

GEOMETRY

(Published)

A. Grothendieck

This edition is a collection of A. Grothendieck's published works on geometry reunited by Mateo Carmona. Remarks, comments, and corrections are welcome.

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Sur une note de Mattuck-Tate

1. Dans un travail récent [4], Mattuck et Tate déduisent l'inégalité fondamentale de A. Weil qui établit l'hypothèse de Riemann pour les corps de fonctions [7] comme conséquence facile du théorème de Riemann-Roch pour les surfaces. En essayant de comprendre la portée exacte de leur méthode, je suis tombé sur l'énoncé suivant, connu en fait depuis 1937 [2] [6] [1] (comme me l'a signalé J. P. Serre), mais apparemment peu connu et utilisé:

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2. Nous allons déduire sur X , nous désignerons par $l(D)$ la dimension de l'espace vectoriel des fonctions f sur X telles que $(f) \geq -D$ donc $l(D)$ ne dépend que de la classe de D . Rappelons l'inégalité de Riemann-Roch

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3. Ce qui précède n'utilisait pas à proprement parler la méthode de Mattuck-Tate (si ce n'est en utilisant l'inégalité de Riemann-Roch sur les surfaces). Nous allons indiquer maintenant comment la méthode de ces auteurs, convenablement généralisée, donne d'autres inégalités que celle de A. Weil. Nous nous appuyerons sur le

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Remarques. Le corollaire 1 devient faux si on ne fait pas l'hypothèse que $K/2$ est encore une classe de diviseurs. En effet, toutes les hypothèses sauf cette dernière sont vérifiées si X est une surface non singulière *rationnelle*. Or, à partir d'une telle surface, on construit facilement une surface birationnellement équivalente par éclatements successifs, dont l'index τ soit < 0 (contrairement à (3.7 ter)). En effet, on vérifie aisément que lorsqu'on fait éclater un point dans une surface non singulière projective, l'index diminue d'une unité. (Cette remarque, ainsi que l'interprétation de l'inégalité (3.7) à l'aide de l'index, m'a été signalée par J. P. Serre).

La disparité des énoncés qu'on déduit du théorème (3.2) est due au fait qu'il n'est pas relatif à un élément arbitraire de l'espace vectoriel E de Néron-Séveri introduit plus haut, mais à un élément du "lattice" provenant des diviseurs sur X . On notera d'ailleurs que dans le cas particulier où X est le produit des deux courbes C et C' , le théorème 3.2 ne contient rien de plus que l'inégalité de A. Weil.

The cohomology theory of abstract algebraic varieties

It is less than four years since cohomological methods (i.e. methods of Homological Algebra) were introduced into Algebraic geometry in Serre's fundamental paper [11], and it seems already certain that they are to overflow this part of mathematics in the coming years, from the foundations up to the most advanced parts. All we can do here is to sketch briefly some of the ideas and results. None of these have been published in their final form, but most of them originated in or were suggested by Serre's paper.

Let us first give an outline of the main topics of cohomological investigation in Algebraic geometry, as they appear at present. The need of a theory of cohomology for 'abstract' algebraic varieties was first emphasized by Weil, in order to be able to give a precise meaning to his celebrated conjectures in Diophantine geometry [20]. Therefore the initial aim was to find the '*Weil cohomology*' of an algebraic variety, which should have as coefficients something 'at least as good' as a field of characteristic 0, and have such formal properties (e.g. duality, Künneth formula) as to yield the analogue of Lefschetz's 'fixed-point formula'. Serre's general idea has been that the usual 'Zariski topology' of a variety (in which the closed sets are the algebraic subset) is a suitable one for applying methods of Algebraic Topology. His first approach was hoped to yield at least the right Betti numbers of a variety, it being evident from the start that it could not be considered as the Weil cohomology itself, as the coefficient field for cohomology was the ground field of a variety, and therefore not in general of characteristic 0. In fact, even the hope of getting the 'true' Betti numbers has failed, and so have other attempts of Serre's [12] to get Weil's cohomology by taking the cohomology of the variety with values, not in the sheaf of local rings themselves, but in the sheaves of Witt-vectors constructed on the latter. He gets in this way modules over the ring $W(k)$ of infinite Witt vectors on the ground field k , and $W(k)$ is a ring of characteristic 0 even if k is of characteristic $p \neq 0$. Unfortunately, modules thus obtained over $W(k)$ may be infinitely generated, even when the variety V is an abelian variety [13]. Although interesting relations must certainly exist between these cohomology groups and the 'true ones', it seems certain now that the Weil cohomology has to be defined by a completely different approach. Such an approach was recently suggested to me by the *connections between sheaf-theoretic cohomology and cohomology of Galois groups on the one hand, and the classification of unramified coverings of a variety on the other* (as explained quite unsystematically in Serre's tentative Mexico paper [12]), and by Serre's idea that a 'reasonable' algebraic

