GROTHENDIECK-SERRE
CORRESPONDENCE

GROTHENDIECK-SERRE CORRESPONDENCE

Résumé. — This volume contains a large part of the mathematical correspondence between A.Grothendieck and J-P.Serre. This correspondence forms a vivid introduction to the algebraic geometry of the years 55-65 (a rich period if ever there was one). The readers will discover, for instance, the genesis of some of Grothendieck's ideas: Sheaf cohomology (Tôhoku), Schemes, Riemann-Roch, Fundamental Group, Existence Theorems and Motives... They also will get an idea of the mathematical atmosphere of this time (Bourbaki, seminars, Paris, Harvard, Princeton, Algeria war, ...).

***Abstract* (Correspondance Grothendieck-Serre)**

Ce volume contient une grande partie de la correspondance mathématique entre A.Grothendieck et J-P.Serre. Cette correspondance constitue une introduction particulièrement vivante à la géométrie algébrique des années 1955–1965 (période faste s'il en fut). Le lecteur y découvrira, en particulier, la genèse de certaines des idées de Grothendieck: cohomologie des faisceaux (Tôhoku), schémas, Riemann-Roch, groupe fondamental, théorèmes d'existence, motifs... Il se fera aussi une idée de l'atmosphère mathématique de cette époque (Bourbaki, Paris, Harvard, Princeton, guerre d'Algérie, ...).

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FOREWORD

A large part of my correspondence with Grothendieck between 1955 and 1970 is reproduced in the present volume of the series "Mathematical Documents".

I have chosen those of our letters which seemed to me most likely to be of interest to the reader, either from a purely mathematical point of view or from a historical point of view.

Of course, this choice is far from representing all the questions I discussed with Grothendieck over these fifteen years: when we were both in Paris, our usual means of communication was the telephone. The letters reflect rather the periods when we were separated: one of us being in Princeton or Harvard and the other being in Paris.

I have also included several of the letters that we exchanged between 1984 and 1987, at the moment when *Récoltes et Semailles* was being written.

The only changes which have been made to the original texts are the following:

       — suppression of abbreviations, such as "com." for "commutative" g;

       — correction of both spelling mistakes and obvious mis-typings.

       — suppression of certain personal passages [1] which have been replaced by suspension points between brackets [...].

On the other hand, wrong statements have been preserved as they were. They are corrected (insofar as it was possible) in the *Notes* placed at the end of the volume. These notes also give some more recent results. They are indicated in the text by reference numbers in the margin.

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[1] For curiosity's sake, here is one of the suppressed passages, on the subject of a mathematician who I will call X: "... the horrible prose of X, grand master wizard of the den of horrors, who of course is careful to keep from writing a clear little paper, in which the helpful claims would appear in a usable form..." It will be understood why I wished to spare X from reading this passage.

Jean-Pierre Serre, Paris, December 2000

This volume would never have seen the day without the help of a large number of people. Jean Malgoire authorized publication of this correspondence in Grothendieck's name. A (small) army took on the work of typing the letters in TeX which in certain cases was no light task. Once the typing was finished, the readers pinpointed a certain number of points deserving commentary, which contributed to filling out the notes written by Serre.

When the volume was reasonably advanced, we had to think about publication. Since none of the S.M.F.'s existing series had room for a volume of this kind, it was decided to create a new series, "Mathematical Documents", whose raison d'être would be the publication of historico-mathematical texts.

I would like to thank all the people mentioned above for the enthusiasm with which they participated in this undertaking, and I hope the reader will have as much pleasure consulting this correspondence as I had editing it.

Pierre Colmez, Paris, January 2001

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Facsimile of Grothendieck's letter of 1.28.55, page 1

CORRESPONDENCE

January 28, 1955 Alexandre Grothendieck

J.-P. Serre : Grothendieck was in Lawrence (Kansas), where he had been invited (by N.Aronszajn, I believe) because of his work on Topological Vector Spaces. He had decided to change subjects and move towards topology and functions of complex variables, which had led him to algebraic geometry (as it had led me, a year or two earlier).

Dear Serre,

A really annoying thing has happened: as I could not carry all my papers on the plane, I sent part of them in two packages, which were posted together the day I left. But only one of them has arrived, a week ago, and by now it is unlikely that the other will ever arrive. It contained (among other things) those lectures from the Schwartz, Cartan and Cartier seminars that existed at the time, your paper on algebraic sheaves and the Kodaira- Spencer papers. I am very upset about this, as it will probably not be possible to replace the last two for some time, and I had not even had time to glance at Kodaira-Spencer. In any case, send me the complete set of all the lectures from all three seminars, if there are any left. I hope Barros remembers to bring you the Schwartz lectures. Can you also tell Spencer I have not received any of the papers he wanted to send me from Princeton?

Here I have practically all my time to myself; I am giving a few talks on Malgrange's thesis, but this hardly takes any time. I will probably bother you shortly with various technical questions; I have started reading Thom's "varieties" J.-P. Serre : "Thom's 'varieties' ". This is a reference to:

R. Thom, *Quelques propriétés globales des variétés différentiables*, Comment. Math. Helv. **28** (1954), 17–86., which is horribly badly written, but probably

useful to look at, and your article on $H(\Pi; q, \mathbf{Z}_2)$ J.-P. Serre : "your article on $H(\Pi; q, \mathbf{Z}_2)$" : [**Se53b**]. which has pleased me so far. I have also gone thoroughly over the basic principles of ho- and coho-mology; I needed to do so. I came across several questions of a standard type, to which you may have an answer. Let X be a space with a "Φ family", $X' \subset X$ a closed subset, $U = \complement X'$, and F a sheaf on X. If B is a subset of X, then let $\Phi \cap B$ be the set of all $A \cap B$ $(A \in \Phi)$, and let Φ_B be the set of all $A \in \Phi$ which are in B. $\mathcal{O}$ denotes an arbitrary open neighborhood of X'. There are canonical homomorphisms, (taking the singular (co)homology point of view, for the sake of argument)

$$\begin{cases} H^p_\Phi(X \bmod X', F) \leftarrow H^p_{\Phi_U}(U, F) & \text{bijective if } X \text{ and } X' \text{ are HLC, for example.} \\ H^\Phi_p(X \bmod X', F) \rightarrow H^{\Phi \cap U}_p(U, F) & \textit{when is this bijective?} \end{cases}$$

J.-P. Serre : (HLC) means "homologically locally connected". A space X has this property if for any $x \in X$, any integer $p \geq 0$ and any neighborhood V of x, there is a neighborhood U of x in X contained in V such that the homomorphism $H_p(U, \mathbf{Z}) \rightarrow H_p(V, \mathbf{Z})$ is trivial. (Here, H_p denotes the p-th group of *singular homology* if $p > 0$; when $p = 0$, this is replaced with the corresponding "reduced group", i.e. the kernel of $H_0 \rightarrow \mathbf{Z}$.) Every locally contractible space (any simplicial complex, for example) is (HLC).

In the Cartan seminar of 1948/1949, Cartan showed that if X is a locally compact (HLC) space, then the singular cohomology of X can be identified with its sheaf cohomology.

$$\begin{cases} H^\Phi_p(X \bmod U, F) \rightarrow \varinjlim_\mathcal{O} H^{\Phi \cap \mathcal{O}}_p(\mathcal{O}, F) & \text{bijective if X has a normal neighborhood} \\ H^p_\Phi(X \bmod U, F) \leftarrow \varprojlim_\mathcal{O} H^p_{\Phi_\mathcal{O}}(\mathcal{O}, F) & \textit{when is this bijective?} \end{cases}$$

$$\begin{cases} H^p_{\Phi \cap X'}(X', F) \leftarrow \varinjlim_\mathcal{O} H^p_{\Phi \cap \mathcal{O}}(\mathcal{O}, F) & \text{always bijective.} \\ H^{\Phi \cap X'}_p(X', F) \rightarrow \varprojlim_\mathcal{O} H^{\Phi_\mathcal{O}}_p(\mathcal{O}, F) & \textit{when is this bijective?} \end{cases}$$

Homomorphisms which are dual to each other are bracketed together. $\varinjlim =$ inductive limit, $\varprojlim =$ projective limit; in any case it is always clear what I mean. The first two pairs of homomorphisms arise when one seeks to eliminate "relative" groups by interpreting them in terms of "absolute groups". The questions marked (?) are those to which I have only managed to give very unsatisfactory answers by duality arguments: I am forced to assume that F is at least locally constant, and that Φ is the family of all closed sets (co), respectively, of all compact sets (ho). A reasonable conjecture would be that everything works if X *and* X' are both HLC. Here is an analogous question: is

the homomorphism

$$H^p(X, F) \to \varprojlim_K H^p(K, F)$$

(where K runs over all compact sets of X, which is assumed to be locally compact and paracompact) bijective, at least in certain cases? Once again, I have a partial answer by duality if F is locally constant, but this is scarcely satisfactory. This question arises in justifying the "passage to the limit" in Cartan's theorems A and B once the compact case is established, for example.

As an exercise, I have been thinking about your question about a Künneth formula for cohomology with coefficients in algebraic coherent sheaves over projective varieties. I have shown carefully that it is enough to prove that, if A is *locally free* on X^r and *acyclic* and B is *locally free* on X^s and *acyclic* (where X^r and X^s are projective spaces), then $A \widehat{\otimes} B$ (the "good" tensor product, obtained from $A \otimes B$ by extension of scalars from $\mathcal{O}_{X^r} \otimes \mathcal{O}_{X^s}$ to $\mathcal{O}_{X^r \times X^s}$) is acyclic. Or even that $A(n) \widehat{\otimes} B$ is acyclic for n large enough. Does this help you at all? I did not get any further.

You said that Bourbaki wanted to send me a draft by Samuel on algebraic geometry (and commutative algebra?). I would be happy to get it, and the same goes for any other draft, interesting reprint, etc. My address is:

Dep. of Math., University of Kansas, Lawrence (Kansas) USA

Thank you in advance for your help. Yours,

A.Grothendieck

January 29th. That blasted package has just turned up unannounced after all! All's well that ends well. Let me remind you that I have exactly 5 copies of each of the seminars.

February 18, 1955 Alexandre Grothendieck

Dear Serre,

I am writing to you with several questions and remarks.

In your article on the mod 2 cohomology of Eilenberg-Mac Lane complexes, §1.2, property 2.2 of Steenrod squares one should observe that Sq^i and ∂ commute provided they only act on cohomology classes of degree $q \geq i$, otherwise this is clearly false. For this reason, an iterated square Sq^I does not commute with the connecting homomorphism unless restrictions are imposed on the degree of the classes on which it acts ($i_r \leq q, i_{r-1} \leq i_r + q, \ldots, i_1 \leq i_2 + \cdots + i_r + q$), and for the same reason, Sq^I only commutes with the transgression under the same conditions. Luckily, these conditions are satisfied if $i_k = 2^{r-k}a$, when working with classes of degree $\geq a$, and this is why the argument given in §2.7 is correct. I noticed something funny going on because the argument by which you establish the axiomatic characterization of the Sq^I seemed in fact to prove that they were all identically 0, with the result that for a whole day I was no longer sure which way was up. Let me point out, furthermore, that in §2.10, th. 3, you have forgotten the generator u_q itself, so "$i_r > 1$" should read "$i_r \neq 1$". ∗Moreover at the bottom of page 223 I do not see how "the preceding corollary shows…" directly, except when one restricts to applying Sq^I of degree n to H^q where $q \leq n$. All I am prepared to admit is that a posteriori, the Wu-Wen-Tsun-Adem formula gives the result (and I have not actually really understood why, not t hat I have made much effort to see it). ∗

Otherwise, I think the following idea for proving a Kunneth formula for projective varieties with coherent algebraic sheaves should work (though you probably already have a proof). As I said, szyzygytic resolutions ∗(what is the correct spelling?) ∗ show that that the sheaves F and G can be assumed to be locally free on the projective spaces X and Y. Moreover, although I have not checked it (it is not my job!) I am convinced that the Leray spectral sequence for a continuous map (or at the very least for a bundle projection) is valid for a non-separated space, the E_2 term being $\mathrm{E}_2^{pq} = \mathrm{H}^p(\mathrm{B}, \mathrm{A}^q)$, where $\mathrm{A} = \sum \mathrm{A}^q$ is the sheaf over the base space B whose local group at $b \in B$ is equal to the inductive limit of the cohomology groups (with coefficients in H, the given sheaf of coefficients on the bundle E) of the inverse images in E of open neighborhoods of b; the E_∞ being, as it should be, the graded group associated to some suitable filtration on the cohomology of E. Setting $\mathrm{E} = \mathrm{X} \times \mathrm{Y}, \mathrm{B} = \mathrm{Y}, \mathrm{H} = \mathrm{F}\widehat{\otimes}\mathrm{G}$, one needs to compute A_y for $y \in \mathrm{Y}$. But now there is a fundamental system of neighborhoods U which are affine varieties

without singular points, so one must compute $H(X \times U, F\widehat{\otimes}G)$. To do this, it will be enough to compute using cochains associated to the open cover given by the $X_i \times U$ (which are affine varieties). However, a section of $F\widehat{\otimes}G$ over $X_i \times U$ is clearly an element of $\Gamma(X_i, F) \otimes \Gamma(U, G)$ (to see this, one may assume if necessary that the U are small enough for F to be globally free, and the problem is reduced to the case $F = \mathcal{O}_U$; if desired, the same simplification can be made on X by passing to a finer open cover by open affine subspaces X'_j). The cochain complex for $(X_i \times U)$ with coefficients in $F\widehat{\otimes}G$ is therefore the tensor product of the complex for (U_i) with coefficients in F and $\Gamma(U, G)$. By ordinary Künneth, its cohomology is therefore $H(X, F) \otimes \Gamma(U, G)$. This proves that A_y is the group of germs of rational sections of $H(X, F) \otimes W$, where W is the vector bundle which produces G. Hence, A is the same as the tensor product of the constant sheaf $H(X, F)$ on Y with G, and re-Künneth shows that this is exactly $H(X, F) \otimes H(Y, G)$. This gives the desired result, I think, on recalling that there is already a canonical map from $H(X, F) \otimes H(Y, G)$ into $H(X \times Y, F\widehat{\otimes}G)$, and thence into E_∞ etc., which should imply that the successive differentials are trivial from ∂_2 onwards. I have not looked into this, since I know that in fact it would be enough to prove the theorem for acyclic F and G, and in this case the result is obvious for degree reasons. I note also that with a little more technique, presenting no essential difficulty, it can be shown that if E is an algebraic bundle over an algebraic base space B whose fiber is a projective variety F, then for any coherent algebraic sheaf on E, H, the sheaf A on B which appears in $E_2^{pq} = H^p(B, A^q)$ is a coherent sheaf on B. It should be quite fun to compute the cohomology of plenty of classical varieties using this spectral sequence and see whether this does indeed give what is expected. ∗ I do not see why it would not be possible to introduce Chern et al. classes via universal spaces (from the homological point of view, as explained in Borel's thesis) and classifying spaces, which would then play the same role in an algebraic Riemann-Roch as in the one (due to Hirzebruch) that you vaguely explained to me, ∗ which works in the complex case.

I have been giving some thought to Stein varieties. It strikes me that it should be possible to greatly simplify the proofs of Cartan's theorems A and B by use of szyzygytic resolutions, and to avoid the big theorem stating that a holomorphic vector bundle over a cube is holomorphically trivial. On the other hand, the triviality of the $d_{\bar{z}}$-cohomology over a polycylinder is needed, which is substantially easier. There is only one remaining difficulty, which I believe to be minor (I do not have the necessary background information at

hand: I may have to use general results on elliptic equations). In any case, for a given coherent analytic sheaf, it is more or less immediate that it is acyclic on any sufficiently small polycylindrical neighborhood of a given point: using a szyzygytic resolution whose length is greater than the dimension of the space, the problem is immediately reduced to proving the same result for a free sheaf, hence for $\mathcal{O}$, and for the latter it follows from the local and global triviality of the $d_{\bar{z}}$ -cohomology. In any case, as you said, this makes the "projective" variants of theorems A and B independent of all this "Stein" mess. — A question: I have understood the cohomological principle (if it can be called that) underlying szyzygytic resolutions, but the following question remains unanswered every time: ∗ is a finitely generated projective module over the ring in question (for instance a ring of polynomials or of holomorphic functions) free? Is this easy to see in interesting special cases?∗ If I understand correctly, in the case of polynomials, it is not even known whether this theorem is true, and one has to restrict oneself to graded rings to get a result. — I intend to give a course on homological algebra here, following the (presumed !) outline of Cartan and Eilenberg's book, and it would be nice to be able to include szyzygytic resolutions. For my own sake, I have made a systematic (as yet unfinished) review of my ideas of homological algebra. I find it very agreeable to stick all sorts of things, which are not much fun when taken individually, together under the heading of derived functors.

I have also been thinking about the problem of a holomorphic bundle whose fiber and base are both Stein. J.-P. Serre : "the problem of a holomorphic bundle whose fiber and base are both Stein": the question is whether or not such a variety is itself a Stein variety. The answer is "no", cf. H. Skoda (Invent. Math. **43** (1977), 97–107) and J-P. Demailly (*ibid.* **48** (1978), 293–302). All I have done so far is to obtain more exact versions of things I already knew, and which I think other people must also know: everything works when the fiber is a subvariety of $\mathbf{C}^n$ with a (not necessarily holomorphic) structural group which is induced by a group of affine complex transformations. (More generally, what is needed is a structural group G such that the space H(F) of holomorphic functions on F is the closure of a union H_0 of finite-dimensional G-invariant subvector spaces such that every subset of F on which every function $\in H_0$ is bounded is relatively compact; the first condition is automatically satisfied if the structural group is compact, and hence if the structural group is a connected Lie group. ∗ In fact, do you know of any example of a Stein variety whose automorphism group is not small enough and whose H_0 is therefore not large enough for these conditions to be satisfied?∗). In particular, this shows

that a holomorphic principal bundle whose base is Stein and whose fiber is a linear complex group (for example, a complex connected Lie group, a finite group, a finitely generated abelian group, or any group having a composition sequence whose successive factors are of these types) is Stein. *For all practical purposes* this seems to cover every case, including the universal cover J.-P. Serre : "universal cover": Grothendieck used "recouvrement" (cover, as in "open cover"), instead of "revêtement" (covering, in the geometric sense). of a Stein variety when the fundamental group is linear. ∗By the way, are there any known examples of finitely generated discrete groups which are not isomorphic to closed subgroups of linear groups (or alternatively, on which there is no separating system of functions which generate finite-dimensional subspaces under left translation?)∗. The answer is probably well known, but I do not even know it for fundamental groups of Riemann surfaces, nor in particular for the quotient by its center of the group of all matrices $\begin{pmatrix} a & b \\ c & d \end{pmatrix}$ where a, b, c and d are integers.

I have not yet got very far with topology yet, as I have been devoting part of my time to other things (Fourier on Lie groups). But I have overcome my phobia of the spectral sequence.

Yours,

A. Grothendieck

P.S. A question I forgot: Is it known whether the quotient of a Stein variety by a "fixed point free" discrete group is Stein? The passages enclosed in ∗'s are those on which I would like to have your opinion, if possible.

February 26, 1955 Jean-Pierre Serre

Dear Grothendieck,

Before leaving for a Bourbaki congress, I will try to answer the torrent of questions you asked in your last letter.

1) The quotient of a Stein variety by a "fixed-point free" discrete group J.-P. Serre : "discrete group". Grothendieck probably meant "finite group"; in this case, the answer to his question is "yes". is not always Stein, since it can even be a compact variety! Cf. elliptic curves, other curves, automorphic functions, etc. Let us forget about it!

2) I spurn your criticisms of my "mod 2 cohomology":

a) It is true that $\mathrm{Sq}^{\mathrm{I}} \circ d = d \circ \mathrm{Sq}^{\mathrm{I}}$ for classes of *any* dimension. Indeed, this is trivial for classes of low dimension (everything vanishes); you agree with me for classes of sufficiently high dimension, and it is therefore enough to show that the d of a square is always 0, which quickly follows from a trivial computation.

b) In n⁰ 30), you have been led into error by the following fact: the transgression τ is a homomorphism from a subgroup of the cohomology of the bundle into a *quotient* of the cohomology of the base. It is an abuse of language to think of $\tau(x)$, $x \in \mathrm{H}^*(\mathrm{F})$, as a cohomology class of B: it is a class which is defined *modulo* certain other classes. When one says that Sq^{I} and τ commute, this obviously means only that they commute modulo the classes in question (which, moreover, is made clear on p. 457 of my thesis — cf. also the paper by Borel and myself on reduced Steenrod powers). In fact, the argument given in my n⁰ 30 is nevertheless correct, since there I use τ precisely in those dimensions for which I am sure that this "modulo" is trivial.

c) You are right about th. 3 in n⁰ 10; one should add the Sq^{I} corresponding to $\mathrm{I} = \emptyset$ (moreover, one may maintain that the "last term" of the empty sequence is ≥ 1, if one wants to play the logician!).

d) p. 223, you are right again: at first glance the preceding corollary appears to give the result only on applying Sq^{I} to elements of degree $q \geq n = \deg(\mathrm{I})$. In fact, once such a result is established for classes of "high" degree, it extends all by itself to classes of low degree: you can see this by looking at u_q and applying a (downwards!) induction on q, for example: apply the transgression and use the fact that it is one-to-one. J.-P. Serre : "one-to-one" = injective.

3) The correct spelling is "syzygetic". Moreover, Cartan says one should simply write "free resolution" or simply "resolution" and that the adjective syzygetic does not add anything. I confess I only use it because it has such a lovely 19th century feel...

4) I had also considered the possibility of using "resolutions" to prove theorems A and B for Stein varieties, but I had not got very far. I agree that this method allows you to prove that every point has a basis of neighborhoods made up of polycylinders over which the cohomology of the mess vanishes. But this does not appear to be enough to give the result even for a "big" polycylinder; theorem A seems to be needed to construct global resolutions, and it is precisely here that the theorem on invertible holomorphic matrices is used. Cartan is of the opinion that this theorem is a key point of the proof,

and I would be very glad if you managed to get rid of it. Give me more details if you manage to do so.

You say that the finiteness theorem J.-P. Serre : This is a reference to the finiteness theorem that was proved by Grothendieck in [**Gr56a**]. (the only thing used in the proof of theorems A and B for projective varieties) can be proved using only "small polycylinders"? In this case I assume you use the proof given in your paper, and not the one given in my paper with Cartan J.-P. Serre : "the one given in my paper with Cartan": see [**CS53**]. in the Comptes Rendus (since we needed the fact that the intersection of two "good" open sets is a good open set, which is a result that is no longer available to you).

5) Of course, when the fiber F of an analytic bundle can be embedded in $\mathbf{C}^n$ on which the group acts linearly, the bundle is Stein since it can be embedded in a vector bundle, for which it is known that the result holds. But I do not know what the significance of this remark might be. In particular, which complex Lie groups can be embedded in $\mathrm{GL}(n, \mathbf{C})$ as *closed* subgroups? Not all of them, in any case (abelian varieties), but according to Papa Cartan[2], all semi-simple complex groups can, which is already a fair number. I am just as ignorant about discrete groups: the answer is yes for the fundamental group of a Riemann surface, since it is well known that this can be embedded in the complex group of Möbius transformations, which can itself be embedded into a linear group (easy to see, or a special case of the theorem of Cartan's cited above). For an arbitrary finitely generated discrete group, I am skeptical. J.-P. Serre : Of course, there are many finitely generated groups (and even finitely presented groups) which cannot be embedded into a linear group! There is a lot of choice.

6) No, one should not try to define "Chern classes" as elements of certain $\mathrm{H}^q(\mathrm{X})$ with coefficients in coherent sheaves, since these are vector spaces over the base field and the aim is to be able to define intersections with integral coefficients. Moreover, the "last" Chern class is already known, namely the canonical class, and it is a divisor class, defined up to linear equivalence. It is absolutely certain that it is possible to define J.-P. Serre : A little later, Grothendieck was to construct a theory of Chern classes of the sort that I was asking for; see [**Gr58**]. the other classes as equivalence classes of algebraic cycles up to "numerical" or "algebraic" equivalence. Even more: this should not be difficult.

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[2]Papa Cartan = Elie Cartan, father of Henri.

(It should be clearly understood that the two sides of the Riemann-Roch formula are *integers* and not integers modulo p, even when working in characteristic p.)

7) Your method for computing $H^*(X \times Y)$ may perhaps work. The tricky thing will be to set up the spectral sequence of a projection, and get the d^r to vanish. But I think there is a more brute-force method that gives the result: simply take the product of two open covers of X and Y by open affine sets, and compute with that. I have not yet had the courage to investigate this in detail (it is an Eilenberg-Zilber-type argument based on the comparison of a "product" complex and a "tensor product" complex). It will certainly work. J.-P. Serre : This method for proving the Künneth formula (for coherent algebraic sheaves) is explained in *Groupes Algébriques et Corps de Classes*, p.186.

8) A finitely generated projective module over a local Noetherian ring A is free. This is the result that is used all the time in free resolutions. The proof is based on the fact that if M is projective and K is the quotient field of A by its maximal ideal $\mathfrak{m}$, then $\mathrm{Tor}_q(M, K) = 0$ for $q \neq 0$. Take a basis of $M/\mathfrak{m}M$ and use it to define a homomorphism $\varphi : L \to M$, where L is free, which gives an isomorphism from $L/\mathfrak{m}L$ to $M/\mathfrak{m}M$. Conclude first that $\varphi : L \to M$ is surjective (set $Q = M/\varphi(L)$, and note that $\mathfrak{m}Q = Q$, which implies that $Q = 0$), and then that $\varphi : L \to M$ is injective (by writing down the exact sequence and bearing in mind that $\mathrm{Tor}_1(M, K) = 0$. — Of course, one also needs the obvious fact that $M/\mathfrak{m}M = M \otimes K$). Q.E.D.

The same proof works, and the same result follows, if A is a Noetherian graded ring and M is projective and graded.

I have no other news,

Yours,

J-P. Serre

February 26, 1955 ALEXANDRE GROTHENDIECK

Dear Serre,

Thank you for your letter and the parcel you tell me I am going to get. I agree that homology with coefficients in a sheaf is not very interesting if the sheaf is not locally constant, but one is forced to look at what happens at least for locally constant sheaves, and one then realizes that when everything behaves nicely, it is no more work to do without any restrictions on the sheaf (example: duality!). As for your impression that if the answer to my questions on cohomology is affirmative in dimension 0, then under the same conditions it must be affirmative in all dimensions: you are mistaken, at least for Čech cohomology. Thus, the formula $H^p_\Phi(X \bmod U, F) = \varprojlim H^p_{\Phi_O}(O, F)$ (where O runs over all open neighborhoods of the complement of the open U) is obviously true for $p = 0$ but is false for $p = 1$ if, for example, X is the interval $[0, 1]$ and U is the complement of a sequence which converges towards 0 and $F = \mathbb{Z}$. I presume this is why X and U have to be HLC; note that the example above is no longer a counterexample for singular cohomology, and it may be that the envisaged relation is always valid for singular cohomology (and as this coincides with Čech cohomology if X and $\complement U$ are HLC, the conjectured result for Čech cohomology would follow). I have not continued looking into these questions, whose interest is clearly limited.

Otherwise, I put in a systematic form, once and for all, the "Mittag-Leffler" approximation process, which you mentioned to me for a special case. I recommend to Bourbaki's attention the following theorem, which has been proved umpteen times in all kinds of special cases: J.-P. Serre : "I recommend to Bourbaki's attention the following theorem": Bourbaki included a statement of this kind in TG II, §3, no5.

Let $(A_i)_{i \in I}$ and $(B_i)_{i \in I}$ be two projective systems of groups, let (ϕ_i) be a homomorphism from the first to the second, ϕ the homomorphism from $\varprojlim A_i$ to $\varprojlim B_i$ defined by the ϕ_i, and (N_i) the "kernel" of (ϕ_i). Assume that I contains a cofinal *sequence* and that (N_i) satisfies the following property:

(A) The N_i have Hausdorff topologies, compatible with the group structure, for which the maps $N_i \to N_j$ $(i > j)$ are continuous, such that for any i there exists a $j \geq i$ such that for any $k \geq j$, the image of N_k in N_i is dense in the image of N_j.

Under these conditions, an element $b \in B$ is contained in the image of φ if and only if for all i, its component b_i in B_i is an element of the image of φ_i.

(Here is another way of saying the same thing: consider the following property h_1 of a projective system (N_i); for any exact sequence $0 \to (N_i) \to$

$(A_i) \to (B_i) \to 0$ of projective systems, the corresponding sequence of projective limits is exact, i.e. $\varprojlim A_i \to \varprojlim B_i$ is surjective. In cohomological terms, for abelian groups, this means that the first derived functor $\lim^1$ of the "projective limit" functor defined on the category of projective systems over a fixed I vanishes for (N_i): $\lim^1(N_i) = 0$. The theorem then says that condition (A) ("approximation ") implies h_1). In particular, condition (A) may hold upon simply taking the discrete topology; this then gives a condition which I call (A_0): for every i, there exists $j \geq i$ such that $k \geq j$ implies that N_k and N_j have the same image in N_i.

Now let X be a locally compact space which is countable at infinity. A sheaf F on X is said to satisfy condition (A) resp. (A_0) if the projective system of its sections over the relatively compact subsets of X satisfies (A) resp. (A_0) (one may choose either sections over compact subsets or relatively compact subsets). In this case, if the kernel of a sheaf homomorphism $F \to G$ satisfies (A) [and hence in particular if it satisfies (A_0)], then a section of G over X is the image of a section of F if and only if the same thing is true over any compact subspace. This is frequently used in the theory of *elliptic analytic* differential equations over a variety for example, since the existence "over any compact subspace" of an elementary kernel allows us to prove that every function is in the image of the differential operator in question over any relatively compact open set. Here is another application: Let (C_i) be a projective system of graded groups, $C_i = \sum_n C_i^n$, equipped with a derivation of degree 1. Let C be its projective limit; there is always a homomorphism, $H^n(C) \to \varprojlim H^n(C_i)$, but unfortunately this is not an isomorphism in general. It is, however, easy to see that it will definitely be an isomorphism if the system (Z_i^{n-1}) of $(n-1)$ cycles and the system (B_i^n) of n-boundaries satisfy h_1, and in particular if they satisfy (A). Note now that the quotient of a projective system which satisfies (A) resp. (A_0) satisfies the same condition. Therefore, if the two systems $(C_i^{n-2}), (C_i^{n-1})$ satisfy (A), resp. (A_0), then it is automatic that (B_i^n) and (B_i^{n-1}) also do, and it follows immediately that the same will hold for (Z_i^{n-1}) if and only if it holds for its quotient (N_i^{n-1}) by the subsystem (B_i^{n-1}). *Hence, if C^{n-2} and C^{n-1} satisfy* (A) *and furthermore one assumes that the same holds of* (H_i^{n-1}), *then* $H^n(C) = \varprojlim H^n(C_i)$. Note that for example the projective system defined above for a sheaf C^n clearly satisfies (A_0) whenever C^n is a *fine sheaf.* If one now computes the cohomology of X with coefficients in F using a resolution of F by fine sheaves, one sees that $H^n(X, F)$ *can be identified with the projective limit of the $H^n(U, F)$* (U relatively compact)

provided that the projective system formed by the $H^{n-1}(U, F)$ satisfies (A) [and in particular if it satisfies (A_0)]. It seems to me that this is the essential point to remember. It shows, as you have already told me, that theorems A and B on Stein varieties are proved once they are proved for compact analytically convex sets; the dimension 1 case does not present any extra difficulty, since a coherent analytic sheaf on a Stein variety satisfies (A), which is easy to see "without leaving compact sets" by the approximation theorem: a holomorphic function on an analytically convex compact set is a limit of holomorphic functions defined on arbitrarily large compact sets.

