should give the appearance that the author was none other than himself, even though they had just learned, or would learn in the following days, that this was not the case. It must have been some sort of inadmissible misunderstanding that a vague participant in the Colloquium should be the author of such a remarkable theorem, and everyone did their utmost to rectify the situation and establish a consensus which attributed authorship to the one who was clearly the right person for the job - the one who **should have** been the author[13] (**).

[12](*) Compare with pages 10 and 11 of the article quoted.

(June 7) For details on the art of escamotage, see the following note "Le Prestidigitateur", n° 75".

[13](**) (June 5) everything fits together! The reflection that continued in the "l'Elève" procession (following on from the "Le Colloque"), and a certain tone as well (notably again in a recent and brief exchange of letters with Deligne, see first footnote to the note "Les obsèques", n° 70), show me that for Deligne and my other cohomology students, it is clear that

Characteristically, **Mebkhout's paper does not appear in the Colloquium proceedings**. Verdier had asked Mebkhout not to write his paper, saying that the Colloquium was intended to present new results, whereas Mebkhout's had already been published for over two years.

When you don't get bogged down in a technical discourse, and look at what's actually been done, you'll be able to see what's really going on.

p. 291 the forces and appetites that animated uni 'and the others, you'd think you were watching a film about mafia rule in the underworld of some distant Megapolis, It's
The actors are among the noblest jewels of French and international science. The Grand Chef, who runs the operation with his finger on the pulse, is none other than the man who once looked to me like a modest, smiling spiritual son, or at least a (no less modest and smiling) legitimate heir. As for the one who can be drilled and cut, the "soft" one in a world of "hard" ones who don't give quarter, by a strange "coincidence" whose meaning I still don't fully grasp, he too is closely linked to me. He's my "pupil" like the Great Chief (and like him, "pupil" with quotation marks...) - the one who took me on when I'd already been declared dead and buried for years... .

11.1.3. The conjurer

Note! 75" (June 7) The "memorable article" (referred to in the previous two notes) displays a consummate art of casual evasion. The equivalence of categories that has been the essential motivation of the whole work is introduced for the first time in a sentence in the fourth rage of the Introduction (page 10, lines 9 to 15), without giving it a name, only to be followed immediately by the kyrielle of consequences for the notion of the so-called "perverse" bundle (pages 10 and 11). No further mention is made until the end of page 16, when we read[14] (*):

"We would like to point out that on the following points, **which would have found their place in these notes**, we have failed in our task.

- The relationship between perverse beams and holonomic modules. As mentioned in this introduction, it has played an important heuristic role. The essential statement is 4.1.9 (**not proved here**). . . "

(To continue with other "points that would have found their place. . . ")

I hasten to find out what this "essential statement" is that the authors haven't found the leisure to include in their work, or at least not to demonstrate. Let's look for it:° 4.1.9. ... I'm looking for an "essential statement", a theorem in the form of a scholia, with a reference **where** the authors have demonstrated it or are going to demonstrate it, since they don't prove it **here**. . .

p. 292 But no matter how hard I look, there's no trace of a "theorem 4.1.9" - there's only one passage that answers the number 4.1.9- So I start reading the "remark" at random (without conviction- there must be a mistake of
numbering. ...), I read that "the analogue of 4.1.1 in complex cohomology is true... . "Unfortunately, I'll have to go back to 4.1.1 to find out what it's all about. I skipped over it and skimmed through the text that followed - and lo and behold, I couldn't believe it, eleven lines later, a sentence that starts with "We know that... . ." and ends with "induces an equivalence of the category . . with that of perverse beams".

Phew - so that was it after all! But no matter how hard I looked, I couldn't find the slightest hint to clarify that cryptic "We know that... . ". Readers who didn't already "know" it must be feeling pretty silly, not to the

it's been a long time since Deligne should have been the one to discover and master staggered cohomology; and at a certain level (that which commands behavior and attitudes), they're convinced that it's really him, next to whom I'd be a sort of clumsy, clumsy auxiliary who would be more detrimental than anything else to the harmonious unfolding of a theory (leading to Deligne's theorem-ex-Weil's conjectures) and to a distribution of roles satisfactory to all concerned. . .

[14] (*)Emphasis added.

the situation. What's clear to him in any case (apart from the fact that he's not up to it), is that this result "which would have found its place in his notes", which is "recalled" here in the course of a technical remark - something the reader should know anyway - is obviously due to the authors of the "notes" in question, or to one of them; the most prestigious perhaps and who wrote the article (there's an unmistakable "house style" .), or the one who gave the oral presentation, whose well-known modesty prevents him from saying "it's me! - but everyone understood without having to say it...

It immediately brings back memories of my reflections over the last few weeks. The very first is Deligne's first work in 1968, which I finally (sixteen years later) took the trouble to look at a little more closely in the note "L'éviction" (n° 63) of April 22 (three days after the discovery of the pot-aux-roses LN 900). Here I find the same style, with variations no doubt due to the intervening thirteen years of "breaking-in". In the 1968 article, whose main inspiration came from me, he names me in passing and in a sybilline way towards the end of the article, just to be "in order". Here, he no longer takes such care - experience has long since shown him that there's absolutely no point! On the other hand, in the article of his young age, since he felt obliged to name me, he compensated by entirely retracting the initial motivation for his work (and the yoga of the weights with it, only to release it under an alternate paternity six years later, while awaiting the exhumation of the motives eight years later still... .). In any case, even hiding (and keeping for his own benefit. ...) the article's essential arithmetical motivation, it "stood up," this article was perfectly understandable, living up to the author's reputation for doing things perfectly. p. 293
Here, the theory he develops would be incomprehensible without the heuristic motivation. So he points to the latter, referring to it as "the essential statement", while treating it from under the leg - without honouring it with a name, or a formal statement baptized theorem or proposition, there isn't even a "correspondence" (known as Riemann-Hilbert) - he left that to his friends Verdier and Teissier. He doesn't have to give it a name (given the few[15] (*) - surely he'd demonstrate it in five minutes!) or name anyone - others will take care of that for him and to his complete satisfaction. There is clearly a yoga, a philosophy, that the author handles with perfect mastery and authority, without having to name anything - this "little" that he pretends to disdain ("which would have found its place in these notes"), he knows full well he'll get more of, as long as he knows how to keep quiet and wait. The first time he played this game successfully, the "few" were "weight considerations" alluded to in a sibylline remark (waiting to bring out the philosophy of weights with great fanfare, six years later). The second time, as far as I know, was when I left in 1970 - the "little" was the "dream of motives", which for twelve years didn't deserve to be honored with a word (just think - a dream, and a dead man's dream at that, not to mention unpublished!), while we wait to discover the real motifs this time (and what we can do with them) and to claim, as modestly as ever, undisputed authorship[16] (***).

11.1.4. Perversity

Note 76 (May 4) I well remember the first time I heard the name "faisceaux pervers", must be two or three years ago, that it struck me unpleasantly, arousing in me a feeling of unease. This feeling reappeared the two or three times I heard this unusual name again. There was a sort of inner "recoil", which remained at the surface of my consciousness and would have been expressed without a doubt (if I had stopped to examine it), but which was not.

[15](*) (June 14) To put this "little" in context, I'd like to remind you that Deligne devoted a seminar at the IHES to trying to develop a translation of constructible discrete coeffi cients in terms of continuous coeffi cients, without arriving at a satisfactory result. On this subject, see the note "L'inconnu de service et le théorème du bon Dieu", n° 48'.

[16](**)For further comments on this technique of "appropriation through contempt", see the following day's note, n° 59'.

then) by something like: what an idea to give such a name to a mathematical thing! Or even

p. 294 any other living thing or being, except at a pinch a person - for it is obvious that of all "things" of the universe, we humans are the only ones to whom this term can sometimes be applied. ...

It seems to me (although I'm not entirely sure) that it was none other than Deligne himself who first spoke to me about so-called "perverse" beams, when he dropped by my place after the Colloque de Luminy[17] (*). It must even have been one of the last mathematical conversations between us - there were no others after his visit. It was precisely during this visit that this "sign" appeared, which led me a few weeks or months later (while this sign was reconfirming itself in the exchange of mathematical letters that followed this encounter) to put an end to a communication on the mathematical level[18] (**). (For this episode, see the note "Two turning points", n° 66.)

Coming back to the so-called (wrongly i) "perverse" beams, it's obvious that "normally", these beams should have been called "Mebkhout beams", which would only have been fair. (On more than one occasion, I've named mathematical notions I've worked out and studied after predecessors or colleagues who were much less closely associated with them than Mebkhout was with this beautiful notion - which, incidentally, would seem to me to be more "sublime" than perverse!) The circumstances in which Deligne found himself at the time he was discovering and naming this notion derived from Mebkhout's work, preparing to rob him when he himself was already "fulfilled beyond measure" - these circumstances can rightly be called "perverse". Surely my friend himself must have felt it in his innermost being, at a certain level where one is not fooled by the facades one likes to flaunt. I sense in the attribution of this name (which seems aberrant at first sight) an act of **bravado**, a kind of drunkenness in a power so total, that it can even allow itself to display (symbolically, by the display of a provocative name whose true meaning no **one** will allow themselves to read!) its true nature of "perverse" spoliation of others.

p. 295 It seems by no means impossible that at some deep level, I perceived the tone of these dispositions in my friend, and that this contributed to the unease I mentioned[19] (*). This uneasiness was expressed in particular by my inattention to the explanations he had to give me, although I don't think there had been an occasion before this meeting when I hadn't followed what he was telling me with sustained attention, and especially when it concerned mathematics. There was a kind of blockage in me with regard to this notion called (God knows why) "perverse" - I didn't really want to hear about it, even though it was very closely linked to issues I was (and still am to some extent) very close to.

In fact, the whole article by Deligne et al. was typical "**grothendieckery**" and all

[17](*) If this is indeed the case (as I'm now convinced it is) I must give credit to my friend's modesty, for I had no idea (on a conscious level at least) that it was none other than he who had introduced and named them. I had to read the "memorable article" to realize this.

(May 28) To tell the truth, the article doesn't say this any more than it says that Deligne is the father of the Riemann-Hilbert correspondence. However, I had no doubts about his authorship of the term "faisceaux pervers", which was subsequently confirmed to me.

[18](**) On a purely personal level, this relationship continued in the same tone of affectionate friendship as before, with no apparent change. My friend used to come every other year or so to visit me, usually on some kind of hike. I did have a visit again last summer, which was a welcome opportunity to get to know his wife Lena and their infant daughter Natacha. I think it was on the way back from yet another Colloque de Luminy, about which I've heard very little (apart from a few vague, morose allusions from Mebkhout, who had been given the honor of being invited again, and who could think of nothing better to do than to get back into the game... .). They stayed at my place for two or three days, and the contact was excellent all round.

[19](*) I would even be inclined to think that this is indeed the case. On more than one occasion, I've been able to see for myself the extent to which the deepest perception of things is of a fi nesse and acuity that have no comparison with what skims the surface at the conscious level. The fully "awakened" man is undoubtedly the one in whom these perceptions are constantly integrated into conscious vision and conscious experience - the one who lives fully according to his true means, and not just on a paltry portion of those means.

that could just as easily have come from my pen (with the sole exception of the name of the main concept)! It's something I've already expressed in the second part of the previous note (n° (75)), and something I've also sensed from the moment I read the article quoted - but without this diffuse feeling yet being embodied in the striking observation I've just made. It makes me aware once again, in a striking way, of the profound contradiction of the person who cannot help (in a certain sense
sense) to reproduce and assimilate the very one it is a question of denying, of handing over to disdain - the one it is a question of burying, and who is also at the same time the one one **wants to be** and that (in a certain sense) one **is**. p. 296

The day before yesterday, as I was writing the previous note ("l'Iniquité - ou le sens d'un retour"), I had already been struck by the coincidence that this turning point in the relationship between my friend and me, suddenly impoverished of a communion in a common passion, which had been its raison d'être and most powerful mainspring, took place on my friend's return from that memorable Colloque, the meaning of which had just revealed itself to me. What had puzzled me at our meeting in July '81, which on one level was as friendly and affectionate as on the other occasions we met, was this "sign", discreet in tone and air, yet brutally obvious, of a deliberate gesture of disdain. It was like a sort of **down payment** that my friend was making, this time at the level of a personal relationship, on the implicit and equally "discreet" (and just as "brutally obvious") disdain that he had just publicly expressed towards me, as a public figure, at the Colloque de Luminy, in the context of a brilliant display of technical virtuosity between the stars of the day. It was the same "disdain" that had just been expressed (but this time with an altogether different "perverse" brutality) towards the man who had dared (even a little) to claim to be me, and who had thereby condemned himself to be, for my friend Pierre (at a certain level at least), nothing more than "another Grothendieck"[20] (*) who had to be crushed at all costs. . .

11.1.5. **Pouce!**

Note 77 (May 5) Another detail struck me as I perused this memorable article[21] (**) which dominated (at this that no less memorable Colloque de Luminy in June 1981. The last chapter, under the suggestive title "From F to C", describes at length a remarkable principle that I had introduced into algebraic geometry twenty years ago - that must have been before the birth of the notion of pattern (which in
gives the most deep illustrations, via Weil's ex-conjectures). This principle ensures that for some p . 297
In the case of statements concerning schemes of finite type over a body, it suffices to prove them over a finite base body (i.e., in a situation "of an arithmetical nature") to deduce their validity over any body, and in particular over the body of complexes - in which case sometimes the algebraic-geometric result envisaged can be reformulated by transcendental means (e.g., in terms of integer or rational cohomology, or in terms of Hodge structures etc.)[22] (*). My friend learned this from none other than me and from me, on numerous examples over the years[23] (**). The authorship of this principle (which in an elementary form is even spelled out in EGA IV - don't ask me which paragraph and which number... .) is well known[24] (***). So much so that

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[20](*) In our personal relationship, my friend calls me by the affectionate diminutive (of Russian origin) of my first name, Alexander, which is also what my family and closest friends have called me since childhood.

[21](**) See note n◦ 75 about the "memorable article".

[22](*) (May 6) It seems to me that the first example of the use of such a principle can be found in Lazard's theorem on the nilpotence of algebraic group laws on the affine space E (over any body). I was struck by his demonstration, and drew inspiration from it for a number of other statements, as well as for a "philosophy" that has dominated my thinking on pattern theory.

[23](**) See the note "Eviction" (n◦ 63) for one such example.

[24](***) (June 5) It is perhaps abusive for me to claim to be the "father" of a principle whose first known application is by Lazard (see previous note (*)). My role, as on other occasions, was to sense the generality of someone else's idea,

When my brilliant friend was awarded the Fields Medal at the Helsinki Congress in 1978, N. Katz couldn't resist mentioning it in passing in his speech in honor of P. Deligne, thus rectifying a somewhat embarrassing systematic "oversight" on the part of his illustrious laureate. I read this speech just a few days ago, along with the "memorable article" itself.

In any case, in this article, the philosophy behind the transition from "arithmetic" to "geometry" is presented in such terms that there can be no doubt in the mind of an uninformed reader that the brilliant lead

p. 298 author (excuse l'impair. . .) has only just discovered this wonderful principle of such far-reaching significance.

It's true that I haven't patented the method, and nowhere does my brilliant friend say that he's the brilliant inventor; nor does he claim in plain English that he's the father of that famous "correspondence" (admire the term, which smacks of his nineteenth century!) modestly attributed to Riemann and Hilbert (men worthy of sponsoring the children of such a prestigious successor) - nor does he specify in the "memorable volume" (LH 900) that it was indeed he who invented motives, motivic Galois groups and the whole philosophy that goes with them (and of which he has still only released a fragment). There's nothing to be said either for this famous SGA $4\frac{1}{2}$, where I've even been honored to be listed as a "contributor" to this volume, which so brilliantly develops ab ovo étale cohomology, deigning to call on (despite their regrettable gangue of superfluous details etc.) the two satellite volumes SGA 4 and SGA 3.) to the two satellite volumes, SGA 4 and SGA 5, which have been consigned to oblivion, but to which I am generously credited with providing a few technical additions and digressions (some of them even "very interesting")[25] (*).

In all these cases - and in many other micro-cases I've witnessed over the last five or six years, without it ever occurring to me to **pinpoint my discomfort** and give a name to what I was witnessing or co-acting in[26] (**) - in all these cases, I recognize the same **style**. My friend is always and totally "**thumbed**" - he can help himself at ease, with the complete good conscience that comes from admiring his peers and his blunders (with all due respect), guaranteeing total impunity.

11.1.6. The Chinese emperor's robe

p. 299 **Note** 77 (May 7) Of course, those who see what my friend Deligne is doing and are in the know at all for the ins and outs, I mean those who haven't just learned about the maths "being done" from the publications of the person concerned himself, or other brilliant (though not always golden) stars of his generation - these colleagues (and they're not that rare after all!) are well aware, at **some level**, of what's going on. They must have sensed, in the "big" cases, that particular uneasiness that I myself have felt on more than one occasion in the face of these "micro-cases" a hundred times less serious than the "big" ones.

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and systematize it to the point of making it a "reflex" or "second nature". In the context of the yoga of weights and patterns, it's likely that the first to use this principle was Serre (not me), with his idea of virtual Betti numbers, which set me on the path to just such a general yoga of weights and patterns. (See note n◦ 46_9 for Serre's idea in question.) It is
It's also true that it's common practice to attribute the authorship of a "principle" of reasoning that has become commonplace, not to the author, but to the author's "principle".
It's not the first time we've seen a trace of it, but the one who first perceived its general scope, systematized and popularized it. In this sense, we can say that N. Katz's correction (mentioned in the following sentence), attributing the paternity of this principle to me, is justified.

[25](*) For details of the "SGA AT operation", see the four notes "La table rase", "L'être part see the four notes "La table rase", "L'être à part", "Le Feu vert", "Le renvers- ment" (notes n◦ s 67, 67', 68, 68').

[26](**) The first step towards "pinpointing my discomfort" in a specific case was taken in Récoltes et Semailles less than a year ago. three months ago, in the reflection (which turned out to be quite laborious - and with good reason◦) "The note, or the new ethic" (section 33). This reflection was taken up again in a note to that reflection, "Le snobisme des jeunes, ou les défenseurs de la pureté" (note n◦ 27), and then again less than two weeks ago (under the impact of the discovery (the day before) of the "memorable volume" (LN 900)) with note n◦ 59: "La nouvelle éthique (2) - ou la foire d'empoigne". As I wrote this, a nuance remained in my mind
I had no hesitation in using the rather crude term "foire d'empoigne". The discoveries that have followed since have shown me that no hesitation was required.

bigger than these. But what they sensed was so **enormous**, so **incredible** that it must never have surfaced - as it finally began to surface with me, in the course of a **work**, which expressed itself in these two texts around a micro-case referred to in the previous b. de p. note. Indeed, I've never heard of anything like it in the history of our science or any other. Instead of "surfacing", for some people "it" must have **become the norm**, or at least been considered **normal** - as long as an obviously brilliant man, admired by all, practised it with the greatest naturalness in the world, in full view of everyone and without the thing ever (as far as I know) eliciting the slightest comment.

Over the past few days, I've been reminded many times of the tale "The Dress of the Emperor of China", in which the aforementioned emperor, deceived by unscrupulous swindlers and his own vanity, announces that he will appear in a solemn procession wearing the most sumptuous garments the world has ever seen, prepared for him at great expense by so-called tailor artists. And when he appears in the procession, surrounded by the pomp and circumstance of his Court in full regalia, the "artists" bowing and scraping, and the entire imperial family, no one in the procession or among the people gathered to contemplate the seventh wonder dares to believe the testimony of his eyes, and everyone makes a point of admiring and raving about the unsurpassable splendor of the garments with which he is now adorned. Until a small child who had strayed into the crowd exclaimed, "But the emperor is naked!" - and then all of a sudden the whole crowd, as if with one voice, cried out with the little child: "But the emperor is naked!

And I feel like the little child who believes the testimony of his eyes, even though what he sees is quite unheard of, never seen before and ignored and denied by all.

Whether the child's voice will be enough to bring some back to the humble testimony of their healthy faculties, that's another story. A tale is a tale, it tells us something about reality - but it's not reality[27] (*). p. 300

11.1.7. Encounters from beyond the grave

Note 78 (May 6) It's only been five days since I received this generous package of documents from my friend Zoghman Mebkhout, including above all the two texts already examined from the "memorable Colloquium" - that Colloquium built around a monumental **mystification**! The note "l'Iniquité - ou le sens d'un retour", in which I try to assimilate the quite incredible meaning of this new "event", was written on the very day (the day after May 1st) that I received these documents, still in the emotion of discovery[28] (**).

Since April 19, when I finally became aware of the "memorable volume" of the Notes readings (LN 900 - see notes (51) (52)), this has been the third great discovery on the subject of the solemnities of the Great Burial, and the one that seems to me to be of the greatest significance, both in terms of the light it sheds on the actions of the "Great Burial".

[27](*) (June 14) After writing this note, the name "The robe of the Chinese Emperor" struck me as a natural sub-title for the Burial, expressing a particularly striking aspect of it. Later, as the focus shifted to my students as a whole, and even to "the entire congregation" of the Mathematical Establishment, this subtitle seemed less appropriate. However, I've come to realize that the parable that first came to mind when thinking of my friend Deligne, applies equally to all aspects and adventures of the Burial, which at every step reach the Ubuesque in the unbelievable (which everyone makes a point of modestly ignoring) that is nonetheless true. For reflections along these lines, see in particular the notes "On n'arrête pas le progrès!", "Le Colloque", "La Victime - ou les deux silences", "La plaisanterie - ou les deux silences", "La plaisanterie - ou les deux silences", "La plaisanterie - ou les silences", "La plaisanterie - ou les silences".

ou les complexes poids", "La mystifi cation", "Le Fossoyeur - ou la Congrégation toute entière" (n° s 50, 75', 83, 85', 97), none of which particularly concern my friend Pierre.

[28](**) Along with the section "The note - or the new ethic (1)", this note is the only note or section I've had to rewrite several times, because what "came out" in the first version (and even in the next one) was weighted down by the inertia of my customary vision of things, which fell far short of the reality I was examining.

of people to whom I have been closely linked, than by its implications as a "tableau de moeurs" of an era, apparently unique (but it is true that I am ignorant of history. . .).

p. 301 The second discovery had closely followed the first - that of the exhuma□tion of the "motifs", for twelve years buried. After the "memorable volume", I was treated to the "memorable seminar" - that "seminar" that never took place, given a bogus name (both SGA and number 4 1/2), and enriched with the "State 0" of a phantom thesis, not to mention a central presentation from the (real) SGA 5 seminar (which appears later, even though it predates it by twelve years); a presentation "borrowed" for the purposes of the operation without further ado. This brilliant operation, and the role it played in the strange vicissitudes that befell this poor SGA 5 seminar (dismantled from the head, the tail and the middle!) were gradually revealed in the course of a reflection that continued between April 24 and 30. (See the five notes "Le compère", "La table rase", "L' Etre à part", "Le signal", "Le renversement", n° s 63"', 67, 67', 68, 68'.)

As soon as I had digested this discovery, and as my retrospective reflection on "Mon ami Pierre" drew to a close, and on April 30 I had proudly put the final and definitive mark (that was a sure thing!) on my life, I decided to take the plunge.
- this time I was finally there i) under this interminable Burial, with the "final note" with the doubly euphoric name "Epilogue - ou l'Accord Unanime" - that I receive this package of misfortune, which calls into question final point, epilogue, page layouts and numbering... . A quick glance at the documentation and the accompanying annotations and letters made it clear that my period was gone, as were the beautiful arrangements for a first-class Funeral, the final details of which I was about to polish - I was ready to take up the master of ceremonies' harness. . .

God knows my friend Zoghman had plenty of time to inform me of the situation! It must have been going on for ten years in latent form, and three years at least in "acute form" (and that's putting it mildly) - ever since the Colloquium in question, where he must have sensed the wind without having to wait for the publication the following year of the highly official "Proceedings" under the patronage of his illustrious ex-patron and protector.

A few months after he had defended his thesis (in February 1979), he had come to bring me a copy to the
p. 302 village where I had lived for six years. Unluckily, I had just left (never to return).
return, except in passing...) a few days before, to retire in solitude. He only met my daughter, who later handed me the thesis. It was the following year, I think□that we finally got to know each other, in college.
in Montpellier, where we chatted for an hour or two. I wasn't really into maths at the time, and couldn't really remember either a thesis I'd flipped through in a few minutes, or the name of its author. That didn't stop the contact from being warm. I remember an immediate current of mutual sympathy. We didn't talk so much about maths (not that I can remember), but mostly about more or less personal things. Zoghman told me afterwards (something I'd forgotten) that he'd been able to explain the D-Modules "philosophy" to me a little, and that he'd been pleased with the meeting, to have felt me "vibrate" if at all by learning new things from him, and yet also (in a way) "expected". What I remember most of all was the impression he made on me - an impression of stubborn, calm strength, that of a "go-getter". At the time, much more than when we met last year or during the correspondence that followed, I had the impression of a strong affinity of temperaments - this "go-getter" side in particular. But the two or three years that have elapsed between the two encounters seem to have dented it quite a bit. ...

I don't remember Zoghman telling me at our first, brief meeting about the isolation in which he had worked, the lack of any encouragement from the "luminaries" who had been my students. If he hinted at it, he must not have insisted. Even then, the whole thing didn't appeal to me.

surprise[29] (*). I couldn't say whether this was before or after the Colloque de Luminy in June 1981[30] (**). If it was afterwards, he would still have had some hot stuff on his stomach - and he really didn't give the impression of it. Rather that of a man who knows what he wants to do and what he wants, and who follows his instincts.
quietly, without seeking trouble and without being sought out.

We didn't continue then to write to each other. But I remembered him well, and early last year I wrote to him p. 303
I wrote him a note, at random, to ask if he might be in a position to tackle a magnificent work on the foundations of a "moderate topology" which (it seemed to me) was just waiting for someone of his calibre to take it up. Although Zoghman didn't make it clear to me at first, it turned out that he wasn't really interested in this prospect - on the other hand, he seemed happy to seize the opportunity of a new encounter. At the time, I was too out of the loop to fully appreciate the situation, and imagined that D-Module theory was now a done deal, as is, say, coherent duality theory (78_1), and that Mebkhout had perhaps run out of "big tasks". It was only when we met last summer that I realized that in the very theory he had started, there was no shortage of "big tasks" - and some of them had not even been started, because they had not even been seen!

In any case, it was a perfect opportunity for a second meeting, and this time not as casual as the first. Zoghman must have stayed at my place for maybe a week last summer, in June I think. Mathematically speaking, our meeting served mainly to bring me up to speed as best we could on D-Module yoga. I've been slow to "thaw out", having lost touch with my old cohomological loves, and being mostly embroiled in the writing of "Poursuite des Champs", which is set in rather different registers. Zoghman wasn't discouraged to see me listening with a slightly distracted ear, and returned to the charge without tiring, with a touching patience. I was finally triggered, I think, when I realized that these famous D-Modules were nothing other than what I had long ago called **module crystals**, and that as such they still made sense in singular spaces. All of a sudden, I saw a whole network of intuitions from my crystalline-differential past rising up from forgotten depths, and slightly rusty reflexes from my "six operations" past being reactivated...

Perhaps it was Zoghman who was a bit of a loose cannon, or maybe it was more that he decided afterwards that he wasn't going to risk his fingers in that particular gear (any more than my friend Pierre wanted to put his - although he'd been all fire and brimstone while I'd been around... .). (⇒ 78')

Note 78_1 There are, however, a number of "fine" results of consistent duality, notably on the struc- p. 304
ture of "dualizing differential modules", their relation to "naive" differential modules, and trace and residue applications in the non-smooth flat case, which I had developed in the late fifties
which, to my knowledge, have never been published. Nevertheless, for the most part, the theory of coherent duality (in the schematic framework at least), as well as that of stellar duality (and its variant for the discrete cohomology of locally compact spaces, developed by Verdier on the stellar model), or linear algebra or general topology, appear to be theories that have essentially **been completed**[31] (*), in the nature, therefore, **of tools that are** perfectly perfected and ready for use, and not so much of a **substance.**

[29](*) (May 30) That's not quite true - I'm reprojecting more recent disillusioned dispositions onto the past. When I met Zoghman just last summer, I remember being surprised that none of my cohomology students (Deligne, Verdier, Berthelot, Illusie in particular) had supported Zoghman in his work. This surprise was repeated when Deligne came to see me ten days later (I must have mentioned something to him about Zoghman, but I got no response) and by the
(See "Mystification", n° 85').

[30](**) (June 3) That was back in February 1980, a year after he defended his thesis.

[31](*) (June 12) This is not quite true for stale duality, until the purity conjectures and the "biduality theorem" are proved in all generality.

that would have to be penetrated and assimilated.

11.1.8. The victim - or the two silences

Note 78 We met in an atmosphere of friendly trust and affection. This atmosphere, however, did not live up to its promise. I realize now that from that moment on, my friend's trust was far from complete. It was two years after the famous Colloque, and a year after the publication of the "Actes" in Astérisque[32] (**)-at a time when he was the victim of a scandalous spoliation.

But he didn't bother to inform me until just four days ago! When he came last year, he was returning from
p. 305 another Colloque de Luminy[33] ('***) (this time squarely on the theme of D-Modules)'où où on which he had again generously invited and rushed to attend. He spoke of it in terms both bitter and vague, suggesting that now that he'd pulled the chestnuts out of the fire, it was "the others who had done it all". I could imagine the picture indeed - especially Verdier suddenly remembering the paternity of the triangulated categories (and derived ones too, while we're at it!) he had left to one side for ten or fifteen years, barely tolerating his "pupil" Mebkhout's use of them in his work... . (81).

Although he didn't want to explain himself clearly at the time, Zoghman seemed to have his heart set on Verdier, which was understandable given his ex-boss's less-than-encouraging behavior. And yet, my other cohomology students - Deligne, Berthelot, Illusie - hadn't bothered to take an interest in what he was doing or to support him in any way. But it almost seemed as if Zoghman took this for granted, having never (or so it seemed) experienced anything other than this attitude among his elders. If he held a grudge against any of my former students, it was solely and exclusively against Verdier.

From Zoghman's hints (which he obviously didn't want to spell out), I understood that "they" were systematically putting the scope of what he'd done - period. This is, after all, the most common thing in the world. Since judging the importance of a thing is largely subjective, it's commonplace and almost universal to attribute more merit and importance to one's own work, to that of one's buddies and allies, than to that of others, and especially to those one feels like minimizing for one reason or another. (And the "reason" in this case wasn't exactly a mystery to me!) Nothing could have made me suspect that, far beyond such common attitudes, there was here an operation of pure and simple swindling, where there was no question whatsoever of "minimizing", but rather of **swindling** Mebkhout's authorship of the ideas and results that were breathing life back into where there had been stagnation... .

And yet, if there was one person in the world to whom it was natural for my friend to open up, it was me. whose work had inspired him during those years of obstinate work, sometimes bitterly, against the fashion of the day - I who received him affectionately in my home, making myself a bit his pupil at my
p. 306 learning as best I could what he took pleasure in teaching me[34] (*).

[32](**) (October 9) Zoghman tells me that these "Actes" were not published until early 1984.

[33](***) (May 7) There's a slight memory lapse here - I think he was getting ready to go to the Colloquium. At the time, of course, there was no shortage of reasons for those "bitter terms" (and vague ones) I remembered. But this bitterness was further heightened by his visit to Luminy after his stay with me. I had echoes of it in a phone call he gave me on his return from Luminy. From that moment on, I had the distinct feeling that he had come to Luminy for the pleasure of being mistreated by "the people" (without really asking me which ones) who had generously invited him, for the pleasure of being able to treat him as a negligible quantity. I must have told him so, or let him know, which must not have improved my friend's attitude towards me.

[34](*) Zoghman didn't tell me about mine, and he didn't tell me about his own funeral either, even though he'd had a front-row seat to the proceedings for nearly ten years! To tell the truth, his "protectors" (a little reluctant on the edges) had even agreed to let him carry with his hands a small corner of the coffin carrying my remains - but they couldn't forgive him for being the only one among the guests who sometimes took the liberty of uttering the name that all the others kept quiet!

So my friend must have felt at odds in his relationship with me, and he couldn't find it in himself to take on the responsibility.

After my friend had passed through an atmosphere of warm affection, there was an immediate "backlash". I had the impression that he had decided to transfer to me the mistrust and bitterness that had built up in him over the past eight or ten years, under the sting of the indifference and disdain he had encountered in some of my former students. In the months that followed, the correspondence between us never left the aigredoux register - it finally stopped with a New Year's greetings card, which never received a reply.

It was only at the end of March that I contacted Zoghman again, to send him "Le poids d'un passé" and the notes I had then added to this section (n° s 45, 46, 47, 50). It was to ask him if he would agree to my including him, as I had done, in the short reflection on my work (in the note "My orphans", n° 46), when it would be clear to all that I was using information he had given me, and which he might consider confidential. I was by no means sure that my friend would not prefer (like others before him) to "crush rather than displease". It would have hurt my feelings if he had.

It took me a long time to get her answer, which I received only ten days later. I was somewhat expecting her to would still be half-flesh, half-fish - but this time she was downright warm. He'd give me his p. 307
I agreed wholeheartedly, even emotionally, with the terms in which I spoke of him.

It's on page 6 of his long (eight-page) letter that he points out, as if in passing and with reference to the "impressive number" of applications of his theorem ("both in the framework of stale topology and in the transcendental framework") that it still appears in the literature under the name of the "Riemann-Hilbert correspondence"[35] (*). He says it in such an almost incidental way, and with such a delightfully illegible handwriting, that it almost went completely unnoticed! But then I remembered, it really was a strange thing. So strange, in fact, that it hardly seemed believable, and then perhaps my friend was exaggerating, obviously he was angry with everyone, including me, even though I only wanted good things for him. So I added a note (holy Zoghman, I thought I'd finished!) called "L'inconnu de service et le théorème du bon Dieu", in addition to two others "L'instinct et la mode - ou la loi du plus fort" (I'd also thought a lot about him, among others, when writing it) and "Poids en conserve et douze ans de secret". This note on "L'inconnu de service", I wrote at first without total conviction; Zoghman seemed to me so knotted up and full of contradictions that I wondered what I was getting myself into by simply echoing him, without knowing the facts for myself. The thought hadn't occurred to me that there might be a scam, let alone that Verdier or Deligne themselves were involved. There was nothing in what Zoghman had told me to suggest this... .

Yet both of them were so closely linked to this theorem of the good Lord, that its authorship could hardly be concealed without at least their tacit agreement. It must have worked on me in the days that followed. I remembered that Deligne had given it a lot of thought, this problem solved (ten years later) by Zoghman - and then Verdier, after all, acted as research director; even if he didn't go out of his way for his pupil and would rather have beaten him cold and discouraged him than anything else, he
must at least have known what the two main theorems in this work were - Zoghman surely explained them to him, during those famous "interviews" that Verdier was kind enough to grant him! I have therefore added to the note on p. 308
a commentary on the relationship of Mebkhout's work to an earlier attempt by Deligne, and a b. de p. note on the role of Verdier. At the same time, it was also a sounding board for my friend

a past fraught (as mine was) with ambiguity, and speak to me plainly and clearly. Talking about his funeral also meant talking about mine and the role he himself had played in it ... . In any case, if I ended up discovering this famous funeral in all its splendor, it was against a kind of "conspiracy of silence" that encompassed both my friend Zoghman and my friend Pierre - and no doubt most of the friends I had in the "great mathematical world".

(June 3) For further details, see note n° !78" below.

[35](*) See the quotation from his letter in the note "A feeling of injustice and powerlessness", n° 44.

Zoghman...

You'd think that Zoghman would jump at the chance to finally, finally reveal his batteries, hidden for three years, which will finally bring out the clear truth and triumph the cause of the oppressed! But not at all! Fifteen days of silence, followed by a letter about everything (in maths) except God's theorem - or rather, he confined himself to giving me the precise reference in his thesis, which I had asked for. (I still wanted to know where this famous theorem, to which I was so firmly committed, had been proved!)

In my reply to this letter, I had to say a few words to him about the "vast swindle with regard to my work" I had just discovered (with the "memorable volume" LN 900, and moreover "promising me much pleasure" in the days to come in making the acquaintance of SGA 4[1] in the college library) - so[2]that after another ten days' silence, my friend finally got in touch!

This time, at last, he "pulled out all the stops" - a **great deal**, in fact, of judiciously-chosen documents, enabling me (who hardly ever haunts libraries, or even the piles of separate prints piling up in my office at university... .) to give me a well-balanced idea of an "atmosphere", in which many of those who didn't take part in my long and solemn Obsèques still remain[36] (*). Alongside the main "piece of evidence" (the two articles from the famous Colloquium, exposing the incredible mystifi- cation), and another "memorable article" (this time from the pen of Verdier[37] (**)). there was the speech by N. Katz on the "Fields Laureate" Deligne, plus a presentation by Langlands and another by Manin at the same Helsinki Congress 1978; then Deligne's "Théorie de Hodge I" at the Nice Congress 1970 (where he is made
p. 309 another allusion in line 3 to a "conjectural theory of Grothendieck's motives" (78_1), and "Weight in the Cohomology of Algebraic Varieties" by the same Deligne, Vancouver Congress 1974 (where my name is not mentioned (78_2)); plus finally a correspondence with A. Borel (yet another old friend, whom I learn at the same time is back in Zurich. . .), and two notes to Mebkhout's CRAS, one of which from 1980 is a summary of Chap. V of his thesis (passed the previous year), giving a little more emphasis to the theorem of the good Lord[38] (*). Not to mention another document - shhh! communicated under the seal of secrecy, and of which I won't say another word here. ... .

Two letters accompany this substantial dispatch (letters of April 27 and 29), one very long and both substantial. Now that he's finally let the cat out of the bag (the real one, this time!), Zoghman continues to urge me to exercise extreme caution, as he had been doing ever since I contacted him again. If I listened to him, I'd be careful not to make public my reflective notes, which would remain an absolute secret between him and me - at least not the part that implicates anyone, since "they" have "all the power" and "everyone is with them"[39] (**)! And yet, I had warned Zoghman that these notes, from which I sent him the extracts concerning him, were destined to be made public, and as soon as possible.

All the elements seem at last to be in place for the just cause of the oppressed to triumph, but the "victim" seems to be doing everything in his power to continue muddying the waters as if by magic.

[36](*) (June 12) Katz, Manin, Langlands don't seem to be part of it. . .

(March 1985) For a different take on Katz, see "Dotting the I's", n◦ 164 (II5), and "Maneuvers" (n◦ 169), "Episode 2".
(April 1985) Similarly for Langlands, see note "Pre-exhumation (2)", n◦ 175 . 1

[37](**) For more on this article, see "Les bonnes références", n◦ 82.

[38](*) For a precise reference for this note, Mebkhout's thesis and the Good God Theorem, see the note "Le pavé et le beau monde - ou vessies et lanternes", n◦ 80.

[39](**) (May 30) Carried away by my impetus, I'm exaggerating a little here. At no time did Zoghman suggest that I refrain from publishing this or that part of my notes. Lately, he's even been insisting that these notes should actually appear in book form, for the benefit of "posterity", whereas a limited edition like a preprint seems to him to be a bit "like a sword in the water".

secret regret (one would say) at having sold that famous "fuse" of which Zoghman must have been (until the fateful May 2) the one and only holder. This ambiguity is apparent in every line (I'm hardly exaggerating), right down to the latest letters I've just received - including the very last one, in which he sends me, with an air of sombre triumph, the "memorable article" in its entirety (whereas, with the "big package" he first sent me, he had only managed to part with the first twenty pages of this masterpiece[40] (***)).

As for the friend Pierre I mean Deligne (who is neither Pierre nor "friend" to everyone. . .), it's p. 310
it's just that he doesn't sing its emotional praises - it seems that it's no longer he, Zoghman, who's the "victim", but no, it's Deligne, poor fellow, who's been so badly influenced by those around him - the only villain, and the one who surrounded him so badly, is Verdier (and yet. . . follow my gaze instead, . .): it's clear that I "must have done something" to Verdier for him to be such a coward for the sheer pleasure of doing harm, not to mention the fact that I was also his boss and I was also the one who awarded him the title of doctor and the glory and all the rest - the means, in short, of "absolute power"![41] (*)

Clearly, if my friend has a grudge against anyone, it's not really against his illustrious ex-boss, whom he's only had the honor of meeting for an "interview" three times in ten years in all (if I've understood correctly what he wrote to me most recently) - a vertiginously distant man, entirely out of reach - but it's the one he can come and see whenever he pleases, and share both his bread and his lodgings... . [42](**).

Each time Zoghman takes a new step to divulge some new element, making me a little more aware of a situation of despoilment in which he is the victim (and can help a little to unravel it), I feel that it's like a **wrench**, the culmination of an exhausting inner struggle. I
has a **role** with which he seems to have identified body and soul, clinging to it as if it were his most precious
possession - a role with which he seems to have identified body and soul, clinging to it as if it were his most precious
possession.
this role of victim which he can only maintain by keeping around this role and the situation that justifies it, p. 311
absolute secrecy[43] (*). And he may indeed be torn, and resent me more than ever, at this moment when, with his reluctant collaboration (snatched away, as it were, by the logic of a situation created by none other than me, with those unfortunate reflections on an uneventful Funeral. . .), this secret will come to an end, and with it, perhaps, this role in which it has pleased him to maintain himself, for how long I cannot say.

This "burial" of my friend Zoghman was achieved by the combined care of **two silences**, each responding to the other and provoking it in turn, in a seamless round in which the role of one closely matches the role of the other - the despoilers and the despoiled. If on more than one occasion I was struck by the fact that the "burier" was at the same time, and more profoundly, his own "buried", I was equally struck by the fact that in the person of another friend, the "buried" was at the same time, and more profoundly, his own "burier" - in close connivance.

[40](***) (October 9) Zoghman told me that, in fact, he didn't have a Xerox of the complete article in his possession at first, which he pulled out only later.

[41](*) It's not the first time I've heard this "absolute power" claptrap, with which one would like to convince oneself of one's own powerlessness and justify it. If anyone has invested anyone with "absolute power" over himself, Zoghman, it's none other than Zoghman himself!

[42](**) (May 8) It's no coincidence, moreover, that the unequivocal signs of conflict in my friend's relationship with me appeared in the very aftermath of this stay, when he "shared my hand and my bed" in an atmosphere of unreserved affection, abolishing a feeling of "distance" that our first brief encounter no doubt couldn't entirely erase.

Here I come across a situation with which I have long been familiar, and which I discuss (in relatively general terms) in the two notes "The Enemy Father (l), (2)" (sections n◦ s 29, 30). Little did I suspect, when I wrote them as a commentary on the preceding reflections, the extent to which the archetypal situation I describe there would become the constant focus of a a long reflection yet to come, just when I thought I was nearing the end of my journey!

[43](*) (May 30) Since these lines were written (May 6), my friend's attitude has changed dramatically, and lately I've seen no signs of attachment to a victim role. It goes without saying that the lines which follow (like those which preceded) concern certain episodes in my friend's life, and in no way claim to define a temperament or describe a permanent bias.

with the very people whose willing victim he delights in being.

And I can see that the person primarily responsible for his own spoliation is none other than my friend Zoghman himself, who for three years has acquiesced by his silence to his humiliation by those who take their pleasure in him. He had everything in his hands to fight for - and for three years he chose to forget he even had hands, and to be defeated without having fought[44] (**).

Note 78_1 I had never held this short preliminary communication in my hands, but only the more circumstantial "Hodge Theory II, III" publications that appeared in Publications Mathématiques. This is why I had been under the impression that Deligne had not seen fit to ever allude to a role

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motive theory in the genesis of his ideas on Hodge theory. I thought that if he had wanted to mention any role I might have played with him[45] (*), he would probably have done so with "Hodge Theory II", his thesis work, which was the perfect opportunity to mention such things[46] (**). I've just seen that he's fulfilled the formality of mentioning me once and for all, with this lapidary line[47] (***) alluding to "Grothendieck's conjectural theory of motives", with even a reference at the key (to Demazure's talk at the Bourbaki seminar).

Once again, nothing to say! The idea never occurred to him to specify that he had learned this theory (all conjectural, let's not forget!) from **another source** than this meagre text by Demazure, which can give no image of a theory of great richness (all conjectural!), which runs like a thread through all Deligne's subsequent work on weight yoga - pending the escalation of the "pirate volume" LN 900, where the motivic Galois groups are finally exhumed (fifteen years later) (this time without even a laconic reference line containing the name of the deceased. . .).

On reflection, in this laconic quote, I recognize the same "thumb!" style. - a quote from pure form, to be fair, with a reference that is in no way likely to enlighten the reader (in this case,

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on obvious and deep-rooted relationships with ideas that it is precisely to hide[48] (*) - and which have remained hidden during the twelve years that followed), but **of a nature to deceive him**.

Note 78_2 I didn't have to hold this[49] (**) text in my hands (which I learned about a few years ago). weeks) to know that my name wasn't on it. Nor was Serre's, who was the first to glimpse a "philosophy of weights", which I later worked out in great detail.

[44](**) (May 30; This is an admittedly subjective view of someone with the temperament of a fighter, someone in whom this fi bre might have seemed absent. It would seem, however, that since these lines were written, my friend's fighting spirit has been reawakened, and he is determined to fight back against an iniquity of which he has been the victim.

(April 18, 1985) For a different, less "harsh" take on my friend's provisions, see also the "Roots" note (n◦ 171).[3]

[45](*) (May 30) Until a few weeks ago, I systematically downplayed this role. See note "Being apart" n◦ 67' of May 27, where I first became aware of this attitude in myself and perceived its meaning.

[46](**) (May 30) Nor do I remember being asked to sit on the thesis jury. The funeral was already going well train. . .

[47](***) Serre is also implied in the same line by the cross-reference sign [3] - the curious reader will find his name in the bibliography at Hodge I. This expeditious reference line is the only one between 1968 and the present day where there is any allusion (however cryptic) to the "sources" it mentions in a single breath: Serre (alias [3]), motifs, Grothendieck. ...

(May 28) However, I have since come across another such allusion, very interesting in view of the very special occasion. On this subject, see the note "L'Eloge Funèbre (1) - ou les compliments" n◦ 104 , and the end of the note that precedes it ("Le Fossoyeur - ou la Congrégation toute entière" n◦ 97), situating this "particular occasion".

[48](*) As I write these lines, I am reminded of a revealing incident involving "weights" two years earlier, mentioned at the start of the note "Canned weights and twelve years of secrecy" (n◦ 49), and in more detail at the start of the note "The eviction" (n◦ 63). For the "pouce! style" in general, see the reflection in the note "Pouce!" (n◦ 76). It's a style with which I'm becoming quite familiar!

[49](**) "Weights in the Cohomology of Algebraic Varieties", by P. Deligne, Vancouver Congress 1974, Proceedings, pp. 78-85.

11.1.9. The Boss

Note! 78" (June 3) Zoghman explained to me that he only gradually became aware, and confused at first, of the "swindle" that was going on around my work. The manuscript Verdier had given him in 1975 (see "Les bonnes références" note n° 82) had been providential for him, notably in introducing him & to the notion of constructibility and its essential properties, as well as to the biduality theorem, from which he had drawn inspiration for the biduality theorem (or "local duality") in the context of D-Modules. It was only years later, while reading SGA 5 (a massacre edition, admittedly, but not so massacred as to give an attentive reader like him the slip) that he began to realize something. For a long time, he had been filled with admiration and gratitude for his distant elder, convinced that the ideas he drew on were his own. In fact, it would seem that for years he was convinced that the theory of duality known as "de Verdier" was indeed due to Verdier, or at least to "Serre-Verdier", and that the idea of duality he calls "de Poincaré-Verdier" was also due to Verdier. It was only around 1979 (the year he defended his thesis) that he began to realize that there was something wrong - but I presume he had to be careful not to let anything show about it to his prestigious "boss", nor to me, when we met in February 1980 and June 1983.

It was only with the Colloque Pervers, in June 1981' then that he began to sense the escamotage that was p. 314 that he also began to realize more clearly the world he had strayed into[50] (*)! Surely, for him, I had to be part of that world, where my former students (or at least some of them) had the upper hand and plundered the posthumous pupil with the same casualness as the departed master. The only difference, as it turned out, was that I was dead and they were alive and kicking. ...

I can imagine that even after the Colloque Pervers, Zoghman still found it hard to believe the testimony of his healthy faculties, which told him quite clearly what had happened. He only received the famous Introduction to the Colloquium Proceedings, signed by B. Teissier and his "patron-sic" Verdier, in January 1984. After denying the evidence for almost three years, the shock was all the greater, I understand. It was two months later that I contacted him again, sending him the notes "Mes orphelins" and "Refus d'un héritage - ou le prix d'une contradiction" at the end of March, and it was another month later that he finally decided to "let me in on the joke" and tell me about the "Mystification du Colloque Pervers".

11.1.10. My friends

Note 79 And here I am about to finish and make public this reflection which will put an end to the secrecy that Zoghman himself has maintained around the spoliation of which he is the victim, and from which he also reaps the obscure benefits[51] (**). Perhaps it will be unwelcome to him, just as it may be unwelcome to my friend...

Pierre, to whom I'll hand-deliver it as soon as it's finished and the text cleaned up and printed[52] (***).

The best thing I have to offer my friend Zoghman as well as my friend Pierre, perhaps both the p. 315

[50](*) Zoghman ended up having such a low opinion of his ex-boss, that he was convinced that everything Verdier had done in the sixties (which I review in a b. de p. note to note no.° 81 "Thesis on credit and comprehensive insurance") had been more or less dictated or at least blown by me.

[51](**) (May 30) I would remind you that this reflection was inspired by a disposition in my friend that now seems outdated. (Compare two b. de p. notes of May 30 with note n° 78'.)

[52](***) I didn't think I'd ever have the chance, in the years that lie ahead, to go back for a few more years. days in the capital. But my friend Pierre has travelled often enough, for more than ten years, to meet me deep in the remote countryside, for me to travel on this exceptional occasion, following up on an often-repeated invitation that has never yet been put to good use.

Will they receive it as the worst: as a calamity, or as an outrage. All the worse, since my testimony is public - just as the silences of both have been rites of public acts, and which commit one as they commit the other.

Whether they reject or accept my testimony is their choice, and the same goes for Jean-Louis, whom I counted among my friends, just as Zoghman and Pierre do today. These choices affect me closely, and they are not mine. I have no temptation to predict what they will be. It won't be long before I know, and I await what the weeks and months ahead will bring with intense interest, suspense - and without a shadow of anxiety. My only concern and responsibility is that what I offer is the best I have to offer - that is, to be true.

Some may be surprised that I speak so bluntly of people I call "friends", and see in this name a stylistic clause, or even an intonation of irony that is absent. When I refer to Zoghman Mebkhout or Pierre Deligne as "friends", it's a reminder of the feelings of sympathy, affection and respect that are within me as I write. Respect tells me that I don't have to "spare" a friend, any more than I have to "spare" myself - like me, he is worthy of encountering the humble truth, and no more than me, he needs sparing.

If I don't refer to Jean-Louis Verdier as a "friend", it's in no way because I consider him less "good", or less "deserving", than my friends Zoghman and Pierre, or myself, but because life has distanced us from each other. The feelings of sympathy and affection that bound me to him fifteen or more years ago have more or less faded with time, and have not had the opportunity to be revived by any kind of personal contact. The few attempts I have made to re-establish such contact have met with no response, and I don't know whether reading these reflections will revive a relationship that had frozen. But even though he's no longer a "friend" to me, I don't think I'd be disrespecting him by not treating him more kindly than myself or my friends, and I know that to do otherwise would be a disservice to him or anyone else. Not to mention that both he and my friend Pierre, if any

that they insist on "defending" (or attacking) themselves rather than taking the risk of looking at themselves, are not lacking in means or support. And not to mention that where they have had the opportunity to discourage

or to crush, more than once both have done so, ruthlessly and mercilessly.

p. 316

11.1.11. The pavement and the beautiful world (or: bladders and lanterns. . .)

Note 80 (May 9) It's about time I finally gave a reference for this famous Riemann-Hilbert-(Deligne qui ne dit pas son nom) theorem - Adam and Eve - bon Dieu - (and especially not Mebkhout), which everyone quotes at length (including myself), and for which apparently nobody has yet thought to ask where it is demonstrated. Having been led to believe by my friend Zoghman that the "me- morable theorem" was to be found in his thesis, I did indeed find it in the table of contents, under the name (admittedly down-to-earth and worthy of a cad) "Une équivalence de catégories", Chap. III, par. 3, p..
75. To make matters worse, it's not even entitled to the name "theorem" but is called "Proposition 3.3" (and what's worse, my name appears, again underlined, on the same page). I'll even admit that, having failed to read the previous 75 pages to recognize myself, I wasn't entirely sure whether this was it - Zoghman confirmed that it was, and I trust him[53] . The demonstration (it would seem) is the subject of Chap V of the same thesis - which was passed at the University of Paris VII on February 15, 1979, before a Jury comprising D. Bertrand,

[53](*) (April 17, 1985) It would appear that the generally used form of the "God's theorem" is not that of the theorem quoted here, but a related form demonstrable by the same methods. See the note "Eclosion d'une vision - ou l'intrus" (n◦ 171_1 , and in particular today's b. de p. note therein.

R. Godement, G. Houzel, Le Dung Trang, J.L. Verdier. Interested parties who have not yet received a copy from the author (who sent his thesis to all those he could suspect, rightly or wrongly, might be interested) need only ask him, and he will be pleased to do so... ... Of course, he sent a copy to each of my former cohomology students, none of whom has been heard from since. They must have changed subject in the meantime, unluckily. ...

It has to be said that Zoghman definitely doesn't have the knack of selling his merchandise, of presenting it in a clear and appealing way - these are things that have to be learned, and he wasn't as lucky as my former students to learn the ropes from a virtuoso of the trade who didn't skimp on his time. But he can't complain, he's had his "three interviews", and perhaps one day one of the "luminaries" will have the idea of even acknowledging his indigestible pamphlet. He must have realized himself that the paving stone was not easy to read (even if it wasn't lost on Riemann or Hilbert. . .): he wrote a note to the CRAS, which is still shorter, to draw attention to his famous theorem, the title of which I'll give you in a thousand: "On the Hilbert-Riemann problem"! I knew my friend Pierre Deligne wasn't any better at history than I was, so all he had to do was restore the chronological order, and contribute the pretty folklop designation . 317 "Zoghman had it coming. ... This note is dated 3.3.1980, Series A, p. 415-417.

Verdier must have learned of the theorem in one of the "three interviews" he gave to his pupil - sic (or at the time of the defense), but he must not have realized it. As for Deligne, he finally realized something, I can't say when, but what's certain is that he knew about it in October 1980, and so did Bernstein and Beilinson, according to what he himself says. Mebkhout himself went to Moscow to explain his results (at length) to Beilinson and Bernstein (in case they had trouble reading him). I don't know if they or Deligne ever read the thesis or the subsequent note to the CRAS, but they must have figured out what was in it, since next year's "memorable Colloque" at Luminy was, coincidentally, all about it.

To sum up, and according to the latest information from my intelligence service, there were at least five people perfectly aware of the situation, who took part in the mystification known as the "Colloque Pervers", namely (in alphabetical order of the actors) A.A. Beilinson, J.Bernstein, p. Deligne, J.L. Verdier and Z. Mebkhout - plus a whole Colloque acultees, surely brilliant mathematicians to boot, who apparently wanted nothing better than to be mystified and take bladders. Mebkhout - plus a whole Colloquium of aculturous people, surely brilliant mathematicians to boot, who apparently wanted nothing better than to be mys- tified and to take bladders for lanterns[54] (*). Which proves once again that we mathematicians, from the illustrious Medalist to the obscure unknown student, are not a hair smarter or wiser than the average person.

11.2. VIlI L' Elève - alias le Patron

11.2.1. Credit thesis and comprehensive insurance

Note 81 (May 8) It seems time to express myself in more detail on the "thesis-" affair. p. 319 phantom", which I had only mentioned "in the aftermath" in two previous notes (notes (48) and (63''')). An inattentive or ill-disposed reader might say that I am simultaneously reproaching my ex-student J.L. Verdier for two contradictory things - for having "buried" the derived categories, and for having "published" them

[54](*) (June 3) In fact, it appears that all Colloquium participants, without exception, had been briefed on the situation on the spot. On this subject, see the note "Le Colloque", n° 75', written today.

(in SGA $4\frac{1}{2}$) and claim authorship; just as the same reader would say that I reproached P. Deligne for both "burying" the motifs, and exhuming them (in LN 900). So it may not be superfluous to give a retrospective of the situation, from 1960 to the present day.

Around 1960 or 1961, I proposed to Verdier, as a possible thesis project, the development of new foundations for homological algebra, based on the formalism of derived categories that I had developed and used in previous years for the purposes of a coherent duality formalism in the context of schemes. It was understood that in the program I was proposing to him, there were no serious technical difficulties in prospect, but above all a conceptual work whose starting point was acquired, and which would probably require considerable developments, of dimensions comparable to those of the Cartan-Eilenberg book of foundations. Verdier accepted the proposed subject. His foundations work continued satisfactorily, materializing in 1963 in a "Etat 0" on derived and triangulated categories, multigraphied by the IHES. This 50-page text is reproduced as an Appendix to SGA $4\frac{1}{2}$ in 1977.
(as stated in note (63' "))[55] (*).

p. 320 If the defense didn't take place in 1963, but in 1967, it's because it was unthinkable that this 50-page text, the embryo of a foundational work yet to come, could constitute a state doctorate thesis - and the question of course didn't even arise. For the same reason, when he defended his thesis on June 14, 1967 (before a Jury including C. Chevalley, R. Godement and myself, who presided), there was no question of presenting this work as a thesis. The text submitted to the jury, 17 pages long (+ bibliography), is presented as an **introduction** to a major work in progress. It outlines the main ideas behind this work, placing them in the context of their many uses. Pages 10, 11 give a detailed description of the chapters and paragraphs planned for this seminal work.

If the title of Doctor of Science was awarded to J.L. Verdier on the strength of this 17-page text, outlining ideas which he himself says are not his own[56] (*), it was clearly a contract of good faith.

p. 321 between the jury and himself: that he was committed to completing and making available to the public this work for which he

[55](*) This text alone may seem a meagre result for two or three years' work by a gifted young researcher. But most of Verdier's energy was then devoted to acquiring the indispensable basics of homological algebra and algebraic geometry, by attending my seminars in particular, and by working one-on-one. His contributions to the duality formalism (see below) came later, once Artin and I had developed the stale duality formalism in detail in SGA (1963/64), when I suggested (in parallel with his work on the foundations of derived categories) that he develop the same formalism in the context of "ordinary" topological spaces and smooth morphisms of such spaces.

