

Chapter 14

B) Pierre and Motives

14.1 IV Motives (burial of a newborn)

Memory from a dream - or the birth of motives

51 [This note is referenced in note 46 p. 186]

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(April 19) Since writing the above lines (ending with the note “My orphans”, n°46) less than a month ago, I have come to realize that they delay the chain of events to a certain extent! 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 kindly transmitted to me along with a list of his publications. This collection of six texts, published in 1982, constitutes an interesting new piece of information since 1970, in that motives are mentioned in the title and present in the text, however modestly, especially through the notion of “motivic Galois group”. Of course, we are still far from a panoramic picture of a theory of motives, which for the past fifteen to twenty years has been awaiting the impetus of a bold mathematician who will be willing to “paint it, in a vast enough way to serve as a source of inspiration, as a golden thread and a horizon, for one or several generations of arithmetic geometers who will have the privilege of establishing its validity (or in any case of discovering the final word on the reality of motives...) (53).

1982 also seems to mark the year since which¹(*) the changes in fashion are beginning to slowly turn in favor of derived categories; Zoghman Mebkhout (in a perhaps euphoric rush) already sees them as being on the brink of “invading all domains of mathematics”. If their utility, which was made evident by mere mathematical instinct (for well-informed individuals) since the beginning of the 1960s, is now starting to be recognized, it is (or so it seems to me) thanks to Mebkhout’s solitary efforts and his willingness to take on the thankless role of

¹(*) (May 25) I am delaying the events once again, by one year this time - the turning point took place in June 1981 with the Luminy Colloquium, see the note “The Inequity - or a feeling of return”, n°75.

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guinea-pig for seven years, with the courage of those who continue to trust their instinct in the face of tyrannical customs...

Remarkably, in reading this first publication which marks (twelve years after my departure from the mathematical world) a modest re-entry of the notion of motive into the apparatus of admissible mathematical notions, I could find nothing which would indicate to the uninformed reader that my humble person was involved in any way in the origins of this notion, long considered a taboo. Nor is there any allusion to an authorship of some form (51₁) behind the development of a rich and precise “yoga”, which appears in the article (in piecemeal form) as if it came out of the void.

When, just three weeks ago, I laid down my thoughts on the yoga of motives in a page or two, qualifying it as one of the “orphans” whom I held closer to my heart than any other, I must have been sorely mistaken! Surely was I dreaming, when I seemed to remember years of gestation of a vision, tenuous and elusive at first, and growing richer and more precise over the course of the months and the years, as a result of a persistent effort to grasp the common “motive”, the common quintessence of which the several cohomology theories known at the time (54) were but various incarnations, each telling us in its own language about the nature of the “motive” of which it was one of the directly tangible manifestations. Surely I was still dreaming when I remembered the strong impression that Serre’s intuition had made upon me, regarding his conception of a profinite Galois group, an object which appeared to be of a discrete nature (or at least could be tautologically reduced to simple systems of **finite** groups) yet gave rise to an immense projective system of l -adic **analytic** or even **algebraic** groups over \mathbb{Q}_l (by passing to appropriate (algebraic envelopes?)), groups which even had a tendency to be reductive - hence lending themselves to the panoply of intuitions and methods (Lie style) of analytic and algebraic groups. This construction made sense for any prime number l , and I felt (or dreamt that I felt...) that there lay a mystery to be probed, regarding the relationship between these algebraic groups associated to different prime numbers; I felt that they must all come from a single projective system of algebraic groups over the only natural common sub-field to all of these base fields, namely the field \mathbb{Q} , the “absolute” field of characteristic zero. And since I like dreaming, I continue dreamed that I remember having gained access to this glimpsed mystery, through work which surely was but a dream since I did not “prove” anything; and I eventually understood how the notion of motives provided the key to understanding this mystery - that, through the mere presence of a category (here, the category of “smooth” motives over a given base scheme, for instance motives over a given ground field), possessing internal structures similar to those which can be 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 by Serre as well at an earlier time), one would be able to reconstruct such a pro-group (given the data of an appropriate “fiber functor”), and to interpret the “abstract” category as the category of its linear representations.

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This approach towards a “motivic Galois theory” was suggested to me by the approach which I had found, years earlier, to describe the fundamental group

of a topological space or scheme (or even of an arbitrary topos - but here I risk offending delicate ears to whom “topoi are no fun”...) in terms of the category of étale coverings of the “space” under consideration, and fiber functors on this category. The very language of “**motivic Galois groups**” (which I could also have called motivic “fundamental groups”, the two intuitions amounting to the same thing in my view since the end of the 1950s...) and of “fiber functors” (corresponding precisely to the “manifest incarnations” mentioned earlier, namely the different “cohomology theories” which may be applied to a given category of motives) is tailor-made to express the profound nature of these groups, and to suggest their intimate ties with ordinary Galois groups and fundamental groups.

I still remember the pleasure and awe which I felt, in playing this game with fiber functors and (torsors under Galois groups?) which send them to one another by “twisting”, upon retrieving in a particularly concrete and fascinating situation the entire panoply of tools from non-commutative cohomology developed in Giraud’s book, with the gerbe of fiber functors (here taken over the étale topos or, better, the fpqc topos of \mathbb{Q} - interesting and non-trivial topoi if there ever were any!), as well as the (“link”?) (in algebraic groups or pro-groups) connecting this gerbe to the avatars of this link, all being realized by various algebraic groups or pro-groups, corresponding to the different “sections” of the gerbe, corresponding to the different cohomological functors. The various complex points (for instance) of a scheme over characteristic zero each give rise (via the corresponding Hodge functors) to sections of the gerbe, and to torsors providing a transition from one to the next; and both these torsors and the pro-groups acting on them possess remarkable algebro-geometric properties, expressing specific structures of Hodge cohomology - but now I am getting ahead of myself and speaking about another chapter of my dream about motives... This was a time when those who issued the decrees of fashion had not yet declared that topoi, gerbes, and the like did not entertain them as such as it was stupid to speak of them (not that this would have prevented me from recognizing topoi and gerbes when I saw them...). Now that twelve years have elapsed, these very people are pretending to be discovering and are teaching to others the fact that gerbes (if not topoi for now) are indeed relevant to the study of the cohomology of algebraic varieties, and even to that of periods of abelian integrals... p. 208

I could also evoke the dream of another memory (or the memory of another dream...) surrounding the dream about motives, which was also born from a “strong impression” (I am decidedly in full subjective mode!) which some comments by Serre regarding a certain “philosophy” behind the Weil conjectures had made upon me. Their translation into cohomological terms, for l -adic coefficients with varying l , led one to suspect the existence of remarkable structures on the corresponding cohomology theories - the structure of a “weight filtration”^{2(*)}. Surely, the common “motive” to the various l -adic cohomology

^{2(*)} (January 24 1985) For a rectification of this distorted memory, see note n°164 (I4), as well as sub-note n°164₁, which give more details on the filiation of the “yoga of weights”.

theories had to be the ultimate support for this essential arithmetic structure, which as such took on a geometric aspect, that of the remarkable structure of the geometric object that is a “motive”. It would once again be inaccurate of me to speak of a “work” when the task was to “guess” (with the internal coherence of a vision in progress as only guide, using the sparse known or conjectured elements lying here and there...) the specific structure of the various cohomological “avatars” of a motive, and how the weight filtration was expressed therein ^{3(**)}, beginning with the Hodge avatar (at a time where Hodge-Deligne theory had not yet been developed, and for good reason...^{4(***)}). This allowed me (in a dream) to view within a single vast painting the Tate conjecture on algebraic cycles (a third “strong impression” which inspired the Dreamer in his dream about motives!) and the Hodge conjecture (55), and to formulate two or three additional conjectures of the same type, about which I spoke to certain people who must have forgotten as I have never heard anyone mention them since, subjected to the same silence as the “standard conjectures”. In any event, these were only conjectures (unpublished on top of that...). One of them did not concern a specific cohomology theory; rather, it gave a direct interpretation of the weight filtration on the motivic cohomology of a non-singular projective variety over a field in terms of the geometric filtration of this variety by closed subsets of given codimension (with codimension playing the role of “weight”) ^{5(*)}.

There was also the work (I should be putting quotation marks around “work”, but I can’t find the resolve to do so!) I did towards “guessing” the behavior of weights with respect to the six operations (completely lost since then...). Here again, I never felt that I was inventing anything, but discovering - or rather listening to what things were telling me whenever I sat down to listen to them with a pen in hand. What they were saying was of a peremptory, and as such unmistakable, precision.