If one asks whether or not $H^p(X, F) = \varprojlim H^p(U, F)$ when F is a sheaf of locally constant modules, then another method is needed. Let us use *singular* cohomology and introduce the "product topology" on the space of chains with coefficients in F with respect to the discrete topology on F_x. Assume that the F_x are linearly compact, in the sense that every descending nested family of translates of submodules has a non-empty intersection (it is enough that the descending chain condition be satisfied, for example). Then the same holds for the module of cochains in any given degree. This easily implies the desired result. In particular, therefore, the result holds if the F_x are fields or finite-dimensional vector spaces or finite groups etc. In particular, it holds if $F = \mathbf{Z}/m\mathbf{Z}$. I think that this should be enough to prove the same formula for $\mathbb{Z}$ via universal coefficient formulas, and maybe even for any system of local groups, but this does not seem to be completely obvious and I have not continued in this direction.

I have noticed that, formulating the theory of derived functors for more general categories than modules, one obtains at the same time the cohomology of a space with coefficients in a sheaf with little extra effort; take the category of sheaves on a given space X, consider the functor $\Gamma_\Phi(F)$ which takes values in the category of abelian groups, and consider its derived functors. Their existence follows from a general criterion, in which the fine sheaves play the role of "injective" modules. One also obtains the fundamental spectral sequence as a special case of the delectable and useful general spectral sequences. But I am not yet sure if everything works out so well for non-separated spaces, and I am reminded of your doubts as to the existence of a cohomological exact sequence in dimension ≥ 2. Moreover, all this is probably contained more or less explicitly in the Cartan-Eilenberg book, which I have not yet had the pleasure of seeing.

I am finishing reading your paper on Eilenberg-Mac Lane complexes, but the last page is noticeably more difficult than all the rest because you are not

kind enough there to remind the reader of everything explicitly. In any case, I will have to come back to it after having force-fed myself Whitehead products, Blakers-Massey elements, etc.

I learnt a few weeks ago that about a hundred associate professorships are being created in France, and that they will be open to foreigners. Do you know whether or not this is making any progress, whether I might have a chance of getting one, and how and when one goes about applying? I would be extremely interested, as there can be no question of me staying in America, and I would much prefer to stay in France than go to Germany or even South America!

Yours,

A. Grothendieck

P.S, You wrote that the theory of coherent sheaves on affine varieties also works for spectra J.-P. Serre : "the theory of coherent sheaves... also works for spectra of commutative rings for which any prime ideal ...". This first approximation to the theory of affine schemes was "in the air" in 1954-1955, at least with some restrictions on the rings in question. One of Grothendieck's contributions was to remove these restrictions: the best category of commutative rings is the category of *all* commutative rings! of commutative rings for which any prime ideal is an intersection of maximal ideals. Is the sheaf of local rings thus obtained automatically coherent? If this works well, I hope that for the pleasure of the reader, you will present the results of your paper which are special cases of this as such; it cannot but help in understanding the whole mess. J.-P. Serre : "your paper": FAC.

March 12, 1955 Jean-Pierre Serre

Dear Grothendieck,

Your stuff on topologies on projective limits looks very nice, and it is a great pleasure to see the limit process in the theory of Stein varieties finally swallowed up by a general argument. You seem to be ready to write a comprehensive presentation of the proof of theorems A and B (which, by the way, would be a darn useful piece of work for everybody; you should include the definition of topologies on the sections of a coherent sheaf, and the finiteness theorem, all of which would go well together; if you ever write it up, then I would like a copy — Or, even better: publish it!)

The fact that sheaf cohomology is a special case of derived functors (at least in the paracompact case) is not in Cartan-Sammy J.-P. Serre : "Sammy" = Samuel Eilenberg, of course. . Cartan was aware of it, and had told Buchsbaum to do it, but it appears that he never did. The point would be to see exactly which properties of fine sheaves are needed; it might then be possible to work out whether or not there are enough fine sheaves on non-separated spaces (I think the answer is no, but I am not at all sure!)

Yes, it is true that if A is a Noetherian Hilbert ring (i.e. one in which any prime ideal is an intersection of maximal ideals) then the sheaf of local rings of A, considered as a sheaf on the spectrum of A, is coherent: this is practically trivial. All the theorems of the affine theory hold without exception. But you surely realize that I am not going to change my paper on algebraic coherent sheaves for this! Moreover, I have already corrected the proofs, and it will be coming out in the March or the May issue of the Annals. I may write a short paper on the subject in the near future. There is no doubt that this is the right point of view, at least for the affine theory.

We have no details of the associate professorships. How many will there be in the whole of France? A mystery. Cartan has a candidate for Paris: Chevalley (confidential!). Will there be any positions in the provinces? In any case, you may be sure that if there is an opening for you we will jump at it (the Strasbourg group were actually more or less thinking about you, at one point when they thought they were going to get a position).

Otherwise, I know that Spencer has strongly recommended you for a (good) job at Stanford, or some such place. You may soon receive a letter from them. J-P. Serre

June 4, 1955 ALEXANDRE GROTHENDIECK

Dear Serre,

You will find enclosed a neat draft of the outcome of my initial reflections on the foundations of homological algebra. You will see in particular that what I told you about the existence of enough "projective" sheaves was mistaken, and why, and on the other hand that it is indeed true that the right $H^1(X, F)$ are those computed using open covers. Moreover, I have found the reason for the problem with the spectral sequence of the graded derivation sheaf Ω on an algebraic variety: the cohomology sheaf *is not* 0 in dimension > 0 (for example, if X is a curve of genus 1 and therefore a complex torus, a rational function with only 2 poles cannot be the derivative of a rational function). Therefore, all one can say is that the cohomology of X with coefficients in Ω, the graded derivation sheaf, has two different filtrations; the E_1 term corresponding to the first one is $E_1^{pq} = H^p(X, \Omega^q)$ (and it is likely that if X is complete then all the d_1 etc. differentials vanish); the E_2 term corresponding to the second one is $E_2^{pq} = H^p(X, H^q(\Omega))$ (where the cohomology groups on X are defined axiomatically, of course). If the opportunity arises it would be interesting to try to look into this more closely, in order to obtain some information on $H^0(X, H^q(\Omega))$ in particular. I will start by taking a good look at the theory of spectral sequences in abelian classes J.-P. Serre : "abelian class"= abelian category.; if you are interested, I could type up a summary for your personal use. I am already convinced that the Bourbakic way of doing homological algebra consists of changing the abelian class at any moment, as one might change the base field, or the topology in Functional Analysis.

In no6, I have marked two passages with a "?" sign in the margin, to indicate that if you feel that such unhatched chickens have no place in a Bourbaki talk, then you can simply delete them. Please remember to set a mimeographed copy aside for me; I have not made a copy, and it may be useful to have one later for when I write up a neater version.

Yours,

A.Grothendieck

May one know where you will be during the summer? I will not be budging from here except during August, when I will be in Chicago (unless I am already back in France because of my mother). You should preferably write to me at my personal address, since I no longer have anything to do at the university.

July 13, 1955 Jean-Pierre Serre

Dear Grothendieck,

You must think me a terrible correspondent for not having answered your letter sooner, but I have just come back from the Bourbaki meeting, and I had loads of things to do.

The Bourbaki meeting went very nicely. You will see the report in the Tribu. J.-P. Serre : "the Tribu". This is the newsletter produced by Bourbaki for the use of participants, containing a list of the drafts which each one has accepted to write. Here is the news concerning you:

1) Your paper on homological Algebra was read carefully and converted everyone (even Dieudonné, who seems to be completely functorised) to your point of view. Sammy has decided to write up a draft (for Bourbaki) along these lines, in which Chapter I would be the general theory of homology in abelian classes, Chapter II the application to modules and Chapter III the application to sheaves. He will contact you for details of the proofs and the writing up.

Thanks to a letter you sent me, we managed to reconstitute the proof of lines 3 and 4 on page 8 of your paper. However, we couldn't see how to prove the 1º just before.

2) We would really like you to come to the Bourbaki meeting in October, if possible (and ditto for the others, of course! I don't remember exactly what the program is to be (in any case, there will be a reading of my draft on filtered rings etc.), and I don't think you will find it particularly interesting. But one is not in Bourbaki for fun, as Dieudonné never stops repeating.. . .

3) Bourbaki would like you to write up a draft on the theory of coherent analytic sheaves on Stein varieties (theorems A and B, basically). This would be useful from several points of view: 1) it would give some idea as to what should be included in the elementary theory of analytic functions, 2) ditto for sheaf theory, 3) it would get work going on a whole group of questions which I think Bourbaki would be perfectly capable of writing up.

So much for Bourbaki. However, your paper on homological algebra raises a totally disjoint question, namely that of publishing it in a journal. You probably know that in his thesis (to appear in the Trans. Amer. Soc.) and his appendix to Cartan-Sammy, Buchsbaum developed a system very similar to your abelian classes (I don't know whether you knew this when you wrote

them up, but it doesn't matter). I don't know what axioms he started with, but Sammy claims they are equivalent to C_1, C_2, C_3. He had noticed (and stated) that the existence of sufficiently many injectives implies the existence of a well-behaved theory of derived functors. But he was unable to show that sheaves possess sufficiently many injectives, as he lacked a proposition like the one you give on pages 7-8. Sammy therefore suggests that you publish a paper in the Transactions in which you give your axioms $C_{4,5,6}$, the concept of the generator of a class, the fact that injectives exist when there is a generator and that..., the fact that sheaves satisfy your axioms, and the comparison between traditional sheaf cohomology and the cohomology obtained by your procedure. As you could use Buchsbaum for all the trivial results on classes,

you would basically only need to write up the interesting part, and that would be very good. All this could probably be written up briefly and without too much trouble, and it would be useful to a lot of people. What do you think? Obviously, Sammy could arrange to get you a copy of Buchsbaum's thesis.

[...]
Yours, J-P.Serre

December 15, 1955 ALEXANDRE GROTHENDIECK

My dear Serre,

Thinking a bit about your duality theorem, I notice that its general form is almost obvious, and in fact I just checked that (for a projective space) it is implicitly contained in your theorem J.-P. Serre : This is a reference to th.1 of FAC, no72. giving the $T^q(M)$ in terms of Exts. (I have the impression, you bastard, that S§3 and 4 in your Chap.3 could be done without any computation). Let $\mathrm{Hom}_{\mathcal{O}}(X, F, G)$ denote the *group* of $\mathcal{O}$-homomorphisms from the sheaf F of $\mathcal{O}$-modules on X to some other sheaf G, and $\mathrm{Ext}_{\mathcal{O}}^p(X, F, G)$ its derived functors (so this is a special case of Exts in abelian classes, but I put the X into the notation to avoid the obvious misunderstanding). $\underline{\mathrm{Hom}}_{\mathcal{O}}(F, G)$ is the *sheaf* of germs of $\mathcal{O}$-homomorphisms from F to G, with derived functors $\underline{\mathrm{Ext}}_{\mathcal{O}}^p(F, G)$, so these are sheaves. As I told you, there is a natural filtration on $\mathrm{Hom}_{\mathcal{O}}(X, F, G)$, and the associated graded object is the E_∞ term of a spectral sequence whose E_2^{pq} term is $\mathrm{H}^p(X, \underline{\mathrm{Ext}}_{\mathcal{O}}^q(F, G))$. This said ($X$ is now a projective algebraic variety (n-dimensional, without singular points), F a coherent algebraic sheaf on X, and Ω^n the sheaf of germs of differential n-forms on X), the dual of $\mathrm{H}^p(X, F)$ can be canonically identified with $\mathrm{Ext}_{\mathcal{O}}^{n-p}(X, F, \Omega^n)$, in the following way: in general, there exist pairings $\mathrm{Ext}_{\mathcal{O}}^p(X, F, G) \times \mathrm{Ext}_{\mathcal{O}}^q(X, G, H) \longrightarrow \mathrm{Ext}_{\mathcal{O}}^{p+q}(X, F, H)$ [valid in any abelian class], whence in particular

$$\mathrm{Ext}_{\mathcal{O}}^p(X, \mathcal{O}, F) \times \mathrm{Ext}_{\mathcal{O}}^{n-p}(X, F, \Omega^n) \longrightarrow \mathrm{Ext}_{\mathcal{O}}^n(X, \mathcal{O}, \Omega^n);$$

it is easy to see from the spectral sequences that the first and last terms are $\mathrm{H}^p(X, F)$ and $\mathrm{H}^n(X, \Omega^n)$ respectively; the latter is canonically isomorphic to k, which gives the desired pairing, i.e. a homomorphism

$$\mathrm{H}^p(X, F)' \longrightarrow \mathrm{Ext}_{\mathcal{O}}^{n-p}(X, F, \Omega^n)$$

This homomorphism is obviously a homomorphism of ∂-functors; furthermore, by your duality theorem, it is a bijection if F is locally free. It is then easy to show by induction on the length of a locally free resolution of F and the five lemma that it is bijective for any F.

Note that in this way, one gets a natural filtration on $\mathrm{H}^*(X, F)$, whose associated graded object is the E_∞ term of a spectral sequence whose E_2^{pq} term is $\mathrm{Ext}^{n-p}(X, \underline{\mathrm{Ext}}_{\mathcal{O}}^q(F, \Omega^n), \Omega^n)$ (this spectral sequence is obtained by dualizing the spectral sequence given above for $\mathrm{Ext}_{\mathcal{O}}(X, F, \Omega^n)$; but I don't see any direct interpretation of it).

Apart from this, there is no news; I am trying to learn things, but there is so much to look at, and it is so slow! — Cartier seems to be an amazing

person, especially his speed of understanding, and the incredible amount of things he reads and grasps; I really have the impression that in a few years he will be where you are now. I am exploiting him most profitably.

Yours,

A. Grothendieck

December 22, 1955 JEAN-PIERRE SERRE
Dear Grothendieck,
I find your formula

$$\mathrm{H}^{n-p}(X, F)' = \mathrm{Ext}^p_{\mathcal{O}}(X, F, \Omega^n)$$

very exciting, as I am quite convinced that it is *the* right way to state the duality theorem in both the analytic case and the algebraic case (the more important one – for me!).

I did know (although I never wrote down the details of the proof) a less precise formula, namely the following:

(*)$\mathrm{H}^{n-p}(X, F)'$ is the "abutment" of a spectral sequence whose E_2 term is $\sum_{r+s=p} \mathrm{H}^r(X, \mathrm{Ext}^s_{\mathcal{O}}(F, \Omega^n))$.

This formula is an obvious consequence of yours and of your general spectral sequences. Furthermore, I should confess that I only had an outline of the proof: 1) I had not yet written down a careful construction of $\mathrm{Ext}^s_{\mathcal{O}}(F, \Omega^n)$, but this is now morally done, thanks to your abelian classes; 2) Assuming 1), I had initially proved (*) when X is a projective space (using my explicit results in this case), and I was trying to reduce the general case to this one, using the following formula:

If X is an m-dimensional subvariety of V, then J.-P. Serre : The Ext in formula (**) is Ext^{m-n}, where $m = \dim V$ and $n = \dim X$.

(**) $$\Omega^n_X = \mathrm{Ext}_{\mathcal{O}_V}(\mathcal{O}_X, \Omega^m_V).$$

There is no point in giving you more details – it would be a waste of time, since your formula is certainly better.

Nevertheless, there are still a couple of natural questions:

1) You really should have a go at finding a proof of your formula which is independent of the vector bundle case; it can't be difficult, if you deal with projective space first and then use (**) or some similar formula to pass to the general case. I must say that the main difficulty appears to me to be the following: *there is no satisfactory definition* J.-P. Serre : "... no satisfactory definition of the sheaf Ω^n_X": the dualizing sheaf! (from a sheaf theoretic, functorial etc. point of view) *of the sheaf* Ω^n_X! Couldn't you find one by some ingenious use of Exts?

Here is another reason for wanting a direct proof of your formula: my proof (for fiber bundles) *uses* the result for curves and cannot be used to derive it, which is not the case, of course, for a proof based on reduction to

projective space. This is even the main reason which led me to look for a general formulation of the duality theorem!

2) It would be very interesting to have a theorem analogous to yours for analytic varieties (under the usual hypothesis that d'' is a J.-P. Serre : "homomorphism". A continuous linear map $f : E \to F$ (where E and F are topological vector spaces) was called a "homomorphism" if the canonical map $E/\mathrm{Ker}(f) \to \mathrm{Im}(f)$ is a homeomorphism. Such maps are now called "strict morphisms", cf. Bourbaki T.G. III.26. homomorphism). For example, in the case of Stein varieties, I tend to believe that the dual of $H^{n-p}_*(X, F)$ J.-P. Serre : "H^{n-p}_*". The subscript star means "compact support". is isomorphic to the space of sections of the sheaf $\mathrm{Ext}^p(F, \Omega^n)$. The problem is clearly that F does not necessarily have a locally free resolution. Do you see any way of getting around this?

Here is another remark: you used the fact that $H^n(X, \Omega^n)$ has a canonical basis. This is true (I finally proved it), J.-P. Serre : "I finally proved it". The "it" in question is the following result: for any non-singular, projective, connected X of dimension n, there exists a basis e_X of the vector space $H^n(X, \Omega^n_X)$ having the following property:

($**$) For any non-singular irreducible divisor Y of X, the homomorphism

$$\delta : H^{n-1}(Y, \Omega^{n-1}_Y) \longrightarrow H^n(X, \Omega^n_X)$$

sends e_Y onto e_X. (Note that δ is the connecting map associated to the exact sequence of sheaves

$$0 \longrightarrow \Omega^n_X \longrightarrow \Omega^n_X(Y) \overset{r}{\longrightarrow} \Omega^{n-1}_Y \longrightarrow 0,$$

where $\Omega^n_X(Y)$ is the sheaf of differential n-forms on X whose divisor is locally $\geq -Y$. As for r, it is the "residue" map, defined by $\eta \wedge dt/t \mapsto \eta_{|Y}$ where $t = 0$ is a local equation for Y and η is a local section of Ω^{n-1}_X.) The *uniqueness* of such an e_X is clear by induction on n (if $n = 0$, e_X is required to be the "unit" element of $H^0(X, \mathcal{O}_X)$). The *existence* is less obvious. It seems that I had constructed a proof of this, but I never wrote it up, since the result is a special case of the duality theorems proved shortly afterwards by Grothendieck. but it is not that trivial using induction. It would be interesting to have a direct proof of this fact.

All this raises a question: How should this stuff be written up? Could you do it – unless we write a joint paper together? At any rate, there is no hurry, since I will not be able to take care of it seriously before the month

of May, when I return to Paris: first I have to write up my "Tor" formula J.-P. Serre : "Tor formula" : this was to be the subject of my course at the Collège de France in 1957–58, published under the title " *Algèbre Locale – Multiplicités*" (revised English translation, with additional material: *Local Algebra*, Springer-Verlag, 200). as well as the analytic theory. On that topic, I have finally convinced myself that you were right about analytic and algebraic bundles: if G is an algebraic subgroup of $GL(n, \mathbb{C})$ such that $GL(n, \mathbb{C})/G$ has a rational section, then the classes of analytic and algebraic G bundles are in bijective correspondence. With your permission, I intend to include this in my analytic = algebraic diplodocus. J.-P. Serre : "analytic = algebraic diplodocus" = [**Se56a**], i.e. GAGA.

This raises a few questions: 1) What is a homogeneous space, from the algebraic point of view? 2) Have you checked that the usual embedding of the projective group into the linear group J.-P. Serre : No, the usual embedding of $\mathbf{PGL}_n$ in $\mathbf{GL}_{n^2}$ does not satisfy this condition if $n > 1$; I realized this shortly afterwards. I came back to this question with Grothendieck in the Chevalley Seminar of 1958. satisfies your condition? 3) Have you asked Chevalley if by any chance *all* algebraic subgroups of GL satisfy this condition? I would not be at all surprised.

On this topic, I should point out to you that one can do without what you told me about rational maps; one proves:

1) Let X be an algebraic variety (over $\mathbb{C}$) and Y another algebraic variety. Let $f : X \longrightarrow Y$ be a holomorphic map. If the graph of f is a closed algebraic subvariety of $X \times Y$, then f is regular. (Reduce to the case $Y = \mathbb{C}$.)

2) Let Z be an algebraic variety, and X an analytic subvariety of Z. If X is compact, then X is algebraic.

This reduces to Chow's theorem.

3) Let E be an algebraic bundle with compact base. Then every holomorphic section of E is algebraic and regular.

[More generally, by 1) and 2), every holomorphic map from a compact algebraic variety to another one is algebraic and regular.]

4) If two algebraic bundles with the same compact base are analytically isomorphic, they are algebraically isomorphic.

Apply 3) to the bundle of isomorphisms of the fibers of one bundle to the fibers of the other.

Etc.

Another thing I should tell you is that I have produced a little paper J.-P. Serre : "a little paper ": [**Se57**]. on the cohomology of abstract affine varieties, which I have sent to Cartan. Ask him to show it to you. In it, I prove:

THEOREM 1. — *Affine varieties are characterized by the fact that their cohomology vanishes.*

THEOREM 2. — *If F is a coherent sheaf on an algebraic variety X, then $H^q(X, F) = 0$ for $q > \dim X$.*

THEOREM 3. — *If X is complete, $H^0(X, F)$ is finite-dimensional.*

The proof of Theorem 1 is "standard", the proofs of Theorems 2 and 3 are definitely more entertaining.

Yours,

J-P. Serre

January 12, 1956 ALEXANDRE GROTHENDIECK

Dear Serre,

I am glad you like your Ext formula. J.-P. Serre : "your Ext formula". The "your" is probably a slip for "my". The proof I sent you was only meant to convince myself that the formula is right, but it is certain that it can be proved directly in the standard way using projective spaces. I haven't looked at that in detail yet, since I am working on something else right now, but I will write it up properly when I have a moment; it involves a little technical homological algebra, juggling with the formula $\Omega_X^n = \text{Ext}_{\mathcal{O}_V}^{m-n}(\mathcal{O}_X, \Omega_V^m)$ where X^n is a subvariety without singularities of the variety without singularities V^m. Let me just point out that in the case where X is a point, this gives an intrinsic expression of the vector space of n-forms at the point x, as the dual of $\text{Ext}_{\mathcal{O}_x}^m(k, \mathcal{O}_x)$, which yields an intrinsic definition of Ω^n (but maybe not exactly of the type you are looking for.) Let me show you how one can easily get hold of the canonical class in $H^n(X, \Omega^n)$. Embedding X^n into a projective space V^m, $H^n(X, \Omega_X^n)$ is then (by the duality theorem *for projective spaces*) $\text{Ext}_{\mathcal{O}_V}^{m-n}(V, \Omega_X^n, \Omega_V^m)$; the E_2^{pq} term of the spectral sequence is $H^p(V, \text{Ext}_{\mathcal{O}_V}^q(\Omega_X^n, \Omega_V^m))$; the Ext which occurs here is zero except when $q = m - n$, and in this case, by your formula, it is $\mathcal{O}_X$, so the global Ext is $H^0(V, \text{Ext}_{\mathcal{O}_V}^{m-n}(\Omega_X^n, \Omega_V^m)) = H^0(V, \mathcal{O}_X) = k$ (canonically). It follows that every embedding of X into a projective space gives rise to an isomorphism of $H^n(X, \Omega_X^n)$ with k. This gives a *canonical* pairing (for an arbitrary coherent algebraic sheaf F on X) between $H^k(X, F)$ and $H^{n-k}(X, F, \Omega_X^n) \otimes T_X$, where $T_X = H^n(X, \Omega_X^n)'$ (a one-dimensional vector space associated to X). The embedding into projective space then shows that this pairing is non-degenerate. Finally, one needs to exhibit a *canonical* isomorphism of T_X with k. To do this, note that the above computation can be repeated (now that the duality theorem is known for any V^m without singularities) for any embedding of X^n into an arbitrary projective variety V^m without singularities, and gives a canonical isomorphism (depending on the embedding) the other way, from T_V to T_X. Checking the fact that every immersion of X into a projective space gives rise to the same fundamental class for T_X can be done as follows: If X is embedded into projective spaces P_1 and P_2, one may assume that P_1 and P_2 are subvarieties of the same projective space P (I am assuming that the variety obtained by gluing P_1 and P_2 together along X is projective).J.-P. Serre : "the variety obtained by gluing P_1 and P_2 along X is projective". It was not a priori clear that this variety even exists, but this was proved by both D. Ferrand in his thesis (*Conducteur, Descente et Pincement*, Paris, 1970) and S. Anatharaman, *Schémas en groupes, espaces homogènes et espaces algébriques sur une base de dimension* 1, Bull. S.M.F., Mém. **33** (1973).

However, such a variety is not in general projective; it is projective only if the two invertible sheaves on X defined by $X \to P_1$ and $X \to P_2$ are proportional (in $\mathbf{Q} \otimes \mathrm{Pic}(X)$). Therefore, the method proposed by Grothendieck here does not work. The problem is then reduced to proving two things which should not be too tiring:

a) a transitivity property for the isomorphisms between the T_X associated to embeddings;

b) if a *projective space* P_1 is embedded into another projective space P, the associated isomorphism $T_P \to T_{P_1}$ transforms fundamental class into fundamental class.

Unfortunately, the role played by projective spaces in all this still seems rather excessive. I feel like looking into whether one doesn't get something for "regular arithmetic varieties" which are "complete" (i.e. obtained by gluing together spectra of regular rings). But to start with, do you have any idea of what complete really means in this context? The definition given in the Chevalley seminar (every valuation ring of the field of rational functions which contains the base ring contains a place) is not the right one, because it includes affine spectra of rings of algebraic integers, which would be crazy. I would also like to look at whether one can't state a duality theorem for projective varieties which may have singularities, and whether a more general and technical statement might not actually be simpler to prove than the one we are considering now.

I have read the draft you sent to Cartan; if I understand correctly, if one knows how to show for every projective variety that there exists *a* coherent non-torsion algebraic sheaf F such that the $H^q(X, F)$ are finite-dimensional, then all the $H^q(X, F)$ are (for arbitrary F). — Congratulations on getting down to writing an algebraic=analytic diplodocus; it was about time! — I have just read Chevalley's new book on class field theory; I am not really doing any research, but trying to cultivate myself.

Regards,

A. Grothendieck

16.1.56 ALEXANDRE GROTHENDIECK

My dear Serre,

Cartan has just found a spectral sequence (as it happens, the Leray spectral sequence) which throws a lot of light on the relationship between functorial cohomology and the cohomology computed using open covers (denoted H and $\check{H}$, respectively). If $\mathcal{U} = (U_i)$ is an *arbitrary* open cover of an *arbitrary* space X, and F is a sheaf on X, then $H(X, F)$ is the "abutment" of a spectral sequence such that $E_2^{pq} = H^p(\mathcal{U}, H^q(F))$, where $H^q(F)$ is the *presheaf* which has the value $H^q(V, F)$ on the open set V. The proof is entirely functorial and very easy; everything goes through because:

a) the restriction of an injective sheaf to a open set is injective (trivial)

b) if F is injective, then $H^p(\mathcal{U}, F) = 0$ whenever $p > 0$ (this can be seen by embedding F into the sheaf of germs of *possibly non-continuous* sections of F).

In particular, this gives the canonical homomorphisms $H^p(\mathcal{U}, F) \to H^p(X, F)$. Passing to the inductive limit gives another spectral sequence, abutting at $H(X, F)$, whose E_2 term is $E_2^{pq} = \varinjlim H^p(\mathcal{U}, H^q(F))$ (where the inductive limit is taken over the classes of open covers $\mathcal{U}$ of X). The nice thing is that here E_2^{0n} always vanishes for $n > 0$ (since the sheaf associated to $H^n(F)$ vanishes, as can be seen using an injective resolution of F). One can conclude from this that if the topology on X has a basis of open sets U_i such that $\check{H}^q(U_i, F)$ vanishes for $0 < q < n$ then the same holds for $H^q(U_i, F)$ (and conversely), and that the canonical homomorphism $\check{H}^q(X, F) \to H^q(X, F)$ is then bijective whenever $0 < q \leq n$ (induction on n). One recovers the fact that $\check{H}^1 = H^1$, and also the fact that for example the cohomology of coherent algebraic sheaves computed using open covers is the right one; in fact the question boils down to establishing that if X is *affine*, then $\check{H}^q(X, F) = 0$ for $q > 0$, which is quite easy in the general context of Cartier-Serre type ring spectra. J.-P. Serre : "Cartier-Serre type ring spectra": these were later to be called affine schemes. — When X is paracompact, one recovers $\check{H} = H$ by observing that the last term E_2^{pq} is trivial whenever $q > 0$, since the $\check{H}$ computed for a presheaf depends only on its associated sheaf, which is zero for the presheaf $H^q(F)$. To get the Φ-cohomology, Cartan suggests considering it as an inductive limit of ordinary cohomologies. . . — Let me also point out, as it might be useful, that given a continuous map f from a space X to a space Y, and a sheaf F on X, the Leray spectral sequence is valid with no additional conditions, provided that one does not use a family Φ: $H(X, F)$ is the abutment of a spectral sequence such that $E_2^{pq} = H^p(Y, F_q)$, where F_q is

the sheaf on Y associated to the presheaf $H^q(f^{-1}(U), F)$. The proof is easy (using derivations of compositions of functors, as usual; the key fact is that if F is injective, then so is its direct image F_0, which is easy).