It was around the time I began my "Séminaires de Géométrie Algébrique" series with SGA 1 (in 1960) that I was contacted by Verdier, along with Jean Giraud and Michel Demazure, asking if I had any work for them - and they were knocking at the right door! A coincidence that struck me from the moment I wrote the note "Mes Orphans" (n◦ 46) when the three of them contacted me, they had just formed a small seminar called the "Orphans' Seminar" (on the theme of automorphic functions, approaching calculations with a zinc strand), given that their boss (or godfather to the CNRS?) had just left for a year without warning, leaving them hungry and a little empty. This void was quickly filled... .

[56](*) The beginning of the thesis reads:

"This thesis was written under the supervision of A. Grothendieck. The essential ideas it contains are due to him. Without his initial inspiration, his constant help and his fruitful criticism, I could not have completed it. I would like to express my deep gratitude to him.

I would like to thank Claude Chevalley for chairing my thesis jury and for his patience in reading this text. My thanks to R. Godement and N. Bourbaki for introducing me to mathematics.

"The term "this thesis" can hardly refer to anything other than the body of foundational work undertaken, of which the text submitted constitutes the introduction - work which was therefore not, strictly speaking, "completed" at the time of the defense.

(May 30) This inconsistency reflects the ambiguity of a situation for which I was primarily responsible, as thesis director and (if the cover of the copy of the thesis in my possession is to be believed) as president of the Jury. At
For me, the lack of "rigor" towards a brilliant pupil is a complacency in the same direction as the one I showed towards Deligne (see the note "L'être à part", n◦ 67'), and has contributed its share to bringing about the same results.

presented a brilliant introduction. This contract was not kept by the candidate[57] (*): the text he announced, a text on the foundations of homological algebra from a new, proven point of view, was never published.

Clearly, if Verdier's work between 1961 and 1967 had been limited to writing the skeletal "Etat 0" of 1963, the jury would not have considered accepting this "thesis on credit". The writing of his work had to be sufficiently advanced to allow completion within a year or two, and for practical reasons it seemed appropriate that Verdier should have the title without waiting for the work on which it was based to be completed.

It should be added that between 1964 and 1967, Verdier had made some interesting contributions to duality for- malism (81_1), which, together with the foundational work he was supposed to be pursuing, could justify the credit given to him. His contributions to duality as a whole could, in a pinch, have constituted a reasonable doctoral thesis. Such a thesis, however, would by no means have been in the style of the work I am accustomed to proposing, all of which consists in the systematic development to completion of a theory whose need and urgency I sense (82_2). I don't recall Verdier ever raising the question of presenting such a "thesis on titles", and I doubt I would have accepted, since such a thesis would have in no way corresponded to the "contract" that was signed between him and me, when I entrusted him with the beautiful subject of derived categories, with the task of developing foundations on a vast scale.

As J.L. Verdier's thesis supervisor and president of the jury, I accept full responsibility for my thoughtlessness in awarding him (jointly with C. Chevalley and R. Godement, trusting the
the title of doctor on work that had not yet been done[58] (**).

I am not justified in complaining if I now see some of the fruits of my levity. But this p. 322
does not prevent me from stating this publicly, and that the actions of my ex-student J.L. Verdier are his responsibility alone, and that of no one else.

Not to keep the contract he had made with me and with the Jury who had placed their trust in him, was a way of burying the point of view of derived categories that I had introduced and that he had taken on the task of founding through a major work. This work may have been done, but it was never made available to the user. It was a way of "writing off" a set of ideas that he himself had helped to develop.

Mebkhout's revival of the notion of derived category met with no encouragement from Verdier (nor, for that matter, from any of my other cohomological "luminaries"). The de facto boycott of derived categories seems to me to have been total until about 1981[59] (*), when they made their comeback in force at the "memorable Colloque" at Luminy (see note (75)), under the sudden impetus of need.

However, State 0 of Verdier's "thesis" had already been published four years earlier, in 1977, as an appendix to the

[57](*) It is all the more remarkable that J.L. Verdier should have refused my proposal to sit on Contou-Carrère's thesis jury in December 1983, with J. Giraud, and myself acting as research director, believing that the thesis (though entirely written and carefully read by J. Giraud) and the jury would not offer sufficient guarantees of seriousness, without referring to the control of a Commission des Thèses des Universités **Parisiennes** (Sic).

[58](**) To this responsibility, I should add that of not having ensured, during the two years that followed (before my departure from the mathematical scene) that Verdier actually kept to the contract he had made. It has to be said that my energies were so focused on pursuing the foundational work I had taken on myself, not to mention motivational reflections and the like, that I didn't have to think too much about the unpleasant task of reminding others of their obligations. I had to learn of Verdier's decision to abandon the publication of the planned work in the early '70s, at a time when I was absolutely no longer into maths, and when the idea would not have occurred to me to "react".

[59](*) (May 30) These somewhat dubious forms of style are in fact out of place. As Zoghman Mebkhout (who paid to find out) confided to me, what I said about the status of homological algebra "Grothendieck style" corresponds to reality.

volume SGA $4\frac{1}{2}$ (see note n° 63"') - so ten years after the defense of his thesis, and at a time when (to my knowledge(*)) Mebkhout was the only one to make use of derived categories in his work, against the fashion of the seven years that had preceded. Unless I'm mistaken(*) , he remained the only one to do so, right up to the time of the great "rush" around the famous "Riemann-Hilbert correspondence" at the Colloquium already mentioned, where Deligne alias Riemann-Hilbert appeared as the father of this "correspondence" - sic, and Verdier (with his providential Etat 0 abundantly quoted by his generous friend) appeared as the father of derived categories and algebra.

p. 323 homological style 2000, with no mention of my humble self and even less of Mebkhout[60] (**).

In light of these events, I believe I understand the reason for the unexpected publication of this State 0 which (as it says in the introduction to SGA $4\frac{1}{2}$ by the same friend) "had become unobtainable" and that nobody cared about "finding", except (perhaps) Zoghman Mebkhout[61] (*). So there was just this one unfortunate fellow who, in his own corner and against all odds, persisted in using these notions of a bygone age, without anyone really knowing what he was getting at - so stubborn, in fact, that we began to doubt whether he wouldn't one day come up with something that would do the trick, you never knew... . After all, the man to whom he sometimes imprudently referred as one of his sources of inspiration (alongside the Master's works) had, in the past, proved or found things with all that, things that couldn't be ignored even if the author - and the Master himself - were forgotten, Jean-Louis Verdier himself, had he not made his start to stardom with this "Lefschetz-Verdier" formula, which he would have been hard-pressed even to write down, let alone prove, without all these notions fit for the dustbin. . .

While my influential ex-student of almost ten years (since he had rid himself of a certain annoying formality. . .) **was betting against** derived categories and would continue to bet against them until time X (of the famous Colloquium), he must have thought it prudent (you never knew. . .) to pre-empt events that might occur. .) to anticipate events that might occur, in other words, to take out "all-risk insurance", by publishing (not the large-scale work that was one day supposed to constitute a thesis, but) a "text-witness", a sort of exhibit "in case... . ."; a text that would "attest to his claim to paternity over an **orphan** whom he] had taken a liking to, and whom he continued, pending events, to disown[62] (**).

p. 324 **Note** 81_1 The contributions in question are: 1) Foundations of a duality formalism in the context. of locally compact spaces and 2) that of Galoisian modules (in collaboration with J. Tate); 3) the Leschetz-Verdier **fixed-point** formula; 4) duality in locally compact spaces.

Contributions 2) and 3) are "unexpected" compared with what was known. The most important contribution seems to me to be 3). Its demonstration follows easily from the duality formalism (both for "discrete" and "continuous" coefficients), which does not prevent it from being an important ingredient in the ar- senal of "all-purpose" formulas available to us in cohomology. The existence of this formula was discovered by Verdier, and came as a (pleasant!) surprise to me[63] .

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[60](**) Compare with the comments in the notes "Le compère" and "L'Iniquité - ou le sens d'un retour" (n° s 63"'and 75).

[61](*) In any case, it was while perusing the bibliography of a work by Z. Mebkhout that I had just received, towards the end of April, that I learned of the publication of this "State 0", when I had even forgotten the existence of this text from another age. ...

[62](**) If J.L. Verdier had really wanted to make known the yoga of derived categories, which has been buried for seven years, he would have chosen to publish the introductory text which constitutes his thesis, rather than a technical text which nobody cared about and which only acquires interest in the background of yoga and its many uses. But it's understandable that he had no desire to append to the 50-page text the 17 pages of his thesis, containing now embarrassing statements about the role of the one who must not be named. ...

[63](*) (April 19, 1985) I return to this beautiful formula, its role and its strange vicissitudes during the Burial, in the three notes "Les vraies maths. . . ", ". . and "nonsense"", "Tricks and creation" (n° 169_5, 169_6, 169_6), in the fourth part of Récoltes et Semailles.

The formalism of duality in the context of locally compact spaces is essentially the "necessary" adaptation of what I had done in the context of scalar cohomology of schemes (and without the difficulties inherent in this situation where everything was still to be done). He did, however, contribute an interesting new idea, that of the direct construction of the functor $f^!$ (without prior lissification of f) as a right-hand adjoint of $Rf_!$, with an existence theorem to boot. This procedure was taken up by Deligne in étale cohomology, enabling him to define $f^!$ in this framework, without any lissification hypothesis.

These comments make it clear, I think, that by 1967 Verdier had demonstrated his capacity for original mathematical work, which of course; was the determining factor in the credit he received.

Note 81_2 As another example, I would point to the detailed development of the duality formalism in the context of locally compact spaces, in the spirit of the "all-purpose" formalism of the six operations and derived categories, of which Verdier's presentation at the Bourbaki Seminar would constitute an embryo. Even in the context of topological **varieties** alone, there is still, to my knowledge, no satisfactory reference text for Poincaré's duality formalism.

(June 5) There are two other directions in which I note with regret that Verdier did not see fit to go all the way. p. 325
the end of work that he had started off strongly enough to **take credit for** (I mean, by starting up a duality formalism in the context of discrete coefficients and locally compact topological spaces), whereas the essential ideas are not due to him and he doesn't care (any more than for the derived categories) to make himself the **servant of a task** and put at the user's disposal a complete formalism (as I tried to do in the three seminars SGA 4, SGA 5, SGA 7).

The duality program that I was planning and that I suggested he develop was part of the
general topological spaces (not necessarily locally compact) and applications between such that are "separated" and locally "smoothifiable" (i.e. locally the source is immersed in a $Y \times \mathrm{R}^n$, where Y is the goal space). This was clearly suggested by the analogy with the stale cohomology framework
any schemes. Verdier was able to see, in the context of locally compact spaces, that the assumption of local smoothness of applications was unnecessary (which came as a surprise). Nevertheless, the context of locally compact spaces (which excludes "parameter spaces" that are not locally compact) is visibly short-sighted. A more satisfactory context would be the one that would cover both the one chosen by Verdier, and the one I was planning, i.e. the one where topological spaces (or even topos?) are (more or less?) arbitrary, and where applications $f : X \to Y$ are subject to the restriction of being 1) separate and 2) "locally compactifiable", i.e. X plunges locally into a compact $Y \times K$, K.

In this context, the fibers of an "admitted" application would be locally compact quel- conical spaces. Another step would be to admit that X and Y , instead of being topological spaces, are "topological multiplicities" (i.e. topos that are "locally like a topological space"), or even topos of any kind, by restricting the applications in a suitable way (to be made explicit), so as to find fibers that are **locally compact multiplicities**, subject if need be to additional conditions (close perhaps to the point of view of Satake's G-varieties), for example (and lastly, to the point of view of Satake's *G-varieties).*
rigorously!) to be locally of the form (X, G), where X is a compact space with **finite** operator group G.
To my knowledge, even the "ordinary" de Poincaré duality has not been developed in the case of multipli- p. 326
smooth compact topological cities (smooth: which are locally like a topological variety). The case of a classifying space of a finite group seems to show that we can hardly hope to have a duality theorem (absolute global) other than module torsion, more precisely, by working with a ring of coefficients that is

a Q-algebra. With this restriction, I wouldn't be surprised if Poincaré's duality ("six opera- tions" style) worked as is in this context. It's not surprising that nobody has ever looked at it (except some unrepentant differential geometers, pretending to look at the cohomology of the "leaf space" of a foliage), given the general boycott of the very notion of multiplicity, instituted by my cohomology students, led by Deligne and Verdier.

To put it bluntly, what's missing is a fundamental reflection of the following type: describe (if you can) in the context of any topos and bundles of "discrete" coefficients on them, notions of "cleanliness", "smoothness", "local cleanliness", "separation" for a morphism of topos, enabling us to derive a notion of "admissible morphism" of topos $f : X \to Y$, for which the two operations $Rf_!$ and $Lf^!$ make sense (one adjoint of the other) so as to obtain the usual properties of the six-operation formalism. Here, topos are considered as non-ringed, or perhaps as provided with Rings (which are assumed if necessary to be constant or locally constant), assuming (initially at least) which ringed topos morphisms $f : (X, \mathcal{A}) \to (Y, \mathcal{B})$ are such that $f^{-1} (B) \to \mathcal{A}$ **is an isomorphism** (81_3). The foregoing considerations suggest that when we restrict ourselves to Rings of coefficients of carac-

(i.e., which are Q-algebras), we can be much broader in the notion of "morphism".
admissible", so as to encompass "fibers" that are e.g. multiplicities (topological or schematic), rather than ordinary "spaces" (topological or schematic).

A first step in this direction (apart from the cases treated by me, then by Verdier on the same model) is due to Tate and Verdier, in the context of discrete or profinite groups. The memory of this first step encouraged me to pursue a reflection along these lines last year, in the context of small categories (generalizing discrete groups) serving as homotopic models. Without going very far, this reflection was nevertheless enough to convince me that there must be a complete formalism of the six operations in
p. 327 the context (Cat) of the category of small categories. (See on this subject the "Pursuit of Fields", Chap.VII, par.136, 137.) The development of such a theory in (Cat), or even in Pro(Cat), just like a theory of this type in the context of topological or schematic spaces and multiplicities, would for me have as its main interest to be a step towards a better understanding of "discrete duality" in the context of general topos.

Illusie told me last year that he had struggled with duality perplexities in the case of semisimplicial spaces (or schemes). It seemed to me to be the same old tobacco - to be able to detect the existence of a six-operation formalism in a particular case, and to understand it. But it would seem that the mere prospect of a fundamental reflection has the gift of freezing each and every one of my former students - at least among my cohomology students. If I went to any trouble with them, it was with the conviction that they would not stop right where they had left off (in terms of conceptual work) with me, and remain wringing their hands every time a new situation showed that the work they and their buddies had done with me was insufficient. The conceptual work we do is **always** insufficient in the long run, and it's by taking it up again and going beyond it, and not otherwise, that mathematics progresses. Between 1955 and 1970, each year again I found that what I had done in previous years was not sufficient to the task, and I went back to the drawing board, at least when someone else (e.g. Mike Artin}with the point of view of "algebraic varieties" in his sense) hadn't already done so. But it would seem that my students have also buried the example I set them, along with myself and my work.

Note 81₃ I seem to recall that in the formalism of the six variances in (say) staggered cohomology, the assumption that the ring bundles serving as coefficients are locally constant is unnecessary - the essential assumption is that they are prime torsion bundles with residual characteristics, **and** that $f^{-1}(B) \to A$ is an isomorphism. When we abandon this last assumption, we have to enter a theory (never yet made explicit, to my knowledge) that "mixes" the "discrete space" duality, and the "coherent" duality (relative to the rings of coefficients and their homomorphisms). As a result, we envisage
replace, on the diagrams (or more general topos) X, Y, the rings of coefficients A , B by relative (not necessarily affine) schemes X' , Y' on X, Y, and ringed topos morphisms p. 328
$(X, A) \to (Y, B)$ by commutative diagrams of the type

$$\begin{array}{ccc} X' & \xrightarrow{\;\;//\;\;} & X \\ \downarrow & & \downarrow \\ Y' & \xrightarrow{\;\;/\;\;} & Y \end{array}$$

with a "six operations" formalism in such a context. When X, Y, etc. . . are the ponc- tual topos, we should find the usual coherent duality.

11.2.2. The right references

Note 82 (May 8) This is J.L. Verdier's article "Classe d'homologie associée à un cycle", published in Astérisque n° 36 (SMF), p.101-151 in 1976. In a way, this rather unbelievable article (although nothing should surprise me any more. . .) is a counterpart to the "perverse article" by Deligne et al. With one reservation: it practically consists in **copying** over fifty pages, in a slightly different context, notions, constructions and reasoning that I had developed at length ten or fifteen years earlier - terminology, notations, everything is there verbatim! I'd have thought I'd returned to a session of the APG 5 seminar held in 1965/66, where these things were spelled out (apparently to the satiation of the participants[64] (*)) for an entire year. After this seminar, at least, all these things became part of the "well-known" for people in the know[65] (**) Verdier had attended, of course, as had Deligne (the only one who was never left behind, even though it was the first time he'd set foot in my séminaire[66] (*) - it took p. 329
do it...). It's true, well, well, that in 1976 the "writing-sic" of this famous seminar by "volunteers-sic" who were fed up with it had been dragging on for ten years - I see now that one of these "volunteers" took charge of the "writing" in his own way, even before the publication of SGA 5 in

[64](*)For comments along these lines, see notes n° s 68, 68' "Le signal" and "Le renversement", in which I examine the strange vicissitudes of the writing of this seminar, and the relationship between these and Deligne's "SGA 4 operation[1] , The following reflections are my own.
2
reveals another unforeseen aspect of these vicissitudes and of the dismemberment of the mother seminary by the combined efforts of Verdier and Deligne. Verdier's and Deligne's publications on this dismemberment date from 1976 and 1977 - they constitute the "green light" given to Illusie to prepare (eleven years later. . .) the publication of SGA 5 (which, Deligne dixit in SGA 4[1] , "can
2
be seen as a series of digressions, some of them very interesting").

[65](**) For a reflection on this "hasty" impression, see the note on "Silence" (n° 84).

[66](*) The year of this seminar was (I think) the year I met Deligne, who must have been nineteen at the time. He was "got into the swing of things" very quickly, and even took on the task of writing up my lectures on staggered duality from the previous year (which he must have known from my explanations and notes), and also the lecture on the cohomology class associated with a cycle, which was discussed in the note quoted n° 68' ("Le renversement"), and which will be discussed a little more in this one. The fact that with the means at his disposal, and a complete mastery of the subject, he waited eleven years to write the essay, to include it then in his SGA 4[1] without informing me, now shows me, in retrospect, that as early as 1966 (and not only as early as 1968 as
2
I may have assumed - see note n° 63, "L'éviction") - therefore as early as the first year of our meeting, there was a profound ambiguity in my friend's relationship with me, expressing itself from that moment on in a perfectly clear way, of which I I've refrained from reading it until now!

1977 ! It would appear that the vicissitudes of this unfortunate seminar were not only to Deligne's advantage, as he took advantage of the situation in his own way. But at the time, Deligne still took care to mention in his essay (on the cohomology class associated with a cycle) "d'après un exposé de Grothendieck"[2] (after a paper by Grothendieck), while dismantling SGA 5 from one of his key lectures and attaching it to his SGA 4[1] as a matter of course. (It's true that this compensated him for being able to present me as his "collaborator"! - see note "Le renversement", n° 68').

Coming back to the **homology** class (not to be confused!) associated with a cycle (which, according to the title, is the subject of Verdier's article), I had developed this formalism in great detail, over several presentations, during the oral seminar, before an audience that, incidentally, begged for mercy (except always the only Deligne, always dashing and fresh...). It was one of the innumerable "long exercises" I developed that year on the formalism of duality in the étale frame, feeling the need to arrive at a complete mastery of all the points I felt needed to be thoroughly understood. The interest here was to have a valid formalism on an ambient scheme that was not necessarily regular - the passage to the **cohomology** class in the case of

p. 330 regular, and the link with my old construction using cohomology with supports and immediately giving compatibility with cups-products, being immediate. I also found that this part of the seminar does part of the lot that didn't make it into the published version - no doubt Illusie (on whom all the hard work of preparing a releasable (hmm) edition eventually fell) must have been quite happy that Verdier took care of it, mutatis mutandis (i.e. here: without changing a thing!).

As the saying goes, "it hardly needs saying" that my name does not appear in either the text or the bibliography (except implicitly by the ever-present reference SGA 4, which we'd still have to find a replacement for... .). There's no allusion to a "Seminar on Algebraic Geometry" under the acronym SGA 5, which the author might have heard of - although I seem to remember seeing him busy taking notes (like everyone else, except Deligne of course. . .).

Incidentally, I've exaggerated just a little by saying that my name is absent from the text - it makes a single, mysterious and lapidary appearance on page 38, section 3.5, "Fundamental cohomology class, intersection" (here we come, the crux of the matter!). The reference consists of a cryptic sentence whose meaning escapes me, I confess: "The idea of systematically using weight complexes (??? those damn weights again!) is due to Grothendieck and was put into shape by Deligne" - without any further explanation of these mysterious "weight complexes" whose idea I had and of which I hear mention here for the first time. There will be no further mention of them in the rest of this article (nor was there any mention of them in the 37 pages before). Understand who can! As for the content of the said section, it is copied without further ado from the SGA 5 seminar which had taken place ten years earlier (and by which time this construction was already five or six years old, see note n° 68'), a seminar which he is careful not to quote. The reference to Deligne (who is said to have "perfected" an idea that had already been perfected when my friend was still in high school!) is a "flower", the idea of which no doubt came to the author because the young and newcomer Deligne had indeed taken on the task of writing my paper on this subject (and refrained from doing so for eleven years, for the benefits we know, see note cited). This "flower" is part of the exchange of courtesies between inseparable friends.

There is, however, one (undoubtedly) new and very interesting result in the article (th.3.3.1., page 9) on stability of discrete bundles analytically constructible by direct higher images through an analytic and proper

p. 331 morphism. Verdier had learned about all-round constructibility from me one day. fifteen years earlier, as well as the stability conjecture, which I had asked myself (and told who would listen) in the late fifties, before I had the pleasure of making his acquaintance. Reading the article, the idea wouldn't occur to an uninformed reader (but those are becoming rare. . .). I

I'm still repeating myself, I'm afraid) that the author isn't serving up hot-off-the-press notions and statements he's only just discovered. He doesn't have to say it's him - because it's self-evident. It's the famous "thumb" style that's so obviously catching on.

Apart from this detail (which, I feel, is in line with the new canons of the trade), there must still be around ten pages (out of fifty), around this interesting result, that present the author's personal work. All things considered, what strikes me most about Verdier, as with Deligne, is that he's perfectly capable of doing beautiful mathematics. Even in this sad article, there's a hint of it in the theorem quoted. But by keeping himself (like his friend) in a gravedigger's mood, he operates, like his prestigious friend, on a paltry fraction of his means. A sign (which astounded me) of apparent mediocrity, in a mathematician who nevertheless gave proof of as- tuce and flair, was the total lack of instinct to feel the scope of the work of his "pupil-sic" Mebkhout, whom he took pleasure in treating from the height of his greatness, without ever having been able to do himself a work of comparable depth and originality[67] (*). Not that he isn't just as capable as Mebkhout or me. But he has never given himself a chance to do great things, that is, to let go of a passion - rather than **using** mathematics and its gifts **to** dazzle, dominate or crush. Up until now, he's been content to take up as is the fruitful notions and points of view that have already been baked in. Indeed, he seems to have totally lost touch with the meaning of **mathematical creation**.

Yet I seem to remember that when he worked with me, that sense was still there. Nothing ex- ternal about him prevents that sense from resurfacing. Just as in his friend, in whom I often felt thisp . 332
same eclipse of something delicate and lively, obturated by the same fatuity.

This incredible 50-page article, which appeared in a standing magazine, sheds new light on the "The note - or the new ethic" incident (s.33). where a note to the CRAS of **a few pages**, summarizing a solid and **original** work, on an important subject (in my humble opinion), the fruit of **two years' work** by a highly gifted young mathematician, was rejected by two eminences as "devoid of interest"[68] (*). One of these eminences was none other than Pierre Deligne - the same Deligne who did not disdain to copy in toto and in person the humble doctoral thesis of one of my students (whom he made a point of quoting). (This duplicate, enhanced by a prestigious signature, makes the largest article in the "memorable volume" LN 900 of a no less prestigious collection! See end of notes (52), (67)).

The "tableau de moeurs" is growing by the day, without my having to come out of retirement and hit the streets to mingle with the "big world". A few hours spent here and there leafing through a few well-chosen "great texts" were enough to edify me...

11.2.3. The joke or the weight complex

Note 83 (May 8-9) I've been thinking about this "weight complex" referred to in the "reference - thumb" in Verdier's memorable article[69] (**) - a reference that's sheer nonsense. As soon as I saw this ludicrous reference, an association came to mind that kept running through my head. This isn't the first time, far from it, that I've found myself faced with something

[67](*) The same astonishing lack of flair was evident on this same occasion in Deligne, who didn't "feel the wind" (the importance of Mebkhout's ideas) until 1980 it seems, even though Mebkhout had been working in this direction since 1974. On more than one occasion, I've had occasion to observe my friend's natural flair blocked by suffi ciency, especially since 1977 (or 78), which seems to have been a first "turning point" (see notes "Two turning points" and "Funerals", n◦ s 66,70).

[68](*) For details, see the note "Casket 4 - or topos without flowers or wreaths", n◦ 96.

[69](**) See previous note "Good references".

that seem to defy rational explanation - even though the meaning is clear and unambiguous and clearly perceived, but at a different level from that of conventional logic. This was the only one on which I had consciously operated for most of my life - with the result that I was constantly overwhelmed by "bizarre", incomprehensible events - distressing in their irreducible saugrenousness! My life changed a lot from the moment (less than ten years ago) when I started
to live on a wider register of my faculties. I've come to understand that every absurdity, every so-called
p. 333 "nonsense" has a **meaning** - and the mere fact of knowing this, and from then on being curious about the meaning behind the nonsense, often
opens me up to its obvious meaning.

In this nonsense about "weight complexes", I think I sense an act of **bravado** of the same nature as in the appellation "faisceaux pervers"[70] (*) - the pleasure, in this case, of proving to oneself that one **can afford**, in a journal of standing and in a text that claims to be a standard reference text[71] (**), to say a related nonsense, and that **nobody** will dare to even ask a question! And I'm convinced that the wager contained in this bravado, in the eight years since the article appeared, has **been won to this** very day: that I was the first today to put the naive question to the author.

Of course, the time (or place) at which a saugrenuité appears, in this case at the precise moment of the one and only reference to my person, is by no means coincidental; nor is the form it takes, here by allusion to a type of notion, "weight", entirely foreign to the theme of the entire article, and by the improvisation of a composite notion "weight complex" that never existed! The association that immediately presented itself to me could well provide the key to the more precise meaning of the saugrenuité, beyond the bravado, the demonstration of power. It's the association with an allusion just as sibylline and just as purely formal (but without yet having the added dimension of saugrenuité!) in Deligne's article quoted at the beginning of note (49)[72] (***). It was an obscure allusion, in an article where the word "weight" was rigorously absent and where nobody but Serre or I would have been able to see them, to "weight considerations" which had led me to conjecture (in a less general form, it is clearly stated) the main result of the work. As I explain in the more detailed note "The eviction" (n° 63), behind this rhetorical allusion lies the intention to **conceal** both my role, and the ideas (concerning "weights" and "weights") that I'd been working on.
their relationship to cohomology in general, and Hodge's in particular) which he intended to reserve for
p. 334 himself alone. This intention must have been all the more clearly perceived by Verdier as he himself me "operates" on the
same diapason (in his relationship with me, at least, which seems to me to be the main cement between the two inseparable friends). In either case, an honest presentation would have consisted in starting the article by clearly indicating the source(s) for the main ideas, or for the question(s) that motivated the article.

Having said this, here's the meaning I see behind the symbolic language of apparent nonsense: I can allow myself, without the slightest embarrassment, to display patent **nonsense in** front of everyone, and at the same time express through this nonsense my true intention, with this absurd allusion-reference to the "weight complex": that is, I have no more intention of revealing anything about the role of Gr. in this work, any more than Deligne had such an intention with his empty allusion to "**weight** considerations"-which allusion made no more sense to the reader then than it does now with the imaginary "**weight-complexes**" I've just mentioned.

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[70](*) See "Perversity", n° 76.

[71](**) And it would seem that this text is indeed a standard reference today - or at least it has been for years.
was one of Zoghman's bedside texts (he recently sent it to me). It was there that he learned about the notion of constructibility (which plays an essential role in his theorem), and for a long time he was convinced that Verdier was the brilliant inventor of this crucial notion for him.

[72](***) This is the note "Poids en conserve - et douze ans de secret". For a more detailed examination of this Deligne article from the point of view that interests us here, see "L'éviction", note no.° 63, quoted below.

to invent right now, for the sake of the cause and my own pleasure!

I've just posted this note, written yesterday - I was interrupted earlier by a phone call from Verdier, whom I'd tried to reach during the day, to ask him just that question. I explained to him that I was trying to learn a little about cohomology, something he knew I'd never understood, and that Mebkhout had passed on to me for my instruction an old article by him, Verdier, a work that had long served as his bedside text. I was now trying my best to read it, but there was this cryptic reference - it was nice of him to quote me, of course - but I had no idea what he was talking about.

He was quite happy, even a little flattered, but yes, with a broad smile that protruded behind an air of paternal joviality, that I'd end up like this in my old age, learning cohomology on this old paper of his. I didn't expect him to contradict me when I said that he knew I'd never understood anything about cohomology - obviously that had been agreed long ago. ... As for those famous "weight complexes", I could feel his broad smile again at the end of the line (I'm making it up!), delighted that someone (and the addressee himself, no less) had finally picked up on his point.
something that had gone by the wayside for so long. At the same time, there was also a hint of embarrassment - more the embarrassment (I think) of not having been able to hide from a pleasure (like the pleasure one would take in ap . 335
slightly salacious story...), than not knowing what to answer. Dumped as I was, he really didn't have to worry about that! Without a moment's hesitation, he turned to Deligne (whose name I hadn't mentioned), who had given a demonstration in one of his articles, in which he also quoted me (he couldn't quite remember where) - in any case, it was a question of weights, but yes, he'd forgotten a little, of course - but not arithmetical weights, because I was quite right, it wasn't the same....

His tone was jovial and unapologetic, and he made it clear that he'd already given me quite a bit of his time - with a slightly hurried air, but without losing his debonair, slightly protective tone. I apologized for bothering him like that, with a rather stupid question, and thanked him for his explanation. My apologies were sincere and so were my thanks - he had indeed taught me everything I wanted to know.
[73](*).

11.3. IX My students

11.3.1. The silence

Note 84 (May 9) I was perhaps a little brisk yesterday, writing that in "the correct reference" (see note (82)) p. 337
what the author and ex-student shamelessly recopied as "part of the realm of the 'well known' for those in the know". I tried to explain who these "people in the know" were - with the conclusion that **they were no more, no less, than the dear listeners of this** SGA 5 **seminar** in 1965/66 - listeners, as I've had occasion to say, and judging by the vicissitudes of the writing of this seminar in the hands of volunteers whose lack of conviction I hadn't wanted to sense, it was often more "more" than "less" (always with the exception of the same Deligne ; of course). Indeed, there was no risk of other people "getting involved" as long as SGA 5 had not been written and published, precisely to enable people to "get involved" by reading it! This seminar was in fact published (as fate would have it) after the two "memorable

[73](*) Even with my droopy airs, I didn't really feel like I was putting on an act (I don't have the gifts for it), it was perfectly natural - in truth, I'm a bit droopy in all this stuff I haven't handled for nearly fifteen years! But I think that even when I'm old and ripe for the hearse, I'll still be able to feel the difference between an empty walnut and a full one. .

publications" by two of my dearest students and comrades-in-arms, namely the article in question by Verdier in 1976 (in which he says nothing about the origin of the ideas he develops, published there under his pen and for the first time), and Deligne with SGA $4\frac{1}{2}$, which has already been discussed at length[74] (*). After that, we cordially invite Illusie to take care of publishing the rest!

I can't remember in detail who took part in this seminar - whether Artin was there or not, for example. I think that more or less all my students from the first period must have been there in any case.
- with the exception of Mrs. Sinh and Saavedra (whom I hadn't met at the time) and perhaps Mrs. Hakim. There was also Bucur (since deceased). Houzel, Ferrand - I'm not counting Serre - who never had a taste for big cohomological fuss, and who came to put his feet up cautiously from time to time. While no one except Deligne perhaps had a good sense of where all this was leading,
it seems to me that there must have been ten or twelve listeners (not very involved) who were at least
p. 338 following enough to be considered "in the loop"' the thought that ran through my head
since yesterday, it's that among all these people "in the know", thus representing cohomological expertise (if not all "luminaries" like Illusie and Berthelot, with their "cohomological" theses that were decidedly weighty), and even apart from Verdier and Deligne - there must be quite a few who've had Verdier's article in their hands! A certain air in Verdier gives me the conviction that nobody ever suggested to him that something might be wrong. And I also know that nobody ever drew my attention to it - I learned of the existence of this article on May 2. exactly a week ago today, thanks to Mebkhout,. who of course had known about the scam for years.

This gives concrete meaning to the euphoric observation of the "Unanimous Agreement" (to bury my modest self) made ten days ago (note (74))!This agreement encompasses many (if not all) of my "pre-1970" students, i.e. many of those who today set the tone in the mathematical world; and it includes (or has included) my friend Zoghman himself, treated as a Cinderella by the beau monde and clinging against all odds to a kind of "fidelity to my work" (to use his own expression[75] (*)), which he has had the temerity and obstinacy to claim for himself at times, with the consequences that we know. Go figure!

In short, I was wrong to suggest that such and such a standing journal published a sort of "boilerplate" article that merely copied what was "well known". What the author was copying in full view (if not of everyone, but) of many witnesses was neither published nor "well known" (except for the cohomology class of a cycle in the coherent framework, where I had published it ages ago); and these were additional ideas that I would be remiss to play down, given that I didn't consider it a waste of my time to spend a year developing these and other ideas in a seminar, in front of a large audience. Verdier's article is probably a useful and well-done "digest" of a small part of the ideas and techniques I had developed: precisely so that they can be passed on to a wider audience.
the realm of the "well-known", the daily bread of those who use cohomology (or homology) for objects that more
p. 339 or less deserve the name "varieties"' From this point of view, then, Verdier has done what was useful
to do[76] (*), and in the end I have no reason to be unhappy. However, from what I sensed from my ex-student

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[74](*) See notes n◦ s 67, 67', 58, 68'.

[75](*) (June 7) Reading all the notes on L'Enterrement during a recent visit. Zoghman points out that this expression that he had used of "fi délité à mon oeuvre" didn't really capture his thoughts. Rather, he had in mind a confidence in his own judgment and mathematical instincts, which told him that my work provided him with some of the ideas he needed. It's all about **self-confidence, which is** essential if you want to do something truly innovative.

[76](*) He did so, it's true, at the expense of the "dismantling" of the original SGA 5 seminar, of which he and Deligne were the main players and "beneficiaries".

(June 7) The reflection of May 12, three days later (see note "The massacre" n◦ 87) made it clear that Illusie was even more directly associated than Verdier with what appears to be more a "massacre" indeed than a dismantling - even if he and friend even today, on the phone, and through many other things I've sensed from him (the "biggest" of which, or at least the most "spectacular", is the mystification of the Pervers Colloquium) - I can feel that **something is amiss**. That memorable Colloquium was certainly brilliant, mathematically speaking, in many respects. What's "wrong" is at a completely different level. I could try to define it in words, but I'm afraid that wouldn't make much sense. Anyone who doesn't feel what's wrong with this Colloquium - and I'm sure with many other Colloquia too, without mystification or anything - won't feel it a hair more, once I've made this attempt to "pin it down" and even succeeded in doing so to my complete satisfaction... ...

The question that remains open for me is whether this "sign" represented by what is undoubtedly a relatively common occurrence today (of an author presenting as his own the unpublished ideas of others) - whether this sign is that of a general degradation of morals, So, is it just a typical sign of a "spirit of the times" in today's mathematical world, or does it have more to teach me about myself - about the person I once was, and who is now coming back to me, through the attitudes towards me of those who were my students?

The two possible meanings are by no means mutually exclusive. My ex-students' relationship with me could not have found this way of expressing itself, if a certain state of morality didn't encourage them to do so. In fact, even before this "sign", I saw many others that seem to me to be even more eloquent in terms of a "picture of morals". What struck me about this sign is the particularity that sets it apart from all the others: it seems to **involve most of my former pupils at the same time**.

Such a circumstance cannot be fortuitous. To simply put it down to a "deterioration of the mores" (all that's real) would be a way of evading its more personal meaning, which implicates me as a "person".

it involves each and every one of my ex-students. If I say "each", which seems to go beyond the actual am□plitude of this p. 340

sign, I'm weighing my words carefully. For this sign is a timely reminder that it is scarcely conceivable that one o f my former students has not at least been confronted with situations of this kind. For years now, I've felt a certain "wind" about me, blowing through the world of mathematicians I've left (a wind whose origin and reasons I now clearly see, it seems). There's no way that any of them could have failed to feel the breath of this wind, be it on the occasion of an "incident" such as the publication of this gravedigger-article, or on any other occasion. Whether he wanted to or not, such an encounter inevitably raised (or raised again) the question of his relationship with me, who had taught him his trade. And the sign I've noticed, beyond the one that just brought me to this point, is that **I haven't heard a single echo on this subject from any of my students**[77] (*). This is a "coincidence" the meaning of which still escapes me - but which cannot fail to make sense (84).$_1$ p. 340

The day is dawning - I feel it's time to stop. I'm not sure that this is the time and place, in Harvest and Sowing, to pursue further the meaning of this striking coincidence. It's a harvest perhaps reserved for other tomorrows, if my reflections of this night meet with an echo in one or other of those who were my students. ⇒ 85

Note 84_1 (May 16) This perfect agreement between my former students, in this complete silence towards me, goes in the same direction as other signs. One is the complete silence that also greeted the episode "Les étrangers" (see section 24) - a silence I have already pondered somewhat in note n° 23v.

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was not the "beneficiary" and that he acted on behalf of others.

[77](*) (May 31) Interestingly, the one and only person who ever hinted at the existence of a funeral was an African friend of mine who had done a 3° cycle thesis with me about ten years ago (so "post-1970 student", and of modest status), with whom I have remained on friendly terms. The letter in which he implied this must be from two or three years ago, at a time when I wasn't at all surprised. I didn't then ask for details about his impressions, which he has only recently returned to.

On the other hand, with the exception of Berthelot, who sent me numerous separate prints, and Deligne, who sent me four (out of some fifty publications) and one from Illusie, I haven't received separate prints from any of my former students. That says a lot about the ambivalence in their relationship with me. Send prints to

p. 341 part, even though it was doubtful whether I would ever make use of it in my work[78] (*), would have been the way to

p. 341 to let the person who had taught them their trade know that this trade in their hands did not remain inert, that it was alive and active. But it is also true that for at least some of them, their publications also testify to their participation in a tacit burial of which it was better not to inform the anticipated deceased, trade or no trade... . On the other hand, I have received numerous offprints from several authors working in crystalline cohomology[79] (**), and even a good number of offprints from fellow analysts whom I hardly know by name, when their work takes up (and sometimes solves) questions I had asked thirty years ago or more, when it was obvious that I would not return to the subject I had left and that, from a "utilitarian" point of view, they were wasted offprints. But these colleagues must have sensed something that my students didn't want to sense. - Of course, in the sixties, my students were the first to be served for all my publications, both my articles and the great EGA and SGA series, and every one of them (except Mrs. Sinh and maybe Saavedra) must be in possession of my complete work published between 1955 and 1970 (in the ten thousand pages I presume).

It's true that my ex-students are in good company: none of my former close friends in the mathematical "big world", including those whose work is closely related to mine or who played a role in the development of my program of work in the sixties, have seen fit to conti-...
continue to send me separate prints after my departure from[80] (***). Only recently, among

p. 342 the fifteen or twenty friends of yesteryear (including some students) to whom I sent the Esquisse d'un Programme

p. 342 (which, among other things, announced the resumption of intense research activity, after a hiatus of fourteen years and on research themes closely related to those we used to pursue together), only two (Malgrange and Demazure) took the trouble to send me a few lines of thanks. The few more detailed (and, what's more, warm) feedbacks I've received have come from young mathematicians I've only recently met, and from my old friend Nico Kuiper, who is in no way connected to the kind of things I do. He found out about the text via an intermediary, and was delighted with my unexpected "homecoming"[81] (*).

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[78](*) (May 31) This may even have seemed out of the question until 1976, when I made it quite clear in the early '70s that I had no intention of ever returning to mathematics. The lecture I gave at the IHES in 1976, on De Rham complexes with divided powers, showed quite clearly that I was still interested in mathematics.

[79](**) (May 31) These are young authors whom I don't know personally, and I presume they've followed the example of Berthelot, who for them must be a figurehead. The strange thing here is that for at least the last two years (since the Colloque de Luminy, September 6-10, 1982), Berthelot is actively trying to bury me (see on this subject the b. de p. note of May 22 to the note "les cohéritiers. .. . ", n° 91) - could this be a recent turning point in his relationship with me? I don't recall receiving the offprint of the article-survey on crystalline cohomology et al. in which he passes my name under silence - he had to be careful not to send it to me!

[80](***) (May 31) Of course, the psychological reasons that might have prompted them to send me some were far less strong than in the case of my students - but, one might naively think, far stronger than among my fellow analysts, or even among the many algebraic geometers whose prints I received separately, and whom I know little or nothing about personally. Clearly, after my departure from the common milieu, the fact of having been friends has created or reinforced, in my former friends in the mathematical world, the automatisms of rejection that I have had occasion to observe. (On the subject of these attitudes, alluded to in passing here and there in Récoltes et Semailles, see the note "Le Fossoyeur ou la Congrégation toute entière" of the 24th.
May, n° 97.)

[81](*) (May 31) This is almost the only echo from an old friend of mine (or one of my former students) in the of acquiescence to my "homecoming". This is hardly surprising, given that the appearance of the deceased unseemly interrupts the normal course of a funeral ceremony... ...

(June 17) However, I recently had the pleasure of receiving a warm letter from Mumford, who says he is "thrilled" and "very excited" by the ideas sketched out in the Esquisse, and who confirms that the key technical result I needed was

11.3.2. solidarity

Note 85 (May 11) This story of the ill-fated SGA 5 seminar keeps running through my head. The "good reference"[82] (**) definitely sheds new light on this story, and at the same time gives new meaning to the brilliant "SGA 4 operation[1] ". [2]

The more I think about it, the **bigger** the SGA 5 story seems**.** My first impression, when I "disembarked" just a few weeks ago (see notes n° s 68, 68'), was that a situation of debandade among the poor ex-auditors of this seminar in 65/66 had been put to good use in his own way by my friend Pierre, for his famous operation, and that no one else had anything to do with it. And as for the misfortunes of SGA 5, this was neither he nor anyone else, but rather "ut other than myself, who had not, alas, known how to enthuse my volunteer-editor listeners, nor do for them the work they stubbornly refused to do while saying they p. 343 were about to get down to business. Then it turned out over the last few days that there was one, after all, whose enthusiasm was reawakened ten years later, to publish (without reference to the seminar) what he liked to take from it, thus creating a good reference for his own account, at a time when the other "volunteers" still hadn't decided to get going.

What's become increasingly clear to me since yesterday is that it's not just two "villains", but **every single one of my "cohomologist" students** who are directly involved in the cover-up that took place at this seminar. Unless I'm mistaken, every single one of them attended this seminar - namely (in chronological order of appearance of my "cohomologist" students): Verdier, Berthelot, Illusie, Deligne, Jouanolou. (I'm not counting Jean Giraud, who operated on quite different registers from those mostly discussed in SGA 5 or its predecessor SGA 4).

This seminar, which I did **for the benefit of my students** in the first place, and even though they sometimes asked, grace - **I consider that it wasn't crap**. Every one of them, during that year, learned a good deal about his job as a "cohomology user mathematician"! The things I was doing to them, taking up in the spread-out framework and in a much more circumstantial way ideas I'd first developed in the coherent framework - these things they couldn't find anywhere else but in that one seminar made for their benefit, given that nobody before me had ever bothered to do them - and that nobody but me even felt what there was to do, and why. (Except always Deligne, who learned it over the months in this very seminar, being quicker on the uptake than the others). It was having taken this seminar (and the previous one) and having worked on it at home as best they could, and nothing else, that meant they were now "in the know" about duality formalism, and they were **the only ones** to be. This **privilege**, it seems to me, created an **obligation** for them: to ensure that this privilege did not remain in their hands alone, and that what they had learned from me, and which has been indispensable baggage in all their subsequent work up to the present day, was made available to all, and this within a reasonable and customary timeframe - of the order of a year at most, or even two at a pinch.

We'll say, not without some reason, that it was up to me above all others to see to that. But if I accepted p. 344 in good faith when students and other listeners offered to help with the writing (which, for those who took it seriously, could only be good for them) - not for the benefit of being able to twiddle my thumbs while they did a job that fell to me. I've

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for my combinatorial description of Teichmüller's tower is well and truly proven. This is the first time since 1978 that an old friend of mine has latched on to my "Anabelian" ideas, whose exceptional scope (comparable to that of pattern yoga) has been obvious to me since the very beginning. ...

(March 28, 1985) Since these lines were written, I have also received a very warm letter from I.M. Gelfand (dated Sept. 3, 1984), in response to the Esquisse.

[82](**) See note n° 82.

continued, with the help of Dieudonné and others (including, incidentally, Berthelot and Illusie in 1966/67) to develop' basic texts that seemed equally urgent to me, and that no one else would have done in my place or without my assistance[83] (*). These texts have themselves become indispensable references, including for my "cohomology students", who are just as happy as everyone else to find them ready when they need them.

With the mastery of cohomological ideas and techniques they had acquired through their work with me and the seminars they had attended or participated in, writing this seminar through their joint efforts represented a task of derisory dimensions, if we compare it with the service that was being rendered to the famous "mathematical community", or perhaps also, later on, with an obligation of loyalty they might feel towards me. I've already said that for me (the one with the helping hand), it must have been a job of the order of a few months to write the entire seminar. By dividing the work between the five of them, with the writing experience they had each acquired in those years, and with my detailed manuscript notes at their disposal, the investment for each of them was of the order of a month or two at the very least. They were much better equipped to do this than other editors, such as Bucur, who would have liked nothing better than to entrust a task, which was clearly beyond him, to younger, more directly motivated hands.

As long as I was around (so in the three years that followed), I can see how a "leave it to me" reflex might have come into play - I was supposed to coordinate everything and deal with the "volunteers". It's likely that if I'd asked each of them to give two or three presentations
within a short space of time, it was up to me to do the same, and finally get it over with, they wouldn't have recused themselves. It was from the moment I withdrew from the mathematical world that the situation changed completely. They
p. 345 found themselves **the sole trustees of a certain inheritance**, both implicit (in the absence of a will) and very concrete. It's true that, from a practical point of view, my departure was tantamount to a **disappearance** - I was indeed "deceased", in the sense that there was no one outside them to know about the inheritance, to be able to use it and to be concerned (for better or for worse. . .) about its fate.

If, for the seven years following my departure, this heritage remained hidden (apart from "the good reference" in 1976!), it's because **my students didn't want it to become public during that time**. All things considered, the situation doesn't seem very different to me from that of the "yoga of motives", which was thoroughly known only by Deligne (apart from myself), and which he saw fit to keep to himself for his own benefit. If there is a difference at first sight, it is that in this case there is only one "beneficiary" instead of five, and that there is no common measure between the depth of what was, concealed by one, and what was jointly concealed by the five.

I certainly don't know everyone's deeper motivations - even in Deligne's case I have an apprehension that remains hazy and no doubt will remain so. But on a "practical" level, Deligne's game (with the SGA 4 "s operation - and all the rest) is quite clear. And what's also clear is that these operations couldn't have been carried out **without everyone's solidarity**. It seems to me that Jouanolou isn't too much in the picture - he doesn't strike me as a "luminary", I have the impression that he has long since left the cohomological quagmires (85_1). But I can't imagine Illusie and Berthelot not having had their hands on both SGA $4\frac{1}{2}$ and "la
good reference", and they can read as well as me and are no stupider than I am.

If Illusie suddenly became involved in the publication of SGA 5, at the precise moment when Verdier used, where
where Deligne needs a logistical base for his famous SGA $4\frac{1}{2}$ (by unpacking it)
the two seminars from which this text and all his work derive), whereas Illusie had

[83](*) Between the 1960s and 1970s, I had to work at an average rate of a thousand pages a year of texts (EGA, SGA, articles), almost all of which were to become standard references (something that was quite clear to me when I wrote them, or when I encouraged a collaborator to do so with my assistance).

had ten years to do it, it's surely no coincidence. If the closing presentation on open problems
and conjectures that I a□vais made in 1966 "has unfortunately not been written, any moreover [sic] than p. 346
his very fine introductory talk, which reviewed the formulas of Euler-Poincaré and Lefschetz in various contexts (topological, complex analytic, algebraic)", it's surely no coincidence either - but that's a funeral I don't know anything about. And it's no coincidence either that it seemed as natural to Illusie as it did to Deligne (and just worthy of a mention in passing among the "changes of detail") to amputate the seminar from one of its key presentations, which passes into SGA 4$\frac{1}{2}$ without further ado[84] (*).

I don't know what were the intentions (conscious or unconscious) of Luc Illusie, whom I like like Pierre Deligne, and who (like him) has always shown me great kindness[85] (**). But I've noticed that, alongside Deligne, he's become the co-actor of a **shameless mystification**: that which passes off the SGA 5 mother seminar of 1965/66 (the very one in which Deligne first heard of schemas, stale cohomology, duality and other "digressions") as a kind of shapeless, vaguely ridiculous appendix to a collection of texts with the misleading name SGA 4$\frac{1}{2}$ written eight years later, which pretends to present itself as anterior (both by the number in its title, and by the number of publication in the Notes readings, and finally by the author's unusual comment "Its existence (of SGA 4$\frac{1}{2}$) will soon allow us to publish SGA 5 **as it stands**" - emphasis added) - and which, moreover, affects to treat with undisguised disdain the works from which this meagre collection is entirely derived.

Without these works, treated with this beautiful casualness, **none** of Deligne's great works, which are the foundation of his career, would have been possible.

its well-deserved prestige, would not be written now, nor in a hundred years (and the same without
doubt for Illusie and my other cohomology students). There is in the es□prit of this "SGA 4 V operation a p. 347
impudence, of which Illusie is the guarantor (without even realizing it, no doubt), and which could only have been displayed with the tacit approval of a **consensus**. The first people involved in this consensus, apart from Deligne himself, are the very people who were my students and the main beneficiaries of a certain heritage, handed over before their very eyes to the vagaries of the jockeying for position and disdain.

And those airs of peremptory smugness, those paternal, protective airs that I was able to appreciate in my ex-student as recently as the day before yesterday in our telephone conversation[86] (*), and also those more discreet airs of condescension that I was able to appreciate in my friend Pierre in the aftermath of the brilliant double operation "SGA 4$\frac{1}{2}$ - SGA 5" (of which I was far from having the slightest suspicion at the time and for another seven years) - these airs are **not the** products of solitude, but the signs of a consensus that has **never been called into question**. These tunes tell me something not only about Verdier and Deligne, but also about all those who were my students, and before all others, about those who were (by virtue of their work themes and the tools they wielded every day) the first to be concerned.

The term "mystification", which came to me without having sought it out, opportunely reminds me of that other mystification, in which the same cynicism is on display - that of the so-called "Pervers" Colloquium. The two now seem **intimately, indissolubly linked - it's the same spirit that made both possible**. With the possible exception of Jouanolou, who is no longer so much a part of the "big world", I consider these same ex-students to be the same.

[84](*) (May 16) In fact, as I discovered the very next day (see note n° 87), there had been a veritable "massacre" of the mother seminary (or father!) SGA 5, at the hands of Verdier, Deligne and Illusie.

[85](**) Even after I left in 1970, Illusie showed me a lot of kindness - for a long time he sent me beautiful Christmas cards at the end of the year. I'm afraid I didn't have to write back very often to thank him and give him some sign of life - these signs of a faithful friendship came to me like messengers from a past that seemed so infi nitely distant, and with which I'd lost touch.

(May 16) On the other hand, there has been no inclination on Illusie's part to continue or resume contact on a mathematical level, and even last year (when I contacted him about mathematical questions) I sensed his reluctance. In the fourteen years since I left, I have received one and only one offprint from him, dated 1979.

[86](*) For this conversation, see the note "Jokes - or 'weight complexes'" (n° 83).

cohomologists jointly and severally responsible for this disgrace. As far as Berthelot and Illusie are concerned, nothing allows me to prejudge malice or bad faith (which cannot be doubted in the case of Verdier as in that of Deligne). But at the very least, I note a blindness, a blockage in the use of healthy faculties, the underlying reason for which, of course, escapes me. If it weren't for a deliberate intention of indifference and disdain, surely Zoghman Mebkhout, as the only person in the '70s to openly claim to be an admirer of my work, and on subjects that were close to both of them (without that they deign to notice it), would have had the benefit of the minimum "favorable prejudice" so that they would at least be aware so soit little of what he does, and hence realize the interest of the direction
p. 348 in which he had been engaged since 1974, an interest that was **obvious**! Neither of them deigned to notice anything, coming from a vague stranger who still pretends to be a Grothendieck. I don't know if they've opened it, or if they've gone through the shorter, more digestible texts that explain what it's all about - in any case, they haven't deigned to acknowledge receipt of it (nor has Deligne, who obviously sets the tone).

That didn't stop them and the other participants in the memorable Colloque[87] (*) from learning about the remarkable "Riemann-Hilbert correspondence", without the slightest thought of questioning its origin or authorship, or at least (as solid mathematicians) where it was demonstrated (85). But I trust that Deligne was happy to explain this demonstration, which is surely quite obvious to people like them - precisely the kind of demonstration, using Hironaka-style resolution of singularities, that they learned a long time ago from none other than me (85_2). Riemann-Hilbert, Hironaka abracadabra - that was it!

Clearly, like Verdier and Deligne, they've completely forgotten what **mathematical creation** is all about: a vision that gradually unravels over months and years, bringing to light the "obvious" thing that no-one had seen, taking shape in an "obvious" statement that no-one had thought of (even though, in this case, Deligne had been trying in vain for a whole year. . .).) - and which anyone can then demonstrate in five minutes, using the ready-made techniques he had the advantage of learning sitting on the benches of a distant seminar he doesn't deign (or hasn't kept) to remember. ...

If I've spoken bluntly of Berthelot and Illusie, it's not because I particularly want to smear them (after an initial settling of scores with their two friends). I know that they're no "worse" or dumber than most of their dear colleagues or me, and that the lack of flair and sound judgment that I see in them in this instance (and sometimes, too, that of the necessary respect for others. . .) is by no means inveterate, but the effect of a **choice**. This choice undoubtedly offered **them returns** that pleased them - and may have done so.

Perhaps this other "return" that comes with my reflection will be unwelcome to one or the other. If it were, it would simply be that he's still reproducing the same choice, which is also that of operating on a tiny part
p. 349 of his faculties, even if it means mistaking bladders for lanterns and vice versa, and hopelessly confusing empty nuts (from the boyfriend) and full nuts (from a vague stranger). To each his own! (⇒ 86, 87)

Note 85_1 Jouanolou is the only one of my students, along with Verdier, who did not publish his thesis. This seems to me to be a sign of disaffection with the foundational work he had developed, namely that of χ -*adique* cohomology from the point of view of derived categories. Since most of his work on this theme took place **after** my departure, i.e. at a time when my students, Deligne and Verdier in

[87](*) (June 12) I have since learned that neither of them took part in this Colloque (Luminy, June 1981). See, however, the note "La mystifi cation", n◦ 85'.

head, had given the signal for a general disaffection with the ideas I had introduced into homolo- gical algebra, and in particular that of the derived category, the context hardly encouraged Jouanolou to identify with his work and to do him the (well-deserved) honor of publishing it. As these same Deligne and Verdier, in the wake of the work of Zoghman Mebkhout (aka Elève Inconnu (de Verdier) aka élève posthume (de Grothendieck)), have come to discover (with great fanfare and mutual publicity) the importance of derived categories (see notes n° s 75,77,81), Jouanolou's scorned thesis has, since the Colloque Pervers, regained all its topicality ; a relevance it would never have ceased to have, had the development of the cohomo- logical theory of schemas continued normally after my departure in 1970. A striking detail that illustrates a certain drastic "turn" in Deligne's options after my departure: it was Deligne himself (who had clearly understood the importance of developing the formalism of χ*-adic* cohomology in the context of triangulated categories) who provided Jouanolou with a key technical idea for a formal definition of the triangulated χ*-adic* categories he was studying, an idea that is developed in the thesis. (See my 1969 "Report" on Deligne's work, par.8.)

(May 30) See also, on the subject of Jouanolou's work, the note "Les cohéritiers. . . ", n° 91.

Note 85_2 Significantly, it was in this same SGA 5 seminar that everyone learned this demonstration principle, which is used to prove the biduality theorem in cohomology as well.
(in cases where singularity resolution is available), that □finitude theorems for p . 350
$R f^i_*$ without any cleanliness assumption on f , and similarly for <u>*RHom*</u>, $Lf^!$. (These finiteness theorems were also omitted from the published version of SGA 5, to be appended to SGA $4\frac{1}{2}$, without Illusie even seeing fit to point this out in his introduction - I only realize this as I write these lines!) Zoghman, who didn't have the advantage of attending the seminar (he got "the right reference" instead), learned the procedure in another place where I had used it (for De Rham's theorem for smooth schemes on <u>*C*</u>).

He could also learn it from "the good reference", where my demonstrations are copied into the analytical framework, to establish what my students and listeners at SGA 5 have since liked to call the "Verdier duality" (which was known to me before I had the pleasure of making his acquaintance). It all adds up! **The same demonstration** (copied from me at the same time as the statement) is used by Verdier as a title of authorship for a duality he learned nowhere else than in that dislocated and scorned SGA 5 seminar - and it is used **against** Mebkhout, becoming (by its very "obviousness") a (tacit) pretext and means for shamelessly robbing him of the credit for an important discovery.

(May 30) It seems to me that the first time I used singularity resolution à la Hironaka, and understood the extraordinary power of resolution as a demonstration tool, was for a "three spoonfuls" demonstration of the Grauert-Remmert theorem, describing a complex analytic structure on certain finite coverings of a complex analytic space, and the analogous statement

in the case of schemes of finite type on C. (It's not impossible that the principle has been blown to me, in this on the same occasion, by Serre). This last result is the main ingredient in the proof of the comparison theorem between stale cohomology and ordinary cohomology (the rest being reduced to unscrewing, thanks to the formalism of $Rf_!$, plus a bit of solving to go from $Rf_!$ to Rf_* . . .)