principal fiber space with structure group G , defined on a variety V , if it is not locally trivial, should become locally trivial on some covering of V *unramified* over a given point of V . This has been the starting point of a definition of the Weil cohomology (involving both ‘spatial’ and Galois cohomology), which seems to be the right one, and which gives clear suggestions how Weil’s conjectures may be attacked by the machinery of Homological algebra. As I have not begun these investigations seriously as yet, and as moreover this theory has a quite distinct flavor from the one of the theory of algebraic coherent sheaves which we shall now be concerned with, we shall not dwell any longer on Weil’s cohomology. Let us merely remark that the definition alluded to has already been the starting-point of a theory of cohomological dimension of fields, developed recently by Tate [18].

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Standard Conjectures on Algebraic Cycles

- 1 We state two conjectures on algebraic cycles, which arose from an attempt at understanding the conjectures of Weil on the ζ -functions of algebraic varieties. These are not really new, and they were worked out about three years ago independently by Bombieri and myself.

The first is an existence assertion for algebraic cycles (considerably weaker than the Tate conjectures), and is inspired by and formally analogous to Lefschetz's structure theorem on the cohomology of a smooth projective variety over the complex field.

The second is a statement of positivity, generalising Weil's well-known positivity theorem in the theory of abelian varieties. It is formally analogous to the famous Hodge inequalities, and is in fact a consequence of these in characteristic zero.

WHAT REMAINS TO BE PROVED OF WEIL'S CONJECTURES? Before stating our conjectures, let us recall what remains to be proved in respect of the Weil conjectures, when approached through ℓ -adic cohomology.

Let X/\mathbf{F}_q be a smooth irreducible projective variety of dimension n over the finite field \mathbf{F}_q with q elements, and ℓ a prime different from the characteristic. It has then been proved by M. Artin and myself that the Z -function of X can be expressed as

$$\begin{aligned} Z(t) &= \frac{L'(t)}{L(t)}, \\ L(t) &= \frac{L_0(t)L_2(t)\dots L_{2n}(t)}{L_1(t)L_3(t)\dots L_{2n-1}(t)}, \\ L_i(t) &= \frac{1}{P_i(t)}, \end{aligned}$$

where $P_i(t) = t^{\dim H^i(\bar{X})} Q_i(t^{-1})$, Q_i being the characteristic polynomial of the action of the Frobenius endomorphism of X on $H^i(\bar{X})$ (here H^i stands for the i^{th} ℓ -adic cohomology group and \bar{X} is deduced from X by base extension to the algebraic closure of \mathbf{F}_q). But it has not been proved so far that

- (a) the $P_i(t)$ have integral coefficients, independent of $\ell (\neq \text{char } \mathbf{F})$;
- (b) the eigenvalues of the Frobenius endomorphisms on $H^i(\bar{X})$, i.e., the reciprocals of the roots of $P_i(t)$, are of absolute value $q^{i/2}$.

Our first conjecture meets question (a). The first and second together would, by an idea essentially due to Serre [4], imply (b).

Introduction

- 2 From now on, we work with varieties over a ground field k which is algebraically closed and of arbitrary characteristic. Then (a) leads to the following question: If f is an endomorphism of a variety X/k and $\ell \neq \text{char } k$, f induces

$$f^i : H^i(X) \rightarrow H^i(X),$$

and each of these f^i has a characteristic polynomial. *Are the coefficients of these polynomials rational integers, and are they independent of ℓ ?* When X is smooth and proper of dimension n , the same question is meaningful when f is replaced by any cycle of dimension n in $X \times X$, considered as an algebraic correspondence.

In characteristic zero, one sees that this is so by using integral cohomology. If $\text{char } k > 0$, one feels certain that this is so, but this has not been proved so far.

Let us fix for simplicity an isomorphism

$$\ell^{\infty} k^* \simeq \mathbf{Q}_\ell / \mathbf{Z}_\ell \quad (\text{a heresy!}).$$

We then have a map

$$: \mathcal{F}^i(X) \otimes_{\mathbf{Z}} \mathbf{Q} \rightarrow H_\ell^{2i}(X)$$

which associates to an algebraic cycle its cohomology class. We denote by $C_\ell^i(X)$, and refer to its elements as *algebraic cohomology classes*.

