

# Harvest and Sowing

Reflection and testimony  
on a past as a mathematician

by

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Second Part:

The Burial (I)

or the robe of the Chinese Emperor

To those who were my friends  
as well as the very few who have remained so  
and the great many who came join my Funeral

To the memory of a memorable Colloquium...

and to the entire Congregation...

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# A) HERITAGE AND INHERITORS

## I. The posthumous student

(44') 1. The failure of teaching (2) - or creation and fatuity (50) 略

(!44'') 2. A sense of injustice and powerlessness... 略

(45) 略

## II. My orphans

(46) 1. My orphans (50)

I would like to take this opportunity to say a few words about the mathematical notions and ideas, among all those I have brought to light, which seem to me to have (by far) the greatest significance (46.1) <sup>(1)</sup>. These are, above all, five closely related key notions, which I will quickly review, in the order of increasing specificity and richness (and depth).

This concerns first of all the idea of derived category in homological algebra (48), and its use for a "catch-all" formalism, the so-called "formalism of six operations" (namely the operations  $\otimes^L$ ,  $Lf^*$ ,  $Rf_*$ ,  $RHom$ ,  $Rf^*$ ,  $Lf^!$ ) (46.2) for the cohomology of the most important types of "spaces" that have been introduced so far in geometry: "algebraic" spaces (such as schemes, schematic multiplicities, etc...), analytic spaces (both complex analytic and rigid-analytic and similars), topological spaces (awaiting, of course, the context of "tame spaces" of all kinds, and surely many others, such as that of the category **Cat** of small categories, serving as homotopic models... ). This formalism encompasses just as well coefficients of a discrete nature and "continuous" coefficients.

The progressive discovery of this duality formalism and its ubiquity was made through a solitary, obstinate and demanding reflection, which continued between 1956 and 1963. It is during this reflection that the notion of derived category gradually emerged, as well as an understanding of its role in homological algebra.

What was still missing in my vision of the cohomological formalism of "spaces" was an understanding of the link that one could sense between discrete coefficients and continuous coefficients, beyond the familiar case of local systems and their interpretation in terms of integrable connections on modules, or [*cristaux de modules*]. This deep link, 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 with

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<sup>1</sup> The reader will find in notes no. 46.1 to 46.9 some more technical comments on the notions reviewed in this note. On the other hand, independently of the particular notions I will introduce, the reader will find reflections on what I consider to be the "master part" of my work (within the part of my "fully completed" work), in note no. 88 "The corpse".

the help of "constructible" discrete coefficients, and on the other hand with the help of the notion of "D-module" or "complex of differential operators" (46.3).

For nearly ten years, for lack of encouragement from those of my former students who were in the best position to give him encouragement, and to support him by their interest and by the experience they had acquired through their contact with me, Zoghman Mebkhout has continued his remarkable work in almost total isolation. This did not prevent him from uncovering and proving two key theorems<sup>(2)</sup> of a new crystalline theory that was being born bit by bit in general indifference, both of them (it was definitely a bad sign!) expressed in terms of derived categories: one giving the category equivalence pointed out earlier between "discrete constructible" coefficients and crystalline coefficients (satisfying certain "holonomy" and "regularity" conditions) (48'), the other being "the" crystalline global duality theorem, for the constant map of a smooth complex analytic space (not necessarily compact, which means considerable additional technical difficulties need to be dealt with) to a point. These are deep theorems<sup>(3)</sup>, which shed new light on the cohomology of both analytic and schematic spaces (in zero characteristic for the moment), and hold the promise of a vast renewal of the cohomology theory of these spaces. They finally earned their author, after two applications to the CNRS rejected, 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 struggling with the considerable technical difficulties due to the transcendental context, about the "formalism of the six variances", which is well known to my students<sup>(4)</sup>, but does not appear "on the net" anywhere. He finally learned of its existence from me last year (in the form of a form which, apparently, is known only to me...), when he was kind enough and patient enough to explain what he had done, to me, who was not so much into cohomology anymore... Nor did anyone think to suggest to him that it might be more "profitable" to first connect to the context of schemes of characteristic zero, where the difficulties inherent in the transcendental context disappear, and where in contrast the conceptual questions fundamental to the theory appear all the more clearly. Nobody thought of pointing out to him (or only noticed what was known to me from the time I introduced the crystals<sup>(5)</sup>) that the "D-modules" on smooth (analytic or schematic) spaces are neither more, nor less than "*cristaux de modules*" (when one ignores all

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<sup>2</sup> (7 June) Mebkhout points out to me that to these two theorems, a third one should be added, also expressed in terms of derived categories, namely what he has called (somewhat improperly perhaps) the "biduality theorem" for D-modules, and which is the most difficult of the three. For an overview of Mebkhout's ideas and results and their uses, see Le Dung Trang and Zoghman Mebkhout, *Introduction to linear differential systems*, Proc. of Symposia in Pure Mathematics, vol. 40 (1983) part. 2, p. 31-63.

<sup>3</sup> (30 May) The proof of the second theorem runs into the technical difficulties common for the transcendental contexts, which requires the use of "évêques" techniques, I guess it belongs to the class of "difficult" proofs. The proof of the first theorem is "obvious" - and profound, using the full force of Hironaka's resolution of singularities. As I pointed out in the penultimate paragraph of the note "solidarity" (no. 85), once the theorem itself is clear, "the first person" who is well informed will be able to prove it. Compare also with the observation of J. H. C. Whitehead quoted in the note "The snobbery of the young - or the defenders of purity" (no. 27). When I was writing this last note, as if under the silent dictation of a secret prescience, I did not realise how much reality would surpass my timid and groping suggestions!

<sup>4</sup> They learned this first hand in the SGA 4 and SGA 5 seminars, and through texts in R. Hartshorne's 'Residues and Duality'.

<sup>5</sup> (30 May) But I forgot it over time - and remembered it thanks to the second meeting with Mebkhout last year. (See the note "Encounter from beyond the grave", no. 78.

questions of "coherence" for both), and that the latter was a catch-all notion which worked just as well for "spaces" with any singularities as for smooth spaces (46.4).

Given the capabilities (and uncommon courage) shown by Mebkhout, it is quite clear to me that, given a sympathetic atmosphere, he would have had no trouble but great pleasure in establishing the complete formalism of the "six variances" in the context of the crystal cohomology of schemes of zero characteristic, when all the essential ideas for such a large-scale programme (including his own in addition to those of the school of Sato and mine) were already, it seems to me, in place. For someone of his calibre, this was a matter of a few years' work, just as the development of a catch-all formalism of étale cohomology was a matter of a few years (1962-1965), since the guiding thread of the six operations was already known (in addition to the two key theorems of basis change). It is 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 discuss, that of scheme, and the closely related notion of topos. The latter is the more intrinsic version of the notion of site, which I had first introduced to formalise the topological intuition of a 'localisation'. (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 schemes and topos in quick succession. This pair of notions potentially contains a vast renewal of both algebraic geometry and arithmetic, as well as of topology, by a synthesis of these "worlds", too long separated, in a common geometrical intuition.

The renewal of algebraic geometry and arithmetic through the point of view of schemes and the language of sites (or of "descent"), and through twelve years of work on the key foundations (not counting the work of my students and other good wills who joined in) has been accomplished over the last twenty years: the notion of scheme, and that of étale cohomology of schemes (if not that of étale topos and that of étale multiplicity) have finally become part of the common heritage.

On the other hand, this vast synthesis which could also include topology, while for twenty years the essential ideas and the main technical tools required seem to me to have been gathered and ready<sup>(6)</sup>, is still waiting for its time. For fifteen years (since my departure from the mathematical scene), the notion of topos, which is the

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<sup>6</sup> (15 May) These 'essential ideas and principal technical tools' 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 an unrecognisable, 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. of the following page). These vicissitudes and their meaning are gradually revealed in the course of the reflections of the last four weeks, continuing in the notes "The compère", "La table rase", "L'être à part", "Le signal", "Le renversement", "Le silence", "La solidarité", "La mystification", "Le défunt", "Le massacre", "La dépouille", notes no. 63", 67, 67', 68, 68' and 84-88.



fertile unifying idea and powerful tool of discovery, is maintained by a certain fashion<sup>(7)</sup> in banishment of notions considered serious. 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, differentiable and other spaces that the topologist handles on a daily basis are, along with the schemes (of which he has heard) and the topological, differentiable or schematic multiplicités (of which no one speaks), so many embodiments of the same type of remarkable geometric objects, the ringed topos (46.5), which play the role of "spaces" in which the intuitions coming from topology, algebraic geometry, and arithmetic converge into a common geometric vision. The "modular" multiplicités of all kinds that one encounters at every step (provided one has eyes open to see) provide many striking examples (46.6).

Their in-depth study is a first-rate guiding thread to penetrate further into the essential properties of geometric objects (and others, if there are objects that are not geometric...) whose modular multiplicités describe the modalities of variation, degeneration and generalisation. However, this richness remains ignored, since the notion that allows us to describe it in detail does not fit into the commonly accepted categories.

Another unexpected aspect brought by this recused synthesis<sup>(8)</sup> is that the familiar homotopic invariants of some of the most common spaces (46.7) (or more precisely, their profinite compactifications) are provided with unexpected arithmetic structures, in particular with operations of some profinite Galois groups...

However, for nearly fifteen years, it has been part of the good taste in the "big world" to look down upon anyone who dares to pronounce the word "topos", unless it is in jest or if he has the excuse of being a logician. (These are people known to be different from the others and to whom one must forgive certain whims...) The yoga of derived categories, to express the homology and cohomology of topological spaces, has not penetrated into topologists either, for whom the Künneth formula (for coefficients in a ring which is not a field) still continues to be a system of two spectral sequences (or at most, a myriad 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 a smooth morphism for instance), which (in the neighbouring framework of stellar cohomology) constituted the crucial turning point for the "start" in force of this cohomology (46.8).<sup>8</sup> and which still ignore the base change theorems (for a proper morphism or by a smooth morphism for instance), which (in the neighbouring framework of étale cohomology) constituted the crucial turning point for the "start-up" on the strength of this cohomology (46.8). I have nothing to be surprised about, when the very people who

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<sup>7</sup> (13 May) After further reflections in the six weeks since these lines were written (end of March), it became clear that this 'fashion' was established first and foremost by some of my students - by those who were in the best position to make a certain vision, ideas and technical tools their own, and who chose to appropriate working tools, while disavowing both the vision that had given rise to them, and the person in whom that vision had originated.

<sup>8</sup> 略

contributed to the development of this yoga have forgotten it a long time ago, and beat coldly the unfortunate person who pretends to want to use it! <sup>(9)</sup>

The fifth notion that is dearest to my heart, perhaps more than any other, is that of 'motive'. It differs from the four previous ones in that "the" right notion of motive (even if only over a base field, let alone any base scheme) has not been satisfactorily defined so far, even if one were to admit all the "reasonable" conjectures one would need for that purpose. Or rather, obviously, the "reasonable conjecture" to be made, in a first step, would be that of the existence of a theory, satisfying such and such data and properties, which it would not be at all difficult (and quite fascinating!), for someone in the know<sup>(10)</sup>, to make fully explicit. I came close to doing so, shortly before I "quit maths".

In some respects, the situation resembles that of the "infinitesimals" 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, which our predecessors lacked. And secondly, in spite of having these tools at our disposal and for more than twenty years since this visibly essential notion appeared, no one has deigned (or dared, in defiance of those who do not deign to. . . ) to get down to work and draw up the broad outlines of a theory of motives, as our predecessors did for the infinitesimal calculus, without going into the details. It is as clear now for motives as it was once for "infinitesimals", that these beasts exist, and that they manifest themselves at every step in algebraic geometry, as long as one is interested in the cohomology of algebraic varieties and families of such varieties, and more particularly in the "arithmetical" properties of these. Perhaps even more than for the other four notions I have mentioned, the notion of motive, which is the most specific and richest of all, is associated with a multitude of intuitions of all kinds, by no means vague but often formulable with perfect precision (even if it means admitting a few motivic premises). The most fascinating of these "motivic" intuitions was for me that of "motivic Galois group" which, in a sense, allows to "put a motivic structure" on the profinite Galois groups of fields and schemes of finite type (in the absolute case). (The technical work required to give a precise meaning to this notion, in terms of the "premises" which give a provisional foundation for the notion of motive, was done in Neantro Saavedra's thesis on "Tannakian categories").

The current consensus is somewhat more nuanced for the notion of motive than for its three unfortunate brothers (or sisters) (derived categories, the duality formalism known as the "six operations", topos), in the sense that it is not outright called "bombinage"<sup>(11)</sup>. In practice, however, it amounts to the same thing: as long as there is no way to "define" a motive and "prove" something, serious people only refrain from talking about it (with the greatest regret, of course, but one is either serious or one is not. . . ). Obviously, one will never be able

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<sup>9</sup> (13 May) In the course of subsequent reflection, it became clear that the situation began to change with the Luminy Colloquium in June 1981: we saw those who had "forgotten" (or rather, buried...) these notions strutting about with them, without however ceasing to beat coldly at the same "unfortunate" without whom this brilliant Colloquium would never have taken place. (See notes no. 75 and 81 on this memorable Colloquium).

<sup>10</sup> 略

<sup>11</sup> 略

to construct a theory of motives and "prove" anything about them, as long as he declares that it is not serious to even 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, one can prove many things. That is to say that, as of today, in fact since the notion appeared in the wake of Weil's conjectures (proven, however, by Deligne, which is a good thing nonetheless!), the yoga of motives does indeed exist. But it has the status of a secret science, with admittedly very few insiders<sup>(12)</sup>.

It may be "not serious", but it nevertheless allows those rare insiders to say in a host of cohomological situations "what one should expect". It thus gives rise to a multitude of intuitions and partial conjectures, which are sometimes accessible after the fact by the means at hand, in the light of the understandings provided by the 'yoga'. Several of Deligne's works are inspired by this yoga<sup>(13)</sup>, in particular the one which (if I am not mistaken) was his first published work, establishing the degeneracy of the Leray spectral sequence for a smooth projective morphism of algebraic varieties (in characteristic zero, which is needed in the proof). This result was suggested by considerations of "weight", of an arithmetical nature. These are typically "motivic" considerations, I mean: formulable in terms of the "geometry" of motives. Deligne proved this statement with Lefschetz-Hodge theory and (if I remember correctly) did not say a word about motivation (49), without which certainly no one would have had the idea to expect something so implausible!