Cartan's results enable me to prove that if X is an algebraic variety without singularities (it is even enough to assume that the local rings are factorial) then $H^i(X, k^*) = 0$ for $i \geq 2$ (which previously I had only proved for $\check{H}$). Indeed, $H^i(X, A) = 0$ $(i > 0)$ when A is constant, since this is already known for $\check{H}$. Then the exact sequence $0 \to k^* \to \mathcal{R}^* \to \mathcal{D} \to 0$ ($\mathcal{R}^* =$ germs of rational functions $\neq 0$, $\mathcal{D} =$ germs of divisors) gives an exact sequence of cohomology, and the problem is reduced to proving that $H^i(X, \mathcal{R}^*) = 0$ when $i > 0$, which follows from the fact that $\mathcal{R}^*$ is constant and $H^i(X, \mathcal{D}) = 0$ for $i > 0$, which follows from $\mathcal{D} = \sum \mathbf{Z}_V$ (summing over all irreducible hypersurfaces V in X) and $H^i(V, \mathbf{Z}) = 0$. Moreover, going back to my Kansas talk, I saw that one can give a universal functorial definition of the second connecting map $H^1(X, \mathcal{H}) \xrightarrow{\partial} H^2(X, \mathcal{F})$ associated to an exact sequence of sheaves of (not necessarily abelian) groups $e \to \mathcal{F} \to \mathcal{G} \to \mathcal{H} \to e$, where $\mathcal{F}$ is central in $\mathcal{G}$ (this also works if $\mathcal{F}$ is only abelian and invariant, provided that one twists $\mathcal{F}$, see my talk). Here $H^2(X, \mathcal{F})$ is of course the "right" cohomology, and ∂ is defined on the whole of $H^1(X, \mathcal{H})$ and not only on the part consisting of classes of cocycles which can be lifted to cochains of $\mathcal{G}$. This proves in particular that a projective algebraic bundle J.-P. Serre : "A projective algebraic bundle ...comes from a vector bundle" . Beware! The term "bundle" is here used in Weil's sense. The bundles in question are thus locally trivial. It was not until my lecture in the Chevalley seminar of 1958 that other bundles appeared; namely, "locally isotrivial" bundles; Grothendieck's statement does not apply to these. over a base without singularities comes from a vector bundle (as I thought I had already shown last summer); in the case where X is a projective complex variety without singularities, it is known that algebraic classification $=$ analytic classification, and thus one gets an answer to a question of Kodaira's by a very entertaining hyperfunctorial method.

Godement has found a trivial.J.-P. Serre : Grothendieck reproduced this construction in [**Gr57b**], p.156. way to have enough injective sheaves (sheaves of modules over a sheaf of rings $\mathcal{O}$): at every $x \in X$ one takes an injective $\mathcal{O}_x$-module A_x, and then one takes the sheaf of germs of all sections (without continuity conditions) of the family of the A_x. To think there are people who got stuck on this question! Yours,   A. Grothendieck

N.B. I can show that if X is a " Zariski space" of "dimension" $\leq n$, then for *any* sheaf F on X, $H^i(X, F) = 0$ for $i > n$. In particular, if X is an algebraic *curve*, $H^2(X, k^*) = 0$, and hence projective bundles can be lifted to vector bundles (even in the presence of singularities).

January 30, 1956 Alexandre Grothendieck

My dear Serre,

Let me briefly answer your questions. J.-P. Serre : "Let me briefly answer your questions". Grothendieck is probably referring to questions I had asked him in a previous letter, of which I have not kept a copy.

1) I must have forgotten a ˘ symbol somewhere in my explanations. Cartan's spectral sequence shows that if there is a basis of open sets U_i such that over any finite intersection U of U_i's the $\check{H}(U, F)$ cohomology (*computed using open covers*) vanishes for $i > 0$, then the same holds for the good $H^i(U, F)$, and then, by considering an open cover $\mathcal{U}$ made up of U_i's, one gets $\check{H}(\mathcal{U}, F) = H(X, F) = \check{H}(X, F)$ (Proof by induction on i, using the fact that in Cartan's second spectral sequence, the E_2^{0n} vanish for $n > 0$). This does indeed prove that for coherent algebraic sheaves on a variety X, the cohomology computed using open covers is the right one (take the U_i to be the affine open sets).

2) I use the fact that X has no singularities essentially to prove that $H^2(X, k^*) = 0$; thus it is not superfluous for the moment. (However, I have the impression that one should have $H^2(X, k^*) = 0$ without any hypotheses on X. I have proved this when there is only a finite number of singular points, but didn't continue). I agree there is no need to sweat blood over the curve case! – As for the algebraic classification = analytic classification question for projective bundles, I confess that I was taking it on trust that the usual embedding of the projective group into the linear group has a rational section, J.-P. Serre : "I was taking it on trust... has a rational section". No, it does not, cf. note 2212555. since everybody seemed convinced that that should always happen for a fibration by a linear group (you see what an irresponsible individual I am!). It is true that Chevalley does not know of any theorem in this direction except for Rosenlicht's, and it would be very sad if you already had a counterexample for the projective group. Actually, remark that to get such a counterexample, it is enough to note that in the fibration of the linear group G by the projective subgroup $GP(n-1)$ (base $G/GP(n-1)$) one cannot lift the structural group to the linear group $GL(n)$ (and this is also necessary if one assumes that an algebraic bundle of group $GL(n)$ is locally trivial; but I think you only proved this for projective varieties); it would be enough, for example, to prove that the associated bundle with fiber the projective space is not homologically equivalent with *integral* coefficients to the product $P(n-1) \times G/GP(n-1)$.

3) Sketch of the proof that $H^i(X, F) = 0$ for $i > n$ if X is a "Zariski space" of "combinatorial dimension" (via chains of closed irreducible sets) $\leq n$.

a) Prove that if X is compact or a Zariski space, then the cohomology $H(X, F)$ commutes with inductive limits of sheaves. This allows us to reduce the problem to the case where F is generated by a finite number of sections over open sets U_i, i.e. F is a quotient of $\sum_{i=1}^n \mathbf{Z}_{U_i}$ (where $\mathbf{Z}_U$ is the zero sheaf on $\complement U$ and $\mathbf{Z}$ on U). — Use induction on n, assuming the theorem proved up to dimension $n - 1$, and proving it for X of dimension $\leq n$.

b) Using your trick, reduce to irreducible X.

c) If X is irreducible, then $H^i(X, \mathbf{Z}_U) = 0$ for any open set U and $i > n$. Indeed, setting $Y = \complement U$, the exact sequence $0 \to \mathbf{Z}_U \to \mathbf{Z} \to \mathbf{Z}_Y \to 0$ gives $H^{i-1}(X, \mathbf{Z}_Y) \to H^i(X, \mathbf{Z}_U) \to H^i(X, \mathbf{Z})$; the first term $= H^{i-1}(Y, \mathbf{Z})$ vanishes by the induction hypothesis, and the last term vanishes because X is irreducible.

d) To show that $H^i(X, F) = 0$ when $i > n$ and F is generated by a finite number N of sections, reduce the problem by induction on N to the case $N = 1$, i.e. $F = \mathbf{Z}_U/R$. Using the exact sequence and c), one is then reduced to proving that $H^i(X, R) = 0$ when R is a subsheaf of $\mathbf{Z}_U$. By a) one can assume that the subsheaf is generated by a *finite* number of *constant* sections over open sets $U_i \subset U$.

e) If F is such a subsheaf of $\mathbf{Z}_U$, then at every $x \in X$, F_x is a subgroup of $\mathbf{Z}$ generated by a generator $d_x \geq 0$, and one can assume that there exists some $d_x \neq 0$ (otherwise $F = 0$!); let d be the least of the $d_x > 0$ and x a point such that $d_x = d$, then $d_y = d$ in some open neighborhood V of x. Consider the injection $\mathbf{Z}_V \to F$ which is multiplication by d over V; this gives an exact sequence $0 \to \mathbf{Z}_V \to F \to F/\mathbf{Z}_V \to 0$. The quotient sheaf is supported on $\complement V$, which is of dimension $\leq n - 1$; the cohomology exact sequence then gives the desired result, using c) and the induction hypothesis.

I can show (using the spectral sequence for global Ext) that if X is an algebraic variety without singularities of dimension $\leq n$, then the coherent algebraic sheaves form an abelian class of finite "dimension", at any rate $\leq 2n - 1$. It would be nice to be able to replace this by n! One is led to prove that if A is a regular local ring and M and N are two finitely generated modules over A, then the dimension of the "support" of $\mathrm{Ext}_A^i(M, N)$ is $\leq \max(0, \nu - i)$, where $\nu = \dim.\mathrm{supp}\ N$. (Moreover, using your dévissages, one is reduced to the case $M = A/\mathfrak{p}$, $N = A/\mathfrak{q}$, for prime ideals $\mathfrak{p}$ and $\mathfrak{q}$). Is it at least always

true that
$$\text{dim.supp. } \text{Ext}^i_A(M, N) \leq n - i,$$
if n is the Krull dimension of A? If you happen to know any information at all about Ext^i, I would be interested! Yours,

A. Grothendieck

February 2, 1956 Jean-Pierre Serre

Dear Grothendieck,

Here is the answer to the question you asked me on dim.Supp. of Ext. Let A be a regular local ring of dimension n, and let $\mathfrak{p}$ be a prime ideal of A, of dimension r; then one knows J.-P. Serre : I had proved in [**Se56b**] that:

A regular $\Rightarrow A_{\mathfrak{p}}$ regular for every prime ideal $\mathfrak{p}$ of A. (cf. my Bourbaki draft) that $A_{\mathfrak{p}}$ is a regular local ring of dimension $n - r$. Now, if M and N are two A-modules of finite type, it is easy to prove (using the fact that $A_{\mathfrak{p}}$ is A-flat) the formula:

$$\mathrm{Ext}_A^q(M, N) \otimes_A A_{\mathfrak{p}} = \mathrm{Ext}_{A_{\mathfrak{p}}}^q(M \otimes_A A_{\mathfrak{p}}, N \otimes_A A_{\mathfrak{p}}),$$

and by the syzygies theorem, the right-hand side is trivial if $q > n - r$, i.e. if $r > n - q$. But if Q is an arbitrary A-module, then one knows that $\mathfrak{p} \in \mathrm{Supp}(Q)$ is equivalent to $Q \otimes_A A_{\mathfrak{p}} \neq 0$. It follows that the support of $\mathrm{Ext}_A^q(M, N)$ is of dimension $\leq n - q$, which proves one of the conjectures you made on the said support.

Similarly, taking $M = N = A/\mathfrak{p}$ and $q = n - r$, one finds

$$\mathrm{Ext}_A^{n-r}(A/\mathfrak{p}, A/\mathfrak{p}) \otimes_A A_{\mathfrak{p}} \neq 0,$$

which makes it possible to easily construct counterexamples to the first of your conjectures.

Is the first formula enough for you to prove that the class of coherent sheaves is of dimension $\leq n$?

On the subject of bundles whose group is the projective group: I am now practically certain that if this group is embedded into the linear group, there is no rational section. I have finally understood the Rosenlicht-type theorems on the existence of sections; they are entirely specific to the multiplicative and additive groups, and depend on the fact that if L/K is a Galois extension with group G then $H^1(G, L) = 0$ and $H^1(G, L^*) = 0$; this is false for other groups J.-P. Serre : Non-commutative Galois cohomology makes its appearance. (the projective group, for example, essentially gives the Brauer group of K), but is on the other hand true for the linear group and the unimodular group. I will tell you about this in more detail another time.

I have no comments on the rest of your letter (except that I am delighted to see this "hypercohomology" yield some concrete results in algebraic geometry — Weil will be furious!), because I have hardly had the time to study it in detail. I am busy with my blasted analytic paper J.-P. Serre : "my blasted analytic paper ": [**Se56a**]. (I write horribly slowly), and at the same time, I am trying

to understand the pretty things Lang is doingJ.-P. Serre : "the pretty things Lang is doing... ". Cf:

S. Lang, *Algebraic groups over finite fields*, Amer. J. Math. **78** (1956), 555–563;

" , *Unramified class field theory over function fields in several variables*, Ann. Math. **64** (1956), 285–325. with abelian varieties. But keep sending me your news (and remind Cartan to send me his Seminar: I only have n°s 1–5).

Yours,
J-P. Serre

February 16, 1956 ALEXANDRE GROTHENDIECK

My dear Serre,

Thank you for the information on the Ext. I can prove the following for arithmetic varieties obtained by gluing together spectra of commutative Noetherian rings: J.-P. Serre : "arithmetic varieties obtained by gluing together spectra of commutative Noetherian rings". Here one sees scheme theory taking shape: all that is missing is the name, which was in fact already being used by Chevalley and Nagata. Let X be such a variety; Cartier defines quasi-coherent algebraic sheaves on X, which are technically very handy because they have certain properties of coherent sheaves but are not required to satisfy any finiteness property (in the affine case, they correspond to *all* modules over the coordinate ring, not just finitely generated modules); thus the quasi-coherent sheaves are stable under direct image by a morphism, and to check that a quasi-coherent sheaf is coherent it is enough to check that its sections over a sufficiently small open affine set form a finite module over the coordinate ring of the latter. This said, one can show that in the abelian class of quasi-coherent sheaves on X there are enough injectives, that the Ext of two quasi-coherent sheaves in this class is indeed the one given by the theory of arbitrary sheaves of $\mathcal{O}$-modules [denoted $\mathrm{Ext}^p_{\mathcal{O}}(X, F, G)$], and finally, that this class is of cohomological dimension $\leq n$ if and only if the local rings (at places of dimension 0) are of cohomological dimension $\leq n$. None of this is difficult, but it needs a little care. — Using the Leray spectral sequence, I can show that if F is a coherent algebraic sheaf on a complete variety X then $H^1(X, F)$ is finite-dimensional. For this, I use the fact that there exists a regular birational map from a projective variety X' onto X (Chow) and that by your dévissage, J.-P. Serre : "by your dévissage": this is a reference to [**Se57**], which Grothendieck interprets as a method of dévissage, cf. the letter of March 8, 1956. it is enough to find *one* coherent sheaf F on X with support X for which the theorem holds: I take the image by f of the sheaf $\mathcal{O}_{X'}$; it is not difficult to prove that it is coherent (true whenever f is "proper" i.e. "X' is complete over X" in the sense given in the Chevalley-Cartan seminar). The Leray spectral sequence shows that $H^1(X, F) \subset H^1(X', \mathcal{O}_{X'})$, and the result is in the bag. To prove the same result for H^i for any i, this method reduces the problem to showing that the sheaves F_q which occur in the Leray spectral sequence are coherent; the sections of F_q over an open affine set $U \subset X$ are precisely $H^q(U', \mathcal{O}_{X'})$ where $U' = f^{-1}(U)$. But I only know that they are *quasi*-coherent (as I said above). The Leray spectral sequence also enables us to show that an algebraic variety which is a locally trivial fibration over an affine base space with affine

fibers is affine (via your cohomological characterization of affine varieties). I have also set up a variant of the open cover spectral sequence, in the case of finite groups with fixed-point free actions, for both additive and multiplicative sheaves $\mathcal{O}^*$. In the additive case (quasi-coherent algebraic sheaves) one finds exactly what one expected to, but for $\mathcal{O}^*$ it is markedly different from the classical situation.

Cartan and Mac Lane are all excited about the "cobar-construction" suggested by an algebraic construction of Cartier's. In principle, starting with a graded differential "coalgebra" (i.e. a module with an associative diagonal map) this should give a graded differential "hyperalgebra" (i.e. something that is both an algebra and a coalgebra), and starting with the singular complex of a module J.-P. Serre : "of a module" is probably a slip for "of a space" . should give us the singular complex of the loop space (roughly); in any case, using heuristic computations, Cartan has already been able to recover the homological structure of the loop space and the loop space of the loop space of the sphere (for the latter, he has rediscovered a complex that was already found last year by Hilton, I believe) but this formalism is not yet refined enough to yield $\pi_6(\mathbf{S}^3)$. — All in all, I don't really know whether I am going to throw myself into these constructions as well, or study algebraic geometry more deeply. — By the way, going back to your method for the Plücker formulas, J.-P. Serre : "your method for the Plücker formulas": I do not see what Grothendieck is alluding to here (it is possible that I had explained to him that these formulas can be proved using computations on cycles in a Grassmannian). given a "proper" regular map from a variety X onto a variety X', with discrete fibers, it is immediate to reduce the classification of algebraic bundles (with structural group) over X' to the classification over X, hence in principle the classification of ordinary bundles over a curve of genus 0 which may have multiple points. But I haven't yet looked at what this might give for the classification over $\mathbf{P}_2$. — I would be very interested in having details of your interpretation of Rosenlicht's result on the existence of sections. Yours,

A. Grothendieck

P.S. What kind of a joke is it to have me invited to the Algebraic Topology conference in Louvain!

March 8, 1956 ALEXANDRE GROTHENDIECK

Dear Serre,

It should be possible to condense the arguments for theorems 2 and 3 of your draft J.-P. Serre : This is another reference to [**Se57**]. on algebraic sheaves to the following statement: let X be an algebraic variety (or even a finite-dimensional arithmetic variety), $\mathcal{C}$ the class of coherent algebraic sheaves on X, F^* a cohomological functor in degrees ≥ 0 on $\mathcal{C}$ (with values in an arbitrary abelian class $\mathcal{C}'$; it is enough for F to be defined in degrees $\leq N$); let us be given, for every d-dimensional irreducible subvariety Y^d of X, a torsion-free coherent algebraic sheaf $\mathcal{A}_{Y^d}$ on Y_d with support Y^d; then for every $\mathcal{A} \in \mathcal{C}$ and every integer $k \leq N$, $F^k(\mathcal{A})$ belongs to the "smallest "thick" subclass" of $\mathcal{C}$ which contains all the $F^i(\mathcal{A}_{Y^d})$ for $k - (n-d) \leq i \leq k$. A "thick" subclass corresponds to what you call a class of groups in your paper on classes of abelian groups. J.-P. Serre : "your paper on classes of abelian groups": [**Se53a**]. There is an alternative version of this statement for homological functors, never mind. It contains both of the above-mentioned theorems, and also: if F is a left exact functor, then $F(\mathcal{A})$ belongs to the thick class generated by the $F(\mathcal{A}_{Y^d})$, so that to check that $F(\mathcal{A})$ always belongs to a thick subclass, it is enough to check that for every $Y^d \subset X$ there is at least *one* torsion-free coherent algebraic sheaf on Y^d with support Y^d for which this holds. A first example of this is the case of the functor H^0, and a second one is the case where one takes a regular map f from X into a variety Y, and set $F(\mathcal{A}) = $ the direct image of $\mathcal{A}$ by f (with values in the algebraic sheaves on Y). To check that $f_*(\mathcal{A})$ is always *coherent* it suffices for example to check that the $f_*(\mathcal{O}_{Y^d})$ are coherent, which is immediate if f is *proper*. (Another application is the proof that $H^1(X, \mathcal{A})$ is finite-dimensional if $\mathcal{A}$ is coherent over a complete variety X).

Let G be a group which acts by homeomorphisms on a topological space X (without additional hypotheses for the moment; G doesn't necessarily act faithfully, in fact the case where G acts trivially is particularly interesting). Then the notion of a sheaf on X with G-action is obvious; let us call such a sheaf a G-X-sheaf (of abelian groups, implicitly); such sheaves form an abelian class $C^G(X)$ with enough injectives. The functor Γ_X (sections) takes values in C^G, the class of G-modules, so the same holds for the derived functors $H^p(X, F)$; now let $\Gamma_X^G = \Gamma^G \Gamma_X$ be the functor $F \mapsto \Gamma_X(F)^G$ which takes $C^G(X)$ to the class of abelian groups; this is a left exact functor, whose derived functors are denoted $H^p(X, G; F)$. Let $Y = X/G$ with the quotient topology; let G act trivially on Y. Then $C^G(Y)$ is the class of sheaves on Y

on which G acts via sheaf automorphisms, or alternatively sheaves of $\mathbf{Z}(G)$-modules, where $\mathbf{Z}(G)$ is viewed as a constant sheaf of rings on Y. Thus, here, $H^p(Y, G; A)$ is just $\text{Ext}^p_{\mathbf{Z}(G)}(Y, \mathbf{Z}, A)$, where $\mathbf{Z}$ is viewed as a G-sheaf on Y in an obvious way; it is therefore the abutment of a spectral sequence whose first term is $'E_2^{pq} = H^p(Y, \mathcal{H}^q(G, A))$, where $\mathcal{H}^q(G, A) = \mathcal{E}xt^q_{\mathbf{Z}(G)}(\mathbf{Z}, A)$, which I already talked to you about in a more general context; in the good cases (such as when G is finite), one has $\mathcal{H}^q(G, A)_y = H^q(G, A_y)$ at every point $y \in Y$. – Going back to a G-X sheaf F on X, its direct image $f_*(F)$ on Y is a G-Y sheaf. Let me recall the definition of the $f_*^q(F)$ (which arise in the Leray spectral sequence of the continuous map f); it is the sheaf associated to the presheaf $U \mapsto H^q(f^{-1}(U), F)$ on Y. The fundamental theorem on the cohomology of open covers can now be stated as follows:

Let $F \in C^G(X)$, and assume that for $q > 0$, $f_*^{(q)}(F)$ vanishes [in fact, it is enough for the $H^p(Y, G; f_*^{(q)}(F))$ to vanish]; then

$$H^*(X, G; F) = H^*(Y, G; f_*(F))$$

functorially, and furthermore, $H^*(X, G; F)$ is the abutment of a spectral sequence whose first term is $E_2^{pq} = H^p(G, H^q(X, F))$ [and also by what was said above, of a spectral sequence whose first term is $'E_2^{pq} = H^p(Y, \mathcal{H}^q(G, f_*(F)))$].

The hypothesis above is automatically satisfied if G acts trivially; thus, if A is a G-Y sheaf on the space Y, $H^*(Y, G; A)$ is in fact the abutment of *two* spectral sequences: firstly the sequence recalled above and secondly a sequence starting with $E_2^{pq} = H^p(G, H^q(Y, A))$; the fundamental theorem only appears to be more general, (at least under the assumption that $f_*^{(q)}(F) = 0$ for $q > 0$) since its two spectral sequences are in fact the two spectral sequences of the G-Y sheaf $f_*(F)$, bearing in mind that $H^*(X, F) = H^*(Y, f_*(F))$ (thanks to the Leray spectral sequence). — The hypothesis is also satisfied if the following condition (D) holds: Every $x \in X$ has a saturated neighborhood U such that for every $s \in G$ not in the stabilizer G_x of x, one has $s \cdot U \cap U = \emptyset$ (this is the usual condition to express the fact that G is "discrete"). J.-P. Serre : "G is discrete". This means that G, equipped with the discrete topology, acts *properly*, in Bourbaki's sense of the word, T.G. III.32.

Obviously, a condition of this sort is not satisfied if X is a Zariski space; however, for "good" sheaves F, it will still be true that $f_*^{(q)}(F)$ vanishes, as we will see. — Of course, if (D) holds and if furthermore the action of G is fixed-point free, J.-P. Serre : "fixed-point free" = acts freely (in other words, no element of the group except the identity has any fixed point). then the second spectral sequence is trivial, and one gets $H^p(X, G; F) = 'E_2^{p0} = H^p(Y, f_*^G(F))$, where $f_*^G(F)$ is the sheaf on Y given by $U \mapsto \Gamma(f^{-1}(U), F)^G$: then (up to

notation) one recovers the Cartan-Leray spectral sequence ending with the cohomology of $Y = X/G$.

Now let X be a normal algebraic or arithmetic variety (in particular, what follows applies to number fields, or more generally, to the spectra of normal rings), G a finite group of automorphisms of X such that any orbit of G is contained in an open affine set; one can then define a natural variety structure on $Y = X/G$, whose topology is the quotient topology: If $\mathcal{O}_X$ is the sheaf of local rings of X, then the sheaf of local rings of Y is $f_*^G(\mathcal{O}_X) = \mathcal{O}_Y$. It is clear what is meant by an algebraic sheaf on X on which G "acts" ($\mathcal{O}_X$ itself, for example, or more generally a sheaf defined as the inverse image of a vector bundle on Y, sheaves of differential forms, etc.); moreover, by the theory of affine varieties, the condition $f^{(q)}(F) = 0$ is satisfied if F is coherent, whence the two spectral sequences abut to the same group, $H^*(X, G; F)$. I had written you that if the action of G is fixed-point free, then one gets the same thing as in the classical case, i.e. $H^*(X, G; F)$ is identified with $H^*(Y, f_*^G(F))$; this would follow from the formula $H^p(G, f_*(F)_y) = 0$ if $p > 0$, but I realize that I did not give a proof of this in my papers, and I can't seem to improvise one; if $F = \mathcal{O}_X$, this means that if G is a group of automorphisms of a semi-local ring $\mathcal{O}$ which acts simply transitively on its maximal ideals, then $H^p(G, \mathcal{O}) = 0$ for all $p > 0$. I'll eat my hat if this is false! — If one now takes $F = \mathcal{O}_X^*$, then the plot thickens. It is still easy to show that $f_*^{(q)}(F) = 0$ when $q > 0$. Setting $\mathcal{O}_Y' = f_*(\mathcal{O}_X)$ (if X is irreducible, then $\mathcal{O}_Y'$ is the sheaf of integral closures of local rings of Y in the field of rational functions on X) then $f_*(\mathcal{O}_X^*) = \mathcal{O}_Y'^*$, where $\mathcal{O}_y'^*$ is the multiplicative group of invertible elements of the ring $\mathcal{O}_y'$. Set $L^i = H^i(X, G; \mathcal{O}_X^*) = H^i(X, G, \mathcal{O}_Y'^*)$ for short. Assume that X is factorial (i.e. the local rings are factorial): then $H^i(X, \mathcal{O}_X^*) = 0$ for $i \geq 2$; furthermore $H^0(X, \mathcal{O}_X^*) = A_X^*$, the multiplicative group of invertible elements of the ring A_X of regular functions on X, and $H^1(X, \mathcal{O}_X^*) = P_X$, the group of divisor classes on X. The first spectral sequence gives us the ("Gysin") exact sequence:

$$0 \to H^1(G, A_X^*) \to L^1 \to P_X^G \to H^2(G, A_X^*) \to L^2 \to H^1(G, P_X) \to \cdots$$
$$\cdots \to H^n(G, A_X^*) \to L^n \to H^{n-1}(G, P_X) \to \cdots$$

Actually, nothing prevents us from considering the following "flasque" (but non-injective, since it is precisely by use of an injective resolution that one defines the spectral sequence above) resolution of $F = \mathcal{O}_X^*$:

$$0 \longrightarrow \mathcal{O}_X^* \longrightarrow \mathcal{R}_X^* \longrightarrow \mathcal{D}_X \longrightarrow 0$$

($\mathcal{R}^* =$ germs of invertible rational functions; $\mathcal{D}_X =$ germs of divisors), transforming it by Γ_X to obtain a G-complex, and passing to the associated double complex, whose cohomology is exactly L^*, as can be seen using one of the two spectral sequences of this double complex; as for the other spectral sequence, its E_2^{pq} term is the cohomology in dimension p of the complex $0 \to H^q(G, R_X^*) \to H^q(G, D_X) \to 0 \to 0 \cdots$, which gives another exact sequence involving L_i:

$$0 \to A_X^{*G} \to R_X^{*G} \to D_X^G \to L^1 \to H^1(G, R_X^*) \to H^1(G, D_X) \to$$
$$\to L^2 \to H^2(G, R_X^*) \to H^2(G, D_X) \to L^3 \to \cdots$$

(set $R_X =$ field of rational functions on X, and $D_X =$ group of divisors on X); in particular, one finds $L^1 = D_X^G / \mathrm{Im}(R_X^G)$.

(These two exact sequences are amusing when X is the spectrum of a number field $R = R_X$; A_X is then the ring of algebraic integers, A_X^* the group of units, and P_X the group of ideal classes; consequently, the first exact sequence implies that the L^i are *finite* groups, and the second thus implies that $H^i(G, R_X) \to H^i(G, D_X)$ is bijective modulo finite groups; this can be seen very simply using only the exact sequence of G-groups $0 \to A^* \to R^* \to D \to P \to 0$ — since for the moment that is all there is).

Finally, L^* is also the abutment of a spectral sequence whose first term is $E_2^{pq} = H^p(Y, \mathcal{H}^q(G, \mathcal{O}_Y'^*))$. As far as I have been able to see up to now, there is no way of replacing this spectral sequence by an exact sequence except when Y is a curve, so that E^{pq} vanishes for $p \geq 2$; in this case one obtains the exact sequences $0 \to H^1(Y, \mathcal{H}^{n-1}(G, \mathcal{O}_Y'^*)) \to L^n \to H^0(Y, \mathcal{H}^n(G, \mathcal{O}_Y'^*)) \to 0$. Nor have I checked out what the $H^p(G, \mathcal{O}_Y'^*)$ look like; the only thing I know is that if the action of G is fixed-point free (i.e. is simply transitive on the maximal ideals of the semi-local ring $\mathcal{O}_y'$, more generally if the stabilizer of an $x \in X$ acts trivially J.-P. Serre : "acts trivially": a slip of the pen for "acts faithfully". on the residue field at the point x), then $H^1(G, \mathcal{O}_y'^*) = 0$, so $E_2^{pq} = 0$ if $q = 1$. Another bit of partial information, if Y is factorial: $E_2^{p0} = 0$ if $p \geq 2$, since $E_2^{p0} = H^p(Y, \mathcal{O}_Y^*)$. If G acts without fixed points, this enables us to compute some of the L^i:

$$L^1 = P_Y, \quad L^2 = H^0(Y, \mathcal{H}^2(G, \mathcal{O}_Y'^*)),$$

and finally, $H^1(Y, \mathcal{H}^2(G, \mathcal{O}_Y'^*))$ is a subgroup of L_3, and the quotient is the kernel of the transgression homomorphism J.-P. Serre : "transgression homomorphism": This is not a "transgression" in the usual sense of the word. It is merely one of the differentials of the spectral sequence.

$$H^0(Y, \mathcal{H}^3(G, \mathcal{O}_Y'^*)) \mapsto H^2(Y, \mathcal{H}^2(G, \mathcal{O}_Y'^*)).$$

I have no idea whether or not this spectral sequence is actually usable: I showed it to Chevalley, but it didn't seem to inspire him!

I have gone back to the classification of analytic bundles over the Riemann sphere J.-P. Serre : "the classification of analytic bundles over the Riemann sphere": see[**Gr57a**]. with semi-simple structural group, and I have more or less proved my conjecture, which I now prefer to state as a duality theorem. —Do your constructions also show that over a complex algebraic projective variety, the analytic and algebraic classification of bundles, for example with structural group $SO(n)$, are not the same?

Yours,

A. Grothendieck

March 14, 1956 Jean-Pierre Serre

Dear Grothendieck,

Thank you for your letter. I am a bit panic-stricken by this flood of cohomology, but have borne up courageously. Your spectral sequence seems reasonable to me (I thought I had shown that it was wrong in a special case, but I was mistaken, on the contrary it works remarkably well). In any case, set your mind at rest, it is indeed true that

$$H^q(G, M) = 0 \qquad \text{for all } q > 0 \,,$$

when G is a finite group of automorphisms of an A-module M, where A is a semi-local ring (in fact an algebra over a field k such that all the $A/\mathfrak{m}$ for maximal $\mathfrak{m}$ are equal to k) on which G acts in such a way that the induced permutation on the maximal ideals is "fixed-point free". Proof: first show that there is an element $t \in A$ whose trace $\sum g \cdot t$ is 1 (take an element whose trace is 1 modulo the radical of A, and divide it by its trace, which is invertible); from this point on it is entirely formal: it is for example possible to show that every map $f : G \to M$ has an "average" $I(f)$, in the sense given in my paper with Hochschild in the Transactions (n°6 unless I am mistaken): simply write :

$$I(f) = \sum_{g \in G} (g \cdot t) f(g) \qquad \text{where } t \text{ is such that } \sum g \cdot t = 1 \,.$$

(This result actually plays an important role in the construction of coverings of a variety).