A known result, due to Dwork-Faton, shows that for the integrality question (not to speak of the independence of the characteristic polynomial of ℓ), it suffices to prove that

$$f_i^N \in \frac{1}{m} \mathbf{Z} \quad \text{for every } N \geq 0,$$

where m is a fixed positive integer*. Now, the graph Γ_{f^N} in $X \times X$ of f^N defines a cohomology class on $X \times X$, and if the cohomology class Δ of the diagonal in $X \times X$ is written as

$$\Delta = \sum_0^n \pi_i$$

where π_i are the projections of Δ onto $H^i(X) \otimes H^{n-i}(X)$ for the canonical decomposition $H^n(X \times X) \simeq \sum_{i=0}^n H^i(X) \otimes H^{n-i}(X)$, a known calculation shows that

$$(f^N)_{H^i} = (-1)^i (\Gamma_{f^N}) \pi_i \in H^{4n}(X \times X) \approx \mathbf{Q}_\ell.$$

Assume that the π_i are algebraic. Then $\pi_i = \frac{1}{m} (\prod_i)$, where \prod_i is an algebraic cycle, hence

$$(f^N)_{H^i} = (-1)^i (\prod_i \Gamma_{f^N}) \in \frac{1}{m} \mathbf{Z}$$

and we are through.

WEAK FORM OF CONJECTURE 1. ($C(X)$): The elements π_i^ℓ are algebraic, (and come from an element of $\mathcal{F}^i(X) \otimes_{\mathbf{Z}} \mathbf{Q}$, which is independent of ℓ).

N.B.

A weak form of conjecture 1

*This was pointed out to me by S. Kleimann.

1. The statement in parenthesis is needed to establish the independence of P_i on ℓ .
2. If $C(X)$ and $C(Y)$ hold, $C(X \times Y)$ holds, and more generally, the Künneth components of any algebraic cohomology class on $X \times Y$ are algebraic.

- 3 Let X be smooth and projective, and $\xi \in H^2(X)$ the class of a hyperplane section. Then we have a homomorphism

The conjecture 1 (of Lefschetz type)

$$(*) \quad \cup \xi^{n-i} : H^i(X) \rightarrow H^{2n-i}(X) \quad (i \leq n).$$

It is expected (and has been established by Lefschetz [2], {[5]} over the complex field by transcendental methods) that this is an isomorphism for all characteristics. For $i = 2j$, we have the commutative square

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Our conjecture is then: $(A(X))$:

- (a) $(*)$ is always an isomorphism (the mild form);
- (b) if $i = 2j$. $(*)$ induces an isomorphism (or equivalently, an epimorphism) $C^j(X) \rightarrow C^{n-j}(X)$.

N.B. If $C^j(X)$ is assumed to be finite dimensional, (b) is equivalent to the assertion that $\dim C^{n-j}(X) \leq \dim C^j(X)$ (which in particular implies the equality of these dimensions in view of (a)).

An equivalent formulation of the above conjecture (for all varieties X as above) is the following.

$(B(X))$: The Λ -operation (c.f. [5]) of Hodge theory is algebraic.

By this, we mean that there is an algebraic cohomology class λ in $H^*(X \times X)$ such that the map $\Lambda : H^*(X) \rightarrow H^*(X)$ is got by lifting a class from X to $X \times X$ by the first projection, cupping with λ and taking the image in $H^*(X)$ by the Gysin homomorphism associated to the second projection

Note that $B(X) \Rightarrow A(X)$, since the algebraicity of λ implies that of λ^{n-i} , and λ^{n-i} provides an inverse to $\cup \xi^{n-i} : H^i(X) \rightarrow H^{2n-i}(X)$. On the other hand, it is easy to show that $A(X \times X) \Rightarrow B(X)$ and this proves the equivalence of conjectures A and B .

The conjecture seems to be most amenable in the form of B . Note that $B(X)$ is stable for products, hyperplane sections and specialisations. In particular, since it holds for projective spaces, it is also true for smooth varieties which are complete intersections in some projective space. (As a consequence, we deduce for such varieties the wished-for integrality theorem for the Z -function!). It is also verified for Grassmannians, and for abelian varieties (Liebermann [3]).

I have an idea of a possible approach to Conjecture B , which relies in turn on certain unsolved geometric questions, and which should be settled in any case.

Finally, we have the implication $B(X) \Rightarrow C(X)$ (first part), since the π_i can be expressed as polynomials with coefficients in \mathbf{Q} of λ and $L = \cup \xi$. To get the whole of $C(X)$, one should naturally assume further that there is an element of $\mathcal{F}(X \times X) \otimes_{\mathbf{Z}} \mathbf{Q}$ which gives λ for every ℓ .