The yoga of motives was born precisely, in the first place, from this "yoga of weights" that I got from Serre(\*\*). It was he who made me understand the full charm of Weil's conjectures (which became Deligne's theorem). He had explained to me how (modulo a hypothesis of resolution of singularities in the characteristic considered) one could, thanks to the yoga of weights, associate to each algebraic variety (not necessarily smooth or proper) over any field of "virtual Betti numbers" - something which had struck me greatly at the time (46.9). It was this idea, I think, that was the starting point for my thinking about weights, which continued (in the margin of my writing tasks on foundations) throughout the following years. (I also took up this idea in the 1970s, with the notion of a "virtual motive" over some base scheme, with a view to establishing a "six operations" formalism at least for virtual motives). If throughout these years I spoke about this yoga of motives to Deligne (as a privileged interlocutor) and to whoever wanted to hear it<sup>(14)</sup>, it was certainly not so that he and others could keep it in the state of a secret science, reserved for them alone.

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(46.1) I would make an exception at most for the ideas and point of views introduced with the formulation I had made for the Riemann-Roch theorem (and with the two proofs I found for it), as well as for various variants of it. If I remember correctly, such variants appeared in the last exposé of the SGA 5 seminar of 1965/66, which was lost together with various other exposés of the same seminar. It seems to me that the most interesting one is a variant for constructible discrete coefficients, which I do not know if it has been explained in the literature

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<sup>12</sup> 略

<sup>13</sup> 略

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since then<sup>(15)</sup>. Note that this also admits a "motivic" variant, which essentially amounts to asserting that the "characteristic classes" (in the Chow ring of a regular scheme  $Y$ ) associated to constructible  $l$ -adic sheaves for different primes  $l$  (prime to the residual characteristics), when these sheaves come from the same "motive" (for example for  $R^i f_! (\underline{\mathbb{Z}}_l)$  for a given  $f: X \rightarrow Y$ ), are all equal.

(46.2) This formalism can be considered as a kind of quintessential "global duality" formalism in cohomology, in its most "efficient" form, freed from all superfluous hypotheses (notably, of smoothness for the "spaces" and applications under consideration, or of properness for the morphisms.) It should be completed by a local duality formalism, in which we distinguish among the allowed "coefficients" the so-called "dualising" objects or "complexes" (a notion stable under the  $Lf_!$  operation), i.e. those giving rise to a "biduality theorem" (in terms of the  $R\text{Hom}$  operation) for coefficients satisfying suitable finiteness conditions (on the degrees, and of coherence or "constructability" on the local cohomological objects). When I speak of the "six-variance formalism", I hereafter mean 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 complexes of coherent cohomology modules. A second was the discovery (in the context of étale cohomology of schemes) that this formalism also applied for discrete coefficients. These two extreme cases were sufficient to found the conviction for the ubiquity of this formalism in all geometrical situations giving rise to a Poincaré-type "duality" - a conviction that was confirmed by the work of (among others) Verdier, Ramis and Ruget. It will certainly be confirmed for the other types of coefficients, when the blockage which for fifteen years has been exerted against the development and wide use of this formalism breaks down.

This ubiquity seems to me a fact of considerable significance. It made imperative the feeling of a deep unity between Poincaré duality and Serre 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 the phenomena of cohomological duality "in all directions"<sup>(16)</sup>. The fact that this kind of sophisticated structure has not been explained in the past (nor the "good" notion of "triangulated category", of which the Verdier version is still a very provisional and insufficient form) does not change anything; nor 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, just as they ignore the language of the derived categories on which it is based.

(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, from a point of view (as I understand) rather different from that followed by Mebkhout, but closer to my approach.

The various notions of "constructability" for "discrete" coefficients (in the complex-analytic, real-analytic, piecewise linear contexts) were first worked out by me, it seems, in the late fifties (and I took them up again a

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<sup>15</sup> (6 June) I found it again (in a similar form, and under the flattering name of "Deligne-Grothendieck conjecture") in a paper by MacPherson published in 1974. See for details the note no. 87.1

<sup>16</sup> The interested reader will find an outline of this formalism in the Appendix to this volume.

few years later in the context of étale cohomology). I had asked the question of the stability of this notion by direct higher images for a proper morphism of real or complex analytic spaces, and I do not know if this stability has been established in the complex analytic case<sup>(17)</sup>. In the real analytic case, the notion I had considered was not the right one, because I did not have the notion of Hironaka's real subanalytic set, which has the essential property of being stable under direct images. As for operations of a local nature such as  $R\mathrm{Hom}$ , it was clear that the argument establishing the stability of constructible coefficients in the framework of excellent schemes of zero characteristic (using Hironaka's resolution of 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 of the "ubiquity" of the duality formalism, at the early times of étale cohomology (during which a main surprise had precisely been the discovery of this ubiquity).

Returning to the semi-analytical case, the "right" framework in this direction for stability theorems (of coefficients constructible by the six operations) is obviously that of "tame spaces" (see Sketch of a Programme, par. 5, 6).

(46.4) Of course, the "D-modules" point of view, together with the fact that  $D$  is a coherent sheaf of rings, brings out a more hidden notion of "coherence" for *[cristaux de modules]* than the one I used to work with, and which continues to make sense on (analytic or schematic) spaces which are not necessarily smooth. It would be only fair to call it "M-coherence" (M for Mebkhout). It should be quite obvious then, for someone who is in the know (and in full possession of his healthy mathematical instincts), that the "right category of coefficients" which generalises the complexes of "differential operators" in the smooth case, must be none other than the derived category "M-coherent" of that of *[cristaux de modules]* (a complex of crystals being called M-coherent if its cohomology objects are so). This one continues to make reasonable sense without smoothness hypothesis, and should encompass both the theory of ordinary "continuous" (coherent) coefficients, and that of "constructible" discrete coefficients (after introducing for the latter suitable holonomy and regularity hypotheses). If my vision of things is correct, the two new conceptual ingredients of Sato-Mebkhout's theory, compared to the previously known crystalline context, are this notion of M-coherence for *[cristaux de modules]*, and the conditions of holonomy and regularity (of a deeper nature) concerning the M-coherent complexes of crystals. With these notions acquired, a first essential task would be to develop the six-variance formalism in the crystal context, so as to encompass the two special cases (ordinary coherent, discrete) that I had developed more than twenty years ago (and that some of my ex-cohomology students have long since forgotten in favour of undoubtedly more important tasks...).

Mebkhout had indeed eventually learned of the existence of a notion of 'crystal' from my writings, and he felt that his viewpoint must 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 this necessary extensive work on foundations, placed as he was in a climate of haughty indifference from the very people who were the cohomological authorities, best placed to encourage - or discourage...

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<sup>17</sup> (25 May) It was established by J. L. Verdier, see "The good references", note no. 82.

(46.5) (13 May) The focus here is on topos ringed by a local commutative ring. The idea of describing a "variety" structure in terms of the data of such a sheaf of rings on a topological space was first introduced by H. Cartan, and was taken up by Serre in his classical work FAC (Faisceaux algébriques cohérents). It is this work that was the initial impulse for a reflection leading me to the notion of "scheme". What was still missing in Cartan's approach, taken up by Serre, to encompass all the types of "spaces" or "varieties" that have arisen up to now, was the notion of topos (that is to say, precisely the "something" on which the notion of "sheaf of sets" makes sense, and possesses the familiar properties).

(46.6) As other remarkable examples of topos which are not ordinary spaces, and for which there does not seem to have any satisfactory substitute in terms of "accepted" notions either, I will point out: the quotient topos of a topological space by a local equivalence relation (for example, the foliations of varieties, in which case the quotient topos is even a "multiplicity" i.e. is locally a variety); the "classifying" topos for just about any kind of mathematical structure (at least those "expressed in terms of finite projective limits and arbitrary inductive limits). When we take a structure of "variety" (topological, differentiable, real or complex analytic, Nash, etc. ... or even schematically smooth over a given basis) we find in each case a particularly attractive topos, which deserves the name of "universal variety" (of the species considered). 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 for a long time, but for the moment it is by no means the case...

(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 subfield  $K$  of the complex field, such that  $K$  is an extension of finite type of the prime field  $Q$ . The profinite Galois group  $\text{Gal}(\bar{K}/K)$  then operates in a natural way on the profinite homotopic invariants of  $X$ . Often (for example when  $X$  is an odd-dimensional homotopic sphere) one can take for  $K$  the prime field  $Q$ .

(46.8) (May 13) When I first learned about algebraic geometry from Serre's FAC paper (which was to "trigger" me in the direction of schemes), the very notion of a change of basis was virtually unknown in algebraic geometry, except in the special case of the change of base fields. With the introduction of the language of schemes, this operation has become probably the most commonly used in algebraic geometry, where it shows up all the time. The fact that this operation is still practically unknown in topology, except in very special cases, seems to me to be a typical sign (among many others) of the isolation of topology from the ideas and techniques coming from algebraic geometry, and a stubborn inheritance of the inadequate foundations of "geometric" topology.

(46.9) (5 June) Serre's idea was that one should be able to associate to any scheme  $X$  of finite type over a field  $K$ , the integers

$$h^i(X) \quad (i \in \mathbb{N})$$

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

a) for  $Y$  a closed subscheme and  $U$  the complementary open

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

b) for  $X$  smooth projective, we have

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

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

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

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

## (47) 2. Refusal of a legacy - or the price of a contradiction

It will be noted that four of the five notions I have just reviewed (precisely those which pass for "not serious" things) are concerned with 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. That is to say that a main motivation and a constant Leitmotiv in my work, during the fifteen years from 1955 to 1970, was the cohomology of algebraic varieties.

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 about it in last year's IHES brochure<sup>(18)</sup>. I came across this with some surprise. Of course, I was still "on the scene" and all that is trendy, when Deligne (after his beautiful work on the Ramanuyam conjecture) developed his remarkable extension of the Hodge theory. This was above all, for him as well as for me, a first step towards a formal construction of the notion of motives over the complex field - to start with! In the first years after my "turning point" of 1970, I of course also heard about Deligne's proof of the Weil conjectures (which also proved Ramanuyam's conjecture), and in the wake of that, the "hard Lefschetz theorem" in positive characteristic. I expected no less from him! I was even sure that he must have proved at the same time the "standard conjectures", which I had proposed towards the end of the sixties as a first step to found (at least) the notion of "semi-simple" motives over a field, and to translate some of the expected properties of these motives in terms of properties of  $l$ -adic cohomology and groups of algebraic cycles. Deligne later told me that

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his demonstration of Weil's conjectures would certainly not allow him to demonstrate the (stronger) standard conjectures, and that he had no idea how to approach them. That must have been about ten years ago now. Since then, I am not aware of any other really decisive progress in understanding the "motivic" (or "arithmetic") aspects of the cohomology of algebraic varieties. Knowing Deligne's abilities, I had tacitly concluded that his main interest must have turned to other subjects - hence my surprise to read that this was not the case.

What seems to me to be beyond doubt is that for the last twenty years it has hardly been possible to make a large-scale revival in our understanding of the cohomology of algebraic varieties without also being more or less a "continuator of Grothendieck". Zoghman Mebkhout learned this the hard way, and (to a certain extent) so did Carlos Contou-Carrère, who soon realised 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 "formalism of the six variances" in contexts of various coefficients, as close as possible to that of the motives (which for the moment play the role of a kind of ideal "horizon line"): crystalline coefficients in zero characteristic (in the line of the Sato school and Mebkhout, with Grothendieck sauce) or  $p$  (studied especially by Berthelot, Katz, Messing and a whole group of obviously motivated younger researchers), "stratified promodules" à la Deligne (which appear as a dualised variant, or "pro", of the "ind"-notion of coherent  $D$ -module, or " $D$ -coherent" crystal), and finally "Hodge-Deligne" (which seem as good as motives, except that their definition is transcendental and limited to basic schemes which are of finite type over the complex field)... At the other end of the spectrum is the task of clearing the very notion of motive from the mists which surround it (for good reason...), and also, if possible, to tackle such precise questions as the "standard conjectures". (For the latter, I had thought, among other things, of developing a theory of "intermediate Jacobians" for projective and smooth varieties over a field, as a way 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 I "left maths" - burning, juicy things, none of which ever appeared to me as a "wall", a stopping point<sup>(19)</sup>. They were an inexhaustible source of inspiration and substance - something where you just had to pull where it stuck out (and it 'stuck out' everywhere!) for something to come, the expected as well as the unexpected. With the limited means [*moyens*] that I have, but without being divided in my work, I know very well how much can be done if one puts one's mind to it, in a single day, or in a year, or ten years. 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 wants to put 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 lies at the bottom of the 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 moreover with the assistance from him, among many others who have put their hands to the task... ) (59).

I am also thinking of the "Deligne-Mumford" compactification of the moduli multiplicity  $M_{\{g,v\}}$  (over  $\text{Spec}(\mathbb{Z})$ ), for connected smooth algebraic curves of genus  $g$  with  $v$  marked points. They were introduced(\*) on the occasion of the problem of proving the connectedness of the moduli spaces  $M_{\{g,v\}}$  in any characteristic, by a specialization argument from the zero characteristic. These objects  $\hat{M}_{\{g,v\}}$  seem to me (together with the group  $\text{Sl}(2)$ ) the most beautiful, the most fascinating objects I have encountered in mathematics (47.2). Their

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<sup>19</sup> 略



mere existence, with such perfect properties, seems to me a kind of miracle (more importantly, perfectly well understood), of incomparably greater scope than the fact of connectedness that was to be proved. For me, they contain in quintessence what is most essential in algebraic geometry, namely the (more or less) totality of all algebraic curves (over all possible basic fields), which are precisely the ultimate building blocks of all other algebraic varieties. But the kind of objects we are talking about, the "proper and smooth multiplicities on  $\text{Spec}(\mathbb{Z})$ ", still escape the "admitted/*admits*/" categories, that is to say, those that we are willing (for reasons we are not careful to examine) to "admit". The common mortals speak of it at most by allusions, and with an air of apology for appearing to be still doing "general non-sense", whereas care has been taken to say "stack" or "champ", so as not to pronounce the taboo word of "topos" or "multiplicité". This is undoubtedly the reason why these unique gems have not been studied or used (as far as I know) since their introduction more than ten years ago, except by myself in unpublished seminar notes. Instead, one continues to work either with the "coarse" moduli varieties or with finite coverings of moduli multiplicities that have the appearance of being real schemes - both of which, however, are only relatively insignificant and lame shadows of those perfect gems from which they originate, and which remain virtually banned...

Deligne's four works on the Ramanuyam conjecture, on mixed Hodge structures, on the compactification of moduli multiplicities (in collaboration with Mumford), and on Weil's conjectures, each constitute a renewal of our knowledge of algebraic varieties, and thereby a new starting point. These fundamental works follow each other in a space of a few years (1968-73). However, for almost ten years, these great milestones have not been the springboards for a new launch into the glimpse and the unknown, and the means for a renewal on a larger scale. They have led to a situation of morose stagnation (47.3). It is certainly not that the "means" that were there ten years ago, in some and in others, have magically disappeared; nor that the beauty of things at our fingertips has suddenly vanished. But it is not enough that the world is beautiful - we must also deign to rejoice in it...