11.3.3. The mystifi cation

Note! 85' (June 3) In fact, I'm learning that they didn't have to wonder about paternity, since both Berthelot and Illusie learned the theorem from the mouth of Mebkhout, the first...

in February 1982, the second as early as 1979 (the year Mebkhout defended his thesis). Although neither of them took part in the Colloquium in question, they are nonetheless in solidarity with the mystification that
p. 351 took place at the Colloquium.
place at this Colloquium, because it is impossible that they were unaware of the cover-up that took place of Mebkhout's authorship of the theoreme du bon Dieu. I can imagine that, along with all the other Colloque participants, they were the first to be fooled by the collective mystification organized by their friends Verdier and Deligne (a mystification in which four of my five cohomology students appear to be involved). As far as Illusie is concerned, at least, I was struck, during a telephone conversation with him after Mebkhout's visit to my home last summer, by how little he obviously thought of him - he was quite astonished (almost pained on the part of his old master, in whom he would surely have expected better judgment... .) to see me give Mebkhout a leading role in restarting the cohomological theory of algebraic varieties. Consensus of considerable force had decided to rank Mebkhout among the vague unknowns, and my friend Illusie blithely lived with this triple contradiction, without asking himself any questions: the leading role played by the theorem of the good God and the philosophy that goes with it; the evasion surrounding the authorship of these things (an evasion in which he himself participates in the company of many); and the low esteem he has for the format and role of Mebkhout (who he knows perfectly well is the unnamed author of these things, which have renewed a field of mathematics in which he, Illusie, is a figure of eminence).

I find here again the complete blockage of common sense and sound judgment, even in something as seemingly impersonal as judgment on scientific matters, a blockage to which I have had occasion to allude more than once already, and which each time again baffles me. And this contradiction I see here in Illusie's (and surely many others') relationship with Mebkhout, my "posthumous pupil", is surely no more than one of the many effects of a more crucial contradiction to be found in his relationship with me. It is this contradiction, in him more particularly and in my other pupils too, that becomes increasingly clear in the reflection pursued in the notes of the present procession to the Funeral, formed by my pupils of yesteryear... .

11.3.4. The deceased

Note 86 (May 11) As is so often the case, it was with some reluctance that I set about this task.
new reflection, on the theme "SGA 5 - SGA $4\frac{1}{2}$ - Perversity", which might have seemed to have been examined and
re-examined over and over again: "It's going to make a deplorable impression on a reader who's probably had enough of it ever since he heard about it; it doesn't look elegant at all to go into details again, SGA 5 çi SGA $4\frac{1}{2}$ that, it's
and doesn't deserve any more toast... . ".

p. 352 I'm glad I didn't let myself be intimidated by that well-known refrain, which would like to prevent myself from getting to the bottom of something (at least as far as I'm able to go at the time), on the pretext that it's decidedly "not worth it", that there's nothing to do but let it run its course... . If there have been times when I've discovered things that I consider useful and important, it's always been in moments when I've known not to listen to what presents itself as the voice of "reason", or even "decency", and follow this indecent urge inside me to go and see even what is supposed to be "uninteresting" or of poor appearance, or even messed up or indecent. I can't remember a single time in my life when I've regretted having looked at something a little more closely, against inveterate reflexes that would have prevented me from doing so. These inhibition reflexes were even stronger in Récoltes et Semailles than on other occasions, because this reflection is destined to be made public, which immediately imposes certain constraints of discretion (when I involve third parties), and conciseness (for the sake of the reader). But I don't think so, finally, that these constraints never prevented me from either tackling something I wanted to tackle, or going as far into it as I felt I wanted to. In the cases that may at one time have seemed like borderline cases, I forged ahead with the assurance that, should the need arise, I would always have the resource of not including in Harvest and Sowing anything that would "escape" my indiscreet reflection. These "borderline cases" arose exclusively when I hesitated to involve others, and never when it came to involving myself. But even in the first case, it turns out (and this came as a surprise) that I never had to make use of this "resource": the text of Récoltes et Semailles represents the complete version of my reflection - at least of the part of this reflection that found its way into writing to express itself.

I feel that with the brief reflection of the previous note[88] (*), the situation has become considerably clearer. I mean that a certain essential aspect of a situation which had been confusing to pleasure, and which I have just evoked by the triple name of a "theme" (SGA 5 - SGA $4\frac{1}{2}$ - Perversity), has appeared to me in full light: that of a "solidarity", a "connivance" which had only been confusedly perceived until then. This doesn't mean that I believe I've fathomed and understood all the ins and outs of a "solidarity", a "connivance" that had only been dimly perceived until then.
complex situation, directly and particularly obviously involving at least seven people: Zogh-
man Mebkhout (acting in a sense as a "revealer" of a certain situation), my five ex-students p. 353
cohomologists, and myself. I don't even flatter myself to have perceived all the springs and motivations that have been at play in my own person, in relation to the "SGA 5 etc. ... . .", in the nearly twenty years since that "unfortunate seminar" took place! But I feel in a much better position than I was yesterday (or even just this morning), to understand and situate the echoes that I hope will reach me on this subject from at least one or other of the main parties involved.

The main question that arises for me (it seems to me that it was already present at another stage of reflection, and now reappears with new vigour) is (it seems to me) this: is what happened with this Burial by my students, (more or less) in their entirety, something completely **atypical**, linked to certain particularities of my person and my singular destiny (such as my departure from the mathematical scene nearly fifteen years ago, the circumstances surrounding it, etc. .) ? Or is it, on the contrary, something "quite natural", due to a simple combination of circumstances - following the principle that "opportunity makes the thief"? I hesitate to think so, though I can't at the moment discern, or even glimpse, what particular aspect of myself has had the virtue of creating such perfect and unanimous **agreement** among my former students, to bury both the "master", and those who claim to be his followers or whose work clearly bears his mark (without, however, being "theirs"). Is it this sort of "aura" of Father that surrounds me, and which I've had occasion to mention? Or is it the challenge posed to each of them by the mere fact of my departure? At the moment, I wouldn't be able to say, for lack of eyes that can see... . Perhaps the coming months will teach me something on this subject[89] (*).

More than once in the last three weeks, I've thought of this other strange "coincidence": it's
that the discovery of the Burial "in all its splendour" (with the four-stroke LN 900 - SGA $4\frac{1}{2}$ -
SGA 5 - Colloque Pervers, then back to SGA 5 and SGA $4\frac{1}{2}$)- that this discovery came at a time when I had just completed an in-depth reflection on my past as a mathematician and my relationship with my students. It was a time, therefore, when I had just come "to terms with myself" on the subject of my students.

of this past, to the best of my ability, and as far as the facts then known to me allowed, as they were rendered by
often hazy memories. Or to put it another way: it was lep . 354

[88](*) This refers to the note "La solidarité" n◦ 85, dated the same day.

[89](*) (May 30) For a reflection along these lines, see the note "Le Fossoyeur - ou la Congrégation toute entière", n◦ 97.

the exact moment when I was finally **ready** to learn and profit from it.

Chance" did things so well that there wasn't even a break in the meditation. The reflection that had begun with this short retrospective on the fate of the most important notions (in my opinion) that I had introduced[90] (*) (a reflection that remained in a certain vagueness, where only a certain basic tone emerged insistently... .) - this reflection continued quite naturally on Thursday April 19. It was, it's true, still in the throes of the emotion aroused by that impression of "impudence" (to use the term from earlier, which also aptly describes something I felt at the time), on reading the "memorable volume" LN 900.

In this new departure from "the same" thinking, the main driving force was "the boss" - my self-esteem and sense of decency were affected, and by writing down my emotions I freed myself from them to a certain extent. It was indeed "me", "the boss", who visibly led the dance in the ten days that followed - days marked by the absence of smiles and laughter, by unfailing seriousness. I had to go through this ten-day detour before the reflection returned to the center it had left - to myself. I still remember the relief of that return - like coming out of a tunnel when daylight appears again! It was then that I found myself laughing and smiling again, as if we'd never left each other. It was April 29th. The next day, the 30th, the last day of the month, I was happy to put the finishing touches to this ultimate stage of reflection.

It was also the moment, surely, when I was ready to receive the next "package", this time sent by my friend Zoghman - the "Colloque" package received the day after tomorrow. Today is the tenth day I've been working on assimilating the substance of this package. But at this stage, while I've been gnawing at the bit to get to the bottom of this rebound that just kept on rebounding, the smile hasn't fazed me for a single day. And today, I truly believe (for the thirtieth time, it's true!), is finally the day of closure.

Five days ago I'd already had that same feeling that I'd reached the end of my rope, that there was nothing left but work to be done.

of stewardship: adding a few footnotes here and there, retyping pages too overloaded with ra□tures (each time a sign of a thought that had remained somewhat confused, and which needs to be put into

This seemingly mechanical work, from which the text always emerges with a new face... .). . . This was when I had just written what is now the note "Mes amis" (n° 79), which spontaneously flowed into "final p. 355 chords". However, I ended up separating these chords from the beginning of the note. In fact, it turned out that this famous stewardship work had broken down: the "footnotes", typed without line spacing, became real notes (**not** footnotes) of nice dimensions, which had to be retyped with line spacing, and then tried as best I could to fit in here and there. It took days before I realized that another procession, after the one called "The Colloquium", was forming to join the procession - and that the last of the processions would not (as I had decided in my head) be the said Colloquium, but would be led by **the Student**. And just today, as the first procession, reduced to a single note, was enriched by a second ("A feeling of injustice and powerlessness"), I also knew who would lead it: it was "**L'élève posthume**". So the procession, opened by a Pupil (posthumous and lower-case, as befits his humble state) and closed by yet another Pupil (by no means humble this time), seems to me to be complete at last!

It's also time, it seems to me, after a first "false arrival", to return to the chords of a final De Profundis, which are more appropriate today than they were five days ago. Here they are, as I wrote them down then, and which also express my feelings at the moment.

[90](*) See notes "My orphans" and "Refusal of an inheritance - or the price of a contradiction" of March 31 (n° s 46,47).

(May 31) Finally, it was another "false arrival" - the "final agreements" were premature this time too! Twenty days went by, during which the "housekeeping work" continually broke down into rethinking this and that aspect that had been neglected. Six more notes joined the "L' Elève" procession, which was supposed to close the parade. The Fourgon Funèbre appeared in the wake of the Elève, carrying four coffins accompanied by the Fossoyeur. He was definitely lacking to give body and meaning to a funeral convoy that didn't seem to be conveying anyone.

Having become cautious through experience, I'm waiting for events to unfold and won't take any chances for the time being.

to predict whether the procession is finally complete, or whether a forgotten procession will sneak in again at the last minute, so as not to miss the ultimate Ceremony[91] (*). p. 356

11.3.5. The massacre

Note 87 (May 12)[92] (**) For the edification of the somewhat cohomologist reader, and above all for my own, I would like to review the details of this plundering of a splendid seminary, in the hands of two of my former cohomologist students and under the benevolent eye of the others[93] (***) - of the same seminary where they learned, twelve years before anyone else and from the hand of the workman himself, the basics and finesses of the trade that made their reputation.

Two of my oral presentations have never been made available to the public in any form. One is the closing lecture on open problems and conjectures, which "unfortunately was not written up", given how little - and indeed, the author of the introduction to the murder-edition deemed it unnecessary even to mention **what** open problems and conjectures it was. And why should he have taken the trouble, when these were merely problems (which everyone is free to pose as they please!) and conjectures (not even demonstrated!) (87_1). The other was the lecture that opened the seminar, placing it right from the start in a broader context (topological, complex analytic, algebraic) and reviewing formulas of the Euler-Poincaré, Lefschetz, Nielsen-Wecken type, some of which constituted one of the seminar's main applications. The ". . any more than. . . ." with which the author of the introduction goes on to point out, in the course of a sentence, the disappearance of this presentation, speaks volumes about a **casual attitude** which at the time was clearly self-evident, even though the author of the seminar had been out of circulation for seven years.

There's a whole series of talks I gave on the formalism of homology and cohomology classes.
logie associated with a cycle (regular ambient pattern in the cohomological case)[94] (****). They have been equally divided: cohomology for Deligne, homology for Verdier - who nevertheless overflows
a little on cohomology, even if it means making the small reference to Deligne with the famous "complexes". p. 357
weight"[95] (*). (Not to mention the finitude theorem for *RHom* and the biduality theorem, copied verbatim from the seminar - in any case, the lion's share will go to Deligne, which was to be expected. ...) The author of the introduction does not see fit to mention the homology lectures alone. Indeed, there was no need, since the previous year his friend Verdier had taken on the task of providing the missing "good reference" (without alluding to a seminar, or to me).

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[91](*) (June 12) Caution was the order of the day, as a new "Mes élèves" procession separated from the one originally called "L'Elève", which became "L'Elève - alias le Patron".

[92](**) This note follows on from the previous day's reflection on "Solidarity" (n◦ 85).

[93](***) Further reflection reveals that one of these "others" lent a hand effi ciently for this operation. on behalf of others.

[94](****) For details, see note no.◦ 82 "Good references".

[95](*) See note (83) "The joke - or weight-complexes".

There were oral presentations on finiteness theorems for the operations $R\,f^{!}_{\wedge}$ (f not proper), and as a corollary, for the operations $\underline{RHom}_{*}$; and $Lf^{!}$. The key theorem was proved using a Hironaka-style singularity resolution technique (valid only in cases where the resolution is available).
These arguments, which I used, have come into common use since the seminar (see note (85_2)). Deligne has managed to prove these finitude theorems, as well as the biduality theorem, under other, more helpful hypotheses, which have already been verified in most applications. It might have been expected that he would ask to include these refinements in the seminar where he had the privilege of learning étale cohomology, and the ideas and techniques at the basis of all his subsequent work. But this circumstance is used as a "reason" for amputating this part of the seminar. As for the biduality theorem, under Illusie's pen (and within the framework of the diagrams) it became "Deligne's biduality theorem" (introduction to Lecture I). This was only fair, since in the analytical case, Verdier had already claimed it as his own the previous year (without even having to go to the trouble of finding another demonstration).

Then there's Illusie's "generic Kûnneth formula". No one before had thought of developing this kind of statement, inspired by the intuition that "generically" i.e. in the vicinity of the generic point of the basis, a relative scheme behaves like a "locally trivial fiber" in the topological context. In an elegant demonstration similar to the one mentioned above, Deligne manages to eliminate the singularity-resolving hypothesis I had made. It's awarded - presentation deleted and "replaced" by a reference to a presentation by the same Illusie in the so-called "earlier" seminar.

SGA $4\frac{1}{2}$.

p. 358 There is a series of lectures on the formalism of non-commutative traces, developed as a means of explicating the local terms of the Lefschetz-Verdier formula in cases that had never been treated. These lectures were eventually written, it seems, by Bucur, whose manuscript "got lost in a providential move" - it's turning into a vaudeville![96] (*) In the introduction to SGA 5, written by Illusie, these lectures become "Grothendieck's theory of **commutative** traces, generalizing [brilliantly] Stallings'" (which were non-commutative!). The slip of the tongue[97] (**) can only be due to a badly (or too well. . .) inspired secretary, who must have been involved with my friend Ionel Bucur's movers. (The word "brilliantly" is an interpolation of my pen, to better render the thought infallibly suggested by this slip of the tongue, also providential).

I've got nothing to complain about, since Illusie has taken on the job of redoing the work (and even, he tells us, a "more sophisticated" version, since it's put in the Beamtique sauce - I seem to remember, though, Illusie, that you made more "sophisticated" innovations than this in my day... .). It must have taken a long time even, if I remember that I spent weeks putting the machine together; my manuscript also got lost in the same providential move, and God knows if one of the dear listeners, overwhelmed by my oral faconde, was at least able to take comprehensible notes... .

Remarkably, and this is something I hadn't noticed before, he doesn't insert this talk in the place in Lecture XI where it was intended (which no doubt also corresponds to the place it had in the oral seminar), preferring to leave a gaping hole there and make his talk an apocryphal one, called "Calculs de termes locaux" ("Calculations of local terms"). The title does, however, seem to correspond to what I seem to remember him doing in the oral seminar.

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[96](*) It was no doubt this circumstance that inspired Deligne's unexpectedly brilliant criticism of SGA 5, in which the local terms of Lefschetz Verdier's formula (which "remained conjectural", remember!!!) were not even calculated! (See note "la table rase", n◦ 67, about the absurdity of this criticism, which for an informed reader is similar to that of Verdier's famous "weight complex" the previous year (see note n◦ 83). So it was Verdier who became the schoolmaster!)

[97](**) This is the slip of the tongue attributing to me the authorship of a theory of "commutative" traces (for which I was not expected). instead of "non-commutative". That it has survived into the published edition is all the more remarkable given that Illusie was perhaps the most meticulous of my students, down to the last detail.

strange. But from line 1 of his introduction to this talk, the author is quick to detrompt us: "Cetp . 359
exposé, written in January 1977, **does not correspond to any oral presentation in the seminar**". And he continues with Lefschetz-Verdier formulas (that name rings a bell, though, and I thought I'd actually developed a theory of non-commutative traces, in order to calculate "local terms" in certain cases. .), then on a formula by Langlands and a demonstration by Artin-Verdier in 1967 (this was a year after the final agreements of the oral seminar, which must have influenced these authors, at least one if not both of whom followed him). Towards the end of the page, we learn as if in passing, contrary to what had been announced at the beginning, that there is also a "second part of this talk, of a much more technical nature" (I've read this language somewhere. . .) which is (admire the nuance) "**inspired by the method used by Grothendieck** to establish the Lefschetz formula for certain cohomological correspondences on curves", with a reference to Lecture XII of the same seminar and above all to the indispensable SGA $4\frac{1}{2}$; Obviously, there was no reason, for so little, to include this lecture in place of the gaping hole - the "more sophisticated version" of earlier will have done things right. It was even nice of Illusie and Deligne to cite me as a source of "inspiration", when the example of their friend Verdier the previous year had clearly shown that there was absolutely no need for such scruples.

I return to Illusie's introduction to the volume that goes by the name of SGA 5. In it, we learn once again - as Deligne had already announced in his introduction to SGA $4\frac{1}{2}$ - that it was indeed **thanks to his friend** that the seminar has finally been published:

"I would like to thank P. Deligne for having convinced me to write, in a new version of Lecture III, a demonstration of the Lefschetz-Verdier formula, **thus removing one of the obstacles to the publication of this seminar**".

Once again, we're in the middle of a farce - repeated as is by the docile Illusie in the introduction to SGA $4\frac{1}{2}$! If the seminar remained unpublished for more than ten years, it's because no one (until Deligne saved the day in 1977) had yet considered the possibility of publishing it.
good idea to write a demonstration of the so-called (and rightly so) "Lefschetz-Verdier formula", of which none other than his inseparable friend and my ex-student Verdier himself has proudly borne the paternity **since aup** . 360
minus 1964 (87_2), i.e. for at least two years by the time my seminar ended, and was just waiting to be made available to everyone!

Finally, as another and last (?) mutilation of the seminar, there was the disappearance of the fine talk Serre had given on the "(Serre-) Swan module" - a talk entitled "Introduction à la théorie de Brauer". It's fortunate that Serre, seeing the turn events were taking, had the good sense to include his talk in his book "Représentations linéaires des groupes finis" (Hermann, 1971), and make it available to the mathematical public.(87_3)

This time, I think, I've come full circle. The picture of the fate of a seminar in which I had put the best of myself (88)[98] (*), and which I find twenty years later unrecognizable, butchered by the very people who had been its exclusive beneficiaries - or at least by three of them, and with the assent of all the other participants.

I don't regret having taken the trouble, once again, to follow through on what had gradually come to my attention. This "return of things"[99] (**) that I noticed, at the end of a long retros-

[98](*) For the meaning of this expression "of the best of myself", see the following notes "La dépouille. . . ", ". . and the body", n◦ 88, 89. The first of these situates the SGA 5 seminar, with SGA 4 inseparable from it, as the masterpiece of the part of my work "entirely completed".

[99](**) See note of this name (n◦ 73) dated April 30.

pective about my relationship with one of my former students, sensing even then that he wasn't the only one to "bury me with gusto" - I've only now become aware of his breath, his "smell" (to use an expression that then appeared in one of my dreams) - the breath of **violence**. This breath is concealed and revealed at the same time by the speech[100] (***) (seemingly detached and impassive) presenting a highly technical substance. What this violence is aimed at, through a "corpse" delivered at mercy,
is the very person of the one who was the "master", the "Father" - at a time when the "pupils" have long since taken his envied place, without encountering any resistance; and that also long ago they
p. 361 have elected from among themselves the new "Father", called to replace the old and reign over them.

I feel this breath, and yet it remains for me a foreign thing, misunderstood. To "understand" it, this breath would have to live in me, or have lived in me. But four years ago, for the first time, I felt and measured the significance of something in my life that I had never thought about, that had always seemed self-evident: that my identification with my father as a child **was not** marked by conflict - that at no time in my childhood **did I fear or envy my father**, while at the same time devoting unreserved love to him. This relationship, perhaps the most profound that has marked my life (without my even realizing it before this meditation four years ago), which in my childhood was like a relationship with an other self both strong and benevolent - this relationship was not marked by division and conflict. If, through all my often-torn life, the knowledge of the strength that lies within me has remained alive; and if, in my life by no means free of fear, I have not known fear either of a person or of an event - it is to this humble circumstance that I owe it, ignored until well into my fifties. This circumstance has been a priceless privilege, for it is the intimate knowledge of the creative force within one's own person that **is** also that force, enabling it to express itself freely according to its nature, through creation - through a creative life.

And this privilege, which has exempted me from one of the most profound marks of conflict, is at the moment also like a hindrance, like a "**void**" in my experience of life. A void that's hard to fill, where many others have a rich tapestry of emotions, images and associations, offering them the path (provided they're curious enough to take it) to a profound understanding of others as well as themselves, in situations that I manage (by dint of repetition and cross-checking) to apprehend as best I can, but in the face of which I remain like a stranger - with the desire for knowledge within me still hungry.

Note 87_1 (87_1) (May 31) This closing presentation, surely one of the most interesting and substan-
tiels with the opening talk, was obviously not lost on everyone, as I see from reading Mac Pherson's paper "Chern classes for singular algebraic varieties" (Chern classes for
singular algebraic varieties, Annals of Math. (2) 100, 1974, pp. 423-432) (received April 1973)-In this paper, under the name of "Deligne-Grothendieck conjecture", I repeat one of the main conjectures I had
p. 362 introduced in this paper in the schematic framework. It is taken up by Mac Pherson in the transcendental framework of algebraic varieties over the field of complexes, the Chow ring being replaced by the homology group. Deligne had learned this conjecture[101] (*) in my talk in 1966, the same year he had appeared in the seminar where he began to familiarize himself with the language of diagrams and cohomological techniques (see the note "L'être à part" n° 67')-It's nice of you to have done me the honor of including me.

[100](***) These are mainly the introductory texts accompanying SGA 5 (written by Illusie) and SGA 4. [1] (written by Deligne). 2

[101](*) (June 6) In a slightly different form, see the rest of today's note.

(March 1985) For further details, given by Deligne himself, see the note "Les points sur les i", n° 164 (II 1).

in the name of conjecture - a few years later it would no longer have been appropriate... .

(June 6) I'd like to take this opportunity to explain the conjecture I'd put forward in the seminar in the schematic framework, while also pointing out the obvious variant in the complex analytic (or even rigid-analytic) framework. I conceived it as a "Riemann-Roch" theorem, but with discrete coefficients instead of coherent coefficients. (Zoghman Mebkhout told me, incidentally, that his view of D-Modules should make it possible to consider the two Riemann-Roch theorems as contained in a single crystalline Riemann-Roch theorem, which would thus represent in zero characteristic the natural synthesis of the two Riemann-Roch theorems I introduced into mathematics, one in 1957, the other in 1966). We fix a ring of coefficients Λ (not necessarily commutative, but noetherian to simplify and moreover of prime torsion to the characteristics of the schemes under consideration, for the purposes of stale cohomology...). For a scheme X we denote by

$$\mathrm{K.}(X, \Lambda)$$

the Grothendieck group formed by constructible etal bundles of Λ-modules. Using the functors $Rf_!$, this group depends functorially on X, for X noetherian and scheme morphisms which are separate and of finite type. For regular X, I postulated the existence of a homomorphism of groups canonical, playing the role of the "Chern character " in the consistent RR theorem, p. 363

$$\mathrm{ch}_X : \mathrm{K.}(X, \Lambda) \to \mathrm{A}(X) \otimes_7 \mathrm{K.}(\Lambda), \tag{15.1}$$

where A(X) is the Chow ring of X and $K.(\Lambda)$ the Grothendieck group formed with Λ-modules of finite type. This homomorphism was to be determined solely by the validity of the "discrete Riemann-Roch formula", for a **proper** morphism $f : X \to Y$ of regular schemes, which formula is written as the consistent Riemann-Roch formula, with Todd's "multiplier" replaced by the total relative Chern class :

$$ch_Y\,(f_!\,(x)) \;= f_*\;(ch_X\,(x)\,c(f)\,)\;, \tag{15.2}$$

where $c(f) \in A(X)$ is the total Chern class of f . It's not hard to see that in a context where we have the resolution of singularities in Hironaka's strong form, RR's formula does indeed uniquely determine the ch $\cdot_X$

Of course, we assume that we're in a context where the Chow ring is defined (I'm not aware of anyone having even attempted to write a theory of Chow rings for regular schemes of finite type over a body). Alternatively, we can also work in the graduated ring associated with the usual "Grothendieck" ring $K^\circ\ (X)$ in the coherent context, filtered in the usual way (see SGA 6). Alternatively, we can replace $A(X)$ by the even *l-adic* cohomology ring, the direct sum of $H^{2i}\ (X, \underline{Z}_l\ (i))$. This has the disadvantage of introducing an artificial parameter l, and giving formulas that are less purely numerical" fines, while the Chow ring has the charm of having a continuous structure, destroyed by switching to cohomology.

Already in the case where X is a smooth algebraic curve over an algebraically closed field, the calculation of ch_X involves delicate local invariants of the Artin-Serre-Swan type. In other words, the general conjecture is a profound one, the pursuit of which is linked to an understanding of the higher-dimensional analogues of these invariants.

Remark. Designating in the same way by $K^\cdot\ (X, \Lambda)$ "the Grothendieck ring" formed with the construcof finite tor-dimensional Λ-spreads (which ring operates on $K.(X, \Lambda)$ when A is commuta- tive. . .), we must likewise have a homomorphism

$$ch_X : K^{\cdot}(X, \Lambda) \to A(X) \otimes_{\underline{Z}} K^{\cdot}(\Lambda)$$

again giving rise (mutatis mutandis) to the same Riemann-Roch (RR) formula.

p. 364 Let *cons*(X) now be the ring of constructible integer functions on X. We define less tautological canonical homomorphisms

$$K_{.}(X, \Lambda) \to Cons(X) \otimes_{\underline{Z}} K_{.}(\Lambda)\ ,$$

$$K^{\cdot\cdot}(X, \Lambda) \to Cons(X) \otimes_{\underline{Z}} K^{\cdot\cdot}(\Lambda)\ ,$$

If we now restrict ourselves to schemes **of zero characteristic**, then (by using Euler-Poincaré characteristics with proper supports) we see that the group *Cons*(X) is a covariant functor with respect to finite-type morphisms of noetherian schemes (in addition to being contravariant as a ring functor, which is independent of characteristics), and the preceding tautological morphisms are functorial. (This corresponds to the "well-known" fact, which I believe was not proved in the SGA 5 oral seminar, that in **zero characteristic**, for a locally constant bundle of Λ-modules F on an algebraic scheme X, its image by

$$f_! : K^{\cdot}(X, \Lambda) \to K^{\cdot}(e, \Lambda) \,\blacksquare\, K^{\cdot}(\Lambda)$$

is equal to $d\chi(X)$, where d *is* the rank of F , $e = Spec(k)$, k the base field assumed to be algebraically closed. . .). This immediately suggests that the Chern homomorphisms (1$_{.}$) and (1$^{\cdot}$) must be derivable from the tautological homomor- phisms (2$_{.}$), (2$^{\cdot}$) by composing with a "universal" Chern homomorphism (independent of any ring of coefficients Λ)

$$ch_X : Cons(X) \to A(X)\ ,$$

so that the two "Λ-coefficient" versions of the RR formula appear to be formally contained in an RR formula at the level of constructible functions, which is always written in the same form.

When working with schemes on a fixed basic body (again, of any characteristic), or more generally on a fixed **regular** basic scheme S (for example $S = Spec(\underline{Z})$), the form of the Riemann-Roch formula most in line with the usual writing (in the coherent framework familiar since 1957) is obtained by introducing the products

$$ch_X(x)c(X/S) = c_{X/S}(x)$$

p. 365 (where X is in a $K(X, \Lambda)$ or $K^{\cdot}(X, \Lambda)$ indifferently), which we might call **the Chern** class **of** x **relative to the basis** S. When x is the unit element of $K^{\cdot}(X, \Lambda)$ i.e. the class of the constant bundle of value Λ, we find the image of the total relative Chern class of X *with* respect to S, by 1 "canonical homomorphism of $A(X)$ into $A(X) \otimes K^{\cdot}(\Lambda)$. This being the case, RR "s formula is equivalent to the fact that the formation of these relative Chern classes

$$c_{X/S} : K_{.}(X, \Lambda) \to A(X) \otimes K_{.}(\Lambda)\ ,$$

for a regular variable scheme X over S (of finite type over S), with S fixed, is functorial by with respect to eigenmorphisms, and similarly for the variant ($5^\cdot$). In null characteristic, this reduces to the functoriality (for eigenmorphisms) of the corresponding application

$$c_{X/S} : Cons(X) \to A(X) \,.$$

It is in this form of the existence and uniqueness of an absolute "Chern class" application (6), in the case where $S = Spec(\mathbb{C})$, that the conjecture in Mac Pherson's work is presented, the relevant conditions (here as in the general case of zero characteristic) being a) the functoriality of (6) for proper morphisms and b) we have $c_{X/S}(1) = c(X/S)$ (in this case, the "absolute" total Chern class). Compared with my initial conjecture, however, the form presented and proved by Mac Pherson differs in two ways. One is a "minus", in that it is placed, not in the Chow ring, but in the whole cohomology ring, or more precisely the whole homology group, defined by transcendental means. The other is a "plus" - and it is here perhaps that Deligne has made a contribution to my initial conjecture (unless this contribution is due to Mac Pherson himself[102] (*)). It's that for the existence and uniqueness of an application (6), we don't need to restrict ourselves to regular schemes X, provided we replace $A(X)$ by the entire homology group. The same is likely to be true in the general case, where we denote by $A(X)$ (or better, by $A_\cdot(X)$) the **Chow group** (which is no longer a ring in general) of the noetherian scheme X. Or to put it another way: while the heuristic definition of *ch-invariants*$_X$ (x) (for x in $K_\cdot(X, \Lambda)$ or $K^\cdot(X, \Lambda)$) makes essential use of the assumption that the ambient scheme is regular, as soon as we multiply it by by the "multiplier" $c(X/S)$ (when the scheme X is of finite type over a fixed regular scheme S), the product obtained (4) seems to retain a meaning without any assumption of regularity about X, as an element of a tensor product p. 366

$$A_\cdot(X) \otimes K_\cdot(\Lambda) \text{ or } A_\cdot(X) \otimes K^\cdot(\Lambda) \,,$$

where $A_\cdot(X)$ denotes the Chow group of X. The spirit of Mac Pherson's demonstration (which does not use singularity resolution) would suggest the possibility of an explicit "computational" construction of the homomorphism ($5_\cdot$), by "making do" with the singularities of X as they are, as well as with the singularities of the coefficient bundle F (whose class is x), to "collect" a cycle on X with coefficients in $K_\cdot(\Lambda)$. This would also be in the spirit of the ideas I had introduced in 1957 with the coherent Riemann-Roch theorem, where I did self-intersection calculations in particular, taking care not to "move" the cycle under consideration. A first obvious reduction (obtained by immersing X in an S-scheme) would be to the case where X is a firm subscheme of the regular *S-scheme*. . .

The idea that it should be possible to develop a **singular** (coherent) Riemann-Roch theorem was familiar to me, I can't say how long ago, but I never tried to test it seriously. It was more or less this idea (apart from the analogy with the "cohomology, homology, cap-product" formalism) that led me in SGA 6 (in 1966/67) to systematically introduce the $K_\cdot(X)$ and $K^\cdot(X)$ and the $A_\cdot(X)$, $A^\cdot(X)$, instead of just working with the $K^\cdot(X)$. I don't remember whether I also thought of something like this in the SGA 5 seminar in 1966, and whether I hinted at it in the oral presentation. As my handwritten notes have disappeared (in a removal perhaps?) I'll probably never know... .

(June 7) Looking through Mac Pherson's article, I was struck by the fact that the word "Riemann-Roch" is not mentioned - which, incidentally, is why I didn't immediately recognize the conjecture I made in the SGA 5 seminar in 1966, which was for me (and still is) a "Riemann-Roch" type theorem. It seems that when Mac Pherson wrote his article, he didn't even realize that it was a "Riemann-Roch" theorem.

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[102](*) (March 1985) This is indeed the case, cf. note no.° 164 cited in the previous footnote.

of this obvious kinship. I presume that the reason for this is that Deligne, who after my departure put this conjecture into circulation in the form he liked, took care as far as possible to "erase" it.

the obvious kinship with the Riemann-Roch-Grothendieck theorem. I think I sense his motivation for doing so. On the one hand, it weakens the link between this conjecture and myself, and makes it more plau□sible to call-

p. 367 the "Deligne-Grothendieck conjecture" under which it is currently circulating. (NB I don't know if it's in circulation in the schematic case, and if so, I'd be very curious to know under what name). But the deeper reason seems to me to lie in his obsessive desire to deny and destroy, as far as possible, the fundamental unity of my work and my mathematical vision[103] (*). This is a striking example of how, in a mathematician of exceptional means, a fixed idea entirely unrelated to any mathematical motivation, can obscure (or even completely block out) what I have called the "healthy mathematical instinct". This instinct cannot fail to perceive the analogy between the two "continuous" and "discrete" statements of the "same" Riemann-Roch theorem, which I had of course emphasized in the oral presentation. As I indicated yesterday, this kinship will no doubt soon be confirmed by a statement in form (conjectured by Zoghman Mebkhout), at least in the complex analytic case, allowing both to be deduced from a common statement. Clearly, given Deligne's "gravedigging" attitude towards the Riemann-Roch theorem[104] (**), he was not likely to discover the unique statement that links them in the analytic framework, and even less likely to raise the question of an analogous statement in the general schematic framework. Nor was he able, in such circumstances, to find the fruitful point of view of D-Modules in the cohomological theory of algebraic varieties, arising all too naturally from ideas that had to be buried - or even to recognize, for years, the fruitful work of Mebkhout, succeeding where he himself had failed.

Note 87_2 (May 31) This is the year of my Bourbaki paper on the rationality of *L-functions*, in which I heuristically use Verdier's result (? ? ?) (and especially the expected form of local terms in the case of d'espèce), without waiting for Illusie to demonstrate it thirteen years later, at Deligne's invitation. It seemed to me, moreover, when Verdier showed me his ultra-□general formula that came as a surprise,

p. 368 that he demonstrated it with a few lines of "six operations" formalism - this is the kind of formula where (almost) to write it is to demonstrate it! If there was any "difficulty", it could only be at the level of verifying one or two compatibilities[105] (*). What's more, both Illusie and Deligne knew perfectly well that the demonstrations I had given in the seminar for various explicit trace formulas **were complete**, and did not depend in any way on Verdier's general formula, which had simply acted as a "trigger" to encourage us to make explicit and prove trace formulas in as general a range of cases as possible. The bad faith of both is obvious here. As far as Deligne is concerned, it was already clear to me when I wrote the note "La table rase" (n° 67) - but probably not to an uninformed reader, nor of course to an informed reader who renounces the use of his healthy faculties.

[103](*) Compare with the commentary in the note "La dépouille" (n◦ 88) on the deeper meaning of the SGA 4 operation $\frac{1}{2}$, similarly aimed at shattering into an amorphous set of "technical digressions" the deeper unity of my work around staggered cohomology, through the "violent insertion" of the foreign text SGA $4\frac{1}{2}$ between the two indissoluble parts SGA 4 and SGA 5 which develop
this work.

[104](**) These dispositions, with regard precisely to the Riemann-Roch-Grothendieck theorem, are particularly clear in the "Funeral Eulogy"; see the note "Funeral Eulogy (1) - or compliments", n◦ 104.

[105](*) (June 6) It would also seem that, via the biduality theorem (now known as Deligne's theorem), the demonstration
of the Lefschetz-Verdier formula depended on a singularity resolution hypothesis, which Deligne manages to dispense with in the case of fi ni type schemes on a body. This is a good opportunity to fish in troubled waters and give the impression that SGA 5 would be subordinate to the "seminar-sic" SGA $4\frac{1}{2}$ which "precedes" it (and which was actually published before it).
him!).

(June 6) As for Illusie, he played right into his friend's hands, trying to muddy the waters to give the appearance of an ultra-technical oral seminar that didn't even give complete demonstrations of all the results, especially the trace formulas. These were, however, demonstrated there (and for the first time) in 65/66, and it was there that both he and Deligne had the privilege of learning them,
and the delicate technique that goes with it[106] (**).

This reminds me that, of course, I had taken the trouble to demonstrate Lefschetz-'s formula. p. 369
Verdier in the seminar - it was the least I could do, and a particularly striking application of the formalism of local and global duality that I set out to develop. The question came to me these days: why on earth, when there were a dozen or so papers still being written by my dear students, so that Deligne and Illusie were spoilt for choice when it came to naming their technical "obstacle" to the publication of SGA 5, they chose the theorem of their good friend Verdier, who at the time was taking the credit for it as his own, just as he had never bothered to write (or at least make available to the public) the theorem of derived and triangulated categories. There's a kind of **defiance** in the absurdity (or in a kind of collective cynicism in the group of my ex-cohomology students, whom I consider to be in solidarity in this operation-massacre), which reminds me of the "weight-complexes" brilliantly invented by Verdier the previous year (see the note of that name, n° 83), or (in the iniquitous register) with the "perverse" name given by Deligne to bundles that should be called "Mebkhout bundles" (see the note "La Perversité", n° 76). I sense such inventions as acts of domination and contempt towards the entire mathematical community.
- and at the same time a **gamble**, which was clearly won until the unexpected appearance of the deceased, who appears almost as the only one awake before a community of sleepyheads...

note 87_3 (June 5) After this assessment of a massacre, we will appreciate the value of Illusie's statement in line 2 of his introduction to the volume entitled SGA 5:

> "Compared with the original version, the only significant changes concern Lecture II [generic Kûnneth for- mules], which has not been reproduced, and Lecture III [Lefschetz- formula], which has been reproduced.
> Verdier], which has been completely rewritten and expanded with an appendix numbered III B[107] (*). A part some changes of detail and additions of footnotes, the other presentations have been p. 370
> left **as they are**" (emphasis added).

Here again, Illusie complacently echoes another well-sent joke from his inenarrable friend, namely that the existence of SGA $4\frac{1}{2}$ "will soon allow SGA 5 to be published **as is**" (see "Clean slate" note n° 67) - and Illusie does his utmost in his talks and introductions to lend credence to this.

[106](**) In the second paragraph of the Introduction to the volume published as SGA 5, Illusie presents as "the heart of the seminar" the three lectures III, III B, XII on Lefschetz's formula in stale cohomology, whereas we have seen that in the introduction to lecture III B., he takes care to specify (contrary to reality) that "this lecture corresponds to no oral lecture of the seminar" and that in the introductions to lectures III and III B., he does his utmost to give the impression that these are "the heart of the seminar", he is careful to specify (contrary to reality) that "this lecture does not correspond to any oral lecture of the seminar" and that in the introductions to lectures III and III B, he does his utmost to give the impression that these are subordinate to SGA $4\frac{1}{2}$, and that lecture III is presented as "conjectural"! ! In fact, the entire SGA 5 seminar was technically independent of Lecture III (Lefschetz-Verdier formula), which played the role of a heuristic motivator, and Lecture III B is no more than the "hole" (Lecture XI) created by the move to Bucur, which was the welcome pretext for this further dismemberment.

To lend credence to the version of a seminar of "technical digressions" (blown up by his friend Deligne), Illusie was careful to skip the introductory presentation, in which I had painted a preliminary picture of the main themes that were to be developed in this seminar, a picture in which the trace formulas form only a small part (taking on particular importance because of their arithmetical implications, in the direction of Weil's conjectures). For an overview of these "big themes", see sub-note no.◦ 87_5 below.

[107](*) Which is presented as part of the "heart of the seminar"! (See previous b. de p. note.)

imposture (that SGA 5, where he and his friend learned their trade, would depend on the volume-pirate SGA $4\frac{1}{2}$, made of bries and bracs gleaned or plundered over the following twelve years), by a luxury of cross-references to SGA $4\frac{1}{2}$ every page turn...

The final word goes (as it should) to Deligne, who wrote to me a month ago (May 3), in response to a laconic request for information (see the beginning of the note "Les Obsèques", n° 70):

> "In short, if it had been seven years since you were doing maths [?!] when this SGA 4 text$\frac{1}{2}$ appeared, this simply corresponds [?] to the long delay in editing SGA 5, **which was too incomplete to be usefully published as it stood**.
>
> I hope these explanations please you."

If they haven't "approved" me, at least they've edified me. . .

Note 87_4 (June 6) Perhaps it's time to indicate what the main themes were that were deve- loped in the oral seminar, and of which the published text gives an idea only by cross-checking.

I) Local aspects of duality theory, whose essential technical ingredient is (as in the coherent case) the biduality theorem (supplemented by a "cohomological purity" theorem). I have the impression that the geometrical meaning of the latter theorem, as a local Poincaré duality theorem, which I had explained so well in the oral seminar, has since been entirely forgotten by my former students[108] (*).

II) Trace formulas, including "non-commutative" trace formulas more subtle than the formula of the usual traces (where both members are integers, or more generally elements of the ring of coefficients, such as $\underline{Z/nZ}$ or an l-adic ring Z_l, or even Q_l), placing ourselves in the algebra of a finite operating group on the considered scheme, with coefficients in a suitable ring (such as those considered in the previous
p. 371 parenthesis). This generalization came very naturally, since even in the case of Lefschetz formulas of the usual type, but for "twisted" bundles of coefficients, we were led to replace the initial scheme by a Galois coating (usually branched) serving to "untwist" the coefficients, with the Galois group operating on it. Nielsen-Wecken" type formulas are thus naturally introduced into the schematic context.

III) Euler-Poincaré formulas. On the one hand, there was a detailed study of an "absolute" formula for algebraic curves, using Serre-Swan modules (generalizing the case of moderately branched coefficients, giving rise to the more naive Ogg-Chafarévitch-Grothendieck formula). On the other hand, there were novel and profound conjectures of the "discrete" Riemann-Roch type, one of which reappeared seven years later, in a hybrid version, under the name of the "Deligne-Grothendieck conjecture", proved by Mac Pherson by transcendental means (see note n° 87_1).

The comments I couldn't fail to make on the profound relationships between these two themes (Lefschetz for- mules, Euler-Poincaré formulas) were also lost without trace. (As was my habit, I left all my handwritten notes to the volunteer-writers, and no written trace remains of the oral seminar, of which I did, of course, have a complete set of handwritten notes, even if some of them were succinct).

IV) Detailed formalism for homology and cohomology classes associated with a cycle, derived naturally from the general duality formalism and the key idea of working with the cohomology "with supports" in the cycle under consideration, using cohomological purity theorems.

[108](*) This geometrical interpretation has at least been preserved in Illusie's writing.

V) Finiteness theorems (including generic finiteness theorems) and generic Kûnneth theorems for cohomology with any support.

The seminar also developed a technique for passing from torsion coefficients to l-adic coefficients (lectures V and VI). This was the most technical part of the seminar, which as a rule worked with torsion coefficients, and then "go to the limit" to deduce the corresponding l-adic results.
dants. This point of view was a pro visory pis-aller, pending Jouanolou's thesis (still unpublished at (at present) providing the formalism needed directly in the l-adic framework. p. 372

I don't include among the main "themes" the calculations of some classical schemes and the cohomological theory of Chern classes, which Illusie highlights in his introduction as "one of the most interesting" of the seminar. Since the program was so full, I didn't feel it necessary in the oral seminar to dwell on these calculations and this construction, since it was sufficient to repeat, practically verbatim, the reasoning I had given ten years earlier in the context of Chow rings, on the occasion of the Riemann-Roch theorem. On the other hand, it was obvious that it had to be included in the written seminar, to provide a useful reference for the user of étale cohomology. Jouanolou took on this task (Lecture VIII), which he had to regard not as a service to the mathematical community, while learning basic techniques essential for his own use, but as a chore, since its writing dragged on for years[109] (*). It was no different, it seems, for his thesis, which remains a ghostly reference just like Verdier's. ... The "passage à la limite" section shouldn't be counted as one of the seminar's "main themes" either, in the sense that it isn't associated with any particular geometrical idea. Rather, it reflects a technical complication peculiar to the context of stale cohomology (distinguishing it from transcendental contexts), namely that the main theorems on stale cohomology concern in the first place **torsion** coefficients (prime to residual characteristics), and that to have a theory that corresponds to rings of coefficients of zero characteristic (as is necessary for Weil's conjectures), one must pass to the limit on rings
coefficients Z/l^n Z to obtain "l-adic" results.

All this said, the only one of the five main themes of the oral seminar that appears in complete form in the published text is theme I. Themes IV and V have simply disappeared, absorbed by SGA 4. Themes IV and V have disappeared altogether, absorbed by SGA $4\frac{1}{2}$, with the advantage of being able to refer to them extensively and give the impression that SGA 5 depends on a text by Deligne that appears to predate it. Themes II and III appear in the published volume in mutilated form, still maintaining the same imposture of dependence on the SGA 4 text$\frac{1}{2}$ (which in reality emerged entirely from the mother seminar SGA 4, SGA 5).

11.3.6. The skinning

Note 88 (May 16) The set of two consecutive seminars SGA 4 and SGA 5 (which for me are like **a** single "seminar") develops from nothingness both the powerful instrument of synthesis and discovery represented by the **language of** topos, and the perfectly perfected, perfectly effective **tool of** stale co-homology - better understood in its essential formal properties, from that moment on, than even the cohomological theory of ordinary spaces[110] (*). 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 p. 373

[109](*) (June 12) Going through the presentation in question, I was convinced of Jouanolou's perfect connivance with my other cohomology students.

[110](*) Even if we restrict ourselves to the spaces closest to "varieties", such as triangulable spaces.

obvious things, this work represents the most extensive technical "tour de force" I've accomplished in my work as a mathematician[111] (**). For me, these two seminars are indissolubly linked. They represent, in their unity, both the **vision** and the **tool** - the topos, and a complete formalism of scalar cohomology.

While the vision is still rejected today, the tool has, over the last twenty years or so, profoundly re-imagined algebraic geometry in its most fascinating aspect for me - the "arithmetic" aspect, apprehended by an intuition, and by a conceptual and technical baggage, of a "geometric" nature.

It was certainly not only the intention of suggesting that his cohomological "digest" **had anteriority** over the SGA 5 part that motivated Deligne to give it the misleading name SGA $4\frac{1}{2}$ - nothing prevented him from doing so.
after all, while we're at it, to call it *SGA* $3\frac{1}{2}$! In "Operation SGA $4\frac{1}{2}$ " I sense the intention to present the work from which all his own work stems (the work from which he cannot detach himself!) - a work of obvious and profound unity, clearly apparent in the two seminars, SGA 4 and (the real) SGA 5, as thing **divided** (as itself is divided. . .), **cut in two** by this violent insertion of a foreign and disdainful text; of a text that would like to present itself as the living heart, the quintessence of a thought, of a
p. 374 vision in which he had no part[112] (*), and the two "quarters" that surround it as a sort of vaguely grotesque appendix, like a collection of "digressions" and "technical complements" to the work that Deligne claims to be central and essential, and in which my humble self is graciously admitted (before total burial) to the number of "collaborators"[113] (**).

Chance" had done it right. This "corpse left to mercy - this "unfortunate seminar" always left behind by the "editors", and at the time of my departure in the hands and at the discretion of my cohomology students - this was not **just any** part of the master's work! It wasn't SGA 1 and SGA 2 (where I was developing, in my corner and without even realizing it, the tools that were to be the two essential technical aids for the "take-off" of the main work to come), nor SGA 3 (where my contribution consisted mainly of incessant - and sometimes arduous - scales and arpeggios to hone the "all-out" technique of schematics), nor SGA 6 (systematically developing my ten-year-old ideas on the Riemann-Roch theorem and the intersection formalism), or even SGA 7 (which, through the inner logic of a reflection, stems from the possession of the central tool, mastery of cohomology). It is indeed the **main part of** my work, the writing of which had remained unfinished (and by their care. . .), that I have left, at least in part, in the hands of my cohomology students. It was this main part of the work that they chose to massacre and appropriate as their own, forgetting the unity that is their meaning and beauty, and their creative virtue (90).

And it's no coincidence either that, equipped with heterogeneous tools and denying the spirit and vision that had brought them into being from nothing, none were able to discern the innovative work where it was being reborn, against their indifference and disdain. Nor that after six years, when at the end of the line the new tool was finally
apprehended by Deligne, they unanimously buried the one who had created it in solitude - Zoghman Mebkhout, the disowned master's posthumous pupil! And it's no more a coincidence that, after the
p. 375 momentum had died down
Deligne's initial work (which in just a few years had led him to launch a new theory of

[111](**) Some difficult or unforeseen results were obtained by others (Artin, Verdier, Giraud, Deligne), and some parts of the work were done in collaboration with others. This in no way detracts (in my mind at least) from the strength of my appreciation of the place of this work in my body of work. I intend to come back to this point in more detail, in an appendix to the Thematic Outline, and to dot the i's and cross the t's where it has obviously become necessary.

[112](*) This line of thought had reached full maturity, in terms of both key ideas and essential results, even before the young man Deligne appeared on the scene to learn algebraic geometry and cohomological techniques from me, between 1965 and 1969.
(May 30) On this subject, see the note "L'être à part", n◦ 67'.

[113](**) See notes "Le feu vert" , "Le renversement", n◦ s 68, 68'.

Hodge, and towards the demonstration of Weil's conjectures), and despite its prodigious means and the brilliant means of my cohomology students, I note today this "morose stagnation" in a field of prodigious richness where everything still seems to be to be done. This should come as no surprise, when for nearly fifteen years the main source of inspiration and some of the "big problems"[114] (*), even though they are present and confronting us at every step, remain carefully bypassed and concealed, like the messengers of the one whom for fifteen years it has been our constant aim to bury.

11.3.7. . . and the body

Note 89 (May 17) The thought, the vision of things that lived within me and that I had thought I was communicating, I see as a living body, healthy and harmonious, animated by the power of renewal of living things, the power to conceive and engender. And now this living body has become a **corpse**, shared between some and others - this limb or quarter, duly stuffed, serving as a trophy for one, another, butchered, as a puzzle or boomerang for another, and yet another, who knows, as it is, for the family kitchen (we're not that far from it!) - and all the rest is good for rotting in the dump... .

That's the picture I've come to see, in terms that may be colorful but seem to express a certain reality of things. The puzzle may well fracture a skull here and there[115] (**)
- but never will these scattered pieces, neither trophy nor puzzle nor family soup, have the power so simple and obvious in the living body: that of the loving embrace that creates a new being. ...

(May 18) This image of the living body, and of the "remains" with its pieces scattered to the four winds, must have been forming in me throughout the past week. The comical form in which it presented itself under my pen-p . 376 typewriter in no way implies that this image is in the least an **invention**, a tad macabre, a burlesque improvisation on the spur of the moment. The image expresses a **reality**, felt pro- fondly at the moment it took material form through a written formulation. I must have been aware of this reality in bits and pieces, here and there, over the fourteen years since my "departure", and perhaps even before. Bits and pieces of information recorded at first on a superficial level by a distracted attention, absorbed elsewhere - but which all pointed in the same direction, and which must have assembled, on a deeper level, into a certain image - an unformulated image that I didn't bother to take note of, when I had other things to worry about. This image has been considerably enriched and clarified in the course of reflection since the end of March, six or seven weeks ago. More precisely, scattered pieces of information, finally examined by the care of a fully present conscious attention, have been gradually assembled into **another** image, at the more superficial level of the thought that examines and probes, through work that might seem independent of the presence, in deeper layers, of the first. This conscious work culminated six days ago in the sudden vision of the "massacre" that took place - when I felt the "breath", the "smell" of a **violence**, for the first time I believe in the whole reflection[116] (*). It was also the moment when, in the layers close to the surface, that

[114](*) This "main source of inspiration" is, of course, the "yoga of motives". It has been active in Deligne alone, who has kept it to himself for his own "benefit", and in a narrow form deprived of much of its force, denying some of the essential aspects of this yoga. Among the "great problems" inspired by this one, which have been ignored or discreetly discredited, I see right now (outsider that I am) the standard conjectures, and the development of the formalism of the "six operations" for all the usual types of coeffi cients, more or less close to the "motifs" themselves (which play in their respect the role of "universal" coeffi cients - those which give rise to all the others). Compare with the comments to this

See "My orphans", n° 46.

[115](**) (May 31) And it will even be used to prove a theorem "of proverbial diffi culty"!

[116](*) (June 12) In recent years, I've sensed violent intent on the part of some of my ex-students towards some of my "co-students", but never a violence that could be felt as coming from a collective will (grouping five of them here).

the feeling of a living, harmonious body, which is well and truly "massacred" - and also the one in which the deeper, diffuse image must have begun to surface, perhaps bringing to the image-in-formation a carnal dimension, a "smell" that thought alone is powerless to give.

This "carnal" aspect came to the fore again in a dream last night - it's under the impulse of this dream that I now return to the lines I wrote yesterday. In this dream, I was cut quite deeply in several places on my body. First of all, there were cuts on my lips and in my mouth itself, bleeding profusely as I rinsed out my mouth with copious amounts of water (heavily reddened by blood) in front of my face.
an ice pack. Then wounds in the belly, also bleeding profusely, especially one from which blood was coming out.
p. 377 by jerks, as if it were an artery (the Dreamer didn't care about anatomical realism). The thought
I pressed my hand in front of the wound and curled up to stop the blood - it did indeed stop flowing, eventually forming a clot and a very large scab. Later, when I carefully lifted the scab, delicate healing had already begun. I was also cut on one finger, and it was surrounded by an impressive dressing doll... ...

I have no intention of embarking on a more delicate and detailed description of this dream, nor of probing it in depth here (or elsewhere). What this dream "as is" already reveals to me with startling force is that this "body" of which I spoke yesterday, and which as I was writing I saw as detached from me, like a child perhaps that I had conceived and procreated and which had gone out into the world to follow its own path - this body remains today an intimate part of my person: that it is **my** body, made of flesh and blood and a life force that enables it to survive deep wounds and regenerate itself. And my body is also, without doubt, the thing in the world to which I am most deeply, most indissolubly linked. ...

The Dreamer did not follow me in the image of the "massacre" and the sharing of the remains. This image was meant to convey the reality of intentions, of dispositions in **others** that I had strongly perceived, and not the way in which I myself experienced this aggression, this mutilation of which I was the object through something to which I remain closely linked. The Dreamer has just given me a glimpse of the extent to which I remain linked to it. This ties in with what I perceived (albeit less forcefully) in the reflection in the note "Le retour des choses - ou un pied dans le plat" (n° 73), where I try to pinpoint the feeling of this "deep link between the one who conceived a thing, and this thing", which appeared in the course of the reflection that day. Before that reflection on April 30 (barely three weeks ago), and for the rest of my life, I have pretended to ignore this link, or at least to minimize it, following the well-trodden path of current clichés. Worrying about the fate of a work that has left our hands, and of course worrying about whether our name remains attached to it in any way, is felt to be petty and mean-spirited - whereas it seems natural to everyone that we should be deeply touched when a child of the flesh we have raised (and believe we have loved) chooses to repudiate the name he or she was given at birth.

11.3.8. The heir

p. 378 **Note** 90 (May 18) I don't know if any student in the sixties (apart from Deligne) was able to sense this essential unity, beyond the limited work he was doing with me. Some may have sensed this in a confused way, and that this perception was lost without return in the years following my departure. What is certain, however, is that from our first contact in 1965, Deligne sensed this living unity. It was this fine perception of a unity of purpose in a vast design that was surely the main stimulus for his intense interest in everything I had to communicate and pass on. This interest

people) and directed against me, through my work.

demonstrated, without ever wavering, throughout the four years of constant mathematical contact between 1965 and 1969[117] (*). He gave mathematical communication between us that exceptional quality I've already mentioned, and which I've experienced with other mathematician friends only in rare moments. It was this perception of the essential, and the passionate interest it stimulated in him, that enabled him to learn as if by playing everything I could teach him: both the technical means (zinc strand diagram technique, Riemann-Roch yoga and intersections, cohomological formalism, étale cohomology, topos language) and the overall **vision** that unites them, and finally the **yoga of patterns** that was then the main fruit of this vision, and the most powerful source of inspiration I had yet discovered.

What's clear is that Deligne was the only one of my students, right up to the present day, who at a certain point (as early as 1968, I believe) had fully assimilated and made his own the totality of what I had to transmit, in its essential unity as well as in the diversity of its means[118] (**). It was, of course, this circumstance, felt by all, that made him the designated "legitimate heir" to my work.
work. Clearly, this heritage neither encumbered nor limited him - it was not a burden, but gave him a sense of identity.
wings; I mean: he nourished with his vigor these "wings "which he had from birth, like other visions and p. 379
other heritages (less personal of course...) would nourish it...

The heritage he had nurtured in those crucial years of growth and development, and the unity that makes up its beauty and creative virtue, which he had sensed so well and which had become like a part of himself - my friend subsequently[119] (*) disowned them, striving relentlessly to hide the heritage, and to deny and destroy the creative unity that was its soul. He was the first among my students to set an example by appropriating tools, "pieces", while striving to dislocate the unity, the living body from which they came. His own creative impulse was slowed down, absorbed and finally dislocated by this deep division within him, driving him to deny and destroy the very thing that made him strong, that nourished his impulse.

I see this division expressed in three interdependent, indissolubly linked effects. One is the dissipation of energy, scattered in the effort to deny, dislocate, supplant, hide. The other lies in the rejection of certain ideas and means, essential to the subject's "natural" development.
which he has chosen as his central theme[120] (**). The third is the attachment to this theme, of all themes, which is about to supplant, to oust a master who is present every step of the way and who must be constantly erased - the very theme

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who is most intensely invested with the fundamental contradiction that dominated his life as a mathematician.

What I know at first hand, and a basic instinct or flair that has never deceived me, make

117(*) This period comprises five years, of which my friend spent one (1966) in Belgium doing his military service.

118(**) When I say "totality", I mean everything that was essential, both in vision and in means. This doesn't mean, of course, that there weren't unpublished ideas and results that I never thought of telling him about. On the other hand, I don't think there was any mathematical reflection from the years 1965-69 that I didn't talk about "on the spot" to my friend, always with pleasure and profi t.