- 4 For any $i \leq n$, let $P^i(X)$ be the ‘primitive part’ of $H^i(X)$, that is, the kernel of $\cup \xi^{n-i+1} : H^i(X) \rightarrow H^{2n-i+2}(X)$, and put $C_{pr}^j(X) = P^{2j} \cap C^j(X)$. On $C^{\otimes}_{pr}(X)$, we have a \mathbf{Q} -valued symmetric bilinear form given by

$$(x, y) \rightarrow (-1)^j K(xy \xi^{n-2j})$$

where K stands for the isomorphism $H^{2n}(X) \simeq \mathbf{Q}_\ell$. Our conjecture is then that

((X)): *The above form is positive definite.*

One is easily reduced to the case when $\dim X = 2m$ is even, and $j = m$.
REMARKS.

- (1) In characteristic zero, this follows readily from Hodge theory [5].
- (2) $B(X)$ and $Hdg(X \times X)$ imply, by certain arguments of Weil and Serre, the following: if f is an endomorphism of X such that $f^*(\xi) = q\xi$ for some $q \in \mathbf{Q}$ (which is necessarily > 0), then the eigenvalues of $f_{H^i(X)}$ are algebraic integers of absolute value $q^{i/2}$. Thus, this implies all of Weil’s conjectures.
- (3) The conjecture $Hdg(X)$ together with $A(X)(a)$ (the Lefschetz conjecture in cohomology) implies that numerical equivalence of cycles is the same as cohomological equivalence for any ℓ -adic cohomology if and only if $A(X)$ holds.
- (4) In view of (3), $B(X)$ and $Hdg(X)$ imply that numerical equivalence of cycles coincides with \mathbf{Q}_ℓ -equivalence for any ℓ . Further the natural map

$$Z^i(X) \otimes_{\mathbf{Z}} \mathbf{Q}_\ell \rightarrow H_\ell^i(X)$$

is a monomorphism, and in particular, we have

$$\dim_{\mathbf{Q}} C^i(X) \leq \dim_{\mathbf{Q}_\ell} H_\ell^i(X).$$

Note that for the deduction of this, we do not make use of the positivity of the form considered in (X), but only the fact that it is non-degenerate.

Another consequence of $Hdg(X)$ and $B(X)$ is that the stronger version of $B(X)$, viz. that λ comes from an algebraic cycle with rational coefficients *independent of ℓ* , holds.

- 5 The proof of the two standard conjectures would yield results going considerably further than Weil’s conjectures. They would form the basis of the so-called “theory of motives” which is a systematic theory of “arithmetic properties” of algebraic varieties, as embodied in their groups of classes of cycles for numerical equivalence. We have at present only a very small part of this theory in dimension one, as contained in the theory of abelian varieties.

Alongside the problem of resolution of singularities, the proof of the standard conjectures seems to me to be the most urgent task in algebraic geometry.

Conjecture 2 (of Hodge type)

Conclusions

Esquisse d'un Programme

- 1 Comme la conjoncture actuelle rend de plus en plus illusoire pour moi les perspectives d'un enseignement de recherche à l'Université, je me suis résolu à demander mon admission au CNRS, pour pouvoir consacrer mon énergie à développer es travaux et perspectives dont il devient clair qu'il ne se trouvera aucun élève (ni même, semble-t-il, aucun congénère mathématicien) pour les développer à ma place.

En guise de document "Titres et Travaux", on trouvera à la suite de ce texte la reproduction intégrale d'une esquisse, par thèmes, de ce que je considérais comme mes principales contributions mathématiques au moment d'écrire ce rapport, en 1972. Il contient également une liste d'articles publiés à cette date. J'ai cessé toute publication d'articles scientifiques depuis 1970. Dans les lignes qui suivent, je me propose de donner un aperçu au moins sur quelques thèmes principaux de mes réflexions mathématiques depuis lors. Ces réflexions se sont matérialisées au cours des années en deux volumineux cartons de notes manuscrites, difficilement déchiffrables sans doute à tout autre qu'à moi-même, et qui, après des stades de déchantations successives, attendent leur heure peut-être pour une rédaction d'ensemble tout au moins provisoire, à l'intention de la communauté mathématique. Le terme "rédaction" ici est quelque peu impropre, alors qu'il s'agit bien plus de développer des idées et visions multiples amorcées au cours de ces douze dernières années, en les précisant et les approfondissant, avec tous les rebondissements imprévus qui constamment accompagnent ce genre de travail – un travail de découverte donc, et non de compilation de notes pieusement accumulées. Et je compte bien, dans l'écriture des "Réflexions Mathématiques" commencée depuis février 1983, laisser apparaître clairement au fil des pages la démarche de la pensée qui sonde et qui découvre, en tâtonnant dans la pénombre bien souvent, avec des trouées de lumière subites quand quelque tenace image fausse, ou simplement inadéquate, se trouve enfin débusquée et mise à jour, et que les choses qui semblaient de guingois se mettent en place, dans l'harmonie mutuelle qui leur est propre.