Then there was a third “motive-dream”, which was in a sense the wedding of the preceding two dreams - regarding the problem of interpreting, in terms of structures imposed on the motivic Galois groups and on the torsors under these groups which can be used to “twist” a fiber functor to (canonically) obtain any other fiber functor ^{6(**)}, the various additional structures exhibited by the category of motives, with the weight filtration being one of the very first such structures. I seem to remember that this process was less guesswork than at any other point, but rather consisted of accurate mathematical translations. The work involved several new “exercises” on linear representations of algebraic group, which I spent days and weeks solving with great pleasure and the feeling

^{3(**)} (February 28 1985) There was a slight confusion in my mind. I was actually referring to the closely linked filtration by “levels”.

^{4(***)} This was at a time when the young Deligne had probably not yet heard the word “scheme” be said in a mathematical context, nor the word “cohomology”. (He learned these notions from me starting in 1965.)

^{5(*)} (February 28 1985) The filtration in question here is actually the “level” filtration (see preceding footnote).

^{6(**)} Just as the fundamental groups $\pi_1(x), \pi_1(y)$ of some “space” X at two “points” x and y can be deduced from each other using the torsor $\pi_1(x, y)$ of homotopy classes of paths from x to y ...

that I was at last getting closer to a mystery that had been fascinating me for years! Perhaps the most subtle notion that I had to apprehend and formulate in terms of representations was that of “polarization” of a motive, wherein I drew inspiration from Hodge theory, trying to extract the ideas that still made sense in a motivic context. This reflection must have taken place at the same time as my reflection around formulating the “standard conjectures”, with both events inspired by Serre’s idea (as always!) of establishing a “Kähler analogue” to the Weil conjectures. In such a situation, in which the things themselves whisper about their secret nature and the ways in which we will be best able to delicately and faithfully express it, even though several essential facts seem to lie outside of the immediate scope of a proof, instinct dictates that we simply write down on paper what the things are insistently whispering, a message which furthermore grows clearer when we take the time to write it down! There is no need to worry about obtaining complete proofs or constructions - to burden oneself with such expectations at this stage of the process is to bar oneself from accessing the most delicate and essential step of a large scale work of discovery: that of the birth of a vision, gaining shape and substance as it emerges from an apparent void. The simple act of **writing**, **naming**, and of **describing** - if only to describe elusive intuitions or mere “hunches” reticent about taking concrete form - possesses a **creative power**. There lies the most important instrument in enacting the passion for knowledge, when the latter is invested into things which can be apprehended by the intellect. In the process of discovery for such matters, this work is the most creative step of all; it always precedes proof and enables it - or rather, without it, the question of “proving” something would not even arise, as nothing pertaining to the heart of the matter would have yet been formulated and seen. Through simple virtue of the effort of formulating, that which was amorphous takes form and lends itself to examination, in process of separating the visibly wrong from the possible, and above all from that which is so perfectly in accordance with the collection of things known, or guessed, that it itself becomes a tangible and reliable component of the vision being born. The latter grows richer and more precise over the course of the formulation process.

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Ten things which are suspected of being true, none of which with certainty (say for instance the Hodge conjecture), through the process of mutually shedding light on one another, completing each other and concurring with a common mysterious harmony, acquire through this harmony the strength of a vision. Even in the event all ten of these things would turn out to be false, the work which resulted in this provisory vision was not done in vain, in that the harmony which it allowed us to glimpse and to ever-so-slightly penetrate is not an illusion, but rather a reality which it is urging us to unravel. Through this work, and only this way, we were able to establish intimate contact with this reality, this hidden and perfect harmony. Once we realize that things are the way they are for a reason, and that our vocation is to know them, rather than to dominate them, we are able to see the day when a mistake is highlighted as a day for celebration (56) - just as much as the day when a proof shows us beyond all doubt that such a thing which we were imagining is indeed the true and faithful expression of reality itself.

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In either case, such a discovery comes as a reward to labor, and could not have been reached without it. But even though the reward may only come after years at the task, or even in the event that we never reach the final word, an achievement relayed to our successors, the work itself is its own reward, rich in every instant of that which it reveals to us in that instant.

Note 51₁ (June 5) Zoghman Mebkhout just pointed out to me a reference to “Grothendieck motives” on page 261 of the volume in question, in a paper of Deligne which “resumes and completes a letter to Langlands”. It reads: “we will not be working with Grothendieck motives, as he defined them in terms of algebraic cycles, but with **absolute Hodge motives**, defined analogously in terms of absolute Hodge cycles”. “Grothendieck motives” (not underlined) are thus not mentioned as a source of inspiration, but in order to create a demarcation, insisting that the paper is treating of **something else** (the latter having been carefully underlined). This distancing is all the more remarkable given that the validity of the Hodge conjecture (a conjecture known to Deligne, I suppose, as well as to any reader of his paper-letter, beginning with its primary addressee Langlands) would imply that these two notions are **identical**!!

Of course, beginning in 1964 when I formulated the notion of motivic Galois group, I knew full well that a notion of “Hodge motive” could be developed along the same lines, leading to a corresponding notion of “motivic Galois-Hodge group”, which was introduced independently by Tate (whether at an earlier or later time, I cannot recall) and thereafter has been known as the Hodge-Tate group (associated to a Hodge structure). The crude scam (which doesn’t seem to inconvenience anybody, since it comes from such a prestigious character) consists in outright obfuscating the filiation of a novel and profound notion - that of motive - as well as the rich web of intuitions which I have developed surrounding this notion, under the derisory pretext that the technical approach taken to study this notion (via absolute Hodge cycles instead of algebraic cycles) is (maybe, if the Hodge conjecture is false) different from the one which I had (provisionally) adopted. This yoga, which I had developed over the course of nearly a ten-year period, was the principal source of inspiration in Deligne’s work from his very beginnings in 1968. Its fecundity and power as a tool for discovery were clear well ahead of my departure in 1970, and its identity is independent of the particular technical approach chosen to establish the validity of such or such limited part of this yoga. Deligne deserves credit for finding two such approaches, independently of any conjecture. On the other hand, he did not have the honesty to name his source of inspiration, persisting since 1968 in hiding it from everyone so as to maintain exclusive benefits, awaiting the opportunity to (tacitly) claim the credit for himself in 1982.

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The burial - or the New Father

52 Coming back to my dream about motives, I should also mention that I remember dreaming out loud. Granted, a dreamer’s work is solitary in nature

- yet, the ebbs and flow of this unrelenting journey that took course over the years, in parallel with a vast project of foundations which occupied the majority of my time, had a witness on a daily basis, someone paying more close attention than Serre, who contented himself with observing things from a distance...⁷() I wrote about this day-to-day confidant in my reminiscence, saying that he “had taken on a bit of a student role” around the middle of the 1960s, and that I had taught him “the little I knew about algebraic geometry”. I have also added that I had even told him about what I did not “know” in the common sense of the word - these mathematical “dreams” (on the theme of motives as on other topics) which he always welcomed with attentive ears and an alert mind, as eager to understand as I was myself.

It is true that, at the time of writing, when I said that Pierre Deligne had “taken on a bit of a student role”, I was only referring to a wholly subjective impression (57), uncorroborated (as far as I am aware) by any written - or at least by any published - source which would suggest that Deligne may have learned something from me - even though I gladly remember presently that I never once discussed mathematics with him without learning something from the conversation. (And even after I stopped discussing mathematics with him, I continued learning from him about other things which are perhaps more difficult and more important, including on this very day in the writing of these words...) p. 213

I was told about the existence of a text by Deligne and others regarding the question of motives, or at the very least of “tannakian categories”, by a third party who surmised (I wonder how!) that I could be interested. Upon reaching out to Deligne thereafter, I was met by his sincere surprise that something of the sort could be of interest to me. Reading through the copy which he kindly sent me nonetheless, I realized that his surprise was indeed completely well-founded. Visibly, I had never had anything to do with the subject in question. There is at most an allusion made in passing, in the introduction, regarding the fact that certain “standard conjectures” (which I had once formulated, heaven knows why) would have consequences for the structure of the category of motives over a field... The reader wishing to know more would be hard-pressed to do so, as no further precision or reference regarding these conjectures is made in the entire book; nor is any mention made to the one and only published paper in which I explain the way in which the category of motives over a field may be constructed in terms of the standard conjectures; nor is the only other text on the topic of motives published pre-1970 cited, an article by Demazure (produced in the context of a Séminaire Bourbaki, if I remember correctly) which followed my construction principle ad hoc from a slightly different perspective...⁸() p. 214

⁷() (May 25) The beginnings of my reflection surrounding motives nonetheless took place before Deligne’s appearance. My handwritten notes on motivic Galois theory are dated to the year 1964.