119(*) Strangely enough, this division must have been present as early as the first year of our meeting (already expressed in an ambiguous attitude towards the SGA 5 seminar, which was his first contact with schemes, Grothendieck-style cohomological techniques, and staggered cohomology), and at the latest and in unequivocal form as early as 1968 (see note "L'éviction", n° 63).
- at a time, therefore, when mathematical communication was perfect, and when the growth of his mathematical thought seems to me not yet marked by the conflict. At the time, he made a number of interesting contributions (which I take great pleasure in highlighting in the Introduction to SGA 4) on topics that he did his utmost, immediately after my departure, to bury.

120(**) This refusal has manifested itself in the burial of derived and triangulated categories (until 1981), of the formalism of the six variances (until today), of the language of topos (also), and by a sort of "blocking by disdain" of the vast program of foundations for homological and hoinotopic algebra, which I'm now trying (twenty years later) to sketch out with La Poursuite des Champs, and which he had of course also felt the need for. However, even though it was inspired by the yoga of motifs (buried until 1982), this yoga remained mutilated of part of its force, being detached from the formalism of the six variances which constitutes an essential formal aspect. It seems to me that this aspect has also been rigorously banished from Hodge-Deligne's theory.

it's quite clear to me that if Deligne hadn't been torn by this profound contradiction in his own work, mathematics today wouldn't be what it is[121] (*) - that it would have undergone, in several of its essential parts, far-reaching renewals like the one I myself had been the main instrument of - the very one that this same Deligne was bent on countering and diverting![122] (**)

There was also no doubt that he was ideally suited to be the driving force behind a powerful school of geometry, a continuation of the one that had formed around me - a school nourished by the vigor of the one from which it had emerged, and the creative power of the one who was taking over from me. But this school that had formed around me, this nurturing matrix that had surrounded intense years of training - it broke up the day after my death.

p. 381 of my departure. If this was the case, it was precisely because I couldn't find, in the person who was obviously taking over from me[123] (***), the person who would also be the soul of a group united by a common adventure, for a common task.
whose dimensions are beyond anyone's means.

I have the impression that, after my departure, each of my students found themselves in their own corner, with a wealth of work to do - there's no shortage of that anywhere in maths - but without this "corner" fitting into a whole, and without this "work" being carried along by a current,' by a wider purpose. Surely, as soon as I left, if not even before, the eyes of most of my students or ex-students were focused on the designated "successor", the most brilliant among them and also the closest to me. At this sensitive moment, my friend must have felt, perhaps for the first time in his life, the power over others that was suddenly in his hands, the power of life or death he had over the fate of a certain school from which he had come, and whose friends he had rubbed shoulders with for four years were no doubt expecting him to ensure its continuity. The situation was entirely in his hands, and it was he who would set the tone... . He did indeed set the tone, by destroying the legacy, and first and foremost that confidence and expectation[124] (*) which those who, with him, had been pupils of the same master, could not fail to bring him. ...

I'm sure many people are impressed by Deligne's work, and not without reason. But I'm also well aware that this work, beyond the impressive initial impetus (ending with the demonstration of Weil's conjectures), is far from "living up to its potential". It certainly testifies to an uncommon technical mastery and ease, placing him among the "best". But it lacks the humble virtue that

[121](*) When I wrote these lines about "mathematics today", I wasn't just thinking about the more or less profound knowledge we have of mathematical things today. I was also thinking, in the background, of a certain **spirit** in the world of mathematicians, and more particularly in what might be called (without sarcastic or mocking intonation) "the big mathematical world": the one that "sets the tone" for deciding what is "important", even "licit", and what is not, and the one that also controls the means of information and, to a large extent, careers. Perhaps I'm exaggerating the importance that a single person, in the position of fi gure de proue, can have on the "spirit of the times" in a given milieu at a given time. Deligne's career seems to me to be comparable (for better or for worse) to Weil's in the milieu that had welcomed me twenty years earlier, and with which I had identified myself for twenty years.

(May 31) Compare with the (complementary) reflections in the note "Le Fossoyeur - ou la Congrégation toute entière", n◦ 97.

[122](**) (June 16) I'm convinced that the very fact that the key ideas I've introduced into mathematics are already developing normally, on the momentum gained in the sixties (cut short by the "chainsaw effect" discussed in the next two notes. . .), mathematics today, fifteen years after my departure, would have been different from what it is, in some of its essential parts. ...

[123](***) This **de facto succession** was expressed by unequivocal concrete signs: he took over from me at the IHES (from which I left the year after he joined - see note "L'éviction", n◦ 63), and he took over, with the means I had developed at this fi nal for some fifteen years (from 1955 to 1970), the central theme of the cohomology of algebraic varieties.

[124](*) (May 26) In the course of further reflection, I detected yet another "expectation" regarding my tacit heir, this time coming not from my students alone, but from "the entire Congregation" - see the end of the note "Le Fossoyeur - ou la Congrégation toute entière" (n◦ 97). I have little doubt that these two opposing expectations, one linked to a moment in time and the other continuing throughout the fourteen years of Burial, are both real. Much more so, I'm inclined to think that for many of my former students, the two expectations must have been present simultaneously: that of finding in the most brilliant among them the one who would ensure the continuity of a School and a work in which they had their place and their part - and that of seeing erased (if that were possible) all trace of the one whose departure suddenly called out to them with such force, in the quietude of the well-trodden paths. ...

the virtue of renewal. This virtue he carried within him, this freshness or innocence of the little child, has long since been deeply buried, denied. I was about to write that
by this "virtue" and by his not very com□muns gifts, as well as by the exceptional circumstances of which he p. 382
benefited from the deployment of his gifts, Deligne was called upon to "dominate" the mathematics of our time, just as a Riemann, or a Hilbert, had each "dominated" the mathematics of their time. Inveterate habits of thought, rooted in common parlance, have suggested to me this image of "domination", which nevertheless gives a distorted apprehension of reality. These great men undoubtedly fully "grasped", "assimilated", "made their own" the mathematics known in their time, which undoubtedly also gave them an exceptional mastery of technical means. But if they rightly seem "great" to us, it's not through their technical prowess, "wresting" difficult demonstrations from surly substance. It's by the renewal each of them brought to several important parts of mathematics, by simple and fruitful "ideas", that is to say: for having turned their gaze on simple and essential things, to which no one before them had deigned to pay attention. This childlike ability to **see** simple, essential things, however humble and disdained by all - **this is the** power of renewal, the creative power in everyone. This power was present to a rare degree in the young man I knew, unknown to all, a modest and passionate lover of mathematics. Over the years, this humble "power" has seemed to disappear from the person of the admired and feared mathematician, enjoying unfettered his prestige, and the (sometimes discretionary) power it gives him over others.

This **stifling** in my friend of something very delicate and very vivid, neglected by everyone and which has creative power, I've felt it many times since I left, and more and more in recent years. But it took the discoveries of the last few weeks, and the reflection I've been pursuing since the end of March (following on from Récoltes et Semailles), to begin to feel the full extent of the devastating effect of this suffocation in the life of my friend, and among many others I've known closely. Not only on some of my "later" (and assimilated) students, who were subjected to his malevolence (perhaps unconscious in some cases), which was exercised against each and every one of them and weighed heavily on three of them; but also, it seems to me
glimpse it now, among my students "before", by the destruction of a **continuity** in the subject, and
that of a sense of a whole, a unity, giving a deeper and broader meaning to their work than ce□lui p. 383
an accumulation of separate prints bearing their names (91)[125] (*).

More than once in the last seven years, and more than once again in the last few weeks and days, I've felt a sadness, at what feels, on some level, like an immense **waste** - when what is most precious in oneself and in others is squandered or smothered as if for pleasure. Yet I've also come to learn that such "waste" is a staple of the human condition, to be found in one form or another everywhere, in the lives of individuals, from the humblest to the most illustrious, as well as in the lives of peoples and nations. This very "mess", which is nothing other than the action of conflict and division in everyone's life, is a substance of a richness and depth that I have barely begun to fathom, a nourishment that it is up to me to "eat" and assimilate. So this mess, and every other mess I encounter at every step, and every other thing that happens to me at the turn of the road and is so often unwelcome - this mess and other unwelcome things carry within them a **blessing**. If meditation has meaning, if it has the power of renewal, it's insofar as it enables me to receive

[125](*) (June 16) This second aspect only came to my attention in the course of my reflection on L'Enterrement. If ever I saw a prestigious mathematician make use of the "power to discourage", it was the very man who once seemed to me to be my heir apparent. When I wrote the section "The power to discourage", I had thought a lot about him (before the thought came back to me), but without yet having the slightest inkling (at least not on a conscious level) of the extent to which this power had found occasion to be exercised among the very people for whom he must have been (as he was for me) the model of the perfect mathematician. ...

the benefit of that which (through my inveterate reflexes) presents itself as "evil", where it allows me to **nourish** what seems designed to destroy.

Nourishing yourself with your experience, letting it renew you instead of constantly evading it - that's what it means to take full responsibility for your life. I have this power within me, and it's up to me at any given moment to make use of it, or to let it go to waste. It's the same for my friend Pierre, and for each of my students - free, like me, to feed on the "mess" I'm finishing reviewing in these last days of a long meditation. And the same goes for the reader who reads these lines, intended for him or her.

11.3.9. The co-heirs

p. 384 **Note** 91 (May 19) The echoes that have reached me here and there about my former pupils have been more than sparse.

Hardly any of them gave me any sign of life after my departure, if only by sending me prints[126] (*). However, by collecting the few that have reached me, I can form an idea, admittedly very approximate. Perhaps it will become clearer in the months to come, if this reflection prompts some of them to come forward.

I've already had occasion to note the profound rupture in Deligne's work after my departure, even though in some respects he appears, unwillingly, as a successor, and therefore as part of a certain continuity. And I had the feeling that this rupture must have had a profound effect on the work of all my other students. It's this impression that I'd like to explore a little more closely.

The only one of these students whose work seems to be an obvious (at least at first sight) extension of the work he had done with me, seems to be Berthelot[127] (**). He's also the only one who, for a long time, sent me numerous separate prints - perhaps even all his separate prints. They are all on the difficult subject of crystalline cohomology, the systematic start-up of which is the subject of his thesis. Yet it seems to me that, as with my other (commutative) "cohomology" students, his work is marked by the disaffection of some of the main ideas I had introduced: derived categories (and triangulated categories, cleared by Verdier), six-operation formalism, topos (91_1). As Zoghman Mebkhout himself says, his own work, so close in theme to Berthelot's (91_2), is in line with these ideas, combined with those of the Sato school. If they hadn't been repudiated by my cohomology students, led by Deligne and Verdier, there's a good chance that from the very beginning of the seventies,

Mebkhout's crystalline theory (which he began to develop only from 1975, against the disinterest of these p. 385 same students) would already have reached the full maturity of a formalism of six operations, which it still hasn't reached today[128] (*).

Incidentally, I remember talking to Verdier about the question, which intrigued me, of the link between constructible dis- cret coefficients and continuous coefficients, without it seeming to catch his eye. It must have caught on later

[126](*) (May 31) On this subject, see note no. ◦ 84, following the note on "Silence" (no. ◦ 84).

[127](**) Based on the theme of duality that Verdier pursued for several years after I left, in the context of the spaces

I'm sure that, in the case of Berthelot, there's an impression of continuity. But it seems to me that this has been something of a "routine continuity", whereas the one whose signs (or absence of signs) I'm mainly looking for is a creative continuity, continuing an initial impulse into the unknown. ...

[128](*) (June 7) I hesitated to hazard this assessment, which could be interpreted as minimizing the originality of Mebkhout's theory. This would not be at all in line with my thinking, and all the more so as I have an excellent opinion of the abilities of each of my cohomology students (when they are not blocked by prejudices alien to mathematical common sense). My friend Zoghman himself dispelled any scruples I might have had, saying that he was convinced that "normally", it was my students who should have been developing his theory from the very beginning in the 70s. At a certain level, they're surely the first to be convinced: it's they, or Deligne, who **should have** developed it - and with the general degradation of morals, that's all it takes to behave as if they did (or as if Deligne did).

was) well and truly! On this subject, see the notes "Le Colloque" and "La mystifi cation", n ◦ s 75' and 85'.

(See note "L'inconnu de service et le théorème du bon Dieu", n° '48.) In fact, he was so "blocked" by his burial syndrome that, until October 1980, he failed to perceive the importance of Mebkhout's work - and when he finally did, it was in the grave-digging mood we all know (see notes n° s 75 to 76).

As far as I'm aware, Verdier's work since his thesis defense has essentially been limited to redoing in the analytical context (which sometimes presents additional technical difficulties) what I had done in the coherent schematic framework, without introducing any new ideas. It's even rather extraordinary, with the reflexes he was supposed to have developed, and well-informed as he was, that he didn't come across Mebkhout's theory himself, by dint of turning his crank - and that he didn't at least recognize that his "pupil" was doing some interesting things, which had escaped him (as they had escaped Deligne).

To tell the truth, while intrigued by the question of the relationship between discrete coefficients and coefficients continuous, I hadn't really had any inkling of Mebkhout's crystalline theory, which would blossom in the decade following my departure. On the other hand, there was a vast theme, arising from my reflexions of cohomology p. 386 both commutative and non-commutative of the fifties (1955-1960), and which was just beginning (in the "commutative" context i.e. in terms of additive categories) in Verdier's work, started in the early sixties and left behind after his defense (see note n° 81). The non-commutative aspect was initiated later in Giraud's thesis, which developed a geometric language, in terms of 1-fields over a topos, for non-commutative cohomology in dimension ≤ 2. By the second half of the sixties, the inadequacy of these two primers was quite obvious: both in terms of the inadequacy of the notion of "triangulated category" (teased out by Verdier) to account for the richness of structure associated with a derived category (a notion destined to be replaced by the considerably richer notion of **derivator**), and in terms of the need to develop a geometric language for non-commutative cohomology in any dimension, in terms of n-fields and **∞-fields** over a topos. One sensed (or I sensed) the need for a synthesis of these two approaches, which would serve as a common conceptual foundation for homological algebra and homotopic algebra. Such a work was also in direct continuity with Illusie's thesis work, in which both aspects are represented.

Bousfield-Kan's seminal work on homotopic limits (Lecture Notes n° 304), published in 1972, was also in line with this diffuse program, which since at least 1967 had been just begging to be developed. In January of last year, without yet suspecting that I would be embarking on the Pursuit of Fields a month later, I submitted to Illusie some thoughts on the "integration" of homotopy types (familiar to homotopists as "homotopic (inductive) limits"), at a time when I was still completely unaware of the existence of Bousfield and Kan's work, and that this type of operation had already been examined by others than myself. It turned out that Illusie was equally unaware of it, despite the fact that he was supposed to have remained in homologous-homotopic waters for all the time since my "death" in 1970! This just goes to show how far he seems to have lost touch with certain realities that are part and parcel of the fundamental thinking he himself had been pursuing in the sixties[129] (*). He must have made his own little hole, from which he hardly ever emerges. ...

With the disdain that has befallen the very notion of topos and all the "categorical nonsense", it is not surprising p. 387

[129](*) This notion of "integrating" homotopy types had come to me again, in the context of unscrewing stratified structures, which I took up at the end of 1981.

that Giraud now has a total disaffection for what had been his first major theme of work. It's true that Deligne, with the exhumation of motives two years ago, pretended to have suddenly discovered the interest of the arsenal of non-commutative cohomology, sheaves, links and consorts, as if he had just introduced them himself, along with motives and motivic Galois groups[130] (*). It's doubtful that this kind of circus will rekindle a flame that he himself has worked so hard to extinguish. ... In February last year, I sent Giraud a copy of the twenty-page letter that became Chapter 1 of the opening chapter of La Poursuite des Champs. It's a non-technical reflection, in the course of which I managed to "jump with both feet" over the "purgatory" that had once stopped Giraud (and many others) from handling the notion of "non-strict" *n-category* (which I now call "*n-field"*), which remained heuristic and yet was visibly fundamental. This was the start of the Poursuite des Champs. When we met (on friendly terms) last December for Contou-Carrère's thesis defense, I learned from Giraud that he hadn't even had the curiosity to read the letter! I got the impression that he'd drawn a long line under such things. The idea that there might be some rich substance, in a direction he had long since abandoned, didn't even seem to occur to him. I tried, unsuccessfully I'm afraid, to get him to understand that there was a juicy and vast work here that had been waiting for nearly twenty years to be done, and which I finally got round to in my old age, to at least give a broad outline, under the dictation of things themselves, of a rich substance that the "deceased" me continues to feel strongly about, while my students have long since forgotten about it.

Jouanolou also abandoned a research direction he had just begun with his thesis. This direction had become the object of the disdain of a fashion established by the very person who had provided him with a master technical idea for the theme he had chosen. With the "rush" on triangulated categories with the Colloque Pervers three years ago, this same Deligne suddenly pretended (without laughing) to discover the big job of foundations in perspective, the lack of which is suddenly being felt at all ends, and which he had been the first to discourage for ten years - The need for such a job was quite obvious to me as soon as 1963/64 with the beginnings of étale cohomology; and for Deligne just as much, from the moment he started hearing about *l-adic* cohomology and triangulated categories, i.e. when he arrived at my seminar the
p. 388 following year. Beyond the construction of "constructible triangulated categories" on the ring $\underline{Z}_l$ (above a basic scheme, let's say), and the development of the formalism of the "six operations" within this framework (something accomplished, it seems to me, in Jouanolou's thesis), to make analogous work by replacing the base ring $\underline{Z}_l$ by an arbitrary (more or less?) Noetherian $\underline{Z}_l$ -algebra, e.g. Q_l or an (algebraic?) extension of Q_l . This is one of the things for which time has been ripe for some twenty years, and which are still waiting to be done, when the wind of contempt that has blown over them has died down... .

The natural continuation of Ms. Raynaud's work (weak Lefschet theorems in staggered cohomology, in terms of 1-fields) would have been placed in a context of strictly taboo ∞-fields, let's not talk about it! The same goes for Ms. Sinh's work, begun in 1968 and completed only in 1975 - a natural continuation would have been the notion of an enveloping ∞-category of Picard of a so-called "monomial" category, or of triangulated variants of such a category[131] (*) - let's not! Another was to transpose her work in terms of fields onto a topos - what a horror! As for Monique Hakim, she too had the misfortune to write her thesis on a subject which, these days since my untimely departure, looks a little ridiculous on

[130](*) See "Souvenir d'un rêve. . . - or the birth of motifs", note n° 51.

[131](*) See "Souvenir d'un rêve. . . - or the birth of motifs", note n° 51.

edges - relative diagrams on a locally ringed topos, I ask you! His little book on the subject, published in Grundlehren (Springer), must sell three or four copies a year - no wonder I've got bad press there, and they're not too keen to accept any text I might recommend. For me, it was a first test-step towards a "relativization" of all "absolute" notions of "varieties" (algebraic, analytic, etc. . .) on general "bases", the need for which is obvious to me (91_3). It's true that we've done just fine without them until now. But it's also true that we've done without maths for the two million years we've been around.

The fact remains that Monique Hakim, who was not motivated to write her thesis in the same way as I was to offered it to him, surely had no desire to keep any contact with a theme which (detached from p. 389

In the context of a favorable consensus, or stubborn thought pursuing a tenacious and sure vision against all odds, it can no longer make the slightest sense.

As for Neantro Saavedra Rivano, he seems to have disappeared entirely from circulation - I can find no trace of his name even in the official world directory of mathematicians. What is certain is that his somewhat categorical thesis subject could hardly have been in good press with the gentlemen who decide what is serious and what is not. The most natural continuation of this thesis, in my opinion, would have been neither more nor less than this "vaste tableau des motifs", a theme decidedly a little broad for this student's more modest aims. Yet he ended up having the unexpected honor of having his thesis redone ab ovo et in toto by one of these great gentlemen himself, barely two years ago (see notes on "L' Enterrement - ou le Nouveau Père" and "La table rase", n° s 52 and 67.)

Finally, the only ones among my twelve "pre-1970" students for whom it's not too clear to me whether or not there was a more or less drastic or profound **break in** their work, compared to the one they had to follow in my contact, are Michel Demazure and Michel Raynaud (91_4). All I know is that they've continued to do maths, and that they're part (as you'd expect, given their brilliant means) of what I called earlier "the great mathematical world".

The foregoing brief reflection, based on what is sometimes very little data, is of course largely hypothetical and very approximate. I hope that those mentioned here will forgive me for any perhaps gross errors of assessment, which I'll be happy to rectify if they'd be so kind as to let me know. Here again, I realize that everyone's case is surely different from everyone else's, and represents a much more complex reality than someone as distant as me can reasonably apprehend, let alone express in a few lines. All these reservations aside, I have the impression that this reflection has not been in vain, for me at least, to identify by a few concrete facts, a still diffuse impression that had emerged yesterday (and which was undoubtedly present at an informal level for many years): that of a **break** that was made p. 390

in many of my students in the aftermath of my departure, reflecting on a personal level the sudden disappearance, overnight, of a "school" to which they must have felt a part during crucial formative years in their mathematical profession.

Note 91_1 (May 22) I've just come across an article-survey from the Colloque "Analyse *p-adique* et ses applications" at CIRM, Luminy (September 6-10, 1982), by P. Berthelot, entitled "Géométrie rigide et co-homologie des variétés algébriques de car. *p*" (24 pages), which outlines the main ideas for a synthesis of Dwork-Monsky-Washnitzer cohomology and crystalline cohomology. The initial ideas (and the very name) of crystalline cohomology (inspired by Monsky-Washnitzer cohomology), and the idea of complementing these with the introduction of sites formed by rigid-analytic spaces, ideas that I had introduced in the

In the sixties, they became the daily bread for all those working in the field, starting with Berthelot, whose thesis developed and fleshed out some of these initial ideas. Nevertheless, my name is conspicuously absent from both the text itself and the bibliography. Here we have a fourth clearly identified pupil-croquemort. Who's next?

(June 7) It's a remarkable fact that more than fifteen years after I introduced the starting ideas for crystalline cohomology, and more than ten years after Berthelot's thesis established that the theory was indeed "the right one" for clean, smooth schemes, we still haven't reached what I call a situation of "mastery" of crystalline cohomology, comparable to that developed for stellar cohomolo- gy in the SGA 4 and 5 seminar. By "mastery" (in the first degree) of a cohomological formalism including duality phenomena, I mean no more and no less than full possession of a six-operation formalism. While I'm not "in the know" enough to be able to appreciate the difficulties specific to the crystalline context, I wouldn't be surprised if the main reason for this relative stagnation lies in the disaffection of Berthelot and others for the very idea of this formalism, which makes them neglect (just like Deligne's Hodge theory, which remained in its infancy) the first essential "level" to be reached in order to have a fully "adult" cohomological formalism. These are the same kind of dispositions

p. 391 which have surely led him to overlook the relevance of Mebkhout's point of view to his own research. NB When I speak here of "crystalline cohomology" in a context where one abandons assumptions of cleanliness (as is necessary for a "fully grown-up" formalism), it is understood that one is working with a crystalline site whose objects are (power-divided) "thickenings" that are not purely infinitesimal, but are "proper" (power-divided) topological algebras. The need for such an extension of the primitive crystal site (which for me was only a first approximation for the "right" crystal theory) was clear to me from the start, and Berthelot learned it (along with the initial ideas) by null other than myself. A written allusion to this link can be found in Esquisse Thématique, 5 e.

Note 91_2 It's quite extraordinary that nobody but me seems to have realized that Mebkhout's unnamed theory was an essential new part of a crystalline theory. As someone who has been completely "out of cohomology" for nearly fifteen years, I realized this as soon as Mebkhout took the trouble to explain to me what he had done last year. In any case, when I mentioned the matter (as a matter of course) to Illusie, he seemed to see it as a rather "kooky" combination of things (*D-Modules* and crystals) that really had nothing to do with each other. Yet I know first-hand that he has a mathematician's flair, and so do my other students (coho- mologists in this case, starting with Deligne) - but I can see that in certain situations, he's no longer any use to them... ... The more I think about it, the more I find it extraordinary that in such an atmosphere, Meb- khout still managed to do his job, without letting his own mathematical flair be defused by the total incomprehension of his elders, so far above him. . .

Note 91_3 It was especially since my lectures at the Séminaire Cartan on the foundations of the theory of complex analytic es- paces, and on the precise geometrical interpretation of "level modular varieties" à la Teichmüller, towards the end of the fifties, that I understood the importance of a double generalization of the common notions of "variety" we've been working with so far (algebraic, real or complex analytic, differentiable - or subsequently, their "moderate topology" variants). One is to broaden- gir the definition so as to admit arbitrary "singularities", and nilpotent elements in the structural bundle of

p. 392 "scalar functions" - along the lines of my foundational work with the notion of schema.

The other extension is towards a "relativization" above suitable locally annelated topos ("absolute" notions being obtained by taking a punctual topos as a base). This conceptual work, matured for over twenty-five years and initiated in Monique Hakim's thesis, is still waiting to be taken up. A particularly interesting case is that of the notion of relative rigid-analytic space, which allows us to consider ordinary complex analytic spaces and rigid-analytic spaces over local bodies with variable residual characteristics, as "fibers" of the same relative rigid-analytic space; just as the notion of relative scheme (which has finally become commonplace) allows us to link together algebraic varieties defined over bodies of different characteristics.

Note 91_4 While Demazure's thesis work, like Raynaud's, makes essential use of a consummate schematic technique they learned from me, the essential ideas in their respective works are not part of the "Grothendieckian" panoply, which distinguishes their work from that of my other students of the first period. It's possible that this circumstance resulted in a continuity in their work, free from a rupture due to the effect of the "master's burial syndrome". This doesn't necessarily mean that this syndrome didn't affect one or the other in another way. Three years ago, I was struck by Raynaud's attitude towards Contou-Carrère's work on local relative Jacobians. The results announced are profound, difficult and beautiful, and go far beyond a simple genera- lization of "well-known" things. There's an unexpected link with Cartier's theory of typical curves, some wonderful explicit formulas - all entirely within Raynaud's (and my) grasp. The freshness of his welcome must have weighed decisively in Contou-Carrère's strategic retreat, abandoning for profit and loss a subject in which he had invested himself wholeheartedly and which, it may have seemed, was only going to get him into trouble. ... [132](*). My letter to him, in which I expressed my (pained) surprise at his insensitivity to the beauty of these results, went unanswered.

11.3.10. . . and the chainsaw

Note 92 When I moved to the area nearly four years ago, there were not far from p. 393
me a beautiful cherry orchard. Often, when I went for a walk, I'd go and have a look. It was a pleasure to see these thick cherry trees in the prime of their lives, with their powerful trunks, which seemed to have always been one with this piece of land, where wild grasses proliferate freely. They must not have known about fertilizers or pesticides, and in cherry season, that's where I'd go to pick tasty ones. There must have been twenty or thirty trees.

One day, when I went back there, I saw all the trunks cut down to man-height, the crowns slumped on the ground next to the trunk, stumps in the air - a vision of carnage. With a good chainsaw, it must have been done in an hour or so. I'd never seen anything like it - when you cut a tree, you usually take the trouble to bend down and cut it flush with the ground. There's a shortage of cherries, of course, and this cherry orchard wasn't going to produce tons, that's for sure - but these stumps of trunks said something other than shortage and yields... .

Yesterday I had that feeling again, of a vigorous trunk, with powerful roots and generous sap, with strong, multiple branches extending its momentum - cut clean off, at man's height, as if for pleasure. It was taking the trouble to look at the main branches one by one, and seeing each one cut off, that finally made me see what had happened. What was made to unfold, in the continuity of a momentum,

[132](*) For further details, see sub-note no.◦ 95_1 to note "Cercueil 3 - ou les jacobiennes un peu trop relatives", no.◦ 95.

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of a deep-rooted inner necessity, has been sliced clean through, with a clean slice, to see itself designated for all to see as an object of derision.

This reminds me of the "misunderstanding" Zoghman referred to, which supposedly took place between me and my students (except Deligne). What's clear, in fact, is that neither impetus nor vision were communicated from me to any of my students (apart from Deligne, who is decidedly "apart" indeed!). Each of them assimilated a technical baggage, useful (and even indispensable) for doing a well-done job on the subject they had chosen, and which could even be of use to them later on. I can't say whether there was any hint of something else, going beyond that. If there was
had, it didn't stand a chance against the chainsaw, which quickly trimmed it... ...

I'm well aware that if there are still people doing maths - and unless they give up completely
the kind of maths we've been doing for over two millennia - they can't help but one day or other breathe new life into each of these branches that I see lying inert. Some of them have already been taken over by my friend-with-the-saw, and it's quite possible, if God gives him life, that he'll do the same with some or all of them. Most of them, however, are no longer in his style. But perhaps he'll also eventually tire of constantly substituting himself for someone else, something that's surely very tiring and, what's more, not at all profitable, so that he'll be content to be himself (which isn't bad at all).

15. C) THE BEAUTIFUL WORLD

Contents

15.1. VII The Colloquium - or Mebkhout and Perversity bundles

15.1.1. Iniquity or the meaning of a return

p. 285 **Note** 75 (May 2) I'm definitely not done learning! I've just read two texts, which shed unforeseen light (for me at least) on the "escamotage" (of Meb- khout's work) already mentioned ("L'inconnu de service et le théorème du bon Dieu", note (48)). It concerns the role played by the two illustrious colleagues and former students whose disdainful indifference to Zoghman Mebkhout I noted, without however questioning their professional bona fides. Both texts are part of the Proceedings of the **Luminy Colloquium** (July 6-11, 1981) entitled **Analyse et topologie sur les espaces singuliers**, published in Astérisque n° 100 (1982).

The first of these texts is the introduction to the Colloquium, signed by **B.Teissier** and **J.L. Verdier** (the same man who acted as Z. Mebkhout's official thesis supervisor). This one-and-a-half-page text begins with an explanation of a certain "Riemann-Hilbert correspondence", which is clearly destined to play a leading role in the Colloquium (and which is none other than the "theorem of the good Lord" alias Mebkhout). In this correspondence (and this is what gives it its charm and depth, and necessitates the introduction of derived categories), a regular holonomic **module** (i.e., a regular holonomic complex reduced to degree zero) is associated with a constructible complex of C-vector bundles, which can be characterized (it is said) by purely topological properties that make sense for constructible complexes of stale bundles over a not necessarily smooth variety defined over any body.
This, it is explained, is the starting point for the Colloquium's "main theme", "**perversity, intersection complex, purity**"-the (complex of) so-called "**perverse**" beams[1] (*) being none other than thosep . 286
which, "morally", correspond ("à la Mebkhout") to the simplest complexes of regular holonomic differential operators, expressed using a single D-Module.

The second text is part[2] (**) of the long article by **A.A. Beilinson, J. Bernstein and P. Deligne** on perverse bundles, referred to in the introduction as the central work of the Colloquium. As can be seen from the table of contents and the other pages at my disposal, this paper marks the sudden re-entry of derived and triangulated categories into the public arena, in the wake of Mebkhout's obscure work and the famous "Riemann-Hilbert" theorem.

Incredibly, in both texts, Z. Mebkhout's name is absent. Mebkhout is absent, just as he is absent from the bibliography. I should point out that not only was J.L. Verdier perfectly aware of Mebkhout's work (and with good reason!), but so was Deligne (and it would be difficult even to conceive that it could be otherwise, for someone so well informed about current mathematical events, and when it's about the subject that touches him most closely[3] (***)).

I don't know what happened to B. Teissier[4] (****) and the other participants in the Colloque de Luminy, notably the two co-authors with Deligne of the article cited[5] (****). It seems that none of the participants was so curious to know the authorship of the ideas and the key theorem that had had the virtue of mobilizing them.

[1](*) (May 4) See note no.° 76, "Perversity", on this strange application.

[2](**) (May 4) I've since received the full article, which confirms what the part I had already shown me.

[3](***) In particular, Mebkhout's work and his "theorem of the good God" represent a decisive advance on Deligne's earlier work (from 1969), which he refrained from publishing. On this subject, see note n° 48' already quoted.

[4](****) (June 12) B. Teissier had long taken an interest in Mebkhout's work, and had been one of the very few to have an encouraging attitude towards him. He was therefore perfectly aware of the fraud, to which he knowingly lent his support. He justified himself to Mebkhout by assuring him that, in any case, he "couldn't have done anything about it".

[5](*****) (May 28) I have since learned that A.A. Beillinson and J. Bernstein were informed of Mebkhout's results by P. Deligne (in October 1980) and by Mebkhout (in detail in November 1980, at a conference in Moscow). These two authors made essential use of the God's theorem in their demonstration of a famous conjecture known as the Kazhdan-Lusztig conjecture even before the Colloque de Luminy in June 1981. Compare the quotation from Zoghman Mebkhout's letter in the note "Un sentiment d'injustice et d'impuissance" (note n° 44").

(June 3) For further details about the solidarity of all Colloquium participants, see the following note "The Colloquium", n° 75'.

I assume that it was taken for granted, a little (a lot) like in the volume of the lecture Notes LN 900 which, the following year, was to consecrate the re-entry of motifs on this same "public square"[6] (******); that paternity belonged to the brightest among the brilliant mathematicians who had taken the initiative of the Colloquium and

p. 287 had animated it. What everyone knew for sure was that it was neither Riemann nor Hilbert, otherwise the brilliant Colloquium would have taken place in 1900 and not in 1981, two years after the pupil's thesis defense.

Unknown by Jean-Louis Verdier.

The kind of operation I've witnessed here is perhaps now commonplace[7] (*) and perfectly acceptable, as long as it's carried out by mathematicians who are at the top of their game, and the one who pays the price is a vague unknown (even though he's been kindly invited to join in the fun). The fact that one of these men is a great mathematician, both in terms of his means and his work (which puts him above suspicion from the outset), doesn't change the nature of the matter. Surely I'm old-fashioned - in my day, this kind of operation was called a **swindle**, and this one strikes me as a **disgrace** to the generation of mathematicians who tolerate it.

The brilliance of genius takes nothing away from such a disgrace. It adds an unprecedented dimension, perhaps unique in

p. 288 the history of our science[8] (**). Behind the apparent absurdity and gratuitousness of the act (carried out by someone whose lot has been fulfilled beyond measure, yet who delights in plundering. . .), we can glimpse the action of forces other than the mere desire to shine, or the gratuitous desire to humiliate or despair those who feel defenceless and voiceless.), the action of forces other than the mere desire to shine, or the gratuitous desire to humiliate or despair those who feel defenseless and voiceless.

As I'm definitely in the middle of a "tableau de moeurs", I'd like to point out (almost as a matter of course) that my name is equally absent from the quoted texts. Yet I was pleased to note that there is not a single page of the quoted article (among those in my possession[9] (*)) that is not deeply rooted in my work and bears its mark, right down to the notations I introduced, and the names used for the notions that come into play at every step - which are the names I gave them when I first became acquainted with them before they were named. There are, of course, some minor adjustments - for example, the biduality theorem that I had worked out in the fifties[10] (**) has been renamed "Verdier duality" for the occasion, still the same Verdier, there's no mistake. ... [11](***). However, it has not been possible for my name not to appear at least implicitly, through occasional references to texts that are still irreplaceable (despite SGA $4\frac{1}{2}$, which is not quite sufficient for its purpose), namely EGA and SGA. (In the explanation of the acronym SGA = Séminaire de Géométrie Algébrique du Bois Marie, my name of course does not appear, but in EGA, honest or not, the full designation is given, with the names of the authors including mine... .) Another detail that struck me, and which testifies to the obsessive strength of the burial syndrome (in someone who, however, has no obsessive "profile" whatsoever): the two references I saw to SGA make a point of explaining each time especially "Mr. Artin's theorem in SGA 4. ... . ", lest the misguided reader get the idea that said theorem might be due to the carefully non

[6](******) See notes n° s 51,52,59.

[7](*) I'm thinking of two other "operations" along the same lines, which took shape with the publication of LN 900 (see previous b. de p.) and APG $4\frac{1}{2}$ five years earlier (see notes n° s 67, 67', 68, 68').

(May 9) For a third such operation, closely related to the previous ones, see the "Good references" note (n° 82).

on another "memorable article", this time by J.L. Verdier.

[8](**)Nor have I ever heard of such a thing in the history of any other science or art than mathematics.

[9](*) (May 4) And the others too, of which I have since become aware.

[10](**) The same goes for the theory of dualité étale, which becomes "dualité de Verdier" under the pen of his generous friend Deligne!

[11](***) (May 5) Compare notes n° s 48', 63" . Throughout this long Burial, which has been going on for nearly fifteen years, and throughout the discovery that the principal "anticipated deceased" has just made over the past month,

J.L. Verdier seems decidedly inseparable from his prestigious friend, who lavishes him with the wreaths of flowers that are de rigueur on this mournful occasion.

named, when it is quite clear that the presentation was indeed made, thank God, by a named author! (77)

All of this, it seems, is fair game in the "beau monde" today. Without indulging myself (and p. 289 it's not meant for that. ...) this guéguère is not really detrimental to the anticipated deceased, whose symbolic remains are thus left to the vagaries of this fairground, which I have been discovering with wonder for barely two weeks. It doesn't gnaw at my life with the feeling of **iniquity** suffered in impotence. It hasn't broken the joy and impetus that carry me to the encounter with mathematical things and those of the world around me, nor has it burned the delicate beauty of these things in me. I can consider myself happy, and I **am**... .

And I'm happy too about my unexpected "return", the meaning of which had escaped me. If it were to teach me only what I have learned in these past days, this return will not have been in vain, as it has already fulfilled me (⇒ 76).

15.1.2. The symposium

Note 75 (June 3) I have received details of the other participants in the colloquium, which dispel all doubts. Although no talk by Mebkhout had been scheduled in the Colloquium's official program, Verdier was obliged to ask him on the spot and in extremis to give a talk, to make up for the shortcomings of one of the official talks (which had been entrusted to Brylinsk'i, who knew little about D-Module theory). Mebkhout was thus able to set out his ideas and results, and in particular the Good God Theorem, in such a way as to leave no doubt as to the authorship of this theorem, and of the philosophy that goes with it, which had led to the spectacular revival of the cohomology of algebraic varieties, culminating in this Col- loque. So, **all the participants in the colloquium were made aware of this paternity**, through this presentation. I also assume that all of them, without exception, have since been acquainted with the Colloquium Proceedings, and in particular with the Introduction and the cited article by Beilinson, Bernstein and Deligne. Not a single one, apparently, found anything wrong with it - or if they did, they didn't let on. Zoghman Mebkhout received no such feedback. So, all the Colloquium's participants can justifiably be considered to be in solidarity with the mystification that took place during the Colloquium.

This collective mystification was already clear at the Colloquium, since no one found anything wrong with the fact that, in Deligne's oral presentation on "perverse" beams, the name of Mebkhout is not pronounced. The speaker confined himself to stating the good Lord's theorem, saying that he wasn't going to demonstrate this in his talk. He made it clear, moreover (with the modesty with which he p. 290 is accustomed to) that "there was no merit" in guessing the extraordinary and a priori unpredictable properties of the beams he calls "perverse", obviously suggested by the "Riemann-Hilbert correspondence" he had just mentioned[12] (*). Everyone found it normal that he should refrain from naming the person who had had the "merit" of discovering this providential correspondence, and that he should give the appearance that the author was none other than himself, even though they had just learned, or would learn in the following days, that this was not the case. It must have been some sort of inadmissible misunderstanding that a vague participant in the Colloquium should be the author of such a remarkable theorem, and everyone did their utmost to rectify the situation and establish a consensus which attributed authorship to the one who was clearly the right person for the job - the one who **should have** been the author[13] (**).

[12](*) Compare with pages 10 and 11 of the article quoted.

(June 7) For details on the art of escamotage, see the following note "Le Prestidigitateur", n° 75".

[13](**) (June 5) everything fits together! The reflection that continued in the "l'Elève" procession (following on from the "Le Colloque" procession), and a certain tone too (notably again in a recent and brief exchange of letters with Deligne, see first footnote to the note "Les obsèques", n° 70), show me that for Deligne and my other cohomologist students, it's clear

Characteristically, **Mebkhout's paper does not appear in the Colloquium proceedings**. Verdier had asked Mebkhout not to write his paper, saying that the Colloquium was intended to present new results, whereas Mebkhout's had already been published for over two years.

When you don't get bogged down in a technical discourse, and look at what's actually been done, you'll see that it's not just a question of the technical.

p. 291 the forces and appetites that animated uni 'and the others, you'd think you were watching a film about mafia rule in the underworld of some far-off Megapolis, It's

The actors are among the noblest jewels of French and international science. The Grand Chef, who runs the operation with his finger on the pulse, is none other than the man who once looked to me like a modest, smiling spiritual son, or at least a (no less modest and smiling) legitimate heir. As for the one who can be drilled and cut, the "soft" one in a world of "hard" ones who don't give quarter, by a strange "coincidence" whose meaning I still don't fully grasp, he too is closely linked to me. He's my "pupil" as is the Great Chief (and like him, "pupil" with quotation marks...) - the one who took me on when I'd already been declared dead and buried years ago... .

15.1.3. The conjurer

Note! 75" (June 7) The "memorable article" (referred to in the previous two notes) displays a consummate art of casual evasion. The equivalence of categories that has been the essential motivation of the whole work is introduced for the first time in a sentence in the fourth rage of the Introduction (page 10, lines 9 to 15), without giving it a name, only to be followed immediately by the kyrielle of consequences for the notion of the so-called "perverse" bundle (pages 10 and 11). No further mention is made until the end of page 16, when we read[14] (*):

"We would like to point out that on the following points, **which would have found their place in these notes**, we have failed in our task.

- The relationship between perverse beams and holonomic modules. As mentioned in this introduction, it has played an important heuristic role. The essential statement is 4.1.9 (**not proved here**). . . "

(To continue with other "points that would have found their place. . . ")

I hasten to find out what this "essential statement" is that the authors haven't found the leisure to include in their work, or at least not to demonstrate. Let's look for it:° 4.1.9. ... I'm looking for an "essential statement", a theorem in the form of a scholia, with a reference **where** the authors have proved it or will prove it, since they don't prove it **here**. . .

p. 292 But no matter how hard I look, there's no trace of a "theorem 4.1.9" - there's only one passage that answers the number 4.1.9- So I start reading the "remark" at random (without conviction - there must be a mistake of numbering. ...), I read that "the analogue of 4.1.1 in complex cohomology is true... . "Unfortunately, I'll have to go back to 4.1.1 to find out what it's all about. I skipped over it and skimmed through the text that followed - and lo and behold, I couldn't believe it, eleven lines later, a sentence that starts with "We know that... . ." and ends with "induces an equivalence of the category . . with that of perverse beams".

Phew - so that was it after all! But no matter how hard I looked, I couldn't find the slightest hint to clarify that cryptic "We know that... . ". Readers who didn't already "know" it must be feeling pretty silly, not to the

it's been a long time since Deligne should have been the one to discover and master staggered cohomology; and at a certain level (that which commands behavior and attitudes), they're convinced that it's really him, next to whom I'd be a sort of clumsy, clumsy auxiliary who would be more detrimental than anything else to the harmonious unfolding of a theory (leading to Deligne's theorem-ex-Weil's conjectures) and to a distribution of roles satisfactory to all concerned. . .

[14] (*)Emphasis added.

the situation. What's clear to him in any case (apart from the fact that he's not up to it), is that this result "which would have found its place in his notes", which is "recalled" here in the course of a technical remark - something the reader should know anyway - is obviously due to the authors of the "notes" in question, or to one of them; the most prestigious perhaps and who wrote the article (there's an unmistakable "house style" .), or the one who gave the oral presentation, whose well-known modesty prevents him from saying "it's me! - but everyone understood without having to say it...

It immediately brings back memories of my reflections over the last few weeks. The very first is Deligne's first work in 1968, which I finally (sixteen years later) took the trouble to look at a little more closely in the note "L'éviction" (n° 63) of April 22 (three days after the discovery of the pot-aux-roses LN 900). Here I find the same style, with variations no doubt due to the intervening thirteen years of "breaking-in". In the 1968 article, whose main inspiration came from me, he names me in passing and in a sybilline way towards the end of the article, just to be "in order". Here, he no longer takes such care - experience has long since shown him that there's absolutely no point! On the other hand, in the article of his young age, since he felt obliged to name me, he compensated by entirely retracting the initial motivation for his work (and the yoga of the weights with it, only to release it under an alternate paternity six years later, while awaiting the exhumation of the motives eight years later still... .). In any case, even hiding (and keeping for his own benefit. ...) the article's essential arithmetical motivation, it "stood up," this article was perfectly understandable, living up to the author's reputation for doing things perfectly. p. 293
Here, the theory he develops would be incomprehensible without the heuristic motivation. So he points to the latter, referring to it as "the essential statement", while treating it from under the leg - without honouring it with a name, or a formal statement baptized theorem or proposition, there isn't even a "correspondence" (known as Riemann-Hilbert) - he left that to his friends Verdier and Teissier. He doesn't have to give it a name (given the few[15] (*) - surely he'd demonstrate it in five minutes!) or name anyone - others will take care of that for him and to his complete satisfaction. There is clearly a yoga, a philosophy, that the author handles with perfect mastery and authority, without having to name anything - this "little" that he pretends to disdain ("which would have found its place in these notes"), he knows full well he'll get more of, as long as he knows how to keep quiet and wait. The first time he played this game successfully, the "few" were "weight considerations" alluded to in a sibylline remark (waiting to bring out the philosophy of weights with great fanfare, six years later). The second time, as far as I know, was when I left in 1970 - the "little" was the "dream of motives", which for twelve years didn't deserve to be honored with a word (just think - a dream, and a dead man's dream at that, not to mention unpublished!), while we wait to discover the real motifs this time (and what we can do with them) and to claim, as modestly as ever, undisputed authorship[16] (***).

15.1.4. Perversity

Note 76 (May 4) I well remember the first time I heard the name "faisceaux pervers", must be two or three years ago, that it struck me unpleasantly, arousing in me a feeling of unease. This feeling reappeared the two or three times I heard this unusual name again. There was a sort of inner "recoil", which remained close to the surface of my consciousness, and which would undoubtedly have been expressed (if I had stopped to examine it) on the other occasions.

[15](*) (June 14) To put this "little" in context, I'd like to remind you that Deligne devoted a seminar at the IHES to trying to develop a translation of constructible discrete coeffi cients in terms of continuous coeffi cients, without arriving at a satisfactory result. On this subject, see the note "L'inconnu de service et le théorème du bon Dieu", n° 48'.

[16](**)For further comments on this technique of "appropriation through contempt", see the following day's note, n° 59'.

then) by something like: what an idea to give such a name to a mathematical thing! Or even

p. 294 any other thing or living being, except in a pinch a person - for it is obvious that of all "things" of the universe, we humans are the only ones to whom this term can sometimes be applied. ...

It seems to me (although I'm not entirely sure) that it was none other than Deligne himself who first spoke to me about so-called "perverse" beams, when he dropped by my place after the Colloque de Luminy[17] (*). It must even have been one of the last mathematical conversations between us - there were no others after his visit. It was during this very visit that this "sign" appeared, which led me a few weeks or months later (while this sign was being comforted in the exchange of mathematical letters that followed this encounter) to put an end to a communication on the mathematical level[18] (**). (For this episode, see the note "Two turning points", n° 66.)

Coming back to the so-called (wrongly i) "perverse" beams, it's obvious that "normally", these beams should have been called "Mebkhout beams", which would only have been fair. (On more than one occasion, I've named mathematical notions I've worked out and studied after predecessors or colleagues who were much less closely associated with them than Mebkhout was with this beautiful notion - which, incidentally, would seem to me to be more "sublime" than perverse!) The circumstances in which Deligne found himself at the time he was discovering and naming this notion derived from Mebkhout's work, preparing to rob him when he himself was already "fulfilled beyond measure" - these circumstances can rightly be called "perverse". Surely my friend himself must have felt it in his innermost being, at a certain level where one is not fooled by the facades one likes to flaunt. I sense in the attribution of this name (which seems aberrant at first sight) an act of **bravado**, a kind of drunkenness in a power so total, that it can even allow itself to display (symbolically, by the display of a provocative name whose true meaning no **one** will allow themselves to read!) its true nature of "perverse" spoliation of others.

p. 295 It seems by no means impossible that at some deep level, I perceived the tone of these dispositions in my friend, and that this contributed to the unease I mentioned[19] (*). This uneasiness was expressed in particular by my inattention to the explanations he had to give me, although I don't think there had been an occasion before this meeting when I hadn't followed what he was telling me with sustained attention, and especially when it concerned mathematics. There was a kind of blockage in me with regard to this notion called (God knows why) "perverse" - I didn't really want to hear about it, even though it was very closely linked to issues I was (and still am to some extent) very close to.

In fact, the whole article by Deligne et al. was typical "**grothendieckery**" and all

[17](*) If this is indeed the case (as I'm now convinced it is) I must give credit to my friend's modesty, for I had no idea (on a conscious level at least) that it was none other than he who had introduced and named them. I had to read the "memorable article" to realize this.

(May 28) To tell the truth, the article doesn't say this any more than it says that Deligne is the father of the Riemann-Hilbert correspondence. However, I had no doubts about his authorship of the term "faisceaux pervers", which was subsequently confirmed to me.

[18](**) On a purely personal level, this relationship continued in the same tone of affectionate friendship as before, with no apparent change. My friend used to come every other year or so to visit me, usually on some kind of hike. I did have a visit again last summer, which was a welcome opportunity to get to know his wife Lena and their infant daughter Natacha. I think it was on the way back from yet another Colloque de Luminy, about which I've heard very little (apart from a few vague, morose allusions from Mebkhout, who had been given the honor of being invited again, and who could think of nothing better to do than to get back into the game... .). They stayed at my place for two or three days, and the contact was excellent all round.

[19](*) I would even be inclined to think that this is indeed the case. On more than one occasion, I've been able to see for myself the extent to which the deepest perception of things is of a fi nesse and acuity that have no comparison with what skims the surface at the conscious level. The fully "awakened" man is undoubtedly the one in whom these perceptions are constantly integrated into conscious vision and conscious experience - the one who lives fully according to his true means, and not just on a paltry portion of those means.

that could just as easily have come from my pen (with the sole exception of the name of the main concept)! It's something I've already expressed in the second part of the previous note (n° (75)), and something I've also sensed from the moment I read the article quoted - but without this diffuse feeling yet being embodied in the striking observation I've just made. It makes me aware once again, in a striking way, of this profound contradiction of the person who cannot help (in a certain sense) to reproduce and assimilate the very one it is a question of denying, of handing over to disdain - the one it is a question of burying, and who is also at the same time the one one **wants to be** and that (in a certain sense) one **is**. p. 296

The day before yesterday, as I was writing the previous note ("l'Iniquité - ou le sens d'un retour"), I had already been struck by the coincidence that this turning point in the relationship between my friend and me, suddenly impoverished of a communion in a common passion, which had been its raison d'être and most powerful mainspring, took place on my friend's return from that memorable Colloque, the meaning of which had just revealed itself to me. What had puzzled me at our meeting in July '81, which on one level was as friendly and affectionate as on the other occasions we met, was this "sign", discreet in tone and air, yet brutally obvious, of a deliberate gesture of disdain. It was like a sort of **down payment** that my friend was making, this time at the level of a personal relationship, on the implicit and equally "discreet" (and just as "brutally obvious") disdain that he had just publicly expressed towards me, as a public figure, at the Colloque de Luminy, in the context of a brilliant display of technical virtuosity between the stars of the day. It was the same "disdain" that had just been expressed (but this time with an altogether different "perverse" brutality) towards the man who had dared (even a little) to claim to be me, and who had thereby condemned himself to be, for my friend Pierre (at a certain level at least), nothing more than "another Grothendieck"[20] (*) who had to be crushed at all costs. . .

15.1.5. Pouce!

Note 77 (May 5) I was struck by another detail while reading this memorable paper[21] (**), which dominated (so they say) the no less memorable Colloque de Luminy in June 1981. The last chapter, under the suggestive title "From F to C", describes at length a remarkable principle I had introduced into geometry twenty years ago - it must have been even before the birth of the notion of motif (which in gives the most deep illustrations, via Weil's ex-conjectures). This principle ensures that for some p. 297
In the case of statements concerning schemes of finite type over a body, it suffices to prove them over a finite base body (i.e., in a situation "of an arithmetical nature") to deduce their validity over any body, and in particular over the body of complexes - in which case sometimes the algebraic-geometric result envisaged can be reformulated by transcendental means (e.g., in terms of integer or rational cohomology, or in terms of Hodge structures etc.)[22] (*). My friend learned this from none other than me and from me, on numerous examples over the years[23] (**). The authorship of this principle (which in an elementary form is even spelled out in EGA IV - don't ask me which paragraph and which number... .) is well known[24] (***). So much so that

[20](*) In our personal relationship, my friend calls me by the affectionate diminutive (of Russian origin) of my first name, Alexander, which is also what my family and closest friends have called me since childhood.

[21](**) See note n° 75 about the "memorable article".

[22](*) (May 6) It seems to me that the first example of the use of such a principle is to be found in Lazard's theorem on the nilpotence of algebraic group laws on the affine space *E* (over any body). I was struck by its demonstration, and drew inspiration from it for a number of other statements, as well as for a "philosophy" that has dominated my thinking on pattern theory.

[23](**) See the note "Eviction" (n° 63) for one such example.

[24](***) (June 5) It is perhaps abusive for me to claim to be the "father" of a principle whose first known application is due to Lazard (see previous note (*)). My role, as on other occasions, has been to sense the generality of someone else's idea,

When my brilliant friend was awarded the Fields Medal at the Helsinki Congress in 1978, N. Katz couldn't resist mentioning it in passing in his speech in honor of P. Deligne, thus rectifying a somewhat embarrassing systematic "oversight" on the part of his illustrious laureate. I read this speech just a few days ago, along with the "memorable article" itself.

In any case, in this article, the philosophy behind the transition from "arithmetic" to "geometry" is presented in such terms that there can be no doubt in the mind of an uninformed reader that the brilliant lead
p. 298 author (excuse l'impair. . .) has only just discovered this wonderful principle of such far-reaching significance.

It's true that I haven't patented the method, and nowhere does my brilliant friend say that he's the brilliant inventor; nor does he claim in plain English that he's the father of that famous "correspondence" (admire the term, which smacks of his nineteenth century!) modestly attributed to Riemann and Hilbert (men worthy of sponsoring the children of such a prestigious successor) - nor does he specify in the "memorable volume" (LH 900) that it was indeed he who invented motives, motivic Galois groups and the whole philosophy that goes with them (and of which he has still only released a fragment). There's nothing to say either for this famous SGA $4\frac{1}{2}$, where I've even been honored to be listed as a "contributor" to this volume, which so brilliantly develops ab ovo étale cohomology, deigning to call on (despite their regrettable gangue of superfluous details etc.) the two satellite volumes SGA 4 and SGA 3.) to the two satellite volumes SGA 4 and SGA 5, which have been consigned to oblivion, but to which I am generously credited with providing a few technical additions and digressions (some of them even "very interesting")[25] (*).

In all these cases, and in many other micro-cases that I've witnessed over the last five or six years, without the idea ever occurring to me to **pinpoint my discomfort** and give a name to what I witnessed or co-acted[26] (**) - in all these cases, I recognize the same **style**. My friend is always and totally "**thumbed**" - he can help himself at ease, with the complete good conscience that comes from admiring his peers and his blunders (with all due respect), guaranteeing total impunity.

15.1.6. The Chinese emperor's robe

p. 299 **Note** 77 (May 7) Of course, those who see what my friend Deligne is doing and are in the know at all for the ins and outs, I mean those who haven't just learned about the maths "being done" from the publications of the person concerned himself, or other brilliant (though not always golden) stars of his generation - these colleagues (and they're not that rare after all!) are well aware, at **some level**, of what's going on. They must have sensed, in the "big" cases, that particular uneasiness that I myself have felt on more than one occasion in the face of these "micro-cases" a hundred times less serious than the "big" ones.

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and systematize it to the point of making it a "reflex" or "second nature". In the context of the yoga of weights and patterns, it's likely that the first to use this principle was Serre (not me), with his idea of virtual Betti numbers, which set me on the path to just such a general yoga of weights and patterns. (See note no.° 46_9 for Serre's idea in question.) It's also true that it's common practice to attribute the authorship of a "principle" of reasoning that has become commonplace, not to the author where we first find a trace of it, but to the person who first perceived its general scope, systematized and popularized it. In this sense, N. Katz's correction (referred to in the following sentence), attributing the paternity of this principle to me, is justified.

[25](*) For details of the "SGA AT operation", see the four notes "La table rase", "L'être part see the four notes "La table rase", "L'être à part", "Le Feu vert", "Le renvers- ment" (notes n° s 67, 67', 68, 68').

[26](**) The first step towards precisely "pinpointing my malaise" in a specific case was taken in Harvest and Sowing less than three months ago, in the reflection (which turned out to be quite laborious - and for good reason°) "The note, or the new ethic" (section 33). This reflection was taken up again in a note to that reflection, "Le snobisme des jeunes, ou les défenseurs de la pureté" (note n° 27), then again less than two weeks ago (under the impact of the discovery (the day before) of the "memorable volume" (LN 900)) with note n° 59: "La nouvelle éthique (2) - ou la foire d'empoigne". As I was writing this, there remained in me a tinge of hesitation about using the rather blunt term "jumble sale". The discoveries I've made since then have shown me that there's no need for hesitation.

bigger than these. But what they sensed was so **enormous**, so **incredible** that it must never have surfaced - as it finally began to surface with me, in the course of a **work**, which expressed itself in these two texts around a micro-case referred to in the previous b. de p. note. Indeed, I've never heard of anything like it in the history of our science or any other. Instead of "surfacing", for some people "it" must have **become the norm**, or at least been considered **normal** - as long as an obviously brilliant man, admired by all, practised it with the greatest naturalness in the world, in full view of everyone and without the thing ever (as far as I know) eliciting the slightest comment.

Over the past few days, I've been reminded many times of the tale "The Dress of the Emperor of China", in which the aforementioned emperor, deceived by unscrupulous swindlers and his own vanity, announces that he will appear in a solemn procession wearing the most sumptuous garments the world has ever seen, prepared for him at great expense by so-called tailor artists. And when he appears in the procession, surrounded by the pomp and circumstance of his Court in full regalia, the "artists" bowing and scraping, and the entire imperial family, no one in the procession or among the people gathered to contemplate the seventh wonder dares to believe the testimony of his eyes, and everyone makes a point of admiring and raving about the unsurpassable splendor of the garments with which he is now adorned. Until a small child who had strayed into the crowd exclaimed, "But the emperor is naked!" - and then all of a sudden the whole crowd, as if with one voice, cried out with the little child: "But the emperor is naked!

And I feel like the little child who believes the testimony of his eyes, even though what he sees is quite unheard of, never seen before and ignored and denied by all.

Whether the child's voice will be enough to bring some back to the humble testimony of their healthy p. 300
faculties, that's another story. A tale is a tale, it tells us something about reality - but it's not reality[27] (*).