(47.1) I am thinking here of the promising start by Contou-Carrère, five or six years ago, of a theory of local relative Jacobians, their links with global Jacobians (called "generalized Jacobians") for smooth curved schemes and not necessarily proper over any scheme, and with Cartier's theory of formal commutative groups and typical curves. Apart from an encouraging reaction by Cartier, the reception to Contou-Carrère's first note, by those who were best placed to appreciate it, was so cold, that the author refrained from ever publishing the second one he kept in reserve, and hastened to change subject (without avoiding other misadventures)<sup>(20)</sup>.

I had suggested to him the theme of local and global Jacobians, as a first step towards a programme which goes back to the end of the fifties, oriented in particular towards a theory of an "adelic" dualising complex in any dimension, formed with local Jacobians (for local rings of arbitrary dimension), in analogy with the residual complex of a Noetherian scheme (formed with the dualising modules of all its local rings). This part of my cohomological duality programme found itself (along with others) somewhat relegated to oblivion, during the sixties, due to the influx of other tasks which then appeared more urgent.

(47.2) Actually, it is the "Teichmüller tower" in which the family of all these multiplicities fits, and the discrete or profinite paradigm of this tower in terms of fundamental groupoids, which constitutes the richest and most

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<sup>20</sup> (8 June) See sub-note (95.1) to the note "Coffin 3 - or the somewhat too relative Jacobians", no. 95.

fascinating single object I have encountered in mathematics. The group  $Sl(2)$ , with the "arithmetic" structure of the profinitely compactified  $Sl(2, \mathbb{Z})$  (carrying the action of the Galois group  $Gal(\bar{\mathbb{Q}}/\mathbb{Q})$  on it), can be considered as the main building block for the "profinite version" of this tower. See on this subject the indications in "Sketch of a Programme" (awaiting for the volumes of Mathematical Reflections that will be devoted to this theme).

(47.3) This observation of a "morose stagnation" is not a well-considered opinion of someone who is well aware of the main episodes, in the last ten years, around the cohomology of algebraic schemes and varieties. It is a simple impression of an outsider, which I got from conversations and correspondence with Illusie, Verdier, Mebkhout, in 1982 and 1983. There are surely many ways in which this impression could be qualified. For instance, Deligne's work "Conjectures of Weil II", published in 1980, represents a substantial new advance, if not a surprise in terms of the main result. It seems that there has also been progress in crystalline cohomology of  $\text{car. } p > 0$ , not to mention the "rush" around intersection cohomology, which ended up making some people (unwillingly) return to the language of derived categories, and even remembering long repudiated paternities...

### III. The Fashion - or the Life of the Illustrious Men

#### (48) 1. Instinct and fashion - or the law of the strongest 46

As is well known, the theory of derived categories is due to J. L. Verdier. Before he undertook the foundational work I had proposed, I had confined myself to working with derived categories in a heuristic way, with a provisional definition of these categories (which later turned out to be the right one), and with an equally provisional intuition of their essential internal structure (an intuition that turned out to be technically wrong in the intended context, as the cone mapping does not depend functorially on the arrow in a derived category that is supposed to define it, and defines it only up to non-unique isomorphism). The theory of duality of coherent sheaves (i.e. the formalism of the "six variances" in the coherent framework) that I had developed towards the end of the fifties<sup>(21)</sup>, only made sense as part[*module*] of a foundational work on the notion of derived category, which was done by Verdier later.

The text of Verdier's thesis (passed only in 1967), about 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 (several of which are due to Verdier himself). It was only the introduction to a work in progress, which was eventually 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, 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 there is still a copy somewhere...) the only text, to this day, which presents systematic foundations of homological algebra from the point of view of derived categories.

Perhaps I am the only one to regret that neither the introductory text nor the foundations themselves have been published<sup>(22)</sup>, with the result that the technical baggage essential for the use of the language of derived categories is scattered in three different places in the literature<sup>(23)</sup>. This absence of a systematic reference text of comparable weight to the classic Cartan-Eilenberg book seems to me to be both a cause and a typical sign of the disaffection that struck the formalism of derived categories after I left the mathematical scene in 1970.

It is true that as early as 1968 it was already clear (for 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

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<sup>21</sup> Still missing was an operation  $Rf_!$  (cohomology with proper support) for a non proper morphism, which was introduced six or seven years later by Deligne, thanks to the introduction by him 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).

<sup>22</sup> (25 May) After these lines were written, I discovered that the first embryo of Verdier's thesis, dating from 1963 (four years before the defence) was eventually published in 1967. See on this subject the notes "The partner" and "Thesis on credit and all risks insurance", no. 63" and 81.

<sup>23</sup> These places are: Hartshorne's well-known seminar on coherent duality, containing the only published part of the duality theory I had developed in the second half of the 50s; one or two papers by Deligne in SGA 4; one or two chapters of Illusie's voluminous thesis.

category, were insufficient for certain needs, and that a more thorough foundational work remained to be done. A useful but still modest step in this direction was taken (mainly for the purposes of the trace case) 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 reflection on the foundations of homological algebra, as well as on those, intimately linked, of a theory of motives (48.1). However, as far as the former are concerned, all the essential ideas for large-scale foundations seemed to have been acquired in the years before my departure (48.2). (Including the key idea of the "derivator", or "machine for manufacturing derived categories", which seems to be the common richer object underlying the triangulated categories we have encountered so far; an idea that will finally be developed somewhat in a non-additive framework, almost twenty years later, in a chapter in volume 2 of Pursuing Stacks). Moreover, a large part of the foundational work to be done had already been done by Verdier, Hartshorne, Deligne, Illusie, work which could be used as it was for a synthesis taking up of the ideas acquired in the wider perspective of the derivators.

It is true that this disaffection over the past fifteen years<sup>(24)</sup> for the very notion of derived category, which for some has been akin to the disavowal of a past, goes in the direction of a certain fashion, which affects to look with disdain at any reflection on foundations, however urgent it may be<sup>(25)</sup>. On the other hand, it is quite clear to me that the development of étale cohomology, which "everybody" uses today without looking twice (not least implicitly via Weil's conjectures...) could not have been done without the conceptual baggage constituted by the derived categories, the six operations, and the language of sites and topos (developed first for this very purpose), not to mention SGA 1 and SGA 2. And it is equally clear that the stagnation that can be observed today in the cohomological theory of algebraic varieties could not have appeared let alone settled in, if some of those who were my students had known, during those years, how to follow their healthy mathematician instinct rather than a fashion that they were among the first to introduce, and which since a long time ago, and with their support, has become the law.

(48.1) The same can be said (with certain reservations) of my entire programme of foundations of algebraic geometry, of which only a small part was realised: it came to a screeching halt with my departure. I was especially struck by the stop in the duality programme, which I considered particularly juicy. The work of Zoghman Mebkhout, which was continued against all odds, is nevertheless in the line of this programme (renewed by the contribution of unexpected ideas). The same is true of Carlos Contou-Carrère's work from 1976 (mentioned in note (47.1)) - work that he had the prudence to suspend sine die. There was also a work on duality in fppf cohomology of surfaces (Milne). That is all I know of.

It is true that I never thought of writing an outline of the long-term programme of work that had emerged for me in the years between 1955 and 1970, as I did for the last twelve years, with the Sketch of a Programme. The reason for this is simply, I believe, that there was never a particular occasion (such as my application to the CNRS) to motivate such an exhibition. One will find in the letters to Larry Breen (from 1975) which are reproduced as an appendix to Chap I of the History of Models (Mathematical Reflections 2) some indications

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<sup>24</sup> (24 May) There is a need to qualify these "fifteen years" - see on this subject the note no. 47.3, as well as the more detailed note "Thesis on credit and all risks insurance", no. 81.

<sup>25</sup> (25 May) For a reflection on the forces at work in the emergence and persistence of this fashion, see the note "The Gravedigger - or the whole Congregation", no. 97.

on certain theories (of duality in particular) on my agenda before 1970, theories which are still waiting for arms to enter the common heritage.

(48.2) The same is true for the theory of motives, except that it is likely to remain conjectural for some time.

## **(48') 2. The unknown in service and the theorem of the good God 46**

While it is customary to call the key theorems of a theory by the names of those who have done the work of deriving and establishing them, it would seem that the name of Zoghman Mebkhout has been deemed unworthy of this fundamental theorem, the result of four years of obstinate and solitary work (1975-79), against the current fashion and the disdain of his elders. The latter, on the day when the significance of the theorem could no longer be ignored, took pleasure in calling it the 'Riemann-Hilbert theorem', and I trust them (although neither Riemann nor Hilbert would 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 relations between general discrete coefficients and continuous coefficients, appeared against the general indifference, that it was refined and clarified by delicate and patient work, that after successive stages the right statement has finally been found, that it is written down in black and white and proved, and when finally this theorem, the fruit of solitude, has 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 is really no reason to burden one's memory with the name of a vague unknown service!

Encouraged by this precedent, I propose to call henceforth "Adam and Eve's theorem" any theorem that is truly natural and fundamental to a theory, or even to go back even further and give honour where honour is due, by calling it simply "the theorem of the good God"<sup>(26)</sup>.

As far as I know, apart from myself, Deligne was the only one before Mebkhout to feel the interest in understanding the relations between discrete coefficients and continuous coefficients in a wider framework than that of stratified modules, so as to be able to interpret any "constructible" coefficients in "continuous" terms. The first attempt in this direction was the subject of a seminar (still unpublished) by Deligne at the IHES in 1968 or 69, where he introduced the point of view of "stratified promodules" and gave a comparison theorem (over the field of complex numbers) for transcendental discrete cohomology and the associated cohomology of the De Rham type, which still makes sense for schemes of finite type, over any base field of zero characteristic. (Apparently, he was not yet aware at that time of the remarkable result of his distant predecessors Riemann and Hilbert... ) Even more than Verdier<sup>(27)</sup> or Berthelot<sup>(28)</sup>, Deligne was therefore particularly well placed to appreciate the interest of the direction in which Mebkhout's research was heading in 1975, and subsequently the interest of Mebkhout's results and in particular the "theorem of the good God", which gives a more delicate and

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<sup>26</sup> I have not had in my life as a mathematician the pleasure of inspiring, or only being able to encourage, in a student a thesis containing a "theorem of the good God" - at least not of comparable depth and scope.

<sup>27</sup> 略

<sup>28</sup> 略

deeper understanding of discrete coefficients in terms of continuous coefficients, than the one he himself had worked out. 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 is still being withheld today, five years later<sup>(29)</sup>.

**(49) 3. Canned weight and twelve years of secrecy 46 略**

**(50) 4. You can't stop progress! (50) 略**

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<sup>29</sup> 略

## B) PIERRE AND THE MOTIVES

### IV. The motives (burial of a birth)

#### (51) 1. Memory of a dream - or the birth of motives... 46

(19 April) Since these lines (which end the note "My orphans", no. 46) were written, less than a month ago, I have noticed that they are a bit behind the times! I have 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, constitutes an interesting new development since 1970, by the mention of motives in the title and a presence of this notion in the text, however modest, especially via the notion of "motivic Galois group". Of course, we are still a long way from the overall picture of a theory of motives, which for fifteen or twenty years has been waiting for the bold mathematician who is willing to paint it, vast enough to serve as inspiration, as Ariadne's thread and as horizon line for one or more generations of arithmetical geometers, who will have the privilege of establishing its validity (or in any case of discovering the final word of the reality of motives...) (53).

It is also since 1982<sup>(30)</sup>, it seems, that the wind of fashion begins 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 fields of mathematics". If their usefulness, which simple mathematical instinct (for a well-informed person) made quite obvious from the beginning of the sixties, is just beginning to be admitted now, it is (it seems to me) mainly thanks to the solitary efforts of Mebkhout, who for seven years had the thankless task of wiping off the plaster, with the courage of one who trusts his instinct alone, against a tyrannical fashion...

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When, just three weeks ago, I wrote in a page or two about the yoga of motives, as 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 to grasp the common "motive", the common quintessence, of which the many cohomological theories known at the time (54) were so many different incarnations, each one speaking to us in its own language about the nature of the "motive" of which it was one of the directly tangible manifestations. No doubt I am still dreaming, remembering the strong impression made on me by such an intuition of Serre, who had been led to see a profinite Galois group, an object which seemed to be essentially discrete in nature (or, at least, tautologically reduced to simple systems of finite groups), as giving rise to a huge projective system of analytic  $l$ -adic groups, or even algebraic groups on  $\mathbb{Q}_l$  (by passing to suitable algebraic envelopes), which even had a tendency to be reductive - thus bringing in the whole arsenal of intuitions and methods (à la Lie) of analytic and algebraic groups. This construction made sense for any prime  $l$ , and I felt (or I dream I felt...) that there was a mystery to be probed, about the relation of these algebraic groups

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<sup>30</sup> 略

for different primes; that they must all come from the same projective system of algebraic groups over the unique subfield naturally common to all its base fields, namely the field  $\mathbb{Q}$ , the "absolute" field of characteristic zero. 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 was not "proving" anything; that I ended up understanding how the notion of motive provided the key to an understanding of this mystery - how, by the mere fact of the presence of a category (here that of "smooth" motives on a given base scheme, for example motives on a given base field) having internal structures similar to those found on the category of linear representations of an algebraic pro-group over a field  $k$  (the charm of the notion of algebraic pro-group having been revealed to me previously by Serre as well), one can indeed reconstruct such a pro-group (as soon as one has a suitable "fibre functor"), and interpret the "abstract" category as the category of its linear representations.

This approach towards a "motivic Galois theory" was inspired by the approach I had found, years before, to describe the fundamental group of a topological space or a scheme (or even of any topos - but here I feel I am going to hurt delicate ears for which "topos are not amusing" ... ), in terms of the category of étale coverings on the "space" under consideration, and the fibre functors on it. And the very language of "motivic Galois groups" (which I could have just as well called motivic "fundamental groups", the two kinds of intuitions being for me the same thing, since the end of the fifties... ), and that of the "fibre functors" (which correspond very much exactly to the "manifest incarnations" mentioned above, namely to the various "cohomological theories" which apply to a given category of motives) - this language was designed to express the profound nature of these groups, and to suggest obviously their immediate links with the Galois groups and with the ordinary fundamental groups.