Quoi qu'il en soit, l'esquisse qui suit de quelques thèmes de réflexions des dernières dix ou douze années, tiendra lieu en même temps d'esquisse de programme de travail pour les années qui viennent, que je compte consacrer au développement de ces thèmes, ou au moins de certains d'entre eux. Elle est destinée, d'une part aux collègues du Comité National appelés à statuer sur ma demande, d'autre part à quelques autres collègues, anciens élèves, amis, dans l'éventualité où certaines des idées esquissées ici pourraient intéresser l'un d'entre eux.

[Schneps and Lochak (1997)]

Envoi

2	<i>Un jeu de “Lego-Teichmüller” et le groupe de Galois de $\overline{\mathbb{Q}}$ sur \mathbb{Q}</i>
3	
4	<i>Corps de nombres associés à un dessin d’enfant</i>
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	<i>Épilogue</i>

Sketch of a Programme

- 1 As the present situation makes the prospect of teaching at the research level at the University seem more and more illusory, I have resolved to apply for admission to the CNRS, in order to devote my energy to the development of projects and perspectives for which it is becoming clear that no student (nor even, it seems, any mathematical colleague) will be found to develop them in my stead.

Preface

In the role of the document “Titles and Articles”, one can find after this text the complete reproduction of a sketch, by themes, of what I considered to be my principal mathematical contributions at the time of writing that report, in 1972. It also contains a list of articles published at that date. I ceased all publication of scientific articles in 1970. In the following lines, I propose to give a view of at least some of the principal themes of my mathematical reflections since then. These reflections materialised over the years in the form of two voluminous boxes of handwritten notes, doubtless difficult to decipher for anyone but myself, and which, after several successive stages of settling, are perhaps waiting for their moment to be written up together at least in a temporary fashion, for the benefit of the mathematical community. The term “written up” is somewhat incorrect here, since in fact it is much more a question of developing the ideas and the multiple visions begun during these last twelve years, to make them more precise and deeper, with all the unexpected rebounds which constantly accompany this kind of work – a work of discovery, thus, and not of compilation of piously accumulated notes. And in writing the “Mathematical Reflections”, begun since February 1983, I do intend throughout its pages to clearly reveal the process of thought, which feels and discovers, often blindly in the shadows, with sudden flashes of light when some tenacious false or simply inadequate image is finally shown for what it is, and things which seemed all crooked fall into place, with that mutual harmony which is their own.

In any case, the following sketch of some themes of reflection from the last ten or twelve years will also serve as a sketch of my programme of work for the coming years, which I intend to devote to the development of these themes, or at least some of them. It is intended on the one hand for my colleagues of the National Committee whose job it is to decide the fate of my application, and on the other hand for some other colleagues, former students, friends, in the possibility that some of the ideas sketched here might interest one of them.

- 2 The demands of university teaching, addressed to students (including those said to be “advanced”) with a modest (and frequently less than modest) mathematical baggage, led me to a Draconian renewal of the themes of reflection I proposed to my students, and gradually to myself as well. It seemed important to me to start from an intuitive baggage common to everyone, independent of any technical language used to express it, and anterior to any such language – it turned out that the geometric and topological intuition of shapes, particularly two-dimensional shapes, formed such a common ground. This consists of themes which can be grouped under the general name of “topology of surfaces” or “geometry of surfaces”, it being understood in this last expression that the main emphasis is on the topological properties of the surfaces, or the combinatorial aspects which form the most down-to-earth technical expression of them, and not on the differential, conformal, Riemannian, holomorphic aspects, and (from there) on to “complex algebraic curves”. Once this last step is taken, however, algebraic geometry (my former love!) suddenly bursts forth once again, and this via the objects which we can consider as the basic building blocks for all other algebraic varieties. Whereas in my research before 1970, my attention was systematically directed towards objects of maximal generality, in order to uncover a general language adequate for the world of algebraic geometry, and I never restricted myself to algebraic curves except when strictly necessary (notably in étale cohomology), preferring to develop “pass-key” techniques and statements valid in all dimensions and in every place (I mean, over all base schemes, or even base ringed topoi...), here I was brought back, via objects so simple that a child learns them while playing, to the beginnings and origins of algebraic geometry, familiar to Riemann and his followers!