15.1.7. Encounters from beyond the grave

Note 78 (May 6) It's only been five days since I received this generous package of documents from my friend Zoghman Mebkhout, including above all the two texts already examined from the "memorable Colloquium" - that Colloquium built around a monumental **mystification**! The note "l'Iniquité - ou le sens d'un retour", in which I try to assimilate the quite incredible meaning of this new "event", was written on the very day (the day after May 1st) that I received these documents, still in the emotion of discovery[28] (**).

Since April 19, when I finally became aware of the "memorable volume" of the Notes readings (LN 900 - see notes (51) (52)), this has been the third great discovery on the subject of the solemnities of the Great Burial, and the one that seems to me to be of the greatest significance, both in terms of the light it sheds on the actions of the "Great Burial".

[27](*) (June 14) After writing this note, the name "The robe of the Chinese Emperor" struck me as a natural sub-title for the Burial, expressing a particularly striking aspect of it. Later, as the focus shifted to my students as a whole, and even to "the entire congregation" of the Mathematical Establishment, this subtitle seemed less appropriate. However, I've come to realize that the parable that first came to mind when thinking of my friend Deligne, applies equally to all aspects and adventures of the Burial, which at every step reach the Ubuesque in the unbelievable (which everyone makes a point of modestly ignoring) that is nonetheless true. For reflections along these lines, see in particular the notes "On n'arrête pas le progrès!", "Le Colloque", "La Victime - ou les deux silences", "La plaisanterie - ou les complexes poids", "La mystifi cation", "Le Fossoyeur - ou la Congrégation toute entière" (n° s 50, 75', 83, 85', 97), none of which specifically concern my friend Pierre.

[28](**) Along with the section "The note - or the new ethic (1)", this note is the only note or section I've had to rewrite several times, because what "came out" in the first version (and even in the next one) was still burdened by the inertia of my usual vision of things, which fell far short of the reality I was examining.

of people to whom I have been closely linked, than by its implications as a "tableau de moeurs" of an era, apparently unique (but it is true that I am ignorant of history. . .).

p. 301 The second discovery had closely followed the first - that of the exhuma□tion of the "motifs", for twelve years buried. After the "memorable volume", I was treated to the "memorable seminar" - that "seminar" that never took place, given a bogus name (both SGA and number 4 1/2), and enriched with the "State 0" of a phantom thesis, not to mention a central presentation from the (real) SGA 5 seminar (which appears later, even though it predates it by twelve years); a presentation "borrowed" for the purposes of the operation without further ado. This brilliant operation, and the role it played in the strange vicissitudes that befell this poor SGA 5 seminar (dismantled from the head, the tail and the middle!) were gradually revealed in the course of a reflection that continued between April 24 and 30. (See the five notes "Le compère", "La table rase", "L' Etre à part", "Le signal", "Le renversement", n° s 63'", 67, 67', 68, 68'.)

As soon as I had digested this discovery, and as my retrospective reflection on "Mon ami Pierre" drew to a close, and on April 30 I had proudly put the final and definitive mark (that was a sure thing!) on my life, I decided to take the plunge.
- this time I was finally there i) under this interminable Burial, with the "final note" with the doubly euphoric name "Epilogue - ou l'Accord Unanime" - that I receive this package of misfortune, which calls into question final point, epilogue, page layouts and numbering... . A quick glance at the documentation and the accompanying annotations and letters made it clear that my period was gone, as were the beautiful arrangements for a first-class Funeral, the final details of which I was about to polish - I was ready to take up the master of ceremonies' harness. . .

God knows my friend Zoghman had plenty of time to inform me of the situation! It must have been going on for ten years in latent form, and three years at least in "acute form" (and that's putting it mildly) - ever since the Colloquium in question, where he must have sensed the wind without having to wait for the publication the following year of the highly official "Proceedings" under the patronage of his illustrious ex-patron and protector.

A few months after he had defended his thesis (in February 1979), he had come to bring me a copy to the
p. 302 village where I had lived for six years. Unluckily, I had just left (never to return).
return, except in passing...) a few days before, to retire in solitude. He only met my daughter, who later handed me the thesis. It was the following year, I think□that we finally got to know each other, in college.
in Montpellier, where we chatted for an hour or two. I wasn't really into math at the time, and couldn't remember much about the thesis I'd flipped through in a few minutes, or the name of its author. That didn't stop the contact from being warm. I remember an immediate current of mutual sympathy. We didn't talk so much about maths (not that I can remember), but mostly about more or less personal things. Zoghman told me afterwards (something I'd forgotten) that he'd been able to explain the D-Modules "philosophy" to me a little, and that he'd been pleased with the meeting, to have felt me "vibrate" if at all by learning new things from him, and yet also (in a way) "expected". What I remember most of all was the impression he made on me - an impression of stubborn, calm strength, that of a "go-getter". At the time, much more than when we met last year or during the correspondence that followed, I had the impression of a strong affinity of temperaments - this "go-getter" side in particular. But the two or three years that have elapsed between the two encounters seem to have dented it quite a bit. ...

I don't remember Zoghman telling me at our first, brief meeting about the isolation in which he had worked, the lack of any encouragement from the "luminaries" who had been my students. If he hinted at it, he must not have insisted. Even then, the whole thing didn't appeal to me.

surprise[29] (*). I couldn't say whether this was before or after the Colloque de Luminy in June 1981[30] (**). If it was afterwards, he would still have had some hot stuff on his stomach - and he really didn't give the impression of it. Rather that of a man who knows what he wants to do and what he wants, and who follows his instincts.
quietly, without seeking trouble and without being sought out.

We didn't continue then to write to each other. But I remembered him well, and early last year I wrote to him p. 303
I wrote him a note, at random, to ask if he might be in a position to tackle a magnificent work on the foundations of a "moderate topology" which (it seemed to me) was just waiting for someone of his calibre to take it up. Although Zoghman didn't make it clear to me at first, it turned out that he wasn't really interested in this prospect - on the other hand, he seemed happy to seize the opportunity of a new encounter. At the time, I was too out of the loop to fully appreciate the situation, and imagined that D-Module theory was now a done deal, as is, say, coherent duality theory (78_1), and that Mebkhout had perhaps run out of "big tasks". It was only when we met last summer that I realized that in the very theory he had started, there was no shortage of "big tasks" - and some of them had not even been started, because they had not even been seen!

In any case, it was a perfect opportunity for a second meeting, and this time not as casual as the first. Zoghman must have stayed at my place for maybe a week last summer, in June I think. Mathematically speaking, our meeting served mainly to bring me up to speed as best we could on D-Module yoga. I've been slow to "thaw out", having lost touch with my old cohomological loves, and being mostly embroiled in the writing of "Poursuite des Champs", which is set in rather different registers. Zoghman wasn't discouraged to see me listening with a slightly distracted ear, and returned to the charge without tiring, with a touching patience. I was finally triggered, I think, when I realized that these famous D-Modules were nothing other than what I had long ago called **module crystals**, and that as such they still made sense in singular spaces. All of a sudden, I saw a whole network of intuitions from my crystalline-differential past rising up from forgotten depths, and slightly rusty reflexes from my "six operations" past being reactivated...

Perhaps it was Zoghman who was a bit of a loose cannon, or maybe it was more that he decided afterwards that he wasn't going to risk his fingers in that particular gear (any more than my friend Pierre wanted to put his - although he'd been all fire and brimstone while I'd been around... .). (⇒ 78')

Note 78_1 There are, however, a number of "fine" results of consistent duality, notably on the structure of "dualizing differential modules", their relation to "naive" differential modules, and trace and residue applications in the non-smooth flat case, which I had developed in the late fifties p. 304
which, to my knowledge, have never been published. Nevertheless, for the most part, the theory of coherent duality (in the schematic framework at least), just like that of stellar duality (and its variant for the discrete cohomology of locally compact spaces, developed by Verdier on the stellar model), or linear algebra or general topology, appear as theories that have essentially **been completed**[31] (*), in the nature of **tools that are** perfectly perfected and ready for use, and not so much of a **substance.**

[29](*) (May 30) That's not quite true - I'm reprojecting more recent disillusioned dispositions onto the past. When I met Zoghman just last summer, I remember being surprised that none of my cohomology students (Deligne, Verdier, Berthelot, Illusie in particular) had supported Zoghman in his work. This surprise was repeated when Deligne came to see me ten days later (I must have said something to him about Zoghman, but I got no response), and later in a telephone conversation with Illusie (see the note "La mystifi cation", n° 85').

[30](**) (June 3) That was back in February 1980, a year after he defended his thesis.

[31](*) (June 12) This is not quite true for stale duality, until the purity conjectures and the "biduality theorem" are proved in all generality.

that would have to be penetrated and assimilated.

15.1.8. The victim - or the two silences

Note 78 We met in an atmosphere of friendly trust and affection. This atmosphere, however, did not live up to its promise. I realize now that from that moment on, my friend's trust was far from complete. It was two years after the famous Colloque, and a year after the publication of the "Actes" in Astérisque[32] (**)-at a time when he was the victim of a scandalous spoliation.

But he didn't bother to inform me until just four days ago! When he came last year, he was returning from
p. 305 another Colloque de Luminy[33] ('***) (this time squarely on the theme of D-Modules)' où où on
which he had again generously invited and rushed to attend. He spoke of it in terms both bitter and vague, suggesting that now that he'd pulled the chestnuts out of the fire, it was "the others who had done it all". I could imagine the picture indeed - especially Verdier suddenly remembering the paternity of the triangulated categories (and derived ones too, while we're at it!) he had left to one side for ten or fifteen years, barely tolerating his "pupil" Mebkhout's use of them in his work... . (81).

Although he didn't want to explain himself clearly at the time, Zoghman seemed to have his heart set on Verdier, which was understandable given his ex-boss's less-than-encouraging behavior. And yet, my other cohomology students - Deligne, Berthelot, Illusie - hadn't bothered to take an interest in what he was doing or to support him in any way. But it almost seemed as if Zoghman took this for granted, having never (or so it seemed) experienced anything other than this attitude among his elders. If he held a grudge against any of my former students, it was solely and exclusively against Verdier.

From Zoghman's hints (which he obviously didn't want to spell out), I understood that "they" were systematically putting the scope of what he'd done - period. This is, after all, the most common thing in the world. Since judging the importance of a thing is largely subjective, it's commonplace and almost universal to attribute more merit and importance to one's own work, to that of one's buddies and allies, than to that of others, and especially to those one feels like minimizing for one reason or another. (And the "reason" in this case wasn't exactly a mystery to me!) Nothing could have made me suspect that, far beyond such common attitudes, there was here an operation of pure and simple swindling, where there was no question whatsoever of "minimizing", but rather of **swindling** Mebkhout's authorship of the ideas and results that were breathing life back into where there had been stagnation... .

And yet, if there was one person in the world to whom it was natural for my friend to open up, it was me. whose work had inspired him during those years of obstinate work, sometimes bitterly, against the fashion
p. 306 of the day - I who received him affectionately in my home, making myself a bit his pupil at my
learning as best I could what he took pleasure in teaching me[34] (*).

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[32](**) (October 9) Zoghman tells me that these "Actes" were not published until early 1984.

[33](***) (May 7) There's a slight memory lapse here - I think he was getting ready to go to the Colloquium. At the time, of course, there was no shortage of reasons for those "bitter terms" (and vague ones) I remembered. But this bitterness was further heightened by his visit to Luminy after his stay with me. I had echoes of it in a phone call he gave me on his return from Luminy. From that moment on, I had the distinct feeling that he had come to Luminy for the pleasure of being mistreated by "the people" (without really asking me which ones) who had generously invited him, for the pleasure of being able to treat him as a negligible quantity. I must have told him so, or let it be understood, which couldn't have improved my friend's attitude towards me.

[34](*) Zoghman didn't tell me about mine, and he didn't tell me about his own funeral either, even though he'd had a front-row seat to the proceedings for nearly ten years! To tell the truth, his "protectors" (a little reluctant on the edges) had even agreed to let him carry with his hands a small corner of the coffin carrying my remains - but they couldn't forgive him for being the only one among the guests who sometimes took the liberty of uttering the name that all the others kept quiet!

So my friend must have felt at odds in his relationship with me, and he couldn't find it in himself to take on the responsibility.

After my friend had passed through an atmosphere of warm affection, there was an immediate "backlash". I had the impression that he had decided to transfer onto me the mistrust and bitterness that had built up in him over the past eight or ten years, under the sting of the indifference and disdain he had encountered in some of my former students. In the months that followed, the correspondence between us never left the aigredoux register - it finally stopped with a New Year's greetings card, which never received a reply.

It was only at the end of March that I contacted Zoghman again, to send him "Le poids d'un passé" and the notes I had then added to this section (n° s 45, 46, 47, 50). It was to ask him if he would agree to my including him, as I had done, in the short reflection on my work (in the note "My orphans", n° 46), when it would be clear to all that I was using information he had given me, and which he might consider confidential. I was by no means sure that my friend would not prefer (like others before him) to "crush rather than displease". It would have hurt my feelings if he had.

It took me a long time to get her reply, which I received only ten days later. I was somewhat expecting her to would still be half-flesh, half-fish - but this time she was downright warm. He'd give me his p . 307
I agreed wholeheartedly, even emotionally, with the terms in which I spoke of him.

It's on page 6 of his long (eight-page) letter that he points out, as if in passing and with reference to the "impressive number" of applications of his theorem ("both in the framework of stale topology and in the transcendental framework") that it still appears in the literature under the name of the "Riemann-Hilbert correspondence"[35] (*). He says it in such an almost incidental way, and with such a delightfully illegible handwriting, that it almost went completely unnoticed! But then I remembered, it really was a strange thing. So strange, in fact, that it hardly seemed believable, and then perhaps my friend was exaggerating, obviously he was angry with everyone, including me, even though I only wanted good things for him. So I added a note (holy Zoghman, I thought I'd finished!) called "L'inconnu de service et le théorème du bon Dieu", in addition to two others "L'instinct et la mode - ou la loi du plus fort" (I'd also thought a lot about him, among others, when writing it) and "Poids en conserve et douze ans de secret". This note on "L'inconnu de service", I wrote at first without total conviction; Zoghman seemed to me so knotted up and full of contradictions that I wondered what I was getting myself into by simply echoing him, without knowing the facts for myself. The thought hadn't occurred to me that there might be a scam, let alone that Verdier or Deligne themselves were involved. There was nothing in what Zoghman had told me to suggest this... .

Yet both of them were so closely linked to this theorem of the good Lord, that its authorship could hardly be concealed without at least their tacit agreement. It must have worked on me in the days that followed. I remembered that Deligne had given it a lot of thought, this problem solved (ten years later) by Zoghman - and then Verdier, after all, acted as research director; even if he didn't go out of his way for his pupil and would rather have beaten him cold and discouraged him than anything else, he
must at least have known what the two main theorems in this work were - Zoghman surely explained them to him, during those famous "interviews" that Verdier was kind enough to grant him! I have therefore added to the note on p . 308
a commentary on the relationship of Mebkhout's work to an earlier attempt by Deligne, and a b. de p. note on the role of Verdier. At the same time, it was also a sounding board for my friend

a past fraught (as mine was) with ambiguity, and speak to me plainly and clearly. Talking about his funeral also meant talking about mine and the role he himself had played in it ... . In any case, if I ended up discovering this famous funeral in all its splendor, it was against a kind of "conspiracy of silence" that encompassed both my friend Zoghman and my friend Pierre - and no doubt most of the friends I had in the "great mathematical world".

(June 3) For further details, see note n° !78" below.

[35](*) See the quotation from his letter in the note "A feeling of injustice and powerlessness", n° 44.

Zoghman...

You'd think that Zoghman would jump at the chance to finally, finally reveal his batteries, hidden for three years, which will finally bring out the clear truth and triumph the cause of the oppressed! But not at all! Fifteen days of silence, followed by a letter about everything (in maths) except God's theorem - or rather, he confined himself to giving me the precise reference in his thesis, which I had asked for. (I still wanted to know where this famous theorem, to which I was so firmly committed, had been proved!)

In my reply to this letter, I had to say a few words to him about the "vast swindle with regard to my work" I had just discovered (with the "memorable volume" LN 900, and moreover "promising me much pleasure" in the days to come in making the acquaintance of SGA 4[1] in the college library) - so that after another ten days' silence, my friend finally got in touch!

This time, at last, he "pulled out all the stops" - a **great deal**, in fact, of judiciously-chosen documents, enabling me (who hardly ever haunts libraries, or even the piles of separate prints piling up in my office at university... .) to give me a well-balanced idea of an "atmosphere", in which many of those who didn't take part in my long and solemn funeral still remain[36] (*). Alongside the main "piece of evidence" (the two articles from the famous Colloquium, exposing the incredible mystifi- cation), and another "memorable article" (this time from the pen of Verdier[37] (**)). there was the speech by N. Katz on the "Fields Laureate" Deligne, plus a presentation by Langlands and another by Manin at the same Helsinki Congress 1978; then Deligne's "Théorie de Hodge I" at the Nice Congress 1970 (where he is made

p. 309 another allusion in line 3 to a "conjectural theory of Grothendieck's motives" (78_1), and "Weight in the Cohomology of Algebraic Varieties" by the same Deligne, Vancouver Congress 1974 (where my name is not mentioned (78_2)); plus finally a correspondence with A. Borel (yet another old friend, whom I learn at the same time is back in Zurich. . .), and two notes to Mebkhout's CRAS, one of which from 1980 is a summary of Chap. V of his thesis (passed the previous year), giving a little more emphasis to the theorem of the good Lord[38] (*). Not to mention another document - shhh! communicated under the seal of secrecy, and of which I won't say another word here. ... .

Two letters accompany this substantial dispatch (letters of April 27 and 29), one very long and both substantial. Now that he's finally let the cat out of the bag (the real one, this time!), Zoghman continues to urge me to exercise extreme caution, as he had been doing ever since I contacted him again. If I listened to him, I'd be careful not to make public my reflective notes, which would remain a matter of absolute secrecy between him and me - at least not the part that implicates anyone, given that "they" have "all the power" and "everyone is with them"[39] (**)! And yet, I had warned Zoghman that these notes, from which I sent him the extracts concerning him, were destined to be made public, and as soon as possible.

All the elements seem at last to be in place for the just cause of the oppressed to triumph, but the "victim" seems to be doing everything in his power to continue muddying the waters as if by magic.

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[36](*) (June 12) Katz, Manin, Langlands don't seem to be among them. . .
(March 1985) For a different take on Katz, see "Dotting the I's", n° 164 (II5), and "Maneuvers" (n° 169), "Episode 2".
(April 1985) Similarly for Langlands, see note "Pre-exhumation (2)", n° 175 . 1

[37](**) For more on this article, see "Les bonnes références", n° 82.

[38](*) For a precise reference for this note, Mebkhout's thesis and the theorem of the good Lord, see the note "Le pavé et le beau monde - ou vessies et lanternes", n° 80.

[39](**) (May 30) Carried away by my impetus, I'm exaggerating a little here. At no time has Zoghman suggested that I refrain from publishing any part of my notes. Lately, he's even been insisting that these notes should indeed appear in book form, for the benefit of "posterity", whereas a limited edition like a preprint seems to him a bit "like a sword in the water".

secret regret (one would say) at having sold that famous "fuse" of which Zoghman must have been (until the fateful May 2) the one and only holder. This ambiguity is apparent in every line (I'm hardly exaggerating), right down to the latest letters I've just received - including the very last one, in which he sends me, with an air of sombre triumph, the "memorable article" in its entirety (whereas, with the "big package" he first sent me, he had only managed to part with the first twenty pages of this masterpiece[40] (***)).

As for the friend Pierre I mean Deligne (who is neither Pierre nor "friend" to everyone. . .), it's p. 310
it's just that he doesn't sing its emotional praises - it seems that it's no longer he, Zoghman, who's the "victim", but no, it's Deligne, poor fellow, who's been so badly influenced by those around him - the only villain, and the one who surrounded him so badly, is Verdier (and yet. . . follow my gaze instead, . .): it's clear that I "must have done something" to Verdier for him to be such a coward for the sheer pleasure of doing harm, not to mention the fact that I was also his boss and I was also the one who awarded him the title of doctor and the glory and all the rest - the means, in short, of "absolute power"![41] (*)

Clearly, if my friend has a grudge against anyone, it's not really against his illustrious ex-boss, whom he's only had the honor of meeting for an "interview" three times in ten years in all (if I've understood correctly what he wrote to me most recently) - a vertiginously distant man, entirely out of reach - but it's the one he can come and see whenever he pleases, and share both his bread and his lodgings... . [42](**).

Each time Zoghman takes a new step to divulge some new element, making me a little more aware of a situation of despoilment in which he is the victim (and can help a little to unravel it), I feel that it's like a **wrench**, the culmination of an exhausting inner struggle. I
has a **role** with which he seems to have identified body and soul, clinging to it as if it were his most precious
possession - a role with which he seems to have identified body and soul, clinging to it as if it were his most precious
possession.
this role of victim which he can only maintain by keeping around this role and the situation that justifies it, p. 311
absolute secrecy[43] (*). And he may indeed be torn, and resent me more than ever, at this moment when, with his reluctant collaboration (snatched away, as it were, by the logic of a situation created by none other than me, with those unfortunate reflections on an uneventful Funeral. . .), this secret will come to an end, and with it, perhaps, this role in which it has pleased him to maintain himself, for how long I cannot say.

This "burial" of my friend Zoghman was achieved by the combined care of **two silences**, each responding to the other and provoking it in turn, in a seamless round in which the role of one closely matches the role of the other - the despoilers and the despoiled. If on more than one occasion I was struck by the fact that the "burier" was at the same time, and more profoundly, his own "buried", I was equally struck by the fact that in the person of another friend, the "buried" was at the same time, and more profoundly, his own "burier" - in close connivance with him.

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[40](***) (October 9) Zoghman told me that, in fact, he didn't have a Xerox of the complete article in his possession at first, which he pulled out only later.

[41](*) It's not the first time I've heard this "absolute power" claptrap, with which one would like to convince oneself of one's own powerlessness and justify it. If anyone has invested anyone with "absolute power" over himself, Zoghman, it's none other than Zoghman himself!

[42](**) (May 8) It's surely no coincidence that the unequivocal signs of conflict in my friend's relationship with me appeared in the very aftermath of this stay, when he "shared my hand and my bed" in an atmosphere of unreserved affection, abolishing a feeling of "distance" that our first brief encounter probably couldn't entirely erase. Here I encounter a situation with which I have long been familiar, and which I discuss (in relatively general terms) in the two notes "Le Père ennemi (l), (2)" (sections n° s 29, 30). Little did I suspect, when I wrote them as a commentary on the reflections that had preceded them, the extent to which the archetypal situation I describe there would constantly find itself at the center of a long reflection yet to come, just as I thought I was nearing the end of this journey!

[43](*) (May 30) Since these lines were written (May 6), my friend's attitude has changed dramatically, and lately I've seen no signs of attachment to a victim role. It goes without saying that the lines which follow (like those which preceded) concern certain episodes in my friend's life, and in no way claim to define a temperament or describe a permanent bias.

with the very people whose willing victim he delights in being.

And I can see that the person primarily responsible for his own spoliation is none other than my friend Zoghman himself, who for three years has acquiesced by his silence to his humiliation by those who take their ease with him. He had everything in his hands to fight for - and for three years he chose to forget he even had hands, and to be defeated without having fought[44] (**).

Note $78_1'$ I had never held this short preliminary communication in my hands, but only the more circumstantial "Hodge Theory II, III" publications that appeared in Publications Mathématiques. This is why I had been under the impression that Deligne had not seen fit to ever allude to a role

p. 312 motive theory in the genesis of his ideas on Hodge theory. I thought that if he had wanted to mention any role I might have played with him[45] (*), he would probably have done so with "Hodge Theory II", his thesis work, which was the perfect opportunity to mention such things[46] (**). I've just seen that he's fulfilled the formality of mentioning me once and for all, with this lapidary line[47] (***) alluding to "Grothendieck's conjectural theory of motives", with even a reference at the key (to Demazure's talk at the Bourbaki seminar).

Once again, nothing to say! The idea never occurred to him to specify that he had learned this theory (all conjectural, let's not forget!) from **another source** than this meagre text by Demazure, which can give no image of a theory of great richness (all conjectural!), which runs like a thread through all Deligne's subsequent work on weight yoga - pending the escalation of the "pirate volume" LN 900, where the motivic Galois groups are finally exhumed (fifteen years later) (this time without even a laconic reference line containing the name of the deceased. . .).

On reflection, in this laconic quote, I recognize the same "thumb!" style. - a quote from pure form, to be fair, with a reference that is in no way likely to enlighten the reader (in this case,

p. 313 on obvious and deep-rooted relationships with ideas that it is precisely to hide[48] (*) - and which have remained hidden during the twelve years that followed), but **of a nature to deceive him**.

Note $78_2'$ I didn't have to hold this[49] (**) text in my hands (which I learned about a few years ago). weeks) to know that my name wasn't on it. Nor was Serre's, who was the first to glimpse a "philosophy of weights", which I later worked out in great detail.

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[44](**) (May 30; This is an admittedly subjective view of someone with the temperament of a fighter, someone in whom this fi bre might have seemed absent. It would seem, however, that since these lines were written, my friend's fighting spirit has been reawakened, and he is determined to fight back against an iniquity of which he has been the victim.

(April 18, 1985) For a different, less "harsh" take on my friend's provisions, see also the "Roots" note (n° 171).₃

[45](*) (May 30) Until a few weeks ago, I systematically downplayed this role. See note "Being apart" n° 67' of May 27, where I first became aware of this attitude in myself and perceived its meaning.

[46](**) (May 30) Nor do I remember being asked to sit on the thesis jury. The funeral was already well under way...

[47](***) Serre is also implied in the same line by the cross-reference sign [3] - the curious reader will find his name in the bibliography at Hodge I. This expeditious reference line is the only one between 1968 and the present day where there is any allusion (however cryptic) to the "sources" it mentions in a single breath: Serre (alias [3]), motifs, Grothendieck. ...

(May 28) However, I have since come across another such allusion, very interesting in view of the very special occasion. On this subject, see the note "L'Eloge Funèbre (1) - ou les compliments" n° 104 , and the end of the note that precedes it ("Le Fossoyeur - ou la Congrégation toute entière" n° 97), situating this "particular occasion".

[48](*) As I write these lines, I am reminded of a revealing incident involving "weights" two years earlier, mentioned at the start of the note "Canned weights and twelve years of secrecy" (n° 49), and in more detail at the start of the note "The eviction" (n° 63). For the "pouce! style" in general, see the reflection in the note "Pouce!" (n° 76). It's a style with which I'm becoming quite familiar!

[49](**) "Weights in the Cohomology of Algebraic Varieties", by P. Deligne, Vancouver Congress 1974, Proceedings, pp. 78-85.

15.1.9. The Boss

Note! 78" (June 3) Zoghman explained to me that he only gradually became aware, and confused at first, of the "swindle" that was going on around my work. The manuscript Verdier had given him in 1975 (see "Les bonnes références" note n° 82) had been providential for him, notably in introducing him & to the notion of constructibility and its essential properties, as well as to the biduality theorem, from which he had drawn inspiration for the biduality theorem (or "local duality") in the context of D-Modules. It was only years later, while reading SGA 5 (a massacre edition, admittedly, but not so massacred as to give an attentive reader like him the slip) that he began to realize something. For a long time, he had been filled with admiration and gratitude for his distant elder, convinced that the ideas he drew on were his own. In fact, it would seem that for years he was convinced that the theory of duality known as "de Verdier" was indeed due to Verdier, or at least to "Serre-Verdier", and that the idea of duality he calls "de Poincaré-Verdier" was also due to Verdier. It was only around 1979 (the year he defended his thesis) that he began to realize that there was something wrong - but I presume he had to be careful not to let anything show about it to his prestigious "boss", nor to me, when we met in February 1980 and June 1983.
It was only with the Colloque Pervers, in June 1981' then that he began to sense the escamotage that was p. 314 that he also began to realize more clearly the world he had strayed into[50] (*)! Surely, for him, I had to be part of that world, where my former students (or at least some of them) had the upper hand and plundered the posthumous pupil with the same casualness as the departed master. The only difference, as it turned out, was that I was dead and they were alive and kicking. ...

I can imagine that even after the Colloque Pervers, Zoghman still found it hard to believe the testimony of his healthy faculties, which told him quite clearly what had happened. He only received the famous Introduction to the Colloquium Proceedings, signed by B. Teissier and his "patron-sic" Verdier, in January 1984. Having denied the evidence for nearly three years, the shock was all the greater, I understand. It was two months later that I contacted him again, sending him the notes "Mes orphelins" and "Refus d'un héritage - ou le prix d'une contradiction" at the end of March, and it was another month later that he finally decided to "let me in on the joke" and tell me about the "Mystification du Colloque Pervers".

15.1.10. My friends

Note 79 And here I am about to finish and make public this reflection which will put an end to the secrecy that Zoghman himself has maintained around the spoliation of which he is the victim, and from which he also reaps the obscure benefits[51] (**). Perhaps it will be unwelcome to him, just as it may be unwelcome to my friend...
Pierre, to whom I'll hand-deliver it as soon as it's finished and the text cleaned up and printed[52] (***).
The best thing I have to offer my friend Zoghman as well as my friend Pierre, perhaps both the p. 315

[50](*) Zoghman ended up having such a low opinion of his ex-boss, that he was convinced that everything Verdier had done in the sixties (which I review in a b. de p. note to note no.° 81 "Thesis on credit and comprehensive insurance") had been more or less dictated or at least blown by me.

[51](**) (May 30) I would remind you that this reflection was inspired by a disposition in my friend that now seems outdated. (Compare two b. de p. notes of May 30 with note n° 78'.)

[52](***) I didn't think I'd ever have the opportunity, in my remaining years, to return to the capital for a few days. But my friend Pierre has travelled often enough, over more than ten years, to meet me deep in the remote countryside, for me to travel on this exceptional occasion, following up on an often-repeated invitation that has never yet been put to good use.

Will they receive it as the worst: as a calamity, or as an outrage. All the worse, since my testimony is public - just as the silences of both have been rites of public acts, and which commit one as they commit the other.

Whether they reject or accept my testimony is their choice, and the same goes for Jean-Louis, whom I counted among my friends, just as Zoghman and Pierre do today. These choices affect me closely, and they are not mine. I have no temptation to predict what they will be. It won't be long before I know, and I await what the weeks and months ahead will bring with intense interest, suspense - and without a shadow of anxiety. My only concern and responsibility is that what I offer is the best I have to offer - that is, to be true.

Some may be surprised that I speak so bluntly of people I call "friends", and see in this name a stylistic clause, or even an intonation of irony that is absent. When I refer to Zoghman Mebkhout or Pierre Deligne as "friends", it's a reminder of the feelings of sympathy, affection and respect that are within me as I write. Respect tells me that I don't have to "spare" a friend, any more than I have to "spare" myself - like me, he is worthy of encountering the humble truth, and no more than me, he needs sparing.

If I don't refer to Jean-Louis Verdier as a "friend", it's in no way because I consider him less "good", or less "deserving", than my friends Zoghman and Pierre, or myself, but because life has distanced us from each other. The feelings of sympathy and affection that bound me to him fifteen or more years ago have more or less faded with time, and have not had the opportunity to be revived by any kind of personal contact. The few attempts I have made to re-establish such contact have met with no response, and I don't know whether reading these reflections will revive a relationship that had frozen. But even though he's no longer a "friend" to me, I don't think I'm showing him any disrespect by not treating him any more kindly than I treat myself or my friends, and I'm well aware that to do otherwise would be to do him or anyone else a disservice. Not to mention that both he and my friend Pierre, if any that they insist on "defending" themselves (or attacking) rather than taking the risk of looking at themselves, are not lacking in means or support. And not to mention that where they have had the opportunity to discourage or to crush, more than once both have done so, ruthlessly and mercilessly.

p. 316

15.1.11. The pavement and the beautiful world (or: bladders and lanterns. . .)

Note 80 (May 9) It's about time I finally gave a reference for this famous Riemann-Hilbert-(Deligne qui ne dit pas son nom) theorem - Adam and Eve - bon Dieu - (and especially not Mebkhout), which everyone quotes at length (including myself), and for which apparently nobody has yet thought to ask where it is demonstrated. Having been led to believe by my friend Zoghman that the "me- morable theorem" was to be found in his thesis, I did indeed find it in the table of contents, under the name (admittedly down-to-earth and worthy of a cad) "Une équivalence de catégories", Chap. III, par. 3, p.. 75. To make matters worse, it's not even entitled to the name "theorem" but is called "Proposition 3.3" (and what's worse, my name appears, underlined again, on the same page). I'll even admit that, having failed to read the previous 75 pages to recognize myself, I wasn't entirely sure whether this was it - Zoghman confirmed that it was, and I trust him[53] . The demonstration (it would seem) is the subject of Chap V of the same thesis - which was passed at the University of Paris VII on February 15, 1979, before a Jury comprising D. Bertrand,

[53](*) (April 17, 1985) It would appear that the generally used form of the "God's theorem" is not that of the theorem quoted here, but a related form demonstrable by the same methods. See the note "Eclosion d'une vision - ou l'intrus" (n° 171_1 , and in particular today's b. de p. note therein.

R. Godement, G. Houzel, Le Dung Trang, J.L. Verdier. Interested parties who have not yet received a copy from the author (who sent his thesis to all those he could suspect, rightly or wrongly, might be interested) need only ask him, and he will be pleased to do so... ... Of course, he sent a copy to each of my former cohomology students, none of whom has been heard from since. They must have changed subject in the meantime, unluckily. ...

It has to be said that Zoghman definitely doesn't have the knack of selling his merchandise, of presenting it in a clear and appealing way - these are things that have to be learned, and he wasn't as lucky as my former students to learn the ropes from a virtuoso of the trade who didn't skimp on his time. But he can't complain, he's had his "three interviews", and perhaps one of the "luminaries" will one day have the idea of even acknowledging his indigestible pamphlet. He must have realized himself that the paving stone was not easy to read (even if it wasn't lost on Riemann or Hilbert. . .): he wrote a note to the CRAS, which is still shorter, to draw attention to his famous theorem, the title of which I'll give you in a thousand: "On the Hilbert-Riemann problem"! I knew my friend Pierre Deligne wasn't any better at history than I was, so all he had to do was restore the chronological order, and contribute the pretty folklop designation . 317 "correspondence" and that was it, Zoghman had it coming. ... This note is dated 3.3.1980, Series A, p. 415-417.

Verdier must have learned of the theorem in one of the "three interviews" he gave to his pupil - sic (or at the time of the defense), but he must not have realized it. As for Deligne, he finally realized something, I can't say when, but what's certain is that he knew about it in October 1980, and so did Bernstein and Beilinson, according to what he himself says. Mebkhout himself went to Moscow to explain his results (at length) to Beilinson and Bernstein (in case they had trouble reading him). I don't know if they or Deligne ever read the thesis or the subsequent note to the CRAS, but they must have figured out what was in it, since next year's "memorable Colloque" at Luminy was, coincidentally, all about it.

To sum up, and according to the latest information from my intelligence service, there were at least five people perfectly aware of the situation, who took part in the mystification known as the "Colloque Pervers", namely (in alphabetical order of the actors) A.A. Beilinson, J.Bernstein, p. Deligne, J.L. Verdier and Z. Mebkhout - plus a whole Colloque acultees, surely brilliant mathematicians to boot, who apparently wanted nothing better than to be mystified and take bladders. Mebkhout - plus a whole Colloquium of aculturous people, surely brilliant mathematicians to boot, who apparently wanted nothing better than to be mys- tified and to take bladders for lanterns[54] (*). Which proves once again that we mathematicians, from the illustrious Medalist to the obscure unknown student, are not a hair smarter or wiser than the average person.

15.2. VIlI L' Elève - alias le Patron

15.2.1. Credit thesis and comprehensive insurance

Note 81 (May 8) It seems time to express myself more circumstantially on the "thesis-" affair. p. 319
phantom", which I had only mentioned "in the aftermath" in two previous notes (notes (48) and (63''')). An inattentive or ill-disposed reader might say that I'm simultaneously reproaching my ex-student J.L. Verdier for two contradictory things - for having "buried" the derived categories, and for having "published" them

[54](*) (June 3) In fact, it appears that all Colloquium participants, without exception, had been made aware of the situation on the spot. On this subject, see the note "Le Colloque", n° 75', written today.

(in SGA $4\frac{1}{2}$) and claim authorship; just as the same reader would say that I reproached P. Deligne for both "burying" the motifs, and exhuming them (in LN 900). So it may not be superfluous to give a retrospective of the situation, from 1960 to the present day.

Around 1960 or 1961, I proposed to Verdier, as a possible thesis project, the development of new foundations for homological algebra, based on the formalism of derived categories that I had developed and used in previous years for the purposes of a coherent duality formalism in the context of schemes. It was understood that in the program I was proposing to him, there were no serious technical difficulties in prospect, but above all a conceptual work whose starting point was acquired, and which would probably require considerable developments, of dimensions comparable to those of the Cartan-Eilenberg book of foundations. Verdier accepted the proposed subject. His foundations work continued satisfactorily, materializing in 1963 in a "Etat 0" on derived and triangulated categories, multigraphied by the IHES. This 50-page text is reproduced as an Appendix to SGA $4\frac{1}{2}$ in 1977.
(as stated in note (63' "))[55] (*).

p. 320 If the defense didn't take place in 1963, but in 1967, it's because it was unthinkable that this 50-page text, the embryo of a foundational work yet to come, could constitute a state doctorate thesis - and the question of course didn't even arise. For the same reason, when he defended his thesis on June 14, 1967 (before a Jury including C. Chevalley, R. Godement and myself, who presided), there was no question of presenting this work as a thesis. The text submitted to the jury, 17 pages long (+ bibliography), is presented as an **introduction** to a major work in progress. It outlines the main ideas behind this work, placing them in the context of their many uses. Pages 10, 11 give a detailed description of the chapters and paragraphs planned for this seminal work.

If the title of Doctor of Science was awarded to J.L. Verdier on the strength of this 17-page text, outlining ideas which he himself says are not his own[56] (*), it was clearly a contract of good faith.

p. 321 between the jury and himself: that he was committed to completing and making available to the public this work of which he

[55](*) This text alone may seem a meagre result for two or three years' work by a gifted young researcher. But most of Verdier's energy was then devoted to acquiring the indispensable basics of homological algebra and algebraic geometry, by attending my seminars in particular, and by working one-on-one. His contributions to the duality formalism (see below) came later, once Artin and I had developed the stale duality formalism in detail in SGA (1963/64), when I suggested (in parallel with his work on the foundations of derived categories) that he develop the same formalism in the context of "ordinary" topological spaces and smooth morphisms of such spaces.

It was around the time I began my "Séminaires de Géométrie Algébrique" series with SGA 1 (in 1960) that I was contacted by Verdier, along with Jean Giraud and Michel Demazure, asking if I had any work for them - and they were knocking at the right door! Coincidentally, from the moment I wrote the note "Mes Orphelins" (n° 46) when the three of them contacted me, they had just formed a small seminar called "Séminaire des orphelins" (on the theme of automorphic functions, approaching calculations with a zinc strand), as their boss (or sponsor at the CNRS?) had just left for a year without warning, leaving them hungry and a little empty. That emptiness was soon filled... .

[56](*) The beginning of the thesis reads:

"This thesis was written under the supervision of A. Grothendieck. The essential ideas it contains are due to him. Without his initial inspiration, his constant help, his fruitful criticism, I could not have completed it. I would like to express my deep gratitude to him.

I would like to thank Claude Chevalley for chairing my thesis jury and for his patience in reading this text. My thanks to R. Godement and N. Bourbaki for introducing me to mathematics.

"The term "this thesis" can hardly refer to anything other than the body of foundational work undertaken, of which the text submitted constitutes the introduction - work which was therefore not, strictly speaking, "completed" at the time of the defense.

(May 30) This inconsistency reflects the ambiguity of a situation for which I was primarily responsible, as thesis director and (if the cover of the copy of the thesis in my possession is to be believed) as president of the Jury. There was a lack of "rigor" on my part towards a brilliant student, a complacency that goes in the same direction as that which I had shown towards Deligne (see the note "L'être à part", n° 67'), and which contributed its share to make bear the same fruits.

presented a brilliant introduction. This contract was not kept by the candidate[57] (*): the text he announced, a text on the foundations of homological algebra from a new, proven point of view, was never published.

Clearly, if Verdier's work between 1961 and 1967 had been limited to writing the skeletal "Etat 0" of 1963, the jury would not have considered accepting this "thesis on credit". The writing of his work had to be sufficiently advanced to allow completion within a year or two, and for practical reasons it seemed appropriate that Verdier should have the title without waiting for the work on which it was based to be completed.

It should be added that between 1964 and 1967, Verdier had made some interesting contributions to duality for- malism (81_1), which, together with the foundational work he was supposed to be pursuing, could justify the credit given to him. His contributions to duality as a whole could, in a pinch, have constituted a reasonable doctoral thesis. Such a thesis, however, would by no means have been in the style of the work I am accustomed to proposing, all of which consists in the systematic development to completion of a theory whose need and urgency I sense (82_2). I don't recall Verdier ever raising the question of presenting such a "thesis on titles", and I doubt I would have accepted, since such a thesis would have in no way corresponded to the "contract" that was signed between him and me, when I entrusted him with the beautiful subject of derived categories, with the task of developing foundations on a vast scale.

As J.L. Verdier's thesis supervisor and president of the jury, I accept full responsibility for my carelessness in awarding him (jointly with C. Chevalley and R. Godement, trusting the
the title of doctor on work that had not yet been done[58] (**).

I am not justified in complaining if I now see some of the fruits of my levity. But this p . 322
does not prevent me from stating this publicly, and that the actions of my ex-student J.L. Verdier are his responsibility alone, and that of no one else.

Not to keep the contract he had made with me and with the Jury who had placed their trust in him, was a way of burying the point of view of derived categories that I had introduced and that he had taken on the task of founding through a major work. This work may have been done, but it was never made available to the user. It was a way of "writing off" a set of ideas that he himself had helped to develop.

The revival of the notion of derived category in Mebkhout's work met with no encouragement from Verdier (nor, for that matter, from any of my other cohomological "luminaries"). The de facto boycott of derived categories seems to me to have been total until about 1981[59] (*), when they made their comeback in force at the "memorable Colloque" at Luminy (see note (75)), under a sudden surge of demand.

However, State 0 of Verdier's "thesis" had already appeared four years earlier, in 1977, as an appendix to the

[57](*) It is all the more remarkable that J.L. Verdier refused my proposal to sit on Contou-Carrère's thesis jury in December 1983, with J. Giraud, and myself acting as research director, believing that the thesis (though entirely written and carefully read by J. Giraud) and the jury would not offer sufficient guarantees of seriousness, without referring to the control of a Commission des Thèses des Universités **Parisiennes** (Sic).

[58](**) To this responsibility I should add that of not having ensured, during the two years that followed (before my departure from the mathematical scene), that Verdier actually kept to the contract he had signed. It has to be said that my energies were so focused on pursuing the foundational work I had taken on myself, not to mention motivational reflections and the like, that I didn't have to think too much about the unpleasant task of reminding others of their obligations. I had to learn of Verdier's decision to abandon the publication of the planned work in the early '70s, at a time when I was absolutely no longer into maths, and when the idea would not have occurred to me to "react".

[59](*) (May 30) These somewhat dubious forms of style are in fact out of place. As Zoghman Mebkhout confi rmed me (who paid to find out), what I dubiously put forward about the status that was given to homological algebra "Grothendieck style" corresponds well to reality.

volume SGA $4\frac{1}{2}$ (see note n° 63''') - so ten years after the defense of his thesis, and at a time when (to my knowledge(*)) Mebkhout was the only one to make use of derived categories in his work, against the fashion of the seven years that had preceded. Unless I'm mistaken(*) , he remained the only one to do so, right up to the time of the great "rush" around the famous "Riemann-Hilbert correspondence" at the Colloquium already mentioned, where Deligne alias Riemann-Hilbert appeared as the father of this "correspondence" - sic, and Verdier (with his providential Etat 0 abundantly quoted by his generous friend) appeared as the father of derived categories and algebra.

p. 323 homological style 2000, with no mention of my humble self and even less of Mebkhout[60] (**).

In the light of these events, I believe I understand the reason for the unexpected publication of this State 0 which (as it says in the introduction to SGA $4\frac{1}{2}$ by the same friend) "had become unobtainable" and that nobody cared about "finding", except (perhaps) Zoghman Mebkhout[61] (*). So there was just this one unfortunate fellow who, in his own corner and against all odds, persisted in using these notions of a bygone age, without anyone really knowing what he was getting at - so stubborn, in fact, that we began to doubt whether he wouldn't one day come up with something that would do the trick, you never knew... . After all, the man to whom he sometimes imprudently referred as one of his sources of inspiration (alongside the Master's works) had, in the past, proved or found things with all that, things that couldn't be ignored even if the author - and the Master himself - were forgotten, Jean-Louis Verdier himself, had he not made his start to stardom with this "Lefschetz-Verdier" formula, which he would have been hard-pressed even to write down, let alone prove, without all these notions fit for the dustbin. . .

While my influential ex-student of almost ten years (since he had rid himself of a certain annoying formality. . .) **was betting against** derived categories and would continue to bet against them until time X (of the famous Colloquium), he must have thought it prudent (you never knew. . .) to pre-empt events that might occur. .) to anticipate events that might occur, in short, to take out "all-risk insurance", by publishing (not the large-scale work that was one day supposed to constitute a thesis, but) a "text-witness", a sort of exhibit "just in case. . . ."; a text that would "attest to his claim to paternity over an **orphan** whom he] had taken a liking to, and whom he continued, pending events, to disown[62] (**).

p. 324 **Note** 81_1 The contributions in question are: 1) Foundations of a duality formalism in the context. of locally compact spaces and 2) that of Galoisian modules (in collaboration with J. Tate); 3) the Leschetz-Verdier **fixed-point** formula; 4) duality in locally compact spaces.

Contributions 2) and 3) are "unforeseen" compared with what was known. The most important contribution seems to me to be 3). Its demonstration follows easily from the duality formalism (both for "discrete" and "continuous" coefficients), which does not prevent it from being an important ingredient in the ar- senal of "all-purpose" formulas available to us in cohomology. The existence of this formula was discovered by Verdier, and came as a (pleasant!) surprise to me[63] .

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[60](**) Compare with the comments in the notes "Le compère" and "L'Iniquité - ou le sens d'un retour" (n° s 63'''and 75).

[61](*) In any case, it was while perusing the bibliography of a work by Z. Mebkhout that I had just received, towards the end of April, that I learned of the publication of this "Etat 0", when I had even forgotten the existence of this text from another age. ...

[62](**) If J.L. Verdier had really wanted to make known the yoga of derived categories, which has been buried for seven years, he would have chosen to publish the introductory text which constitutes his thesis, rather than a technical text which nobody cared about and which only acquires interest in the background of yoga and its many uses. But it's understandable that he had no desire to append to the 50-page text the 17 pages of his thesis, containing now embarrassing statements about the role of the one who must not be named. ...

[63](*) (April 19, 1985) I return to this beautiful formula, its role and its strange vicissitudes during the Burial, in the three notes "Les vraies maths. . . ", ". . and "nonsense"", "Tricks and creation" (n° 169_5 , 169_6 , 169_6), in the fourth part of Récoltes et Semailles.

The formalism of duality in the context of locally compact spaces is essentially the "necessary" adaptation of what I had done in the context of scalar cohomology of schemes (and without the difficulties inherent in this situation where everything was still to be done). He did, however, contribute an interesting new idea, that of the direct construction of the functor $f^!$ (without prior lissification of f) as a right-hand adjoint of $Rf_!$, with an existence theorem to boot. This procedure was taken up by Deligne in étale cohomology, enabling him to define $f^!$ in this framework, without any lissification hypothesis.

These comments make it clear, I think, that by 1967 Verdier had demonstrated his capacity for original mathematical work, which of course; was the determining factor in the credit he received.

Note 81_2 As another example, I would point to the detailed development of the duality formalism in the context of locally compact spaces, in the spirit of the "all-purpose" formalism of the six operations and derived categories, of which Verdier's presentation at the Bourbaki Seminar would constitute an embryo. Even in the context of topological **varieties** alone, there is still, to my knowledge, no satisfactory reference text for Poincaré's duality formalism.

(June 5) There are two other directions in which I note with regret that Verdier did not see fit to go all the way. p. 325

the end of work that he had started off strongly enough to **take credit for** (I mean, by starting up a duality formalism in the context of discrete coefficients and locally compact topological spaces), whereas the essential ideas are not due to him and he doesn't care (any more than for the derived categories) to make himself the **servant of a task** and put at the user's disposal a complete formalism (as I tried to do in the three seminars SGA 4, SGA 5, SGA 7).

The duality program I was planning and suggested he develop was set in the framework of general topological spaces (not necessarily locally compact) and applications between such that are "separated" and locally "lissifiable" (i.e. locally the source is immersed in a $Y \times \mathrm{R}^n$, where Y is the goal space). This was clearly suggested by the analogy with the framework of the stale cohomology of **arbitrary** schemes. Verdier was able to see, in the context of locally compact spaces, that the assumption of local smoothness of applications was unnecessary (which came as a surprise). Nevertheless, the context of locally compact spaces (which excludes "parameter spaces" that are not locally compact) is visibly short-sighted. A more satisfactory context would be the one that would cover both the one chosen by Verdier, and the one I was planning, i.e. the one where topological (or even topos?) spaces are (more or less?) arbitrary, and where applications $f : X \to Y$ are subject to the restriction of being 1) separate and 2) "locally compactifiable", i.e. X plunges locally into a compact $Y \times K$, K.

In this context, the fibers of an "admitted" application would be locally compact quel- conical spaces. Another step would be to admit that X and Y , instead of being topological spaces, are "topological multiplicities" (i.e. topos that are "locally like a topological space"), or even topos of any kind, by restricting the applications in a suitable way (to be made explicit), so as to find fibers that are **locally compact multiplicities**, subject if need be to additional conditions (close perhaps to the point of view of Satake's G-varieties), for example (and lastly, to the point of view of Satake's *G-varieties).*
rigorously!) to be locally of the form (X, G), where X is a compact space with **finite** operator group G.
To my knowledge, even the "ordinary" de Poincaré duality has not been developed in the case of multipli- p. 326
smooth compact topological cities (smooth: which are locally like a topological variety). The case of a classifying space of a finite group seems to show that we can hardly hope to have a duality theorem (absolute global) other than module torsion, more precisely, by working with a ring of coefficients that is

a Q-algebra. With this restriction, I wouldn't be surprised if Poincaré's duality ("six operations" style) worked as is in this context. No wonder nobody ever looked at it (except unrepentant differential geometers, pretending to look at the cohomology of the "leaf space" of a foliage), given the general boycott on the very notion of multiplicity, instituted by my cohomology students, Deligne and Verdier in the lead.

To put it bluntly, what's missing is a fundamental reflection of the following type: describe (if you can) in the context of any topos and bundles of "discrete" coefficients on them, notions of "cleanliness", "smoothness", "local cleanliness", "separation" for a morphism of topos, enabling us to derive a notion of "admissible morphism" of topos $f : X \to Y$, for which the two operations $Rf_!$ and $Lf^!$ make sense (one adjoint of the other) so as to obtain the usual properties of the six-operation formalism. Here, topos are considered as non-ringed, or perhaps as provided with Rings (which are assumed if necessary to be constant or locally constant), assuming (initially at least) which ringed topos morphisms $f : (X, A) \to (Y, B)$ are such that $f^{-1}(B) \to A$ **is an isomorphism** (81_3). The foregoing reflections suggest that when we restrict ourselves to Rings of coefficients of zero carac- teristics (i.e. which are Q-Algebras), we can be considerably broader in the notion of "admissible morphism", so as to encompass "fibers" which are e.g. (topological or schematic) multiplicities, rather than ordinary (topological or schematic) "spaces".

A first step in this direction (apart from the cases treated by me, then by Verdier on the same model) is due to Tate and Verdier, in the context of discrete or profinite groups. The memory of this first step encouraged me to pursue a reflection along these lines last year, in the context of small categories (generalizing discrete groups) serving as homotopic models. Without going very far, this reflection was nevertheless enough to convince me that there must be a complete formalism of the six operations in
p. 327 the context (Cat) of the category of small categories. (See on this subject the "Pursuit of Fields", Chap.VII, par.136, 137.) The development of such a theory in (Cat), or even in Pro(Cat), just like a theory of this type in the context of topological or schematic spaces and multiplicities, would for me have as its main interest to be a step towards a better understanding of "discrete duality" in the context of general topos.

Illusie told me last year that he had struggled with duality perplexities in the case of semisimplicial spaces (or schemes). It seemed to me to be the same old tobacco - to be able to detect the existence of a six-operation formalism in a particular case, and to understand it. But it would seem that the mere prospect of a fundamental reflection has the gift of freezing each and every one of my former students - at least among my cohomology students. If I went to any trouble with them, it was with the conviction that they would not stop right where they had left off (in terms of conceptual work) with me, and remain wringing their hands every time a new situation showed that the work they and their buddies had done with me was insufficient. The conceptual work we do is **always** insufficient in the long run, and it's by taking it up again and going beyond it, and not otherwise, that mathematics progresses. Between 1955 and 1970, each year again I found that what I had done in previous years was not sufficient to the task, and I went back to the drawing board, at least when someone else (e.g. Mike Artin}with the point of view of "algebraic varieties" in his sense) hadn't already done so. But it would seem that my students have also buried the example I set them, along with myself and my work.

Note 81_3 I seem to recall that in the formalism of the six variances in (say) staggered cohomology, the assumption that the ring bundles serving as coefficients are locally constant is unnecessary - the essential assumption is that they are prime torsion bundles with residual characteristics, **and** that $f^{-1}(B) \to A$ is an isomorphism. When we abandon this last assumption, we have to enter a theory (never yet made explicit, to my knowledge) that "mixes" the "discrete space" duality, and the "coherent" duality (relative to the rings of coefficients and their homomorphisms). As a result, we envisage
replace, on the diagrams (or more general topos) X, Y, the Rings with coefficients A, B by relative (not necessarily affine) schemes X', Y' on X, Y, and ringed topos morphisms p. 328
$(X, A) \to (Y, B)$ by commutative diagrams of the type

$$\begin{tikzcd} X' \arrow[r] \arrow[d] & X \arrow[d] \\ Y' \arrow[r] & Y \end{tikzcd}$$

with a "six operations" formalism in such a context. When X, Y, etc. . . are the ponc- tual topos, we should find the usual coherent duality.

15.2.2. The right references

Note 82 (May 8) This is J.L. Verdier's article "Classe d'homologie associée à un cycle", published in Astérisque n° 36 (SMF), p.101-151 in 1976. In a way, this rather unbelievable article (although nothing should surprise me any more. . .) is a counterpart to the "perverse article" by Deligne et al. With one reservation: it practically consists in **copying** over fifty pages, in a slightly different context, notions, constructions and reasoning that I had developed at length ten or fifteen years earlier - terminology, notations, everything is there verbatim! I'd have thought I'd returned to a session of the APG 5 seminar held in 1965/66, where these things were spelled out (apparently to the satiation of the participants[64] (*)) for an entire year. After this seminar, at least, all these things became part of the "well-known" for people in the know[65] (**) Verdier had attended, of course, as had Deligne (the only one who was never left behind, even though it was the first time he'd set foot' at my séminaire[66] (*) - it took p. 329
do it...). It's true, well, well, that in 1976 it had been ten years since the "writing-sic" of this famous seminar by "volunteers-sic" who were fed up with it was dragging on - I see now that one of these "volunteers" took charge of the "writing-sic" in his own way, even before the publication of SGA 5 in

[64](*)For comments along these lines, see notes n° s 68, 68' "Le signal" and "Le renversement", in which I examine the strange vicissitudes of the writing of this seminar, and the relationship between these and Deligne's "SGA 4 operation[1] , The following reflection reveals to me another unforeseen aspect of these vicissitudes and of the dismemberment of the mother seminar by the combined care of Verdier and Deligne. Verdier's and Deligne's publications on this dismemberment date from 1976 and 1977 - they constitute the "green light" given to Illusie to prepare (eleven years later. . .) the publication of SGA 5 (which, as Deligne says in SGA 4[1] , "may be seen as a series of digressions, some of them very interesting").

[65](**) For a reflection on this "hasty" impression, see the note on "Silence" (n° 84).

[66](*) The year of this seminar was (I think) the year I met Deligne, who must have been nineteen at the time. He "got into the swing of things" very quickly, and even took on the task of writing my lectures on étale duality from the previous year (which he must have known from my explanations and notes), and also the lecture on the cohomology class associated with a cycle, which was discussed in the note quoted n° 68' ("Le renversement"), and which will be discussed a little more in this one. The fact that with the means at his disposal, and a complete mastery of the subject, he waited eleven years to write the essay, to then include it in his SGA 4[1] without informing me, now shows me, in retrospect, that as early as 1966 (and not just 1968 as I may have assumed - see note n° 63, "L'éviction") - i.e. from the very first year of our meeting, there was a profound ambiguity in my friend's relationship with me, expressed from that moment onwards in a perfectly clear way, which I have refrained from learning about to this day!

1977 ! It would appear that the vicissitudes of this unfortunate seminar were not only to Deligne's advantage, as he took advantage of a situation of debandade in his own way. But at the time, Deligne still took care to mention in his essay (on the cohomology class associated with a cycle[2]) "d'après un exposé de Grothendieck" (after a paper by Grothendieck), while dismantling SGA 5 from one of his key lectures and attaching it to his SGA 4[1] as a matter of course. (It's true that this compensated him for being able to present me as his "collaborator"! - see note "Le renversement", n° 68').

Coming back to the **homology** class (not to be confused!) associated with a cycle (which, according to the title, is the subject of Verdier's article), I had developed this formalism in great detail, over several presentations, during the oral seminar, before an audience that, incidentally, begged for mercy (except always the only Deligne, always dashing and fresh...). It was one of the innumerable "long exercises" I developed that year on the formalism of duality in the étale frame, feeling the need to arrive at a complete mastery of all the points I felt needed to be thoroughly understood. The interest here was to have a valid formalism on an ambient scheme that is not necessarily regular - the passage to the **cohomology** class in the case of

p. 330 regular, and the link with my old construction using cohomology with supports and immediately giving compatibility with cups-products, being immediate. I also found that this part of the seminar does part of the lot that didn't make it into the published version - no doubt Illusie (on whom all the hard work of preparing a releasable (hmm) edition eventually fell) must have been quite happy that Verdier took care of it, mutatis mutandis (i.e. here: without changing a thing!).

As the saying goes, "it hardly needs saying" that my name does not appear in either the text or the bibliography (except implicitly by the ever-present reference SGA 4, which we'd still have to find a replacement for... .). There's no allusion to a "Seminar on Algebraic Geometry" under the acronym SGA 5, which the author might have heard of - although I seem to remember seeing him busy taking notes (like everyone else, except Deligne of course. . .).