I still remember the pleasure and the wonder, in playing with fibre functors, and with the torsors of Galois groups which make it possible to pass from one to the other by "twisting", to find in a particularly concrete and fascinating situation the whole arsenal of notions of non-commutative cohomology developed in Giraud's book, with the gerbe of fibre functors (here above the étale topos, or better, the fpqc topos of  $\mathbb{Q}$  - a non-trivial and interesting topos if ever there was one!), with the "link" (in algebraic groups or progroups) which binds this gerbe, and the avatars of this link, realised by various algebraic groups or progroups, corresponding to the various "sections" of the gerbe, in other words to the various cohomological functors. The different complex points (for example) of a scheme of zero characteristic gave rise (via the corresponding Hodge functors) to equally many sections of the gerbe, and to torsors for passing from one to the other, these torsors and the progroups acting on them being equipped with remarkable algebraic-geometric structures, expressing the specific structures of the Hodge cohomology - but here I am bring in another element from the motive dream... It was the time when those who make the fashion today had not yet declared that topos, gerbes and the like did not amuse them and that it was therefore bullshit to talk about them (I wouldn't mind acknowledging topos and gerbes where they stand...). And here we are, twelve years later, and the same people are pretending to discover and teach that gerbes (or even topos) do 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 motives, also born of a "strong impression" (certainly totally subjective!) that some comments of Serre had made on a certain "philosophy" behind Weil's conjectures. Their translation into cohomological terms, for  $l$ -adic coefficients with variable  $l$ , made one suspect remarkable structures on the corresponding cohomologies - the



structure of "filtration by weights"<sup>(31)</sup>. Surely the "motive" common to the different l-adic cohomologies had to be the ultimate support of this essential arithmetical structure, which then took on a geometrical aspect, that of a remarkable structure on the geometrical object "motive". I am surely being misleading again to speak of a "work" (when it was still of course a part of a guessing game, no more no less) when it was a question of "guessing[*deviner*]" (with as the only guide the inner coherence of a vision that was being formed, with the help of scattered elements known or conjectured here and there...), on the specific structure of the different cohomological "avatars" of a motive, how the filtration of weights was translated<sup>(32)</sup>, starting with the Hodge avatar (at a time when the Hodge-Deligne theory had not yet seen the light of day, for good reasons...(33)) This allowed me (in my dream) to see Tate's conjecture on algebraic cycles (here again is a third "strong impression" which inspired the Dreamer in his dream of motives!) and Hodge's conjecture (55), and to come up with two or three conjectures of the same kind, which I told some people about, who must have forgotten them because I never heard of them again, nor of the "standard conjectures". In any case, they were only conjectures (and moreover, not published...). One of them 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 field, in terms of the geometric filtration of this variety itself by closed subsets of given codimension (the codimension playing the role of "weight")<sup>(34)</sup>.

And there was also the work (I should put quotation marks on 'work', but I can't bring myself to do so!) of 'guessing' the behaviour of the weights by the six operations (which have since been lost...). Here again, I never had the impression of inventing, but always discovered - or rather listened to - what things were saying to me, when I took the trouble to listen to them with pen in hand. What they said was peremptorily precise, and could not be mistaken.

Then there was a third "motive-dream", which was like the marriage of the two previous dreams - it was to interpret, in terms of structures on motivic Galois groups and torsors of these groups which serve to "twist" a fibre functor to obtain (canonically) all other fibre functors<sup>(35)</sup>, the various additional structures that the category of motives has, of which one of the very first is that of filtration by weights. I think I remember that here, less than ever, it was not a question of guessing, but of mathematical translations in good and due form. These were all new "exercises" on the linear representations of algebraic groups, which I did with great pleasure for days and weeks, feeling that I was getting closer and closer to a mystery that had fascinated me for years! Perhaps the most subtle notion that had to be grasped and formulated in terms of representations was that of 'polarisation' of a motive, drawing on Hodge's theory and trying to decant what made sense in the motivic

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<sup>31</sup> (24 January 1985) For a rectification of this distorted recollection, see note no. 164 (I 4), and sub-note no. 164.1, giving details of the filiation of the "yoga of weights".

<sup>32</sup> (28 February 1985) There is a slight confusion in my mind here. It is, in fact, the filtration closely linked by the "levels".

<sup>33</sup> This was at a time when the young Deligne had probably not yet heard the word "scheme" in a mathematical context, nor the word 'cohomology'. (He became acquainted with these notions through contact with me, from 1965).

<sup>34</sup> (28 February 1985) It is in fact the filtration by "levels" (cf. previous footnote).

<sup>35</sup> Just as the fundamental groups  $\pi_1(x)$ ,  $\pi_1(y)$  of some "space"  $X$  at two "points"  $x$  and  $y$  reduce from one to the other by "twisting" by the torsor  $\pi_1(x,y)$  of the classes of paths from  $x$  to  $y$ ...

context. This was a reflection that must have taken place around the time of my reflection on a formulation of the "standard conjectures", both inspired by Serre's idea (always him!) of a "Kählerian" analogue of Weil's conjectures.

In such a situation, when things themselves tell us what their hidden nature is and by what means we can most delicately and faithfully express it, while yet many essential facts seem beyond the immediate reach of proof, the simple instinct tells us to simply write down in black and white what things insistently tell us, and they become all the more clear by our taking the trouble to write under their dictation! There is no need to worry about proofs or complete constructions - to burden ourselves with such requirements at this stage of the work would be to deny ourselves access to the most delicate, essential stage of a vast-scale 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, and describing - even if only to describe elusive intuitions or mere "suspicions" that are 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 of all, which always precedes the proofs and gives us the means to prove - or to put it better, without which the question of 'proving' something does not even arise, before nothing that touches the essential has been formulated and seen. By the mere virtue of an effort to formulate, what was shapeless takes shape, lends itself to examination, making the visibly false decant from what is possible, and, especially, from that which fits so perfectly with the whole of what is known, or guessed, that it becomes in turn a tangible and reliable element of the vision which is in the process of being born. This vision becomes richer and more precise in the course of the work of formulation. Ten things are suspected only, none of which (Hodge's conjecture, let us say) leads to convictions, but which mutually enlighten 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 to be false, the work that has led to this provisional vision has not been done in vain, and the harmony that it has given us a glimpse of and allowed us to penetrate to some extent is not an illusion, but a reality, calling us to know it. Only through this work have we been able to come into intimate contact with this reality, this hidden and perfect harmony. When we know that things as they are have their *raison d'être*, that our vocation is to know them, not to dominate them, then the day when an error breaks through is a day of exultation (56) - just as much as the day when a proof teaches us beyond any doubt that such and such a thing as we imagined was indeed the faithful and true expression of reality itself.

Every time, such a discovery comes as a reward for the 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 learn the final word, which is reserved for the others after us, the work itself is its own reward, rich at every moment of what this very moment reveals to us.

(51.1) 略

## **(52) 2. The Burial - or the New Father 略**

(53) 略

(54) Since then, two new cohomological theories for algebraic varieties have appeared (apart from the Hodge-Deligne theory, which is a natural extension, in the "motivic" spirit, of Hodge's cohomology), namely Deligne's

theory of "stratified promodules", and above all the theory of crystals, a "D-modules" version à la Sato-Mebkhout, with the new insight provided by the theorem of the good God (alias Mebkhout) which has been mentioned previously. 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 cohomology. These new theories do not provide new functors on the category of smooth motives of a given scheme, but rather (modulus of a more thorough foundational work than the one done so far) a way to apprehend in a precise way the "Hodge" embodiment of a motive (not necessarily smooth) over a scheme of finite type over the field of complex numbers, or the "De Rham" embodiment on a scheme of finite type over a field of zero characteristic. It is likely that the (apparently still unwritten) theory of Hodge-Deligne coefficients on a finite type scheme over  $\mathbb{C}$ , will eventually appear as contained in the (equally unwritten) theory of crystalline coefficients à la Sato-Mebkhout (with an additional filtration datum), or more precisely as a kind of intersection of the latter with the theory of discrete coefficient constructible  $\mathbb{Q}$ -vector spaces... As for the elucidation of the relations between the crystalline theory à la Mebkhout and the theory developed in positive characteristic by Berthelot and others, this is a task sensed by Mebkhout as early as before 1978, in a climate of general indifference, and which seems to me one of the most fascinating that immediately arises for our understanding of "the" cohomology (unique and indivisible, motivic knowledge!) of algebraic varieties.

(55) I may have been dreaming, but my dream about the relation between motives and Hodge structures made me put my finger, without even doing so on purpose, on an inconsistency in the "generalised" Hodge conjecture as it had been initially formulated by Hodge, and to replace it by a rectified version which for the time being (I would bet) must be neither more nor less false than the "usual" Hodge conjecture on algebraic cycles.

### **(56) 3. Prelude to a Massacre 51**

I am thinking in particular, in the context of the cohomology of algebraic varieties, of Griffiths' discovery of the falsity of a seductive idea that we had long had about algebraic cycles, namely that a cycle homologically equivalent to zero has a multiple which is algebraically equivalent to zero. This discovery of a brand new phenomenon struck me enough that I spent a week working to try to grasp Griffiths' example, by transposing his construction (which was transcendental, on the body  $\mathbb{C}$ ) into a construction "as general as possible", and valid in particular over fields of any characteristic. The extension was not quite obvious, using (if I remember correctly) Leray's spectral sequences and Lefschetz's theorem.

(16 June) This reflection gave me the opportunity to develop, in the étale context, the cohomological theory of "Lefschetz pencils". 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 has followed closely). In the introduction to the volume by P. Deligne, on the other hand, where it is stated that the key results of the volume are talks XV (Picard-Lefschetz formulas in étale cohomology) and XVIII (theory of Lefschetz pencils), the author is careful not to point out that I have something to do with this "key theory" of Lefschetz pencils. The reading of the introduction gives the impression that I have nothing to do with the themes developed in the volume.

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(57) 略

(58) 略

**(59) 4. The New Ethic (2) - or the rat race 47 略**

**(!59\*) 5. Appropriation and contempt 略**

## V. My friend Pierre

### (60) 1. The child

(April 21) To take up this dream of a memory, which is not only the memory of the birth of a vision... I remember well (although I have forgotten so many things!) the renewed pleasure I had each time in talking with the one who had quickly become much more the confidant of all things that intrigued me, or of what was illuminating and enchanting me from day to day in my love affair with mathematics, he had ever been a 'student'. His always awakened interest, the ease with which he took note of everything ("as if he had always known it...") were for me a constant source of enchantment. His listening was perfect, driven by the same thirst for understanding that animated him as me - a highly alert listening, a sign of communion. His comments always met my own intuitions or reservations, when they did not throw some unexpected light on the reality that I was trying to define through the mists that still surrounded it. As I have said elsewhere, he often had answers to the questions I raised, often on the spot, or he developed them in the days or weeks that followed. This means that listening was shared, when he explained to me in turn the answers he had found, that is to say quite simply the reasons of things, which always appeared with the same perfect naturalness, with the same ease that had often enchanted me in the case of some of my elders such as Schwartz and Serre (and also Cartier). It was this same simplicity, this same "obviousness" that I had always pursued in understanding mathematics. Without having to say it, it was clear that by this approach and by this demand, he and I were "of the same family".

I felt from the moment we met that his "means/[*moyens*]", as they say, were of a very rare quality, far beyond the modest means I had at my disposal, whereas by the passion to understand and by the demand towards the comprehension of mathematical things, we were at the same diapason. I also felt, confusedly, without being able to formulate it, that this "strength" that I noticed in him (and that I also felt in myself, but to a lesser degree), that of "seeing" obvious things that no one else saw, was the strength of the children, the innocence of a child's eyes. There was something of the child in him, much more apparent than in other mathematicians I have known, and this is surely no accident. He told me that one day, when he was still in high school I think, he had fun checking the multiplication table (and along the way, by necessity, the addition table as well), for numbers from 1 to 9, in terms of the definitions. He was not expecting any surprises, of course - if there was any surprise (which brings pleasure, as always...), it was that the demonstration could be done nicely and completely in just a few pages, perhaps in half an hour. I felt, when he told me the story with a laugh, that it had been half an hour well spent - and this is something I understand even better today than I did then. This little story had struck me, impressed me even (without my letting it shown, I think) - I sensed in it the sign of an inner autonomy, of a freedom with regard to the knowledge received, which had also been present in my relationship to mathematics in my childhood, from the very first contacts (69)<sup>(36)</sup>.

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<sup>36</sup> It seems to me that this freedom has never entirely disappeared during my life as a mathematician, and that it is present afresh as it was in my childhood.

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## (61) 2. The burial 60

I have had the privilege of seeing a first flowering of a child's impulse, carrying the promise of a vast-scale unfolding. Over the next fifteen years, I came to realise that this promise was constantly being deferred. There was this delicate thing in him that I had been able to feel and recognise (at a time when I was insensitive to so many things!), a thing that is of a different nature from brain power (which crushes as well as penetrates...) - something that is essential above all for any truly creative work. This thing, I had felt in others at times, but in no mathematician I had ever known had it manifested itself with a comparable force. And I expected (as a matter of course) that this thing would continue to blossom in him and to transform itself, and to express itself effortlessly in a unique work, of which I would have been a modest precursor. But then strangely again (and surely there is a deep and simple connection between so many 'strange things') - I have seen this 'delicate thing', this 'strength' that is neither muscle nor brain, gradually fade away over the years, as if buried under successive, and increasingly thick layers - layers of something else which I know only too well - the most common thing in the world! This is not necessarily a bad match for brain power, nor for consummate experience or a practised 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 this is not that which I had in mind when I spoke of "unfolding" or "blossoming". The unfolding 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 this part of the world (such as the vast world of mathematical things... ). It is this alone that has the power of profound renewal, whether it be the renewal of self, or that of the knowledge of the things of this world. It is this which has been fully realised, it seems to me, in the modest person of Riemann<sup>(37)</sup>. This true fulfilment is alien to contempt: contempt for others (for those whom we feel are far below us...), or contempt for things that are too "small" or too obvious for us to deign to take an interest in them, or for those that we consider to be below our legitimate expectations; or contempt for such and such a dream, perhaps, that speaks to us insistently about the things we profess to love... . It is alien to contempt, as it is alien to the fatuity that feeds it.

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But the choices we make, and the actions that express them (even though our words often deny them), we make at our own risk and 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 the setbacks are not an outrage, we often consider them as a price to pay, which we pay with reluctance. But it also happens that one understands that such setbacks are something other than ruthless cashiers, to whom one must pay for the good time one has had, willy-nilly. They are patient and obstinate messengers, who tirelessly return to bring us the same message over and over again; an unwelcome and constantly rejected message - because even more than the setback itself, it is its humble message, which is 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, of which we no longer care...

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<sup>37</sup> The work of Riemann (1826-1866) is a modest volume of about ten works (it is true that he died in his forties), most of which contain simple and essential ideas that have profoundly renewed the mathematics of his time.