⁸() Upon verification, I now realize that other than a few pages on the standard conjectures (Algebraic Geometry, Bombay, 1968, Oxford Univ. Press (1969), pp. 193-199), I have never published a mathematical text on the topic of motives. In Demazure’s talk (Séminaire Bourbaki n°365, 1969/70) following Manin’s talk in Russian, there are references made to a series of talks which I had given at the IHES in 1967 and which were meant (I suppose) to serve as a first broad sketch of a vision on motives. Kleiman also gave a talk on the standard

Nonetheless, Neantro Saavedra, who was lucky to be one of my “pre-1970 students”, was duly cited. He had written a thesis under my supervision about what I remember calling “rigid tensor categories”, and which he named “tannakian categories”. One may again wonder through what miraculous coincidence Saavedra’s thesis had foreseen in advance just the needs of Deligne’s theory of motives, which was only to emerge ten years later! In fact, the work that Saavedra does in his thesis is precisely **the** key step needed for the development of a motivic Galois theory, just as J. L. Verdier’s thesis was in principle **the** key step needed for the development of the formalism of the six operations in cohomology. One difference (among others) to Saavedra’s credit is that he actually published his work; granted, he did not have the combined penmanship of Hartshorne, Deligne, and Illusie to exempt him from such a formality. Yet, ten years after the fact, Saavedra’s thesis is now reproduced ab ovo and nearly in toto in a remarkable book, written this time around by Deligne and Milne.

Writing this book was perhaps not strictly necessary, if all that needed to be done was to rectify two particular points in Saavedra’s work (58). But everything happens for a reason, and I think I understand why Deligne himself took the trouble to do this⁹(), going against his own extremely stringent criteria when it comes to publications, which he is known to apply with exemplary rigor when it comes to authors other than himself..¹⁰(*).

Regarding the paternity of the notions involved and of the yoga of motives, the answer goes without saying in the eyes of the uninformed reader (at a time when informed readers are getting rarer by the day and will one day have run their course...) - and this without having to disturb ancestral figures such as Riemann and Hilbert or even the good Lord. If the prestigious author does not say a word about filiation, letting the pretty result on absolute Hodge cycles and abelian varieties appear as a starting point, or even as the birth, of the theory of motives, it is as an honorable act of modesty, fully in line with the customs and ethics of the profession, which advise that we let others (if needs be) give credit where credit is visibly due: to the legitimate Father...

53 Affected by the vicissitudes of the orphan at hand, and doubting that someone else will do the work whose need and scope I am apparently the only one to perceive to this day, I presume that the “bold mathematician” in question will

conjectures and their connection to the Weil conjectures, in more details than was given at the Bombay congress announcement (Algebraic Cycles and the Weil conjectures, in Dix exposés sur la cohomologie des schémas, Masson-North Holland, 1968, p. 359-386). I am not aware of any reflection on the standard conjectures, notably involving steps taken towards a proof thereof, other than my own pre-1970. Based on the echoes which I have received, it seems to me that the deliberate decision to ignore these key-conjectures (which I considered, as I mentioned in my Bombay sketch, as one of the most important open problems in algebraic geometry, together with the resolution of singularities of (excellent?) schemes) has a lot to do with the impression of stagnation which is currently emanating from the cohomology theory of algebraic varieties.

⁹() See on this topic my reflections in the note “Clean Slate”, n°67.

¹⁰(*) (June 8) All the more so when it comes to publications which bear the sign of my influence - see on this topic the episode “The note - or the new ethics”, section 33.

be none other than myself, once I have reached the end of Pursuing Stacks (a project which I expect to last for about another year).

54 Since then, two new cohomology theories for algebraic varieties have appeared (other than Hodge-Deligne theory, which is a natural outgrowth in the “motivic” spirit of Hodge theory) - namely, Deligne’s theory of “stratified pro-modules”, and most notably the theory of crystals, “ \mathcal{D} -modules-style” à la Sato-Mebkhout, together with the new light shed on the later by the God-given theorem (alias Mebkhout’s theorem) which was discussed earlier. This approach towards constructible discrete coefficients is probably destined to replace Deligne’s older approach, due to the fact that it better lends itself to the expression of the connections with de Rham cohomology. These new theories do not produce new fiber functors on the category of smooth motives over a given scheme, but rather (modulo a more extensive work on foundations than has been done as of yet) they provide a way of grasping more precisely the “Hodge” incarnation of a (not necessarily smooth) motive on a scheme of finite type over the complex numbers, or the “de Rham” incarnation on a scheme of finite type over a field of characteristic zero. It is possible that the theory (apparently still unwritten) of Hodge-Deligne coefficients on a scheme of finite type over \mathbb{C} will eventually appear as embedded into the (equally unwritten) theory of crystalline coefficients à la Sato-Mebkhout (with the key added data of a filtration), or, put more precisely, as the intersection of Hodge-Deligne theory with the theory of constructible discrete coefficients in \mathbb{Q} -vector spaces... There also remains the elucidation of the relationships between crystalline theory à la Mebkhout and the theory developed in positive characteristic by Berthelot and others, a task perceived by Mebkhout before 1978, in the midst of a completely disinterested environment, and which appears to me to be one of the most fascinating questions currently posed in the endeavor of understanding “the” (unique and indivisible, i.e. motivic!) cohomology of algebraic varieties.

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55 Even though I was only dreaming, my dream about the relationship between motives and Hodge structures led me to inadvertently notice an incoherence in the “generalized” Hodge conjecture, such as it was initially formulated by Hodge, and to replace it by a rectified version which itself should be (or so I would wager) no more nor less false than the “usual” Hodge conjecture about algebraic cycles.

Prelude to a massacre

56 I am notably thinking about Griffith’s discovery, in the context of the cohomology of algebraic varieties, regarding the falsity of a tantalizing idea that was circulating for a long time concerning algebraic cycles, namely that a cycle homologous to zero admitted a multiple which is algebraically equivalent to zero. This discover of a brand new phenomenon was so striking that I spent a whole week trying to wrap my head around Griffith’s example, by transposing

his construction (which was transcendental over the field \mathbb{C}) into a “maximally general” construction, making sense over base fields of arbitrary characteristic. This extension was not entirely obvious, involving (if I remember correctly) Leray spectral sequences and the Lefschetz theorem.

(June 16) This reflection lead me to develop the cohomology theory of “Lefschetz pencils” in the étale context. My notes on this topic were developed during the SGA 7 II seminar (by P. Deligne and N. Katz) as well as in the exposés XVII, XVIII, XX by N. Katz (who took the care to reference these notes, which he closely followed). On the other hand, in the volume’s introduction by P. Deligne, where it is said that the key results of the volume are exposé XV (the Picard-Lefschetz formula for étale cohomology) and XVIII (the theory of Lefschetz pencils), the author abstains from indicating that I had anything to do with this “key theory” of Lefschetz pencils. In reading the introduction, one gets the impression that I played no part in the development of the volume’s themes.

The long seminar SGA 7, which took place in the years 1967-69, continued the seminars SGA 1 through SGA 6 which were developed under my impetus between 1960 and 1967, was organized by Deligne and myself, after I kicked off a development of a systematic theory of groups of vanishing cycles. Since the write-up of the talks by various volunteers dragged for some time, the two volumes of the seminar (SGA 7 I and SGA 7 II) were only published in 1973, by Deligne. Even though it was agreed during the seminar that we would be presenting it as a common endeavor, Deligne made the (strange) request after my departure that we **cut the seminar in half**, with a part I presented as being directed by me, and the other half by himself and Katz. I now view this event as part of an “operation” foreshadowing the operation “SGA $4\frac{1}{2}$ ”, which aims (among other things) to present the entirety of the foundational series SGA 1 through SGA 7 - which in his mind and point of view were inseparable from my person, as well as the series EGA, i.e. *Éléments de Géométrie Algébrique*, as a compendium of texts with a variety of authors, where I myself only play an episodic, if not superfluous, role. This tendency appears very clearly, if not brutally, in the volume SGA $4\frac{1}{2}$ and most notably in the seminar SGA 5, which is inextricably linked to that volume. See the note “the clean slate”, and “the massacre”, n°s 67 and 87, and most of all “the remains...” (n°88), among others.

(June 17) I was responsible for the overall structure of the seminar SGA 7 (for which I did not see a need for a separation into parts “I” and “II”, and still do not to this day) while Deligne made important contributions (mentioned in my report on Deligne’s work written in 1969, see n°13, 14 of this report), the most crucial for the needs of the seminar being the Picard-Lefschetz formula, proven via a specialization argument starting from the already known case in the transcendental setting. The cutting of the seminar in two parts was unjustified mathematically, as well as regarding our respective contributions - both Deligne and I brought substantial contributions to each of the two “pieces” of SGA 7.

I would of course have been delighted if Deligne had continued the foundational series SGA, which I had started and was far from reaching its end!