Incidentally, I've exaggerated just a little by saying that my name is absent from the text - it makes a single, mysterious and lapidary appearance on page 38, section 3.5, "Fundamental cohomology class, intersection" (here we come, the crux of the matter!). The reference consists of a cryptic sentence whose meaning escapes me, I confess: "The idea of systematically using weight complexes (??? those damn weights again!) is due to Grothendieck and was put into shape by Deligne" - without any further explanation of these mysterious "weight complexes" whose idea I had and of which I hear mention here for the first time. There will be no further mention of them in the rest of this article (nor was there any mention of them in the 37 pages before). Understand who can! As for the content of the said section, it is copied without further ado from the SGA 5 seminar which had taken place ten years earlier (and by which time this construction was already five or six years old, see note n° 68'), a seminar which he is careful not to quote. The reference to Deligne (who is said to have "perfected" an idea that had already been perfected when my friend was still in high school!) is a "flower", the idea of which no doubt came to the author because the young and newcomer Deligne had indeed taken on the task of writing my paper on this subject (and refrained from doing so for eleven years, for the benefits we know, see note cited). This "flower" is part of the exchange of courtesies between inseparable friends.

There is, however, one (undoubtedly) new and very interesting result in the article (th.3.3.1., page 9) on stability of discrete bundles analytically constructible by direct higher images through an analytic and proper morphism. Verdier had learned about all-round constructibility from me one day.
p. 331 fifteen years earlier, as well as the stability conjecture, which I had asked myself (and told who would listen) in the late fifties, before I'd had the pleasure of making his acquaintance. Reading the article, the idea wouldn't occur to an uninformed reader (but those are becoming rare. . .). I

I'm still repeating myself, I'm afraid) that the author isn't serving up hot-off-the-press notions and statements he's only just discovered. He doesn't have to say it's him - because it's self-evident. It's the famous "thumb" style that's so obviously catching on.

Apart from this detail (which, I feel, is in line with the new canons of the trade), there must still be around ten pages (out of fifty), around this interesting result, that present the author's personal work. All things considered, what strikes me most about Verdier, as with Deligne, is that he's perfectly capable of doing beautiful mathematics. Even in this sad article, there's a hint of it in the theorem quoted. But by keeping himself (like his friend) in a gravedigger's mood, he operates, like his prestigious friend, on a paltry fraction of his means. A sign (which astounded me) of apparent mediocrity, in a mathematician who nevertheless gave proof of as- tuce and flair, was the total lack of instinct to feel the scope of the work of his "pupil-sic" Mebkhout, whom he took pleasure in treating from the height of his greatness, without ever having been able to do himself a work of comparable depth and originality[67] (*). Not that he isn't just as capable as Mebkhout or me. But he has never given himself the chance to do great things, that is, to let go of a passion - rather than **using** mathematics and its gifts **to** dazzle, dominate or crush. Up until now, he's been content to take up as is the fruitful notions and points of view that have already been baked in. Indeed, he seems to have totally lost touch with the meaning of **mathematical creation**.

Yet I seem to remember that when he worked with me, that sense was still there. Nothing ex- ternal about him prevents that sense from resurfacing. Just as in his friend, in whom I often felt thisp . 332
same eclipse of something delicate and lively, obturated by the same fatuity.

This incredible 50-page article, which appeared in a standing magazine, sheds new light on the "The note - or the new ethic" incident (s.33). where a note to the CRAS of **a few pages**, summarizing a solid and **original** work, on an important subject (in my humble opinion), the fruit of **two years' work** by a highly gifted young mathematician, was rejected by two eminences as "devoid of interest"[68] (*). One of these eminences was none other than Pierre Deligne - the same Deligne who did not disdain to copy in toto and in person the humble doctoral thesis of one of my students (whom he made a point of quoting). (This duplicate, enhanced by a prestigious signature, makes the largest article in the "memorable volume" LN 900 of a no less prestigious collection! See end of notes (52), (67)).

The "tableau de moeurs" is growing by the day, without my having to come out of retirement and hit the streets to mingle with the "big world". A few hours spent here and there leafing through a few well-chosen "great texts" were enough to edify me...

15.2.3. The joke or the weight complex

Note 83 (May 8-9) I've been thinking about this "weight complex" referred to in the "reference - thumb" in Verdier's memorable article[69] (**) - a reference that's sheer nonsense. As soon as I saw this ludicrous reference, an association came to mind that kept running through my head. This isn't the first time, far from it, that I've found myself faced with something

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[67](*) The same astonishing lack of flair was evident on this same occasion in Deligne, who didn't "feel the wind" (the importance of Mebkhout's ideas) until 1980, it seems, even though Mebkhout had been working in this direction since 1974. On more than one occasion, I've had occasion to observe my friend's natural flair obturated by suffi ciency, especially since 1977 (or 78), which seems to have been a first "turning point" (see notes "Two turning points" and "Les obsèques", n° s 66,70).

[68](*) For details, see the note "Casket 4 - or topos without flowers or wreaths", n° 96.

[69](**) See previous note "Good references".

that seem to defy rational explanation - even though the meaning is clear and unmistakable and clearly perceived, but at a different level from that of conventional logic. This was the only one on which I had functioned at a conscious level for most of my life - with the result that I was constantly overwhelmed by "bizarre", incomprehensible events - distressing in their irreducible saugrenousness! My life changed a lot from the moment (less than ten years ago) when I started to live on a wider register of my faculties. I've come to understand that every absurdity, every so-called

p. 333 "nonsense" has a **meaning** - and the mere fact of knowing this, and from then on being curious about the meaning behind the nonsense, often opens me up to its obvious meaning.

In this nonsense about "weight complexes", I think I sense an act of **bravado** of the same nature as in the appellation "faisceaux pervers"[70] (*) - the pleasure, in this case, of proving to oneself that one **can afford**, in a journal of standing and in a text that claims to be a standard reference text[71] (**), to say a related nonsense, and that **nobody** will dare to even ask a question! And I'm convinced that the wager contained in this bravado, in the eight years since the article appeared, has **been won to this** very day: that I was the first today to put the naive question to the author.

Of course, the time (or place) at which a saugrenuité appears, in this case at the precise moment of the one and only reference to my person, is by no means coincidental; nor is the form it takes, here by allusion to a type of notion, "weight", entirely foreign to the theme of the entire article, and by the improvisation of a composite notion "weight complex" that never existed! The association that immediately presented itself to me could well provide the key to the more precise meaning of the saugrenuité, beyond the bravado, the demonstration of power. It's the association with an allusion just as sibylline and just as purely formal (but without yet having the added dimension of saugrenuité!) in Deligne's article quoted at the beginning of note (49)[72] (***). It was an obscure allusion, in an article where the word "weight" was rigorously absent and where nobody but Serre or I would have been able to see them, to "weight considerations" which had led me to conjecture (in a less general form, it is clearly stated) the main result of the work. As I explain in the more detailed note "The eviction" (n° 63), behind this rhetorical allusion lies the intention to **conceal** both my role, and the ideas (concerning "weights" and "weights") that I'd been working on.

their relationship to cohomology in general, and Hodge's in particular) which he intended to reserve for

p. 334 himself alone. This intention must have been all the more clearly perceived by Verdier as he himself me "operates" on the same diapason (in his relationship with me, at least, which seems to me to be the main cement between the two inseparable friends). In either case, an honest presentation would have consisted in starting the article by clearly indicating the source(s) for the main ideas, or for the question(s) that motivated the article.

Having said this, here's the meaning I see behind the symbolic language of apparent nonsense: I can allow myself, without the slightest embarrassment, to display patent **nonsense in** front of everyone, and at the same time express through this nonsense my true intention, with this absurd allusion-reference to the "weight complex": that is, I have no more intention of revealing anything about the role of Gr. in this work, any more than Deligne had such an intention with his empty allusion to "**weight** considerations"-which allusion made no more sense to the reader then than it does now with the imaginary "**weight-complexes**" I've just described.

[70](*) See "Perversity", n° 76.

[71](**) And it seems that this text is indeed a standard reference today - at any rate, for years it was one of Zoghman's bedside texts (he recently sent it to me). It was there that he learned about the notion of constructibility (which plays an essential role in his theorem), and for a long time he was convinced that Verdier was the brilliant inventor of this crucial notion for him.

[72](***) This is the note "Poids en conserve - et douze ans de secret". For a more detailed examination of this Deligne article from the point of view that interests us here, see "L'éviction", note no.° 63, quoted below.

to invent right now, for the sake of the cause and my own pleasure!

I've just posted this note, written yesterday - I was interrupted earlier by a phone call from Verdier, whom I'd tried to reach during the day, to ask him just that question. I explained that I was trying to learn a bit about cohomology, which he knew I'd never understood, and that Mebkhout had passed on to me for my instruction an old article by him, Verdier, a work that had long served as his bedside text. I was now trying my best to read it, but there was this cryptic reference - it was nice of him to quote me, of course - but I had no idea what he was talking about.

He was quite happy, even a little flattered, but yes, with a broad smile that protruded behind an air of paternal joviality, that I'd end up like this in my old age, learning cohomology on this old paper of his. I didn't expect him to contradict me when I said that he knew I'd never understood anything about cohomology - obviously that had been agreed long ago. ... As for those famous "weight complexes", I could feel his broad smile again at the end of the line (you'll say I'm making it up!), delighted that someone (and the addressee himself, no less) had finally picked up on his point.
something that had gone by the wayside for so long. At the same time, there was also a hint of embarrassment - more the embarrassment (I think) of not having been able to hide from a pleasure (like the pleasure one would take in ap . 335
slightly salacious story...), than not knowing what to answer. Dumped as I was, he really didn't have to worry about that! Without a moment's hesitation, he turned to Deligne (whose name I hadn't mentioned), who had given a demonstration in one of his articles, in which he also quoted me (he couldn't quite remember where) - in any case, it was a question of weights, but yes, he'd forgotten a little, of course - but not arithmetical weights, because I was quite right, it wasn't the same....

His tone was jovial and unapologetic, and he made it clear that he'd already given me quite a bit of his time - with a slightly hurried air, but without losing his debonair, slightly protective tone. I apologized for bothering him like that, with a rather stupid question, and thanked him for his explanation. My apologies were sincere and so were my thanks - he had indeed taught me everything I wanted to know.
[73](*).

15.3. IX My students

15.3.1. The silence

Note 84 (May 9) I was perhaps a little brisk yesterday, writing that in "the correct reference" (see note (82)) p. 337
what the author and ex-student shamelessly recopied as "part of the realm of the 'well known' for those in the know". I tried to explain who these "people in the know" were - with the conclusion that **they were no more, no less, than the dear listeners of this** SGA 5 **seminar** in 1965/66 - listeners, as I've had occasion to say, and judging by the vicissitudes of the writing of this seminar in the hands of volunteers whose lack of conviction I didn't want to sense, it was often rather "more" than "less" (always with the exception of the same Deligne ; of course). Indeed, there was no risk of other people "getting involved" as long as SGA 5 had not been written and published, precisely to enable people to "get involved" by reading it! This seminar was in fact published (as fate would have it) after the two "memorable

[73](*) Even with my droopy airs, I didn't really feel like I was putting on an act (I don't have the gifts for it), it was perfectly natural - in truth, I'm a bit droopy in all this stuff I haven't handled in nearly fifteen years! But I think that even when I'm old and ripe for the hearse, I'll still be able to feel the difference between an empty walnut and a full one. .

publications" by two of my dearest students and comrades-in-arms, namely the article in question by Verdier in 1976 (in which he says nothing about the origin of the ideas he develops, published there under his pen and for the first time), and Deligne with SGA $4\frac{1}{2}$, which has already been discussed at length[74] (*). After that, we cordially invite Illusie to take care of publishing the rest!

I can't remember in detail who took part in this seminar - whether Artin was there or not, for example. I think that more or less all my students from the first period must have been there in any case.
- with the exception of Mrs. Sinh and Saavedra (whom I hadn't met at the time) and perhaps Mrs. Hakim. There was also Bucur (since deceased). Houzel, Ferrand - I'm not counting Serre - who never had a taste for big cohomological fuss, and who came to put his feet up cautiously from time to time. While no one except Deligne perhaps had a good sense of where all this was leading,
it seems to me that there must have been ten or twelve listeners (not very involved) who were at least
p. 338 following enough to be considered "in the loop"' the thought that ran through my head
since yesterday, it's that among all these people "in the know", thus representing cohomological expertise (if not all "luminaries" like Illusie and Berthelot, with their "cohomological" theses that were decidedly weighty), and even apart from Verdier and Deligne - there must be quite a few who've had Verdier's article in their hands! A certain air in Verdier gives me the conviction that nobody ever suggested to him that something might be wrong. And I also know that nobody ever drew my attention to it - I learned of the existence of this article on May 2. exactly a week ago today, thanks to Mebkhout,. who had of course known about the scam for years.

This gives concrete meaning to the euphoric observation of the "Unanimous Agreement" (to bury my modest self) made ten days ago (note (74))!This agreement encompasses many (if not all) of my "pre-1970" students, i.e. many of those who today set the tone in the mathematical world; and it includes (or has included) my friend Zoghman himself, treated as a Cinderella by the beau monde and clinging against all odds to a kind of "fidelity to my work" (to use his own expression[75] (*)), which he has had the temerity and obstinacy to claim for himself at times, with the consequences that we know. Go figure!

In short, I was wrong to suggest that such and such a standing journal published a sort of "boilerplate" article that merely copied what was "well known". What the author was copying in full view (if not of everyone, but) of many witnesses was neither published nor "well known" (except for the cohomology class of a cycle in the coherent framework, where I had published it ages ago); and these were additional ideas that I would be remiss to play down, given that I didn't consider it a waste of my time to spend a year developing these and other ideas in a seminar, in front of a large audience. Verdier's article is probably a useful and well-done "digest" of a small part of the ideas and techniques I had developed: precisely so that they can be passed on to a wider audience.
the realm of the "well-known", the daily bread of those who use cohomology (or homology) for objects that
p. 339 more or less deserve the name "varieties"' From this point of view, then, Verdier has done what was useful to do[76] (*), and in the end I have no reason to be unhappy. However, from what I sensed from my ex-student

[74](*) See notes n° s 67, 67', 58, 68'.

[75](*) (June 7) Reading all the notes on L'Enterrement during a recent visit. Zoghman pointed out to me that the expression he had used of "reliability in my work" did not really capture his thoughts. Rather, he had in mind a confidence in his own powers of judgment and mathematical instinct, which told him that my work provided him with some of the ideas he needed. It's all about **self-confidence, which is** essential if you want to do something truly innovative.

[76](*) He did so, it's true, at the expense of the "dismantling" of the original SGA 5 seminar, of which he and Deligne were the main players and "beneficiaries".

(June 7) The reflection of May 12, three days later (see note "The massacre" n° 87) made it clear that Illusie was even more directly associated than Verdier with what appears to be more a "massacre" indeed than a dismantling - even if he and friend even today, on the phone, and through many other things I've sensed from him (the "biggest" of which, or at least the most "spectacular", is the mystification of the Pervers Colloquium) - I can feel that **something is amiss**. That memorable Colloquium was certainly brilliant, mathematically speaking, in many respects. What's "wrong" is at a completely different level. I could try to define it in words, but I'm afraid that wouldn't make much sense. Anyone who doesn't feel what's wrong with this Colloquium - and I'm sure with many other Colloquia too, without mystification or anything - won't feel it a hair more, once I've made this attempt to "pin it down" and even succeeded in doing so to my complete satisfaction... ...

The question that remains open for me is whether this "sign" represented by what is undoubtedly a relatively common occurrence today (of an author presenting as his own the unpublished ideas of others) - whether this sign is that of a general degradation of morals, So, is it just a typical sign of a "spirit of the times" in today's mathematical world, or does it have more to teach me about myself - about the person I once was, and who is now coming back to me, through the attitudes towards me of those who were my students?

The two possible meanings are by no means mutually exclusive. My ex-students' relationship with me could not have found this way of expressing itself, if a certain state of morality didn't encourage them to do so. In fact, even before this "sign", I saw many others that seem to me to be even more eloquent in terms of a "picture of morals". What struck me about this sign is the particularity that sets it apart from all the others: it seems to **involve most of my former pupils at the same time**.

Such a circumstance cannot be fortuitous. To simply put it down to a "deterioration of the mores" (all that's real) would be a way of evading its more personal meaning, which implies me as a it involves each and every one of my ex-students. If I say "each", which seems to go beyond the actual amplitude of this p. 340
sign, I'm weighing my words carefully. For this sign is a timely reminder that it is scarcely conceivable that p. 340 one of my former students has not at least been confronted with situations of this kind. For years now, I've felt a certain "wind" about me, blowing through the world of mathematicians I've left behind (a wind whose origin and reasons I can now clearly see, it seems). There is no way that any of them could have failed to feel the breath of this wind, whether on the occasion of an "incident" such as the publication of this gravedigger-article, or on any other occasion. Whether he wanted to or not, such an encounter inevitably raised (or raised again) the question of his relationship with me, who had taught him his trade. And the sign I've noticed, beyond the one that just brought me to this point, is that **I haven't heard a word on the subject from any of my students**[77] (*). This is a "coincidence" the meaning of which still escapes me - but which cannot fail to make sense (84_1).

The day is dawning - I feel it's time to stop. I'm not sure that this is the time and place, in Harvest and Sowing, to pursue the meaning of this striking coincidence. It's a harvest perhaps reserved for other tomorrows, if my reflections of this night meet with an echo in one or other of those who were my students. ⇒ 85

Note 84_1 (May 16) This perfect agreement between my former students, in this complete silence towards me, goes in the same direction as other signs. One is the complete silence that also greeted the episode "Les étrangers" (see section 24) - a silence I have already pondered somewhat in note n° 23v.

was not the "beneficiary" and that he acted on behalf of others.

[77](*) (May 31) Interestingly, the one and only person who ever hinted at the existence of a funeral was an African friend of mine who had passed a 3° cycle thesis with me some ten years ago (so "post-1970 student", and of modest status), with whom I have remained on friendly terms. The letter in which he implied this must have been written two or three years ago, at a time when I was not at all surprised. I did not then ask for details of his impressions, which he has only recently returned to.

On the other hand, with the exception of Berthelot, who sent me numerous separate prints, and Deligne, who sent me four (out of some fifty publications) and one from Illusie, I haven't received separate prints from any of my former students. That says a lot about the ambivalence in their relationship with me. Send prints to

p. 341 part, even though it was doubtful whether I would ever make use of it in my work[78] (*), would have been the way to

p. 341 to let the person who had taught them their trade know that this trade in their hands did not remain inert, that it was alive and active. But it is also true that for at least some of them, their publications also testify to their participation in a tacit burial of which it was better not to inform the anticipated deceased, trade or no trade... . On the other hand, I have received numerous offprints from several authors working in crystalline cohomology[79] (**), and even a good number of offprints from fellow analysts whom I hardly know by name, when their work takes up (and sometimes solves) questions I asked thirty or more years ago, when it was obvious that I would not return to the subject I had left, and that from a "utilitarian" point of view, they were wasted offprints. But these colleagues must have sensed something that my students didn't want to sense. - Of course, in the sixties, my students were the first to be served for all my publications, both my articles and the great EGA and SGA series, and every one of them (except Mrs. Sinh and maybe Saavedra) must be in possession of my complete work published between 1955 and 1970 (in the ten thousand pages I presume).

It's true that my ex-students are in good company: none of my former close friends in the mathematical "big world", including those whose work is closely related to mine or who played a role in the development of my work program in the sixties, have seen fit to conti-.
continue to send me separate prints after my departure from[80] (***). Only recently, among

p. 342 the fifteen or twenty friends of yesteryear (including a few students) to whom I sent the Esquisse d'un Programme

p. 342 (which, among other things, announced the resumption of intense research activity, after a hiatus of fourteen years and on research themes closely related to those we used to pursue together), only two (Malgrange and Demazure) took the trouble to send me a few lines of thanks. The few more detailed (and, what's more, warm) echoes I've received have come from young mathematicians I've only recently met, and from my old friend Nico Kuiper, who is in no way connected to the kind of things I do. He found out about the text via intermediaries, and was delighted with my unexpected "homecoming"[81] (*).

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[78](*) (May 31) This may even have seemed out of the question until 1976, when I made it quite clear in the early '70s that I had no intention of ever returning to mathematics. The lecture I gave at the IHES in 1976, on De Rham complexes with divided powers, showed quite clearly that I was still interested in mathematics.

[79](**) (May 31) These are young authors whom I don't know personally, and I presume they've followed Berthelot's example, who for them must be a figurehead. The strange thing here is that, for at least the last two years (since the Colloque de Luminy, September 6-10, 1982), Berthelot has been actively trying to bury me (see the b. de p. note of May 22 to the note "les cohéritiers. . . ", n° 91) - could this be a recent turning point in his relationship with me? I don't remember receiving the reprint of the Survey article on crystalline cohomology and consorts, in which he passes my name over in silence - he must have been careful not to send it to me!

[80](***) (May 31) Of course, the psychological reasons that might have prompted them to send me some were far less strong than in the case of my students - but, one might naively think, far stronger than among my fellow analysts, or even among the many algebraic geometers whose prints I received separately, and whom I know little or nothing about personally. Clearly, after my departure from the common milieu, the fact of having been friends created or reinforced, among my former friends in the mathematical world, the automatisms of rejection that I have had occasion to observe. (On the subject of these attitudes, alluded to in passing here and there in Récoltes et Semailles, see the note "Le Fossoyeur ou la Congrégation toute entière" of May 24, n° 97.)

[81](*) (May 31) This is almost the only echo from one of my old friends (or one of my former students) in the sense of an acquiescence to my "re-entry". This is hardly surprising, given that the appearance of the deceased unseemly interrupts the normal course of a funeral ceremony... ...

(June 17) However, I recently had the pleasure of receiving a warm letter from Mumford, who says he is "thrilled" and "very excited" by the ideas sketched in the Esquisse, and who confirms that the key technical result I needed was

15.3.2. solidarity

Note 85 (May 11) This story of the ill-fated SGA 5 seminar keeps running through my head. The "good reference"[82] (**) definitely sheds new light on this story, and at the same time gives new meaning to the brilliant "SGA 4 operation ".[1] [2]

The more I think about it, the **bigger** the SGA 5 story seems**.** My first impression, when I "disembarked" just a few weeks ago (see notes n° s 68, 68'), was that a situation of debandade among the poor ex-auditors of this seminar in 65/66 had been put to good use in his own way by my friend Pierre, for his famous operation, and that no one else had anything to do with it. And as for the misfortunes of SGA 5, this was neither he nor anyone else, but rather "ut other than myself, who had not, alas, known how to enthuse my volunteer-editor listeners, nor do for them the work they stubbornly refused to do while saying they p. 343 were about to get down to business. Then it turned out over the last few days that there was one, after all, whose enthusiasm was reawakened ten years later, to publish (without reference to the seminar) what he liked to take from it, thus creating a good reference for his own account, at a time when the other "volunteers" still hadn't decided to get going.

What's become increasingly clear to me since yesterday is that it's not just two "villains", but **every single one of my "cohomologist" students** who are directly involved in the cover-up that took place at this seminar. Unless I'm mistaken, every single one of them attended this seminar - namely (in chronological order of appearance of my "cohomologist" students): Verdier, Berthelot, Illusie, Deligne, Jouanolou. (I'm not counting Jean Giraud, who operated on quite different registers from those mostly discussed in SGA 5 or its predecessor SGA 4).

This seminar, which I did **for the benefit of my students** in the first place, and even though they sometimes asked, grace - **I consider that it wasn't crap**. Every one of them, during that year, learned a good deal about his job as a "cohomology user mathematician"! The things I was doing to them, taking up in the spread-out framework and in a much more circumstantial way ideas I'd first developed in the coherent framework - these things they couldn't find anywhere else but in that one seminar made for their benefit, given that nobody before me had ever bothered to do them - and that nobody but me even felt what there was to do, and why. (Except always Deligne, who learned it over the months in this very seminar, being quicker on the uptake than the others). It was having taken this seminar (and the previous one) and having worked on it at home as best they could, and nothing else, that meant they were now "in the know" about duality formalism, and they were **the only ones** to be. This **privilege**, it seems to me, created an **obligation** for them: to ensure that this privilege did not remain in their hands alone, and that what they had learned from me, and which has been indispensable baggage in all their subsequent work up to the present day, was made available to all, and this within reasonable and customary deadlines - of the order of a year at most, or even two at a pinch.

We'll say, not without some reason, that it was up to me above all others to see to that. But if I accepted p. 344 in good faith when students and other listeners offered to help with the writing (which, for those who took it seriously, could only be good for them) - not for the benefit of being able to twiddle my thumbs while they did a job that fell to me. I've

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for my combinatorial description of Teichmüller's tower is well and truly proven. This is the first time since 1978 that an old friend of mine has latched on to my "Anabelian" ideas, whose exceptional scope (comparable to that of pattern yoga) has been obvious to me since the very beginning. ...

(March 28, 1985) Since these lines were written, I have also received a very warm letter from I.M. Gelfand (dated Sept. 3, 1984), in response to the Esquisse.

[82](**) See note n° 82.

continued, with the help of Dieudonné and others (including, incidentally, Berthelot and Illusie in 1966/67) to develop' basic texts that seemed equally urgent to me, and that no one else would have done in my place or without my assistance[83] (*). These texts have themselves become indispensable references, including for my "cohomology students", who are just as happy as everyone else to find them ready when they need them.

With the mastery of cohomological ideas and techniques they had acquired through their work with me and the seminars they had attended or participated in, writing this seminar through their joint efforts represented a task of derisory dimensions, if we compare it with the service that was being rendered to the famous "mathematical community", or perhaps also, later on, with an obligation of loyalty they might feel towards me. I've already said that for me (the one with the helping hand), it must have been a job of the order of a few months to write the entire seminar. By dividing the work between the five of them, with the writing experience they had each acquired in those years, and with my detailed manuscript notes at their disposal, the investment for each of them was of the order of a month or two at the very least. They were much better equipped to do this than other editors, such as Bucur, who would have liked nothing better than to entrust a task, which was clearly beyond him, to younger, more directly motivated hands.

As long as I was around (so in the three years that followed), I can see how a "leave it to me" reflex might have come into play - I was supposed to coordinate everything and deal with the "volunteers". It's likely that if I'd asked each of them to give two or three presentations within a short space of time, it was up to me to do the same, and finally get it over with, they wouldn't have recused themselves. It was from the moment I withdrew from the mathematical world that the situation changed completely. They

p. 345 found themselves **the sole trustees of a certain inheritance**, both implicit (in the absence of a will) and very concrete. It's true that, from a practical point of view, my departure was tantamount to a **disappearance** - I was indeed "deceased", in the sense that there was no one outside them to know about the inheritance, to be able to use it and to be concerned (for better or for worse. . .) about its fate.

If, for the seven years following my departure, this heritage remained hidden (apart from "the good reference" in 1976!), it's because **my students didn't want it to become public during that time**. All things considered, the situation doesn't seem very different to me from that of the "yoga of motives", which was thoroughly known only by Deligne (apart from myself), and which he saw fit to keep secret for his own benefit. If there is a difference at first sight, it is that in this case there is only one "beneficiary" instead of five, and that there is no common measure between the depth of what was, concealed by one, and what was jointly concealed by the five.

I certainly don't know everyone's deeper motivations - even in Deligne's case I have an apprehension that remains hazy and no doubt will remain so. But on a "practical" level, Deligne's game (with the SGA 4 "s operation - and all the rest) is quite clear. And what's also clear is that these operations couldn't have been carried out **without everyone's solidarity**. It seems to me that Jouanolou isn't too much in the picture - he doesn't strike me as a "luminary", I have the impression that he has long since left the cohomological quagmires (85_1). But I can't imagine Illusie and Berthelot not having had their hands on both SGA $4\frac{1}{2}$ and "la good reference", and they can read as well as me and are no stupider than I am.

If Illusie suddenly became involved in the publication of SGA 5, at the precise moment when Verdier used, where

where Deligne needs a logistical base for his famous SGA $4\frac{1}{2}$ (by unpacking it)
the two seminars from which this text and all his work derive), whereas Illusie had

[83](*) Between the 1960s and 1970s, I had to work at an average rate of a thousand pages a year of texts (EGA, SGA, articles), almost all of which were to become standard references (something that was quite clear to me when I wrote them, or when I encouraged a collaborator to do so with my assistance).

had ten years to do it, it's surely no coincidence. If the closing presentation on open problems and conjectures that I avais made in 1966 "has unfortunately not been written, any moreover [sic] than p. 346
his very fine introductory talk, which reviewed the formulas of Euler-Poincaré and Lefschetz in various contexts (topological, complex analytic, algebraic)", it's surely no coincidence either - but that's a funeral I don't know anything about. And it's no coincidence either that it seemed as natural to Illusie as it did to Deligne (and just worthy of a mention in passing among the "changes of detail") to amputate the seminar from one of its key presentations, which passes into SGA $4\frac{1}{2}$ without further ado[84] (*).

I don't know what were the intentions (conscious or unconscious) of Luc Illusie, whom I like like Pierre Deligne, and who (like him) has always shown me great kindness[85] (**). But I've noticed that, alongside Deligne, he's become the co-actor of a **shameless mystification**: that which passes off the SGA 5 mother seminar of 1965/66 (the very one in which Deligne first heard of schemas, stale cohomology, duality and other "digressions") as a kind of shapeless, vaguely ridiculous appendix to a collection of texts with the misleading name SGA $4\frac{1}{2}$ written eight years later, which pretends to present itself as anterior (both by the number in its title, and by the number of publication in the Notes readings, and finally by the author's unusual comment "Its existence (of SGA $4\frac{1}{2}$) will soon allow us to publish SGA 5 **as it stands**" - emphasis added) - and which, moreover, affects to treat with undisguised disdain the works from which this meagre collection is entirely derived.

Without these works, treated with this beautiful casualness, **none** of Deligne's great works, which are the foundation of his career, would have been possible.
its well-deserved prestige, would not be written now, nor in a hundred years (and the same without doubt for Illusie and my other cohomology students). There is in the esprit of this "SGA 4 V operation a p. 347
impudence, of which Illusie is the guarantor (without even realizing it, no doubt), and which could only have been displayed with the tacit approval of a **consensus**. The first people involved in this consensus, apart from Deligne himself, are the very people who were my students and the main beneficiaries of a certain heritage, handed over before their very eyes to the vagaries of the jockeying for position and disdain.

And those airs of peremptory smugness, those paternal, protective airs that I was able to appreciate in my ex-student as recently as the day before yesterday in our telephone conversation[86] (*), and also those more discreet airs of condescension that I was able to appreciate in my friend Pierre in the aftermath of the brilliant double operation "SGA $4\frac{1}{2}$ - SGA 5" (of which I was far from having the slightest suspicion at the time and for another seven years) - these airs are **not the** products of solitude, but the signs of a consensus that has **never been called into question**. These tunes tell me something not only about Verdier and Deligne, but also about all those who were my students, and before all others, about those who were (by virtue of their work themes and the tools they wielded every day) the first to be concerned.

The term "mystification", which came to me without having sought it out, opportunely reminds me of that other mystification, in which the same cynicism is on display - that of the so-called "Pervers" Colloquium. The two now seem **intimately, indissolubly linked - it's the same spirit that made both possible**. With the possible exception of Jouanolou, who is no longer so much a part of the "big world", I consider these same ex-students to be the same.

[84](*) (May 16) In fact, as I discovered the very next day (see note n° 87), there had been a veritable "massacre" of the mother seminary (or father!) SGA 5, at the hands of Verdier, Deligne and Illusie.

[85](**) Even after I left in 1970, Illusie showed me a lot of kindness - for a long time he sent me beautiful Christmas cards at the end of the year. I'm afraid I didn't have to write back very often to thank him and give him some sign of life - these signs of a faithful friendship came to me like messengers from a past that seemed so infi nitely distant, and with which I'd lost touch.

(May 16) On the other hand, there has been no inclination on Illusie's part to continue or resume contact on a mathematical level, and even last year (when I contacted him about mathematical questions) I sensed his reluctance. In the fourteen years since I left, I have received one and only one offprint from him, dated 1979.

[86](*) For this conversation, see the note "Jokes - or 'weight complexes'" (n° 83).

cohomologists jointly and severally responsible for this disgrace. As far as Berthelot and Illusie are concerned, nothing allows me to prejudge malice or bad faith (which cannot be doubted in the case of Verdier as in that of Deligne). But at the very least, I note a blindness, a blockage in the use of healthy faculties, the underlying reason for which, of course, escapes me. If it weren't for a deliberate intention of indifference and disdain, surely Zoghman Mebkhout, as the only person in the '70s to openly claim to be an admirer of my work, and on subjects that were close to both of them (without that they deign to notice it), would have had the benefit of the minimum "favorable prejudice" so that they would at least be aware so soit little of what he does, and hence realize the interest of the direction
p. 348 in which he had been engaged since 1974, an interest that was **obvious**! Neither of them bothered to notice anything, coming from a vague stranger who still pretends to be a Grothendieck. I don't know if they've opened it, or if they've gone through the shorter, more digestible texts that explain what it's all about - in any case, they haven't deigned to acknowledge receipt of it (nor has Deligne, who obviously sets the tone).

That didn't stop them and the other participants in the memorable Colloque[87] (*) from learning about the remarkable "Riemann-Hilbert correspondence", without the slightest thought of questioning its origin or authorship, or at least (as solid mathematicians) where it was demonstrated (85). But here I trust that Deligne was happy to explain this demonstration, which is surely quite obvious to people like them - precisely the kind of demonstration, using Hironaka-style resolution of singularities, that they learned a long time ago from none other than me (85_2). Riemann-Hilbert, Hironaka abracadabra - that was it!

Clearly, like Verdier and Deligne, they've completely forgotten what **mathematical creation** is all about: a vision that gradually unravels over months and years, bringing to light the "obvious" thing that no-one had seen, taking shape in an "obvious" statement that no-one had thought of (even though, in this case, Deligne had been trying in vain for a whole year. . .).) - and which anyone can then demonstrate in five minutes, using the ready-made techniques he had the advantage of learning sitting on the benches of a distant seminar he doesn't deign (or hasn't kept) to remember. ...

If I've spoken bluntly of Berthelot and Illusie, it's not because I particularly want to smear them (after an initial settling of scores with their two friends). I know they're no "worse" or dumber than most of their dear colleagues or me, and that the lack of flair and sound judgment that I see in them in this instance (and sometimes also, that of the necessary respect for others. . .) is by no means inveterate, but the effect of a **choice**. This choice undoubtedly offered **them returns** that pleased them - and may have done so.

Perhaps this other "return" that comes with my reflection will be unwelcome to one or the other. If it were, it would simply be that he's still reproducing the same choice, which is also that of operating on a tiny part of his faculties, even if it means mistaking bladders for lanterns and vice versa, and hopelessly confusing
p. 349 empty nuts (from the boyfriend) and full nuts (from a vague stranger). To each his own! (⇒ 86, 87)

Note 85_1 Jouanolou is the only one of my students, along with Verdier, who did not publish his thesis. This seems to me to be a sign of disaffection with the foundational work he had developed, namely that of χ -*adique* cohomology from the point of view of derived categories. Since most of his work on this theme took place **after** my departure, i.e. at a time when my students, Deligne and Verdier in particular, were in the process of completing their studies, I'd like to take this opportunity to thank him.

[87](*) (June 12) I have since learned that neither of them took part in this Colloque (Luminy, June 1981). See, however, the note "La mystifi cation", n° 85'.

head, had signaled a general disaffection with the ideas I had introduced into homolo- gical algebra, particularly that of the derived category, the context hardly encouraged Jouanolou to identify with his work and do him the (well-deserved) honor of publishing it. As these same Deligne and Verdier, in the wake of the work of Zoghman Mebkhout (aka Elève Inconnu (de Verdier) aka élève posthume (de Grothendieck)), have come to discover (with great fanfare and mutual publicity) the importance of derived categories (see notes n° s 75,77,81), Jouanolou's scorned thesis has, since the Colloque Pervers, regained all its topicality ; a relevance it would never have ceased to have, had the development of the cohomo- logical theory of schemas continued as normal after my departure in 1970. A striking detail that illustrates a certain drastic "turn" in Deligne's options after my departure: it was Deligne himself (who had clearly understood the importance of developing the formalism of χ-*adic* cohomology in the context of triangulated categories) who provided Jouanolou with a key technical idea for a formal definition of the triangulated χ-*adic* categories he was studying, an idea that is developed in the thesis. (See my 1969 "Report" on Deligne's work, par.8.)

(May 30) See also, on the subject of Jouanolou's work, the note "Les cohéritiers. . . ", n° 91.

Note 85_2 Significantly, it was in this same SGA 5 seminar that everyone learned this demonstration principle, used to prove the biduality theorem in cohomology.
(in cases where the resolution of singularities is available), that the finitude theorems for the p. 350
$R f^i_*$ without any cleanliness assumption on f , and similarly for <u>*RHom*</u>, $Lf^!$. (These finiteness theorems were also omitted from the published version of SGA 5, to be appended to SGA $4\frac{1}{2}$, without Illusie even seeing fit to point this out in his introduction - I only realize this as I write these lines!) Zoghman, who didn't have the advantage of attending the seminar (he got the "right reference" instead), learned the procedure in another place where I had used it (for De Rham's theorem for smooth schemes on <u>*C*</u>).

He could also learn it from "the good reference", where my demonstrations are copied into the analytical framework, to establish what my students and listeners at SGA 5 have since liked to call the "Verdier duality" (which was known to me before I had the pleasure of making his acquaintance). It all adds up! **The same demonstration** (copied from me at the same time as the statement) is used by Verdier as a title of authorship for a duality he learned nowhere else than in that dislocated and scorned SGA 5 seminar - and it is used **against** Mebkhout, becoming (by its very "obviousness") a (tacit) pretext and means for shamelessly robbing him of the credit for an important discovery.

(May 30) It seems to me that the first time I used singularity resolution à la Hironaka, and understood the extraordinary power of resolution as a demonstration tool, was for a "three spoonfuls" demonstration of the Grauert-Remmert theorem, describing a complex analytic structure on certain finite coverings of a complex analytic space, and the analogous statement in the case of finite-type schemes on C. (It's not impossible that the principle was blown to me, on this very occasion, by Serre). This last result is the main ingredient in the proof of the comparison theorem between stale cohomology and ordinary cohomology (the rest being reduced to unscrewing, thanks to the formalism of $Rf_!$, plus a bit of solving to go from $Rf_!$ to Rf_* . . .)

15.3.3. The mystifi cation

Note! 85' (June 3) In fact, I'm learning that they didn't have to wonder about this paternity, since both Berthelot and Illusie learned the theorem from the mouth of Mebkhout, the first...

in February 1982, the second as early as 1979 (the year Mebkhout defended his thesis). Although neither of them took part in the Colloquium in question, they are nonetheless in solidarity with the mystification that took place at the Colloquium.

p. 351 place at this Colloquium, because it is impossible that they were unaware of the cover-up that took place of Mebkhout's authorship of the theoreme du bon Dieu. I can imagine that, along with all the other Colloque participants, they were the first to be fooled by the collective mystification organized by their friends Verdier and Deligne (a mystification in which four of my five cohomology students appear to be involved). As far as Illusie is concerned, at least, I was struck, during a telephone conversation with him after Mebkhout's visit to my home last summer, by how little he obviously thought of him - he was quite astonished (almost pained on the part of his old master, in whom he would surely have expected better judgment... .) to see me give Mebkhout a leading role in restarting the cohomological theory of algebraic varieties. Consensus of considerable force had decided to rank Mebkhout among the vague unknowns, and my friend Illusie blithely lived with this triple contradiction, without asking himself any questions: the leading role played by the theorem of the good God and the philosophy that goes with it; the evasion surrounding the authorship of these things (an evasion in which he himself participates in the company of many); and the low esteem he has for the format and role of Mebkhout (who he knows perfectly well is the unnamed author of these things, which have renewed a field of mathematics in which he, Illusie, is a figure of eminence).

I find here again the complete blockage of common sense and sound judgment, even in something as seemingly impersonal as judgment on scientific matters, a blockage to which I have had occasion to allude more than once already, and which each time again baffles me. And this contradiction I see here in Illusie's (and surely many others') relationship with Mebkhout, my "posthumous pupil", is surely no more than one of the many effects of a more crucial contradiction to be found in his relationship with me. It is this contradiction, in him more particularly and in my other pupils too, that becomes increasingly clear in the reflection pursued in the notes of the present procession to the Funeral, formed by my pupils of yesteryear... .

15.3.4. The deceased

Note 86 (May 11) As is so often the case, it was with some reluctance that I set about this task.
new reflection, on the theme "SGA 5 - SGA $4\frac{1}{2}$ - Perversity", which might have seemed to have been examined and
re-examined over and over again: "It's going to make a deplorable impression on a reader who's probably had enough of it ever since he heard about it; It doesn't look elegant at all to go into details again, SGA 5 this SGA $4\frac{1}{2}$ that, it's
and doesn't deserve any more toast... . ".

p. 352 I'm glad I didn't let myself be intimidated by that well-known refrain, which would like to prevent myself from getting to the bottom of something (at least as far as I'm able to go at the time), on the pretext that it's decidedly "not worth it", that there's nothing to do but let it run its course... . If there have been times when I've discovered things that I consider useful and important, it's always been in moments when I've known not to listen to what presents itself as the voice of "reason", or even "decency", and follow this indecent urge inside me to go and see even what is supposed to be "uninteresting" or of poor appearance, or even messed up or indecent. I can't remember a single time in my life when I've regretted having looked at something a little more closely, against inveterate reflexes that would have prevented me from doing so. These inhibition reflexes were even stronger in Récoltes et Semailles than on other occasions, because this reflection is destined to be made public, which immediately imposes certain constraints of discretion (when I involve third parties), and conciseness (for the sake of the reader). But I don't think so, finally, that these constraints never prevented me from either tackling something I wanted to tackle, or going as far into it as I felt I wanted to. In the cases that may at one time have seemed like borderline cases, I forged ahead with the assurance that, should the need arise, I would always have the resource of not including in Harvest and Sowing anything that would "escape" my indiscreet reflection. These "borderline cases" arose exclusively when I hesitated to involve others, and never when it came to involving myself. But even in the first case, as it happens (and this came as a surprise to me), I never had to make use of this "resource": the text of Récoltes et Semailles represents the complete version of my reflection - at least of the part of this reflection that found its way into writing to express itself.

I feel that with the brief reflection of the previous note[88] (*), the situation has become considerably clearer. I mean that a certain essential aspect of a situation which had been confusing to pleasure, and which I have just evoked by the triple name of a "theme" (SGA 5 - SGA $4\frac{1}{2}$ - Perversity), has appeared to me in full light: that of a "solidarity", a "connivance" which had only been confusedly perceived until then. This doesn't mean that I believe I've fathomed and understood all the ins and outs of a "solidarity", a "connivance" that had only been dimly perceived until then.
complex situation, directly and particularly obviously involving at least seven people: Zogh-
man Mebkhout (acting in a sense as a "revealer" of a certain situation), my five ex-students p. 353
cohomologists, and myself. I don't even flatter myself to have perceived all the springs and motivations that have been at play in my own person, in relation to the "SGA 5 etc. ... . .", in the nearly twenty years since that "unfortunate seminar" took place! But I feel in a much better position than I was yesterday (or even just this morning), to understand and situate the echoes that I hope will reach me on this subject from at least one or other of the main parties involved.

The main question that arises for me (it seems to me that it was already present at another stage of reflection, and now reappears with new vigour) is (it seems to me) this: is what happened with this Burial by my students, (more or less) in their entirety, something completely **atypical**, linked to certain particularities of my person and my singular destiny (such as my departure from the mathematical scene nearly fifteen years ago, the circumstances surrounding it, etc. .) ? Or is it, on the contrary, something "quite natural", due to a simple combination of circumstances - following the principle that "opportunity makes the thief"? I hesitate to think so, though I can't at the moment discern, or even glimpse, what particular aspect of myself has had the virtue of creating such perfect and unanimous **agreement** among my former students, to bury both the "master", and those who claim to be his followers or whose work clearly bears his mark (without, however, being "theirs"). Is it this sort of "aura" of Father that surrounds me, and which I've had occasion to mention? Or is it the challenge posed to each of them by the mere fact of my departure? At the moment, I wouldn't be able to say, for lack of eyes that can see... . Perhaps the coming months will teach me something on this subject[89] (*).

More than once in the last three weeks, I've thought of this other strange "coincidence": it's
that the discovery of the Burial "in all its splendour" (with the four-stroke LN 900 - SGA $4\frac{1}{2}$ -
SGA 5 - Colloque Pervers, then back to SGA 5 and SGA $4\frac{1}{2}$)- that this discovery came at a time when I had just completed an in-depth reflection on my past as a mathematician and my relationship with my students. It was a time, therefore, when I had just come "to terms with myself" on the subject of my students.

of this past, to the best of my ability, and as far as the facts then known to me allowed, as they were rendered by
often hazy memories. Or to put it another way: it was lep . 354

[88](*) This refers to the note "La solidarité" n° 85, dated the same day.
[89](*) (May 30) For a reflection along these lines, see the note "Le Fossoyeur - ou la Congrégation toute entière", n° 97.

the exact moment when I was finally **ready** to learn and profit from it.

Chance" did things so well that there wasn't even a break in the meditation. The reflection that had begun with this short retrospective on the fate of the most important notions (in my opinion) that I had introduced[90] (*) (a reflection that remained in a certain vagueness, where only a certain basic tone emerged insistently... .) - this reflection continued quite naturally on Thursday April 19. It was, it's true, still in the throes of the emotion aroused by that impression of "impudence" (to use the term from earlier, which also aptly describes something I felt at the time), on reading the "memorable volume" LN 900.

In this new departure from "the same" thinking, the main driving force was "the boss" - my self-esteem and sense of decency were affected, and by writing down my emotions I freed myself from them to a certain extent. It was indeed "me", "the boss", who visibly led the dance in the ten days that followed - days marked by the absence of smiles and laughter, by unfailing seriousness. I had to go through this ten-day detour before the reflection returned to the center it had left - to myself. I still remember the relief of that return - like coming out of a tunnel when daylight appears again! It was then that I found myself laughing and smiling again, as if we'd never left each other. It was April 29th. The next day, the 30th, the last day of the month, I was happy to put the finishing touches to this ultimate stage of reflection.

It was also the moment, surely, when I was ready to receive the next "package", this time sent by my friend Zoghman - the "Colloque" package received the day after tomorrow. Today is the tenth day I've been working on assimilating the substance of this package. But at this stage, while I've been gnawing at the bit to get to the bottom of this rebound that just kept on rebounding, the smile hasn't fazed me for a single day. And today, I truly believe (for the thirtieth time, it's true!), is finally the day of closure.

Five days ago I'd already had that same feeling that I'd reached the end of my rope, that there was nothing left but work to be done.

of stewardship: adding a few footnotes here and there, retyping pages too overloaded with ra□tures (each time a sign of a thought that had remained somewhat confused, and which needs to be put into

This seemingly mechanical work, from which the text always emerges with a new face. ...). . . This was when I had just written what is now the note "Mes amis" (n° 79), which spontaneously flowed into "final
p. 355 chords". However, I ended up separating these chords from the beginning of the note. In fact, it turned out that this famous stewardship work had broken down: the "footnotes", typed without line spacing, became real notes (**not** footnotes) of nice dimensions, which had to be retyped with line spacing, and then tried as best I could to fit in here and there. It took days before I realized that another procession, after the one called "The Colloquium", was forming to join the procession - and that the last of the processions would not (as I had decided in my head) be the said Colloquium, but would be led by **the Student**. And just today, as the first procession, reduced to a single note, was enriched by a second ("A feeling of injustice and powerlessness"), I also knew who would lead it: it was "**L'élève posthume**". So the procession, opened by a Pupil (posthumous and lower-case, as befits his humble state) and closed by yet another Pupil (by no means humble this time), seems to me to be complete at last!

It's also time, it seems to me, after a first "false arrival", to return to the chords of a final De Profundis, which are more appropriate today than they were five days ago. Here they are, as I wrote them down then, and which also express my feelings at the moment.

[90](*) See notes "My orphans" and "Refusal of an inheritance - or the price of a contradiction" of March 31 (n° s 46,47).

(May 31) Finally, it was another "false arrival" - the "final agreements" were premature this time too! Twenty days went by, during which the "housekeeping work" continually broke down into rethinking this and that aspect that had been neglected. Six more notes joined the "L' Elève" procession, which was supposed to close the parade. The Fourgon Funèbre appeared in the wake of the Elève, carrying four coffins accompanied by the Fossoyeur. He was definitely lacking to give body and meaning to a funeral convoy that didn't seem to be conveying anyone.

Having become cautious through experience, I'm waiting for events to unfold and won't take any chances for the time being.
to predict whether the procession is finally complete, or whether a forgotten procession will sneak in again at the last minute, so as not to miss the ultimate Ceremony[91] (*). p. 356

15.3.5. The massacre

Note 87 (May 12)[92] (**) For the edification of the somewhat cohomologist reader, and above all for my own, I would like to review the details of this plundering of a splendid seminary, in the hands of two of my former cohomologist students and under the benevolent eye of the others[93] (***) - of the same seminary where they learned, twelve years before anyone else and from the hand of the workman himself, the basics and finesses of the trade that made their reputation.

Two of my oral presentations have never been made available to the public in any form. One is the closing lecture on open problems and conjectures, which "unfortunately was not written up", given how little - and indeed, the author of the introduction to the murder-edition deemed it unnecessary even to mention **what** open problems and conjectures it was. And why should he have taken the trouble, when they were merely problems (which everyone is free to pose as they please!) and conjectures (not even demonstrated!) (87_1). The other was the lecture that opened the seminar, placing it right from the start in a broader context (topological, complex analytic, algebraic) and reviewing formulas of the Euler-Poincaré, Lefschetz, Nielsen-Wecken type, some of which constituted one of the seminar's main applications. The ". . any more than. . . ." with which the author of the introduction follows up to signal, in the nick of time, the disappearance of this presentation, says a lot about the **casual attitude** that was clearly self-evident at the time, even though the author of the seminar had been out of circulation for seven years.

There's a whole series of talks I gave on the formalism of homology and cohomology classes.
logie associated with a cycle (regular ambient pattern in the cohomological case)[94] (****). They have been equally divided: cohomology for Deligne, homology for Verdier - who nevertheless overflows
a little on cohomology, even if it means making the small reference to Deligne with the famous "complexes". p. 357
weight"[95] (*). (Not to mention the finiteness theorem for *RHom* and the biduality theorem, copied verbatim from the seminar - the lion's share of which will go to Deligne, which is to be expected. ...) The author of the introduction does not see fit to mention the homology lectures alone. Indeed, there was no need, since the previous year his friend Verdier had taken on the task of providing the missing "good reference" (without alluding to a seminar, or to me).

[91](*) (June 12) Caution was the order of the day, as a new "Mes élèves" procession separated from the one originally called "L'Elève", which became "L'Elève - alias le Patron".
[92](**) This note follows on from the previous day's reflection on "Solidarity" (n° 85).
[93](***) Further reflection reveals that one of these "others" lent a hand effi ciently to this operation on behalf of others.
[94](****) For details, see note no.° 82 "Good references".
[95](*) See note (83) "The joke - or weight-complexes".

There were oral presentations on finiteness theorems for the operations $R f^{!}_{\wedge}$ (f not proper), and as a corollary, for the operations $\underline{RHom}_*$; and $Lf^!$. The key theorem was proved using a Hironaka-style singularity resolution technique (valid only in cases where the resolution is available).

These arguments, which I used, have come into common use since the seminar (see note (85_2)). Deligne has managed to prove these finitude theorems, as well as the biduality theorem, under other, more helpful hypotheses, which have already been verified in most applications. It might have been expected that he would ask to include these refinements in the seminar where he had the privilege of learning étale cohomology, and the ideas and techniques at the basis of all his subsequent work. But this circumstance is used as a "reason" for amputating this part of the seminar. As for the biduality theorem, under Illusie's pen (and within the framework of the diagrams) it became "Deligne's biduality theorem" (introduction to Lecture I). This was only fair, since in the analytical case, Verdier had already claimed it as his own the previous year (without even having to go to the trouble of finding another demonstration).

Then there's Illusie's paper developing a "generic Kûnneth formula". No one before had thought of developing this kind of statement, inspired by the intuition that "generically" i.e. in the vicinity of the generic point of the basis, a relative scheme behaves like a "locally trivial fiber" in the topological context. In an elegant demonstration similar to the one mentioned above, Deligne manages to eliminate the singularity-resolving hypothesis I had made. It's awarded - presentation deleted and "replaced" by a reference to a presentation by the same Illusie in the so-called "earlier" seminar.
SGA $4\frac{1}{2}$.

p. 358 There is a series of lectures on the formalism of non-commutative traces, developed as a means of explicating the local terms of the Lefschetz-Verdier formula in cases that had never been treated. These lectures were eventually written, it seems, by Bucur, whose manuscript "got lost in a providential move" - it's turning into a vaudeville![96] (*) In the introduction to SGA 5, written by Illusie, these lectures become "Grothendieck's theory of **commutative** traces, generalizing [brilliantly] Stallings'" (which were non-commutative!). The slip of the tongue[97] (**) can only be due to a badly (or too well. . .) inspired secretary, who must have been involved with my friend Ionel Bucur's movers. (The word "brilliantly" is an interpolation of my pen, to better render the thought infallibly suggested by this slip of the tongue, also providential).

I can't complain, since Illusie has taken on the job of redoing the work (and even, he tells us, a "more sophisticated" version, given that it's put in the Beamtique sauce - I seem to remember, though, Illusie, that you made more "sophisticated" innovations than this in my day... .). It must have taken a long time even, if I remember that I spent weeks putting the machine together; my manuscript also got lost in the same providential move, and God knows if one of the dear listeners, overwhelmed by my oral faconde, was at least able to take comprehensible notes... .

Remarkably, and this is something I hadn't noticed before, he doesn't insert this talk in the place in Lecture XI where it was intended (which no doubt also corresponds to the place it had in the oral seminar), preferring to leave a gaping hole there and make his talk an apocryphal one, called "Calculs de termes locaux" ("Calculations of local terms"). The title does, however, seem to correspond to what I seem to remember him doing in the oral seminar.

[96](*) It was this circumstance, no doubt, that inspired Deligne's unexpectedly brilliant criticism of SGA 5, in which the local terms of Lefschetz Verdier's formula (which "remained conjectural", remember!!!) were not even calculated! (See note "la table rase", n° 67, about the absurdity of this criticism, which for an informed reader is similar to that of Verdier's famous "weight complex" the previous year (see note n° 83). So it was Verdier who became the schoolmaster!)

[97](**) This is the slip of the tongue attributing to me the authorship of a theory of "commutative" traces (for which I had not been expected) instead of "non-commutative" ones. That it survived into the published edition is all the more remarkable given that Illusie was perhaps the most meticulous of my students, down to the last detail.

strange. But right from line 1 of his introduction to this talk, the author is quick to detrompt us: "Cetp exposé, written in January 1977, **does not correspond to any oral presentation in the seminar**". And he continues with Lefschetz-Verdier formulas (that name rings a bell, though, and I thought I'd actually developed a theory of non-commutative traces, in order to calculate "local terms" in certain cases. .), then on a formula by Langlands and on a demonstration by Artin-Verdier in 1967 (this was a year after the final agreements of the oral seminar, which must have influenced these authors, at least one if not both of whom followed him). Towards the end of the page, we learn as if in passing, contrary to what was announced at the beginning, that there is also a "second part of this talk, of a much more technical nature" (I've read this language somewhere. . .) which is (admire the nuance) "**inspired by the method used by Grothendieck** to establish the Lefschetz formula for certain cohomological correspondences on curves", with a reference to Lecture XII of the same seminar and above all to the indispensable SGA $4\frac{1}{2}$; Obviously, there was no reason, for so little, to include this lecture in place of the gaping hole - the "more sophisticated version" of earlier will have done things right. It was even nice of Illusie and Deligne to cite me as a source of "inspiration", when the example of their friend Verdier the previous year had clearly shown that there was absolutely no need for such scruples. . 359

I return to Illusie's introduction to the volume that goes by the name of SGA 5. In it, we learn once again - as Deligne had already announced in his introduction to SGA $4\frac{1}{2}$ - that it was indeed **thanks to his friend** that the seminar has finally been published:

"I would like to thank P. Deligne for having convinced me to write, in a new version of Lecture III, a demonstration of the Lefschetz-Verdier formula, **thus removing one of the obstacles to the publication of this seminar**".

Once again, we're in the middle of a farce - repeated as is by the docile Illusie in the introduction to SGA $4\frac{1}{2}$! If the seminar remained unpublished for more than ten years, it's because no one (until Deligne saved the day in 1977) had yet considered the possibility of publishing it.

good idea to write a demonstration of the so-called (and rightly so) "Lefschetz-Verdier formula", of which none other than his inseparable friend and my ex-student Verdier himself has proudly borne the paternity **since aup** . 360

minus 1964 (87_2), i.e. for at least two years by the time my seminar ended, and was just waiting to be made available to everyone!

Finally, as another and last (?) mutilation of the seminar, there was the disappearance of the fine talk Serre had given on the "(Serre-) Swan module" - a talk entitled "Introduction à la théorie de Brauer". It's fortunate that Serre, seeing the turn events were taking, had the good sense to include his talk in his book "Représentations linéaires des groupes finis" (Hermann, 1971), and make it available to the mathematical public.(87_3)

This time, I think, I've come full circle. The picture of the fate of a seminar in which I had put the best of myself (88)[98] (*), and which I find twenty years later unrecognizable, butchered by the very people who had been its exclusive beneficiaries - or at least by three of them, and with the assent of all the other participants.

I don't regret having taken the trouble, once again, to follow through on what had gradually come to my attention. This "return of things"[99] (**) that I noticed, at the end of a long retros-

[98](*) For the meaning of this expression "of the best of myself", see the following notes "La dépouille. . . ", ". . and the body", n° 88, 89. The first of these situates the SGA 5 seminar, with SGA 4 inseparable from it, as the masterpiece of the part of my work "entirely completed".

[99](**) See note of this name (n° 73) dated April 30.

pective about my relationship with one of my former students, sensing even then that he wasn't the only one to "bury me with gusto" - I've only now become aware of his breath, his "smell" (to use an expression that then appeared in one of my dreams) - the breath of **violence**. This breath is concealed and revealed at the same time by the speech[100] (***) (seemingly detached and impassive) presenting a highly technical substance. What this violence is aimed at, through a "corpse" delivered at mercy,
is the very person of the one who was the "master", the "Father" - at a time when the "pupils" have long since taken his envied place, without encountering any resistance; and that also long ago they
p. 361 have elected from among themselves the new "Father", called to replace the old and reign over them.