On the day when we finally welcome the message, our eyes are suddenly opened and we see: what was feared as "the worst" is a liberation, an immense relief - and this crushing weight from which we are suddenly relieved is the very thing which, till yesterday, we were still clinging to as "the best".

### **(62) 3. The event 61 略**

### **(63) 4. The eviction 60**

(22 April) This article<sup>(38)</sup> appeared in Mathematical Publications in 1968, two years before I left the world of mathematicians. Its starting point had been a conjecture I had told Deligne, about a property of degeneracy of spectral sequences which at that time might have seemed quite incredible, which nevertheless became plausible by "arithmetic" ways, as a consequence of the conjectures of Weil. This motivation was in itself of great interest, for it showed all the advantage that could be gained from a "yoga of weights" implicitly contained in Weil's conjectures (a yoga first glimpsed by Serre, in certain important aspects). From that time on I 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 nevertheless had a great probative power, and represented a means of discovery of the highest order. Deligne's 'geometrical' proof of the particular conjecture in question, using Lefschetz's theorem (established then in zero car. only), had an interest in a quite different direction, in addition to the first merit of not depending on any conjecture. The link that the two approaches indicated between two seemingly unrelated things, namely Weil's conjectures (and the yoga of weights which was then for me the most fascinating aspect of them) on the one hand, and Lefschetz's theorem on the other - this link was in itself very instructive.

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(63.1) 略

### **(63') 5. The ascent 略**

### **(!63'') 6. The ambiguity 略**

### **(63''') 7. The compère 48 略**

### **(64) 8. The investiture 60 略**

### **(65) 9. The knot 63 略**

### **(66) 10. Two turns 61 略**

### **(67) 11. The clean slate 略**

(67.1) 略

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<sup>38</sup> This is Deligne's article on the degeneracy of spectral sequences and Lefschetz's theorem (Publications Mathématiques 35, 1968) cited in the note "Poids en conserve et douze ans de secret", no. 49).

(67') 12. Being apart 略

(68) 13. The green light 略

(68') 14. The reversal 略

(69) 15. Squaring the circle 60

(27 April) When I was about eleven or twelve years old, while I was interned in the concentration camp of Rieucros (near Mende), I discovered the games of tracing with the compass, enchanted in particular by the six-branched rosettes obtained by dividing the circumference into six equal parts with the help of the opening of the compass applied to the circumference six times, which makes it fall right back onto the starting point. This experimental finding convinced me that the length of the circumference was exactly six times the radius. When later (at lycée in Mende I think, where I eventually went) I saw in a textbook that the relationship was supposed to be much more complicated, that we had  $l = 2\pi R$  with  $\pi = 3.14\dots$ , I was convinced that the book was wrong, that the authors of the book (and presumably those who had preceded them since antiquity!) must never have done this very simple tracing, which clearly showed that we simply had  $\pi = 3$ . As was typical, I realised my mistake (which consisted in confusing the length of an arc with the length of the string that joins the ends) when I expressed my astonishment at the ignorance of my predecessors to someone else (a prisoner, Maria, who had given me some voluntary private lessons in maths and French), at the very moment when I was about to show her why we should have  $l = 6R$ .

This confidence that a child can have in his or her own enlightenments, relying on his or her own abilities rather than taking things learned at school or read in books at face value, is a precious thing. It is constantly discouraged, however, by those around. Many will see in the experience I relate here an example of a childish presumption, which had to bow to the knowledge received - the facts finally shatter a certain folly. As I experienced this episode, however, there was no feeling of disappointment or ridicule, but rather that of a new discovery (after the one I had hastily made with the false formula  $\pi = 3$ ): a discovery of an error, and at the same time that we should have  $\pi > 3$ , because obviously the length of an arc is greater than the length of the rope that joins the two ends. This inequality was in line with the rejected formula  $\pi = 3,14\dots$  which, as a result, looked reasonable, and at the same time I had to imagine that there were perhaps people who were not so stupid as those who had looked into the question. At that moment, my curiosity was satisfied, and I don't remember wanting to know more about the ins and outs of this number, so important, it seems, that it was destined for a letter of its own<sup>(39)</sup>.

This experience was probably one of the very first to teach me a certain prudence, when my own enlightenment seems to contradict generally accepted knowledge: that such a situation may be worthy of careful consideration.

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<sup>39</sup> (28 April) The above evocation brought up other memories, which show that this famous number  $\pi$  intrigued me more than I first thought I remembered. The approximate value 355/113, 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 no other numbers than fractional numbers at the time, I was intrigued by the appearance of the numerator and denominator of the irreducible fraction that expressed  $\pi$  - they must have been quite remarkable numbers! Needless to say, I didn't get very far with these childish thoughts about squaring the circle.



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 that comes from the original awareness of this power in us.

**(70) 16. The funeral 略**

**(71) 17. The tomb 略**

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

### (72) 1. One foot in the merry-go-round 略

### (73) 2. The return of things (or one foot in the dish)

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The main force, the "drive" behind my investment in my students in general, in the first period of the sixties, was the desire to find "arms" to carry out "tasks" which my instinct indicated to me as urgent and important (at least from my own perspective of mathematics). This "importance" was certainly not purely subjective, it was not a simple question of "taste and colour", and often (I believe) the student who took on such a task that I proposed to him felt that it "matched him [*faisait le poids*]", and also, perhaps, what its place could be within the larger plans.

Yet, in terms of this 'drive', this motivating force within me that pushed me towards the tasks, it was not a certain 'objective' importance that was at stake - whereas the 'importance' of Fermat's conjecture, Riemann's hypothesis or Poincaré's hypothesis left me perfectly cold, I didn't really 'feel' them. What distinguished these tasks from all others, in my relation to them, was that they were my tasks; those I had felt, and made mine own. I knew that having felt them was the result of a delicate and deep work, a creative work, which had allowed me to identify the crucial notions and problems which were the object of this or that task. They were, and no doubt still are (to a large extent) a part of me. The link that bound me (or still binds me today) to them was by no means severed when I entrusted this task to a student - on the contrary, this link acquired a new life, a new vigour! This link did not have to be said (and I am 'saying' it here, if only to myself, for the first time). This link was obvious both to the student who had chosen to work with me, and on a task of his choice, and to me, and also (I am convinced) to all the others. It is the deep connection between the one 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.

This is a connection I have never examined carefully. It seems to me to be deeply rooted in the nature of the "I", and is universal in nature. It is a bond that one sometimes affects to ignore, as if one were above such pettiness - it is possible that I have even entered into such an affectation<sup>(40)</sup>. But on the few occasions in recent years (or in recent days and weeks) when I have been confronted with an attitude in others that affects to ignore that link (of which they are aware) that connects me to some task that has been accomplished (by another, or by myself) or only designated, I am touched at a sensitive place. One may call this place "vanity" or "fatuity" and label it with other words - and I do not claim that these terms are out of place here, but whatever one calls it, I am not ashamed to speak of it or to be as I am, and I know that the thing I speak of is the most universal thing in the world! No doubt this attachment of a person to "his works" does not have the same strength from one person to another. In my life, where "the Doing" has been the constant focus of my great investments of energy since childhood, this link has been strong and remains so today.

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<sup>40</sup> What is certain is that I was following the "right tone" of ignoring such things, contrary to the images de rigueur!

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**(74) 3. The Unanimous Agreement** 略

## C) THE BEAUTIFUL WORLD

### VII. The Colloquium - or sheaves of Mebkhout and Perversity

(75) 1. The iniquity - or the meaning of a return 略

(!75') 2. The Colloquium 略

(!75'') 3. The prestidigitator 略

(76) 4. The Perversity 75 略

(77) 5. Thumbs up!

(5 May) 略 The last chapter, under the suggestive name "From F to C", describes at length a remarkable principle that I had introduced in algebraic geometry twenty years ago - it must have been before the birth of the notion of motive (which gives the most profound illustrations of it, via the ex-conjectures of Weil). This principle ensures that for certain types of statements concerning schemes of finite type over a field, it suffices to prove them over a finite base field (thus in a situation "of arithmetic nature") to deduce their validity over any field, and in particular over the field of complexes - in which case sometimes the algebraic-geometric result envisaged can be reformulated in a transcendental way (e.g. in terms of integer or rational cohomology, or in terms of Hodge structures etc.)(<sup>41</sup>) 略<sup>42</sup> 略

(77') 6. The robe of the emperor of China 略

(78) 7. Encounters from beyond the grave 略

(78.1) There are, however, a number of "fine" results of coherent duality, notably on the structure of "modules of dualising differentials", their relation to modules of "naive" differentials, and trace and residue applications in the flat non-smooth case, which I had developed in the late fifties and which have never been published to my

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<sup>41</sup> (6 May) 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 nilpotency of algebraic group laws on the affine space  $E$  (over any field). I was very impressed by his proof, and I drew inspiration from it in making many other statements and a "philosophy" that dominated my thinking on the theory of motives.

<sup>42</sup> (5 June) It is perhaps abusive for me to claim to be the "father" of a principle whose first known application known to me is due to Lazard (see previous note). My role, as on other occasions, has been to sense the generality of another's idea, and to systematise it to the point of making it a 'reflex' or 'second nature'. In the context of the yoga of weights and motives, it is likely that the first person to use this principle was Serre (not me), with his idea of virtual Betti numbers, which set me on the path precisely to a general yoga of weights and motives. (See note 46.9 for Serre's idea in question.) It is also true that it is common practice to attribute the paternity of a "principle" of reasoning that has become commonplace, not to the author where one finds the first trace of it, but to the one who for the first time perceived its general scope, who systematised and popularised it. In this sense, it can be said that N. Katz's correction (referred to in the following sentence [*in the main paragraph, omitted -Trans.*]), attributing the authorship of this principle to me, is justified.

knowledge. This does not prevent the essentials: the theory of coherent duality (in the schematic framework at least), as well as that of étale duality (and its variant for the discrete cohomology of locally compact spaces, developed by Verdier on the étale model), or linear algebra or general topology, appear as essentially completed theories<sup>(43)</sup>, in the form of perfectly developed and ready-to-use tools, and not of a somewhat unknown substance that one would have to penetrate and assimilate.

**(78') 8. The Victim - or the two silences 略**

(78'.1) 略

(78'.2) 略

**(!78'') 9. The Boss 略**

**(79) 10. My friends 78' 略**

**(80) 11. The pavement and the beautiful world (or: bladders and lanterns...) 略**

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<sup>43</sup> (12 June) This is not quite true for étale duality, until the purity conjectures and the "biduality theorem" are proven in full generality.

## VIII. The Student - alias the Boss

### (81) 1. Thesis on credit and all risks insurance 63''' 略

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

The contributions 2) and 3) are an "unexpectedness" compared to what was known. The most important contribution seems to me to be 3). Its proof is easily derived from the duality formalism (for both "discrete" and "continuous" coefficients), which does not prevent it from being an important ingredient in the arsenal of "catch-all" formulas that we have in cohomology. The existence of this formula was discovered by Verdier, and was a (pleasant!) surprise to me.

The duality formalism in the context of locally compact spaces is essentially the "necessary [*qui s'imposait*]" adaptation of what I had done in the context of étale cohomology of schemes (and without the difficulties inherent in the scheme situation where everything was yet waiting to be done). However, he brings an interesting new idea, that of the direct construction of the functor  $f_!^*$  (without prior smoothing of  $f$ ) as a right-hand adjoint of  $Rf_!^*$ , with an existence theorem in hand. This procedure was taken up by Deligne in étale cohomology, allowing him to define  $f_!^*$  in this framework, without any smoothness 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.

(81.2) As another example, I point out the detailed development of the duality formalism in the context of locally compact spaces, in the spirit of the "catch-all" formalism of the six operations and the derived categories, of which Verdier's talk at the Bourbaki Seminar would constitute an embryo. Even in the context of topological varieties only, there is still no satisfactory reference text for the formalism of Poincaré duality, as far as I know.

(5 June) There are two other directions in which I note with regret which Verdier did not consider useful to pursue to the end the work he had started in a way sufficiently strong as to take credit for it (I mean, by starting 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 does not care (any more than for the derived categories) to make himself the servant of a task and to 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 programme I was foreseeing and suggested him to develop was in the framework of general (not necessarily locally compact) topological spaces and maps between them that are "separated" and locally "smooth" (i.e. locally the source immerse [*plonge*] into a  $Y \times \mathbb{R}^n$ , where  $Y$  is the target space). This was obviously suggested by the analogy with the framework of étale cohomology of any schemes. Verdier was able to see, in the framework of locally compact spaces, that the assumption of local smoothness of the maps was

unnecessary (which came as a surprise). This does not prevent the context of locally compact spaces (thus excluding "parameter spaces" which would not be locally compact) from being obviously too restricted. A more satisfactory context would be one that would cover both the one chosen by Verdier, and the one I was foreseeing, namely the one where the topological spaces (or even topos?) are (more or less?) arbitrary, and where the maps  $f : X \rightarrow Y$  are subject to the restriction of being 1) separate and 2) "locally compactifiable", i.e.  $X$  is locally immersed in a  $Y \times K$ ,  $K$  compact.

In this context, the fibres of an "allowed" map would be any locally compact spaces. Another step would be to allow that  $X$  and  $Y$ , instead of being topological spaces, are "topological multiplicities" (i. e. topos which are "locally like a topological space"), or even any topos, by restricting the maps in a suitable way (to be made explicit), so as to find fibres which are locally compact multiplicities, subject if necessary to additional conditions (perhaps close to the point of view of Satake's  $G$ -varieties), for example (and at the very least!) to be locally of the form  $(X, G)$ , where  $X$  is a compact space with action of the finite group  $G$ . To my knowledge, even the "ordinary" Poincaré duality has not been developed in the case of smooth compact topological multiplicities (smooth: being locally a topological variety). The case of a classifying space of a finite group seems to show that one can hardly hope to have a (global absolute) duality theorem except modulo torsion, or more precisely, by working with a ring of coefficients which is a  $\mathbb{Q}$ -algebra. With this restriction, I would not be surprised if Poincaré's duality ("six operations" style) works as it is in this context. It is not surprising that no one has ever looked at it (except unrepentant differential geometers, pretending to look at the cohomology of the "leaf space" of a foliation), given the general boycott of the very notion of multiplicity, instituted by my cohomology students, Deligne and Verdier in the lead.