There are probably heaps of other possible proofs of this result, for example by passing to the completed semi-local ring B, and using the fact that B is a direct composition J.-P. Serre : "direct composition": direct product. of local rings which are permuted transitively by G, whence $H^q(G, B)$ always vanishes. Etc.

Congratulations on your classification of analytic bundles over the Riemann sphere with semi-simple structural group. How do you do that? I suppose that at the same time you show that they are actually algebraic? (For information on the relationship between the algebraic and analytic classifications, I refer you to my paper J.-P. Serre : "my paper" : [**Se56a**].; Cartan has a copy which he should give you if he hasn't done it already). Here Atiyah and Nakano are getting excited about putting some kind of "algebraic or analytic structure" on classes of vector bundles. Atiyah can more or less do it when the base is a curve and one only considers indecomposable bundles of given degree and

dimension. J.-P. Serre : See M.F. Atiyah, *Vector bundles over an elliptic curve*, Proc. London Math. Soc. **7** (1957), 414–452 (= *Coll. Works*, vol.1, no7). Furthermore, he can completely classify vector bundles over a curve of genus 1 (using a method analogous to the one you used for genus 0).

Here is an entertaining little resultJ.-P. Serre : "an entertaining little result": S.S. Chern, F. Hirzebruch and J-P. Serre. *On the index of a fibered manifold*, Proc. A.M.S. **8** (1957), 587–596. (proved by Chern, Hirzebruch and myself): the index of a fiber bundle is equal to the product of the indices of the base and the fiber [if $\pi_1(B)$ acts trivially on $H(F)$]; here the index is in the sense of Thom.

At the moment, I am mostly getting excited about p and p^n coverings, J.-P. Serre : "p and p^n coverings": see [**Se58a**], n^{os}17–18. of which I am beginning to have a relatively complete theory. But loads of things still remain mysterious (especially concerning abelian varieties). I will tell you about this in Paris.

Yours,

J-P. Serre

April 10, 1956 ALEXANDRE GROTHENDIECK

My dear Serre,

I have taken advantage of the holidays to finally finish off the classification of analytic bundles on the Riemann sphere with complex semi-simple structural group G (which may or may not be connected; one then sees that the result still holds if G is only reductive, but is false for the one-variable affine group $x \mapsto ax + b$). If T is a maximal complex "torus" of G, N its normalizer, $W = N/T$ the Weyl group, one finds: every bundle E can be reduced to the structural group T, and this reduction is unique up to operation of W. As the classes of bundles with group T can be identified with $H^2(X, \Pi) = \Pi$, where $\Pi \simeq \mathbf{Z}^r$ is the unit lattice of the Lie algebra $\mathfrak{h}$ of T, this does indeed give a complete classification; if G is connected, for example, the desired classes correspond to the elements of the lattice Π contained in a Weyl chamber. — Another, more intrinsic way of expressing this result is as follows: let E_0 be the line-bundle on X (with group $\mathbf{C}^*$) whose Chern class is $+1$, then every bundle whose group is G is the bundle associated to E_0 and to a holomorphic map from $\mathbf{C}^*$ into G; the latter is determined up to inner automorphisms of G. — Firstly, here is how to prove *uniqueness*: consider a homomorphism φ from $\mathbf{C}^*$ to G, and let E be the associated bundle with group G, then, for any linear representation u of G the vector bundle associated to E and to u is also the vector bundle associated to E_0 and $\psi = u\varphi$, hence the latter depends only on E; however we already know that the class of ψ modulo inner automorphisms of the linear group is entirely determined by the vector bundle that it defines. Thus, if φ and φ' define isomorphic vector bundles E and E', then for any linear representation u of G $u\varphi$ and $u\varphi'$ are equivalent linear representations of $\mathbf{C}^*$; it is now a problem of pure Lie group theory to show that φ and φ' are conjugate by an inner automorphism of G. This is not hard to prove (and is probably well known). Let us now show that it is *possible to restrict* the structural group to T. In general, let X be a compact analytic variety, G a complex Lie group, P an analytic bundle over X with group G, and E the associated adjoint vector bundle whose fiber is the Lie algebra $\mathfrak{g}$ of G; let s be a holomorphic section of E and assume that at *some* point $x \in X$, $s(x)$ is a *regular* element of the Lie algebra $\mathfrak{g}(x)$. As the coefficients of the characteristic polynomial of $ad(y)$ are holomorphic functions of y, so constant, it follows that $s(y)$ is regular for *all* y. Let $\mathfrak{h}(y)$ be the Cartan subalgebra of $\mathfrak{g}(y)$ which is the centralizer of $s(y)$, then $\mathfrak{h}(y)$ varies holomorphically with y; in other words, the structural group has just been reduced to the normalizer N of the analytic group H

which corresponds to the Cartan algebra $\mathfrak{h}$; *if X is simply connected*, the structural group can even be reduced to T, since N/T is discrete. Let us prove that the initial condition (the existence of s) is satisfied if X is the Riemann sphere and E is *semi-simple*. Then E is actually an orthogonal bundle (Killing form!). It is known that E is isomorphic to a sum of line bundles; let E_n be the sum of those components whose Chern class is $\geq n$ (E_n can be defined intrinsically as the bundle generated by meromorphic sections whose divisor is of degree $\geq n$). Obviously, $[E_n, E_{n'}] \subset E_{n+n'}$, whence it follows that E_1 is a bundle of Lie *subalgebras* and even (denoting the fiber of E_n by $\mathfrak{g}_n$) that $\mathfrak{g}_1$ is a subalgebra of $\mathfrak{g}$ whose elements have nilpotent adjoint actions in $\mathfrak{g}$; in other words, $\mathfrak{g}_1$ is contained in the maximal nilpotent ideal $\mathfrak{n}$ of a maximal solvable subalgebra $\mathfrak{r}$ of $\mathfrak{g}$. Moreover, it follows from the classification of orthogonal bundles that the subbundle of E orthogonal to E_1 is E_0; the fiber $\mathfrak{g}_0$ of E_0 is therefore orthogonal to $\mathfrak{g}_1$, and thus it contains $\mathfrak{r}$ (since $\mathfrak{r}$ is orthogonal to $\mathfrak{n}$ which contains $\mathfrak{g}_1$). Since $\mathfrak{r}$ contains a regular element, $\mathfrak{g}_0$ *contains a regular element.* This is exactly what had to be proved.

I have also continued trying to understand the second spectral sequence of spaces with operators (in the case with fixed points, of course); in some important cases it can be replaced by an exact sequence. Sparing you the details (since I will probably see you shortly) I will just point out that one very easily recovers Smith's theorem on the fixed points of a sphere, as well as the following result of the same type (probably known to experts): if X is a finite-dimensional space, acyclic modulo p (a prime number), on which a group G of order p acts, then the set of fixed points is also acyclic modulo p, as moreover is the quotient space X/G. Furthermore, as X and X/G have the same cohomology in characteristic different from p, it follows that if X is acyclic over the integers, then so is X/G (and the result then obviously extends to a group G which is only assumed to be solvable). — As for the existence of points fixed by *all* elements of G, I have the impression that this cohomological method cannot give anything, since if G is of order p there is no way of showing that F is acyclic *also* in characteristic different from p (this appears to be true, however, but is certainly a deeper result) J.-P. Serre : "this appears to be true, however". This is false, as was later shown. The "coprime to p" part of the cohomology of F can be more or less anything, even if X is a Euclidean space or a simplex. For more information, see:

L. Jones, *The converse of the fixed point theorem of P.A.Smith* I, Ann. Math. **94** (1971), 52–68.

R. Oliver, *Fixed point sets of actions on finite acyclic complexes*, Comment. Math. Helv. **50** (1975), 155–177.

When exactly are you coming back? Yours,

A. Grothendieck

July 23, 1956 Alexandre Grothendieck

My dear Serre,

I was just thinking about "analytic geometry", and I think I have been able to prove that "algebraic=analytic" for algebraic coherent sheaves on a complete compact algebraic variety, J.-P. Serre : See the Cartan Seminar 1956–1957, exposé no2. not assumed to be projective. It's the usual kind of dévissage, using your general dévissage lemma, Chow's lemma, $\mathcal{O}(n)$ on projective space and the Leray spectral sequence. But to write it up I would need to refer to your article several times. Can you lend me a copy (I returned the one that Cartan lent me)? As for embedding a complete algebraic variety in a projective space, I confess that I still don't see how to do it.J.-P. Serre : Hironaka has shown that there exist non-singular complete varieties which cannot be embedded into projective space (cf. R. Hartshorne, *Algebraic Geometry*, Appendix B, Example 3.4.1.). Obviously, if the problem were to be solved shortly, it would be a bit disappointing to write a supplement to your analytic diplodocus, which would then be killed!

I leave for Germany tomorrow, for a week.

Yours,

A. Grothendieck

P.S Thank you for the hint in your last letter!

September 1, 1956 ALEXANDRE GROTHENDIECK

My dear Serre,

I have spent most of the last month writing up my multiplodocus on homological algebra; I have tried to be concise, but even though there are practically no proofs, it will come to more than 100 large pages (of which 80 are written up). Have you any suggestions as to where I should publish it? (not in France, where I am already publishing my long and wretched "Fredholm theory"). J.-P. Serre : "my long and wretched 'Fredholm theory' ": [**Gr56b**]. Actually, it might not be such a bad idea to have it distributed to Bourbaki, like Godement's sheaves draft, so that it could be taken into account in the upcoming draft on Homological Algebra. What do you think? Here is the plan:

I Abelian classes;

II Homological algebra in abelian classes (which contains several useful complements to Cartan and Sammy's book);

III Sheaf cohomology (including Čech, the Leray spectral sequence of a continuous map, and as an added bonus, the theorem on the cohomological dimension of Zariski spaces and lifting of projective algebraic bundles, which will be given in all necessary generality);

IV Ext groups of sheaves of modules $\text{Ext}^*_{\mathcal{O}}(X; A, B)$, including the two spectral sequences and conditions for them to be trivial;

V Spaces with operators: the two spectral sequences for $H^*(X; G, A)$, the explicit spectral sequence $H^p(X/G, \mathcal{H}^q(G, A))$, the case of a "discrete" group action, $\text{Ext}^*_{\mathcal{O},G}(X; A, B)$, (where $\mathcal{O}$ is a sheaf of rings with G-action, and A and B are two sheaves of $\mathcal{O}$-modules with G-actions, the Ext being taken in the category of such things): there are three spectral sequences (and sometimes even four or five) which abut there; the first shows that if A is locally free of finite type the Ext is none other than $H^*(X; G, \mathcal{H}om(A, B))$ (which gives, for example, the classification of extensions of analytic (or algebraic) vector bundles with transformation group G as an $H^1(X; G, \mathcal{H}om(E, F))$).

If it doesn't become too long, I will also give the definition of Steenrod powers, which arise naturally in this context by adapting Godement's approachJ.-P. Serre : "Godement's approach". In the beginning of the 50's, Godement had written up some notes on Steenrod operations which were never published. for singular theory: if A is a sheaf of vector spaces over a space X, and G is a subgroup of the symmetric group $\mathfrak{S}_p$, then one defines a canonical homomorphism $\text{St} : H^*(G, \otimes^p H^*(X, A)) \to H^*(X; G, \otimes^p A)$ (where G acts in the obvious way on the tensor products). This can be composed, if desired, with the natural homomorphism $H^*(X; G, \otimes^p A) \to H^*(X; G, S^p A)$ (where $S^p A$ is

the p-th symmetric power of A); since G acts *trivially* on $S^p A$, this last term can be *canonically* identified with $H^*(G, H^*(X, S^p A))$, which gives

$$\text{St} : H^*(G, \otimes^p H^*(X, A)) \to H^*(G, H^*(X, S^p A)).$$

If the ground field has characteristic exactly p, then these two terms can easily be described explicitly using $H^*(G, k)$ and when G is the group of cyclic permutations, then this gives the Steenrod powers (but I have not yet written down the proof of the fundamental properties of these powers). In particular, these operations are obviously defined in the context of abstract algebraic geometry; in this case it is to our advantage to use sheaf-theoretic tensor and symmetric products *over* $\mathcal{O}$, so that coherent sheaves give rise to coherent sheaves. — Still following Godement (modulo the necessary adaptations), these operations (in their original form with $H^*(X; G, \otimes^p A)$) can then be used to compute the cohomology of $(X \times X \cdots \times X)/G$. — In chapter V, I would also have liked to give some "non-classical" examples of abstract algebraic geometry, but I am afraid that it will end up too long.

By the way, I should point out that I have investigated the exact conditions under which, given an integral and integrally closed local ring $\mathcal{O}$, a Galois extension K' of its fraction field K, and the integral closure $\mathcal{O}'$ of $\mathcal{O}$ in K, one has $\text{Tr}_{K'/K} \mathcal{O}' = \mathcal{O}$ (which also means that $\hat{H}^*(G, M) = 0$ for any $\mathcal{O}'$-module M on which G acts in a way which is compatible with its action on $\mathcal{O}'$) (G = Galois group of K' over K). It is *sufficient* for the order e of the inertia group G_i to be invertible in $\mathcal{O}$, and in the case where G_i is invariant in G (for example if G is abelian, or G_d is invariant) this condition is also *necessary* (I haven't seen if this still holds in the general case). This immediately gives the ramification points of an Artin-Schreier extension in the usual form, for example: the existence of an Artin-Schreier generator in $\mathcal{O}'$, i.e. such that $x^p - x \in \mathcal{O}$. This is necessary because $\hat{H}^{-1}(G, \mathcal{O}') = 0$, whence $1 = \sigma x - x$ with $x \in \mathcal{O}'$ (since $\text{Tr}\, 1 = 0$); it is sufficient because it is enough to show that if there is an $x \in \mathcal{O}'$ such that $\sigma x = x + 1$, then $\text{Tr}\, \mathcal{O}' = \mathcal{O}$, i.e. there exists $y \in \mathcal{O}'$ such that $\text{Tr}\, y$ is invertible in $\mathcal{O}$: set $y = x^{p-1}$.

Let me end as usual with some questions. With notation as above, is it always true that $\mathcal{O}$ is unramified in the fields K_i and K_d? This is true if they are Galois over K, and Lang's report appears to suggest that otherwise it is no longer certain. — As messy as it is, Lang's report was very helpful for my understanding what unramified means; I had previously more or less imagined

that it meant that the action of the Galois group on the maximal ideals of $\mathcal{O}'$ is fixed-point free!

Yours,

A. Grothendieck

September 19, 1956 ALEXANDRE GROTHENDIECK

My dear Serre,

Thank you for your letter. The American Journal won't do for my article, J.-P. Serre : The article in question is [**Gr57b**] ("Tôhoku"). since I am already publishing the vector bundles on the Riemann sphere there; nor will the Transactions, since as I didn't adhere to Sammy's very strict editorial taboos, he will want me to retype my manuscript, which I do not intend to do; unless Bourbaki would be interested in having some copies (you did not reply to this question) and "Bastien"J.-P. Serre : The first secretary of Bourbaki in Nancy was called Bastien. This is a reference to her successor, Andrée Vigneron, later Andrée Aragnol. were to do the necessary work. The "mémoires" would be excellent, but will I find a sponsor for getting the manuscript prepared (Belgodère?? the CNRS?). I will see when the semester begins, if I have not found a place for the article before then. — I am indeed going to drop Steenrod powers and spectral sequences in algebraic geometry from the article, especially as I am adding a fairly substantial section on the Čechist computation of $H^n(X; G, A)$. For instance, one finds the following: let $(U_i)_{i \in I}$ be an open cover of X, and assume that G acts on I in such a way that $g \cdot U_i = U_{g \cdot i}$, and that the cohomology of the $U_{i_0 \cdots i_p}$ with coefficients in the given G-sheaf A vanishes in dimension > 0. Then $H^*(X; G, U)$ is the hyperhomology of G with respect to the complex $C(\mathcal{U}, A)$. If for example the action of G on I is fixed-point free (the problem can always be reduced to this case), then $H^*(X; G, A) = H^*(C(\mathcal{U}, A)^G)$ (here *non*-alternating cochains must be used!) which is a most attractive formula, you will admit. For example, let D (which stands for "fundamental domain") be an open set (or a closed set if X is paracompact or G is finite) such that $X = \bigcup g \cdot D$, and assume that the finite intersections of $g \cdot D$ are acyclic; then take $\mathcal{U} = (g \cdot D)_{g \in G}$, and in all reasonable cases the complex $C(\mathcal{U}, A)^G$ is free and of finite type in all dimensions (where A is assumed to be a constant ring): it suffices to assume that the set L of g such that $gD \cap D \neq \emptyset$ is finite. This then gives $H^*(X; G, A)$, if one knows what the fundamental domain looks like — or more precisely, if one knows the nerve of the finite cover induced by $\mathcal{U}$ on D and the map $g^{-1}g'$ in L (whenever $g^{-1}g'$, g' and g are in L). This should give a method for computing the cohomology of a modular group with several variables (in this case $H^*(X; G, A) = H^*(G, A)$ since X is a half-space, hence acyclic) without examining the "fixed points" as in Godement's method. (The latter consists in considering $H^*(G, A)$ as the abutment of a spectral sequence whose first term is $H^*(Y, \mathcal{H}^*(G, A_X))$ [where A_X is the constant sheaf A on X, $\mathcal{H}^n(G, A)$ is the sheaf on $Y = X/G$

whose local groups are the $H^n(G_y, A)$, and G_y is the stabilizer of a point in the fiber y]; this method works in complex dimension 1, since the set of fixed points and the distribution of the stabilizers are simple!). One just needs to be sure that there exists a (closed) fundamental domain D having the desired properties of acyclicity and finiteness; do you know whether this is always true?

I have not finished looking at the Steenrod powers. But it seems to me that everything can be done much more explicitly than I thought purely in terms of the St_p^i; furthermore, the multiplication formula comes out trivially (modulo an irritating sign error which I have not yet exorcised, but I am not yet at the point where sign errors matter); on the other hand I cannot yet see why the St_p^i (if I may say so) vanish for $i \not\equiv 0, 1 \mod (2(p-1))$. In any case, having moved house recently (for a bit of a change) I have not yet had leisure to look at this.

One more question: I have been reviewing class field theory, of which I finally have the impression that I understand the main results (but not the proofs, of course!). But to my shame I have been unable to find the "corollary" stating that all ideals of K become principal in the maximal abelian extension unramified at finite places. If there is a quick explanation of this, I would be most grateful to you for it.

Is there any way of getting hold of Artin's book "Algebraic Numbers and Algebraic Functions" which is out of print? In particular, if you could buy it for me, that would be perfect. By the way, did you pay for Chern's "Topics in Differential Geometry" which you sent me a year ago?

What is the Pontryagin square? (I am in the process of rapidly reviewing Thom's work, which I had to abandon in Kansas as I lacked the basics).

I should point out that the condition "G_d normal in G" in the acyclicity condition in my last letter is entirely superfluous; actually, it is much simpler to understand the non-ramification criterion for Artin-Schreier extensions using the discriminant! Yours,

A. Grothendieck

September 23, 1956 Jean-Pierre Serre

Dear Grothendieck,

You will find enclosed a little paper on the "Hauptidealsatz" J.-P. Serre : Let K be a number field and L its absolute class field (the maximal unramified abelian extension). Let $\mathcal{O}_K$ be the ring of integers of K, and let $\mathcal{O}_L$ be the ring of integers of L. The *Hauptidealsatz* says that the homomorphism $\mathrm{Cl}(\mathcal{O}_K) \to \mathrm{Cl}(\mathcal{O}_L)$ is trivial; in other words, every ideal of $\mathcal{O}_K$ generates an ideal of $\mathcal{O}_L$ which is *principal*. Artin's reciprocity law transforms this statement into the following, which was proved by Furtwängler (*Hamburg Abh.* **7** (1930), 14–36):

$(*)$ Let G be a finite group, and $H = (G, G)$ its derived subgroup. Assume that H is abelian. Then the *transfer* homomorphism $G/H \to H$ est trivial.

Artin and Tate's *Class Field Theory*, Chap.13, §4, contains a cohomological proof of $(*)$, which is inspired by Witt's (non-cohomological) proof in (*Ges. Abh.* no17). Both of these proofs are substantially shorter than Furtwängler's, but I would have liked (and would still like) something simpler. explaining how the theorem can be reduced to an (actually very mysterious) theorem of group theory. This, in fact, is the reduction given by Artin himself in his paper on the subject (Abh. Hamburg, around volume 7-10); if you could find a beautiful cohomological proof of the said theorem it would be so much the better, but everyone has got stuck on it up to now.

About your paper, Sammy has not answered me yet. I am afraid he may have already left New York for India. But I find your objections to publishing in the Transactions idiotic: all that Sammy demands is that a manuscript should be *readable* without intellectual effort, which is the least one can ask. Armed with patience and a little glue, it would surely take you no more than a day to retype the doubtful passages and make your manuscript presentable; can you really not try? (Unless, of course, you find another solution).

As for getting néo-Bastien (whoever she is) to type it up, I have no opinion on the matter; of course, that would allow me to have a copy fairly quickly, which would be very nice, but doesn't she already have enough work with the Bourbaki drafts? You would do better to discuss this question with someone from Nancy — Delsarte, or failing that, Bruhat.

Turning now to your method for the cohomology of a Fuchsian group; it seems to work on taking the fundamental domain to be a classical polygon (choose a sufficiently "general" point and take the fundamental domain to be the set of points which are closer to that point than to any of its transforms — a method patented by Poincaré); the intersections of this polygon with its transformations look to me like they must be either topological segments or

points; I am too lazy to check. (Let me point out however that these groups are free products of very simple groups, and the cohomology of a free product is given by a general theorem of Lyndon).

If you are interested in this stuff on cohomology and automorphic forms, you can look at a paper published recently in the Amer. J. by a student of Bochner's, Gunning, I believe, which is closely related to Godement's talk, but is more "explicit".

Let me answer your other questions:

a) One can show that the St_p^i, $i \not\equiv 0, 1 \mod 2p - 2$ are identically zero by showing that these operations come from homology classes of the cyclic group $\mathbf{Z}/p\mathbf{Z}$ which become 0 in the symmetric group $\mathfrak{S}_p$. In any case, this is Steenrod's point of view.

b) I will see in Princeton whether I can get hold of Artin's Notes Part 1 (the only part that exists) for you, but I am afraid it will not be possible, as Artin is not there. As for Chern's Topics, it seems to me that I did pay about 2 dollars for them.

c) The "Pontryagin square" J.-P. Serre : For the definition of the Pontryagin square, see for example:

L. Pontryagin, Dokl. Akad. Nauk. U.S.S.R. **34** (1942), 35–37;

N.E. Steenrod, Comm. Math. Helv. **31** (1956), 195–218. is a cohomological operation which has always been something of a mystery to me. If I am not mistaken, it sends $H^q(X, \mathbf{Z}/2\mathbf{Z})$ to $H^{2q}(X, \mathbf{Z}/4\mathbf{Z})$. In fact, it has been noticed that there is a whole bunch of operations of the same type, defined modulo p; they can be gotten either by using Eilenberg-Mac Lane or by Steenrod's method with the symmetric group acting on ... etc. Ask Cartan for details: he knows all this very well.

At any rate, one thing is certain: such operations exist only if one goes into cohomology mod $p^2, \ldots$, etc.

d) I am finishing the part of my paper which deals with coverings. J.-P. Serre : "the part of my paper which deals with coverings" : the final sections of [**Se58a**]. As I do not feel like writing up a huge Bourbaki-style thing, I am resigning myself to giving statements without any proofs; it will certainly not be easy to read! The difficulty is partly that I do not restrict myself to normal varieties, like all the bastards in the literature do.

Write to me at Princeton. Yours,

J-P. Serre

P-S. Is this address "c/o Mr. Harari" permanent?

P-S 2. I hope you have received a letter which was sent to you at my place and then forwarded to you?

November 13, 1956 Alexandre Grothendieck

My dear Serre,

Thank you for the various papers that you generously sent me, as well as for your letter. There is no news over here. I have finished writing up the annoying homological algebra draft (but it is the only way I have of understanding, through sheer persistence, how things work) which I have sent to Delsarte, who as it happens did not have enough articles for the typist; I proposed it to Tannaka for the Tôhoku, as apparently they do not mind very long articles. I have read the statements in Weil's books on abelian varieties, in the hope that the proofs might have been improved since he wrote them; they are really discouraging in Weil, and on top of that his language disgusts me. I spend my time either learning or writing up the varieties, which is amusing but very long, of course; research, however, is out of the question before I have swallowed a mountain of new things. You asked me for details on the Steenrod operations. I have not written up anything, and have not continued in this direction, since I do not personally need them, but I can still tell you more or less what one does. Let X be a space, n an integer > 0, G a subgroup of $\mathfrak{S}_n$, χ a multiplicative character on G with values ± 1 (or, more generally, with values in k^*, where k is a fixed ring of scalars; but in practice it is always the unit character or the "signature" of a permutation). For any abelian sheaf A on X, let $C(A)$ be an injective resolution of A, and let $G \subset \mathfrak{S}_n$ act on $\otimes^n \Gamma C(A)$ by permuting the factors, taking the gradings into account of course, followed by multiplication by $\chi(s)$; write $\otimes^n_\chi \Gamma C(A)$ for this G-complex. Consider the hypercohomology of G with respect to this complex (cohomology of the well-known bicomplex), $\mathcal{R}^* \Gamma^G (\otimes^n_\chi \Gamma C(A))$; this is in fact an invariant associated to A and G (and χ). In the case where A is a sheaf of vector spaces over a field k, this hypercohomology can be *canonically* identified with the term $E_2 = H^*(G, \otimes^n_\chi H^*(X, A))$ of the spectral sequence leading to it. Now note that there is a natural homomorphism

$$\otimes^n_\chi \Gamma C(A) \to \Gamma \boxtimes^n_\chi C(A)$$

(letting $\boxtimes^n A$ denote the n-th tensor product of A *over* X^n), where the subscript χ on the right-hand side indicates the way G is made to act on the sheaf $\boxtimes^n A$. Also, if one chooses a suitable $C(A)$, one may assume that $\boxtimes^n_\chi C(A)$ is a *resolution* of $\boxtimes^n_\chi A$, and as G acts on these sheaves (this action being *compatible* with the G action on X^n), this resolution can be mapped to an *injective* resolution $C(\boxtimes^n_\chi A)$ of the sheaf with operators $\boxtimes^n_\chi A$; this gives a

homomorphism

(1) $\mathcal{R}^*\Gamma^G(\otimes_\chi^n \Gamma C(A)) \to \mathcal{R}^*\Gamma^G(\Gamma C(\boxtimes_\chi^n A)) = H^*(X^n, G, \boxtimes_\chi^n A).$

Restricting to the diagonal, , this can be composed if desired with the natural homomorphisms

$$H^*(X^n, G, \boxtimes_\chi^n A) \to H^*(X, G, \otimes_\chi^n A) \to H^*(X, G, (\otimes_\chi^n A)_G),$$

where this last reduction is made in order to have a sheaf $B = (\otimes_\chi^n A)_G$ on which G acts trivially, so that there is a Künneth formula for computing $H^*(X, G, B)$, which, for example, is equal to $H^*(G) \otimes H^*(X, B)$ when working over a field k. Following Steenrod's idea, one can also use the general natural pairings (here Z denotes a space on which G acts, equipped with a sheaf T with operators, and M is a G-module):

$$H_i(G, M) \times H^n(Z, G, T) \to H^{n-i}(Z/G, T \otimes_G M)$$

where $T \otimes_G M$ denotes the sheafification of the presheaf on Z/G which associates $\Gamma(V, T) \otimes_G M$ to an open set U, V being the preimage of U in Z. In the case in hand, every class $\alpha \in H_i(G, M)$ defines a homomorphism of degree $-i$, denoted by $i(\alpha)$ (the "interior product")

(2) $i(\alpha) : \mathcal{R}^m \Gamma^G(\otimes_\chi^n \Gamma C(A)) \to H^{m-i}(X^n/G, (\boxtimes_\chi^n A) \otimes_G M)$

which can also, if desired, be composed with the homomorphism from the right-hand side to $H^{m-i}(X, (\otimes_\chi^n A) \otimes_G M)$ induced by the injection $X \to X^n/G$. (Taking α to be the unit class in $H_0(G, Z)$, this gives the reduction to $H^*(X, (\otimes_\chi^n A)_G)$ considered above; this is what Steenrod does).

Finally, if χ_0 and χ_1 are the unit and alternating characters respectively, then it is possible to define canonical homomorphisms, corresponding to the n-th-power operation on a representative cocycle:

$$H^{2q}(X, A) \to \mathcal{R}^{2qn}\Gamma^G(\otimes_{\chi_0}^n \Gamma C(A))$$
$$H^{2q+1}(X, A) \to \mathcal{R}^{(2q+1)n}\Gamma^G(\otimes_{\chi_1}^n \Gamma C(A))$$

(a little care is needed in showing that this is independent of the choice of representative cocycle) and the compositions of these homomorphisms with (1) or (2) are the reduced Steenrod powers. Thom's theorem, relative to the case where n is prime, does not state that the homomorphisms from $H^i(X, A)$ to $H^{i+r}(X^n/G, (\boxtimes_\chi^n A)_G)$ or $H^{i+r}(X, (\otimes_\chi^n A)_G)$ corresponding to the $i(\alpha)$ for α contained in the natural basis of $H_0(G, k)$ vanish for r not congruent to 0 or 1 mod $2(n-1)$ (k being a field of characteristic n, A a sheaf of k-vector spaces, G the group of cyclic permutations in $\mathfrak{S}_n$), but that they vanish upon reduction

to $(\boxtimes_\chi^n A)_{\mathfrak{S}_n}$, resp. $(\otimes_\chi^n A)_{\mathfrak{S}_n}$. I don't know much more than this, except that the multiplication formulas come out all by themselves; I have not looked at the method for introducing the $\mathcal{P}_p^i$ starting from the reduced power operations, but this should not create any new difficulties. Neither have I completely studied the question of the extent to which these cohomological operations allow us to determine the cohomology of X^n/G, especially in algebraic geometry, as I am not particularly excited by it.