Since around 1975, it is thus the geometry of (real) surfaces, and starting in 1977 the links between questions of geometry of surfaces and the algebraic geometry of algebraic curves defined over fields such as \mathbf{C} , \mathbf{R} or extensions of \mathbf{Q} of finite type, which were my principal source of inspiration and my constant guiding thread. It is with surprise and wonderment that over the years I discovered (or rather, doubtless, rediscovered) the prodigious, truly inexhaustible richness, the unsuspected depth of this theme, apparently so anodine. I believe I feel a central sensitive point there, a privileged point of convergence of the principal currents of mathematical ideas, and also of the principal structures and visions of things which they express, from the most specific (such as the rings \mathbf{Z} , \mathbf{Q} , $\overline{\mathbf{Q}}$, \mathbf{R} , \mathbf{C} or the group (2) over one of these rings, or general reductive algebraic groups) to the most “abstract”, such as the algebraic “multiplicities”, complex analytic or real analytic. (These are naturally introduced when systematically studying “moduli varieties” for the geometric objects considered, and if we want

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A game of “Lego-Teichmüller” and the Galois group $\overline{\mathbf{Q}}$ over \mathbf{Q}

- 3 Instead of following (as I meant to) a rigorous thematic order, I let myself be carried away by my predilection for a particularly rich and burning theme, to which I intend to devote myself prioritarilly for some time, starting at the beginning of the academic year 84/85. Thus I will take the thematic description up again where I left it, at the very beginning of the preceding paragraph.

My interest in topological surfaces began to appear in 1974, when I proposed to Yves Ladegaillerie the theme of the isotopic study of embeddings of a topological 1-complex into a compact surface. Over the two following years, this study led him to a remarkable isotopy theorem, giving a complete algebraic description of the isotopy classes of embeddings of such 1-complexes, or compact surfaces with boundary, in a compact oriented surface, in terms of certain very simple combinatorial invariants, and the fundamental groups of the protagonists. This theorem, which should be easily generalisable to embeddings of any compact space (triangulable to simplify) in a compact oriented surface, gives as easy corollaries several deep classical results in the theory of surfaces, and in particular Baer's isotopy theorem. It will finally be published, separately from the rest (and ten years later, seeing the difficulty of the times...), in *Topology*. In the work of Ladegaillerie there is also a purely algebraic description, in terms of fundamental groups, of the "isotopic" category of compact surfaces X , equipped with a topological 1-complex K embedded in X . This description, which had the misfortune to run counter to "today's taste" and because of this appears to be unpublishable, nevertheless served (and still serves) as a precious guide in my later reflections, particularly in the context of absolute algebraic geometry in characteristic zero.

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- 4 From the very start of my reflection on 2-dimensional maps, I was most particularly interested by the "regular" maps, those whose automorphism group acts transitively (and consequently, simply transitively) on the set of flags. In the oriented case and in terms of the algebraic-geometric interpretation given in the preceding paragraph, it is these maps which correspond to Galois coverings of the projective line.

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- 5 I would like to say a few words now about some topological considerations which have made me understand the necessity of new foundations for "geometric" topology, in a direction quite different from the notion of topos, and actually independent of the needs of so-called "abstract" algebraic geometry (over general base fields and rings). The problem I started from, which already began to intrigue me some fifteen years ago, was that of defining a theory of "dévissage" for stratified structures, in order to rebuild them, via a canonical process, out of "building blocks" canonically deduced from the original structure. Probably the main example which had led me to that question was that of the canonical stratification of a singular algebraic variety (or a complex or real singular space) through the decreasing sequence of its successive singular loci. But I probably had the premonition of the ubiquity of stratified structures in practically all domains of geometry (which surely others had seen clearly a long time before). Since then, I have seen such structures appear, in particular, in any situation where "moduli" are

Number fields associated to a child's drawing

Regular polyhedra over finite fields

Denunciation of so-called "general" topology, and heuristic reflections towards a so-called "tame" topology

involved for geometric objects which may undergo not only continuous variations, but also “degeneration” (or “specialisation”) phenomena – the strata corresponding then to the various “levels of singularity” (or to the associated combinatorial types) for the objects in question. The compactified modular multiplicities $\widehat{M}_{g,\nu}$ of Mumford-Deligne for the stable algebraic curves of type (g, ν) provide a typical and particularly inspiring example, which played an important motivating role when I returned to my reflection about stratified structures, from December 1981 to January 1982. Two-dimensional geometry provides many other examples of such modular stratified structures, which all (if not using rigidification) appear as “multiplicities” rather than as spaces or manifolds in the usual sense (as the points of these multiplicities may have non-trivial automorphism groups). Among the objects of two-dimensional geometry which give rise to such modular stratified structures in arbitrary dimensions, or even infinite dimension, I would list polygons (Euclidean, spherical or hyperbolic), systems of straight lines in a plane (say projective), systems of “pseudo straight lines” in a projective topological plane, or more general immersed curves with normal crossings, in a given (say compact) surface.