évanescents

However, this “operations SGA 7” is not at all a continuation but rather a sort of brutal “saw cut” (or chainsaw cut...) **putting an end** to the SGA series, with a volume which ostentatiously is distinct from my person, even though it is linked to my work and bears its mark as much as any other. Even though my person is obscured as much as it can be, the tone taken with respect to my work is not yet the barely disguised tone of disdain that characterizes the “operation SGA 4 $\frac{1}{2}$ ” - the latter represents an even more brutal saw cut, affecting the unity of the seminars SGA 4 and 5, and serving as a means and pretext to the lawful plunder of the unpublished part of SGA 5, whose stolen pieces were equitably shared by Deligne and Verdier...

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57 I should quickly mention that the same remark applied to the other gifted mathematician about whom I hazard to say (in note n°19) that he took on a bit of a student’s role, ten years after Deligne.

58 This reminds me that the (publication?) Notes (which had already published six or seven “pre-1970” PhD theses produced under my supervision) never accepted to publish Yves Ladegaillierie’s thesis from “post-1970” (stated reason: they do not publish theses!). It should be said that they did on the other hand publish Saavedra’s thesis for a second time... I had also told Deligne about Ladegaillierie’s beautiful result on isotopy which was refused by every journal (with the secret hope that he would lend a hand to help him publish it) - but it unfortunately did not interest him (stated reason: his incompetency in subject of the topology of surfaces...).

End scene...

The new ethics (2) - or the free-for-all fair

59 (April 20) During the few weeks separating me from the writing of the above lines, which identify a contradiction and its cost, I learned with surprise that the person in question had already found a most simple way to “resolve” said contradiction two years ago - one only had to think about it! This solution could be called “the method of the preemptive burial” (about which the reader can learn in the double note (50), (51) written yesterday, while still freshly affected by the discovery). I apologize in advance if the unexpected reappearance of the preemptively deceased individual in the famous “mathematical world” (which sometimes takes on the airs of a free-for-all fair...) risks introducing technical complications to the flawless execution of this brilliant method! In an earlier note (“deontological consensus - and control of information”, n°6) I felt (still confusedly) that the most universally admitted deontological rule in the scientific profession “went unheeded” as long as the individuals who have control over the circulation of scientific information did not respect the right that any scientist has to make their ideas and results known. Around this stage of the reflection, I also took the time to thoroughly describe a case study in which the disregard shown for this right was flagrant in my eyes - so much so

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that I felt that the disregard displayed was bordering on disdain for the number one rule, which itself is admitted by general consensus. (See “The note - or the new ethics”, section 30).

This wasn’t the only time that I felt this very particular sense of unease, witnessing the **spirit** of the number one rule being disregarded while the very perpetrator was displaying a “thumbs up” through his position (above all suspicion!), his means, and the casualness of the execution. I attempt to pin down this uneasiness in the note “The snobbery of the youth - or the defenders of purity”, in connection with the aforementioned section. Once we begin disregarding the “obvious” things about which I am speaking, as well as (I should add) the (possibly deep) things which are neither proven nor patented as “conjectures” published and known by all, we may as well (given what little there is!) consider them to be public property (trivial, of course)¹¹(*), and as such, when the time comes, as “one’s own”, with the greatest nonchalance and the most unaffected conscience - because it goes without saying that we would never claim ownership over a difficult proof of ten or a hundred pages (or even ten lines) which establishes a result that “we would not have been able to prove” (59’). I did not think I was being so sensitive or accurate (regarding the “unheeded

missing sentence

rule”)¹²(**)

I fortunately have the ability to defend myself - being able to express with some accuracy what I feel and want to say, having acquired (rightly or wrongly) a certain credibility, and having the chance of being heard when I have something to say, or having the possibility to publish if I feel the need to do so. On the other hand, I now vividly realize the “feeling of injustice and powerlessness” to which aggrieved individuals without my privilege are subjected, feeling like their hands are tied in the face while “those who own everything” may dispose of them arbitrarily - a power which they use however they please.

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It is true that I have at times displayed such condemnable behavior in my own life as a mathematician, in equally good conscience - and I have had the occasion in my reflection to speak about some of these instances as my conscience brought them out of my subconscious, where they had been buried together with their unexamined ambiguity. In probing these events I finally understood that I had no reason to be surprised at the fact that today (and for quite some time) the student has surpassed the master, and that I shouldn’t repudiate anyone towards whom I feel sympathy or affection. It is nonetheless healthy for me and for others to call a cat a cat, whether that cat be from our home or someone else’s home.

Appropriation and contempt

¹¹(*) Such was the fate of the “God-given theorem” (aka Mebkhout’s theorem),

(June 8) And this, as for the yoga of motives, while also deftly creating an impression of filiation, without ever saying so explicitly! See on this topic (as a case study) the note “The Prestidigitator” n°75, and for the brilliant general method or style, the note “Thumbs up!” n°77, as well as the note to follow “Appropriation and disdain”, n°59’.

¹²(**)

!59' (June 8) I am no longer convinced of the above, concerning my friend Pierre Deligne, as I have witnessed that he eventually gave in to the game of “tacit filiation” vis-à-vis the tool of l -adic cohomology, i.e. what I call the “mastery” of étale cohomology. A remarkable evolution occurred between the “operation SGA 4 $\frac{1}{2}$ ” (where my name is still spoken, albeit with an attitude of flippant disdain towards this central component of my work, from which his own draws its origins) and the “The Funeral Service” in which any reference to even the word “cohomology” is scratched in relation to my name. (See the notes “Clean slate” and “One of a kind” for the initial phase, and the notes “The Funeral Service (1), (2)” for the final phase.)

Among the intermediary phases in this escalation, there was the “memorable paper” of 1981 on so-called “perverse” sheaves (see on this topic the notes “The inequity - or a feeling of return” and “Thumbs up!”, n°75 and 77), and the exhumation of motives in LN 900 the following year (the Funeral Service taking place the year after that, in 1983). In all of these cases and others of lesser scale, I was able to realize that the internal attitude and “method” which allowed Deligne to claim credit for others’ ideas with flawless good conscience was that of **disdain** (one which remains partially tacit, all the while being deftly suggested) towards the “little” which we are about to appropriate - so “little” in fact that there is no need to even speak about it, especially given that we are about to use it right away to do truly powerful things - think Weil conjectures, theory of so-called “perverse” sheaves, ... After the operation is finished, and the appropriation is complete and accepted by all, there is always time to rectify the situation and to modestly show off that which has been appropriated. The same contribution is treated with offhand disdain while it remains attached to the name of one of those who are to be buried, only to be highlighted once it has been appropriated by himself (l -adic cohomology, motives, Mebkhout’s yoga) or by a good friend (yoga of derived categories and yoga of duality, appropriated by Verdier under Deligne’s active encouragement).

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14.2 V My friend Pierre

The child

60 (April 21) Coming back to my dream about a memory, which concerns more than the birth of a vision... I remember (even though I have forgotten so many things!) the ever-renewed pleasure which I took in discussing with the person who had quickly become my confidant on everything which captivated me, as well as each step forward and enchanting discovering in my day-to-day love story with mathematics - and as such he never really was a “student”. His perennial receptiveness and the ease with which he learned about each thing (“as if he had always known it...”) acted as a constant source of enchantment. He was an ideal listener, moved by the same thirst for understanding as my own - he has an extremely sharp ear, signaling a communion between us. His comments always ran ahead of my own intuitions or restraint, or shed some

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new light on the reality I was painstakingly trying to grasp through the mist that still surrounded it. As I have said elsewhere, he often has answers to the questions which I was asking, sometimes even on the spot, while other times he would reach the answer in the days or weeks that followed. The role of the listener was reciprocated when he took his turn in sharing the answers which he had found, which he presented as no more than the nature of things, always appearing with perfect naturality, and presented with the same ease which had tantalized me and certain of my elders such as Schwartz and Serre (as well as Cartier). This was the same simplicity, the same “evidence” which I had always pursued in my quest to understand mathematical things. Without needing to mention it, it was clear through our shared approach and standards we both belonged to “the same family”.

Ever since we first met, I had felt that his “abilities”, as we say, were of a very rare quality, and far exceeded the modest abilities which I myself possessed, even though we were of the same breed when it came to our passion for understanding and our exigency regarding the comprehension of mathematical things. I also sensed, dimly, without yet being able to put my finger on it that this “strength” which I noticed in him (and which I also noticed in myself, although present to a lesser degree) of “seeing” the obvious things which nobody else could see was the faculty of a child as well as the innocence of a child’s eyes. He held within him something of a child, much more visibly than other mathematicians whom I have known, and this was surely not an accident. He once told me that one day, while he was still in high school I believe, he independently took the time to verify the multiplication table (as well as the addition table along the way) for the numbers 1 through 9 from the definitions. Not that he expected to find anything surprising - other than possibly the (pleasant as always...) surprise that the proof could be accomplished so beautifully and completely in a matter of a few pages, and in just about half an hour. I could sense, while he cheerily related this anecdote, that this had been a half hour well spent, and that is something I understand today even better than I did then. This story of his had marked me, even impressed me (even though I don’t recall making that known to him) - because I saw it as the sign of a **self autonomy**, and of a certain freedom with regards to received knowledge, both of which had accompanied my relationship to mathematics since childhood, at the very first contact. (69)¹³().