Yours, (remember if possible to dig up a copy of Artin's notes on functions and algebraic numbers for me) A. Grothendieck

November 17, 1956 JEAN-PIERRE SERRE

Dear Grothendieck,

Thank you for your letter; I am waiting impatiently for Bourbaki to print your diplodocus homologicus fonctoricus, and I pity the poor Japanese printers who are going to have to struggle with your hand-written corrections... By the way, I have seen Tannaka, who told me he was going to ask you to cut your paper in two; I hope you will manage to find a reasonably natural cut-off point.

There is not much news here. I do not know whether you know that Papa(kyriakopoulos) has proved Dehn's lemma J.-P. Serre : Reference: C. Papakyriakopoulos, *On Dehn's lemma and the asphericity of knots*, Ann. Math. **66** (1957), 1 —26. and the asphericity of knots; and the experts say it is correct! (On the other hand, Nakano has just told me that the paper he had written which I sent you has a flaw in it — the snag of course lying in the use of those awful Chow coordinates. Those things are really horrendous, and I am very glad that I do not need them to define quotients of varieties by a finite group.)

I have given a little seminar on characteristic p. I started by presenting Cartier's map (on differential forms), then the classification of inseparable isogenies J.-P. Serre : "the classification of inseparable isogenies": this is §2 of [**Se58b**]. of algebraic groups. I gave quite a few applications to the theory of curves and abelian varieties.

I have been getting excited recently about the question of birational invariance of cohomology (which is more interesting now that the symmetry does not hold). J.-P. Serre : "the symmetry does not hold": in characteristic $\neq 0$ it is possible to have $h^{p,q} \neq h^{q,p}$. I do not have a general proof, but nevertheless I can prove this:

Let X be an algebraic variety, P a simple point of X, $X' = Q_P X$ the "blow-up" of the variety X at the point P; let F be a coherent algebraic sheaf on X; assume that F_P is a $\mathcal{O}_P$-module of homological dimension ≤ 1 (in particular, a free module will do); let F' denote the sheaf defined by F on X'. Then $H^q(X, F) = H^q(X', F')$ for any $q \geq 0$.

There is probably an analogous result for blow-ups along a subvariety, but I cannot prove it yet. The method I use is obviously the spectral sequence of the projection $X' \to X$ (it is actually possible to present this without the spectral sequence, but why bother?). On this subject, have you written up something on your theory of the images of coherent sheaves under proper maps and the corresponding spectral sequence? It would definitely be worthwhile. From the

point of view of coverings, where I work, it is not immediate; for instance, I am unable to prove Leray's theorem on acyclic covering spaces (with a sheaf) in general; can you do this?

On the subject of Weil's book on abelian varieties: I hardly like its style any more than you do. But it has to be admitted that certain results in the book (the most important ones) appear to be accessible only by his methods (by which I mean the use of generic points, generic divisors, etc.): this holds in particular for the construction of the Jacobian, which is a little masterpiece of juggling with generic points. I will be curious to see how Chevalley goes about doing this (assuming he deals with it in his seminar).

Yours,

J-P. Serre

November 22, 1956 Alexandre Grothendieck

My dear Serre,

You asked for some mathematical gossip; I will tell you the little I know (I am not very well-informed in general). As Cartier is not participating in the Cartan seminar, Cartan has abandoned the idea of talking about formal groups, and is dissecting various Topology articles as they come. We have had a talk on the construction of classifying spaces using joins (by Milnor) and a talk on Kan's algebrization of homotopy, given by Shih, which was actually very successful. (For your information, Shih has just been recruited as an Attaché de Recherche, which solves an important problem). Chevalley is classifying semi-simple algebraic groups, which will take all year: Cartier has made the link between schemes and varieties, and I will be spending a month presenting Borel's article, as preparation for Chevalley's work. My "seminar" is nothing of the sort, but is rather an introductory course intended to get people to read Cartan-Sammy's book, together with the complements you already know about.

What do you mean by birational invariance of cohomology? Surely you don't mean to prove that the Poincaré polynomial of $\mathbf{P}(2)$ is equal to that of $\mathbf{P}(1) \times \mathbf{P}(1)$. — Leray's general theorem on open acyclic covers not only holds without any conditions, but is practically trivial; it appears in the diplodocus, of course. I have written a rough draft of my stuff on direct images of coherent algebraic sheaves under proper maps, but I don't know when I will find the time to write it up properly: probably not until someone actually needs it.

I am sad to hear that one cannot present Weil's results without juggling with generic points; as a matter of fact, the unbridled abuse of generic points necessarily hides the few situations in which their use is truly essential, such as the proof that every endomorphism of a Jacobian comes from a correspondence. As Chevalley says, one feels frustrated when faced with a proof like that one. I hope Lang will write up his report as intended; in the meantime, can you tell me whether the algebraic structure on the Jacobian variety P can be axiomatically defined by a) the existence of a bundle of group k^* on $X \times P$ such that for every $p \in P$, its restriction to $X \times (p)$ is of class p, and b) the fact that every algebraic system of divisors on X defines a regular map from the parametrizing variety into P. Chevalley says that the second condition holds (according to Matsusaka), but does not know if the first does, which is irritating.

I have read Weil's 1952 paper on the Picard variety J.-P. Serre : "Weil's 1952 paper on the Picard variety" : A. Weil, Oe. Sci. [1952e].; his fundamental theorem is in fact much more general and elementary than it appears from the paper. If X is a compact analytic space, then $P = H^1(X, \mathcal{O}^*)$ is naturally a complex Lie group whose connected component containing 0 is $H^1(X, \mathcal{O})/H^1(X, \mathbf{Z})$ (N.B. One can show that $H^1(X, \mathbf{R}) \to H^1(X, \mathcal{O})$ is injective). It is then very elementary to show (using the fact that if S is a Stein space, then by virtue of a topological-vector variant of the Künneth formula, one has $H^1(X \times S, \mathcal{O}) = H^1(X, \mathcal{O}) \otimes H^0(S, \mathcal{O})$) that:

a) every holomorphic bundle of group $\mathbf{C}^*$ on a product $X \times S$ (where S is an arbitrary analytic space) defines a holomorphic map $S \to P$;

b) conversely, any such map comes from a bundle, which moreover is well-determined up to multiplication by a bundle on $X \times S$ coming from a bundle on S. This last remark is also equivalent to the existence of a bundle on $X \times P$ defining the identity map $P \to P$. When furthermore X and P_0 are projective varieties (it is probably enough for X to be projective — at least this is true by Hodge-Weil whenever X is non-singular) the bundle in question is algebraic, giving a meromorphic section, and thus a divisor on $X \times P_0$. That's what Weil was after. Actually, by multiplying the bundle E by a sufficiently high power of a bundle $L_X \otimes L_{P_0}$ (where L_X and L_{P_0} come from projective immersions of X and P_0), one can also assume that E is sufficiently ample, and one then obtains a family of linearly equivalent *positive* divisors without fixed points, which define the map $p \mapsto p + \text{cst}$ from P_0 to P.

It is not such a great loss if Nakano's paper is wrong, since it seems to me that it did not answer any of the specific questions that arise (Weil-style characterization of the variety of classes of bundles, does one obtain varieties which are complete, irreducible, etc.?)

Here is a possibly stupid question on which I am stuck: let X be an algebraic curve (for example, an elliptic curve), take a representation of its fundamental group by unimodular matrices with integral coefficients, which gives a holomorphic bundle on X whose fiber is the group $\mathbf{C}^{*n}$. Is it true that this bundle will almost never be an algebraic variety J.-P. Serre : "almost never be an algebraic variety"; indeed, this is only the case if the monodromy (i.e. the image of the fundamental group in $\mathbf{GL}_n(\mathbf{Z})$) is *finite*, cf. SGA 3, exposé X, Rem. 1.3. (with regular projection and composition law)? Obviously, I am looking for an algebraic definition of the fundamental group, and I want to be sure that my idea cannot give anything.

Yours,

A. Grothendieck

December 4, 1956 Jean-Pierre Serre

Dear Grothendieck,

[...]

By "birational invariance" I mean the obvious thing. For example, I am quite convinced that the following result is true:

Let $X' \to X$ be a regular birational map, where X and X' are both projective. Let $\mathcal{F}$ be a locally free sheaf on X, and let $\mathcal{F}'$ be the sheaf it canonically determines on X'. Then $H^q(X, \mathcal{F}) = H^q(X', \mathcal{F}')$ for every $q \geq 0$.J.-P. Serre : It should be understood that X and X' are non-singular. The question is then equivalent to proving that $R^q f(\mathcal{O}_{X'}) = 0$ for all $q > 0$, where f is the given map $X' \to X$. Grothendieck was to quote this question in his lecture at the Edinburgh Congress in 1958, cf. [**Gr60**], Problem B, p. 116.

I do not know how to prove this in general, but I know how to do it when X' is the variety obtained by "blowing-up" a point of X (indeed, in this case it is enough to assume that the point is a simple point of X and the sheaf $\mathcal{F}$ is such that $\mathrm{hd}(\mathcal{F}_x) \leq 1$, where x denotes the point that is blown up.)

In particular, for a surface, one sees that $h^{0,1}(X) = h^{0,1}(X')$ and that

$$h^{1,1}(X') = h^{1,1}(X) + 1.$$

(The latter result follows from the fact that taking $\mathcal{F}$ to be the sheaf Ω^1, then the sheaf $\mathcal{F}'$ is not $\Omega^1(X')$, but is related to it in a very simple way).

This is what I mean by birational invariance. It is entirely reasonable.

As for your question a) on Jacobians: it is certainly true. Indeed, Weil has defined a divisor Θ on P with fabulous properties, whence a bundle on P, then one on $P \times X$ (using $P \times P \to P$), then one on $X \times P$ (using $X \to P$); I am more or less certain — I am too lazy to check, I leave it to you — that this bundle has the desired properties.

On the other hand, I have no idea J.-P. Serre : "no idea..." : see note 2211561 on the letter of November 22, 1956. about your holomorphic bundles with fiber $\mathbf{C}^{*n}$. I do not see how to prove that they are "almost never" algebraic.

Here is a question I asked Cartan, on which you may have some ideas: let $X' \to X$ be a proper map from an analytic space X' to another one, X, such that the preimage of every point is finite. Is it true that if $\mathcal{O}'$ is the sheaf of holomorphic functions on X', then the direct image (in the sense that you know) of $\mathcal{O}'$ is a coherent $\mathcal{O}$-sheaf?J.-P. Serre : Yes, it is true, cf. Séminaire

Cartan 1960/1961, exposé no19 (C. Houzel), §5. This is the (much harder) analytic equivalent of what you had to prove in the algebraic case.

See you soon. Yours,

J-P. Serre

November 1, 1957 ALEXANDRE GROTHENDIECK

My dear Serre,

You will find enclosed a very simple proof of Riemann-Roch J.-P. Serre : "a very simple proof of Riemann-Roch": this proof is reproduced (with some interpolations of my own) in SGA 6, 71–77. independent of the characteristic. Almost all of my report has now become useless, particularly: the systematic use of the λ-ring structure, virtually all of chapter 1 (from which only the definition of the homomorphism ch and formula (1.30) need to be kept), theorem 2.14 which required a rather difficult passage to Grassmannians (and which is now proved more simply); even theorem 2.12 and homomorphism (2.34) $\varphi : A(X) \to G\underline{K}(X)$ are no longer needed; basically all that is needed are the formulas (2.15) and the unwritten formula which gives the multiplicative structure on $\underline{K}(X)$ and the inverse images explicitly without going through vector bundles. I should also point out that passing to blown-up varieties makes it possible to prove (2.35) in full generality.

However, it seems to me that this new proof goes less to the heart of the matter and has a narrower scope than the old one. It will probably give nothing for arithmetic varieties (for which the "denominatorless" formulas (v) and (2.37) are certainly valid); we shall have to look into the situation for trickier Riemann-Roch formulas which involve a group of operators on sheaves... Moreover, I have not managed to get this new method to work for "denominatorless" formulas, except when $\dim Y < (n-1)/2$ (where $n = \dim X$); whichever way I masturbate it, the torsion in $\underline{K}(X)$ resp. $A(X)$ screws up. But this is certainly not serious, since these formulas are known to hold in characteristic zero (even though $\underline{K}(X)$ and $A(X)$ have just as much torsion in that case!) — Thus, I will still try to get my first method to work in characteristic p, even if it will be much longer.

Let me point out that "morally", this new method is based on the determination of $\underline{K}(X)$ and $\underline{A}(X)$ when X is either a projective bundle associated to a vector bundle or a variety obtained by blowing up a non-singular subvariety. In the second case, I have not yet finished the computation; a little something is still missing. With the notation $(Y, X; Y', X')$, (i, j, f, g) from the little paper, what has to be proved is formula (9) from this paper (but as an *equality*)

$$f^*(i_*(y)) = j_*(y \, C^{p-1}(F)), \quad y \in A(Y).$$

Note that both sides of the equation have the same image under f_* and under j^*, so the situation is equivalent to

(9 bis) $$\mathrm{Ker}\, f_* \cap \mathrm{Ker}\, j^* = 0.$$

In any case, (9) holds after reduction to $G(\underline{K}(X'))$ (which at the very worst destroys a bit of torsion) since it then follows from (6); there is therefore hardly any doubt that it is true. Moreover, (9 bis) would be immediate if the following were known (and I would like to know what the experts know about this):

If $x' \in A(X')$ induces 0 in $A(Y')$, then there exists a cycle representing x' which does not meet Y' (?)

(This does not contain blow-ups any more than Tôhoku!) One can ask an analogous question for algebraic equivalence, numerical equivalence, etc.

Let me point out that *equality* (9) follows easily and entirely formally from (6), without using the bothersome (6 bis). Of course, this is heuristically how I accidentally came across (6 bis). I haven't yet checked this formula directly, but it is obviously no more than a simple exercise.

Finally, I have another kind of question. You are moving out of your apartment; do you think it might be possible for me to inherit it? As the rent is not very high, if I remember rightly, I would then be able to buy some furniture (on credit). I am interested in it for my mother, who isn't very happy in Bois-Colombes, and is terribly isolated.

Regards,

A.Grothendieck

November 12, 1957 ALEXANDRE GROTHENDIECK

My dear Serre,

I agree with all your correctionsJ.-P. Serre : "I agree with all your corrections": Grothendieck is replying to a letter I had sent him, of which I have not kept a copy., in particular the one concerning $\gamma^{p-1}(E)$, which should read $\gamma^{p-1}(E-p)$ throughout. The wrong part of lemma 1 (which I do not use, but had included for symmetry's sake) should read: If Riemann-Roch holds for fg and g, then it also holds for f in $\mathrm{Im}(g_!)$ (note that if $Z \neq Y$ then $g_!$ is never surjective!). Finally, I should add that formula (2.22) of RRR J.-P. Serre : RRR = Report on Riemann-Roch, reproduced in SGA 6, 20–71. is rather a rash assertion, and it would be prudent to assume that X is a bundle over Y; I pretend to use this result (for singular varieties) in the proof of proposition 2.9, but actually this can also be done using only non-singular varieties.

As for proposition 2.3 which frightens you so much, I only used it in the case of cycles of dimension $> \dim Y$ (see the corollary), for which it is trivial. What makes me think that it always holds is that it becomes true after reduction to $G\underline{K}(X)$, $G\underline{K}(Y)$ (which only kills a little torsion, at the very worst). But it would be very nice to have a direct proof, in order to have an elegant theory of Chern classes (cf. below).

On this topic, note that it would be nice to be able to prove directly that $A(X) = G\underline{K}(X)$ (which I am more or less convinced is the case). This would give an attractive definition of linear equivalence of cycles, which would also be valid for singular varieties; moreover, it does seem that many things are easy to prove in $G\underline{K}(X)$, but not in $A(X)$. This is the case for both proposition 2.3 and also the intersection formula

(iii b) $$i^*(i_*(x)) = x\, C^q(T_{X/Y})$$

for an injective map $Y \overset{i}{\longrightarrow} X$ and an $x \in A(Y)$. I have not found a direct proof of this. (N.B. (iv b) is a consequence of (iii b).)

I should also mention that I can prove that if X is a (not necessarily quasi-projective) non-singular variety, then every coherent algebraic sheaf is a quotient of a direct sum of sheaves defined by divisors. This gives a definition of Chern classes for sheaves, provided one already exists for vector bundles. The Riemann-Roch theorem can then also be *stated* in the case of non-projective varieties (for instance, this is the case for classical varieties, via the transcendental definition of Chern classes); and furthermore, using the revised and corrected version of lemma 1 from my little paper, it follows that Riemann-Roch *holds* whenever $y \in \underline{K}(Y)$ is of the form $g_!(z)$, where $g : Z \to Y$

is a proper morphism from a non-singular quasi-projective variety Z to Y. If one knew for example that every subvariety of Y was the image of a *non-singular* quasi-projective variety by a proper map (compare with the Chow lemma!) then the result would be in the bag.

Now, about the theory of Chern classes for vector bundles J.-P. Serre : The theory of Chern classes sketched in this letter is presented in detail in [**Gr58**]., the following remarks work just as well in the topological setting (where they give a very elementary theory of Chern classes, without obstructions or Borel's subtle theorems). Assume that there is a commutative graded ring $A(X)$ associated to every non-singular algebraic variety X, and that this is a contravariant functor in X. Assume that a functorial homomorphism $P(X) \to A^1(X)$ is given, where $P(X)$ denotes the group of divisor classes, and that for every variety X or every subvariety Y of codimension p, a group homomorphism $i_* : A(Y) \to A(X)$ is also given, which increases the degree by p. The axioms are

a) When $Y \subset X$ is a non-singular hypersurface, $P(X) \to A(X)$ is given by $i_*(1_Y)$, (where 1_Y is the identity in $A(Y)$).

b) If $Y \subset X$ is non-singular and irreducible, then $i_*(y\,i^*(x)) = x\,i_*(y)$ (where $i : Y \to X$ is the inclusion).

c) If E is a vector bundle of rank p on X, X' is the associated projective bundle, L is the famous rank 1 bundle on X', and ξ is its class in $A^1(X')$, then the ξ^i $(0 \le i \le p-1)$ form a basis for $A(X')$ over $A(X)$.

Under these conditions, there is a unique way to associate to every vector bundle E on a non-singular variety a class $c(E) = 1 + \sum_{i=1}^{p} c^i(E) \in A(X)$, so that the Hirzebruch conditions are satisfied:

(i) functoriality,

(ii) $c^1(E) = d(E)$ if E is of rank 1 ($d(E)$ denotes the image in $A^1(X)$ of the class of divisors defined by E),

(iii) $c(E) = c(E')c(E'')$ if E is an extension of E' by E''.

Moreover, these classes satisfy conditions (iv), (v), and (vi), which allow us to compute the Chern classes of a tensor product, an exterior power and a dual space. The $c^i(E)$ are also characterized by the formula

$$(1) \qquad \sum_{i=0}^{p} c^i(E)\xi^{p-i} = 0.$$

The proof is very easy. Uniqueness follows from c), which implies that if X'' is a flag bundle over X, then $A(X) \to A(X'')$ is injective. For existence, define $c^i(E)$ by formula (1), which is possible thanks to c). The proof of (i) and (ii) is trivial, and (iii) is proved by a little trick using a) and b). Formulas

(iv), (v), (vi) then become trivial by Hirzebruch's method. — As an added bonus, one gets the structure of $A(X') = A(X)[\xi]/(\sum_{i=0}^{p} c^i \xi^{p-i})$.

Of course, the essential thing to check is condition c). If one takes $A(X) = \sum H^p(X, \Omega^q)$ (in the topological case) it is easy thanks to the spectral sequence for fiber bundles. When $A(X)$ is the Chow ring, one needs to work a little more, and give a more detailed set of axioms which implies c). The final result is as follows. Assume that for every non-singular variety X, a commutative ring $A(X)$ is given (but not initially assumed to be graded, so the axioms can be applied to $\underline{K}(X)$). $A(X)$ is a contravariant functor in X, and is also covariant with respect to *proper* morphisms. Let us postulate a rather large number of rather trivial formal properties, together with two more important axioms:

1) an *exactness axiom*: if Y is a non-singular subvariety of X whose complement is U, then the sequence $A(Y) \to A(X) \to A(U) \to 0$ should be *exact*;

2) a *continuity axiom* which says that the homomorphism $A(X) \to A(X \times \underline{k})$ is surjective (and hence in fact bijective).

Actually, under some rather weak conditions, which hold both for the Chow ring and for $\underline{K}(X)$, the continuity axiom can be deduced from the exactness axiom, by the argument from proposition 2.9 of RRR.

If all these conditions are satisfied, then proposition 2.13 in RRR (cell decomposition) goes over without modification. Moreover, a similar argument proves the following (which does not use the continuity axiom, but does use the exactness axiom in an essential way):

Let P be a non-singular variety such that for any non-singular variety X, the natural homomorphism $A(X) \otimes A(P) \to A(X \times P)$ is surjective (which is the case, for instance, if P has a cell decomposition). Then let X' be a locally trivial bundle over X of fiber P; let ξ_α (elements of $A(X')$) and $x \in X$ be such that the images of the ξ_α in $A(P_x)$ ($P_x=$ the fiber of X' over x) generate this group. Then the same still holds for any other $y \in X$ *and the ξ_α generate the $A(X)$-module $A(X')$*.

From this it follows that if the $A(X)$ are graded, then if a functorial homomorphism $P(X) \to A(X)$ satisfies a) and b) as above, c) is also satisfied.

So the question is simply whether one can also define the Chow ring in the case of a non-quasi-projective variety, as a co- and contravariant functor of X, which satisfies reasonable properties that I have not given you, plus the continuity axiom. (As Chow has not sent me his article, I have not been able

to examine whether his proofs could be adapted). Or whether the "inverse image" maps in $\underline{K}(X)$ are compatible with the filtrations (which also implies that $\underline{K}(X)$ is a filtered ring, so that $G\underline{K}(X)$ satisfies all the necessary properties for a Chern class theory).

In all these questions, it is obviously possible to restrict oneself to quasi-projective varieties, but one then needs to know that the projective bundle associated to a vector bundle over a non-singular quasi-projective space is still quasi-projective (which is probably easy using the rank one bundle L on X'). I should also point out that rigorously speaking, Riemann-Roch is for the moment proved only under the assumption that the blow-up of a quasi-projective variety is quasi-projective. Needless to say, I have not been amusing myself by looking at this question.

Finally, I would like to know whether people who have really studied blown-up varieties have determined what their rings of cycles are. It is annoying not to know whether formula (9) of LPRR (little paper on Riemann Roch) is an equality (once again, this would a priori only be true modulo the hypothetical kernel of the map $A(X') \rightarrow G\underline{K}(X')$, since formula (6) of LPRR can be considered as being proved via a purely local Tor computation).

Having been influenced by several people, I will after all give the Peccot lecture course on Riemann-Roch. Moreover, at the moment I have just dropped research in order to finally start writing up the varieties, J.-P. Serre : "writing up the varieties": this is a reference to a draft for Bourbaki. of which I hadn't written a single word until the day before yesterday!

Yours,
A.Grothendieck

October 17, 1958 ALEXANDRE GROTHENDIECK

My dear Serre,

You will find enclosed a short paper for Bourbaki (inspired by conversations with Chevalley); please pass it on to him when the opportunity arises. Would you please check that it is understood at Nancy that the Bourbaki papers are to be sent to Tate, since there is no doubt that he is now a member. Over here, I haven't much news; I have started writing up some short papers and commentaries for Dieudonné, who seems to have gotten off to a good start on writing up schemes. J.-P. Serre : The writing up of the EGA's had just started. I hope that by spring, the first four chapters will be written up (chapter 4 will contain cohomology theorems such as finiteness etc.) and it will be possible to have them published by Motchane. I have suggested to him that we include our "joint paper" on covering spaces J.-P. Serre : "our "joint paper" on covering spaces". Grothendieck and I had intended to publish a paper together on the theory of covering spaces. This never happened, but the subject was taken up in SGA 1. in chapter 5 if you agree. Indeed, this paper would fit perfectly into the general plan at this point; chapter 6 will contain generalities on group schemes J.-P. Serre : Likewise, group schemes were published in SGA 3 and not in the EGA's. and principal schemes over them, including the sorites on generalized Galois coverings, which will round off the "qualitative" part of the theory. The second part will contain 7. Intersections 8. Duality and Residues 9. Riemann-Roch 10. Abelian varieties 11. Weil cohomology. — The fact of including our paper as part of this series would have the advantage of sparing us a large amount of tiresome "background " on ideas that nobody knows yet.

I would like to prove the following result: Let X be a scheme over Y, proper over Y, whose "tangent map is everywhere surjective". J.-P. Serre : "tangent map is everywhere surjective": this was later to be called a "simple" morphism, then a "smooth" morphism. As for the invariance property of the fundamental group stated by Grothendieck, he quickly realized it is a little too optimistic. Show that the "geometric" fundamental group of the fiber $f^{-1}(y)$ (geometric means that one passes to the algebraic closure of the ground field $K(Y)$) is independent of y. (I forgot to say that for the sake of simplicity, the schemes are assumed to be connected and without singularities). This result would be of interest mainly when Y is of unequal characteristics. Given a "marked point" x in X, one would need to be able to define maps from $\pi_1(X, x)$ to

$\pi_1(F, x)$, where F is the "geometric" fiber passing through x. Have you ever thought about questions of this flavor?

Abhyankar has sent me some reprints, which I have flipped through. He has proved the Serre-Lang conjecture for nicely ramified coverings of a curve. We should be able to get something out of this for ourselves. J.-P. Serre : "We should be able to get something out of this": indeed, we extracted from it the very useful result called *Abhyankar's lemma* for tamely ramified coverings (here called "nicely ramified coverings"). Yours,

A. Grothendieck

October 22, 1958 JEAN-PIERRE SERRE

Dear Grothendieck,

I am beginning to feel guilty for not having replied to your letters sooner: the sad truth is that I have nothing serious to say about them.

The Bourbaki meeting was very pleasant; we all stayed in the home of a man called Guérin (a friend of Schwartz's — a political one, I think); Guérin himself was in Paris and we had the whole house to ourselves. We worked outside most of the time, the weather was beautiful, we went swimming almost every day; in short, it was one of the best meetings I have ever been to.

As for work, we mostly looked at Schwartz's report on elliptic equations, much helped by the presence of Malgrange and his comments. We read it in enough detail to feel that we understood it; I do not remember exactly what was decided; J.-P. Serre : Nothing concrete came out of this Bourbaki draft on partial differential equations, which is a great pity. I think that Malgrange is going to write a report on the hyperbolic case and Dixmier (or Bruhat?) is going to write a second draft of the elliptic case.

We did not look at Koszul's draft on dimensions;J.-P. Serre : This is a reference to the writing of the book *Commutative Algebra*. The chapter on flat modules (Chap. 1) was published very quickly (1961). The chapter on dimensions (Chap. 8) had to wait till 1983, as did the Cohen theorems (Chap. 9). my first supplement on exterior algebra was looked at briefly (and unanimously deemed incomprehensible). The flatness draft (which was handed out at the meeting — I don't think you have it) was read carefully, and Chevalley is currently writing the next version. He will have to hurry, since the Commutative Algebra book depends on it. Cartier has not written up the Cohen theorems, but we noticed with relief that they cannot be discussed as long as the concept of a discrete valuation ring is not available; hence this wretched draft will not trouble the publication of the first volume of Commutative Algebra.

The second edition of topological groups (Dieudonné) was surprisingly well received; we decided to put proper maps (using Chevalley's morphic definition) into General Topology J.-P. Serre : "put proper maps into General Topology" : see T.G. I, §10, which came out in 1971., and I wrote them up on my way back from La Ciotat. You will see, it works out quite nicely.

This draft has taken up all, or nearly all, of my time since I came back from the meeting. I have only just started working on local fields again, to prepare my course. Lang has sent me the secret draft by Artin and Tate on higher

ramification and conductors, which is very pretty; on translating this draft into the language of "isogenies", one can get absolutely everything to work in the case of an arbitrary perfect residue field (including the fact that the Artin representations are "integral"). Of course, the proofs remain artificial, and will probably stay that way until the direct definition of this representation is found.

I have received the proofs of the joint paper with Borel on Riemann-Roch from Gauthier-Villars.J.-P. Serre : "paper on Riemann-Roch with Borel": [**BS58**]. Have you received the proofs of your supplement on Chern classes? Choquet was very worried when I told him you were in the USA. He wondered whether your mail was reaching you; can you let me know? Have you asked anyone to forward your mail?

I have not thought any more about the computation of the fundamental group of a scheme starting from that of its irreducible components. The subject doesn't really inspire me.... It is, however, a question to be dealt with, which may not be trivial. Say hello to Tate and Zariski from me. Yours,

J-P. Serre

November 5, 1958 ALEXANDRE GROTHENDIECK

My dear Serre,

Choquet has probably received the corrections of the proofs for Gauthier-Villars by now. I asked that 100 reprints of my article be sent to you, since there is no point sending them here to Harvard. If they ask you to pay, tell them to contact me; naturally, following an illustrious precedent, I will not pay.

I have the impression I am making progress with the π_1.J.-P. Serre : Fundamental group theory was one of the first major successes of scheme theory. Grothendieck presented it to the Bourbaki seminar 1958/1959, no182, and made it the subject of SGA 1. It seems to me that one of the fundamental things to prove is the following:

Let X be proper over J.-P. Serre : In this statement, it should be assumed that the local ring $\mathcal{O}$ is Noetherian (which is implicit) and *complete.* The latter hypothesis is required because the statement b) below is wrong, even if $\mathcal{O}$ is a discrete valuation ring: Raynaud showed me a counterexample. a *local* ring $\mathcal{O}$, and let F be the "geometric" fiber of the origin in $\mathrm{Spec}(\mathcal{O})$. Then the homomorphism

$$(*) \quad \pi_1(F) \to \pi_1(X)$$

is *injective.*

(This is the result that should enable us to map the π_1 of the "generic fiber" to $\pi_1(F)$.)

In fact, this claim is really an existence theorem; it comes down to saying that for every unramified F^* over F, there is an unramified covering $\mathcal{O}'$ of $\mathcal{O}$ and an unramified covering X'^* of $X' = X \otimes_{\mathcal{O}} \mathcal{O}'$ which induces a covering F'^* of the fiber $F' = F + \cdots + F$ which is isomorphic to F^* over a connected component.

This claim breaks down into two parts:
a) $\pi_1(F) \to \pi_1(\widehat{X})$ *is injective*
(where $\widehat{X} = X \otimes_{\mathcal{O}} \widehat{\mathcal{O}}$; in fact, there is an exact sequence

$$0 \to \pi_1(F) \to \pi_1(\widehat{X}) \to \pi_1(\widehat{\mathcal{O}}) = \pi_1(k) \to 0.)$$

b) $\pi_1(\widehat{X}) \to \pi_1(X)$ *is injective.*

I have practically proved a), thanks to a sort of GAGA-style theory, J.-P. Serre : About *formal GAGA*, see EGA 111, S§4,5. which says more or less that the data of a proper $\widehat{X}$ over $\widehat{\mathcal{O}}$ is functorially equivalent to the data

of projective systems $\widehat{X}_n = \widehat{X} \otimes_{\widehat{\mathcal{O}}} \widehat{\mathcal{O}}/\widehat{\mathfrak{m}}^n$, and similarly for sheaves. As F^* functorially defines unramified coverings F_n^* of the $\widehat{X}_n$ (for simplicity's sake I $\quad$ 1/2 assume that $k = \widehat{\mathcal{O}}/\widehat{\mathfrak{m}}$ is algebraically closed) — which differ from F only by some extra nilpotent elements — then these systems F_n^* do indeed define an unramified covering of $\widehat{X}$.