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- 6 One of the most interesting foundational theorems of (tame) topology which should be developed would be a theorem of “dévissage” (again!) of a proper tame map of tame spaces

$$f : X \rightarrow Y,$$

via a

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“Differentiable theories” (à la Nash)
and “tame theories”

- 7 Since the month of March last year, so nearly a year ago, the greater part of my energy has been devoted to a work of reflection on the *foundations of non-commutative (co)homological algebra*, or what is the same, after all, of *homotopical algebra*. These reflections have taken the concrete form of a voluminous stack of typed notes, destined to form the first volume (now being finished) of a work in two volumes to be published by Hermann, under the overall title “*Pursuing Stacks*”. I now foresee (after successive extensions of the initial project) that the manuscript of the whole of the two volumes, which I hope to finish definitively in the course of this year, will be about 1500 typed pages in length. These two volumes are moreover for me the first in a vaster series, under the overall title “*Mathematical Reflections*”, in which I intend to develop some of the themes sketched in the present report.

Since I am speaking here of work which is actually now being written up and is even almost finished, the first volume of which will doubtless appear this year and will contain a detailed introduction, it is undoubtedly less interesting for me to develop this theme of reflection here, and I will content myself with speaking of it only very briefly. This work seems to me to be somewhat marginal with respect to the themes I sketched before, and does not (it seems to me) represent a real renewal of viewpoint or approach with respect to my interests and my mathematical vision of before 1970. If I suddenly resolved to do it, it is almost out of desperation, for nearly twenty years have gone by since certain visibly

Pursuing Stacks

fundamental questions, which were ripe to be thoroughly investigated, without anyone seeing them or taking the trouble to fathom them. Still today, the basic structures which occur in the homotopical point of view in topology are not understood, and to my knowledge, after the work of Verdier, Giraud and Illusie on this theme (which are so many beginnings still waiting for continuations...) there has been no effort in this direction. I should probably make an exception for the axiomatisation work done by Quillen on the notion of a category of models, at the end of the sixties, and taken up in various forms by various authors. At that time, and still now, this work seduced me and taught me a great deal, even while going in quite a different direction from the one which was and still is close to my heart. Certainly, it introduces derived categories in various non-commutative contexts, but without entering into the question of the essential internal structures of such a category, also left open in the commutative case by Verdier, and after him by Illusie. Similarly, the question of putting one's finger on the natural "coefficients" for a non-commutative cohomological formalism, beyond the stacks (which should be called 1-stacks) studied in the book by Giraud, remained open – or rather, the rich and precise intuitions concerning it, taken from the numerous examples coming in particular from algebraic geometry, are still waiting for a precise and supple language to give them form.

I returned to certain aspects of these foundational questions in 1975, on the occasion (I seem to remember) of a correspondence with Larry Breen (two letters from this correspondence will be reproduced as an appendix to Chap. 1 of volume 1, "History of Models", of Pursuing Stacks). At that moment the intuition appeared that ∞ -groupoids should constitute particularly adequate models for homotopy types, the n -groupoids corresponding to *truncated* homotopy types (with $\pi_i = 0$ pour $i > n$). This same intuition, via very different routes, was discovered by Ronnie Brown and some of his students in Bangor, but using a rather restrictive notion of ∞ -groupoid (which, among the 1-connected homotopy types, model only products of Eilenberg-Mac Lane spaces). Stimulated by a rather haphazard correspondence with Ronnie Brown, I finally began this reflection, starting with an attempt to define a wider notion of ∞ -groupoid (later rebaptised stack in ∞ -groupoids or simply "stack", the implication being: over the 1-point topos), and which, from one thing to another, led me to Pursuing Stacks. The volume "History of Models" is actually a completely unintended digression with respect to the initial project (the famous stacks being temporarily forgotten, and supposed to reappear only around page 1000...).

This work is not completely isolated with respect to my more recent interests. For example, my reflection on the modular multiplicities $\widehat{M}_{g,n}$, and their stratified structure renewed the reflection on a theorem of van Kampen in dimension > 1 (also one of the preferred themes of the group in Bangor), and perhaps also contributed to preparing the ground for the more important work of the following year. This also links up from time to time with a reflection dating from the same year 1975 (or the following year) on a "De Rham complex with divided powers", which was the subject of my last public lecture, at the IHES in 1976; I lent the manuscript of it to I don't remember whom after the talk, and it is now lost. It was at the moment of this reflection that the intuition of a "schematisation" of homotopy types germinated, and seven years later

I am trying to make it precise in a (particularly hypothetical) chapter of the History of Models.

The work of reflection undertaken in Pursuing Stacks is a little like a debt which I am paying towards a scientific past where, for about fifteen years (from 1955 to 1970), the development of cohomological tools was the constant Leitmotiv in my foundational work on algebraic geometry. If in this renewal of my interest in this theme, it has taken on unexpected dimensions, it is however not out of pity for a past, but because of the numerous unexpected phenomena which ceaselessly appear and unceremoniously shatter the previously laid plans and projects – rather like in the thousand and one nights, where one awaits with bated breath through twenty other tales the final end of the first.