This status of privileged interlocutor which we shared with one another, at a time when we saw each other nearly every day¹⁴(*), continued over a period of

¹³() In fact, I believe that this freedom has never entirely disappeared over the course of my mathematical life, and that it is now as present as it had been during my childhood. Two or three years ago, I reminded my friend about the multiplication table episode. He seemed embarrassed by this evocation of a childhood memory, which visibly no longer corresponded to the image he had of himself. I wasn’t really surprised by this embarrassment, but it pained me to see once again a confirmation of something which I had by then understood but struggled to admit...

¹⁴(*) Such was the case while I was in Bures, and he was housed in an IHES accommodation. Starting from 1967 (when I moved to Massy), I think that we still saw each other once or twice a week, at least during the time when I was still invested in mathematics.

5 years - from 1965 (if I remember correctly) to 1969 included. I still remember the pleasure I took, during that year, in writing a comprehensive report of his works as part of my proposal to appoint him as professor at the institution at which I had been working since its creation (in 1958), and where I produced the largest part of my mathematical work. I no longer possess a copy of that report (64), in which I had reviewed around a dozen papers by my friend. Almost all of them are still unpublished (in fact, many remain unpublished), and most if not all of them individually contained, in my opinion, enough substance to constitute a solid Ph.D. thesis. It made me prouder and happier to present this eloquent report on his behalf than if I had been presenting a report on my own works (something which I have only done twice in my life, and even then only by forcing myself...). Many of his papers brought answers to questions which I had asked (the only published one being the aforementioned paper on the degeneracy of the Leray spectral sequence for a smooth and proper morphism of schemes (63)). Nonetheless, the two most important papers constituted answers p. 225 to questions that Deligne had asked himself, and in this case it was clear that their scope far surpassed that of a “solid PhD thesis”. I am referring to his work on the Ramanujan conjecture (which appeared in the Bourbaki seminar), as well as his work on mixed Hodge structures, also known as “Hodge-Deligne theory”.

At the time of writing of this shining report, the thought that I would be leaving the institution at which I was about to appoint my young and impressive friend, and where I was hoping to dwell for the foreseeable future, would have appeared to me as strange and very much unsuspected. Likewise (as I am now able to connect these two pairs of double-events), it seems strange, and probably not “coincidental”, that this same (no longer young!) friend of mind has communicated to me a month or two ago his own departure from that very institution, just about a year after I had resumed a regular mathematical activity in the form of an unexpected “return” to the mathematical world (if not to its “high spheres”...).

I expressed myself about this departure - this “salutary disconnection” - more than once in *Récoltes et Semailles*, and I wrote even more often about the “awakening” that happened soon thereafter, rendering this episode a crucial turning point in my life. During the intense years that followed, the mathematical world, as well as the members whom I had loved and the components of mathematics itself which had most fascinated me, became very distant from my mind - as if drowned into the mist, memories of another “me” who had been dead for ages...

Well before this episode, as well as during the years that followed this first great turning point, I knew that he who had been my students (a little¹⁵(*)) as well as a confidant and a friend (a lot) needed only follow discontinuous impulse of the child in him, at play and in the pursuit of knowledge, to discover and bring forth new and unexpected worlds, probing them and understanding their

¹⁵(*) For more on this hesitation to consider the (overly!) brilliant Deligne as one of my students, see the note “One of a kind” (n°67).

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intimate nature - and by so doing revealing them to his peers and to himself. As such, if I could envision a “bold mathematician” busy laying out a rough outline (to begin with...) after my departure (with no intention to return!) of the vast scenery which I had glimpsed, and of which I had only made a few cursory and provisional sketches. It would definitely have to be him - as he had everything in place to do so! To paint this far reaching tableau, a “master plan” uniting in a common vision the essential points of what was known and already guessed about the cohomology of algebraic varieties, required the work of a few months rather than years, for a person within whom this vision was already in place and ready to take form out of the mist of the yet unwritten. (Even if it involves further developing it over the course of years or generations if needed - until the end word of the reality of motives has been fully understood and established.) At that time, I had no doubts that this work, which felt “burning hot” would be accomplished at any moment, at the very least during the two or three following years. After my departure, there remained a single person who was called, by his very impetus towards knowledge, to pursue this pressing and fascinating work, even if it meant letting others further pursue this work. Once the master plan has been written and proofread, one can always launch into other adventures within the world of mathematical things, where every twist of the road brings the promise of a new and limitless world, for those who come with fresh and open eyes...

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At the time when my life was still unraveling in the warm scientific cocoon, isolated from the noises of the world, and when Deligne was developing his extension of Hodge theory (the year must have been 1968 or 69) it went without saying that this work was a first step that needed to be taken in order to realize, test, and make more precise a certain **part** of this “tableau of motives”, which had not been written out in its entirety.^{16(*)} In the years following my departure from the cocoon, at a time when mathematics seemed very distant from me, I was not surprised upon learning that the Weil conjectures had been finally proven. (If anything surprised me it was that the “standard conjectures” had not been proven in the same stride, even though they had been formulated as a possible approach towards the Weil conjectures, as well as a way to establish, at the very least, a theory of semisimple motives over field.^{17(**)}.) I knew that neither this first jet towards a general theory of Hodge-type coefficients, nor through this proof of certain key conjectures (among many others that were more or less known) he was still not operating at full measure - he was in fact falling significantly short of that. And I was impatiently waiting for him to reach his full potential, even when the bulk of my attention was taken up by

^{16(*)} The fact that this Hodge-Deligne theory never (as far as I am aware) developed beyond the stage it reached through this first jet, to grow into a theory of “Hodge-Deligne coefficients” (and of the associated “six functor formalism”) on schemes of finite type over the complex numbers is indissociable from another strange fact: namely, that this vast “tableau of motives” has never been painted, its very existence having been carefully silenced to this day...

^{17(**)} It is only in the past few years that I began to vaguely realize (albeit with more precision as of late!) that the “standard conjectures”, as well as the very notion of motives to which they provided a “constructive” approach, had been **buried**, for reasons that now appear to me as particularly clear. (Compare also with the preceding footnote).

other things. (-> 61)

The burial

61 I had the privilege to witness his first youthful spirit carrying the promise of a vast expansion to come. During the following fifteen years, I started realizing that this promise was being repeatedly delayed. There was something delicate within him that I had been able to sense and recognize (even at a time when I was insensitive to so many things!) something altogether different than sheer intellectual horsepower (which is as smothering as it is penetrating...) - the most essential thing of all for any truly creative work. I had sensed this in others at times, but I had never felt it to such a degree in any other mathematician. I had expected (as is natural) that this force would continue blossoming and developing within him, to later effortlessly express itself in the form of a unique work of which I would have been the modest precursor. Equally as strangely (and there surely must be a deep and simple link connecting all of these “strange phenomena”), I saw this delicate thing, this “force”, which pertains neither to muscle nor to brain go progressively dimmer over the course of the years as if **buried** under successive layers, ever thicker layers of **something else** that I know too well - the most common thing in the world! Said thing doesn’t necessarily collide with intellectual horsepower, nor would it impede a consummate experience or a skilled flair in a particular field, and both of these skills would force the admiration of some or fear of others or both at once through the accumulation of accomplishments that are potentially brilliant and would surely project force and beauty. Yet it is not **this** which I had in mind when I spoke of “expansion” or of “blossoming”. The blossoming I had in mind was to be the fruit of innocence, hungry for knowledge, and always ready to rejoice at the beauty of things both small and large in this inexhaustible world of ours, or in some particular region of this world (such as the vast universe universe of mathematical things...). Only a blossoming of this kind holds the potential for a profound renewal, being the renewal of oneself or that of ones understanding of things in the world. This potential was fully realized, I believe, in the humble man Riemann¹⁸(*). This true blossoming removes one from disdain; p. 228
be it disdain towards others (whom one feels are far below oneself...) or disdain towards things that are too “small” or too obvious to warrant attention, or which are deemed below ones legitimate expectations; or even disdain for a given **dream**, which is insistently communicating things which we claim to love... Disdain thereby becomes as foreign as the self-satisfaction which feeds it.

Granted, because of his impressive “means” as well as this delicate things which impresses no one yet **creates**, the “student” was destined to far surpass the “master”. I did not doubt that right after my departure from this milieu where I had witnessed such a beautiful ascent, Deligne would give his full measure to the development of a vast and profound work which I will have helped initiate. The echoes of such work would surely reach me over the course of the

¹⁸(*)

years, and as I will be engaged in separate quests far from mathematics, I will only be able to partially grasp the full scope and beauty of the worlds that he was about to discover.