As for b), I am not yet convinced it is true without any regularity conditions on $X \to \mathrm{Spec}(\mathcal{O})$. The general problem here is (roughly) as follows: to descend any construction over $\widehat{\mathcal{O}}$ to an unramified covering $\mathcal{O}'$ of $\mathcal{O}$ — a problem you are familiar with! I believe there are some results: the proofs, for the moment, use scheme-theoretic techniques; I have a patented method for descending everything to an $\mathcal{O}'$ over $\mathcal{O}$ which is unramified over *some* point of $\mathrm{Spec}(\mathcal{O}')$ lying over the origin in $\mathrm{Spec}(\mathcal{O})$.

Moreover, my GAGA-style results and your idea J.-P. Serre : "your idea of considering an Artinian ring as the set of k-rational points ...": This is a reference to the *Greenberg functor* which I had used in order to construct a "geometric" local class field theory, cf. the letter of November 9, 1958, as well as [**Se61a**]. of considering an Artinian ring as the set of k-rational points of a ring variety defined over k make me think it is possible to canonically lift J.-P. Serre : "it is possible to canonically lift ...". Too optimistic! But it is true for ordinary abelian varieties, as I later realized, cf. the letter of August 2-3 1964. any variety X_0 defined over a perfect field of characteristic $p \neq 0$ to a sort of "holomorphic variety" X defined over any complete local ring $\mathcal{O}$ having the same residue field. <u>If</u> one is lucky enough for this "holomorphic variety" to come from an "algebraic variety" X defined over $\mathcal{O}$, then the latter is unique and depends functorially on X_0, etc. As this is not exactly what I need for π_1, I am trying to get Tate interested in it, but he is fairly skeptical.

Otherwise, I am working quite a lot on the book with Dieudonné. Everything is going very well for schemes, particularly for all questions of the type: if something is given, or holds ... over a generic point, then it holds over a non-empty open set... There is one hitch: I foresee that I will have to redo everything to deal with gluing spectra of complete $\mathfrak{m}$-adic rings (instead of merely spectra of discrete rings). In any case, my GAGA basically consists in proving the "finiteness theorem" for such schemes by going back to Grauert's idea, then applying your proof of GAGA. Yours,

A. Grothendieck

November 9, 1958 JEAN-PIERRE SERRE

Dear Grothendieck,

Your letter makes me want to take stock of what I am doing with local fields J.-P. Serre : The results stated in this letter were published in [**Se60b**] and [**Se61a**]: see also *Corps locaux et isogénies*, Séminaire Bourbaki, 1958/59, no185.. Here is where I stand:

1. Isogenies.

Let G be a commutative algebraic group, defined over an algebraically closed field k (I restrict myself to this case throughout everything that follows). Assume G is connected, and consider all isogenies $G' \to G$ for connected G' (these isogenies are separable — or rather, one should work "modulo" inseparability). These G' form a filtered ordered set, and by passing to the limit one obtains a group $\overline{G}$, which is a projective limit of algebraic groups, and an exact sequence:

$$0 \to \pi_1(G) \to \overline{G} \to G \to 0,$$

where $\pi_1(G)$ is a Galois-type group, as Tate would say. Tate was actually the first to encounter this construction: when G is an abelian variety or a group $\mathbf{G}_m$, the group $\overline{G}$ is nothing other than the projective limit of the G_n, where $G_n = G$ and $G_{nm} \to G_n$ is multiplication by m. On the other hand, no such simple description of $\overline{G}$ and $\pi_1(G)$ exists for $\mathbf{G}_a$ or for a unipotent group, and that is why Tate did not manage to find a "class formation" back then — see no5 below.

When G is not connected, with connected component G_0, set $\pi_0(G) = G/G_0$ and $\pi_1(G) = \pi_1(G_0)$.

These constructions yield formal results of the usual kind. The only surprising result is the following:

1.1 If $G \subset G'$, then $\pi_1(G) \to \pi_1(G')$ *is injective.*
(There is then a delightful exact sequence

$$0 \to \pi_1(G) \to \pi_1(G') \to \cdots \to \pi_0(G') \to \pi_0(G'/G) \to 0.$$

The proof is trivial, of course.)

I have no trivial proof of 1.1, and I doubt that one exists (since it is more or less equivalent to saying that some π_2 is zero). I manage it by knowing the commutative algebraic groups explicitly; by dévissage, the problem can be reduced to the case where G and G'/G are either $\mathbf{G}_a$ or $\mathbf{G}_m$, or else an abelian variety A. One can then use what is known about the classification of

extensions; two cases need close investigation, namely $G = \mathbf{G}_a$ and $G'/G = \mathbf{G}_a$ or A.

All this can be extended to "proalgebraic" groups, i.e. projective limits of algebraic groups, by passing to the limit.

2. The case of local fields.

Let k be algebraically closed, and consider complete valuation fields with remainder field k. J.-P. Serre : remainder field = residue field. If $U(K)$ is the group of units of such a field, K, then it is known that $U(K)$ is canonically equipped with a proalgebraic group structure (provided the characteristic of k is $p \neq 0$, which is the only interesting case — when the characteristic of k is $= 0$, it is also necessary to choose a lifting of k to K).

Let L/K be a Galois extension, with Galois group $\mathfrak{g}$. It is known that $H^q(\mathfrak{g}, L^*) = 0$ for all q (this is a local Tsen theorem, if you like — which gets proved automatically when setting up conductor theory, cf. no3). The exact sequence:

$$0 \to U(L) \to L^* \to \mathbf{Z} \to 0$$

then shows that $H^q(\mathfrak{g}, U(L)) = H^{q-1}(\mathfrak{g}, \mathbf{Z})$ for $q \in \mathbf{Z}$, and in particular, taking $q = -1$, $H^{-1}(\mathfrak{g}, U(L)) = \mathfrak{g}/\mathfrak{g}'$.

This can be interpreted as follows: the norm map

$$N : U(L) \to U(K),$$

has a kernel $I_{L/K}$, which is a proalgebraic subgroup of $U(L)$; the set of combinations of the $x^{1-\sigma}$, $\sigma \in \mathfrak{g}$, form a subgroup I' of I which is obviously connected. The result above means that $I/I' = \mathfrak{g}/\mathfrak{g}'$. Therefore I' *is the connected component* of I. Thus, there is an isogeny: 2/3

$$0 \to \mathfrak{g}/\mathfrak{g}' \to U(L)/I' \to U(K) \to 0.$$

As $U(L)$ is connected, this gives rise to a canonical surjection:

$$\pi_1(U(L)) \to \mathfrak{g}/\mathfrak{g}'.$$

Passing to the limit (or alternatively, boldly taking L to be the separable closure of K), one obtains a surjective homomorphism:

$$\pi_1(U(L)) \to A(K),$$

where $A(K)$ denotes the Galois group of the maximal abelian extension of K.

EXISTENCE THEOREM: *The map $\pi_1(U(K)) \to A(K)$ is bijective*

Or, in other words, every isogeny of $U(K)$ comes from a unique abelian extension of K.

Once again, I have no "nice" proof of this theorem. I construct the required fields L/K explicitly, using the Whaples-MacKenzie equations J.-P. Serre : "Whaples-MacKenzie equations": in [**Se61a**], §4.4, they are called "Artin-Schreier equations". (Amer. J. of Math. 1956) $x^p - x - \lambda = 0$, with certain restrictions on the valuation of λ; first showing that such an equation defines a cyclic extension of order p, and computing the corresponding isogeny more or less explicitly, then observing that one gets enough of them (except when there is a p-th root of unity in K, in which case one has to add the Kummer equations $x^p = \alpha$).

It is not really very complicated. One could ask more precise questions. Determining the structure of $U(K)$, for example, to begin with — this was solved by Shafarevich: it is a product of (infinite) additive Witt groups, except when K contains a p-th root of unity, in which case $U(K)$ is a quotient of a product of Witt groups by a discrete group isomorphic to $\mathbf{Z}$. Using this structure and the classification of isogenies, one could ask for an explicit description of $\pi_1(U(K)) \to A(K)$, the group $A(K)$ being described by Kummer theory. This is known as the "explicit formulas" problem J.-P. Serre : The "explicit formulas" have been much studied since Shafarevich: see for example chapters 7 and 8 of:

I.B. Fesenko and S.V. Vostokov, *Local Fields and their Extensions*, A.M.S. Transl. of Math. Monographs **121** (1993). for local systems $(a, b)_{p^n}$, which in principle was solved by Shafarevich. The solution is very complicated, and does not seem worth the effort needed to get it. Ask Tate what he thinks about it. In any case, I have not yet found the courage to check Shafarevich's paper and translate it into my language — all I know is that it is possible.

3. Local fields. Conductor theory and higher ramification.

Since the subgroup $U(K)$ is filtered by subgroups $U_n(K)$, the same is true of the group $\pi_1(U(K))$, and one is inevitably led to examine the filtration obtained on $A(K)$ by transport de structure. In other words, one is led to define the "conductor" $f(L/K)$ of a finite extension L/K as the smallest integer n such that the associated isogeny $U(L)/I' \to U(K)$ comes from an isogeny of $U(K)/U_n(K)$.

It is easy to guess that this will give Hilbert's ramification groups, intelligently indexed (so they pass to the quotient). And indeed, that is what one proves; in fact one obtains a more precise result:

Let L/K be a Galois extension with Galois group $\mathfrak{g}$; let $a(\sigma)$, $\sigma \neq 1 \in \mathfrak{g}$ be the valuation of $\sigma x - x$, where x is a uniformizing parameter of L, and set $a(1) = +\infty$. Hilbert's ramification groups V_x are defined by

$$\sigma \in V_x \Longleftrightarrow x + 1 \leq a(\sigma), \quad x \in \mathbf{R}.$$

From this, one deduces the existence of a piecewise linear function $\varphi_{L/K}(x)$, one of whose many definitions is as follows:

$$\varphi_{L/K}(x) = \frac{1}{e} \sum_{\sigma \in \mathfrak{g}} \inf(a(\sigma), x + 1) - 1 \ , \quad e = [L : K].$$

There is no point in giving the fundamental properties of $\varphi_{L/K}$ here; they are well known (see Kawada, Annals, 1953, for example) and can be formally deduced from the properties of $a(\sigma)$. In particular, $\varphi_{L/K}(x) = \varphi_{L'/K} \circ \varphi_{L/L'}(x)$ whenever $L \supset L' \supset K$. Denoting the inverse function of $\varphi(x)$ by $\psi(x)$, one can define a new indexation of the higher ramification groups by setting:

$$V^x = V_{\psi(x)}, \quad x \in \mathbf{R}.$$

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The V^x are now numbered in such a way that they "pass to quotients" (whereas the V_x "passed to subgroups"). It should be noted that the "jumps" of the V_x are at the points $x = \psi(n)$, where n is a "jump" point of ψ, hence an integer; x on the other hand is not necessarily an integer.

THEOREM: a) *If L/K is an abelian extension, then the jumps of the function ψ are integers.*

b) *Under the same hypothesis, the conductor of L/K is $c + 1$, where c is the largest integer n such that ψ jumps at n.*

The two claims are proved simultaneously. The method is the same as in the original paper by Hasse (which dealt with the case where the residue field is finite), except for the fact that computations of norm indices are replaced by isogenies. More precisely, one shows the following:

LEMMA: *Let L/K be a Galois extension with Galois group $\mathfrak{g}$. For any integer n, the norm map $N : U(L) \to U(K)$ sends $U_{\psi(n)}(L)$ to $U_n(K)$ and $U_{\psi(n)+1}(L)$ to*

$U_{n+1}(K)$; *the homomorphism*

$$N : U_{\psi(n)}(L)/U_{\psi(n)+1}(L) \rightarrow U_n(K)/U_{n+1}(K)$$

defines an isogeny whose separable degree is $\psi'_{d/g}(n)$.

[The quotient of the right derivative of ψ by its left derivative is denoted $\psi'_{d/g}(n)$; this quotient is 1 except when ψ has a "jump". Also, for any real number x, define $U_x(L)$ to be $U_m(L)$, where m is the smallest integer $\geq x$.]

For the cyclic case of prime order p, the lemma is proved by a brute force computation of the norm (an old computation dating from the beginnings of number theory): the general case can be trivially deduced from this by dévissage, taking the transitivity property of ψ into account.

Let us apply this lemma to the *abelian* case. It is then known (no2) that the separable degree of the isogeny deduced from N is equal to $[L : K]$. Furthermore, this lemma shows what are the "partial isogenies" which appear above $U_n(K)/U_{n+1}(K)$. One first deduces from this that the separable degree of this isogeny divides the product $\prod \psi'_{d/g}(n)$, where n runs over the set of integers ≥ 0. However, it is immediate from the definition of ψ that the product of *all* the $\psi'_{d/g}(x)$, for real x, is equal to $[L : K]$. Comparing, one sees that $\psi'_{d/g}(x)$ is necessarily equal to 1 for all non-integral x, which is exactly a). Claim b) for conductors follows.

COROLLARY *The Artin conductor is an integer.*

Indeed, it is well known that it is enough to check that the conductor is an integer for a cyclic extension, with the character being an isomorphism of the group in question; in this case, the conductor is $\varphi(m) + 1$, where m is the largest integer such that φ jumps at m; the theorem says that $c = \varphi(m)$ is an integer. We also see that the Artin conductor coincides with the conductor defined above in the abelian case, as it should.

[I thought for a long time that this last result was new, in the case of an arbitrary algebraically closed residue field — and thus, trivially, for an arbitrary perfect residue field. — In fact, C. Arf (J. Crelle, t. 181) had already proved it in 1940, by an extremely complicated computational method.]

4. The case of local fields. A "class formation".

Keeping the conditions above, associate to any local field K the group $B_K = \pi_1(U(K))$.

THEOREM: *The B_K form a "class formation".*

The proof uses the fact that the B_K are isomorphic to the Galois groups $A(K)$ of the maximal abelian extension of K. One needs to show (cf. my lecture notes at the Collège) that if L/K is a finite Galois extension, then the transfer homomorphism $A(K) \to A(L)$ maps $A(K)$ isomorphically onto the fixed points of $\mathfrak{g}_{L/K}$ in $A(L)$; but one sees immediately that the transfer $A(K) \to A(L)$ translates into the homomorphism from $\pi_1(U(K))$ to $\pi_1(U(L))$ induced by   6/7 the injection $U(K) \to U(L)$. Using the exact sequence from no1, i.e. the fact that π_1 is an exact functor on connected groups, it follows that $\pi_1(U(K))$ can indeed be identified with those elements of $\pi_1(U(L))$ which are fixed by $\mathfrak{g}$, which gives the result.

(This proof is rather unsatisfactory, since it comes *after* the existence theorem, on which it fundamentally depends. It should be possible to show *a priori* that $\pi_1(U(K))$ is a class formation, using the exact sequences:

$$\begin{array}{ccccccccc}
0 & \to & U(K) & \to & K^* & \to & \mathbf{Z} & \to & 0 \\
0 & \to & \pi_1(U(K)) & \to & \overline{U(K)} & \to & U(K) & \to & 0
\end{array}$$

The cohomology of K^* is known to vanish; one needs to prove that the same holds for $\overline{U(K)}$; unfortunately, I cannot see why it does.)J.-P. Serre : "unfortunately, I cannot see why it does...". Tate explained this to me: cf. [**Se61a**], prop. 8.

5. The global case.

Here, it is unfortunately necessary to restrict to the case of equal characteristic. Consider a non-singular algebraic curve X over an algebraically closed field k, and the group $C_0(X)$ of ideles of degree 0 on X. This is a proalgebraic group (generalized Jacobians!) and the Lang-Rosenlicht method shows that:

THEOREM : *The Galois group of the maximal abelian extension of $k(X)$ is isomorphic to $\pi_1(C_0(X))$.*

Thus, as in the previous no:

THEOREM: *The $\pi_1(C_0(X))$ are a class formation.*

When restricting to "tamely ramified" extensions, it is enough to consider generalized Jacobians whose modules are of the type $\sum P_i$ with distinct P_i. These are extensions of the Jacobian by tori, whose π_1 can be constructed by Tate's methods, cf. no1. One recovers a "class formation" which had already been defined by Tate. J.-P. Serre : "which had already been defined by Tate. . . ". The results in question have probably not been published.

This is how far I have gotten for the present. I still need to clarify the case where the residue field is not algebraically closed, particularly when it is finite (or "quasi-finite", i.e. when the Galois group of the algebraic closure is the completion $\widehat{\mathbf{Z}}$ of $\mathbf{Z}$). I can see more or less how to do it, but the details are boring.J.-P. Serre : "the details are boring. . . ". These details were summarized at the end of the Bourbaki lecture quoted in 3.1, and they have been given in M. Hazewinkel's thesis (Amsterdam, 1959 — see also the Appendix to Chap V of Demazure-Gabriel, *Groupes Algébriques*, Masson and North-Holland, Amsterdam, 1970). It seems that group isogenies are not enough, and homogeneous spaces will be required, J.-P. Serre : "Homogeneous spaces will be required. . .". Grothendieck was to come back to this point a little later, cf. the letter of 8/9/1960. which is very annoying. Give me some courage. . .

In any case, I see clearly why local class field theory works over "quasi-finite" fields, while the global one does not. The point is that the Châtelet-Weil groups $H^q(G/k)$ have to be trivial over k when G is defined over k, and this is not true (except for a finite field) unless G is linear — which holds for local class fields, but not for global ones, because of that stupid Jacobian. Thus, one sees that in the global case the reciprocity map is not always surjective — on the other hand, the principal ideles are still contained in its kernel, and therefore Artin's reciprocity law gets cut into two, in a rather unexpected way!

I hope you will find the wherewithal to test your various conjectures in the above. Your letter was very interesting, but I had a hell of a time trying to decipher it; I had to type it out myself! (In reply to one thing you say, I should point out that the Société Mathématique de France gives 100 free reprints —

you will not therefore have to follow my illustrious example — relative to the Journal de Maths pures et appliquées.)

I have no interesting news. I have been spending my time lately studying local fields — as you have seen, I could not avoid the detailed study of the horrible functions φ and ψ. Fortunately, Lang sent me an Artin-Tate II report on the subject, which was a great help.

I also needed to understand the notion of a different, which plays a crucial role in the proofs. Here is a statement (essentially due to Dedekind, modulo the language) which surprised me a great deal:

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Let A be a Dedekind ring with fraction field K. Let L be a finite separable extension of K, and let B be the integral closure of A in L. Then the module $D_A(B)$ of differentials of the A-algebra B is monogenous, and its annihilator is the different $\mathfrak{d}_{B/A}$ (defined as the inverse ideal in B of B^*, the complement of B).

Don't you find this a very pretty statement? In the geometric case, it contains the fact that the different is the ideal generated by dT/dt (where T and t are uniformizing parameters of A and B).

The funny thing is that I just received a manuscript by Auslander and Buchsbaum J.-P. Serre : This is a reference to: M. Auslander and D.A. Buchsbaum, *On ramification theory in Noetherian rings*, Amer. J. Math. **81** (1959), 749–765. to referee for the Amer. Journal, which contains a homological definition of the different and (I hope) the property in question. They also define unramified extensions of rings in terms of the vanishing of the different; we shall have to compare this with our own stuff. Say hello to Tate from me (and if he is interested, you can show him this letter). Yours,

J-P. Serre

July 18, 1959 Alexandre Grothendieck

My dear Serre,

Lately I have been thinking again about the general formalism of (Weil) cohomology and homology of schemes, and so doing, it seems to me that I have managed to find the correct definition of "homotopic" invariants; of course, I have not gotten past the definitions, and proving they have reasonable properties is a very different matter; perhaps it will be possible to attack it via cohomology (which can be got hold of using sheaf theoretic methods, for algebraic curves for example) and the "theory modulo C". Furthermore, the same restrictions apply to the scope of my definitions as to the definition of π_1 (which ignores such phenomena as inseparability). This is only natural if one wants "topological invariants", i.e. invariants of morphisms $X' \to X$ which are "geometrically" homeomorphisms, i.e. proper, surjective and geometrically injective.

Here is the general set-up. Let $\mathcal{C}$ be a category equipped with products. For $T \in C$ and for any finite set I, one can consider the object T^I, which gives a contravariant functor K_T from finite sets to $\mathcal{C}$, i.e. a simplicial object of $\mathcal{C}$. If T' "dominates" T, i.e. if there exists a morphism $T' \to T$, then from this morphism one derives a simplicial homomorphism $K_{T'} \to K_T$, whose homotopy class is independent of the choice of $T' \to T$; thus, when T varies in $\mathcal{C}$, the K_T form a projective system with values in the homotopic category (i.e. the morphisms are simplicial homomorphisms modulo homotopy) of simplicial objects of $\mathcal{C}$. If (for example) F is a contravariant functor $\mathcal{C} \to \mathcal{C}'$, then $F(K_T)$ is a cosimplicial object in $\mathcal{C}'$, so if $\mathcal{C}'$ is abelian, then it has a cohomology which plays the same role as the Čech cohomology of a covering (which is a special case of it!) and which will be denoted $H^*(T/-, F)$. The $T/-$ indicates that T is considered as an object over the right unit in $\mathcal{C}$, if it exists; if T lies over an object S in $\mathcal{C}$ then let us write T/S and hence $K_{T/S}$ and $H^*(T/S, F)$ to indicate that T is considered as as an object in the category of objects over S (in which S is a right unit); F is therefore a functor on the latter (or on a "larger" category: $\mathcal{C}$ itself, for example). Furthermore, as T varies, the $H^*(T/S, F)$ form an inductive system of objects of $\mathcal{C}'$, whose inductive limit (if it exists) will be denoted by $\check{H}^*(-/S, F)$. Of course, instead of taking the inductive limit over the whole of $\mathcal{C}$, one can take it over a part of $\mathcal{C}$, but this boils down to replacing $\mathcal{C}$ by a subcategory. Here $\check{H}^*(-/S, F)$ plays the role of Čech cohomology with coefficients in F (which plays the role of a *pre*sheaf of coefficients). This is a cohomological functor of F, and under certain conditions which can be easily written down, it depends contravariantly

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on T (when $\mathcal{C}$, F and T all vary...). If S is a scheme, take $\mathcal{C}$ to be the category of S-schemes which are finite unions of flat coverings of open sets of S which form an open cover of S; a contravariant functor F from $\mathcal{C}$ into abelian groups can be called a Weil *pre*sheaf on S, and $\check{H}^*(-/S, F)$ is the Čech-Weil type cohomology of S with coefficients in F. If F is actually a Weil *sheaf* this can be mapped to the true Weil cohomology with coefficients in F (this is also valid in the general category-theoretic set-up), namely $H^*(S, F)$, and this will be an isomorphism under rather general conditions (for instance if F comes from a flat group scheme of finite type over S); if F comes from a group scheme which is *simple* and of finite type over S, then one can even replace the category $\mathcal{C}$ by the smaller category in which flat coverings are replaced by unramified coverings. In particular, this will be the case if F comes from an ordinary finite abelian group, which gives rise to the "Čechist" computation of the cohomology $H^*(S, F)$ for a finite group F (on which the fundamental group of S may act, if one wants "twisted" coefficients).

In the same way, a covariant functor $G : \mathcal{C} \to \mathcal{C}'$ defines a simplicial object $G(K_T)$ in $\mathcal{C}'$ for every $T \in \mathcal{C}$; thus, as T varies, one gets a projective system of simplicial objects in $\mathcal{C}'$; if $\mathcal{C}'$ is the category of sets, this is a projective system of topological spaces, which in turn gives cohomology or homology groups via inductive resp. projective limits, which are actually special cases of those considered above. If G even takes values in the category of pointed spaces, then the $G(K_T)$ form a projective system of pointed simplicial spaces, giving homotopy groups etc. by passing to the projective limit. Thus, given such a G, one gets the definition of homotopy groups with "coefficients in G" $\pi_i(T/-, G)$, $\pi_i(T/S, G)$, $\pi_i(-/S, G)$ having the obvious variance properties.

Starting with a base scheme S, take $\pi_0(T)$, the covariant functor on $\mathcal{C}$ with values in the category of sets (sets of connected components); in order for this functor to take values in the category of pointed sets, choose a geometric point $a \in S$ and consider the category $\mathcal{C}$ of objects (of the specified type) which are *pointed* over a (morphisms of such objects should send the marked geometric point to the marked geometric point). Define homotopy groups $\pi_i(T, S)$ and $\pi_i(-, S) = \pi_i(S)$ in this way (it is understood that S and T denote pointed objects). Of course, $\pi_0(S)$ and $\pi_1(S)$ are the ones which are already known. Furthermore, if F is a finite abelian group, since the value at T of the "sheaf" associated to F (also called F) is nothing other than the set of maps from $\pi_0(T)$ to F, it follows that $H^*(T/S, F) = H^*(\pi_0(K_{T/S}), F)$, and hence $H^*(S, F) = \varinjlim_T H^*(\pi_0(K_{T/S}), F)$. The "Hurewicz" type theorems should thus be entirely formal in the context of sheaf homotopy and cohomology

(modulo the classical Hurewicz theorem). — I forgot to mention that here, $\mathcal{C}$ is taken to be the category of flat pointed S-schemes which are unions of *unramified* coverings of open subsets which cover S; this choice being made, the system of $\pi_0(K_{T/S})$, and hence also the $\pi_i(S)$, are topological invariants of S in the sense given above.

To round things off, here are two typical examples of simplicial sets arising from a pair T/S. If T is an unramified Galois covering of S with group G, then $\pi_0(K_{T/S})$ is the simplicial set which in dimension n is given by G^{n+1}/G (G acting on G^{n+1} on the right via the diagonal); this is a Kan simplicial set, and unless I am mistaken it is nothing other than B_G (the classifying space of G). If S is normal and connected, mark the generic point of S, i.e. choose an algebraically closed extension Ω of its fraction field. Let G be the Galois group over K of the algebraic closure of K in Ω. Giving $T \in \mathcal{G}$ is then essentially equivalent to giving a finite set E on which G acts (with a marked point if one is considering objects T with marked points) such that the following condition is satisfied: for any $x \in E$, let G_x be the stabilizer and let $U_x \subset S$ be the set of points of S at which the finite extension of E corresponding to G_x is unramified; one wants $\bigcup U_x = S$ (N.B. If one is interested only in a cofinal subset of the set of T, one may assume that the G_x are invariant in G.) With T defined this way, $\pi_0(K_{T/S})$ is the simplicial set which is equal to E^{n+1}/G in dimension n. I have no idea what the corresponding topological space looks like (are there only a finite number of non-zero homotopy groups, are these groups finite, etc.) and in fact I am realizing the extent of my ignorance of homotopy. If you were to have some more or less secret introductory papers on this subject, I would be most interested. Actually, I am thinking of going to Die one of these days and getting Cartan to go through the basics with me. I may possibly temporarily abandon my cogitations on Picard and Co, which I had hoped to put in their final form this year, in order to have a closer look at scheme topology. To begin with, this would require a little multiplodocus on categories, which seems to me more and more indispensable "to be able to speak".

Regards,

A. Grothendieck

August 18, 1959 Alexandre Grothendieck

My dear Serre, Tate has written to me about his elliptic curves stuff, and has asked me if I had any ideas for a global definition of analytic varieties J.-P. Serre : This is a reference to the p-adic theory of "Tate curves", which is at the origin of rigid analytic geometry.

Tate's text, which was written in 1959, was published (and completed) in 1995: J. Tate, *A review of non-archimedean elliptic functions*, in "Elliptic Curves, Modular Forms and Fermat's Last Theorem" (J. Coates and S. T. Yau, eds.), Intern. Press, Boston, 1995, 162–184. over complete valuation fields. I must admit that I have absolutely not understood why his results might suggest the existence of such a definition, and I remain skeptical. Nor do I have the impression of having understood his theorem at all; it does nothing more than exhibit, via brute formulas, a certain isomorphism of analytic groups; one could conceive that other equally explicit formulas might give another one which would be no worse than his (until proof to the contrary!)

I have given just enough thought to the "infinitesimal" part of the fundamental group to convince myself that it exists and is reasonable. Here is a (surely insufficient, actually) context in which it works. Take a connected scheme S (for instance an algebraic scheme over a field), an auxiliary category $\mathcal{C}$ (for instance the category of finite algebraic schemes over k), a category $\mathcal{G}$ whose objects are $\mathcal{C}$-groups, and whose morphisms are $\mathcal{C}$-group morphisms (for instance finite algebraic groups over k). Assume that fiber products exist in $\mathcal{C}$ and that $\mathcal{G}$ satisfies the following properties: (i) $\mathcal{G}$ is stable under products, and if $u, v : G \to G'$ are morphisms in $\mathcal{G}$, then the kernel of the pair (u, v), i.e. the maximal subgroup on which they coincide — which exists, since it can be expressed using a fiber product — lies in $\mathcal{G}$. (ii) If $u : G \to G'$ is a morphism in $\mathcal{G}$, then the image group exists, is isomorphic to a quotient of G (as it should be), and belongs to $\mathcal{G}$. (iii) Every decreasing sequence of subgroups $\in \mathcal{G}$ of a $G \in \mathcal{G}$ is eventually stationary. Moreover, assume given a covariant functor F from $\mathcal{G}$ to group schemes over S (for instance $G \to S \times_k G$), and assume that : (iv) The functor F commutes with products, kernels of pairs of morphisms and images (one could say that F is "exact"). (v) If H is of the form $F(G)$ ($G \in \mathcal{G}$), then H is "special", i.e. there is an exact sequence of finite flat group schemes over S,

$$e \to H_{\text{inf}} \to H \to H_{\text{sep}} \to 0$$

where H_{inf} is purely infinitesimal (i.e. the projection $H_{\text{inf}} \to S$ is geometrically injective) and H_{sep} is unramified over S. (In the case of a base field k, such an exact sequence can be deduced from an analogous exact sequence for a

finite algebraic group G over k; moreover, if k is perfect, this sequence splits canonically, since G_{red} is then a subgroup of G which is isomorphic to G_{sep} under the projection $G \to G_{\mathrm{sep}}$. Note, however, that G_{red} does act on G_{inf}; one has only a semi-direct product). Finally, (vi) S is reduced.