In fact, what is missing is a reflection on the foundations of the following type: to describe (if possible) in the context of any topos and sheaves of "discrete" coefficients on them, the notions of "cleanliness [*propreté*]", "smoothness", "local cleanliness", "separation" for a morphism of topos, enabling us to derive a notion of "admissible morphism" of topos  $f : X \rightarrow Y$ , for which the two operations  $Rf_!$  and  $Lf_!$  make sense (one adjoint of the other), in order to obtain the usual properties of the six operations formalism. Here the topos are considered as non-ringed, or perhaps as carrying Rings (which are assumed to be constant or locally constant if necessary), assuming (at least at the beginning) that the ringed topos morphisms  $f : (X, A) \rightarrow (Y, B)$  are such that  $f^{\{-1\}}(B) \rightarrow A$  is an isomorphism (81.3). The above reflections suggest that when one restricts oneself to Rings of coefficients of zero characteristic (i.e. which are  $\mathbb{Q}$ -Algebras), one can be much broader in the notion of "admissible morphism", so as to encompass "fibres" which are e.g. (topological or schematic) multiplicities, rather than ordinary (topological or schematic) "spaces".

A first start 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 beginning had encouraged me to pursue a reflection in this direction last year, in the context of small categories (generalising discrete groups) serving as homotopic models. Without going very far, this reflection was nevertheless sufficient to convince me that there must exist a complete formalism of the six operations in the context of **Cat** of the category of small categories. (See on this subject the "Pursuing Stacks", Chap. VII, par. 136, 137.) The development of such a theory in **Cat**, or even in **ProCat**, as well as a theory of this type in the context of topological or schematic spaces and multiplicities, would have for me as a 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 perplexities of duality in the case of semisimplicial spaces (or schemes). It seemed to me that it was always the same tobacco - to manage to detect the existence of a six-operation formalism in a given case, and to understand it. But it would seem that the mere prospect of thinking about the foundations has the gift of chilling each and every one of my former students - at least among my cohomology students. If I took the trouble with them, it was with the conviction that they were not going to stop (in terms of conceptual work) right where they had gone with me, and remain wringing their hands every time a new situation showed that the work they and their friends had done with me was insufficient. The conceptual work we do is always insufficient in the long run, and it is by taking it up and going beyond it, not otherwise, that mathematics progresses. Between 1955 and 1970, each year again I found that what I had done in the previous years was not sufficient for the needs, and I went back to work right away, at least when someone else (e.g. Mike Artin, with the "algebraic varieties" point of view in his sense) had not already started. But it seems that my students have also buried the example I set for them, along with myself and my work.

(81.3) I seem to recall that in the formalism of the six variances in (say) étale cohomology, the assumption that the sheaves of rings serving as coefficients being locally constant is unnecessary - the essential assumption is that they are torsion sheaves prime to residual characteristics, and that  $f^*_{-1}(B) \rightarrow A$  is an isomorphism. When one abandons this last hypothesis, one must enter a theory (still never elucidated, to my knowledge) which "mixes" the "discrete space" duality, and the "coherent" duality (relative to the Rings of coefficients and their homomorphisms). As a result, we envisage replacing, on the diagrams (or more general topos)  $X, Y$  and the Rings of coefficients  $A, B$  by relative (not necessarily affine) diagrams  $X', Y'$  over  $X, Y$ , and the morphisms of ringed topos  $(X, A) \rightarrow (Y, B)$  by commutative diagrams of the type

$$\begin{array}{ccc} X' & \longrightarrow & X \\ \downarrow & & \downarrow \\ Y' & \longrightarrow & Y \end{array}$$

with a "six operations" formalism in such a context. When  $X, Y$ , etc... are point topos, we should recover the usual coherent duality.

## (82) 2. The good references 78' 略

To come back to the homology (not to be confused!) class associated with a cycle (which, according to the title, is the object of Verdier's article), I had developed this formalism with a luxury of details, in several exposés, in the course of the oral seminar, in front of an audience that was begging for mercy (always only except Deligne, who was always spirited and fresh [*fringant et frais*]...). It was one of the innumerable "long exercises" that I developed that year on the formalism of duality in the étale framework, feeling the need to arrive at a complete mastery of all the points that I felt needed to be understood thoroughly. The interest here was to have a valid formalism on a not necessarily regular ambient scheme - the transition to the cohomology class in the regular case, and the link with my old construction using cohomology with supports and giving immediate



compatibility with cup-products, being immediate. I also noticed that this part of the seminar is part of the batch of what was not taken up in the published version - no doubt Illusie (on whom all the work of preparing a releasable edition (hmm) ended up falling) must have been quite happy that Verdier took charge of it, mutatis mutandis (that is to say: without changing anything!)

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**(83) 3. The joke - or the "weight complexes" 略**

## IX. My students

### (84) 1. The silence 略

(84.1) 略

### (85) 2. The solidarity 略

(85.1) 略

(85.2) 略

(30 May) It seems to me that the first time I used Hironaka's resolution of singularities, and understood the extraordinary power of the resolution as a tool for doing proofs, was for a "three shakes of a lamb's tail" proof 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 over  $\mathbb{C}$ . (It is not impossible that the principle was breathed [*soufflé*] to me, on this very occasion, by Serre.) This last result is the main ingredient of the proof of the comparison theorem of étale cohomology and ordinary cohomology (the rest being reduced to dévissages, thanks to the  $Rf_*$  formalism, plus a bit of the resolution for passing from  $Rf_*$  to  $Rf_*$ ...).

### (85') 3. The mystification 略

### (86) 4. The deceased 略

### (87) 5. The massacre 85 略

Two of my oral exposés have never been made available to the public in any form. One is the closing exposé on open problems and conjectures, which "was unfortunately not written up", given the little - and the author of the introduction to the edition - massacre deemed it in effect unnecessary to even mention what open problems and conjectures were involved. And why should he have bothered, when they were only problems (which everyone is free to pose as he pleases!) and conjectures (not even proved!) (87.1). The other is the talk that opened the seminar, and placed it from the outset in a wider context (topological, complex analytic, algebraic) and reviewed formulas of Euler-Poincaré, Lefschetz, Nielsen-Wecken types, some of which constituted one of the main applications of the seminar. 略

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There was the exposé developing a "generic Künneth formula", which had been written by Illusie. No one before had thought of developing this kind of statement, inspired by the intuition that "generically" i.e. in the neighbourhood of the generic point of the base, a relative scheme behaves like a "locally trivial fibre" in the topological context. By an elegant proof close to his proof mentioned above, Deligne manages to eliminate the hypothesis of resolution of singularities that I had made. It is awarded - the exposé was deleted and "replaced" by a reference to an exposé by the same Illusie in the so-called "earlier" seminar SGA 4 1/2.

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I feel this breath, and yet it remains for me something foreign, incomprehensible. To comprehend", it would probably be necessary that this breath lives in me, or has lived in me. But four years ago I felt for the first time and measured the significance of something in my life that I had never thought about, a thing that had always seemed to me to be self-evident: that my identification with my father, in my childhood, was not marked by conflict - that in my childhood at no time had I feared or envied my father, whilst devoting myself to him with unconditional love. This relationship, perhaps the most profound that has marked my life (without my even realising it until this meditation four years ago), which in my childhood was like the relationship to another self that was both strong and benevolent - this relationship was not marked by division and conflict. If, throughout my much torn life, the awareness of the strength that lies within me has remained living; and if, in my by no means fearless life, I have not known fear either of a person or of an event - it is to this humble circumstance that I am indebted, yet unaware until well into my fiftieth year. This circumstance has been a priceless privilege, for it is the intimate awareness of the creative force in one's own person that is also that force, which allows 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 this moment also like a hindrance, like a "void/vide]" in my experience of life. A void that is difficult to fill, where many others have a rich web of emotions, images, associations, offering them the path (if they are curious to take it) to a deep understanding of others as well as of themselves, in situations that I manage (by dint of repetition and cross-checking) to apprehend as well as I can, but in front of which I nevertheless remain like a stranger - with a desire in me for awareness remaining hungry.

(87.1) (31 May) This closing exposé, surely one of the most interesting and substantial together with the opening one, was obviously not lost for everyone, as I see from MacPherson's paper "Chern classes for singular algebraic varieties" (*Chern classes for singular algebraic varieties*, Annals of Math. (2) 100, 1974, pp. 423-432) (received in April 1973). I find there, under the name of "Deligne-Grothendieck conjecture", one of the main conjectures I had introduced in this exposé in the schematic framework. It is taken up again by MacPherson in the transcendental framework of algebraic varieties over the field of complex numbers, with the Chow ring replaced by the homology group. Deligne had learnt this conjecture(\*) in my exposé in 1966, therefore the same year he appeared in the seminar where he started to familiarise himself with the language of schemes and cohomological techniques (see the note "L'être à part" no. 67). It is still nice to have been given the honour of being included in the name of the conjecture - a few years later it would not have been appropriate...

(6 June) I take this opportunity to explain here what the conjecture was that I had stated in the seminar in the schematic framework, surely pointing out the obvious variant in the complex analytic (or even rigid-analytic) framework. I conceived it as a "Riemann-Roch" type theorem, but with discrete coefficients instead of coherent coefficients. (Zoghman Mebkhout told me, moreover, that his point of view of D-modules should make it possible to consider the two Riemann-Roch theorems as contained in the same crystalline Riemann-Roch theorem, which would thus represent in zero characteristic the natural synthesis of the two Riemann-Roch theorems that I introduced in mathematics, one in 1957, the other in 1966). We fix a ring of coefficients  $\Lambda$  (not necessarily commutative, but noetherian for simplification, and, moreover, of torsion prime to the

characteristics of the schemes considered, for the purposes of étale cohomology... ). For a scheme  $X$  we denote by

$$K.(X,\Lambda)$$

the Grothendieck group of constructible étale sheaves of  $\Lambda$ -modules. Using functors  $Rf_!$ , this group depends functorially on  $X$ , for  $X$  noetherian and morphisms of schemes which are separated and of finite type. For regular  $X$ , I postulated the existence of a canonical group homomorphism, playing the role of the "Chern character" in the coherent RR theorem,

$$ch\_X: K.(X,\Lambda) \rightarrow A(X) \otimes_{\mathbb{Z}} K.(\Lambda), (1.)$$

where  $A(X)$  is the Chow ring of  $X$  and  $K.(\Lambda)$  the Grothendieck group of  $\Lambda$ -modules of finite type. This homomorphism is to be uniquely determined by the validity of the "discrete Riemann-Roch formula", for a proper morphism  $f: X \rightarrow Y$  of regular schemes, whose formula is written just as the coherent Riemann-Roch formula, with the Todd "multiplier" replaced by the total relative Chern class:

$$ch\_Y((f_!(x))) = f_*(ch\_X(x)c(f)), (RR)$$

where  $c(f) \in A(X)$  is the total Chern class of  $f$ . It is not difficult to see that in a context where we have Hironaka's strong form of resolution of singularities, the RR formula does determine  $ch\_X$  in a unique way.

Of course, we assume that we are in a context where the Chow ring is defined (I am not aware that anyone has even tried to write a theory of Chow rings, for regular schemes which are not of finite type over a field). Alternatively, one can also work in the graded ring associated to the usual "Grothendieck" ring  $K.(X)$  in the coherent context, filtered in the usual way (see SGA 6). One can also replace  $A(X)$  by the even  $l$ -adic cohomology ring: direct sum of  $H^{2i}(X, \mathbb{Z}_l(i))$ . This has the disadvantage of introducing an artificial parameter  $l$ , and of giving less sharp "purely numerical" formulas, whereas the Chow ring has the charm of having a continuous structure, which is destroyed by passing 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 Artin-Serre-Swan type. This means that the general conjecture is a deep conjecture, the pursuit of which is linked to an understanding of the higher dimensional analogues of these invariants.

Remark. Denoting in the same way by  $K^.(X,\Lambda)$  "the Grothendieck ring" of the constructible complexes of  $\Lambda$ -étale sheaves of finite tor-dimension (a ring acting on  $K.(X,\Lambda)$  when  $\Lambda$  is commutative... ), we must likewise have a homomorphism

$$ch\_X: K^.(X,\Lambda) \rightarrow A(X) \otimes_{\mathbb{Z}} K^.(\Lambda), (1')$$

again giving rise (mutatis mutandis) to the same Riemann-Roch (RR) formula. Now let  $\text{Cons}(X)$  be the ring of constructible integer[*entières*] functions on  $X$ . We define in a more or less tautological way canonical homomorphisms

$$\begin{aligned} K(X, \Lambda) &\rightarrow \text{Cons}(X) \otimes_{\mathbb{Z}} K(\Lambda), (2.) \\ K^*(X, \Lambda) &\rightarrow \text{Cons}(X) \otimes_{\mathbb{Z}} K^*(\Lambda). (2') \end{aligned}$$

If we now restrict ourselves to schemes of zero characteristic, then (by using Euler-Poincaré characteristics with proper support) we see that the group  $\text{Cons}(X)$  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 previous tautological morphisms are functorial. (This corresponds to the "well-known" fact, but which I believe was not proved in the SGA 5 oral seminar, that in zero characteristic, for a locally constant sheaf of  $\Lambda$ -modules  $F$  on an algebraic scheme  $X$ , its image by

$$f_! : K^*(X, \Lambda) \rightarrow K^*(e, \Lambda) \otimes_{\mathbb{Z}} K^*(\Lambda)$$

is equal to  $d\chi(X)$ , where  $d$  is the rank of  $F$ ,  $e = \text{Spec}(k)$ ,  $k$  the base field which is assumed to be algebraically closed...). This immediately suggests that the Chern homomorphisms (1.) and (1') must be derivable from the tautological homomorphisms (2.), (2') by composing with a "universal" Chern homomorphism (independent of any coefficient ring  $\Lambda$ )

$$\text{ch}_X : \text{Cons}(X) \rightarrow A(X), (3)$$

so that the two "coefficient  $\Lambda$ " versions of the RR formula appear to be formally contained in a formula of RR at the level of constructible functions, and which is always written in the same form.