However, the student cannot surpass the master while simultaneously inwardly **disavowing** him, secretly laboring towards the suppression of any trace of what he had contributed (whether that contribution had been for the better or the worse...), both from his own eyes and the eyes of others - in the same way that the son cannot truly surpass the father while simultaneously disavowing him. This is something that I have learned mostly through my relationship with my own children, but also (later) through my relationships with certain past students of mine; mostly the one student, among others, whom I had always refrained from calling a “student”, having felt since the day we met that I was about to learn from him as much as he would be learning from me¹⁹(*). Only ten years after that day, post 1975, and especially since I began meditating upon the meaning of my experiences, have I begun sending this **hindrance** within this person who nonetheless remained dear to me. I also sensed, obscurely, that this secret disavowal of my person and of the role which I had played during these crucial years of his life, were also, on a deeper level, a disavowal **of himself**, (so it goes, godlessly, every time that we disavow and wish to erase something which has happened to us, when it is our role to harvest the fruit...).

Having failed to remain “plugged into” “what has been happening in mathematics”, including what he had been doing himself²⁰(*), I had never realized, until I stopped to reflect a couple of weeks ago, the extent to which this hindrance had weighed equally on that which he had given his whole: his mathematical work. Admittedly, more than once in the last eight or nine years, I have seen his mathematical instinct and simple common sense be erased through a deliberate manifestation of disdain (towards me) or of contempt (towards other whom he had the power to discourage) (66). He also wasn’t the only one of my ex-students - with or without quotation marks - in whom I witnessed similar attitudes towards people whom I held dear (or towards others). None of these other occurrences affected me as painfully. More than once over the course of the past two months I have alluded to this experience in my reflection, “the most bitter experience of my life as a mathematician” - and I have also shared what it taught me as part of my reflection. This pain was so acute, it taught me something profound about a person whom I had always held dear (while I was also busy excavating what he taught me about myself and my past...), that the question of whether or not this event had an impact on mathematical creativity whether within him or within those who were discouraged or humiliated, became entirely accessory not to say derisory.

The note “refusal of a heritage - or the premise of contradiction” is the first written reflection in which I present a summary of what had occurred to me bit by bit, over the course of the years, both regarding the “state of the art” as well as regarding the work of the person whom I had known so well yet so

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little. This marks the first time that I finally saw, at a glance, the “**price**”, or the weight within his mathematical work, of this refusal that he must have been carrying within him for over fifteen years. Even then, in writing this note, I was “delaying” the chain of events, in that for more than two years already (without anyone deeming it useful to update me), motives had become public knowledge and removed from the secretive state in which they had been kept for twelve years. . . Today, as I write the ultimate section (I think) of my reflection on my mathematical past, and just two days after having learned the broad p. 230 strokes of the memorable volume consecrating this furtive “introduction”, I am stricken by the crushing weight that transpires. By this I mean the weight that is carried around, day after day and through circuitous paths, by a being born to fly - lightly and delicately, with joy and confidence towards the unknown, out of sheer joy for oneself and for the carrying wind. . . ²¹(*)

If he does not fly, instead contenting himself with being admired and feared by others, by accumulating proofs of his superiority, I need not worry. He must surely find satisfaction in carrying around this weight - just as I in turn carried some weights and continue to this day to carry those which I did not know how to leave behind. He took what he liked from what I had to offer, both good and bad, and left the rest. I need not worry about such choices which pertain only to himself; nor need I hereby decree which choices are better than others (62). What is “better” for one is worth for the other, or sometimes even for the same person (who may have changed over time, albeit this is a rare occurrence. . .).

The choices I do make and the acts that follow (even when our words deny them), are made at my own risk. If they often bring us the expected rewards (which we receive as “the goods”), such rewards often end up having repercussions (which we recuse as “bad” and often react with outrage). Once we understand that the repercussions cannot be recused, we start considering them as a price to pay, and we so oblige with reluctance. Yet we sometimes understand that these repercussions need not be seen as cashiers who must be paid for the good time we had. Rather, they can be seen as patient and persistent messengers who tirelessly come back to us with the same message; an unwelcome message constantly recused - because this humble message, even moreso than the repercussion itself, is what appears to us as “the worst”; worse than a thousand repercussions, worse than a thousand deaths, or the destruction of the entire universe, about which we no longer give an f. . . p. 231

The day we finally decide to welcome this message is the day when our eyes suddenly open to see that what was feared as “the worst” is really a **liberation**, an immense salvation - and that the crushing weight from which we are suddenly relieved is the very thing to which we were holding on just yesterday as “the goods”.

²¹(*)

The event

62 (April 21) If there is no reason for me to worry, one may ask why I am elaborating for page after page about a personal relationship, when this relationship only concerns me and the person in questions!

If I felt the need to launch into this retrospective reflection on certain important aspects of this relationship, it is due to the impact of a specific event which hit close to home (even though I am only learning about it two years after the fact). This event is furthermore public information, even more so than the behaviors and routine actions of renowned mathematicians (such as Deligne and myself) towards lesser known or beginner mathematicians (even though the impact of these acts on people's lives is often far greater than in the case at hand). The even in question (namely the publication of the “memorable volume” that is the Lecture Notes LN 900, aka “the burial volume”) as well as the circumstances surrounding it appeared to me as **toxic**, at least in my eyes. As such, it seemed healthy for everybody, starting with the “person of interest” himself, to give a more circumstantial testimony regarding certain ins and outs of the matter, so as to lay things down as I perceive them today.

Through this testimony and this reflection, I am not trying to convince anybody of anything (something far too tiring, not to mention hopeless!)^{22(*)} But rather to understand the events and situations in which I found myself involved. If this leads others to a true reflection, beyond the usual formalities, then this testimony will not have been published in vain.

The eviction

63 This article^{23(*)} in the Publications Mathématiques in 1968, two years after my departure from the mathematical world. Its starting point had been a conjecture which I had shared with Deligne, regarding a degeneracy property of certain spectral sequences, which at the time could have seemed rather incredible, yet which nonetheless became plausible through the “arithmetic” viewpoint, as a consequence of the Weil conjectures. This motivation was of great interest in itself, as it showed just how much one could gain from using the “yoga of weights” implicitly contained in the Weil conjectures (a yoga that was first glimpsed by Serre, in certain important aspects of it). Already then, I currently applied it to all kinds of analogous situations, in order to extract conclusions about the “geometric” nature (for the cohomology of algebraic varieties) using “arithmetic” arguments. This happened on a heuristic level for as long as the Weil conjectures had not yet been established, but it nonetheless had a great power as a proving mechanism, representing a **tool of discovery** of the highest order. Deligne’s “geometric” proof for the particular conjecture in question, using the Lefschetz theorem (which had at the time only been established in the characteristic zero setting) was interesting for a different reason, in addition to being independent of any conjecture. The link indicated by these two approaches

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between two things which could at first sight seem to be unrelated, namely the Weil conjectures on the one hand (and the associated yoga of weights, which for me constituted the most fascinating aspect), and the Lefschetz theorem on the other hand, was extremely instructive in itself.

Most interesting for the present purposes, is something which only fully appeared to me today, namely that the reader of this article would be given little reason to think that I had something to do with the initial motivation of the main result, and no reason at all to learn what exactly this motivation was. (See also the beginning of note (49.)) The **spontaneous** approach (including, I am sure, for the author himself) in introducing such a result would have been to begin with the (striking) conjecture, then to indicate the first piece of motivation, equally striking, which would have constituted a good occasion to finally “sell” this famous yoga of weights, a much farther reaching accomplishment than the principal result of the article ²⁴(*) then to follow up with the “Lefschetz theorem” ²⁵ viewpoint, allowing one to prove the initial conjecture under slightly more general conditions, over an arbitrary base scheme, not necessarily smooth and proper over a field, but only in zero characteristic. On the other hand, the chosen approach begins with homological algebra generalities (very pretty of course, and presented with the customary elegance of the author), generalities which he has probably since forgotten, like everyone else, as a sort of axiomatization of the Lefschetz theorem. The main result (the only one of course which everybody remembers) appears around corollary X in the middle of the paper, while the word “weight” and my name are only pronounced in “remark 3.9” somewhere near the end (without the reader really knowing why)... p. 233

I don’t recall my reaction upon first discovering the paper - as I was still in the game, I probably just took a brief glance at it. I probably felt an intention from him to “distance himself”, but at the same time brushed it off as a natural decision from my friend so as not to risk appearing as a disciple (or “protégé”) of a “master”. ²⁶(***) Nonetheless, if he had had a quiet confidence in his own strength, he wouldn’t have hesitated to write a further reaching and more universally useful (including for himself) work without fearing that it be seen for what it is... (65). p. 234

The situation was fairly analogous upon the publication of his first far-reaching work in the following year, concerning mixed hodge theory. (I then considered this work to have a comparable scope to Hodge theory itself, as I conceived of it as a starting point for a theory of “Hodge-Deligne” coefficients which unfortunately never saw the light of day...) As I mentioned earlier, it was obvious for both of us that this work took its “motivation” from the yoga of motives, which I had developed over the course of the preceding years - namely, it constituted a first approach towards a tangible incarnation of this yoga. Highlighting such a link in his work would have, I believe (and as I probably believed then as well), would have made his work even more wide-ranging than they already were by virtue of their own merit. At the same time, this would also have

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been an occasion to call the attention of the reader towards the world of motives which could be felt at every step underlying that of Hodge structures (63₁).