Conditions (v) and (vi) look ugly, and are essentially temporary. They are useful because of this

LEMMA: *Let H be as in* (v)*, S as in* (vi)*, P a homogeneous principal H-bundle, Q another, u and v two isomorphisms from P to Q taking some given "marked point" into another one; then $u = v$.*

By twisting H, the problem can be reduced to the case where P is trivial, and the result then follows from the following fact: a section of Q is an isomorphism between S and a connected component of Q_{red}.

Note, however, that conditions (i)-(vi) do not preclude the possibility of twisted structural groups (with respect to a base field k).

Now let a be a "marked point" of S (i.e. an algebraically closed extension of the residue field of some $s \in S$). For every $G \in \mathcal{G}$, let $Z(S, a; G)$ or simply $Z(G)$ denote the set of classes (up to isomorphism) of homogeneous principal bundles over S with group $F(G)$, equipped with a marked point over a. Obviously $Z(G)$ is a functor from $\mathcal{G}$ into the category of sets. This functor has the following properties: 1) It commutes with products (since F does); 2) It commutes with kernels of pairs: in other words, if $G'' \longrightarrow G \underset{v}{\overset{u}{\rightrightarrows}} G'$ is exact in $\mathcal{G}$,

then $Z(G'') \longrightarrow Z(G) \underset{Z(v)}{\overset{Z(u)}{\rightrightarrows}} Z(G')$ is exact, i.e. $Z(G'')$ can be identified with

the set of elements in $Z(G)$ whose images in $Z(G')$ under $Z(u)$ and $Z(v)$ are the same. (This follows from the exactness of F and the *lemma*).

It follows formally from these two properties that one can find a filtered projective system $(G_i)_{i \in I}$ in $\mathcal{G}$, with morphisms $G_i \to G_j$ which are epimorphisms in $\mathcal{C}$, and which is "essentially unique" in an easily specified sense, such that there is a functorial isomorphism

$$Z(G) = \varinjlim \mathrm{Hom}_{\mathcal{G}}(G_i, G).$$

It is this projective system (considered modulo an equivalence which intuitively means that one passes to the projective limit of the G_i) which may be denoted $\pi_1^{\mathcal{G}}(S, a)$ and which plays the role of the fundamental group of S at a (with respect to the category $\mathcal{G}$ of groups, equipped with the functor F). In the base field k case, where $\mathcal{G}$ is the category of finite algebraic groups over k,

one could write[3] $\pi_1(S/k, a)$: it is the proalgebraic fundamental group (with infinitesimal part) of the k-scheme S. When k is perfect, the decomposition of a finite algebraic group into its infinitesimal part and its reduced part shows that the fundamental group is the semi-direct product of its reduced or separable part (which corresponds to an ordinary discontinuous compact group on which the ordinary fundamental group of k — i.e. the Galois group of $\overline{k}$ over k — acts) with its infinitesimal part, which actually no longer depends on the choice of the base point a of S. Note that the separable part of the fundamental group can easily be recovered using the ordinary fundamental group of S and its natural homomorphism into $\mathrm{Gal}(\overline{k}/k)$, but is "larger" than the ordinary fundamental group, since it corresponds to the classification of principal coverings with structural group not only an ordinary finite group, but also a finite group which is separable over k (i.e. an ordinary finite group on which $\mathrm{Gal}(\overline{k}/k)$ acts not necessarily trivially.) I admit that if k is not algebraically closed, then the fundamental group described above is probably not the right one; one should probably consider the fundamental group of $S \otimes_k \overline{k}$, which is a proalgebraic group defined over $\overline{k}$; note that it is equipped with descent data from $\overline{k}$ to k and thus is actually defined over k. This would work whenever the base point is *k-rational*. In any case, there should be a "pro-group scheme" $\underline{\pi}_1(S)$ over S (a local system of fundamental groups).   3/4

Here is how to prove that the projective system (G_i) exists. A pair (G, z) with $G \in \mathcal{G}$ and $z \in Z(G)$ is said to be *minimal* if there is no subgroup $G' \subset G$, $G' \neq G$, such that z is of the form $Z(i)(z')$ where $z' \in Z(G')$ and $i : G' \to G$ is the inclusion. Let us say that a pair (G, z) is *dominated* by a pair (G', z') if there is a homomorphism $u : G' \to G$ such that $z = Z(u)(z')$. It follows from (iii) that every pair (G, z) is dominated by a minimal pair, and from property 2) of Z that if (G', z') is minimal and dominates (G, z), then there is a *unique* $u : G' \to G$ such that $z = Z(u)(z')$. From this it follows that the minimal pairs (G, z) form a projective system for the relation of domination, which is in fact a directed set (since (G, z) and (G', z') are dominated by $(G \times G', (z, z'))$, which is itself dominated by some minimal (G'', z'')): this is the desired system. If one wants, one can choose a pair (G, z) in every system of isomorphic minimal pairs, in such a way that the set I of indices becomes ordered and not simply pre-ordered. (N.B. This is also the kind of formal argument that is used in "moduli theory"...).

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[3]note in the margin: wrongly!

I do not yet know how to formulate the homotopy exact sequence; to do it right, the inclusion of an infinitesimal part into the fundamental group should provide a satisfactory theory of the behavior of the fundamental group under specialization. I hope that if $f : X \to Y$ is a proper separable morphism with absolutely connected fibers, i.e. such that $f(\mathcal{O}_X) = \mathcal{O}_Y$, where for simplicity's sake X is equipped with a section s over Y which determines base points on the fibers, then the total fundamental groups of the fibers of X form a pro-group scheme over Y $\pi_1(X/Y, S)$: more precisely, I hope that it is possible to find a filtered projective system $(G_i)_{i \in I}$ of "special" group schemes G_i over Y, and homomorphisms $G_i \to G_j$ which are epimorphisms of Y-schemes (i.e. correspond to an injective homomorphism of coherent sheaves on Y), such that the total fundamental groups of the fibers of X can be deduced from the said pro-group scheme simply by specializing. (In any case, this is what the case where X is an abelian scheme over Y seems to suggest, cf. below.) Even without choosing a section, one could deduce the existence *over X* of a pro-group scheme $\underline{\pi}_1(X/Y)$, "the fundamental group of the fibers". When there is no base field k, however, it is not clear how and to which fundamental groups of X and Y one can attach the fundamental groups of the fibers to replace an exact homotopy sequence. If there is a base field, an initial conjecture would be that $\underline{\pi}_1(X/Y)$, the pro-group scheme $\underline{\pi}_1(X)$ over X of local systems of fundamental groups of X at its various points, and the preimage under f of the analogous pro-group scheme $\underline{\pi}_1(Y)$ on Y, are related by an exact sequence. Have fun with higher homotopy groups. . .

It is not said that the conjecture I look for is more difficult to prove than the part that is already known for ordinary fundamental groups. It would imply that for complete schemes X and Y over an algebraically closed field k, $\pi_1(X \times Y/k) = \pi_1(X/k) \times \pi_1(Y/k)$. Using your arguments, it follows that if X is an abelian variety over k, then $\pi_1(X/k)$ is abelian, and a *minimal* principal covering X' of X is an abelian variety which is isogenous to X. It follows that

$$\pi_1(X/k) = \varprojlim {}_nX,$$

where ${}_nX$ is the kernel (with its infinitesimal part as well, of course) of multiplication by n, the homomorphism of ${}_{mn}X$ into ${}_nX$ being induced by multiplication by m. (By Cartier's results, this fundamental group is dual, in Cartier's sense of the word, to the ind-algebraic group which is the inductive-limit of the ${}_nX^*$, where X^* is the dual variety of X.) Taking the p-component of this fundamental group, one obtains something which, for a prime number p, should play the role of the Weil module. There is no doubt that it is possible

(and Cartier must have already done it) to functorially associate J.-P. Serre : "It is possible ... a module over the Witt ring". This was done (and continues to be done in increasing generality) by Cartier, Gabriel, Manin, Fontaine ... See for instance Chap.V of the work by Demazure and Gabriel quoted in note 9.2. a module over the Witt ring to an abelian infinitesimal pro-algebraic group, and that it is easy to check that the module decomposes into the three parts you know (corresponding to the three principal types of abelian algebraic p-groups). Thus, one obtains a more natural formulation of your theorem (which should give a universal proof which does not distinguish the cases $\ell \neq p$ and $\ell = p$), and at the same time, one sees that your bewildering sum behaves well when X varies in a family, i.e. if X is an abelian scheme over Y (since in this case the $_nX$ are wonderful finite flat group schemes over Y, whose projective limit can be taken formally...).

Next year, I hope to getJ.-P. Serre : "Next year, I hope to get..." Hope springs eternal! This reminds one of the beginnings of Bourbaki, who had hoped to be done within a few years.

In fact, the EGA's stopped after chapter IV, a text of almost 800 pages, whose last part came out in 1967. a satisfactory theory of the fundamental group, and finish up the writing of chapters IV, V, VI, VII (the last one being the fundamental group) at the same time as categories. In two years, residues, duality, intersections, Chern, and Riemann-Roch. In three years, Weil cohomology, and a little homotopy, God willing. In between, I don't know when, the "big existence theorem" with Picard etc., some algebraic curves, and abelian schemes. Unless there are unexpected difficulties or I get bogged down, the multiplodocus should be ready in 3 years time, or 4 at the outside. We will then be able to start doing algebraic geometry! Yours, A. Grothendieck

October 25, 1959 Jean-Pierre Serre
Dear Grothendieck,

How are the categories? I read in the Tribu that you are "in the process" of writing them. Is this true, and have you temporarily abandoned the Multiplodocus? I would also like to know how the latter is getting on, and if we can count on a rapid publication.

Princeton is very interesting this year, much more so than usual. Atiyah-Hirzebruch are giving us a wonderful seminar on their $K(X)$. Weil is telling us equally exciting things about adelic groupsJ.-P. Serre : This lecture course was published by the IAS and later reproduced by Birkhäuser: A. Weil, *Adèles and Algebraic groups* (notes by M. Demazure and T. Ono), Birkhäuser, Boston, 1982.

See also: A. Weil, *Adèles et groupes algébriques*, Séminaire Bourbaki 1958/59, no186 = Oe. Sci. [1959a].; it has recently been discovered that the question of convergence of the famous product measure that you know is linked to the computation of the number of points of reductions modulo p; more precisely, let V be non-singular, defined over a number field (or a field of functions) and let ω be a differential form of maximal degree on V, everywhere regular and non-zero (on a group take the translation-invariant form). Let $n = \dim(V)$, and let V_p (p prime in the field) be the integral p-adic points of V; then

$$\int_{V_p} |\omega|_p = \tfrac{1}{(Np)^n}(\text{number of points of } V_p \text{ reduced modulo } p),$$

which holds for almost all p. (Anyway, V_p and its reduction are only "intrinsic" for almost all p.) The question of convergence of the infinite product of the $\int_{V_p} |\omega|_p$ is hence reduced to a typically Weil-type question about numbers of points. If V is a semi-simple group, one knows how to do this computation (I did it with Hertzig in the old days) and thereby prove convergence for *all* semi-simple groups, including exceptional groups. Finite volume or the Tamagawa number are still a long way off, but this is still something. It also shows (assuming the Weil conjectures) that there will be divergence for any complete variety.

Another interesting question raised by Weil is that of the restriction of scalars. This problem can be perfectly well expressed in terms of schemes: given two "base preschemes" S and T, and a morphism $T \to S$, one would like to associate to any T-prescheme V an S-prescheme $R_{T/S}(V)$ and a morphism p :

$R_{T/S}(V) \times_S T \to V$ such that for every S-prescheme U, the obvious map from

$$\mathrm{Morph}_S(U, R_{T/S}(V)) \quad \text{to} \quad \mathrm{Morph}_T(U \times_S T, V)$$

is bijective.

   This probably doesn't exist except under very strong conditions J.-P. Serre : "This probably doesn't exist...": see for instance §7.6 in S. Bosch, W. Lütkebohmert and M. Raynaud, *Néron Models*, Springer-Verlag, 1990., like T finite over S, and the image sheaf of the structural sheaf of T locally free over that of S. In any case, these conditions make the affine case work, but neither Tate nor I have had the courage to go farther. The projective case needs to be dealt with. In any case, the scheme $R_{T/S}(V)$ does not always exist even in classical algebraic geometry; there are Nagata-type counterexamples. What is the link between this and the descent data you told me about?

   Lang says he finds all this insufficient, and that one should find a big bag containing all this stuff together with the "Greenberg functor" (which Greenberg has apparently only written up for affine spaces). Have you done this?

   I have spent my time thinking about the Artin representation and the Weil conjectures (particularly the L series formalism). I have nothing precise to tell you. I am wondering (and have not yet been able to decide) if the generalization of the Weil formula $\sigma(X.X') \geq 0$ might not be $\sigma_n(X.X') \geq 0$, where X is a algebraic correspondence on V (non-singular of dimension n), $X' = \mathrm{transp}(X)$,J.-P. Serre : No, this is not the right way to define X' if one wants to generalize the Castelnuovo-Weil formula $\sigma(X \cdot X') \geq 0$. One has to introduce a *polarization* on V, cf. [**Se60a**]. and σ_n denotes the trace of the homology representation in dimension exactly n. A priori, this is not ridiculous, and would lead to a natural plan of a proof of the Weil conjectures on the absolute value of the eigenvalues of Frobenius: in dimension n, the equation $F.F' = q^n \cdot 1$ ($F = $ Frobenius), together with the positivity of the trace, shows that this eigenvalue is $q^{n/2}$, as it should be: in dimension $< n$, a Lefschetz-type theorem will hopefully allow us to reduce to a hyperplane section; in dimension $> n$, Poincaré duality allows us to reduce to the previous case. It is very tempting, but one should at least check that $\sigma_n(X.X')$ is ≥ 0 in the classical case, by a Kählerian argument, and I have not managed either to do it or to find a counterexample!

Our seminar on the Multiplodocus is progressing nicely. On Saturday, I will do spectra of graded rings and start projective morphisms. Here are some comments from Tate, Borel, Demazure. . . on what we have done so far:

1) Let us say that a space is "locally ringed" if it is a ringed space whose rings at points are all local rings. For such spaces, the notion of homomorphism is restricted, as usual (by requiring them to be "local homomorphisms"); by the way, the definition you give of them in Chap.I, §2, p.23, Déf.3 is wrong, as the rings $\mathcal{O}_x$ and $\mathcal{O}_{\varphi(x)}$ have been exchanged.

Then, for a locally ringed space $(Y, \mathcal{O}_Y)$ to be an affine scheme, it is necessary and sufficient that for every locally ringed space $(X, \mathcal{O}_X)$, the obvious map from $\mathrm{Hom}(X, Y)$ into $\mathrm{Hom}(\Gamma(\mathcal{O}_Y), \Gamma(\mathcal{O}_X))$ should be bijective.

This is a more precise statement than prop.3, chap.I, §2, and it is very easy to prove, as you can guess.

2) The definition of sub-preschemes (I, §3, def.1) is incorrect as given. Why not define immersions directly, which is easy, and then indicate an ad hoc procedure for constructing a canonical element in any class of equivalent immersions, called a sub-prescheme? That would be more honest.

3) In the products (I, §2, prop.10), it is a pity that you use the fact that two extensions of the same field can be embedded in a third. It would be more in the spirit of the Multiplodocus to say that the points of $X \times_S Y$ over a pair (x, y) correspond to points of $\mathrm{Spec}(k(x)) \times_{\mathrm{Spec}(k(s))} \mathrm{Spec}(k(y))$, and this ring is never zero, so has a non-empty spectrum. This has the advantage of showing that the points in question correspond to "compositions" of $k(x)$ and $k(y)$, and if you want to refer to Bourbaki, you would do better to refer to Chap VIII of Algebra.

4) To show that a finite morphism $\pi : Y \to X$ is projective, can't you just exhibit a graded algebra, namely $\underline{A}[T]$, where $\underline{A} = \pi_*(\mathcal{O}_Y)$?

5) When you define locally free sheaves in Chap. II, you say they are coherent; this is obviously false without the hypothesis that the sheaf of rings is itself coherent.

By the way, here is an amusing example of a "coherent" ring (i.e. for which the module of relations between a finite set of elements is finitely generated) which is not Noetherian: a non-discrete valuation ring. Another amusing example: let K be a "compact Stein variety" (for instance a real compact analytic variety), and let A be the algebra of holomorphic functions on a

neighborhood. One can show that A is coherent, but it is not known to be Noetherian.

6) It seems to me that in the homogeneous spectrum of a graded ring, you have forgotten to say that the local ring at a point can be identified with the degree zero component of you know what; after all, this gives a definition of Proj( ), which is nice.

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I haven't got much other news. Ask Dieudonné if there is any chance he might send me a copy of the final version of all this. I would be very interested to see it.

Tate gave a very pretty lecture on adelic points from the point of view of schemes; they are the same J.-P. Serre : I have a very vivid memory of attempting to explain to Weil (resp. Grothendieck) that Grothendieck's (resp. Weil's) definition is equivalent to his. Neither of them wanted to listen: his definition was obviously "the right one", why go looking for another one? as Weil's in the separated case. For an arbitrary prescheme, two distinct adelic points can correspond to the same point in each local field; I constructed an example of this. Everything I needed was actually already contained in your Chap.I, §5, since the spectra of local fields form a dense open set in the spectrum of adeles.

I have taken on the task of presenting Abhyankar's work in Weil's seminarJ.-P. Serre : This is a reference to:

S. Abhyankar, *Tame coverings and fundamental groups of algebraic varieties*I, Amer. J. Math. **81** (1959), 46–94.

I presented this work in a Bourbaki seminar (1959/60, no204).. It is rather simple in the end, except that the guy appears to muddle everything in sight. Be particularly suspicious of the statements in his paper noI in the Amer. J.; all the main theorems are false (or rather, their proofs are false — it is possible that some of them are true nevertheless) J.-P. Serre : "it is possible that some of them are true nevertheless. . . " Indeed, they were justified 20 years later by the work of W. Fulton and J. Hansen:

W. Fulton and J. Hansen, *A connectedness theorem for projective varieties, with applications to intersections and singularities of mappings*, Ann. Math. **109** (1979), 159–166;

W. Fulton, *On the fundamental group of the complement of a node curve*, Ann. Math. **111** (1980), 407–409.

See also Deligne's talk in the Bourbaki seminar 1979/80, no543. when the curves have actual double points. This is contained in the noII which will appear soon. I may talk about it in a Bourbaki seminar shortly.

I cannot resist the pleasure of showing you a cute proof (there are others, of course) of the fact that the projective space $\mathbf{P}_n$ is simply connected: Let V be an unramified covering of the said space, and let W be the projective cone over V (that is to say, the affine variety whose coordinate ring is equal to $\sum H^0(V, \mathcal{O}_V(n))$. It is immediate that this is a normal variety, and that it is a covering of the plane k^{n+1} which is unramified except (perhaps) at the origin. By the famous "purity theorem" it is not ramified at all, whence etc.

Yours,

J-P. Serre

Borel has invited you to come to the Institute next year. Are you tempted? I would ask for nothing better, especially if you only stay for the first semester, like me.

October 31, 1959 ALEXANDRE GROTHENDIECK

My dear Serre,

No, I have not abandoned the multiplodocus for the categories, and am only writing up the latter in so far as the MasterJ.-P. Serre : "The Master": Bourbaki, of course. obliges me to do so, which means I will probably end up with a completed manuscript in a year's time. I worked on it for a month this summer but have dropped it for the moment, in order to spend the last couple of weeks getting down to the general existence theorem in scheme theory. I am hopeful of getting it out shortly, and will give a Bourbaki talk on what I know about descent techniques (which, together with the theorems of formal geometry, notably its "existence theorem", should lead to a characteristion of functors $T \mapsto \mathrm{Hom}_S(T, X)$). Dieudonné has sent Chapters 0 and I to the press, and I think he will have finished Chapters II and II in time for them to be printed in December or January. As for me, Chapters IV and V are essentially finished, and I will be able to send notes to Dieudonné as soon as necessary; if he works hard we might yet finish Chapters IV, V and VI next yearJ.-P. Serre : In fact, neither chapter V nor chapter VI were ever written up. A part of the preliminary notes for Chap. V were translated into English and published: J. Blass, P. Blass and S. Klasa, *EGA V, Parts I-IV*, Ulam Quarterly **1-2** (1992–1993). (VI being descent techniques and existence theorems).

Thank you for your comments on the old draft; various corrections had already been made. I agree with your logical criticism of the sub-preschemes; I was also uncomfortable with the definition. However, it does not seem to me to be sufficiently wrong to require yet another change (no more than your comment on products, with which I otherwise also agree).

I have no comments on your attempts to generalize the Weil-Castelnuovo inequality; I confess that these positivity questions have not really penetrated into my yoga yet; besides, as you know, I have a sketch of a proof J.-P. Serre : This "sketch of a proof" did not work, cf. the letter of April 2, 1964. of the Weil conjectures based on the curves case, which means I am not that excited about your idea. By the way, did you receive a letter from me two months ago in which I told you about the fundamental group and its infinitesimal part? You probably have nothing to say about that either! Actually, I have hardly looked at the question since then myself, having been distracted in the meanwhile by questions of classification (et al.) for algebraic (or formal) groups (the latter, in characteristic $p \neq 0$, form the dual category of the category of pro-algebraic groups — in which nilpotent elements are allowed — which are projective limits of *finite* algebraic groups). I have tried

1/2

in vain to extract something precise from Cartier, who only half-remembers
his own old work, and who has sold me various wrong statements (which will
no doubt become useful when their time comes, however). Alas, he is getting
worked up about probabilities, being in the army, J.-P. Serre : The expression
in the original text is "il est bonvoust", ENS slang term for "he is doing his
military service". I already foresee that I will be compelled to include a chapter
on commutative groups (i.e. commutative group schemes, of course) in the
multiplodocus, because of the intersection with Weil cohomology, homotopy
groups (starting with the fundamental group), and abelian schemes. Maybe,
if it seems convenient, we could write that chapter together, if you do not
object in principle to collaborating sporadically on the Multiplodocus. By the
way, when and how are you thinking of giving us your Historical Notes?J.-P.
Serre : Grothendieck had suggested I write *Historical Notes* for the EGA's. I
never did. If you still intend to do it, it would be reasonable if you were to
write at least something that would go after Chapter III; after that, Chap. VI
would certainly give you another excellent opportunity to play the historian
(Chapters IV and V are more technical in nature and will probably inspire you
less).

I have indeed fully clarified the Weil and Greenberg operations, J.-P.
Serre : Alas, Grothendieck never wrote up anything on the Greenberg functor.
which at one time I was even thinking of presenting in a Bourbaki seminar; as
Lang said, they go in the same bag, otherwise the proofs have to be repeated.
For simplicity's sake, let me only discuss Weil here: Given T over S and X
over T, one wants a prescheme N over S and an isomorphism of functors
in S'/S

$$\mathrm{Hom}_S(S', N) = \text{sections of } (X' = X \times_S S')$$
$$\text{over } T' = T \times_S S' = \ T\text{-morphisms } T \times_S S' \to X$$

Such an N, which is obviously unique up to unique isomorphism, could be
denoted by $\underline{\mathrm{Hom}}_{T/S}(T/T, X/T)$; more generally, given X and Y over T, one
might ask whether $\underline{\mathrm{Hom}}_{T/S}(X/T, Y/T)$ exists, but this can be reduced to the
previous question, namely the existence of $\underline{\mathrm{Hom}}_{X/S}(X/X, X \times_T Y/X)$. An
interesting special case of this problem is the existence of $\underline{\mathrm{Hom}}_S(X, Y)$ for
two S-preschemes X and Y, i.e. the existence of $\underline{\mathrm{Hom}}_{X/S}(X/X, X \times_S Y/X)$; if
it exists and X and Y are proper over a locally Noetherian S, then $\underline{\mathrm{Is}}_S(X, Y)$
also exists, and is an open sub-prescheme (when $X = Y$, this is Matsusaka's
situation, and one gets a group prescheme, $\underline{\mathrm{Aut}}_S(X)$). Actually, in Weil's

case $\underline{\mathrm{Hom}}_{T/S}(T/T, X/T)$, assuming $\underline{\mathrm{Hom}}_S(T, X)$ and $\underline{\mathrm{Hom}}_S(T, T)$ exist, then so does the former, and the following formula holds:

$$\underline{\mathrm{Hom}}_{T/S}(T/T, X/T) = \underline{\mathrm{Hom}}_S(T, X) \times_{\underline{\mathrm{Hom}}_S(T,T)} S$$

(the preimage of the identity section of $\underline{\mathrm{Hom}}_S(T, T)$ under the obvious morphism $\underline{\mathrm{Hom}}_S(T, X) \to \underline{\mathrm{Hom}}_S(T, T)$ induced by $X \to T$). However, it is possible for the Weil scheme to exist even if $\underline{\mathrm{Hom}}_S(T, X)$ and $\underline{\mathrm{Hom}}_S(T, T)$ do not both exist, which is why it seems to me that the basic fundamental operation to which all the others can be reduced (unless something goes wrong with existence) really is the Weil operation. As for terminology and notation, the latter deserves to be called either the *direct image* of X/T under $T \to S$ or the *norm prescheme* $N_{T/S}(X/T)$. I prefer the second notation, inspired by the case $T = S \times I$, where I is a finite set as usual (and T is a direct sum of open sub-objects T_i which are all isomorphic to S): one then has $N_{T/S}(X/T) = \prod_{i \in I}(X/T_i)$, which corresponds well to the idea of a norm (the fiber at $s \in S$ is the product of the fibers of X at those points of T lying over s). The natural map $\Gamma(X/T) \to \Gamma(N/S)$ (the inverse of the bijection in the other direction deduced from the definition of N) deserves to be called the *norm map*. When T is *finite* and "locally free" of rank r over S, and when $X = Y \times_S T$ and there is a symmetric S-morphism $Y^r \to Z$ (the Cartesian product obviously being taken over S), it is possible to define a corresponding map $N_{T/S}(X/T) = \underline{\mathrm{Hom}}_S(T, Y) \to Z$, whence, given the norm map defined above, there is a composition map[4] $\Gamma(X/T) = \Gamma(Y \times_S T/T) \to \Gamma(Z/S)$, again called the *norm map* (associated to the symmetric S-morphism $Y^r \to Z$). If the symmetric power $\underline{S}^r(Y/S)$ of Y over S exists, one should introduce the universal morphism $\underline{\mathrm{Hom}}_S(T, Y) \to \underline{S}^r(Y/S)$; when Y is a commutative group prescheme over S, one similarly obtains a map (usually called the norm map) $\Gamma(Y \times_S T/T) \to \Gamma(Y/S)$; for the additive or multiplicative groups, one obviously recovers the usual trace and norm maps. As for the point of view of the "direct image" of X/T under $T \to S$, it is suggested by the formula

$$N_{T/S}(\check{\underline{V}}(\underline{E})) = \check{\underline{V}}(p_*(E))$$

(valid for S, T as above), where E is locally free (of finite rank) over T, and $\check{\underline{V}}$ denotes the vector bundle covariantly associated to a locally free sheaf (the multiplodocus forces me to keep the notation $\underline{V}$ for the contravariant functor, the only one generally defined for an arbitrary quasi-coherent sheaf). However, I do not want to use the notation $p_*(X/T)$, as I had started to do, since this leads

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[4]N.B. In fact, one starts by defining the latter directly

to terrible confusion when X happens to be a closed sub-prescheme of T (which is a very important case, despite its pathological appearance). Finally, to finish up with these formal generalities, if X is equipped with descent data, i.e. with a $T \times_S T$-morphism from $X \times_S T$ to $T \times_S X$, then *if* the descent data is effective (i.e. makes X isomorphic to some $Y \times_S T$) and *if* furthermore $N_{T/S}(X/T)$ exists, then Y can be identified with the sub-prescheme of $N_{T/S}(X/T)$ defined by the coinciding of the two natural morphisms $N_{T/S}(X/T) \to N_{T \times_S T/S}(X \times_S T) \xrightarrow{\sim} N_{T \times_S T/S}(T \times_S X)$ and $N_{T/S}(X/T) \to N_{T \times_S T/S}(T \times_S X)$, induced respectively by the two projections from $T \times_S T$ to T; of course this is simply the definition of the terms that appear, and expresses the fact that the sections of Y over S should "be the same as" the sections of X over T whose preimages over $X \times_S T$ and $T \times_S X$ over $T \times_S T$ correspond to each other under the descent isomorphism.