- 8 Up to now I have spoken very little of the more down-to-earth reflections on two-dimensional topological geometry, directly associated to my activities of teaching and “directing research”. Several times, I saw opening before me vast and rich fields ripe for the harvest, without ever succeeding in communicating this vision, and the spark which accompanies it, to one of my students, and having it open out into a more or less long-term common exploration. Each time up through today, after a few days or weeks of investigating where I, as scout, discovered riches at first unsuspected, the voyage suddenly stopped, upon its becoming clear that I would be pursuing it alone. Stronger interests then took precedence over a voyage which at that point appeared more as a digression or even a dispersion, than a common adventure.

Digressions on 2-dimensional geometry

One of these themes was that of planar polygons, centred around the modular varieties which can be associated to them. One of the surprises here was the irruption of algebraic geometry in a context which had seemed to me quite distant. This kind of surprise, linked to the omnipresence of algebraic geometry in plain geometry, occurred several times.

- 9 The occasion appears to be auspicious for a brief assessment of my teaching activity since 1970, that is, since it has taken place in a university. This contact with a very different reality taught me many things, of a completely different order than simply pedagogic or scientific. Here is not the place to dwell on this subject. I also mentioned at the beginning of this report the role which this change of professional milieu played in the renewal of my approach to mathematics, and that of my centres of interest in mathematics. If I pursue this assessment of my teaching activity on the research level, I come to the conclusion of a clear and solid failure. In the more than ten years that this activity has taken place, year after year in the same university, I was never at any moment able to suscite a place where “something happened” – where something “passed”, even among the smallest group of people, linked together by a common adventure. Twice, it is true, around the years 1974 to 1976, I had the pleasure and the privilege of awakening a student to a work of some consequence, pursued with enthusiasm: Yves Ladegaillerie in the work mentioned earlier (par. 3) on questions of isotopy in dimension 2, and Carlos Contou-Carrère (whose mathematical passion did not await a meeting with myself to blossom) an unpublished work on the local and global Jacobians over general base schemes (of which one part was announced in a note in the CR). Apart from these two cases, my role has

Assessment of a teaching activity

been limited throughout these ten years to somehow or other conveying the rudiments of the mathematician's trade to about twenty students on the research level, or at least to those among them who persevered with me, reputed to be more demanding than others, long enough to arrive at a first acceptable work written black on white (and even, sometimes, at something better than acceptable and more than just one, done with pleasure and worked out through to the end). Given the circumstances, among the rare people who persevered, even rarer are those who will have the chance of carrying on the trade, and thus, while earning their bread, learning it ever more deeply.

- 10 Since last year, I feel that as regards my teaching activity at the university, I have learned everything I have to learn and taught everything I can teach there, and that it has ceased to be really useful, to myself and to others. To insist on continuing it under these circumstances would appear to me to be a waste both of human resources and of public funds. This is why I have applied for a position in the CNRS (which I left in 1959 as freshly named director of research, to enter the IHES). I know moreover that the employment situation is tight in the CNRS as everywhere else, that the result of my request is doubtful, and that if a position were to be attributed to me, it would be at the expense of a younger researcher who would remain without a position. But it is also true that it would free my position at the USTL to the benefit of someone else. This is why I do not scruple to make this request, and to renew it if it is not accepted this year.

In any case, this application will have been the occasion for me to write this sketch of a programme, which otherwise would probably never have seen the light of day. I have tried to be brief without being sibylline and also, afterwards, to make it easier reading by the addition of a summary. If in spite of this it still appears rather long for the circumstances, I beg to be excused. It seems short to me for its content, knowing that ten years of work would not be too much to explore even the least of the themes sketched here through to the end (assuming that there is an "end"...), and one hundred years would be little for the richest among them!

Behind the apparent disparity of the themes evoked here, an attentive reader will perceive as I do a profound unity. This manifests itself particularly by a common source of inspiration, namely the geometry of surfaces, present in all of these themes, and most often front and centre. This source, with respect to my mathematical "past", represents a renewal, but certainly not a rupture. Rather, it indicates the path to a new approach to the still mysterious reality of "*motives*", which fascinated me more than any other in the last years of this past*. This fascination has certainly not vanished, rather it is a part of the fascination with the most burning of all the themes evoked above. But today I am no longer, as I used to be, the voluntary prisoner of interminable tasks, which so often prevented me from springing into the unknown, mathematical or not. The time of tasks is over for me. If age has brought me something, it is lightness.

Epilogue

*On this subject, see my commentaries in the "Thematic Sketch" of 1972 attached to the present report, in the last section "motivic digressions", (loc. cit. pages 17-18)

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