Only in hindsight do one of these omissions take on their full meaning, overlaid upon six years of silence regarding the yoga of weights²⁷, twelve years of silence (not to say of prohibition) regarding motives²⁸, their surprising reentry in the burial-volume LN 900, the stagnation of Hodge-Deligne theory following dazzling beginnings... But one can't do great things while also serving as an undertaker!

In any event, had I been more mature at the time of my departure from the IHES in 1970, I would have clearly felt the presence of a profound ambivalence towards me, coming from the person who had been my closest friend during the preceding five years. Furthermore, behind the friendly facade of good company inside that same muted institution, the fact was that my departure arranged everybody for reasons which I believe I can perceive in retrospect, and which varied from one person to the next. Visibly, this departure was wonderfully convenient for my young friend, who had recently arrived at the institution, and who would only have had to express solidarity towards me (to counterbalance the hesitating indifference of the other three permanent colleagues) in order to reverse an irresolute situation. If I did not understand the meaning of what was happening then, it must have been that I decidedly did not want to; what was being said was loud and clear! As was often the case at other times in my life, I felt within me an anxiety (without ever quite naming it!) signaling to me a “detachment” between a tangible and straightforward reality, and a picture of reality of which I could not separate myself from: namely, the picture I had of what my role had been in the institution which I was living, and perhaps more importantly, the picture I had of my relationship with my friend. It was this refusal to face an undeniable reality, together with the anxiety signaling this contradiction to which I was clinging, that made this “salutary exit” so painful at the time.^{29(*)}

To tell the truth, having never before engaged in a written reflection surrounding this relationship (except for some brief jets of reflection included in a few of the occasional letters I sent to my friend, never to receive a reply...), I had not realized that the first signs (admittedly discreet, but nonetheless unmistakable) of the ambivalence that affected my friend's relationship with me can be traced back to at least 1968, two years before “the great turning point”. This was still a time when our relationship appeared as perfect, a flawless communion at the mathematical level, in the context of a simple and effectuous friendship. One could therefore be tempted to criticize my previous “tributes” regarding innocence, the creative energy of the child, and all the rest!

Yet I know that this communion was a **reality** and not an illusion; just as this “delicate thing” was a reality - namely, the creative force which is only lightly reflected in the resulting work. “The innocence” and “the conflict” are tangible realities, rather than mere concepts, visible to anyone who cares to look

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^{29(*)}

carefully; and they appear to me as natural opposites, with one excluding the other. Yet, there is no doubt that these two realities existed jointly within my relationship with my friend, at varying levels.³⁰ During the period about which I am writing, it does not seem that the “conflict” interfered with mathematical creativity - at least not with work done in solitude, or during one-on-one meetings. It is also true, that in the two aforementioned papers, which are some of the most tangible fruits of this work, the footprint of the conflict was already clearly visible. With fifteen years of hindsight, and at the turn of my reflection over the past days and weeks, I now see that this footprint (however discreet) strikingly foreshadows the particular hold that this growing conflict was about to have on the initial impetus, stripping it of its core essence, over the course of years, that essence which leads to great destinies(*).

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Note 63₁ (May 26) Compare also with the remark made in the footnote³¹(*) made at the end of note 60, commenting on the “block” faced by the natural development of Hodge-Deligne theory, caused by an attitude of denial towards some of the key idea which I had produced (namely the six operations, to which motives are inexplicably linked), a denial of the same nature as what is being presently examined, and which was therefore visible ever since the publication of *Théorie de Hodge I and II*.

That same attitude, striving to erase each and every trace of my influence to the extent that it was possible (or even beyond!) can also be found in the work (already mentioned in note 47) which he wrote in collaboration with Mumford, on Mumford-Deligne compactification of moduli multiplicities. (This work also came out before my departure.) This paper uses a technique which transports topological results over \mathbb{C} (proven using transcendental methods) to results in characteristic $p > 0$ - I had introduced these results at the end of the 1950s for the theory of the étale fundamental group. Ever since the early 1960s I had been suggesting that this method could be used to prove the connectedness of moduli varieties in every characteristic³². This idea nonetheless presented technical difficulties which stopped Mumford, which were elegantly overcome in their paper via the introduction of moduli **multiplicities** along with a “compactification” of the latter possessing perfect properties. The very idea of moduli multiplicities can be found at least “between the lines” in my Teichmüller exposés at the Cartan seminar, produced at a time when the language of sites and topoi didn’t exist. Deligne’s very language “algebraic stack”, at a time when there was an entire language of sites, topoi, multiplicities, tailor-made to describe this very kind of situation, shows rather clearly, in hindsight, and in light of much more crude operations (at a later time) his intension to obscure the origin of some of the main ideas used in this brilliant paper. It is surely the same attitude (as I anticipate for the first time in the note “Refusal of an inheritance - or the price of contradiction” n°47) which had a “chainsaw effect”, excluding any further

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ref

moduli stacks

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³¹(*)

³²

reflection on moduli multiplicities, objects which appear to me as being some of the most beautiful and fundamental of all “concrete” mathematical objects discovered to this day.

I should signal in passing, that the arguments which I had introduced at the end of the 1950s enable one (via Mumford-Deligne compactification) not only to prove the connectedness of moduli multiplicities in every characteristic, but also to determine their fundamental group prime to p , as being the “profinite compactification prime to p ” of the ordinary Teichmüller group.

The ascension

63' (May 10) With the further hindsight of three weeks' time, I now realize that my desire to show “understanding” towards this “entirely natural” intention to keep his distance was in fact hinting at a lack of clarity, as well as a complacency towards my young and brilliant friend. If I had trusted my perceptive abilities rather than putting on blinders and lying to myself by means of vague clichés parading as “understanding” or even “generosity”, (“I am not going to criticize him just because he doesn't put my name under the spotlight...”), I would have realized then what I see now, sixteen years after the fact. I could have called out his lack of probity towards the reader, myself, and him. Seeing things simply and unafraid to call them by their name, I would have been able to express myself plainly, as I am doing now, leaving to my friend the opportunity to learn a lesson - at the very least he would have understood that, even with his unique abilities, his elders (or at least one of them) expected him to work with the same level of integrity as they did. I thus realize that on this occasion, situated prior to my departure from the mathematical scene, at a time when I was still “in the game” and in possession of a certain moral ascendancy over my young friend, I did not fulfill my responsibility to him due to the **laxity**.³³ This impression was confirmed following the publication of “Théorie de Hodge II”, which was Deligne's thesis work in which he makes no reference to motives or myself. It is true that already at that time, mathematics as well as my friend's person, appeared to me as distant, as if separated from me by a mist!

In light of the my friend's evolution as I see it, both spiritually and mathematically (two aspects which are tightly linked), I realized that upon meeting him and being impressed by his intellectual abilities, his acute vision, and his rapid comprehension of mathematics, I perceived no lack of maturity within him; nor could I anticipate (in what followed) the effects which his vertiginous social ascension would have on him, in the span of nearly four years, rising from the status of unknown student, to start of the mathematical world and permanent professor, possessing considerable privilege and power in an already prestigious institution. I do not regret facilitating and accelerating this ascent - but I do acknowledge that through my own lack of discernment and maturity, I mistakenly thought that I was doing him a “service”. It was not a “service”, for as long as my friend had not reached the term of the harvest which he had

set out for himself, with the help of my care-free assistance.