I feel this breath, and yet it remains for me a foreign thing, misunderstood. To "understand" it, this breath would have to live in me, or have lived in me. But four years ago, for the first time, I felt and measured the significance of something in my life that I had never thought about, that had always seemed self-evident: that my identification with my father as a child **was not** marked by conflict - that at no time in my childhood **did I fear or envy my father**, while at the same time devoting unreserved love to him. This relationship, perhaps the most profound that has marked my life (without my even realizing it before this meditation four years ago), which in my childhood was like a relationship with an other self both strong and benevolent - this relationship was not marked by division and conflict. If, through all my often-torn life, the knowledge of the strength that lies within me has remained alive; and if, in my life by no means free of fear, I have not known fear either of a person or of an event - it is to this humble circumstance that I owe it, ignored until well into my fifties. This circumstance has been a priceless privilege, for it is the intimate knowledge of the creative force within one's own person that **is** also that force, which enables it to express itself freely according to its nature, through creation - through a creative life.

And this privilege, which has exempted me from one of the deepest marks of conflict, is at the moment also like a hindrance, like a "**void**" in my experience of life. A void that's hard to fill, where many others have a rich web of emotions, images and associations, offering them the path (provided they're curious enough to take it) to a profound understanding of others as well as of themselves, in situations that I manage (by dint of repetition and cross-checking) to apprehend as best I can, but in the face of which I remain like a stranger - with the desire for knowledge within me still hungry.

Note 87_1 (87_1) (May 31) This closing presentation, surely one of the most interesting and substan-
tiels with the opening talk, was obviously not lost on everyone, as I see from reading Mac Pherson's paper "Chern classes for singular algebraic varieties" (Chern classes for
singular algebraic varieties, Annals of Math. (2) 100, 1974, pp. 423-432) (received April 1973)-In this paper, under the name of "Deligne-Grothendieck conjecture", I repeat one of the main conjectures I had
p. 362 introduced in this paper in the schematic framework. It is taken up by Mac Pherson in the transcendental framework of algebraic varieties over the field of complexes, the Chow ring being replaced by the homology group. Deligne had learned this conjecture[101] (*) in my talk in 1966, the same year he had appeared in the seminar where he began to familiarize himself with the language of diagrams and cohomological techniques (see the note "L'être à part" n° 67')-It's nice of you to have done me the honor of including me.

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[100](***) These are mainly the introductory texts accompanying SGA 5 (written by Illusie) and SGA 4. [1] 2
(written by Deligne).

[101](*) (June 6) In a slightly different form, see the rest of today's note.
(March 1985) For further details, given by Deligne himself, see the note "Les points sur les i", n° 164 (II 1).

in the name of conjecture - a few years later it would no longer have been appropriate... .

(June 6) I'd like to take this opportunity to explain the conjecture I'd put forward in the seminar in the schematic framework, while surely pointing out the obvious variant in the complex analytic (or even rigid-analytic) framework. I conceived it as a "Riemann-Roch" theorem, but with discrete coefficients instead of coherent coefficients. (Zoghman Mebkhout told me, incidentally, that his view of D-Modules should make it possible to consider the two Riemann-Roch theorems as contained in a single crystalline Riemann-Roch theorem, which would thus represent in zero characteristic the natural synthesis of the two Riemann-Roch theorems I introduced into mathematics, one in 1957, the other in 1966). We fix a ring of coefficients Λ (not necessarily commutative, but noetherian for simplicity and moreover of prime torsion to the characteristics of the schemes under consideration, for the purposes of stale cohomology...). For a scheme X we denote by

$$K.(X, \Lambda)$$

the Grothendieck group formed by constructible etal bundles of Λ-modules. Using the functors $Rf_!$, this group depends functorially on X, for X noetherian and morphisms of schemes that are separate and of finite type. For regular X, I postulated the existence of a homomorphism of groups canonical, playing the role of the "Chern character " in the consistent RR theorem, p. 363

$$ch_X : K.(X, \Lambda) \to A(X) \otimes_7 K.(\Lambda), \tag{15.1}$$

where $A(X)$ is the Chow ring of X and $K.(\Lambda)$ the Grothendieck group formed with Λ-modules of finite type. This homomorphism was to be determined solely by the validity of the "discrete Riemann-Roch formula", for a **proper** morphism $f : X \to Y$ of regular schemes, which formula is written as the consistent Riemann-Roch formula, with Todd's "multiplier" replaced by the total relative Chern class :

$$ch_Y (f_! (x)) = f_* (ch_X (x) c(f)), \tag{15.2}$$

where $c(f) \in A(X)$ is the total Chern class of f . It's not hard to see that in a context where we have the resolution of singularities in Hironaka's strong form, RR's formula does indeed determine the ch_X uniquely.

Of course, we assume that we're in a context where the Chow ring is defined (I'm not aware of anyone having even attempted to write a theory of Chow rings for regular schemes of finite type over a body). Alternatively, we can also work in the graduated ring associated with the usual "Grothendieck" ring $K^{\circ}(X)$ in the coherent context, filtered in the usual way (see SGA 6). Alternatively, we can replace $A(X)$ by the even *l-adic* cohomology ring, the direct sum of $H^{2i}(X, \underline{Z}_l(i))$. This has the disadvantage of introducing an artificial parameter l, and giving formulas that are less purely numerical" fines, while the Chow ring has the charm of having a continuous structure, destroyed by switching to cohomology.

Already in the case where X is a smooth algebraic curve over an algebraically closed field, the calculation of ch_X involves delicate local invariants of the Artin-Serre-Swan type. In other words, the general conjecture is a profound one, the pursuit of which is linked to an understanding of the higher-dimensional analogues of these invariants.

Remark. Designating in the same way by $K^{\cdot}(X, \Lambda)$ "the Grothendieck ring" formed with the construcof finite tor-dimensional Λ-spreads (which ring operates on $K.(X, \Lambda)$ when A is commuta- tive. . .), we must likewise have a homomorphism

$$ch_X : K^{\cdot}(X, \Lambda) \to A(X) \otimes_{\underline{Z}} K^{\cdot}(\Lambda)$$

again giving rise (mutatis mutandis) to the same Riemann-Roch (RR) formula.

p. 364 Let $cons(X)$ now be the ring of constructible integer functions on X. We define less tautological canonical homomorphisms

$$K_{.}(X, \Lambda) \to Cons(X) \otimes_{\underline{Z}} K_{.}(\Lambda) ,$$

$$K^{\cdot\cdot}(X, \Lambda) \to Cons(X) \otimes_{\underline{Z}} K^{\cdot\cdot}(\Lambda) ,$$

If we now restrict ourselves to schemes **of zero characteristic**, then (by using Euler-Poincaré characteristics with proper supports) we see that the group $Cons(X)$ is a <u>covariant</u> functor with respect to finite-type morphisms of noetherian schemes (in addition to being contravariant as a ring functor, which is independent of characteristics), and the preceding tautological morphisms are functorial. (This corresponds to the "well-known" fact, which I believe was not proved in the SGA 5 oral seminar, that in **zero characteristic**, for a locally constant bundle of Λ-modules F on an algebraic scheme X, its image by

$$f_! : K^{\cdot}(X, \Lambda) \to K^{\cdot}(e, \Lambda) \blacksquare\ K^{\cdot}(\Lambda)$$

is equal to $d\chi(X)$, where d *is* the rank of F , $e = Spec(k)$, k the base field assumed to be algebraically closed. . .). This immediately suggests that the Chern homomorphisms ($1_{.}$) and ($1^{\cdot}$) must be derivable from the tautological homomor- phisms ($2_{.}$), ($2^{\cdot}$) by composing with a "universal" Chern homomorphism (independent of any ring of coefficients Λ)

$$ch_X : Cons(X) \to A(X) ,$$

so that the two "Λ-coefficient" versions of the RR formula appear to be formally contained in an RR formula at the level of constructible functions, which is always written in the same form.

When working with schemes on a fixed basic body (again, of any characteristic), or more generally on a fixed **regular** basic scheme S (for example $S = Spec(\underline{Z})$), the form of the Riemann-Roch formula most in line with the usual writing (in the coherent framework familiar since 1957) is obtained by introducing the products

$$ch_X(x)c(X/S) = c_{X/S}(x)$$

p. 365 (where X is in a $K(X, \Lambda)$ or $K^{\cdot}(X, \Lambda)$ indifferently), which we might call **the Chern** class **of** x **relative to the basis** S. When x is the unit element of $K^{\cdot}(X, \Lambda)$ i.e. the class of the constant bundle of value Λ, we find the image of the total relative Chern class of X *with* respect to S, by 1 "canonical homomorphism of $A(X)$ into $A(X) \otimes K^{\cdot}(\Lambda)$. This being the case, RR "s formula is equivalent to the fact that the formation of these relative Chern classes

$$c_{X/S} : K_{.}(X, \Lambda) \to A(X) \otimes K_{.}(\Lambda) ,$$

for a regular variable scheme X over S (of finite type over S), with S fixed, is functorial by with respect to eigenmorphisms, and similarly for the variant ($5^{\cdot}$). In null characteristic, this reduces to the functoriality (for eigenmorphisms) of the corresponding application

$$c_{X/S} : Cons(X) \longrightarrow A(X) \; .$$

It is in this form of the existence and uniqueness of an absolute "Chern class" application (6), in the case where $S = Spec(\mathrm{C})$, that the conjecture in Mac Pherson's work is presented, the relevant conditions (here as in the general zero characteristic case) being a) the functoriality of (6) for proper morphisms and b) we have $c_{X/S}$ (1) = $c(X/S)$ (in this case, the total "absolute" Chern class). Compared with my initial conjecture, however, the form presented and proved by Mac Pherson differs in two ways. One is a "minus", in that it is placed, not in the Chow ring, but in the whole cohomology ring, or more precisely the whole homology group, defined by transcendental means. The other is a "plus" - and it is here perhaps that Deligne has made a contribution to my initial conjecture (unless this contribution is due to Mac Pherson himself[102] (*)). It's that for the existence and uniqueness of an application (6), we don't need to restrict ourselves to regular schemes X, provided we replace $A(X)$ by the entire homology group. The same is likely to be true in the general case, where we denote by $A(X)$ (or better, by $A_{\cdot}(X)$) the **Chow group** (which is no longer a ring in general) of the noetherian scheme X. Or to put it another way: while the heuristic definition of *ch-invariants*$_X$ (x) (for x in $K_{\cdot}(X, \Lambda)$ or $K^{\cdot}(X, \Lambda)$) makes essential use of the assumption that the ambient scheme is regular, as soon as we multiply it by
by the "multiplier" $c(X/S)$ (when the scheme X is of finite type over a fixed regular scheme S), the product obtained (4) seems to retain a meaning without any assumption of regularity about X, as an element of a tensor product p. 366

$$A_{\cdot}(X) \otimes K_{\cdot}(\Lambda) \text{ or } A_{\cdot}(X) \otimes K^{\cdot}(\Lambda) \; ,$$

where $A_{\cdot}(X)$ denotes the Chow group of X. The spirit of Mac Pherson's demonstration (which does not use singularity resolution) would suggest the possibility of an explicit "computational" construction of the homomorphism ($5_{\cdot}$), by "making do" with the singularities of X as they are, as well as with the singularities of the coefficient bundle F (whose class is x), to "collect" a cycle on X with coefficients in $K_{\cdot}(\Lambda)$. This would also be in the spirit of the ideas I had introduced in 1957 with the coherent Riemann-Roch theorem, where I did self-intersection calculations in particular, taking care not to "move" the cycle under consideration. A first obvious reduction (obtained by immersing X in an S-scheme) would be to the case where X is a firm subscheme of the regular *S-scheme*. . .

The idea that it should be possible to develop a **singular** (coherent) Riemann-Roch theorem was familiar to me, I can't say how long ago, but I never tried to test it seriously. It was more or less this idea (apart from the analogy with the "cohomology, homology, cap-product" formalism) that led me in SGA 6 (in 1966/67) to systematically introduce the $K_{\cdot}(X)$ and $K^{\cdot}(X)$ and the $A_{\cdot}(X)$, $A^{\cdot}(X)$, instead of just working with the $K^{\cdot}(X)$. I don't remember whether I also thought of something like this in the SGA 5 seminar in 1966, and whether I hinted at it in the oral presentation. As my handwritten notes have disappeared (in a removal perhaps?) I'll probably never know... .

(June 7) Looking through Mac Pherson's article, I was struck by the fact that the word "Riemann-Roch" is not mentioned - which is why I didn't immediately recognize the conjecture I made in the SGA 5 seminar in 1966, which was for me (and still is) a "Riemann-Roch" theorem. It seems that when Mac Pherson wrote his article, he didn't even realize that it was a "Riemann-Roch" theorem.

[102](*) (March 1985) This is indeed the case, cf. note no.° 164 cited in the previous footnote.

of this obvious kinship. I presume that the reason for this is that Deligne, who after my departure put this conjecture into circulation in the form he liked, took care as far as possible to "erase" it.

the obvious kinship with the Riemann-Roch-Grothendieck theorem. I think I sense his motivation for doing so. On the one hand, it weakens the link between this conjecture and myself, and makes it more plau sible to call-

p. 367 the "Deligne-Grothendieck conjecture" under which it is currently circulating. (NB I don't know if it's in circulation in the schematic case, and if so, I'd be very curious to know under what name). But the deeper reason seems to me to lie in his obsessive desire to deny and destroy, as far as possible, the fundamental unity of my work and my mathematical vision[103] (*). This is a striking example of how, in a mathematician of exceptional means, a fixed idea entirely unrelated to any mathematical motivation, can obscure (or even completely block out) what I have called the "healthy mathematical instinct". This instinct cannot fail to perceive the analogy between the two "continuous" and "discrete" statements of the "same" Riemann-Roch theorem, which I had of course emphasized in the oral presentation. As I indicated yesterday, this kinship will no doubt soon be confirmed by a statement in form (conjectured by Zoghman Mebkhout), at least in the complex analytic case, allowing both to be deduced from a common statement. Clearly, given Deligne's "gravedigging" attitude towards the Riemann-Roch theorem[104] (**), he was not likely to discover the unique statement that links them in the analytic framework, and even less likely to raise the question of an analogous statement in the general schematic framework. Nor was he able, in such circumstances, to identify the fruitful point of view of D-Modules in the cohomological theory of algebraic varieties, arising all too naturally from ideas that had to be buried - or even to recognize, for years, the fruitful work of Mebkhout, succeeding where he himself had failed.

Note 87_2 (May 31) This is the year of my Bourbaki lecture on the rationality of *L-functions,* in which I heuristically use Verdier's result (? ? ?) (and especially the expected form of local terms in the case of d'espèce), without waiting for Illusie to demonstrate it thirteen years later, at Deligne's invitation. It seemed to me, moreover, when Verdier showed me his ultra- general formula that came as a surprise,

p. 368 that he demonstrated it with a few lines of "six operations" formalism - this is the kind of formula where (almost) to write it is to demonstrate it! If there was any "difficulty", it could only be at the level of verifying one or two compatibilities[105] (*). What's more, both Illusie and Deligne knew perfectly well that the demonstrations I had given in the seminar for various explicit trace formulas **were complete**, and in no way depended on Verdier's general formula, which had simply acted as a "trigger" to encourage us to make explicit and prove trace formulas in as general a range of cases as possible. The bad faith of both is obvious here. As far as Deligne is concerned, it was already clear to me when I wrote the note "La table rase" (n° 67) - but probably not to an uninformed reader, nor of course to an informed reader who renounces the use of his healthy faculties.

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[103](*) Compare with the commentary in the note "La dépouille" (n° 88) on the deeper meaning of the SGA 4 operation $\frac{1}{2}$, similarly aimed at shattering into an amorphous set of "technical digressions" the deeper unity of my work around staggered cohomology, through the "violent insertion" of the foreign text SGA 4 $\frac{1}{2}$ between the two indissoluble parts SGA 4 and SGA 5 which develop
this work.

[104](**) These dispositions, with regard precisely to the Riemann-Roch-Grothendieck theorem, are particularly clear in the "Funeral Eulogy"; see the note "Funeral Eulogy (1) - or compliments", n° 104.

[105](*) (June 6) It would also appear that, via the biduality theorem (in the meantime promoted to "Deligne's theorem"), the initial demonstration of the Lefschetz-Verdier formula depended on a singularity resolution hypothesis, which Deligne manages to dispense with in the case of fi ni type schemes over a body. This is a good opportunity to fish in troubled waters and give the impression that SGA 5 is subordinate to the "seminar-sic" SGA 4 $\frac{1}{2}$ which "precedes" it (and which was published well and truly before...).
him!).

(June 6) As for Illusie, he played right into his friend's hands, trying to muddy the waters to give the appearance of an ultra-technical oral seminar that didn't even give complete demonstrations of all the results, especially the trace formulas. These were, however, demonstrated there (and for the first time) in 65/66, and it was there that both he and Deligne had the privilege of learning them,
and the delicate technique that goes with it[106] (**).

This reminds me that, of course, I had taken the trouble to demonstrate Lefschetz-'s formula. p. 369
Verdier in the seminar - it was the least I could do, and a particularly striking application of the formalism of local and global duality that I set out to develop. The question came to me these days: why on earth, when there were ten or so papers still being written by my dear students, so that Deligne and Illusie were spoilt for choice when it came to naming their technical "obstacle" to the publication of SGA 5, they chose the theorem of their good buddy Verdier, who at the time was taking credit for it as his own, just as he had never bothered to write (or at least make available to the public) the theorem of derived and triangulated categories. There's a kind of **defiance** in the absurdity (or in a kind of collective cynicism in the group of my ex-cohomology students, whom I consider to be in solidarity in this operation-massacre), which reminds me of the "weight-complexes" brilliantly invented by Verdier the previous year (see the note of that name, n° 83), or (in the iniquitous register) with the "perverse" name given by Deligne to bundles that should be called "Mebkhout bundles" (see the note "La Perversité", n° 76). I sense such inventions as acts of domination and contempt towards the entire mathematical community.
- and at the same time a **gamble**, which was clearly won until the unexpected appearance of the deceased, who appears almost as the only one awake in a community of sleepers...

note 87_3 (June 5) After this assessment of a massacre, we will appreciate the value of Illusie's statement in line 2 of his introduction to the volume entitled SGA 5:

> "Compared with the original version, the only significant changes concern Lecture II [generic Kûnneth for- mules], which has not been reproduced, and Lecture III [Lefschetz- formula], which has been reproduced.
> Verdier], which has been completely rewritten and expanded with an appendix numbered III B[107] (*). A part some changes of detail and additions of footnotes, the other presentations have been p. 370
> left **as they are**" (emphasis added).

Here again, Illusie complacently echoes another well-sent joke from his inenarrable friend, namely that the existence of SGA $4\frac{1}{2}$ "will soon make it possible to publish SGA 5 **as is**" (see the "Clean slate" note n° 67) - and Illusie does his utmost in his talks and introductions to lend credence to this.

[106](**) In the second paragraph of the Introduction to the volume published as SGA 5, Illusie presents as "the heart of the seminar" the three lectures III, III B, XII on Lefschetz's formula in stale cohomology, whereas we have seen that in the introduction to lecture III B., he takes care to specify (contrary to reality) that "this lecture corresponds to no oral lecture of the seminar" and that in the introductions to lectures III and III B., he does his utmost to give the impression that these are "the heart of the seminar", he is careful to specify (contrary to reality) that "this lecture does not correspond to any oral lecture of the seminar" and that in the introductions to lectures III and III B, he does his utmost to give the impression that these are subordinate to SGA $4\frac{1}{2}$, and that lecture III is presented as "conjectural"! ! In fact, the entire SGA 5 seminar was technically independent of Lecture III (Lefschetz-Verdier formula), which played the role of a heuristic motivator, and Lecture III B is no more than the "hole" (Lecture XI) created by the move to Bucur, which was the welcome pretext for this further dismemberment.

To lend credence to the version of a seminar of "technical digressions" (blown up by his friend Deligne), Illusie was careful to skip the introductory presentation, in which I had painted a preliminary picture of the main themes that were to be developed in this seminar, a picture in which the trace formulas form only a small part (taking on particular importance because of their arithmetical implications, in the direction of Weil's conjectures). For an overview of these "major themes", see sub-note n° 87_5

[107](*) Which is presented as part of the "heart of the seminar"! (See previous b. de p. note.)

imposture (that SGA 5, where he and his friend learned their trade, would depend on the pirate-volume SGA $4\frac{1}{2}$, made of bries and bracs gleaned or plundered over the following twelve years), by a luxury of cross-references to SGA $4\frac{1}{2}$ every page turn...

The final word goes (as it should) to Deligne, who wrote to me a month ago (May 3), in response to a laconic request for information (see the beginning of the note "Les Obsèques", n° 70):

> "In short, if it had been seven years since you were doing maths [?!] when this SGA 4 text$\frac{1}{2}$ appeared, this simply corresponds [?] to the long delay in editing SGA 5, **which was too incomplete to be usefully published as it stood**.
>
> I hope these explanations please you."

If they haven't "approved" me, at least they've edified me. . .

Note 87_4 (June 6) Perhaps it's time to indicate what the main themes were that were deve- loped in the oral seminar, and of which the published text gives an idea only by cross-checking.

I) Local aspects of duality theory, whose essential technical ingredient is (as in the coherent case) the biduality theorem (supplemented by a "cohomological purity" theorem). I have the impression that the geometrical meaning of the latter theorem, as a local Poincaré duality theorem, which I had explained so well in the oral seminar, has since been entirely forgotten by my former students[108] (*).

II) Trace formulas, including "non-commutative" trace formulas more subtle than the formula of the usual traces (where both members are integers, or more generally elements of the ring of coefficients, such as Z/nZ or an l-adic ring Z_l , or even Q_l), placing ourselves in the algebra of a finite operating group on the considered scheme, with coefficients in a suitable ring (such as those considered in the previous
p. 371 parenthesis). This generalization came very naturally, since even in the case of Lefschetz formulas of the usual type, but for "twisted" bundles of coefficients, we were led to replace the initial scheme by a Galois coating (usually branched) serving to "untwist" the coefficients, with the Galois group operating on it. Nielsen-Wecken" type formulas are thus naturally introduced into the schematic context.

III) Euler-Poincaré formulas. On the one hand, there was a detailed study of an "absolute" formula for algebraic curves, using Serre-Swan modules (generalizing the case of moderately branched coefficients, giving rise to the more naive Ogg-Chafarévitch-Grothendieck formula). On the other hand, there were novel and profound conjectures of the "discrete" Riemann-Roch type, one of which reappeared seven years later, in a hybrid version, under the name of the "Deligne-Grothendieck conjecture", proved by Mac Pherson by transcendental means (see note n° 87_1).

The comments I couldn't fail to make on the profound relationships between these two themes (Lefschetz for- mules, Euler-Poincaré formulas) were also lost without trace. (As was my habit, I left all my handwritten notes to the volunteer-writers, and no written trace remains of the oral seminar, of which I did, of course, have a complete set of handwritten notes, even if some of them were succinct).

IV) Detailed formalism for homology and cohomology classes associated with a cycle, derived naturally from the general duality formalism and the key idea of working with the cohomology "with supports" in the cycle under consideration, using cohomological purity theorems.

[108](*) This geometrical interpretation has at least been preserved in Illusie's writing.

V) Finiteness theorems (including generic finiteness theorems) and generic Kûnneth theorems for cohomology with any support.

The seminar also developed a technique for passing from torsion coefficients to l-adic coefficients (lectures V and VI). This was the most technical part of the seminar, which as a rule worked with torsion coefficients, and then "go to the limit" to deduce the corresponding l-adic results. dants. This point of view was a pro visory pis-aller, pending Jouanolou's thesis (still unpublished at (at present) giving the formalism that was needed directly in the l-adic framework. p. 372

I don't include among the main "themes" the calculations of some classical schemes and the cohomological theory of Chern classes, which Illusie highlights in his introduction as "one of the most interesting" of the seminar. As the program was full, I hadn't felt it necessary to dwell on these calculations and this construction in the oral seminar, as it was sufficient to repeat, virtually verbatim, the reasoning I had given ten years earlier in the context of Chow rings, on the occasion of the Riemann-Roch theorem. On the other hand, it was obvious that it had to be included in the written seminar, to provide a useful reference for the user of étale cohomology. Jouanolou took on this task (Lecture VIII), which he had to regard not as a service to the mathematical community, while learning basic techniques essential for his own use, but as a chore, since its writing dragged on for years[109] (*). It was no different, it seems, for his thesis, which remains a ghostly reference just like Verdier's. ... The "passage à la limite" section shouldn't be counted as one of the seminar's "main themes" either, in the sense that it isn't associated with any particular geometrical idea. Rather, it reflects a technical complication peculiar to the context of stale cohomology (distinguishing it from transcendental contexts), namely that the main theorems on stale cohomology concern in the first place **torsion** coefficients (prime to residual characteristics), and that in order to have a theory that corresponds to rings of coefficients of zero characteristic (as is necessary for Weil's conjectures), we need to go to the limit on rings of coefficients Z/l^n Z to obtain "*l-adic*" results.

All this said, the only one of the five main themes of the oral seminar that appears in complete form in the published text is theme I. Themes IV and V have simply disappeared, absorbed by SGA 4. Themes IV and V have disappeared altogether, absorbed by SGA $4\frac{1}{2}$, with the advantage of being able to refer to them extensively and give the impression that SGA 5 depends on a text by Deligne that appears to predate it. Themes II and III appear in the published volume in mutilated form, still maintaining the same imposture of dependence on the SGA 4 text$\frac{1}{2}$ (which in reality emerged entirely from the mother seminar SGA 4, SGA 5).

15.3.6. The skinning

Note 88 (May 16) The set of two consecutive seminars SGA 4 and SGA 5 (which for me are like **a** single "seminar") develops from nothingness both the powerful instrument of synthesis and discovery represented by the **language of** topos, and the perfectly perfected, perfectly effective **tool of** stale co-homology - better understood in its essential formal properties, from that moment on, than even the cohomological theory of ordinary spaces[110] (*). 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 p. 373

[109](*) (June 12) Going through the presentation in question, I was convinced of Jouanolou's perfect connivance with my other cohomology students.

[110](*) Even if we restrict ourselves to the spaces closest to "varieties", such as triangulable spaces.

Obviously, this work represents the most far-reaching technical "tour de force" I've accomplished in my work as a mathematician[111] (**). For me, these two seminars are indissolubly linked. They represent, in their unity, both the **vision** and the **tool** - the topos, and a complete formalism of scalar cohomology.

While the vision is still rejected today, the tool has, over the last twenty years or so, profoundly re-imagined algebraic geometry in its most fascinating aspect for me - the "arithmetic" aspect, apprehended by an intuition, and by a conceptual and technical baggage, of a "geometric" nature.

It was certainly not only the intention of suggesting that his cohomological "digest" **had anteriority** over the SGA 5 part that motivated Deligne to give it the misleading name SGA $4\frac{1}{2}$ - nothing prevented him from doing so.
after all, while we're at it, to call it *SGA* $3\frac{1}{2}$! In "Operation SGA $4\frac{1}{2}$ " I sense the intention to present
the work from which all his own work stems (this work from which he cannot detach himself!) - a work of obvious and profound unity, clearly apparent in the two seminars, SGA 4 and (the real) SGA 5, as
thing **divided** (as itself is divided. . .), **cut in two** by this violent insertion of a foreign and disdainful text; of a
p. 374 text that would like to present itself as the living heart, the quintessence of a thought, of a
vision in which he had no part[112] (*), and the two "quarters" that surround it as a sort of vaguely grotesque appendix, like a collection of "digressions" and "technical complements" to the work that Deligne claims to be central and essential, and in which my humble self is graciously admitted (before total burial) to the number of "collaborators"[113] (**).

Chance" had done it right. This "corpse left to mercy - this "unfortunate seminar" always left behind by the "editors", and at the time of my departure in the hands and at the discretion of my cohomology students - this was not **just any** part of the master's work! It wasn't SGA 1 and SGA 2 (where I was developing, in my own corner and without even realizing it, the tools that were to be the two essential technical aids for the "take-off" of the main work to come), nor SGA 3 (where my contribution consisted mainly of incessant - and sometimes arduous - scales and arpeggios to hone the "all-out" technique of schematics), nor SGA 6 (systematically developing my ten-year-old ideas on the Riemann-Roch theorem and the intersection formalism), or even SGA 7 (which, through the inner logic of reflection, stems from the possession of the central tool, mastery of cohomology). It is indeed the **main part of** my work, the writing of which had remained unfinished (and by their care. . .), that I have left, at least in part, in the hands of my cohomology students. It was this main part of the work that they chose to massacre and appropriate as their own, forgetting the unity that is their meaning and beauty, and their creative virtue (90).

And it's no coincidence either that, equipped with heterogeneous tools and denying the spirit and vision that had brought them into being from nothing, none were able to discern the innovative work where it was being reborn, against their indifference and disdain. Nor that after six years, when at the end of the line the new tool was finally
apprehended by Deligne, they unanimously buried the one who had created it in solitude - Zoghman
p. 375 Mebkhout, the disowned master's posthumous pupil! And it's no more a coincidence that, after the momentum had died down
Deligne's initial work (which in just a few years had led him to launch a new theory of

[111](**) Some difficult or unforeseen results were obtained by others (Artin, Verdier, Giraud, Deligne), and some parts of the work were done in collaboration with others. This in no way detracts (in my mind at least) from the strength of my appreciation of the place of this work in my body of work. I intend to come back to this point in more detail, in an appendix to the Thematic Outline, and to dot the i's and cross the t's where it's obviously become necessary.

[112](*) This line of thought had reached full maturity, in terms of both key ideas and essential results, even before the young man Deligne appeared on the scene to learn algebraic geometry and cohomological techniques from me, between 1965 and 1969.
(May 30) On this subject, see the note "L'être à part", n° 67'.

[113](**) See notes "Le feu vert" , "Le renversement", n° s 68, 68'.

Hodge, and towards the demonstration of Weil's conjectures), and despite its prodigious means and the brilliant means of my cohomology students, I note today this "morose stagnation" in a field of prodigious richness where everything still seems to be to be done. This should come as no surprise, when for nearly fifteen years the main source of inspiration and some of the "big problems"[114] (*), even though they are present and confronting us at every step, remain carefully bypassed and concealed, like the messengers of the one whom for fifteen years it has been our constant aim to bury.

15.3.7. . . and the body

Note 89 (May 17) The thought, the vision of things that lived within me and that I had thought I was communicating, I see as a living body, healthy and harmonious, animated by the power of renewal of living things, the power to conceive and engender. And now this living body has become a **corpse**, shared between some and others - this limb or quarter, duly stuffed, serving as a trophy for one, another, butchered, as a puzzle or boomerang for another, and yet another, who knows, as it is, for the family kitchen (we're not that far from it!) - and all the rest is good for rotting in the dump... .

That's the picture I've come to see, in terms that may be colorful but seem to express a certain reality of things. The puzzle may well fracture a skull here and there[115] (**)
- but never will these scattered pieces, neither trophy nor puzzle nor family soup, have the power so simple and obvious in the living body: that of the loving embrace that creates a new being. ...

(May 18) This image of the living body, and of the "remains" with its pieces scattered to the four winds, must have been forming in me throughout the past week.The comical form in which it presented itself under my pen-p . 376
typewriter in no way means that this image is in the least an **invention**, a tad macabre, a burlesque improvisation on the spur of the moment. The image expresses a **reality**, felt pro- fondly at the moment it took material form through a written formulation. I must have been aware of this reality in bits and pieces, here and there, over the fourteen years since my "departure", and perhaps even before. Bits and pieces of information recorded at first on a superficial level by a distracted attention, absorbed elsewhere - but which all pointed in the same direction, and which must have assembled, on a deeper level, into a certain image - an unformulated image that I didn't bother to take note of, when I had other things to worry about. This image has been considerably enriched and clarified in the course of reflection since the end of March, six or seven weeks ago. More precisely, scattered pieces of information, finally examined by the care of a fully present conscious attention, have been gradually assembled into **another** image, at the more superficial level of the thought that examines and probes, through work that might seem independent of the presence, in deeper layers, of the first. This conscious work culminated six days ago in the sudden vision of the "massacre" that took place - when I felt the "breath", the "smell" of **violence**, for the first time I believe in the whole[116] (*) reflection. It was also the moment when, in the layers already close to the surface, this

[114](*) This "main source of inspiration" is, of course, the "yoga of motives". It has been active in Deligne alone, who has kept it to himself for his own "benefit", and in a narrow form deprived of much of its force, denying some of the essential aspects of this yoga. Among the "great problems" inspired by this one, which have been ignored or discreetly discredited, I see right now (outsider that I am) the standard conjectures, and the development of the formalism of the "six operations" for all the usual types of coeffi cients, more or less close to the "motifs" themselves (which play in their respect the role of "universal" coeffi cients - those which give rise to all the others). Compare with the comments on this subject in the note "My orphans", n° 46.

[115](**) (May 31) And it will even be used to prove a theorem "of proverbial diffi culty"!

[116](*) (June 12) In recent years, I've sensed violent intent on the part of some of my ex-students towards some of my "co-students", but never a violence that could be felt as coming from a collective will (grouping five of them here).

the feeling of a living, harmonious body, which is well and truly "massacred" - and also the one in which the deeper, diffuse image must have begun to surface, perhaps bringing to the image-in-formation a carnal dimension, a "smell" that thought alone is powerless to give.

This "carnal" aspect came to the fore again in a dream last night - it's under the impulse of this dream that I now return to the lines I wrote yesterday. In this dream, I was cut quite deeply in several places on my body. First of all, there were cuts on my lips and in my mouth itself, bleeding profusely as I rinsed out my mouth with copious amounts of water (heavily reddened by blood) in front of my face.
an ice pack. Then wounds in the belly, also bleeding profusely, especially one from which blood was coming out.

p. 377 by jerks, as if it were an artery (the Dreamer didn't care about anatomical realism). The thought
I pressed my hand in front of the wound and curled up to stop the blood - it did indeed stop flowing, eventually forming a clot and a very large scab. Later, when I carefully lifted the scab, delicate healing had already begun. I was also cut on one finger, and it was surrounded by an impressive dressing doll... ...

I have no intention of embarking on a more delicate and detailed description of this dream, nor of probing it in depth here (or elsewhere). What this dream "as is" already reveals to me with startling force is that this "body" of which I spoke yesterday, and which as I was writing I saw as detached from me, like a child perhaps that I had conceived and procreated and which had gone out into the world to follow its own path - this body remains today an intimate part of my person: that it is **my** body, made of flesh and blood and a life force that enables it to survive deep wounds and regenerate itself. And my body is also, without doubt, the thing in the world to which I am most deeply, most indissolubly linked. ...

The Dreamer did not follow me in the image of the "massacre" and the sharing of the remains. This image was meant to convey the reality of intentions, of dispositions in **others** that I had strongly perceived, and not the way in which I myself experienced this aggression, this mutilation of which I was the object through something to which I remain closely linked. The Dreamer has just given me a glimpse of the extent to which I remain linked to it. This ties in with what I perceived (albeit less forcefully) in the reflection in the note "Le retour des choses - ou un pied dans le plat" (n° 73), where I try to pinpoint the feeling of this "deep link between the one who conceived a thing, and this thing", which appeared in the course of the reflection that day. Before that reflection on April 30 (barely three weeks ago), and for the rest of my life, I have pretended to ignore this link, or at least to minimize it, following the well-trodden path of current clichés. Worrying about the fate of a work of art that has left our hands, and of course worrying about whether our name remains attached to it in any way, is felt to be petty and mean-spirited - whereas it seems natural to us all that we should be deeply touched when a child of the flesh whom we have raised (and whom we believe we have loved) chooses to repudiate the name he was given at birth.

15.3.8. The heir

p. 378 **Note** 90 (May 18) I don't know if any student in the sixties (apart from Deligne) was able to sense this essential unity, beyond the limited work he was doing with me. Some may have sensed this in a confused way, and that this perception was lost without return in the years following my departure. What is certain, however, is that from our first contact in 1965, Deligne sensed this living unity. It was this fine perception of a unity of purpose in a vast design that was surely the main stimulus for his intense interest in everything I had to communicate and pass on. This interest

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people) and directed against me, through my work.

demonstrated, without ever wavering, throughout the four years of constant mathematical contact between 1965 and 1969[117] (*). He gave mathematical communication between us that exceptional quality I've already mentioned, and which I've experienced with other mathematician friends only in rare moments. It was this perception of the essential, and the passionate interest it stimulated in him, that enabled him to learn as if by playing everything I could teach him: both the technical means (zinc strand diagram technique, Riemann-Roch yoga and intersections, cohomological formalism, étale cohomology, topos language) and the overall **vision** that unites them, and finally the **yoga of patterns** that was then the main fruit of this vision, and the most powerful source of inspiration I had yet discovered.

What's clear is that Deligne was the only one of my students, right up to the present day, who at a certain point (as early as 1968, I believe) had fully assimilated and made his own the totality of what I had to transmit, in its essential unity as well as in the diversity of its means[118] (**). It was, of course, this circumstance, felt by all, that made him the designated "legitimate heir" to my work.

work. Clearly, this heritage neither encumbered nor limited him - it was not a burden, but gave him a sense of identity.

wings; I mean: he nourished with his vigor these "wings "which he had from birth, like other visions and p . 379

other heritages (less personal of course...) would nourish it...

The heritage he had nurtured in those crucial years of growth and development, and the unity that makes up its beauty and creative virtue, which he had sensed so well and which had become like a part of himself - my friend subsequently[119] (*) denied them, striving relentlessly to hide the heritage, and to deny and destroy the creative unity that was its soul. He was the first among my students to set an example by appropriating tools, "pieces", while striving to dislocate the unity, the living body from which they came. His own creative impulse was slowed down, absorbed and finally dislocated by this deep division within him, driving him to deny and destroy the very thing that made him strong, that nourished his impulse.

I see this division expressed in three interrelated effects. One is the dissipation of energy, scattered in the effort to deny, dislocate, supplant, hide. The other lies in the rejection of certain ideas and means, essential to the subject's "natural" development.

which he has chosen as his central theme[120] (**). The third is the attachment to this theme, of all themes, which is about

to supplant, to oust a master who is present every step of the way and who must be constantly erased - the very theme p . 380

who is most intensely invested with the fundamental contradiction that dominated his life as a mathematician.

What I know at first hand, and a basic instinct or flair that has never deceived me, make

[117](*) This period comprises five years, of which my friend spent one (1966) in Belgium doing his military service.

[118](**) When I say "totality", I mean everything that was essential, both in vision and in means. This doesn't mean, of course, that there weren't unpublished ideas and results that I never thought of telling him about. On the other hand, I don't think there was any mathematical reflection from the years 1965-69 that I didn't talk about "on the spot" to my friend, always with pleasure and profi t.

[119](*) Strangely enough, this division must have been present as early as the first year of our meeting (already expressed in an ambiguous attitude towards the SGA 5 seminar, which was his first contact with schemes, Grothendieck-style cohomological techniques, and staggered cohomology), and at the latest and in unequivocal form as early as 1968 (see note "L'éviction", n° 63).
- at a time, therefore, when mathematical communication was perfect, and when the development of his mathematical thinking seems to me not yet to have been marked by conflict. At the time, he made many interesting contributions (which I take great pleasure in highlighting in the Introduction to SGA 4) on topics that he did his utmost, immediately after my departure, to bury.

[120](**) This refusal manifested itself in the burial of derived and triangulated categories (until 1981), of the formalism of the six variances (until today), of the language of topos (also), and by a sort of "blocking by disdain" of the vast program of foundations for homological and hoinotopic algebra, which I'm now trying (twenty years later) to sketch out with La Poursuite des Champs, and which he had of course also felt the need for. However, even though it was inspired by the yoga of motifs (buried until 1982), this yoga remained mutilated of part of its force, being detached from the formalism of the six variances which constitutes an essential formal aspect of it. It seems to me that this aspect has also been rigorously banished from

Hodge-Deligne's theory.

it's quite clear to me that if Deligne hadn't been torn by this profound contradiction in his own work, mathematics today wouldn't look like it does[121] (*) - that it would have undergone, in several of its essential parts, far-reaching renewals like the one I myself had been the main instrument of - the very one that this same Deligne was bent on countering and diverting![122] (**)

There's also no doubt that he was ideally suited to be the driving force behind a powerful school of geometry, a continuation of the one that had formed around me - a school nourished by the vigor of the one from which it had sprung, and the creative power of the one who was taking over from me. But this school that had formed around me, this nurturing matrix that had surrounded intense years of training - it broke up the day after my death.

p. 381 of my departure. If this was the case, it was precisely because I couldn't find, in the person who was obviously taking over from me[123] (***), the person who would also be the soul of a group united by a common adventure, for a common task.
whose dimensions are beyond anyone's means.

I have the impression that, after my departure, each of my students found themselves in their own corner, with a wealth of work to do - there's no shortage of that anywhere in maths - but without this "corner" fitting into a whole, and without this "work" being carried along by a current,' by a wider purpose. Surely, as soon as I left, if not even before, the eyes of most of my students or ex-students were focused on the designated "successor", the most brilliant among them and also the closest to me. At this sensitive moment, my friend must have felt, perhaps for the first time in his life, the power over others that was suddenly in his hands, the power of life or death that he had over the fate of a certain school from which he had come, and whose friends he had rubbed shoulders with for four years were no doubt expecting him to ensure its continuity. The situation was entirely in his hands, and it was he who would set the tone... . He did indeed set the tone, by destroying the legacy, and first and foremost that confidence and expectation[124] (*) which those who, with him, had been pupils of the same master, could not fail to bring him. ...

I'm sure many people are impressed by Deligne's work, and not without reason. But I'm also well aware that this work, beyond the impressive initial impetus (ending with the demonstration of Weil's conjectures), is far from "living up to its potential". It certainly demonstrates an uncommon technical mastery and ease, placing him among the "best". But it lacks the humble virtue that

[121](*) When I wrote these lines about "mathematics today", I wasn't just thinking about the more or less profound knowledge we have of mathematical things today. I was also thinking, in the background, of a certain **spirit** in the world of mathematicians, and more particularly in what might be called (without sarcastic or mocking intonation) "the big mathematical world": the one that "sets the tone" for deciding what is "important", even "licit", and what is not, and the one that also controls the means of information and, to a large extent, careers. Perhaps I'm exaggerating the importance that a single person, in the position of fi gure de proue, can have on the "spirit of the times" in a given milieu at a given time. Deligne's career seems to me to be comparable (for better or for worse) to Weil's in the milieu that had welcomed me twenty years earlier, and with which I had identified myself for twenty years.

(May 31) Compare with the (complementary) reflections in the note "Le Fossoyeur - ou la Congrégation toute entière", n° 97.

[122] (**) (June 16) I'm convinced that the very fact that the key ideas I've introduced into mathematics are already developing normally, on the momentum gained in the sixties (cut short by the "chainsaw effect" to be discussed in the following two notes. ...), mathematics today, fifteen years after my departure, would have been different from what it is, in some of its essential parts. ...

[123](***) This **de facto succession** was expressed by unequivocal concrete signs: he took over from me at the IHES (from which I left the year after he joined - see note "L'éviction", n° 63), and he took over, with the means I had developed at this fi nal for some fifteen years (from 1955 to 1970), the central theme of the cohomology of algebraic varieties.

[124](*) (May 26) In the course of further reflection, I detected yet another "expectation" regarding my tacit heir, this time coming not from my students alone, but from "the whole Congregation" - see the end of the note "The Gravedigger - or the whole Congregation" (n° 97). I have little doubt that these two opposing expectations, one linked to a very particular moment, the other continuing throughout the fourteen years of the Burial, are both real. What's more, I'd be inclined to think that for many of my former students, the two expectations must have been present simultaneously: that of finding in the most brilliant among them the one who would ensure the continuity of a School and a work in which they had their place and their part - and that of seeing erased (if that were possible) all trace of the one whose departure suddenly called out to them with such force, in the quietude of the well-trodden paths. ...

I perceived in him in his early years - the virtue of renewal. This virtue he carried within him, this freshness or innocence of the little child, has long since been deeply buried, denied. I was about to write that by this "virtue" and by his not very com□muns gifts, as well as by the exceptional circumstances of which he p. 382 benefited from the deployment of his gifts, Deligne was called upon to "dominate" the mathematics of our time, just as a Riemann, or a Hilbert, had each "dominated" the mathematics of their time. Inveterate habits of thought, rooted in common parlance, have suggested to me this image of "domination", which nevertheless gives a distorted apprehension of reality. These great men undoubtedly fully "grasped", "assimilated", "made their own" the mathematics known in their time, which undoubtedly also gave them an exceptional mastery of technical means. But if they rightly seem "great" to us, it's not through their technical prowess, "wresting" difficult demonstrations from surly substance. It's by the renewal each of them brought to several important parts of mathematics, by simple and fruitful "ideas", that is to say: for having turned their gaze on simple and essential things, to which no one before them had deigned to pay attention. This childlike ability to **see** simple, essential things, however humble and disdained by all - **this is the** power of renewal, the creative power in everyone. This power was present to a rare degree in the young man I knew, unknown to all, a modest and passionate lover of mathematics. Over the years, this humble "power" has seemed to disappear from the person of the admired and feared mathematician, enjoying unfettered his prestige, and the (sometimes discretionary) power it gives him over others.

This **stifling** in my friend of something very delicate and very vivid, neglected by all and which has creative power, I've felt it many times since I left, and more and more in recent years. But it took the discoveries of the last few weeks, and the reflection I've been pursuing since the end of March (following on from Récoltes et Semailles), to begin to feel the full extent of the devastating effect of this suffocation in the life of my friend, and among many others I've known closely. Not only on some of my "later" (and assimilated) students, who were subjected to his malevolence (perhaps unconscious in some cases), which was exercised against everyone and weighed heavily on three of them; but also, it seems to me glimpse it now, among my students "before", by the destruction of a **continuity** in the subject, and that of a sense of a whole, a unity, giving a deeper and broader meaning to their work than ce□lui p. 383 an accumulation of separate prints bearing their names (91)[125] (*).

More than once in the last seven years, and more than once again in the last few weeks and days, I've felt a sadness, at what feels, on some level, like an immense **waste** - when what is most precious in oneself and in others is squandered or smothered as if for pleasure. Yet I've also come to learn that such "waste" is a staple of the human condition, to be found in one form or another everywhere, in the lives of individuals, from the humblest to the most illustrious, as well as in the lives of peoples and nations. This very "mess", which is nothing other than the action of conflict and division in everyone's life, is a substance of a richness and depth that I have barely begun to fathom, a nourishment that it is up to me to "eat" and assimilate. So this mess, and every other mess I encounter at every step, and every other thing that happens to me at the turn of the road and is so often unwelcome - this mess and other unwelcome things carry within them a **blessing**. If meditation has meaning, if it has the power of renewal, it's insofar as it allows me to receive

[125](*) (June 16) This second aspect only came to my attention in the course of my reflection on L'Enterrement. If ever I saw a prestigious mathematician make use of the "power to discourage", it was the very man who once seemed to me to be my heir apparent. When I wrote the section "The power to discourage", I had thought a lot about him (before the thought came back to me), but without yet having the slightest inkling (at least not on a conscious level) of the extent to which this power had found occasion to be exercised among the very people for whom he must have been (as he was for me) the model of the perfect mathematician. ...

the benefit of that which (through my inveterate reflexes) presents itself as "evil", where it allows me to **nourish** what seems designed to destroy.

Nourishing yourself with your experience, letting it renew you instead of constantly evading it - that's what it means to take full responsibility for your life. I have this power within me, and it's up to me at any given moment to make use of it, or to let it go to waste. It's the same for my friend Pierre, and for each of my students - free, like me, to feed on the "mess" I'm finishing reviewing in these last days of a long meditation. And the same goes for the reader who reads these lines, intended for him or her.

15.3.9. The co-heirs

p. 384 **Note** 91 (May 19) The echoes that have reached me here and there about my former pupils have been more than sparse.

Hardly any of them gave me any sign of life after my departure, if only by sending me prints[126] (*). However, by gathering together the few that have reached me, I can form an idea, admittedly very approximate. Perhaps it will become clearer in the months to come, if this reflection prompts some of them to come forward.

I've already had occasion to note the profound rupture in Deligne's work after my departure, even though in some respects he appears, unwillingly, as a successor, and therefore as part of a certain continuity. And I had the feeling that this rupture must have had profound repercussions on the work of all my other students. It's this impression that I'd like to explore a little more closely.

The only one of these students whose work seems to be an obvious (at least at first sight) extension of the work he had done with me, seems to be Berthelot[127] (**). He's also the only one who, for a long time, sent me numerous separate prints - perhaps even all his separate prints. They are all on the difficult subject of crystalline cohomology, the systematic start-up of which is the subject of his thesis. Yet it seems to me that, as with my other (commutative) "cohomology" students, his work is marked by the disaffection of some of the main ideas I had introduced: derived categories (and triangulated categories, cleared by Verdier), six-operation formalism, topos (91_1). As Zoghman Mebkhout himself says, his own work, so close in theme to Berthelot's (91_2), is in line with these ideas, combined with those of the Sato school. If they hadn't been repudiated by my cohomology students, led by Deligne and Verdier, there's a good chance that from the very beginning of the seventies,

Mebkhout's crystalline theory (which he began to develop only from 1975, against the disinterest of these p. 385 same students) would already have reached the full maturity of a formalism of six operations, which it still hasn't reached today[128] (*).

Incidentally, I remember talking to Verdier about the question, which intrigued me, of the link between constructible dis- cret coefficients and continuous coefficients, without it seeming to catch his eye. It must have caught on later

[126](*) (May 31) On this subject, see note no.° 84, following the note on "Silence" (no.° 84).

[127](**) According to the duality theme that Verdier pursued for a few years after my departure, in the context of analytic spaces close to the one in which I had developed it, there is an impression of continuity as in Berthelot's case. But it seems to me that this has been a bit of a "routine continuity", whereas the one whose signs (or absence of signs) I'm looking for above all is a creative continuity, continuing an initial impulse into the unknown. ...

[128](*) (June 7) I hesitated to hazard this assessment, which could be interpreted as minimizing the originality of Mebkhout's theory. This would not be at all in line with my thinking, and all the more so as I have an excellent opinion of the abilities of each of my cohomology students (when they are not blocked by prejudices alien to mathematical common sense). My friend Zoghman himself dispelled any scruples I might have had, saying that he was convinced that "normally", it was my students who should have been developing his theory from the very beginning in the 70s. At a certain level, they're surely the first to be convinced: it's they, or Deligne, who **should have** developed it - and with the general degradation of morals, that's all it takes to behave as if they (or Deligne) really were! On this subject, see the notes "Le Colloque" and "La mystifi cation", n° s 75' and 85'.

(See note "L'inconnu de service et le théorème du bon Dieu", n° '48.) In fact, he was so "blocked" by his burial syndrome that, until October 1980, he failed to perceive the importance of Mebkhout's work - and when he finally did, it was in the grave-digging mood we all know (see notes n° s 75 to 76).

As far as I'm aware, Verdier's work since his thesis defense has essentially been limited to redoing in the analytical context (which sometimes presents additional technical difficulties) what I had done in the coherent schematic framework, without introducing any new ideas. It's even rather extraordinary, with the reflexes he was supposed to have developed, and well-informed as he was, that he didn't come across Mebkhout's theory himself, by dint of turning his crank - and that he didn't at least recognize that his "pupil" was doing some interesting things, which had escaped him (as they had escaped Deligne).

To tell the truth, while intrigued by the question of the relationship between discrete coefficients and coefficients continuous, I hadn't really had any inkling of Mebkhout's crystalline theory, which would blossom in the decade following my departure. On the other hand, there was a vast theme, arising from my reflexions of cohomologyp

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both commutative and non-commutative of the fifties (1955-1960), and which was just beginning (in the "commutative" context i.e. in terms of additive categories) in Verdier's work, started in the early sixties and left behind after his defense (see note n° 81). The non-commutative aspect was initiated later in Giraud's thesis, which developed a geometric language, in terms of 1-fields over a topos, for non-commutative cohomology in dimension ≤ 2. By the second half of the sixties, the inadequacy of these two primers was quite obvious: both in terms of the inadequacy of the notion of "triangulated category" (teased out by Verdier) to account for the richness of structure associated with a derived category (a notion destined to be replaced by the considerably richer notion of **derivator**), and in terms of the need to develop a geometric language for non-commutative cohomology in any dimension, in terms of n-fields and **∞-fields** over a topos. One sensed (or I sensed) the need for a synthesis of these two approaches, which would serve as a common conceptual foundation for homological algebra and homotopic algebra. Such a work was also in direct continuity with Illusie's thesis work, in which both aspects are represented.

Bousfield-Kan's seminal work on homotopic limits (Lecture Notes n° 304), published in 1972, was also in line with this diffuse program, which since at least 1967 had been just begging to be developed. In January of last year, without yet suspecting that I would be embarking on the Pursuit of Fields a month later, I submitted to Illusie some thoughts on the "integration" of homotopy types (familiar to homotopists as "homotopic (inductive) limits"), at a time when I was still completely unaware of the existence of Bousfield and Kan's work, and that this type of operation had already been examined by others than myself. It turned out that Illusie was equally unaware of it, despite the fact that he was supposed to have remained in homologous-homotopic waters for all the time since my "death" in 1970! This just goes to show how far he seems to have lost touch with certain realities that are part and parcel of the fundamental thinking he himself had been pursuing in the sixties[129] (*). He must have made his own little hole, from which he hardly ever emerges. ...

With the disdain that has befallen the very notion of topos and all the "categorical nonsense", it is not surprising

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[129](*) This notion of "integrating" homotopy types had come to me again, in the context of unscrewing stratified structures, which I took up at the end of 1981.

that Giraud now has a total disaffection for what had been his first major theme of work. It's true that Deligne, with the exhumation of motives two years ago, pretended to have suddenly discovered the interest of the arsenal of non-commutative cohomology, sheaves, links and consorts, as if he himself had just introduced them, along with motives and motivic Galois groups[130] (*). It's doubtful that this kind of circus will rekindle a flame that he himself has worked so hard to extinguish. ... In February last year, I sent Giraud a copy of the twenty-page letter that became Chapter 1 of the opening chapter of La Poursuite des Champs. It's a non-technical reflection, in the course of which I managed to "jump with both feet" over the "purgatory" that had once stopped Giraud (and many others) from handling the notion of "non-strict" *n-category* (which I now call "*n-field"*), which remained heuristic and yet was visibly fundamental. This was the start of the Poursuite des Champs. When we met (on friendly terms) last December for Contou-Carrère's thesis defense, I learned from Giraud that he hadn't even had the curiosity to read the letter! I got the impression that he'd drawn a long line under such things. The idea that there might be some rich substance, in a direction he had long since abandoned, didn't even seem to occur to him. I tried, unsuccessfully I'm afraid, to get him to understand that there was a juicy and vast work here that had been waiting for nearly twenty years to be done, and which I finally got round to in my old age, to at least give a broad outline, under the dictation of things themselves, of a rich substance that the "deceased" me continues to feel strongly about, while my students have long since forgotten about it.

Jouanolou also abandoned a research direction he had just begun with his thesis. This direction had become the object of the disdain of a fashion established by the very person who had provided him with a master technical idea for the theme he had chosen. With the "rush" on triangulated categories with the Colloque Pervers three years ago, this same Deligne suddenly pretended (without laughing) to discover the big job of foundations in perspective, the lack of which is suddenly felt at all ends, and which he had été the first to discourage for ten years - The need for such a job was quite obvious to me as early as 1963/64 with the beginnings of étale cohomology; and for Deligne just as much, from the moment he started hearing about *l-adic* cohomology and triangulated categories, i.e. when he arrived at my seminar the
p. 388 following year. Beyond the construction of "constructible triangulated categories" on the ring $\underline{Z}_l$ (above a basic scheme, let's say), and the development of the formalism of the "six operations" within this framework (something accomplished, it seems to me, in Jouanolou's thesis), to make analogous work by replacing the base ring $\underline{Z}_l$ by an arbitrary (more or less?) Noetherian $\underline{Z}_l$ -algebra, e.g. Q_l or an (algebraic?) extension of Q_l . This is one of the things for which time has been ripe for some twenty years, and which are still waiting to be done, when the wind of contempt that has blown over them has died down... .

The natural continuation of Ms. Raynaud's work (weak Lefschet theorems in staggered cohomology, in terms of 1-fields) would have been placed in a context of strictly taboo ∞-fields, let's not talk about it! The same goes for Ms. Sinh's work, begun in 1968 and completed only in 1975 - a natural continuation would have been the notion of an enveloping ∞-category of Picard of a so-called "monomial" category, or of triangulated variants of such a category[131] (*) - let's not! Another was to transpose her work in terms of fields onto a topos - what a horror! As for Monique Hakim, she too had the misfortune to write her thesis on a subject which, in the times since my untimely departure, looks a bit ridiculous on

[130](*) See "Souvenir d'un rêve. . . - or the birth of motifs", note n° 51.
[131](*) See "Souvenir d'un rêve. . . - or the birth of motifs", note n° 51.

edges - relative diagrams on a locally ringed topos, I ask you! His little book on the subject, published in Grundlehren (Springer), must sell three or four copies a year - no wonder I've got bad press there, and they're not too keen to accept any text I might recommend. For me, it was a first test-step towards a "relativization" of all "absolute" notions of "varieties" (algebraic, analytic, etc. . .) on general "bases", the need for which is obvious to me (91_3). It's true that we've done just fine without them until now. But it's also true that we've done without maths for the two million years we've been around.

The fact remains that Monique Hakim, who was not motivated to write her thesis in the same way as I was to offered it to him, surely had no desire to keep any contact with a theme which (detached from p. 389

In the context of a favorable consensus, or stubborn thought pursuing a tenacious and sure vision against all odds, it can no longer make the slightest sense.

As for Neantro Saavedra Rivano, he seems to have disappeared entirely from circulation - I can find no trace of his name even in the official world directory of mathematicians. What is certain is that his rather categorical thesis subject could hardly have been in good press with the gentlemen who decide what is serious and what is not. The most natural continuation of this thesis, in my opinion, would have been neither more nor less than this "vaste tableau des motifs", a theme decidedly a little broad for this student's more modest aims. Yet he ended up having the unexpected honor of having his thesis redone ab ovo et in toto by one of these great gentlemen himself, barely two years ago. (On this subject, see the notes "L' Enterrement - ou le Nouveau Père" and "La table rase", n° s 52 and 67.)

Finally, the only ones among my twelve "pre-1970" students for whom it's not too clear to me whether or not there was a more or less drastic or profound **break in** their work, compared to the one they had to follow in my contact, are Michel Demazure and Michel Raynaud (91_4). All I know is that they've continued to do maths, and that they're part (as you'd expect, given their brilliant means) of what I called earlier "the great mathematical world".

The foregoing brief reflection, based on what is sometimes very little data, is of course largely hypothetical and very approximate. I hope that those mentioned here will forgive me for any perhaps gross errors of assessment, which I'll be happy to rectify if they'd be so kind as to let me know. Here again, I realize that everyone's case is surely different from everyone else's, and represents a much more complex reality than someone as distant as me can reasonably apprehend, let alone express in a few lines. All these reservations aside, I have the impression that this reflection has not been in vain, for me at least, to identify by a few concrete facts, a still vague impression that had emerged yesterday (and which was undoubtedly present at an informal level for many years): that of a **break** that was made p. 390

in many of my students in the aftermath of my departure, reflecting on a personal level the sudden disappearance, overnight, of a "school" to which they must have felt a part during crucial formative years in their mathematical profession.