When working with schemes on a fixed base field (now again of any characteristic), or more generally on a fixed regular base scheme  $S$  (for example  $S = \text{Spec}(\mathbb{Z})$ ), the form of the Riemann-Roch formula most in line with the usual form (in the coherent framework familiar since 1957) is obtained by introducing the products

$$\text{ch}_X(x)c(X/S) = c_{\{X/S\}}(x) (4)$$

(where  $x$  is in either  $K(X, \Lambda)$  or  $K^*(X, \Lambda)$ ), which could be called the Chern class of  $x$  relative to the base  $S$ . When  $x$  is the unit element of  $K^*(X, \Lambda)$  i.e. the class of the constant sheaf of value  $\Lambda$  we find the image of the relative total Chern class of  $X$  with respect to  $S$ , by the canonical homomorphism of  $A(X)$  into  $A(X) \otimes_{\mathbb{Z}} K^*(\Lambda)$ . This being the case, the RR formula is equivalent to the fact that the formation of these relative Chern classes

$$c_{\{X/S\}} : K(X, \Lambda) \rightarrow A(X) \otimes_{\mathbb{Z}} K(\Lambda), (5.)$$

for a variable regular scheme  $X$  over  $S$  (of finite type over  $S$ ), with  $S$  fixed, is functorial with respect to proper morphisms, and similarly for the variant (5'). In zero characteristic, this reduces to the functoriality (for proper morphisms) of the corresponding map

$$c_{X/S}: \text{Cons}(X) \rightarrow A(X) . (6)$$

It is in this form of the existence and uniqueness of an absolute "Chern class" map (6), in the case where  $S = \text{Spec}(C)$ , that the conjecture in MacPherson'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) one has  $c_{X/S}(1) = c(X/S)$  (in this case, the total "absolute" Chern class). Compared to my initial conjecture, the form presented and proved by MacPherson differs in two ways. One is a "minus", because it is not in the Chow ring, but in the integral cohomology ring, or more exactly the integral homology group, defined in a transcendental way. The other is a "plus" - and it is perhaps here that Deligne has made a contribution to my initial conjecture (unless this contribution is due to MacPherson himself<sup>(44)</sup>). It is that for the existence and uniqueness of a map (6), one does not need to restrict oneself to regular schemes  $X$ , provided one replaces  $A(X)$  by the integral homology group. It is likely to be the same in the general case, denoting by  $A(X)$  (or better by  $A(X)$ ) the Chow group (which is no longer a ring in general) of the Noetherian scheme  $X$ . Or to put it differently: while the heuristic definition of the invariants  $ch_X(x)$  (for  $x$  in  $K_*(X, \Lambda)$  or  $K^*(X, \Lambda)$ ) makes essential use of the assumption that the ambient scheme is regular, as soon as we multiply it by the "multiplier"  $c(X/S)$  (when the scheme  $X$  is of finite type over a fixed regular scheme  $S$ ), the resulting product (4) seems to make sense without any assumption of regularity over  $X$ , as an element of a tensor product

$$A_*(X) \otimes K_*(\Lambda) \text{ or } A^*(X) \otimes K^*(\Lambda) ,$$

where  $A_*(X)$  denotes the Chow group of  $X$ . The spirit of MacPherson's proof (which does not use resolution of singularities) suggests the possibility of an explicit "computational" construction of the homomorphism (5.), by "dealing" with the singularities of  $X$  as they are, as well as with the singularities of the sheaf of coefficients  $\mathcal{F}$  (whose class is  $x$ ), in order to "gather[recueillir]" a cycle on  $X$  with coefficients in  $K_*(\Lambda)$ . This would also be in the spirit of the ideas I had introduced in 1957 with the coherent Riemann-Roch theorem, where I was doing self-intersection calculations in particular, being careful not to "budge" the cycle under consideration. A first obvious reduction (obtained by immersing  $X$  in a smooth  $S$ -scheme) would be to the case where  $X$  is a closed subscheme of the regular scheme  $S$ ...

The idea that it should be possible to develop a singular (coherent) Riemann-Roch theorem was in fact familiar to me, I can't say since when, but I never tried to test it seriously. It was 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  $K_*(X)$  and  $K^*(X)$  and  $A_*(X)$ ,  $A^*(X)$ , instead of just working with  $K_*(X)$ . I don't remember whether I also thought of something like this in the SGA 5 seminar in 1966, and whether I hinted at it in the oral exposé. As my handwritten notes have disappeared (in a move perhaps?) I will probably never know...

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(87.2) 略

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<sup>44</sup> (March 1985) This is indeed the case, see footnote no. 164 cited in the previous footnote.

(87.3) 略

(87.4) (6 June) This might be a good time to indicate what the main themes were that were developed in the oral seminar, and of which the published text only gives an idea by cross-reference.

I) Local aspects of duality theory, whose essential technical ingredient is (as in the coherent case) the biduality theorem (completed by a "cohomological purity" theorem). I have the impression that the geometrical meaning of this last theorem, as a local Poincaré duality theorem, which I had well explained in the oral seminar, has since been entirely forgotten by those who used to be my students<sup>(45)</sup>.

II) Trace formulas, including the "non-commutative" trace formulas which are more subtle than the usual trace formula (where the two members are integers, or more generally elements of the ring of coefficients, such as  $\mathbb{Z}/n\mathbb{Z}$  or an  $\ell$ -adic ring  $\mathbb{Z}_\ell$ , or even  $\mathbb{Q}_\ell$ ), set in the algebra of a finite group acting on the scheme considered, with coefficients in a suitable ring (such as those envisaged in the previous parenthesis). This generalisation came very naturally, because even in the case of Lefschetz formulae of the usual type, but for sheaves of "twisted" coefficients, one was led to replace the initial scheme by a Galois covering (ramified in general) serving to "untwist" the coefficients, with the Galois group acting on it. This is how formulas of the "Nielsen-Wecken" type are 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 (generalising the case of tamely ramified coefficients, naturally leading to the Ogg-Shafarevitch-Grothendieck formula). On the other hand, there were unpublished and profound conjectures of "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 MacPherson by a transcendental method (see note no. 87.1).

The comments I could not fail to make on the deep relations between these two themes (Lefschetz formulas, Euler-Poincaré formulas) have also been lost without trace. (As was my habit, I left all my handwritten notes to the volunteer-editors-sic, and no written trace remains of the oral seminar, of which I of course had a complete set of handwritten notes, even though some of them were brief.)

IV) Detailed formalism of the homology and cohomology classes associated to a cycle, following naturally from the general duality formalism and the key idea, consisting in working with the cohomology "with supports" in the considered cycle, using the cohomological purity theorems.

V) Finiteness theorems (including generic finiteness theorems) and generic Künneth theorems for cohomology with any support.

The seminar also developed a technique for passing from torsion coefficients to  $\ell$ -adic coefficients (exposés V and VI). This was the most technical part of the seminar, which generally worked with torsion coefficients, and then "passing to the limit" to derive the corresponding  $\ell$ -adic results. This point of view was a provisional

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<sup>45</sup> After verification, I found that this geometrical interpretation was at least preserved in Illusie's writing.

compromise, while waiting for Jouanolou's thesis (still unpublished at the moment) giving the formalism that works directly in the  $\ell$ -adic framework.

I do not count 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 programme was full, I had not thought it necessary in the oral seminar to dwell on these calculations and on this construction, since it was sufficient to repeat, practically verbatim, the reasonings I had given ten years before in the context of Chow rings, on the occasion of the Riemann-Roch theorem. It was also 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 (exposé VIII), which he had to regard not as a service to the mathematical community in which to learn basic techniques essential for his own use, but as a chore, since its writing dragged on for years<sup>(46)</sup>. It was no different, one would think, for his thesis, which still remains a ghostly reference just like Verdier's... The "passage to the limit" part should not be counted as one of the "main themes" of the seminar either, in the sense that it is not associated with a particular geometrical idea. Rather, it reflects a technical complication particular to the context of étale cohomology (distinguishing it from transcendental contexts), namely that the main theorems on étale cohomology primarily concern torsion coefficients (prime to residual characteristics), and that in order to have a theory corresponding to rings of coefficients of zero characteristic (as is necessary for Weil's conjectures), one must pass to the limit on rings of coefficients  $\mathbb{Z}/\ell^n \mathbb{Z}$  to obtain " $\ell$ -adic" results.

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## (88) 6. The corpse...

(16 May) 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 of discovery, represented by the language of topos, and the perfectly developed, perfectly efficient tool of étale cohomology - better understood in its essential formal properties, from that moment on, even than the cohomological theory of ordinary spaces<sup>(47)</sup>. This set represents the most profound and innovative contribution I have made to mathematics, at the level of a fully completed work. At the same time, without intending to be, while at every moment everything unfolds with the naturalness of things obvious, this work represents the most extensive technical "tour de force" that I have accomplished in my work as a mathematician<sup>(48)</sup>. These two seminars are for me indissolubly linked. They represent, in their unity, both the vision, and the tool - the topos, and a complete formalism of étale cohomology.

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<sup>46</sup> 略

<sup>47</sup> Even if we restrict ourselves to the spaces closest to the "varieties", such as triangulable spaces.

<sup>48</sup> Some difficult or unexpected results were obtained by others (Artin, Verdier, Giraud, Deligne), and some parts of the work were done in collaboration with others. This does not detract (in my mind at least) the weight of my appreciation of the place of this work in my entire oeuvre. I am thinking of coming back to this point in more detail, in an appendix to the Thematic Sketch, and to dot the i's where it seems obviously necessary.



While the vision is still rejected today, the tool has for nearly twenty years profoundly renewed algebraic geometry in its most fascinating aspect of all for me - the "arithmetic" aspect, apprehended by an intuition, and by a conceptual and technical baggage, of a "geometrical" nature.

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## **(89) 7. ... and the corps 略**

## **(90) 8. The inheritor 88**

(18 May) I don't know if during the sixties any student (apart from Deligne) was able to feel this essential unity, beyond the limited work he was doing with me. Perhaps some of them felt it vaguely, and that this perception was lost without return after the years that followed my departure. What is certain, however, is that from our first contact in 1965, Deligne had 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 the intense interest in him in everything I had to communicate and transmit. This interest manifested itself, without ever weakening, throughout the four years of constant mathematical contact, between 1965 and 1969<sup>(49)</sup>. It gave the mathematical communication between us that exceptional quality that I have spoken of, and which I have experienced with other mathematician friends only in rare moments. It was this perception of the essential, and this passionate interest that it stimulated in him, that enabled him to learn as if by playing everything I could teach him: It was this perception of the essential, and this passionate interest that it stimulated in him, that allowed him to learn, as if by playing, all that I could teach him: both the technical means (the technique of schemes à brin de zinc, Riemann-Roch yoga and intersections, cohomological formalism, étale cohomology, the language of topos) and the overall vision that makes their unity, and finally the yoga of motives that was then the main fruit of this vision, and the most powerful source of inspiration that it had been given to me to discover.

What is clear is that Deligne was the only one of my students until today who at a certain point (as early as 1968, it seems to me) had fully assimilated and made his own the totality of what I had to transmit, in its essential unity as well as in the diversity of its means<sup>(50)</sup>. It was this circumstance, of course, which I think everyone felt, that made him appear as the designated 'legitimate heir' of my work. Obviously this heritage did not encumber him or limit him - it was not a burden, but gave him wings; I mean: it nourished with its vigour these "wings" that he was born with, just as other visions and other heritages (certainly less personal...) would nourish it...

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<sup>49</sup> This period comprises five years, of which my friend spent one (1966) in Belgium doing his military service.

<sup>50</sup> When I speak of "totality", I mean: for everything that was essential, both in vision and in means. This does not mean, of course, that there were no unpublished ideas and results that I never thought of telling him about. On the other hand, I don't think there is any mathematical thinking from 1965-69 that I didn't talk to my friend about 'on the spot[à chaud]', always with pleasure and profit.

More than once in the last seven years, and again more than once in the last few weeks and days, I have sensed a sadness [*tristesse*], for what feels, on some level, like an immense *gâchis* [waste/mess-Trans.] - when what is most precious in oneself and in others is squandered or smothered as if for pleasure. Yet I have also come to learn that such "gâchis" is a basic feature of the human condition, which in one form or another is found 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 "gâchis", which is nothing other than the action of conflict, of division in the life of each person, is a substance of a richness, of a depth that I have barely begun to fathom - a nourishment that it is up to me to "eat" and assimilate. Hence, this *gâchis*, and every other *gâchis* I encounter at every step, and every other thing that happens to me at the turn of the road and which is so often unwelcome - this *gâchis* and other unwelcome things carry within them a benefit. If meditation has any meaning, if it has the power of renewal, it is insofar as it allows me to receive the benefit of what (through my inveterate reflexes) presents itself as "evil", where it allows me to nourish myself from what seems made to destroy.

To be nourished by one's experience, to allow oneself to be renewed by it instead of constantly evading it - this, is what it means to fully take on one's life. I have this power within me, and I am free to use it at any time, or to let it lie wasted. It is the same for my friend Pierre, and for each of those who were my students - free, like me, to nourish themselves with the "gâchis" that I am finishing to make in these last days of a long meditation. And the same is true for the reader who reads these lines, destined for him.

## **(91) 9. The co-inheritors...**

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To tell the truth, while intrigued by the question of the relations between discrete and continuous coefficients, I had not really had any inkling of Mebkhout's crystal theory, which was to blossom in the decade following my departure. On the other hand, there was a vast theme, stemming from my reflections on both commutative and non-commutative cohomology in the fifties (1955-1960), which was only just barely initiated (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 defence (see note no. 81). The non-commutative aspect was initiated later in Giraud's thesis, which develops a geometric language, in terms of 1-stacks on a topos, for non-commutative cohomology in dimension  $\leq 2$ . By the second half of the sixties, the inadequacy of these two primers was quite obvious: both by the insufficiency of the notion of "triangulated category" (developed by Verdier) to account for the richness of structure associated with a derived category (a notion that was to be replaced by the considerably richer notion of the derivator), and by the need to develop a geometric language for non-commutative cohomology in any dimension, in terms of  $n$ -stacks and  $\infty$ -stacks over a topos. One felt (or I felt) the need for a synthesis of these two approaches, which would serve as a common conceptual foundation for homological algebra and homotopical algebra. Such a work was also in direct continuity with Illusie's thesis work, in which both aspects are represented.

Via the notion of a derivator (valid in a non-commutative as well as in a commutative framework), Bousfield-Kan's fundamental work on homotopic limits (Lecture Notes no. 304), published in 1972, was also in the thread of this diffuse programme, which since at least 1967 had been calling for arms to develop. In January of last

year, not yet suspecting that I would be embarking on Pursuing Stacks a month later, I presented Illusie with some thoughts on the "integration" of homotopy types (familiar to homotopists under the name of "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. 略

略 I had sent Giraud, in February last year, a copy of the twenty-page letter, which became chapter 1 opening Pursuing Stacks. It is 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 " $n$ -stack"), which remained heuristic and yet was obviously fundamental. This was the start of Pursuing Stacks. 略

略 It was a question, beyond the construction of "constructible triangulated categories" on the ring  $\mathbb{Z}_\ell$  (above a base scheme, let's say), and the development of the formalism of the "six operations" in this framework (something accomplished, it seems to me, in Jouanolou's thesis), of doing an analogous work by replacing the base ring  $\mathbb{Z}_\ell$  by an arbitrary (more or less?) noetherian  $\mathbb{Z}_\ell$ -algebra, for example  $\mathbb{Q}_\ell$  or an (algebraic?) extension of  $\mathbb{Q}_\ell$ . This is one of those things for which the time has been ripe for a couple of decades, and which are still waiting to be done, when the wind of contempt that has blown over them has died down...