The ambiguity

63” (June 1) In the three weeks following my acknowledgement of a “laxity” (or the “complacency”, to use what has since become the more appropriate expression) within my relationship with my friend Pierre, I have had the occasion to realize more clearly a certain lack of rigour and complacency within myself. The latter appeared first and foremost with the context of my relationship with the one who I considered to be “one of a kind”, but they also appeared in relation to other mathematicians who viewed me as an elder. Namely, what I have been able to detect so far is a certain ambiguity, both on my end and on behalf of the student, in situations wherein the student appropriated ideas and methods which they had learned from me, or even a detailed maître d’oeuvre of one of their project without clearly indicating their source or even alluding to it. Situations of this sort were rather frequent during the 1960s, after my departure, and even up to the last few years. I believe that I already felt the ambiguity at a certain level in all of these situations, in the form of a lurking feeling of unease, never examined up until the past few days. What made me play along with such a complacency and led me to overcome this uneasiness without paying it too much attention, was the desire to **conform** to a certain self-image I held, which revolved around a so-called “generosity”. But true generosity does not come from a need to conform, or a desire to be (and appear, both to oneself and to others) “generous”. The repressed uneasiness was a clear sign that this “generosity” was artificial, that it was an **attitude**, and not the spontaneous and unqualified gifting which characterizes true generosity. p. 239

I can distinguish two components of a different origin within this uneasiness. One comes from the “boss”, the “me” who remained frustrated, because he was unable to win at both games, that is, to share the credit for a work in which he was involved in (a more or less large) part, while at the same time rising to the standards of a certain brand name, which involves (among many other things) the label of “generosity”. The other component comes from the “child”, that is the being within me that is not fooled by attitudes and facades, and who is candid enough to see what was unnatural about the situation³⁴(*) - unnatural for myself as well as for others. All in all, my “generosity” boiled down to playing a game where one person collects, as his own, ideas which come from another, thereby projecting an image of himself and of certain reality which we both pertinently know is false. The two of us are therefore stand in solidarity in what may be called trickery, wherein both parties benefit. This was at least a “trickery” according to the consensus which prevailed “in my day”, and which seem to still be professed as lip-service. I would surely not have played along, had the ideas involved come from someone other than myself and my “portége”³⁵(*) used them as if he had discovered them himself. Nonetheless, the fact that I p. 240

³⁴(*)(June 5)

³⁵(*)

gave my tacit agreement for ideas of mine to be presented as somebody else's does not change anything, in my eyes, about the nature of the situation - the only difference being that in this case there are two cheaters instead of just one. And even if we were to set aside this aspect of my person (namely, that I am myself participating in a scam, and therefore that my behavior is contrary to the very consensus which I pretend to uphold), it is clear that there is nothing generous about encouraging others to cheat (even if it only costs the person doing it - which is not even the case), nor about holding an ambiguous attitude towards a consensus, to which I was pretending to adhere whilst breaching it. True generosity should be well-meaning towards all, starting with the person admitting it, and the one on the receiving end. My ambiguous attitude, inducing or encouraging ambiguity in the recipient, and allowing me to pose as "generous" while the recipient should logically appear as borderline cheating (when in fact we are both engaged in fraud) is neither a blessing for myself nor for the recipient.

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One only needed to look at the situation for the evidence to make itself known, without needing to refer to past experience or some "life lesson". Yet, it is experience which eventually led me to this self-examination, making me discover evidence which I would have been just as able to find thirty years ago, even before the appearance of a student coming to learn a trade with me and to absorb a certain spirit or philosophy underlying the practice of this trade through his interactions with me. I have spoken about the "rigour" involved in the work itself at an earlier occasion, something which I believe I have upheld (see the section "Rigour and rigour"). But I am now realizing that outside of the "work" proper, I showed a lack of rigour, as expressed by the ambiguity and the complacency aforementioned. I don't recall any of my elders pointing out this ambiguity in me to me, even though (I think) they all had the same expectations for me as they did for themselves. Beyond ambiguity pertaining to a specific attitude, I can detect ambiguity in my very person, about which I speak more than once in the first part of . This ambiguity began resolving itself with my discovery of meditation in 1976, but certain aspects of this ambiguity, expressed in what have now become habitual attitudes and behaviors (notably in my relationships to my students) have probably persisted to this day.

Visibly, my ambiguity was favourably received by some of my students. What used to be done tacitly even became, or so it seems, a backdrop within the mores of the mathematical high society of today. Namely, to go fishing in troubled waters (with or without the consent of the "relevant party"), or even downright looting (legitimate as long as the partaker is part of the intangible elite), seem to have become so common, that nobody appears surprised anymore, and in fact everybody refrains from speaking about it. The "boss" within me would like to stand up, denounce, take offence - yet in so doing I would only be perpetuating this same ambiguity within myself which has already yielded such a prolific harvest.

The accomplice

63” (April 24)³⁶(*) While going through a private article which I just received from Mebkhout two days ago, I stumbled upon a reference to J.L. Verdier called “Catégories Dérivées, Etat 0” which appeared in SGA 4 $\frac{1}{2}$ (Lecture notes n°569 p. 262-311). I can be excused for having taken so long to become aware of this publication, having never had the honor of holding this book in my hands to this day, since neither Verdier nor Deligne (the author) deemed it useful to send me a copy, be it after the first print run, or afterwords. I ignore if either C. Chevalley or R. Godement, the other members of the jury, who attributed to J.L. Verdier the title of “docteur es sciences” on the basis of a 17 page introduction (still unpublished) had the chance yen years later to receive “L’état 0” (50 pages long this time around) of this rather one-of-a-kind thesis! I think I can remember once holding in my hands a serious foundational work of about one-hundred pages, which could have reasonably stood as a good Ph.D. thesis, and which roughly corresponded to the foundational work that I had proposed to Verdier around 1960 - with the caveat that it had already become clear by then, that the context of “triangulated categories” which he had developed so as to express the internal structure of derived categories) was insufficient.

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It goes without saying that my name is nowhere to be seen in this “Etat 0” of a thesis. One could wonder what I could have possibly had to do with this work: it is well-known that derived categories were introduced by Verdier to allow him to develop the “Poincaré-Verdier duality” of topological spaces, as well as the so-called “Serre-Verdier” duality of analytic spaces, before some unknown author ³⁷(*) developed a synthesis of the two for him, rightly called “Poincaré-Serre-Verdier duality” (the Unknown Author could not have been less recognized!). After all this, I only had to follow the motions and make the few necessary adjustments in order to develop Poincaré-Verdier duality, as well as Serre-Verdier duality in the particular settings of étale or coherent cohomology of schemes. . .

I have finally browsed through that SGA 4 $\frac{1}{2}$ (libraries can be useful!) ³⁸(**), where I am kindly included as coauthor, or rather as “collaborator” (sic) of Deligne (without deeming it useful to inform me of this fact, let alone consult me). This was visibly a precursor to the memorable “burial volume” which appeared five years later, and whose existence I just heard about a few days ago (see notes ??, and onwards, inspired by this event). But in any case, I did not need to hold this pre-burial volume in my hands, and thereby see for myself the evidence for this ghost-thesis whose name must not be spoken, in order to realize already a year ago, that the next stage of this “thesis” would never be written by anyone other than myself. This is why I set out to work on **Pursing Stacks** seventeen years ago, picking where my illustrious ex-student chose to leave off.

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³⁶(*)

³⁷(*)

³⁸(**)

The investiture

64 (April 25) I nonetheless found a copy of it on my desk yesterday at the university. It actually consists of two surveys written one year apart, the first in April (?) 1968, and the second in April 1969. There, I review, in the span of seventeen pages, fifteen projects that he had pursued during three years of scientific activity at the IHES. Among them is his work on the Ramanuyam conjecture, on the compactification of moduli sites, and on an extension of Hodge theory. The collection of projects reviewed in this survey (or even merely the ones I just named) is a testament to a prodigious creativity deployed with perfect ease, as if at play. Putting aside the proof of the Weil conjectures, which occurred in the wake of his first expedition into the unknown, it appears to me that the subsequent work gives only a pale image of the unique search for a young spirit gifted with exceptional means, and benefiting from equally exceptional conditions to his growth. Yet, it appears that something in these “exceptional conditions” nourished another force, disjoint from the impulse to understand, which eventually took over the latter, diverting and absorbing the initial impetus. And visibly this “something” was tied to my person. . . ³⁹(*)

This brief annotated survey (which I think I should include as an appendix to the present volume) seems interesting to me for more than one reason, including from the mathematical viewpoint (in that some of the projects reviewed therein remain unpublished to this day). At several points in the survey, I predict that certain projects, which Deligne had only sketched broadly, and for which he had addressed some of the crucial points would be developed by future students. These student never appeared, in light of the changes that took place thereafter in his relationship with mere mortals. ⁴⁰(**) Among the ideas that I review, the only one which to my knowledge was developed by somebody else (who would therefore appear to have been a student of Deligne) is the theory of cohomological descent, developed by Saint Donat in SGA 4 (that is, during the period of the initial impetus), a theory which has since become one of the most commonly used tools in the cohomological arsenal.

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The knot

65

Two turning points

66

Clean slate

67

³⁹(*)

⁴⁰(**)

Note 67₁

One of a kind

67'

The green light

68

The reversal

!68'

Squaring the circle

69

The funeral

70

The coffin