Note 91_1 (May 22) I've just come across an article-survey from the Colloque "Analyse *p-adique* et ses applications" at CIRM, Luminy (September 6-10, 1982), by P. Berthelot, entitled "Géométrie rigide et cohomologie des variétés algébriques de car. *p*" (24 pages), which outlines the main ideas for a synthesis of Dwork-Monsky-Washnitzer cohomology and crystalline cohomology. The initial ideas (and the very name) of crystalline cohomology (inspired by Monsky-Washnitzer cohomology), and the idea of complementing these with the introduction of sites formed by rigid-analytic spaces, ideas that I had introduced in the

In the sixties, they became the daily bread for all those working on the subject, starting with Berthelot, whose thesis developed and fleshed out some of these initial ideas. Nevertheless, my name is conspicuously absent from both the text itself and the bibliography. Here we have a fourth clearly identified pupil-croquemort. Who's next?

(June 7) It's a remarkable fact that more than fifteen years after I introduced the starting ideas of crystalline cohomology, and more than ten years after Berthelot's thesis established that the theory was indeed "the right one" for clean and smooth schemes, we still haven't reached what I call a situation of "mastery" of crystalline cohomology, comparable to that developed for stellar cohomolo- gy in the SGA 4 and 5 seminar. By "mastery" (in the first degree) of a cohomological formalism including duality phenomena, I mean no more and no less than full possession of a six-operation formalism. While I'm not "in the know" enough to be able to appreciate the difficulties specific to the crystalline context, I wouldn't be surprised if the main reason for this relative stagnation lies in the disaffection of Berthelot and others for the very idea of this formalism, which makes them neglect (just like Deligne's Hodge theory, which remained in its infancy) the first essential "level" to be reached in order to have a fully "adult" cohomological formalism. These are the same kind of dispositions

p. 391 which have surely led him to overlook the relevance of Mebkhout's point of view to his own research. NB When I speak here of "crystalline cohomology" in a context where one abandons assumptions of cleanliness (as is necessary for a "fully grown-up" formalism), it is understood that one is working with a crystalline site whose objects are (power-divided) "thickenings" that are not purely infinitesimal, but are "proper" (power-divided) topological algebras. The need for such an extension of the primitive crystal site (which for me was only a first approximation for the "right" crystal theory) was clear to me from the start, and Berthelot learned it (along with the initial ideas) by null other than myself. A written allusion to this link can be found in Esquisse Thématique, 5 e.

Note 91_2 It's quite extraordinary that nobody but me seems to have realized that Mebkhout's unnamed theory was an essential new part of a crystalline theory. As someone who has been completely "out of cohomology" for nearly fifteen years, I realized this as soon as Mebkhout took the trouble to explain to me what he had done last year. In any case, when I mentioned the matter (as a matter of course) to Illusie, he seemed to see it as a rather "kooky" combination of things (*D-Modules* and crystals) that really had nothing to do with each other. Yet I know first-hand that he has a mathematician's flair, and so do my other students (coho- mologists in this case, starting with Deligne) - but I can see that in certain situations, he's no longer any use to them... ... The more I think about it, the more I find it extraordinary that in such an atmosphere, Meb- khout still managed to do his job, without letting his own mathematical flair be defused by the total incomprehension of his elders, so far above him. . .

Note 91_3 It was especially since my lectures at the Séminaire Cartan on the foundations of the theory of complex analytic es- paces, and on the precise geometrical interpretation of "level modular varieties" à la Teichmüller, towards the end of the fifties, that I understood the importance of a double generalization of the common notions of "variety" we've been working with so far (algebraic, real or complex analytic, differentiable - or subsequently, their "moderate topology" variants). One is to broaden- gir the definition so as to admit arbitrary "singularities", and nilpotent elements in the structural bundle of

p. 392 "scalar functions" - along the lines of my foundational work with the notion of schema.

The other extension is towards a "relativization" above suitable locally annelated topos ("absolute" notions being obtained by taking a punctual topos as a base). This conceptual work, matured for over twenty-five years and initiated in Monique Hakim's thesis, is still waiting to be taken up. A particularly interesting case is that of the notion of relative rigid-analytic space, which allows us to consider ordinary complex analytic spaces and rigid-analytic spaces over local bodies with variable residual characteristics, as "fibers" of the same relative rigid-analytic space; just as the notion of relative scheme (which has finally become commonplace) allows us to link together algebraic varieties defined over bodies of different characteristics.

Note 91_4 While Demazure's thesis work, like Raynaud's, makes essential use of a consummate schematic technique they learned from me, the essential ideas in their respective works are not part of the "Grothendieckian" panoply, which distinguishes their work from that of my other students of the first period. It's possible that this circumstance resulted in a continuity in their work, free from a rupture due to the effect of the "master's burial syndrome". This doesn't necessarily mean that this syndrome didn't affect one or the other in another way. Three years ago, I was struck by Raynaud's attitude towards Contou-Carrère's work on local relative Jacobians. The results announced are profound, difficult and beautiful, and go far beyond a simple genera- lization of "well-known" things. There's an unexpected link with Cartier's theory of typical curves, some wonderful explicit formulas - all entirely within Raynaud's (and my) grasp. The freshness of his welcome must have weighed decisively in Contou-Carrère's strategic retreat, abandoning for profit and loss a subject in which he had invested himself wholeheartedly and which, it may have seemed, was only going to get him into trouble. ... [132](*). My letter to him, in which I expressed my (pained) surprise at his insensitivity to the beauty of these results, went unanswered.

15.3.10. . . and the chainsaw

Note 92 When I moved to the area nearly four years ago, there were not far from p . 393
me a beautiful cherry orchard. Often, when I went for a walk, I'd go and have a look. It was a pleasure to see these thick cherry trees, in their prime, with their powerful trunks, which seemed to have always been one with this piece of land, where wild grasses proliferate freely. They must not have known about fertilizers or pesticides, and in cherry season, that's where I'd go to pick the tasty ones. There must have been twenty or thirty trees.

One day, when I went back there, I saw all the trunks cut down to man-height, the crowns slumped on the ground next to the trunk, stumps in the air - a vision of carnage. With a good chainsaw, it must have been done in an hour or so. I'd never seen anything like it - when you cut a tree, you usually take the trouble to bend down and cut it flush with the ground. There's a shortage of cherries, of course, and this cherry orchard wasn't going to produce tons, that's for sure - but these stumps of trunks said something other than shortages and yields... .

Yesterday I had that feeling again, of a vigorous trunk, with powerful roots and generous sap, with strong, multiple branches extending its momentum - cut clean off, at man's height, as if for pleasure. It was taking the trouble to look at the main branches one by one, and seeing each one cut off, that finally made me see what had happened. What was made to unfold, in the continuity of a momentum,

[132](*) For further details, see sub-note no.° 95_1 to note "Cercueil 3 - ou les jacobiennes un peu trop relatives", no.° 95.

of a deep-rooted inner necessity, has been sliced clean through, with a clean slice, to see itself designated for all to see as an object of derision.

This reminds me of the "misunderstanding" Zoghman referred to, which supposedly took place between me and my students (except Deligne). What's clear, in fact, is that neither impetus nor vision were communicated from me to any of my students (apart from Deligne, who is decidedly "apart" indeed!). Each of them assimilated a technical baggage, useful (and even indispensable) for doing a well-done job on the subject they had chosen, and which could even be of use to them later on. I can't say whether there was any hint of something else, going beyond that. If there was

p. 394 had, it didn't stand a chance against the chainsaw, which quickly trimmed it... ...

I'm well aware that if there are still people doing maths - and unless they give up completely

the kind of maths we've been doing for over two millennia - they won't be able to resist reviving each and every one of the branches I see lying inert. Some of them have already been taken over by my friend-with-the-saw, and it's quite possible, if God gives him life, that he'll do the same with some or all of them. Most of them, however, are no longer in his style. But perhaps he'll also eventually tire of constantly substituting himself for someone else, which is surely very tiring and not at all profitable, and just be himself (which isn't bad at all).

16. D) IN-GROUND

Contents

16.1. X Le Fourgon Funeral

16.1.1. Coffin 1 - or the grateful D-Modules

Note 93 □(May 21) It's been a couple of weeks now that my thoughts have been lingering on my "bon teint" students, those p. 395
"before". Each day, the reflection appeared as a "final addition", as a matter of conscience, to a reflection that seemed (practically) finished. More than once, it was an innocuous footnote, carelessly branching off from the previous day's or the day before's reflection, that grew and grew to the dimensions of an autonomous "note". Each time, it quickly found its name, distinguishing it from all the others, and inserting itself into its funeral procession, just in the right place, as if it had always been there! Every other day, I was there to redo (each time with pleasure) at least the end of the table of contents, which seemed to be closed and which was then lengthened by two or three new participants in the Procession, when it wasn't a whole new procession...
...

This Procession ends up taking on worrying dimensions - no one's ever going to want to read all that! But if it grows so long, it's not, to be honest, for the dubious benefit of a hypothetical reader, but first and foremost for my own benefit - just like when I do maths. I've never had any regrets about having embarked on these "last complements", which I embark on each time as if against my will. By dint of these last supplements, I've learned many things that I wouldn't have been able to learn otherwise, by doing without a "piecemeal" reflection. And one by one, these things came together to form a vividly colored, vastly proportioned and multi-faceted picture. Even now, I can see that it's not entirely finished - there are still two places that seem to need a final brushstroke.

I think it's time, after my "good-natured students", to talk a little about the **buried.** -of those who "with me are entitled to the honors of this funeral through silence and disdain". No more than I or those who bury with gusto, these buried are not saints and have no vocation for martyrdom. I don't think there's one of them who didn't resent me for the trouble I was quite unwittingly getting him into (simply because of

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that he'd been unwise enough to bet on me, on a certain approach to mathematics and on a certain style. . .) - or that he had at least tried to distance himself from me, once he'd recognized that the bet was definitely a loser[1] (*). As I've seen, this is a wasted effort - once spotted, it's all over, and to stand out is to fuel contempt, to provide tacit justification, instead of disarming it. More than once, too, and in many ways, I've seen the roles of burier and buried rubbed together and confused[2] (**). These aspects of ambiguity are undoubtedly the cause of a long-standing reluctance on my part to talk about the "buried" in any more detailed way than the allusions I've already made to them in passing. It's possible that, with the possible exception of Zoghman, none of the other three I know would be grateful to me for giving him "publicity" here, as if I hadn't given him enough trouble as it is.

Like so many times in the course of Harvesting and Sowing, I'm finally overcoming such reluctance in myself. I tell myself that even when it comes to people who have had to suffer because of me (because of a choice they made at a given time and where, for one reason or another, they were happy with it, although they had no more idea than I did of the disadvantages attached to their choice) - even towards them my role is not to help them evade a very real situation, in which they are involved whether they like it or not, and which surely makes sense even if it has serious disadvantages.

Before I embark on the black series of the four coffins of my late co-deceased and co-buried, I should perhaps cheer up the reader with a less funereal note. First of all, in my dealings at the "local" level of my University's Institute of Mathematics, I have by no means had the experience that the good I might say about a candidate for a post, or the fact that a candidate was one of my students (after 1970, needless to say), or that his work was influenced by mine, necessarily worked against him. A

such an attitude of systematic boycott uniquely characterizes the relationship of the mathematical "big world" to my person, and by extension, to those who appear to be linked to me "after 1970". This boycott has been

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As far as I've been able to ascertain, his service has been virtually faultless in the fourteen years since I left, albeit with two modest exceptions. One concerns a student who, after a promising start, was supposed to be working with me on a state doctorate thesis on a most tantalizing subject, and whose application for a position as assistant professor at USTL had been rejected by my University's Commission of Specialists. He was "drafted" to the national level, with the help of Demazure, to whom I had written about this student's work[3] (*). In addition, on two occasions, the journal **Topology** accepted articles by students of mine: an article "Factorisations de Stein et découpes" by Jean Malgoire and Christine Voisin, and a forthcoming article by Yves Ladegaillerie, containing the central result of his 1976 thesis (See note n° 94).

I've already had occasion to talk about Zoghman Mebkhout in particular, and I'll mention him again here "for the record"[4] (**). Mebkhout began to draw inspiration from my work in 1974, I believe, and has continued to do so ever since.

[1](*) (February 1985) I am aware of a total of seven or eight (short) publications, outside my University, presenting (in summary form) work done with me and inspired by me, since I have been in Montpellier. My name is absent from all of them.

[2](**) (September 2) In different ways from one to the next, each of them has at some point internalized and taken on board the disdain for their work, acquiescing to the consensus that dismisses this work or classifies it as "uninteresting".

[3](*) At the "practical" level of promotion or accession to a position and status, the record of my teaching activity since 1970 boils down, in all, to two accessions to a position with status at stake, once as a maître-assistant and another time as an assistant. By a strange irony, on both occasions, these accessions signalled a sudden and radical halt to all research activity on the part of the person concerned.

[4](**) Apart from Introduction (6) (L'Enterrement), Mebkhout is mentioned in the notes "Mes orphelins", "L'inconnu de service

to this day. I am not aware of any of my "official" students having produced a work of comparable scope - although Mebkhout's work is necessarily affected by the conditions of adversity in which it had to be pursued. As I said in the Introduction (6), for the past four years, Mebkhout's ideas and results have been used by everyone, while his name has been carefully withheld[5] (***). It's a mystery to me how my friend was able to continue doing maths, while at the same time
suffering disdain, then iniquity as a kind of inescapable fate - a fate that came to him through
people he must have (and still must) felt vertiginously above him[6] (*), people he p. 398
must have first heard them referred to as the "gods of the stadium", at a time when he (like myself) was a modest emigrant student with precarious resources. When he defended his thesis in 1979, he had an assistant's post in Orléans. He then did everything he could to get into the CNRS, coming back three times - on the third occasion (in October 1982) he was finally given a position as a research fellow (equivalent to that of assistant or maître-assistant at the University). This gave him, if not a statutory guarantee, at least a degree of relative security.

Of the four "co-buried" mathematicians of whom I am aware, Mebkhout is the only one who has continued to pursue his work against all odds, trusting his mathematical instincts without letting himself be stopped by considerations of prudence and expediency that might have inspired a merciless fashion. There was in him, who was not of a combative nature, an elementary **faith** in his own judgment, which is also a **generosity**, and which (much more than cerebral "means") is the primary condition for doing innovative and profound work. My idea of his work is surely incomplete. From what I know of the main part of his work, it seems to me that with the brilliant means at his disposal, placed in an atmosphere of warm and active sympathy, he could have accomplished it, and brought it to greater maturity, in three or four years instead of ten, and in joy and not in bitterness. But three years or ten, and "maturity" or not, the remarkable thing is that the innovative work appeared, and that it could have appeared in many such conditions.

16.1.2. Coffin 2 - or cut to size

Note 94 Yves Ladegaillerie began working with me in 1974. It was "just by chance", in a hollow mo- ment at his place - I submitted to him some naive reflections on the plunging of topological 1-complexes into surfaces, at a time when I knew nothing about surfaces (except the notion of
and he even less so. It was a bit grothendieckery (where I come from, anyway, it always starts-
days like that. ...), and it clicked with him more or less, until one day it finally "clicked", I don't know p. 399
when and why. Perhaps it was when an obviously juicy question was emerging, a certain key conjecture about determining the isotopy classes of a compact 1-complex in a compact oriented-edge surface. True - false? That was the suspense, which lasted a good six months,

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et le théorème du bon Dieu", "L'Iniquité - ou le sens d'un retour", "La Perversité", "Rencontres d'outre-tombe", "La Victime - ou les deux silences", "Le Pavé et le beau monde", "Thèse à crédit et assurance tous risques" (notes n° s 46, 48', 75, 76, 78, 78', 80, 81).

[5](***) Legion of people acted as gravediggers at this funeral, in which practically the entire Colloque de Luminy (June 1981) took part. Apart from my cohomology students (see note "Mes élèves (2) : la solidarité", n° 85), those whose professional good faith is directly and gravely in question here and of whom I am aware are J.L. Verdier, B. Teissier, P. Deligne, A.A. Beilinson, J. Bernstein.

[6](*) Of course, Zoghman Mebkhout is no more of an idiot than I am, and he's sufficiently in the know to have a precise idea of the work of each of my cohomologist students, and to realize its scope as well as its limits, without any propensity to idealize it. Nevertheless, inhibitions of considerable power have held him back from even the idea of publicly questioning any of them, even where malice is patent.

a year, during which Yves brought himself (and me) up to speed on the key theorems of surface theory, while pushing on with the "foundation" parts of his work. The known results made the conjecture rather plausible, but were obviously far from the mark - whereas the conjecture implied cow results from Baer and Epstein, and other things that had unusual, even suspicious, aspects. He finally managed to prove the key conjecture in the summer of 1975. Essentially, it is equivalent to a complete algebraic description, in terms of fundamental groups, of the set of isotopy classes of plungers of a triangular compact space (say) in an oriented compact-edge surface[7] (*).

From the moment Yves had "hooked", he did his thesis in a year, a year and a half, results, writing, everything, and on top of it all. It was a brilliant thesis, not as thick as most of those done with me, but as substantial as any of those eleven theses. I defended it in May 1976.

The thesis is still unpublished today. It may not have been thick, but apparently it was still too thick to be publishable, among many other excellent reasons I've been given. I mention some of them in the note "On n'arrête pas le progrès" (n° 50). The story of my efforts to "place" this unfortunate thesis, one of the best I've ever had the pleasure of inspiring, would make a small book, which would surely be instructive but which I've given up writing. Among the close friends of yesteryear who had such good reasons for forgetting to read the results and burying the whole thing with their eyes closed, there is
are (in order of appearance on stage) Norbert A. Campo, Barry Mazur, Valentin Poenaru, Pierre Deligne
p. 400 - not counting B. Eckmann via Springer[8] (*). The central result will finally appear,
nine or ten years later and reduced to the bone, in a short article in Topology (shhh - I have an accomplice on the Editorial Board of this esteemed journal. . .). The rest of the work, on the one hand, demonstrated things that everyone has always used without demonstration (and we certainly did well without it!), and on the other hand, developed typical grothendieckeries, completely contrary to usage and good morals. I'm well aware that if my friend Deligne doesn't take it upon himself to "discover" them in the next ten years, others won't be able to resist repeating them in thirty or fifty years' time, since my healthy instinct tells me that these are fundamental things. They have been a precious thread in my Anabelian cogitations, and if God lends me life, I'll have ample opportunity to refer to them in the part of Mathematical Reflections developing the yoga of Anabelian algebraic geometry.

This adventure was a revelation for me, the first of its kind - the revelation of something I didn't become fully aware of until the reflection of l'Enterrement. I've tended to forget it ever since, my mind being absorbed elsewhere. Yves Ladegaillerie, one of the most brilliant students I've ever had, understood right from the start that to be accepted in today's mathematical world, it's not enough to put in the hard work and meet all the requirements of excellence. Having more than one string to his bow, for seven years he devoted himself to more down-to-earth tasks and to the

[7](*) The "analogous" statement in the non-oriented case is false - decidedly a tricky result, carefully "sliced" into a set of equally "plausible" but nonetheless false hypothesis-conclusions! For further comments on Ladegaillerie's work, see Esquisse d'un Programme, especially the beginning of par.3.

[8](*) I don't know Eckmann personally, and my correspondence to have Yves' thesis published by Lecture Notes was with Dr. Peters, in charge of LN at Springer. I believe that through the fifteen or so volumes of LN that were published by me (SGA in particular) or by students (theses) in the sixties, I was among those who contributed by their endorsement to the credit and unprecedented success of this series, still in its infancy. The reason given for rejecting the work I recommended (that they didn't publish theses) was a joke.

My first experience of the New Look in correspondence also dates from this episode: with a truly impressive ensemble, A. Campo, B. Mazur, V. Poenaru and Dr. Peters refrained from honouring me with a reply to a second letter, when naively (I have a slow comprehension. . .) I returned the charge, after their reticent reply which showed that they hadn't bothered to acquaint themselves with the results set out in the introduction to Ladegaillerie's work.

less problematic returns. He is fortunate to have held a position as assistant professor eve

n before his unfortunate encounter with me, providing him with security that his misadventure did not jeopardize. L'anp . 401
last a mathematical spark seems to have awakened again, on a theme very close to those I've been interested in over the last few years - hyperbolic geometry à la Thurston and its relationship to the Teichmüller group. It's even possible that we'll go a little way together again, or that he'll take his own personal walk, just for the fun of it, and without expecting any return other than that which mathematics itself can give. He knows that if he expects any more, it's in his interest to change his interlocutor or fellow traveller (and even his past. . .).

16.1.3. Coffin 3 - or jacobiennes a little too relative

Note 95 My first encounters with Carlos Contou-Carrère were in the corridors of the Institut de Math, shortly after my arrival in Montpellier in 1973. He'd corner me in some obscure corner and pour a torrent of mathematical explanations on me, before I'd even had time to apologize politely and duck out of the way. What he was pouring out at an impressive rate went right over my head, without him even pretending to notice, or being the least bit bothered when I timidly let him hear. He was in desperate need of someone to talk to, and I wasn't his only "unwilling interlocutor". This was at a time when I was absolutely not into maths. For a year or two, I would run away as soon as I saw his (easily repaired) silhouette appear at the end of a corridor. It was like that until Lyndon, who had been at Montpellier for a year as an associate professor, let me know that Contou-Carrère had some unusual resources and was about to be shipwrecked for lack of knowing how to use them. Until then, the question of whether or not what Contou-Carrère was spouting at me held water, and whether or not he had the means, hadn't even crossed my mind, so far away was all that. Perhaps Lyndon's suggestion came at a time when I was beginning to take an interest in mathematical questions again. In any case, I took the bit by the teeth and asked Contou-Carrère if he would explain something he had done, so that I could understand him. I suspect I was the first to ask him such a thing, at least in the many years he'd already been in France. It wasn't easy to get him to explain something, but it wasn't easy.
by no means impossible, and it was well worth the effort. I soon realized that Lyndon hadn't been wrong - that p. 402
Contou-Carrère was full of ideas that just needed to be carefully worked out and developed, and he had an immediate and very sure intuition in practically any mathematical situation that could be put before him. This rapidity and certainty of intuition, even in things with which he was completely unfamiliar, was beyond me and impressed me - the only other pupil where I experienced it to a comparable degree was Deligne[9] (*). On the other hand, he had an almost total block against writing! Incredibly, he could do maths **without writing** - God knows how he managed to do even that little, not to mention communicating with others, where he was a total "shipwreck" (see above).

If I had something urgent and useful to teach Contou-Carrère, it was the art of writing, or more frus- tely, just making him understand that maths is done by **writing** it. I must have tried for two years, maybe three, until 76 or 77[10] (**), without being quite sure whether I really succeeded entirely. His first major work written entirely in black and white was his thesis on the cycles of

[9](*) I'm not sure I've come across it in any other mathematicians, except Pierre Cartier (who impressed me in his youth with this remarkable ability) and Olivier Leroy, mentioned in the following note.
[10](**) (June 7) Checked, it was until February 1978.

Schubert, defended only last December (1983)[11] (***). Between 1978 and the present day, our relations have been very episodic, my role being practically confined to supporting him as best I can in the nom-
p. 403 many occasions when he d found himself trapped in one way or another in his professional life, constantly suspended on the most precarious assistant-delegate positions.

For two or three years, I had been trying to provide Contou-Carrère with the foundations of a precise and flexible mathematical language and some principles of systematics. With this background, and his resources and wealth of ideas, he was spoilt for choice as to what to branch out into. Rather than start with ideas of his own, he branched off into the theory of relative local and global Jacobians, which I had mentioned to him as a possible thesis topic. Once I'd left him to his own devices, in the space of barely a year he produced some very fine work, part of which is announced in a note to CRAS (95_1). Going all the way would have meant a few years of exciting, highly motivating work, while at the same time learning all the finer points of schematic technique. I had no doubts at the time - it was obvious to me that Cartier, Deligne and Raynaud would all give a warm welcome to the work already done, which was profound, difficult and unexpected in many ways. Cartier was delighted to see some of his old ideas take on new relevance. Raynaud, on the other hand, was indifferent, as was Deligne, who kept the complete manuscript in his drawers for six months, without deigning to give any sign of life[12] (*).

It was two against one - enough to feel the wind. The slightly too relative Jacobeans are sine die written off. The chainsaw did its job...

That didn't mean we didn't have a few mishaps at Contou-Carrère, a detailed account of which would be useful.
well another little book, which I willingly give up writing. It's around this time, I think, that for the first and only time since I left (in 1970) the institution I'd been the only one to work in for four years (1958-), I decided to write another little book.
p. 404 62) to represent and make credible "in the field", during the years when it still had no roof over its head to her - it's the only time I've ever taken it upon myself to recommend someone for an invitation (for a year, in this case), at a time when Contou-Carrère was in danger of finding himself out of a job and on the street. I knew that the person I was recommending, just as unknown as Hironaka, Artin or Deligne had been when I warmly welcomed them to the IHES, would do as much honor as they had to the institution that welcomed him. Of course, I didn't fail to say so. Fortunately for Contou-Carrère, his position as assistant delegate (admittedly unworthy of the honor of an invitation to such a select institution) was finally renewed[13] (*).

I wasn't that surprised by this episode, knowing Deligne's disposition even then, and given that

[11](***) It's a long piece of work (which I haven't read) in which he carefully develops ideas in which I have no part, giving, among other things, an explicit resolution of the singularities of all "Schubert"-type cycles - something nobody has been able to do before him. For once, when he did write a formal essay, he was criticized for it being too detailed (not to mention that his statements were too general. . .)! For my part, if I have a criticism to make, it would be in the opposite direction: while Contou-Carrère asserts that his methods should apply to all types of semisimple groups and Schubert cycles, he has only done the work in the case of the general linear group - so he hasn't gone all the way with the work that needs to be done on the precise question: description of the resolutions of equivariant singularities of universal Schubert cycles, **and** of the singular loci of said Schubert cycles. This lacuna seems to me to be a legacy of this "block" against piecework and writing, which had long been his main handicap.

[12](*) Contou-Carrère had been ahead of the game, however, and didn't breathe a word in his note about me, who had provided him with the initial program. It was all in vain - no matter how much he put into it, there's an unmistakable "style" attached, whether you like it or not, to certain themes, which it's best to avoid if you want to make a career in maths today. (June 7) After checking with the person concerned, I note that I am confusing here two different episodes concerning Contou-Carrère's work on Jacobians. See the following note (n° 95) for further details and precise references.

[13](*) I can't complain, since five or six years later, on the occasion of IHES's twenty-fifth jubilee last year, I was indeed honored with an invitation, and even given the choice between the solemn reception with the minister's speech, or a subsequent week's stay at IHES, again all expenses paid (I was assured). I told my old friend Nico Kuiper that it was very kind of him to have thought of me like that, but that I didn't travel anymore at my age... ...

Nico Kuiper had warned me that everything depended on him in this particular case. (It didn't even occur to me to suggest to him that this might also apply to the other members of the Scientific Council, given the case in point... .) The episode that touched me the most, however, of all Contou-Carrère's misadventures (my "protégé", as Verdier had taken it for granted to call him in a letter....), takes place in October 1981, in connection with his application for a teaching post in Perpignan. Colleagues in Perpignan (where he had his position as assistant delegate) surely appreciated the presence among them of someone who was at ease and could be consulted in virtually all branches of mathematics. When a teaching post became vacant, they put him forward as the sole candidate for the position. - a rare move, which made it clear that they wanted him and no one else in the job. C.C. had relatively few publications apart from his doctoral thesis in Argentina with Santalo, mostly notes to CRAS, announcing results (some of them profound), but without demonstration. No one had ever suggested to him that, in these times of unprecedented competition, it's better to have articles with complete demonstrations as "exhibits".
which I'd preached to him enough myself, but from a less utilitarian point of view[14] (**). Still
that Contou-Carrère's candidateu□re deemed inadmissible by the Comité Consultatif des Universités and the p. 405
case referred. The thing that struck me at the time was that neither the President of the CCU (the national body that made the decision), on behalf of the Committee, nor any of the members in a personal capacity, had the minimum respect to write, either to the main interested party Contou-Carrère himself, or at least to the Director of the Institut de Mathématiques de Perpignan, to give a few words of explanation on the meaning of this vote, which in the absence of any explanation could only be received as a stinging disavowal of the choice of the Perpignan colleagues, and as a disavowal of their only candidate as suitable to honorably fill the post for which he was proposed. The Council included three of my former students, two of whom knew Contou-Carrère personally. Of course, they knew that he had been a pupil of mine as well as theirs, especially as the dossier included a particularly glowing report by me on the candidate's work. None of them, nor any of the other members of the Council, thought about the affront represented by this vote-couperet without any other form of trial, and the torpedoing of a mathematician just as honorable as any of them.

It was this incident that, for the first time in my life as a mathematician, made me feel that "breath" I've spoken of more than once in the course of my reflections. I had already felt it four years earlier, with the episode of the[15] (*) strangers. But it wasn't within the world that had been mine, blowing on **one of the theirs** - on someone who unreservedly identified with this world. It made me feel sick, for
weeks; perhaps months. To free myself from an anguish that then embraced me without□that I cared p. 406
to read it[16] (*), I became restless, writing letters left and right, and a thirty-page text "Le Cerveau et le Mépris" (The Brain and Contempt), in a darkly humorous vein, which I finally decided not to publish[17] (**). With hindsight, I realize that it was the perfect time to **meditate** on the meaning of this

[14](**) The year before, Contou-Carrère had applied for a professorship in Rennes, where he knew Berthelot and Larry Breen. His application was considered admissible by the CCU, but the position was awarded to another candidate. No one bothered to warn him that if he was to have any chance of a position, he would have to publish detailed demonstrations of the results he was announcing. The following year's disavowal by the CCU came as a complete surprise to Contou-Carrère, his Perpignan colleagues and myself. With hindsight and in the light of the present reflection, I doubt whether the situation will really change with the writing of his thesis (already declared "unpublishable" as it stands) and its defense, and whether he will have any chance of finding a teaching post in France.

[15](*) See "My farewells - or strangers", s.24. - 406

[16](*) I became aware of this anguish only during a long period of meditation the following year, when I discovered the role of anguish in my life, whose presence (chronic until 1976, and occasional after 1976) had been "the world's best-kept secret" all my life. There were highly effective mechanisms in place, which concealed all the generally recognized signs of anguish, which remained ignored by myself and those close to me.

[17](**) I was discouraged from publishing it by the very people for whom I was about to go to war, to whom I had the good sense to say: "We're going to war!

that was happening. The funny thing is that what was "preventing" me from even realizing the need for in-depth meditation, was a long meditation I was engaged in at the time[18] (***) and about which I've had occasion to speak) - and a meditation, what's more, on my relationship to mathematics (if not on my past as a mathematician)! She was troubled by an episode in which life was calling out to me forcefully - and I evaded the challenge by getting agitated, then diving back into "meditation". Looking back, I realize that this "meditation" didn't fully deserve the name, that it lacked an essential dimension of true meditation: attention to myself at the **very moment**. I was "meditating" on the meaning of certain more or less remote events, while ignoring a repressed anguish (perfectly controlled, it's true, as a result of a long habit of such control), a sign of my refusal to take cognizance of the message this rejected "breath" was bringing me.

But I'm getting away from my point. The torpedoing, of course, had the effect it was bound to have. The Perpignan colleagues were called to order once, and that was enough. Apparently, they don't even have an assistant delegate position any more, at least not for Contou-Carrère. He has found a replacement at short notice in Montpellier, for the current year, whose incumbent will return next year.

I'm not too worried about his future, though, as it's been a while since Contou-Carrère had the wisdom to get ahead of the curve, and plugged into IT. With the means

p. 407 his brilliance, he must have been dominating the subject from on high for a long time now, while doing the math he's been

loves in his spare time. He's a father with two children, and maths in these times, with the past hanging over him, is decidedly hazardous, not to say violent. It's in his interest to have a brilliant career as a computer scientist, where no one will hold it against him that he was even the slightest bit my pupil.

Note 95_1 (June 7) It was towards the end of '77 that I submitted to Contou-Carrère a detailed work plan for a theory of local and global relative Jacobians, including, in the local case, the suggestion of "screwing back" Cartier's Jacobian and ind-group, in order to find a "complete" Jacobian with a more beautiful universal property, and which would be "autodual". I had no idea of a demonstration to propose, and didn't bother with his work after February '78, having realized that my presence inhibited rather than stimulated his abilities. He managed to "get going" within a year, and his first note "The generalized Jacobian of a relative curve, construction and universal factorization property" (concerning the global case) appeared on 16.7.1979 (CRAS t.289, Série A - 203).

The following month he found the decisive results for the local Jacobian, but didn't publish anything on the subject for a year and a half, when he published "half" (the universal property of the ordinary local relative Jacobian, unrevised with the Cartier group), in a note to CRAS dated March 2, 1981, under the name (not very convincing at first sight) "Corps de classes local géométrique relatif" (CRAS t.292, Série I - 481). As for the theory of the complete local Jacobian, even more interesting in my opinion, there is a draft note to the CRAS, which was never published, under the title: "Local Jacobian, universal Witt bivector group and tame symbol". Of course, I was informed of his results as early as 1979, i.e. a complete realization of the provisional program I had proposed to him, for which it had been necessary to overcome considerable technical difficulties, requiring a great deal of imagination and technical power. I was aware (unless I was mistaken) only of the first note, and was astonished that he didn't publish the rest, i.e. the local part, without ever giving a clear explanation for it - but he was obviously disappointed by the reception given to this work.

p. 408 first note. After his unsuccessful application to Rennes in 1980, and given that my letter of support attached to his application file reported remarkable results on global and local relative Jacobians,

show my text before attempting to make it public.

[18](***) See "The troublemaker boss - or the pressure cooker", s.43.

he must still have considered it prudent (in preparation for his candidacy the following year in Perpignan) to publish at least one more note on the local Jacobeans, or else empty his entire bag. It was only two months later, in May '81, that he sent the draft of his third note to Deligne and Raynaud (no doubt Cartier had known about it for a long time), presumably to test the waters. (I don't think he would have had the slightest difficulty in getting Cartan to present this third note, at any time since August 1979 when he had the results in hand). Neither Raynaud nor Deligne gave him any sign of life - but in March 1982 Deligne sent him the manuscript of an article "A remark on tame symbols", dedicated to Deligne, by Kazuya Kato, which makes Contou-Carrère's theory in the case of a basic body, and conjectures its validity on any basic ring. Contou-Carrère told me about it, saying he was convinced that Deligne had communicated his results (without naming him, or giving any indication of a demonstration) to K. Kato. At the time, the thing seemed so incredible that I didn't take Contou-Carrère seriously - although now I realize that it would be quite in the usual "thumbs up!" style of my brilliant friend Deligne. Contou-Carrère seemed genuinely outraged that anyone would "presume to conjecture" about something he seemed to regard as a kind of private property. Yet Contou-Carrère himself took his conjectures from me, without even thinking it necessary to allude to me in any of the three notes[19] (*)! From him to me, it must have seemed self-evident, whereas the mere presumption that Deligne would do the same to him outraged him, but he didn't dare breathe a word of it to the interested party. (I had urged him to explain himself to Deligne, which he did not. . .)

In a way, I imagine he's had to do violence to himself over the years not to publish some very fine results, in which he's had to invest his heart and soul in making them. If he's done himself such violence, it's out of concern for an economic climate that's clearly not conducive to this kind of grothendieckery. In the last few days, he was astonished to receive a letter from the same Deligne, expressing surprise that he had not published
his note on "total" Jacobians, and asking him for everything he possessed on the subject and even on others. Zoghman Mebkhout had already told me a few days before that Deligne was using these things etp . 409
that he had even named Contou-Carrère in this context. It would seem that the time is ripe for Contou-Carrère to finally recognize a child of his own, whom he has been careful to bury for nearly five years. Perhaps, who knows, the time has even come for a reconciliation between the two "pupil-enemies"; these two most brilliant of my pupils, one a medal-winning academician and the other a delegated assistant, and yet (whether they reconcile or not) for a long time two **brothers**.

16.1.4. Casket 4 - or topos without flowers or wreaths

Note 96 (May 22) I'd hardly be exaggerating if I said I'd never met Olivier Leroy. What is certain is that from the moment he heard about me, he decided to avoid me like the plague. His reasons, I confess, escape me. Perhaps an instinct told him that I was only going to get him into trouble, or maybe Contou-Carrère (who was very friendly with him for a long time) told him so - I may never know. All the same, I had the honor and pleasure of two substantial conversations with Leroy, which I remember very well.

The first time must have been in '76, '77, when Contou-Carrère and I went to see him at his place, out of the blue, just to talk math a bit - I don't know if we had any ulterior motives. But perhaps it was understood that Olivier was thinking of embarking on a 3° cycle doctorate, and I certainly had plenty of subjects up my sleeves. Having seen him once or twice at Contou-Carrère's, and from what Contou-

[19](*) On the subject of a certain conniving role I've often played in this kind of situation with some of my students, see the note "Ambiguity", n° 63".

As Carrère himself suggested, I had a feeling that Olivier must be a quick study, and not just in maths. It was a memorable evening for the three of us. I soon had to tell Olivier about a program for a fundamental group theory of a topos and van Kampen theorems in the topossic framework, and he seemed interested. He must have had some topossic background from Contou-Carrère's algebraic geometry seminar, and he seemed interested in having an opportunity to "get his teeth into" the language of topos on an example of concrete theory. For a good two or three hours, I had to pour over him a detailed masterpiece of the theory I had in mind to develop, which was fleshing out
as I talked about it, and as a host of concrete situations in algebraic geometry and topology came up in my mind - situations that had to be expressed in the cadre topossique, and that with each
p. 410
times I first had to "remind" someone who was hearing about it for the first time. More than once in the course of the evening, Contou-Carrère (who has read almost everything and has a strong stomach) looked vague and disoriented, and even for him it was a lot to take in - and more than once I thought it prudent to ask Olivier if it wouldn't be better to stop for today and start again another day. I could have saved myself the trouble - Olivier was visibly fresh, bright-eyed and perfectly at ease, and I even laughed, because I couldn't believe he wasn't cracking up, but he wasn't! He was a young guy, maybe twenty years old, who must have had just a tincture of schematics, a bit of topology and topos, and he'd worked quite a bit with infinite discrete groups, I think... . It was three times nothing, to tell the truth, and with that he still managed to fill in all the blanks and "feel" effortlessly what I, an old veteran, was telling him at top speed in two or three hours on the basis of a fifteen-year familiarity with the subject. I'd never come across anything like it, or at most with Deligne, and perhaps Cartier, who had also been quite extraordinary in this line in his younger days.

Olivier was going to write his 3° cycle thesis on the subject in question. Little did he know what was waiting for him at the end. In any case, I never saw him again during the two years he worked on the project and beyond. His official boss was Contou-Carrère, but it would have been a pleasure to chat with such a hip guy. In fact, I wasn't even notified of the defense, and don't think I ever received a copy of this thesis - but I remember
having held a copy in my hands, from someone who had been entitled to it[20] (*). I couldn't say whether the defense was done before or after the "pouring" of the note to the CRASs where Olivier summarized his work. I
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discusses this casting, in some detail but without naming anyone, in the section "La note - ou la nouvelle éthique (1)" (s.33). The two mathematicians who took care of this casting were Pierre Cartier (the same one whose incredible speed of intuition had come back to me when talking about that of his young non-colleague, whom Cartier was casting so kindly and with all the regret in the world), and the other was Pierre Deligne,

[20](*) All this secrecy is all the more unusual in that I was surely, along with Contou-Carrère, the only person in the whole Languedoc region who could understand anything of Olivier Leroy's work. Needless to say, I never got my hands on Leroy's draft note to the CRAS either. Perhaps I'm deluding myself, but it seems to me that if I hadn't been so drastically sidelined that it was practically impossible for me to intervene, I would have found a way to get this unfortunate note published, through Cartan or Serre if need be, who aren't connected, but who would have trusted me if I had guaranteed the seriousness of the work. (June 7) I must have learned long afterwards that Leroy had passed his thesis, and was too busy on my side to think of asking myself then how it was that I hadn't even been informed. It didn't "click" until after Contou-Carrère's own thesis defense, for which I'm supposed to have been the thesis supervisor (x). He found a way to ensure that, as the only member of the jury, I was not entitled to the definitive, offi cial copy of his thesis! I fi nally just received a copy today - he had thought (he wrote) that I "wasn't interested" in having one. . .

(x) More precisely, for a year or two, C.C. had cautiously played on two "directors" at once (you never knew. . .), each of them unaware of the existence of a "parallel" director. I was informed of Verdier's role as director in extremis, when C.C. finally fell back on me in the spring of 1983, when it became clear that Verdier decidedly wanted his hide anyway!

with his historic remark that mathematics "didn't amuse him", (although it did "amuse" him at a young age. . .) I should add Contou-Carrère himself, who didn't lift a finger to defend his pupil.

- This exposed him to the risk of upsetting powerful men. He must have suggested to Olivier Leroy that it would be better to forget the episode of his unfortunate thesis. What is clear, however, is that Leroy has indeed drawn a line under this episode - even if the opportunity arose to publish not just a note to the CRAS, but his entire work, I doubt he would make use of it[21] (*). This time again, the chainsaw has done its job[22] (**).

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Despite this misadventure, I still had the pleasure of seeing Leroy regularly for several months in early 1981. It was at a micro-seminar I was giving on the algebraic-arithmetical theory of Teichmüller's tower (discussed at some length in Esquisse d'un Programme). The only literal listeners were Contou-Carrère and Leroy. Even for an ultra-selective Parisian audience (and I know what I'm talking about), there wouldn't have been three or four of them in a whole room, so as not to be left out. To tell the truth, the reason I was doing this seminar, at a time when Contou-Carrère was entirely taken up with fine-tuning his ideas on Schubert's cycles, was for Leroy, thinking that maybe he'd catch on to such a splendid subject. He obviously "sensed" what I was doing, but had decided in advance (I think) that he wouldn't "catch on". Strange that he even bothered to come - something must have fascinated him, just as it did me, and he wasn't too clear himself about what he really wanted. When I realized he wasn't going to go for it, I called it a day. I wasn't interested in continuing a monologue in front of two spectators, no matter how brilliant they were. It was at this point that I had my second and last conversation with Leroy. I don't think I've seen him since.

There's been no real mathematical discussion between Leroy and me, apart from the one seven years ago. - which explains why I know virtually nothing about the work he did, apart from his unfortunate topossical work. His misadventure must not have increased his confidence in people like me, or even Contou-Carrère or other people from the beau monde mathématique. I heard he was doing a seminar

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at the Faculté des Lettres, where there's a group of sympathetic mathematicians who get on well with each other. There are

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[21](*) An eloquent sign of this "gros trait": in Olivier Leroy's application for an assistant's position at Montpellier; submitted during a vacancy two years ago, Leroy mentions neither the title of his post-graduate thesis, nor the name of Contou-Carrère who had been his boss, nor does he mention any personal work whatsoever. Clearly, he hadn't decided whether he wanted the job or not - which is why, despite his impressive credentials, it was awarded to another candidate with a solid track record and for whom there was no doubt as to his intentions.

[22](**) By an interesting coincidence, I recently heard that Cartier had dedicated one of his Bourbaki lectures to me (the first time such a thing has happened to me, I believe), and that the lecture was devoted to the theory of topos - the very topos that Cartier had deemed unworthy of inclusion in a note to the CRAS. Sign of a change in the fashion winds of recent years? Certainly not, and it all ties together again: the presentation in question concerned the use of topos in logic! My friend Cartier's touching dedication seems to me to be in the same vein as the Funeral Eulogy delivered on a special occasion last year (see the note "L'Eloge Funèbre - ou les compliments", n° 104), in which the word "topos" is pronounced (among other well-sent compliments), only to add (as a unique and eloquent comment) that they are "used today in logic" - and nowhere else, need I say, at least as long as my friends who are lavish with compliments can prevent it, by the power in their hands. . .

(Reference to Cartier's talk: Catégories, logiques et faisceaux, modèles de la théorie des ensembles, Séminaire Bour- baki n° 513, Feb. 1978).

I sense in the condescending (and boycotting. . .) attitude of some (such as Deligne, Cartier, Quillen, among those who set the tone. . .), towards innovative and profound notions such as that of topos in geometry, a phenomenal **haughtiness.** Even supposing that just one of them had the stuff (or the innocence. . .) to draw out of nothing, as I have done with the introduction of étales and crystalline topos, a new topological vision of algebraic varieties (and from there, the means for a profound renewal of algebraic geometry and arithmetic, while awaiting topology) - there's no doubt that the very attitude of contempt that he likes to cultivate in himself and arouse in others, defuses this power of vision and renewal, for the sole benefit of a fatuity.

would expound ideas of combinatorial topology - a subject that's been right up my alley for nearly ten years. As I'm by nature a discreet person (but yes, yes!), I didn't ask any questions about what he was talking about, and I don't know whether he intended it for publication. As far as his situation is concerned, he leads a most illegal existence (although he's neither a foreigner nor in an illegal situation), doing tutorial work right and left, paid for (shhh. . .) by I don't know what slush funds and under the noses of the treasurer-payer and the Cour des Comptes. I don't think he's made up his mind whether or not he's finally going to pursue a career in mathematics, and it must be an uncomfortable situation in the long run, Cour des Comptes or not. I'd be delighted if my edifying painting of a Funeral, in which he appears as the fourth assistant coffin, could help him dispel his perplexities, this time with full knowledge of the facts.

16.1.5. The Gravedigger - or the entire congregation

Note 97 (May 24) It was against a certain reluctance on my part that I finally decided to mention by name some of my close friends and colleagues in the mathematical world of yesteryear, whom I have seen act as "gravediggers" (or "chainsaws"), cutting short, from the outset, the attempts made by certain mathematicians of modest or precarious status, to take up some of my ideas and develop them according to their own logic, or only (as in the case of Yves Ladegaillerie) to follow in their footsteps.
an approach and style that bears the stamp of my influence. As I've said over and over again, such
p. 414 reluctance to involve others, or only to name them[23] (*) without a voir consulting them, was not uncommon during Récoltes
and Semailles. In each case, I eventually examined the reticence and realized that it was unfounded, that its source was not delicacy but confusion, not to say pusillanimity. In all the cases (it seems to me) where I mentioned by name the acts or attitudes of others, these were in no way of a "confidential" nature. They concerned the professional life of the person concerned, with all the repercussions that implies in the professional lives (and by extension, in the lives of all) of other colleagues, including myself. Each of those I involve is just as responsible for his or her actions and attitudes, and for the full range of their implications (whether or not he or she likes to ignore them), as I am for mine. He has no right to take offence if some of the consequences of his actions come back to him in one form or another, for example that of a public "mise en cause", in this case through me. If at times my language is colourful and harsh, my intention is in no way polemical, nor to offend or outrage anyone, but rather to describe the facts and the way I feel about them, as an incentive for everyone (and first and foremost for those I imply) to examine them on their own, rather than dismissing them one way or another (as I often did myself before the Harvest and Sow reflection). If the person being questioned chooses to be offended, that's his or her choice. This choice may pain me, coming from people I hold in esteem or even affection, but it doesn't weigh on me. The reluctance I mentioned, a sign of a certain confusion in my vision of things, vanished without a trace as soon as it was

[23](*) I was reluctant, for example, to include a note (note n° 19) in which mention would be made by name of all the students who had prepared and completed a state doctorate thesis with me. This hesitation in me must have stemmed from the reluctance of many of my students to see themselves associated with me, a reluctance I must have perceived at an informal level for some years already. The only ones among my former students (with or without quotation marks) where the desire to distance themselves from my person had been clearly perceived by me at the time, were Contou-Carrère (with whom I had only just discovered it), and Deligne (where the thing had already been quite clear since 1968, without my suspecting however how far this desire would lead him). In Deligne's case, I was particularly reluctant to name him as having been "more or less" a pupil, as I didn't want to seem like I was taking advantage of such a brilliant "pupil", when he himself didn't want to reveal the link that bound him to me and my work. My reflection made me realize, moreover, that this link had taken on an infi nitely greater significance in the life and work of my young friend than I had ever suspected.

(June 1) See the note dated March 27 (three days later) "L'être à part" (n° 67').

understood and, by the same token, surpassed.

At no time during the reflection on l'Enterrement did I feel that some vast "conspiracy" had been hatched against my work and against those who had the temerity to draw inspiration from it (rather than simply borrowing tools, keeping quiet about the name of the worker who had fashioned them and placed them in their hands). There is no conspiracy, but there is a **consensus** which, in what I have called "the great mathematical world",
has so far appeared to me to be without fault. This consensus, except at most in rare exceptions, is in no way fueled by any cons□cient "malice" toward my person or my work. In p. 415
Only in exceptional cases did it express itself in unequivocal malevolence towards one or other of the four "co-sitters" mentioned in the previous notes[24] (*). But surely such malevolence could not have proliferated in any of my former pupils, and could only have been expressed unhindered by the encouragement of general consensus.

In most, if not all, of my former friends and students, this consensus manifests itself not in "malicious" attitudes, but in (I believe) entirely unconscious mechanisms of unrolling uniformity and flawless efficiency, sweeping aside like chaff common sense and healthy mathematical instinct, to make way for purely automatic **attitudes of rejection**[25] (**). Such automatic attitudes, I suspect, are not only aroused by me and those whose mathematical "smell" recalls it in some way - but also by any mathematician who does not present himself as already invested with the **tacit endorsement of** a certain "establishment"; either he himself is already part of it, or
that he appears as the "protégé" (to use an expression from Verdier's pen) of one of these.
It seemed to me □that in almost all mathematicians, dispositions of a minimum "openness p. 416
mathematical instinct" (necessary for this "common sense" and "healthy mathematical instinct" to come into play) **can only be triggered in relation to someone who has already been granted such a guarantee**.

This kind of mechanism must be practically universal, not only in the mathematical world, but in all sectors of society without exception. It goes far beyond any individual case. If (as it seems to me) there is an exceptional situation in the case of my person, and of those who in the eyes of the establishment are "my protégés", it is because in the past I was invested with the status of "one of theirs", with the usual effect of a "minimum of openness" towards me and "mine". This status was taken away from me when I left in 1970. Or more precisely, by my own choice, clearly expressed on more than one occasion in the years following my departure, and by my way of life to this very day, I have indeed ceased to be one of "them". In fact, I myself no longer felt "one of them", and I left a world that was common to us with no spirit of return. Even today, my "return to maths" is by no means a return "among them", to the establishment, but a return to mathematics itself; more

[24](*) Only in the cases of Deligne and Verdier did I learn of what I consider to be unequivocal acts of malice.

[25](**) These attitudes of rejection, of course, never present themselves as such, even in extreme cases like those of my friend Deligne, or Verdier. They are almost invisible in terms of conscious attitudes towards me, which (as I've already had occasion to say) are almost always (perhaps even always), in the case of my friends and pupils of yesteryear, attitudes of sympathy (from which some of them sometimes try as best they can to defend themselves) and respect. Such dispositions of sympathy and respect are present, not only at the superficial level of conscious "opinions", but even at the deeper level of real attraction (or repulsion), and of real knowledge of others (independently of the images in which we strive to enclose them).

Here we find ourselves in a typical situation **of ambivalence** (collective, I'd almost be tempted to say) where, as far as the eye can see, we "see" nothing! (Compare with the reflection in "The Enemy Father (1), (2)" (sections 29, 30), where for the first time in Harvest and Sowing I address this ambivalent aspect that has marked many relationships in my life, and not only in the mathematical milieu). And yet, at the level of concrete manifestations (examined at length in Burial), the "resultant" of these ambivalent forces no longer has anything ambivalent about it, it seemed to me, but it does present itself, with "baffling uniformity and unfailing effi cacy", as the "attitude of automatic rejection" that I'm about to examine more closely.

precisely, a "return" to a continuous mathematical investment, and to an activity of publishing my mathematical reflections.

I'm only just beginning to realize how much my departure was felt as a kind of "desertion", even as an "outrage" by my former friends and pupils[26] (*). It must have been the easiest way to evacuate the meaning of my departure, the questioning it could arouse in them, by such a diffuse feeling of a **wrong received**, and the automatic reaction of a grudge, expressing itself in an act of **retaliation** (which rarely had to be perceived as such, or even as an act, on a conscious level) : since he has cut himself off from us, we cut ourselves off from him - we cease to grant him and "his kind" the benefit of the "automa- tism of attention" reserved for "ours" - he and his kind will be entitled, like the first-timers, to the rigours
automatic rejection!

p. 417 The situation is further complicated (for my former friends and students) by the fact that not only was I doing
part of the establishment, but that moreover it is impossible for any of them to do their job as mathematicians without using at every step notions, ideas, tools and results of which I am the author. I don't know if there has ever been, in the history of our science or any other science, an example of such an embarrassing paradox! Seen in this light, the chainsaw effects (by no means limited to my friend Deligne) to cut off any vel- leity of development for ideas that bear my imprint (when such development could only increase this perplexity) now present themselves to me as driven by an implacable inner logic, as a **necessity** from a certain choice already made - the choice of rejection. And the same is true of the efforts I see being made just about everywhere to sweep under the carpet the origins of those notions, ideas, tools and results that have become part of our common heritage, and which we can no longer do without, whether we like it or not. This "indifference" that I've noticed in the face of Deligne's very large "operations", pretending to claim, one by one, the paternity of a certain number of my main contributions to mathematics (or, for the crumbs, generously attributing them to some inseparable friend) - this is by no means indifference, but **tacit approval**. Deligne is simply doing what the establishment's collective unconscious expects him to do: **erasing** the name of the man who cut himself off from everyone else, and thus resolving the intolerable paradox **by replacing a real but unacceptable paternity with a tolerable fake paternity**.

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

I have little doubt that this vision of things essentially expresses the reality of things: everything at
p. 418 at least on a collective level. Surely my "return," which unexpectedly ends a funeral that was going on so satisfactorily for all, or (if it doesn't end it) at least disrupts so
unseemly and inadmissible the unfolding of a ceremony that seemed to have been settled in advance - this return is going to inconvenience and displease not only this or that among the principal officiants, but also embarrass the entire congregation assembled for this funereal occasion! And I have no idea, of course, what "pa- rade" this famous collective unconscious will come up with, to clear up the mess created by the untimely return of the deceased, suddenly (and unacceptably scandalously) stepping out of the cozy coffin intended for him, and pretending to officiate his own funeral in his own way. I trust, however, that the congregation will find

[26](*) My friend Zoghman Mebkhout expressed this way of seeing and feeling things particularly eloquently. It's because of this desertion that I'm responsible for his difficulties with the mathematical world, since he alone has been deprived of the "protection" and support that those who now treat him like a trollop once found with me.

a good way of evacuating this little additional contradiction in the mathematical edifice!

I seem to see quite clearly now, at the level of the images and attitudes of each individual, the reflection and general form taken by the collective consensus, and the collective will to erase, to bury. It's the universally used system of "two mutually contradictory tables" on which we operate simultaneously, and which I first had occasion to discuss in Harvest and Sowing in the case of my own person. (See the section "Merit and contempt", s. 12.) I doubt there's anyone who would say flatly and plainly: "Grothendieck has done nothing but bogus mathematics, let's not talk about it any more and get down to business". As it stands, this would be too explicitly contrary to the axioms of the establishment, for the time being at least. In the foreseeable future, in twenty or thirty years' time, the question won't even arise, as the name will no longer even be mentioned. The common tactic, both individually and collectively, is that of silence: we don't think of the deceased, not as a mathematician at least, we don't talk about him, and we don't mention him (except, when we can't do otherwise, by the providential acronym SGA or EGA, until these references are replaced by others from which all trace of the deceased is absent).

Yet there are occasions, exceptional no doubt, when complete silence becomes impracticable. One such occasion, I imagine, was my application for admission to the CNRS, which must have embarrassed more than a few of my colleagues.
from a[27] (*). An au□tre will be the preliminary distribution of Récoltes et Semailles[28] (*), pending its publication

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in volume 1 of Réflexions Mathématiques (if my publisher doesn't crack and refuse to blame the entire scientific establishment). These are opportunities created by the inadmissible deviations of the deceased himself, unhappily stepping out of the role assigned to him. Another occasion (perhaps more instructive for an understanding of the Burial, before its disruption by an unruly deceased) is the twenty-fifth anniversary jubilee of the IHES, which was celebrated last year "with great pomp". As "the first of the four Fields medals of the IHES", it would have been difficult to pass over me entirely in silence on this solemn occasion - even if my role in giving the IHES a real existence in the four heroic years of its existence was overlooked. The eulogy concocted in my honor, in the brochure published to mark this jubilee (to which I have already referred twice), seems to me to be a model of its kind - as an elegant and discreet way of resolving, to everyone's satisfaction, this "little contradiction" in contemporary mathematics... .

And suddenly I'm all revived - like the horse that's starting to smell the stable! Almost two weeks ago, I began to reflect on this instructive episode, in a note that immediately took the name "L'Eloge Funèbre - ou les compliments". After some hesitation as to where to place this note (taken from a late footnote to the first of the notes written for the Funeral), it appeared that the most natural place to insert it was (not the "chronological" place, but) in the "Funeral Ceremony" which is to complete the Funeral. And so, without having looked for it, the "thread" I've been following for the last three weeks, through the last three processions "The Colloquium", "The Student" and finally "The Funeral Van", which has only just joined the convoy, connects with the final part of the Burial, namely the Funeral Ceremony;

[27](*) (May 26) I've just learned that my colleagues on the Comité National at the CNRS have made an effort on my behalf, by offering me a two-year "position d'accueil". I don't know whether they did so enthusiastically - but none of my friends on the Committee went out of their way to give me a call or a note to tell me the good news (dated May 15).

(September) I was informed of this in a letter from the CNRS dated August 16 - it was a one-year (not two-year) appointment to a research associate post.

[28]This is a limited edition (150 copies) produced by my university for distribution among my closest colleagues and friends.

p. 420 this ceremony marked above all, precisely, by this chef d'oeuvre Funeral Eulogy which I began to examine on May 12, and which now constitutes the note following quite naturally from this one[29] (*).

I'm finally (again?) on target! And at the same time, this beginning of reflection on a eulogy suddenly takes on a new dimension. It's no longer just the ingenious invention of a powerful brain at the service of a fixed idea, expending itself before the indifference or commanding attention of the distinguished guests at an official "grand occasion" - but it's above all the perfect, deftly served response, made on this delicate occasion of all, to a collective expectation, about the attitude that should be taken towards my person. If anyone of his generation has earned the unreserved gratitude of the entire congregation, it's my friend Pierre Deligne, who fulfilled the role expected of him with his unmistakable perfection.

[29](*) (November 1984) Following an unforeseen episode of illness, the note in question (n° 104) is separated from "this one" by a new procession - "The deceased - still not deceased" (n° s 98-103).