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(91.1) (22 May) I have just read a paper from the Colloquium "Analyse  $p$ -adique et ses applications" of the CIRM, Luminy (6-10 September 1982), by P. Berthelot, entitled "Géométrie rigide et cohomologie des variétés algébriques de car.  $p$ " (24 pages), which outlines the principal ideas for a synthesis of Dwork-Monsky-Washnitzer and crystalline cohomology. The starting ideas (and the name itself) of crystalline cohomology (inspired by Monsky-Washnitzer), and the idea of completing them by introducing sites formed by rigid-analytic spaces, ideas that I introduced in the sixties, have become the daily bread for all those who work in the subject, starting with Berthelot, whose thesis consisted in developing and fleshing out some of these starting ideas. This does not prevent my name from being rigorously absent both from the text itself and from the bibliography. Here is a fourth clearly identified student-undertaker. Who's next?

(7 June) It is a remarkable thing that more than fifteen years after I introduced the starting ideas of crystalline cohomology, and more than ten years after Berthelot's thesis establishing that the theory was indeed "the right one" for proper and smooth schemes, one has still not reached what I call a situation of "mastery" of crystalline cohomology, comparable to the one developed for étale cohomology in SGA Seminar 4 and 5. By 'mastery' (in the first degree) of a cohomological formalism including duality phenomena, I mean no more and no less than the full possession of a formalism of the six operations. While I am not "in the know" enough to appreciate the specific difficulties of the crystalline context, I would not be surprised if the main reason for this relative stagnation is in the disaffection of Berthelot and others for the very idea of such a formalism, which makes them neglect (as does Deligne for his Hodge theory, which remained in its infancy) the first essential "step" to be reached in order to have a fully "adult" cohomological formalism. It is the same kind of disposition that made him ignore the interest of Mebkhout's point of view for his own research.

NB When I speak here of "crystalline cohomology" in a context where one abandons properness assumptions (as is necessary for a "full-adult" formalism), it is understood that one is working with a crystalline site whose objects are (divided power) "thickenings" which are not purely infinitesimal, but are "suitable" (divided power) topological algebras. The need for such an extension of the primitive crystalline 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 (with the initial ideas) from none other than me. A written allusion to this link is found in Thematic Sketch, 5e.

(91.2) 略

(91.3) 略

(91.4) 略

## **(92) 10. ... and the chainsaw**

When I came to live in the region, almost four years ago, there was a beautiful cherry orchard not far from my house. Often when I went for a walk I would go and have a look at it. I was pleased 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 grass proliferates freely. They must not have known fertiliser or pesticides, and in cherry season, that was where I went to pick some that tasted good. There must have been twenty or thirty trees.

One day when I went back there, I saw all the trunks cut off at man's height, the crowns lying on the ground next to the trunk, stumps in the air - a vision of carnage. With a good chainsaw, it must have been a quick job, 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 to the ground. There was a shortage of cherries, alright, and this cherry orchard wasn't going to yield much, that's true - but these stumps of trunks said something else than shortages and yields...

Yesterday I had that feeling again, of a vigorous trunk, with powerful roots and generous sap[sève], with strong and multiple branches extending its impetus - cut cleanly, at man's height, as if for pleasure. It was taking the trouble to look at the main branches one by one, and to see each one cut off, that finally made me see what had happened. What was meant to unfold, in continuity with a deep-rooted inner impetus and necessity, was sharply cut off, by a clean cut, to be designated for all to see as an object of derision.

This reminds me of the 'misunderstanding' that Zoghman spoke of, which took place between my students (except Deligne) and me. What is clear, in fact, is that neither impetus nor vision was 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) to do a well-done job on the subject they had chosen, and which could even be useful to them later on. I can't say whether there was any beginning of something else, going beyond. If there was a beginning, it didn't stand a chance in front of the chainsaw, which cut it down quickly...

I know that if there are still people doing maths - and unless they completely abandon the kind of maths we've been doing for more than two millennia - they won't be able to stop themselves from reviving each of these branches that I see lying inert. Some of them have already been taken over by my friend the chainsaw, and it is quite possible, if God gives him life, that he will do the same with a few others or even with all. Most of them,

however, are no longer in his style. But perhaps he will also eventually get tired of constantly substituting himself for someone else, which is surely very fatiguing and, moreover, not very profitable, and will be content to be himself (which is not so bad anyway).

## X. The Funeral Van

### (93) Coffin 1 - or the grateful D-modules

(21 May) It has been two weeks since my reflection on my “dyed-in-the-wool/*bon teint*” students, those from “before”. Each day, the reflection has been a ‘last supplement’, for the sake of conscience, to a reflection that seemed (practically) finished. More than once, it was an insignificant footnote, carelessly branching off from the previous day’s or the day before’s reflection, which 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 in its funeral cortege, 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 had seemed to be closed and which was then extended by two or three new participants in the Procession, if not by a whole new cortege...

This Procession ends up taking on disturbing dimensions, no one will ever want to read it all! But if it grows like this, it is not, in fact, for the dubious benefit of a hypothetical reader, but first and foremost for my own benefit - just like when I do maths. For these ‘last completions’, which I embark on each time as if against my will, I have never had any regrets about having embarked on them. By dint of these last supplements, I have learned many things that I would not otherwise have been able to learn, without having to think about them “on records/*sur pièces*”. And these things came together, one by one, in a vividly coloured, vastly proportioned and multi-faceted picture. Even now I see that it is not entirely finished - there are two places that still seem to need a final brushstroke.

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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 which might have inspired a merciless fashion. There was in him, who is not of a combative nature, an elementary faith in his own judgement, which is also a generosity, and which (much more than brain ‘means’) is the first condition for doing innovative and profound work.

The idea I have of his work is surely incomplete. From what I know of the main part of his work, it seems to me that with his brilliant means, placed in an atmosphere of warm and active sympathy, he could have accomplished it, and brought it to a greater maturity, in three or four years instead of ten, and in joy rather than bitterness. But three years or ten, and “maturity” or not, the remarkable thing is that the innovative work appeared, and that it could appear under such conditions.

### (94) Coffin 2 - or the truncated cuts

Yves Ladegaillerie started working with me in 1974. It was “by chance”, in a moment of depression/*creux* for him - I submitted to him some naive reflections on the immersions of topological 1-complexes into surfaces, at a time when I knew nothing about surfaces (except the notion of genus), and he even less. It was a bit

grothendieckery (it always starts like that with me anyway...), and it caught on with him more or less, until one day it finally "clicked", I don't really know when and why. It was perhaps at the moment when a visibly juicy question was emerging, a certain key conjecture on the determination of the isotopy classes of a compact 1-complex on a compact oriented surface. True - false? That was the suspense, which lasted well over six months, a year, during which Yves became aware of [*mis au courant*] (and brought me aware of) the key theorems of surface theory, while pushing on the "foundational" parts of his work. The known results made the conjecture rather plausible, but were obviously far from the mark - while the conjecture involved nasty results of 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. It amounts, essentially, to a complete algebraic description, in terms of fundamental groups, of the set of isotopy classes of immersions of a compact triangulable space (say) into a compact oriented surface with boundary<sup>(51)</sup>.

From the moment Yves got "hooked", he did his thesis in a year, a year and a half, results, writing, everything, and even to the nines. It was a brilliant thesis, not as thick as most of the ones done with me, but as substantial as any of those eleven theses. The defence took place in May 1976.

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### **(95) Coffin 3 - or the somewhat too relative Jacobians**

My first encounters with Carlos Contou-Carrère were in the corridors of the Math Institute, the day after my arrival in Montpellier in 1973. He would corner me in some obscure corner to pour a flood of mathematical explanations on me, even before I had time to apologise politely and to get away. What he was pouring out in a jumbled fashion at an impressive rate went completely over my head, without him even pretending to notice, or to be the least bit bothered when I timidly suggested it to him. He was in dire need of someone to talk to, and I was not his only 'unwilling interlocutor'. This was at a time when I was not at all interested in maths. For a year or two, I would run away as soon as I saw his (easily spotted) 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, told me that Contou-Carrère had some unusual means and that he was about to be shipwrecked because he did not know how to use them. Until then, the question of whether or not what Contou-Carrère was pouring out on me made sense, and whether or not he had the means, had not even occurred to me, who was so far removed from all these. Perhaps Lyndon's suggestion came at a time when I was beginning to take some interest in mathematical questions again. Anyway, 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 that I was the first to ask him such a thing, at least for the many years he had been in France. It wasn't easy to get him to explain something, but it was by no means impossible, and it was worth the effort. I soon realised that Lyndon had not been wrong - that Contou-Carrère was full of ideas that only needed to be carefully worked out and developed, and that he had an immediate and very sure intuition in practically every mathematical situation that could be put before him. By this rapidity and sureness of intuition, even in things with which he was in no way familiar, he surpassed me and impressed me - the only

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<sup>51</sup> The "analogous" statement in the non-oriented case is false - decidedly a tricky result, carefully "sliced" into a set of equally "plausible" but nonetheless false hypothesis-conclusions! For further comments on Ladegaillerie's work, see Sketch of a Programme especially the beginning of par. 3.

other student in whom I knew it to a comparable degree was Deligne<sup>(52)</sup>. On the other hand, he had an almost total block against writing! Incredibly, he did maths without writing - God knows how he managed to do even that little, let alone communicate with others, where the "shipwreck" was total (see above).

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略 Looking back, I realise that it was the perfect time to meditate on the meaning of what was happening. The funny thing is that what 'prevented' me from even realising the need for deep meditation was a long meditation I was engaged in at the time, which I have had occasion to talk about<sup>(53)</sup> - and a meditation, what's more, on my relationship to mathematics (if not on my mathematical background)! It was troubled by an episode where life was challenging me forcefully - and I was evading the challenge by becoming agitated, and then plunging back into 'meditation'. In retrospect, I realise that this 'meditation' at the time did not fully deserve the name, that it lacked an essential dimension of true meditation: attention to my own self at the very moment. I was then "meditating" on the meaning of certain more or less remote events, while ignoring a repressed anxiety (perfectly controlled, it's true, as a result of a long habit of such control), a sign of my refusal to take note of the message that this rejected "breath" brought me.

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(95.1) 略

## **(96) Coffin 4 - or the topos without flowers or coronets**

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The first time must have been in 76, 77, Contou-Carrère and I went to see him at his place, out of the blue, just to discuss maths a bit - I don't know if we had any ulterior motive in mind. Perhaps it was understood that Olivier was thinking of embarking on a postgraduate doctorate, and I certainly had plenty of subjects up my sleeve. Having seen him once or twice at Contou-Carrère's, and from what Contou-Carrère himself let me hear, I had the impression that Olivier must have a quick grasp of things, and not only in maths. That evening of three was memorable. I had to tell Olivier about a programme for a fundamental group theory of a topos and van Kampen theorems in the topos framework, and he seemed interested. He must have had some toposic background from Contou-Carrère's algebraic geometry seminar, and he seemed interested in having an opportunity to "get his hands on" the language of topos on an example of a concrete theory. For a good two or three hours, I had to pour over him a thorough masterpiece of the theory that I saw to develop, which grew in substance as I spoke, and brought up in me a host of concrete situations of algebraic geometry and topology - situations that had to be expressed in the topos framework, and which each time I had to first "remind" someone who was hearing about it for the first time. More than once during the evening, Contou-Carrère (who actually

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<sup>52</sup> I am not sure that I have encountered it in other mathematicians, except in Pierre Cartier (who impressed me a lot in his youth with this remarkable ability) and in Olivier Leroy, who will be discussed in the following notes.

<sup>53</sup> See on this subject "The troublemaker boss - or the pressure cooker", s. 43.



had read almost everything and has a strong stomach) looked confused and lost, even for him that was quite a lot at once - 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 obviously fresh and ready, keen-eyed and perfectly at ease, I even laughed, so much so that I couldn't believe he wasn't cracking up, but not at all! He was a young guy, maybe twenty years old, who must have just a little bit of schematics, a little bit of topology and topos, he could also manipulate discrete infinite groups I think... It was three times nothing, to be honest, and with that he managed to fill in all the blanks and to "feel" effortlessly what I, an old veteran, was telling him at full speed in two or three hours on the basis of a fifteen-year familiarity with the subject. I had never encountered anything like this, or at most with Deligne, and perhaps with Cartier, who had also been quite extraordinary in this line, in his younger days.

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## (97) The Gravedigger - or the entire Congregation

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This consensus manifests itself, in most if not all of my former friends or students, not in attitudes of "malice", but in (I believe) entirely unconscious mechanisms, of puzzling uniformity and unfailing efficiency, sweeping aside like a fete[*fêtu*] of the straws of good sense and healthy mathematical instinct, to give way to purely automatic attitudes of rejection<sup>(54)</sup>. Such automatic attitudes, I suspect, are not only aroused by me and by those whose mathematical "odour" reminds one of me - but also by any mathematician who has not stood as already invested with the tacit security deposit[*caution*] of a certain "establishment"; either he himself is already part of it, or he appears as the "protégé" (to use this expression from Verdier's pen) of one of these. It seems to me that in almost all mathematicians, a minimum of "mathematical openness" (necessary for this "good sense" and this "healthy mathematical instinct" to come into play) is only triggered in relation to someone already invested with such a security deposit.

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

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<sup>54</sup> These attitudes of rejection, of course, never present themselves as such, even in extreme cases such as of my friend Deligne, or Verdier. They are almost invisible at the level of conscious dispositions towards me, which (as I have already had occasion to say) are almost always (perhaps even always), in my friends and students of yesteryear, dispositions of sympathy (of which sometimes some of them try to defend) and of 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 understanding of others (independently of the images in which one tries to enclose them).

We are here in a typical situation of ambivalence (collective, I would almost be tempted to say) where, at a glance, one "sees" 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.) Yet, at the level of concrete manifestations (discussed at length in The Burial), the "resultant" of these ambivalent forces no longer has anything ambivalent about it, it seemed to me, but it presents itself well and truly, with "puzzling uniformity and unfailing effectiveness", as the "attitude of automatic rejection" that I am about to examine more closely.

situation in the case of myself and those who in the eyes of the establishment are "my protégés", it is because in the past I have been invested with the status of "one of theirs", with the usual effect of a "minimum of openness" towards me and "my own". This status was taken away from me by my departure in 1970. Or more precisely, by my own choice, clearly expressed in more than one occasions in the years following my departure and by my way of life to this very day, I have indeed ceased to be one of "theirs". In fact, I myself no longer felt to be "one of theirs", and I left a world that was common to us with no spirit to return. Even today, my 'return to maths' is by no means a return 'to them', to the establishment, but a return to mathematics itself; more precisely, a 'return' to a continuous mathematical investment, and to an activity of publishing my mathematical reflections.

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