As for existence questions, note first that if $\underline{\mathrm{Hom}}_S(T, Y)$ is to exist for every Y (or even just for Y finite over S), then T *must* be flat over S, and thus it is natural to restrict to this case [although it is possible that there are other interesting cases in which descent is possible for certain pairs (Y, T)]. Furthermore, it is easy to convince oneself from the affine case that there is no reasonable hope that $\underline{\mathrm{Hom}}_S(T, Y)$ or $N_{T/S}(X/T)$ should exist when T is not proper over S (here, again, there is an interesting exception under certain conditions, if X is a closed sub-prescheme of T and T is not assumed to be proper over S; such a situation arises when seeking to define the *center* of a group scheme, for instance, or more generally the kernel of a representation of a group scheme by operations on a scheme, or the largest scheme invariant under G, etc.) Finally, Nagata's counterexample shows that, at the very least, additional hypotheses such as quasi-projectivity are needed on X/T. It does seem to me that my general existence theorem for schemes (which is not yet absolutely complete) should show that if S is locally Noetherian, then these conditions (T/S flat and proper, X/T quasi-projective) are sufficient in order for $N_{T/S}(X/T)$ to exist. In fact, when T/S is finite and locally free, it is not difficult (without Noetherian conditions) to do it by direct construction. Then, if X/T is affine, $N_{T/S}(X/T)$ exists and is affine (as you checked with Tate). Furthermore, if every finite subset of X contained in a fiber of X *over S* is contained in an open affine subset of X, — in particular, if X/T is quasi-projective — then $N_{T/S}(X/T)$ exists and is a union of the open sets $N_{T/S}(U/T)$, where U runs over the open affine sets of X. Moreover, in the preceding condition, it would have been enough to consider the finite subsets of X which come from sections of $X \otimes_S K$ over $T \otimes_S K$, where K is a finite extension of a

residue field of S; in particular, this condition is satisfied if the morphism $T \to S$ is *purely inseparable* without any condition on X/T. [This case is particularly interesting J.-P. Serre : "This case is particularly interesting . . . ". It is especially interesting when $T = \mathrm{Spec}(L)$, $S = \mathrm{Spec}(K)$, where K is a field and L is a purely inseparable extension of K, cf. J. Oesterlé, *Nombres de Tamagawa et groupes unipotents en caractéristique p*, Invent. Math. **78** (1984), 13–88. in the infinitesimal theory of bundles; Weil's extended varieties are nothing other than $\underline{\mathrm{Hom}}_S(T, X)$, where T corresponds to a locally free sheaf of augmented local algebras over S; the famous transitivity relation $(V^A)^B = V^{A \otimes B}$ is nothing other than $\underline{\mathrm{Hom}}_S(T \times T', X) = \underline{\mathrm{Hom}}_S(T, \underline{\mathrm{Hom}}_S(T', X))$; I spent a day or two looking into these things, and the operation $\underline{\mathrm{Hom}}$ really seems to me to be the key element in the differential theory of bundles: in particular, Weil's heavy proofs (and statements)J.-P. Serre : "Weil's heavy proofs. . . ". This is a reference to a Bourbaki draft on varieties. If V is a k-variety, and A is a finite-dimensional local k-algebra, with residue field k, Weil defines the variety V^A of *neighboring points* of V of type A; if $A = k[\epsilon]$ with $\epsilon^2 = 0$, for example, then V^A is the tangent bundle of V. One of his main results is an isomorphism $(V^A)^B \cong V^{A \otimes B}$, cf. Weil, Oe. II, p. 107. get reduced to almost nothing.] Restricting for safety's sake to the existence cases I have just mentioned, one even easily shows the following: if $f : X \to X'$ is an affine map (resp. of finite type, resp. an embedding, resp. an open embedding, resp. a closed embedding), then so is $N_{T/S}(f)$; thus if f is separated, so is $N_{T/S}(f)$. If f is quasi-projective, so is $N_{T/S}(f)$, and in the case $X' = T$, a very ample invertible sheaf on X/T naturally gives rise to one on $N_{T/S}(X/T)$. (N.B. every locally free sheaf E on X gives rise to one on $N \times_S T$ by inverse image, and this, by projection, determines a locally free sheaf on N, of rank rn if E is of rank n; if E is invertible, then the determinant sheaf of the said sheaf on N is an invertible sheaf on N, which is very ample if E is). Beware: if X/T is projective, $N_{T/S}(X/T)$ is not projective in general; that will only hold if T/S is etale (or, as I said above, unramified). In the latter case (which is in fact the one considered by Weil), $N_{T/S}$ also takes proper morphisms to proper morphisms. Note further that even if X/T is finite, it is still possible for $N_{T/S}(X/T)$ to be neither proper nor quasi-finite over S: for example, the algebraic scheme of endomorphisms (or automorphisms) of a finite algebraic group over a field k (take any which is non-separable) is an affine group which is in general of dimension > 0. Once again, the dimension of the fibers of a morphism is only conserved by the functor $N_{T/S}$ if T/S is etale. Neither does flatness seem to behave well under the functor $N_{T/S}$ (except for this very

special case which morally is hardly different from $T = S \times I$ for a finite set I). This makes it all the more pleasing that the following thing is true: provided only that norm schemes exist (and assuming that S is locally Noetherian and that X, X', $N_{S/T}(X/T)$, $N_{S/T}(X'/T)$ are of finite type over T) $N_{T/S}$ *takes simple maps* (resp. *etale maps* — i.e. unramified maps in my old terminology) *to simple maps* (resp. *etale maps*) J.-P. Serre : "simple" is now called "smooth".; in this case, the dimension of the fibers is multiplied by exactly r (and in the finite case, there is an analogous bound on the maximal number of geometric points in a fiber). In fact, this result even holds without any conditions on T/S (not finiteness, nor properness, nor flatness; only that S and T are locally Noetherian), thanks to the following characterization of simple maps: let $f : X \to Y$ be an S-morphism, where X and Y are of finite type over a locally Noetherian S. For f to be simple, it is necessary and sufficient that for any affine S' of finite type over S, every closed subscheme S'_1 of S' having the same reduced subscheme as S', and every commutative diagram of S-morphisms

$$
\begin{array}{ccc}
X & \xrightarrow{\ f\ } & Y \\
\uparrow & & \uparrow \\
S'_1 & \xrightarrow{\ i\ } & S'
\end{array}
$$

(where i is the injection), there is an arrow $S' \to X$ such that this diagram remains commutative (N.B. This can be reduced to the case $Y = S$; in this case, it is enough to check the criterion if S' is finite over S, and local Artinian.).

This implies that the functor J.-P. Serre : See D. Ferrand, *Un foncteur norme*, Bull. S.M.F. **126** (1998), 1–49. $N_{T/S}$ transforms simple principal bundles into simple principal bundles, etc. When T/S is finite and the geometric rank of the fibers of T/S is locally constant, one sees that if X is an etale covering of T, then $N_{T/S}(X, T)$ over S has the same property as T/S, and, being etale, is *finite* over S, i.e. is an etale covering of S. By formal arguments, it follows that $T \to S$ has a canonical factorization through $T \to T' \to S$, where $T \to T'$ is a purely inseparable flat covering, and $T' \to S$ is an etale covering.

Yours,

A. Grothendieck

November 5, 1959 Alexandre Grothendieck

My dear Serre,

I beg your pardon for the unusual colour of this letter[5] — but as Mireille is still unwell, I have not had the time to go and buy another ribbon (and you must have seen from the last letter why a change of ribbon was necessary).

The "Grand existence theorem" J.-P. Serre : "Grand existence theorem". See the Bourbaki talks by Grothendieck collected in FGA. is progressing little by little; many technical difficulties remain, but I am more and more convinced that there is an absolutely marvellous technique at the end of all this. I have already come to the practical conclusion that every time that my criteria show that no moduli variety (or rather, moduli scheme) for the classification of (global or infinitesimal) variations of certain structures (complete non-singular varieties, vector bundles etc.) can exist, despite good hypotheses of flatness, properness, and if necessary non-singularity, the only reason is the existence of automorphisms of the structure which prevent the descent from working. However, I am convinced it is true (and that I will shortly be able to prove) that there exists a scheme M defined over $\mathbf{Z}$ which is locally of finite type over $\mathbf{Z}$, and a simple projective scheme V over M such that the scheme $\underline{\mathrm{Aut}}_M(V)$ is reduced to the trivial group scheme over M, so that for every locally Noetherian prescheme S and every W over S with the same properties as V/M, there exists a unique morphism $S \longrightarrow M$ such that W is isomorphic to the inverse image of V/M under the said morphism. However, as can already be seen for elliptic curves (even with "non-exceptional" invariant, i.e. with automorphism group $\mathbf{Z}/2$), no such thing is true if there are automorphisms in the family under consideration. The remedy in moduli theory seems to me to be to eliminate bothersome automorphisms by introducing additional structures on the objects being studied: points or differential forms etc. on the varying varieties (a process which is already used for curves), trivializations at sufficiently many points of the vector bundles one wants to vary, etc. By the way, the precise condition $\underline{\mathrm{Aut}}_M(V) = (e)$ can be given explicitly: for every "geometric" fiber X (i.e. over an algebraically closed field k) of W/S, X does not have any "finite" or "infinitesimal" automorphism, this last condition meaning that $H^0(X, \mathcal{G}_{X/k}) = 0$.J.-P. Serre : The notation $\mathcal{G}_{X/k}$ means the sheaf of k-derivations of $\mathcal{O}_X$ (tangent sheaf), cf. FGA, 195–197. It follows from the first in characteristic 0.

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[5]It was written in red ink and the previous one was almost transparent!

In any case, with what I know already, I should be able to show that if X is such a variety (non-singular, and projective for safety's sake) without finite or infinitesimal automorphisms, then there exists a *smallest* subfield K of k, which is obviously a finitely generated extension of the original field, over which X can be defined. Assuming the existence of some (M, V) as above, X/k is defined by a k-valued point of V, and K will be the image in k of the residue field at the corresponding point x in M. Furthermore, the formal moduli variety of X/k (whose existence I already know in any case) will be the spectrum of the completion of the local ring of the k-rational point of $M \otimes_{\mathbf{Z}} k$ corresponding to x (N.B. I am taking the formal moduli variety to be defined over k; if, in characteristic $p \neq 0$, one prefers to think of it as being defined over the p-ring $A(k)$ defined by k — which is better — then in the above one should take $M \otimes_{\mathbf{Z}} A(k)$, or alternatively, if one wants an expression intrinsic to $M/\mathbf{Z}$ without base change, it is also the spectrum of $\varprojlim \mathrm{P}_{M/\mathbf{Z}}^{(n)}(x) \otimes_{\kappa(x)} k$, where the P^n are as in my Bourbaki report). Assume for simplicity that the base taken for M is not Z but the prime field k_0 of k. Then one has the inequalities:

(1) $\dim K/k_0 = \dim \kappa(x)/k_0 \leq \dim(\text{Formal moduli variety of X})$

(2) $= \text{dimension of } M \text{ in a neighborhood of } x$

(3) $\leq \dim H^1(X, \mathcal{G}_{X/k}).$

(The inequalities which do not involve M itself can probably be proved as of now). The first inequality is an equality if and only if x is a generic point of a component of M, and the second is an equality if and only if the variety M (or the formal moduli variety, it's all the same) is smooth at x. The equality

$$\dim K/k_0 = \dim H^1(X, \mathcal{G}_{X/k})$$

thus means a combination of both.

I am writing this to you because it should enable you to give me an example answering (in the negative) Weil's question on whether the absolutely irreducible components of the Chow variety are defined over the prime field (it would serve them right). J.-P. Serre : "an example...in the negative". I constructed such an example five years later, cf. [**Se64b**]. Indeed, Weil's conjecture implies that every simple projective algebraic variety X can be obtained from an algebraic family of such varieties, whose parameter variety T is defined (i.e. absolutely irreducible) over the prime field. If X had no (finite or infinitesimal) automorphisms, such a family would come from a morphism from T to M, and it would follow that if in addition the point s in M corresponding to X

is generic, then the smallest field of definition K of X is a *regular* extension of the prime field. However, this looks very unlikely to me. For example, it shouldn't take witchcraft to find a projective simple algebraic variety defined over $\mathbf{C}$, without finite automorphisms, such that in addition $H^1(X, \mathcal{G}_X) = 0$, and which can therefore be defined (by the above) over a number field, but cannot be defined over $\mathbf{Q}$ itself (which can be seen by showing that there is an automorphism s of the field of complex numbers such that X is not isomorphic to X^s). If you find such an example, then it would of course be interesting to take a closer look at the topological structure of X and X^s, in case X and X^s might happen to have different topological invariants. [This then assumes that $s \neq$ complex conjugation.]

I forgot to tell you in my last letter that Dieudonné only has his own personal copy of the definitive version of Chap. 0 and 1 of the Elements (he is currently writing Chap. II). He has however sent you the only section you have not been able to read, namely the sorite on formal schemes. I admit that we have the impression that this is the heaviest section, and its presentation does not yet seem very refined; your comments will be particularly welcome. As for the sequel, you will read it (if you have the courage) in Paris.

I also forgot to reply to Borel about his "initial contact". It would be easiest if you were to tell him that there can be no question of my coming to Princeton next year, since, having already visited the USA recently on an Exchange Visa, I am not allowed to set foot on their precious soil until two years have passed. Anyway, I must say that if I were going to the USA at all, I would be more tempted by Harvard, which I find more pleasant.

After various interruptions, I come back to this letter. About the project for a counterexample to Weil's question, let me add that using formal moduli theory and the existence theory from Formal Geometry (i.e. things that have been written up), the following is easy to prove: let S be an irreducible algebraic scheme over an algebraically closed k, and X a proper simple S-scheme; assume that for every $s \in S$, $H^1(X_s, \mathcal{G}_{X_s/\kappa(s)}) = 0$ (N.B. in any case, the set of s having this property forms an open subset of S); *then all the X_t* (where t runs over the k-points of S) *are isomorphic to each other.* Conclusion: to find a counterexample to Weil's question, it is enough to find a projective and smooth algebraic scheme over k such that $H^1(X, \mathcal{G}_{X/k}) = 0$, and which is not isomorphic to all its conjugates. In other words, there is then no need to worry about the existence of automorphisms of X.

There was a slip in my last letter: when I assert that $N_{T/S}(X/T)$ is simple over S if X is simple over T, I forgot to specify that T should be affine over S, otherwise the proof is obviously wrong, and it is even easy to give counterexamples. As it happens, I have just received a letter from Lang, who is worried that I may publish something on Greenberg before Greenberg himself does. He can calm down; it is intended for Chapter 5, which will probably not come out before the end of 1960 at the very best. Lang claims that in the case of a local Artinian ring A with perfect residue field k, Greenberg knows how to "completely characterize" the schemes over k which come from simple schemes over A, and talks vaguely about successive bundles with affine fibers... In fact, knowing Lang, J.-P. Serre : "In fact, knowing Lang...". Grothendieck was right: neither Greenberg nor anybody else has been able to give such a characterization. I mentioned this question in my talk at the Stockholm ICM, [**Se62b**]. I very much doubt that there really is such a "characterization", which I think must involve additional rather subtle structures (higher order connections for equal characteristic, in particular). In any case, this is the question I have not yet cleared up, and which seems to me to be the really crucial point if the Greenberg functor is actually to be useful — which I still hope. In any case, I would be interested to know if Greenberg knows something, and if so, what. (The fibration business is obvious in any case).

It may be that the Greenberg functor (modulo clarification of the question above) enables us to resolve by purely formal explicit constructions the question I asked Tate, namely: if X is an abelian variety over an algebraically closed field k of characteristic $p \neq 0$, is it true that its formal moduli variety M (in the category of simple proper schemes with marked section) is regular and that the corresponding simple proper formal scheme V/M is abelian (i.e. equipped with a group scheme structure for which the marked section is the identity)? In fact, this is an existence question: over the ring B of formal series in n^2 variables ($n = \dim X$) over the p-ring A with residue field k, one needs to find an abelian scheme V/B such that, reducing B modulo the maximal ideal pA in A *and* the square of the maximal ideal of B (so that what remains is the ring $k + V$, where V is a vector space of dimension n^2 over k whose square is zero in the ring in question), the element of $H^1(X, \mathcal{G}_{X/k}) \otimes V$ which expresses the class of the reduced scheme of V (cf. my Bourbaki lecture) defines an *isomorphism* from V' to $H^1(X, \mathcal{G}_{X/k})$. If this can be proved via explicit constructions (using hyperalgebras) it would probably be possible to prove at the same time that there exists an abelian scheme dual to an abelian scheme (without the polarisability condition) which should itself be *abelian*.

I have also just received your letter containing your result on eigenvalues. J.-P. Serre : This is a reference to a letter to Weil, published in 1960 in Ann. Math., cf. [**Se60a**]. It looks very good, of course, but I will probably not meditate much on it for the moment! Regards, A. Grothendieck

November 13, 1959 Jean-Pierre Serre

Dear Grothendieck,

Thank you for your letter, but you are laboring under an illusion if you think it is easy to give an example of a variety "without moduli" which cannot be defined over **Q**. The varieties one constructs are usually members of "families", and of course these families are always (at least in all the cases I know) defined over **Q**. How can one define *one* variety by itself? I really don't know, and rather tend to think that Weil's conjecture is true.J.-P. Serre : "Is true . . . ". In fact, it is false, cf. note 31.1. (I do not think, however, that Weil ever made a "conjecture" out of it). Be that as it may, I will look at it a little more to see if I can construct an example, but I don't have much hope.

We are advancing through the Multiplodocus. I have given two talks on $\mathrm{Proj}(S)$ and projective morphisms. As you have not yet sent this chapter to the printer, it may be worthwhile sending you criticisms of some detailsJ.-P. Serre : Once again, most of these corrections were incorporated into the EGA's by Grothendieck:

1) In everything concerning morphisms, one is somewhat bothered by Noetherian conditions which seem to be unnecessary. I have checked that some of them really are unnecessary: for others, I am morally certain of it but would have been obliged to reconstruct tiresome proofs. Here are the main examples:

(a) p. II-97, th 3: the Noetherian condition is not needed for (v) and (vi). Indeed, in general, let $\mathcal{C}$ be a category of morphisms satisfying the following three axioms: (I) Every identity morphism is $\in \mathcal{C}$, (II) If $X \longrightarrow Y$ is a closed immersion and if $Y \longrightarrow Z$ is $\in \mathcal{C}$, then $X \longrightarrow Z$ is $\in \mathcal{C}$, (III) If $X \longrightarrow Y$ is $\in \mathcal{C}$ and $Y' \longrightarrow Y$ is arbitrary, then $X \times_Y Y' \longrightarrow Y'$ is $\in \mathcal{C}$. Then $\mathcal{C}$ satisfies your properties (v) and (vi); in other words (I am copying (v)): if $X \longrightarrow Y \longrightarrow Z$ is $\in \mathcal{C}$ and $Y \longrightarrow Z$ is separated then $X \longrightarrow Y$ is $\in \mathcal{C}$ (proof: factor $X \longrightarrow X \times_Z Y \longrightarrow Y$). The same holds for (vi). It is trivial that projective morphisms satisfy (I), (II) and (III) (although (II) is not stated in the Multiplodocus, it obviously follows from the definition!).

The situation for quasi-projective morphisms is even better; they satisfy axiom (II'): If $X \longrightarrow Y$ is an embedding, and $Y \longrightarrow Z$ is $\in \mathcal{C}$,then $X \longrightarrow Z$ is $\in \mathcal{C}$. The argument given above then shows they satisfy (v'): If $X \longrightarrow Y \longrightarrow Z$ is $\in \mathcal{C}$, then $X \longrightarrow Y$ is $\in \mathcal{C}$. In other words, in your prop. 25, p. II-100, you can remove all the conditions on g in (v) and your condition on Y_{red} in (vi).

One might actually wonder whether it might not be worthwhile to have a short text on morphism classes (in general category theory, for example) in which it would be shown that certain properties imply others. For example, if the category is stable under composition, then the weakest axiom on products (i.e. my axiom (III)) implies the strongest one (the one on $f \times_S f'$). It is irritating to keep having to redo these arguments.

(b) p.II-98, th.4, I think (but would have to check) that the Noetherian condition is unnecessary. As this theorem is more or less analogous to the "extension of specializations" theorem, and the latter does not use any Noetherian conditions, this would be rather natural, and at the same time it is rather important. There are two parts to the proof of th. 4:

To start with, one may assume that Y is affine; this is not serious. Then assume that $X = \mathbf{P}(\mathcal{E})$ for $\mathcal{E}$ finitely generated, and reduce the problem to proving that $f(X)$ is closed; this is done by invoking property 21 (ii) in which the Noetherian hypothesis is used: I think (but have not checked in detail)

that it could be removed, provided of course that S is assumed to be generated by a finite number of elements of degree 1.

The second step consists of proving that $f(X)$ is closed. But $f(X)$ is the set of points where $\mathcal{E}_y/\mathfrak{m}_y\mathcal{E}_y \neq 0$; as $\mathcal{E}$ is finitely generated, Nakayama shows that this means $\mathcal{E}_y \neq 0$, and if $\mathfrak{a}$ is the annihilator of $\mathcal{E}$, this means that the ideal $\mathfrak{p}_y$ corresponding to y contains $\mathfrak{a}$: we thus get a closed set (the reference to FAC is thus unnecessary).

2) p.II-90, p.II-104 d).   You start with a surjective homomorphism $q^*(\mathcal{E}) \longrightarrow \mathcal{L}$ from which you want to deduce a morphism $r : X \longrightarrow \mathbf{P}$. You deduce from this an algebra morphism $\mathcal{S}(\varphi) : \text{mess} \longrightarrow \sum \mathcal{L}^n$, but it seems to me you do not exploit it enough: indeed, it is clear that $\text{Proj}(\sum \mathcal{L}^n)$ can be identified with X, and as $\text{Proj}(\text{mess}) = \mathbf{P}$, you get the required morphism r (upon noting that the algebra homomorphism is surjective). I cannot understand why you don't do it this way.

3) p.III-83 : in the proof of Cor.7, you say that $R(X)$ does not contain any element which is algebraic over $R(Y)$, but in fact, it is perfectly clear that $R(X) = R(Y)$.

I have not plunged into holomorphic function theory yet. I would like to simplify your proof, but I am stuck. Could you tell me if you can prove (or counterexample) the following:

Let A be a Noetherian ring, $S = A[T_0, \ldots, T_n]$, and let M be a graded finitely generated S-module. For every i, consider $H(T^i, M)$, in the sense given in Chap.III,§2 (I have not specified the degree of the cohomology, which does not matter); when i varies, the $H(T^i, M)$ form an inductive system, which becomes "constant" after a certain rank; let $i(M)$ be this rank. My question is the following: given an ideal $\mathfrak{a}$ in A, are the $i(M/\mathfrak{a}^k M)$ bounded? If so, there is an essentially trivial proof of the theorem of holomorphic functions (in the case of a projective morphism). Have you looked at it from this point of view?

Math. Reviews have given Weil the task of reviewingJ.-P. Serre : Weil's "review" can be found in *Math. Reviews* , vol. **21**, no4155. Kähler's 380 page memoir on "Geometrica aritmetica", i.e. Kähler's version of schemes. Apparently there is not much in it, apart from the following, which I did not know:

Let V be a non-singular projective variety, defined over a number field K; let $\Omega(V)$ be the K-vector space of differential forms of the first kind on V. There is then a canonical finitely generated A-submodule Ω_A in $\Omega(V)$ such that $\Omega_A \otimes_A K = \Omega(V)$! Its definition is as follows: a form $\omega \in \Omega(V)$ lies in Ω_A if for every discrete valuation ring U containing A with fraction field $K(V)$, ω can be written in the form $\sum x_i dy_i$ with $x_i, y_i \in U$. This is trivially canonical. What is not obvious is that $\Omega_A \otimes_A K$ is equal to $\Omega(V)$: in fact Kähler does not prove this in the general case, but I have checked it using a rather amusing method based on the existence of a normal projective scheme S over $\mathrm{Spec}(A)$ which gives V over K; if $\Omega_{S/A}$ denotes the differential forms of S relative to A, and if Ω'' denotes its bidual sheaf, it is easy to show that the Kähler differentials contain the sections of $\Omega_{S/A}$ (modulo torsion) and are contained in the sections of Ω'', which gives the result. If in addition the model is simple over $\mathrm{Spec}(A)$, one finds that the Kähler differentials coincide with the sections of $\Omega_{S/A}$, and thus the latter do not depend on the chosen model (this case is of course quite rare).

In particular, given an elliptic curve over $\mathbf{Q}$, it has a distinguished differential of the first kind, defined *up to sign*; Kähler determines it in one or two cases, and I can assure you that it is not easy.

Question: is it possible to define the Kähler differentials morphically as the inductive limit of sections of $\Omega_{S/A}$ when S runs over the ordered set of all the projective $\mathrm{Spec}(A)$-schemes which give V?

There isn't a lot of other news. I have told Borel about your refusal for next year — I did *not* tell him your comments on Harvard (on the other hand, Tate has seen them, since I gave him your letter).

Yours,

J-P. Serre

November 15, 1959 Jean-Pierre Serre

Dear Grothendieck,

First of all, a surprising piece of news: Dwork phoned Tate the evening of the day before yesterday to say he had proved the rationality of zeta functions (in the most general case: arbitrary singularities). He did not say how he did it (Karin J.-P. Serre : Karin = Karin Tate, née Artin. took the call, not Tate), but I had already seen a manuscript of his in which he proves a substantially weaker result: his method consists in assuming that the variety is a hypersurface in affine space (every variety being birationally isomorphic to such a hypersurface, so that an easy dévissage lets us to pass from this to the general case); in this case he does a computation using "Gauss sums" analogous to Weil's computation for an equation $\sum a_i x_i^{n_i} = b$. Of course, Weil himself had tried to extend his method, but with no success; Delsarte also worked on it; it is thus rather surprising that Dwork was able to do it. Let us wait for confirmation! J.-P. Serre : Shortly afterwards, Dwork came to IAS to explain his proof. I found it so interesting that I volunteered to be the referee for his text (published in 1960 in the Amer. J. Math), and I presented it in the Bourbaki seminar 1959/60, no198.

On your side of things, how is rationality for arbitrary singularities shaping up? Do you think you can get it from your stuff? Is there still a link with homology, and what is it?

Coming back to your projective morphisms and the elimination of the Noetherian conditions: I have now actually checked everything that I claimed in my letter of the day before yesterday. In other words, if $X \longrightarrow Y$ is a projective morphism (Y an arbitrary prescheme), there is always a quasi-coherent $\mathcal{O}_Y$-algebra $\mathcal{S}$ generated by a finitely generated $\mathcal{S}_1$, and such that $X = \mathrm{Proj}(\mathcal{S})$. Thus, it would be worthwhile to define projective morphisms this way!

The proof comes down to checking that part (ii) of Prop 21 in §3 (p.II-70) holds assuming only that S is generated by a finite number of elements of degree 1; this is indeed the case. More generally, under these conditions, if M is a graded S-module and F is a quasi-coherent subsheaf of $\widetilde{M}$, then the submodule N of M made of the guys that locally "belong" to F is such that $\widetilde{N} = F$; the proof is straightforward and very simple.

Once this is done, one feels much more comfortable. In part (ii) of th.3, p.II-97, you can replace "Z Noetherian" by "Z quasi-compact". In th.4,

p.II-98, you can remove all the conditions on Y. By the way, I had not noticed (Tate pointed it out to me yesterday) that the proof of the said th.4 is wrong; it can of course immediately be corrected: the aim is to show that if $Y = \mathrm{Spec}(A)$, and $X = \mathrm{Proj}(S)$, where S satisfies our usual conditions, then $f(X)$ is closed. Now, let $\mathfrak{a}_n$ be the annihilator of the A-module S_n: as $S_n \cdot S_1 = S_{n+1}$, we have $\mathfrak{a}_n \subset \mathfrak{a}_{n+1}$; let $\mathfrak{a}$ be the union of the $\mathfrak{a}_n$. Then, saying that $\mathfrak{p}$ contains $\mathfrak{a}$ is equivalent to saying that $(S_n)_\mathfrak{p} \neq 0$ for all n, which means precisely that the inverse image of $\mathfrak{p}$ in X is non-empty, so that $f(X)$ is closed (and corresponds to $\mathfrak{a}$).

It follows in particular that projective morphisms are "proper" in a stronger sense than the one of your §5: one may put any prescheme Y' as a direct factor. As I have not gone into the theory of properness in detail, I do not know which definition is right (this should be made clear by "Chow's lemma"). Nevertheless, th.2, p.II-116, for example, is surely true if A is any valuation ring: I cannot believe that there is anything Noetherian behind it.

By the way, do you intend ever to give the criterion for properness via valuations, J.-P. Serre : "... to give the criterion for properness via valuations": Despite his dislike of valuations (cf. the letter of October 19, 1961), Grothendieck included this criterion in the EGA's: Chap. II, th. 7.3.8. which is actually very convenient: if $f : X \longrightarrow Y$ is a morphism (separated and of finite type, I suppose), X and Y are irreducible and reduced, and $f(X)$ is dense in X, so that the field of functions $R(X)$ on X can be identified with a subfield of $R(Y)$, then for f to be proper it is necessary and sufficient that every valuation ring in $R(Y)$ which dominates a local ring of X also dominates a local ring of Y. (Under Noetherian conditions, discrete valuation rings should be enough).

Yours,

J-P. Serre

August 9, 1960 Alexandre Grothendieck

My dear Serre,

I have spent the last month thinking (quite outside of my planned program). about generalized Jacobians, local symbols, adeles, and duality theorems. I am beginning to understand a little, largely heuristically for the moment, and have remarked that my duality theory for coherent sheaves will be a wonderful guide to constructing a general duality theory encompassing it together with duality theory for algebraic groups or group schemes and duality of Weil cohomology. This has led me to similarly expand my planned program, since such questions now seem much more amenable to attack than previously. Once this theory has been developed, I hope the Weil conjectures will come out all by themselves. I will work on this next year at the same time as I put the finishing touches to the theory of torsors and cotorsors.

Today I would like to make a few comments on your Rosenlicht-Lang course. J.-P. Serre : "Your Rosenlicht-Lang course.". This is a reference to the first course I gave at the Collège de France (1956/1957), later published under the title "Groupes Algébriques et Corps de Classes" (Hermann, Paris, 1959). In my opinion, there is not enough emphasis on principal homogeneous spaces P under algebraic groups (over an arbitrary field k or even an arbitrary base S). Giving such a P is equivalent to giving a group scheme P^*, augmented towards the group scheme $\mathbf{Z}_k$, where P is the inverse image P^1 of the section 1 in $\mathbf{Z}_k$. Homomorphisms of principal homogeneous spaces (with varying groups) correspond to homomorphisms of augmented group (schemes) etc. Given a k-scheme X, the functor which associates $\mathrm{Hom}_k(X, P)$ to all P is left exact, and hence (since the P are Artinian objects) it is prorepresentable, whence one obtains a generalized Jacobian (an extension of $\mathbf{Z}_k$ by an algebraic progroup) which represents this functor. Starting from an extension K/k, i.e. setting $X = \mathrm{Spec}(K)$, one similarly obtains a generalized Jacobian $\mathrm{J}^*_{K/k}$ which represents the functor $P \mapsto P(K)$, which is also (if K is finitely generated over k) the projective limit of the generalized Jacobians $\mathrm{J}^*_{X/k}$ for models X of K. Of course, there is no reason for excluding the nilpotent elements of the local rings (of algebraic groups, for example). The definition and existence of Jacobians is therefore trivial; on the other hand, even the behavior of $\mathrm{J}^*_{X/k}$ for an extension k' of the base k is a serious problem, even in the base-field case. When k' is finite over k (and consequently also when it is an algebraic extension, X being assumed of finite type over k), it is easy to see that base change works well, i.e. $\mathrm{J}^*_{X'/k'}$ can be identified with $\mathrm{J}^*_{X/k} \otimes_k k'$. When X is a

non-complete algebraic curve, one probably needs Rosenlicht's theory to get the same result for an arbitrary base change; hopefully, it will be possible to pass from there to the case where X is of finite type over k (and otherwise arbitrary).

Consider an algebraic group G, and the augmented group extensions of J^* by G. In the absence of a thorough analysis of groups over k not of finite type, such as J^*, nobody seems to have noticed that it is extensions of such groups (and not of J^0) which give the right intrinsic functorial object for the application of generalized Jacobians to class field theory. (From the "classical" point of view, such an extension can be interpreted as being given by the following data: an extension E^0 of J^0 by G, a principal homogeneous space E^1 under E_0, and an isomorphism between $E^1 \times_{E^0} J^0$ — the principal homogeneous space associated to E^1 via $E^0 \longrightarrow J^0$ — and J^1). When $J^* = J^*_{X/k}$ (where X is any k-scheme, for instance $X = \mathrm{Spec}(K)$), the canonical morphism $X \longrightarrow J^1$ defines by pullback a canonical functor from the category of extensions of J^* (by commutative algebraic groups) to the category of principal bundles over X with varying commutative algebraic groups. It follows easily from the definition of the generalized Jacobian that this functor is *fully faithful*; in particular, if a principal bundle on X comes from an extension E of J, then the latter is determined up to unique isomorphism. As to whether or not such an E exists (for a given structural group, for example), uniqueness makes it possible to apply descent theory and observe that the problem is in fact geometric (i.e. one can pass to the algebraic closure of k). It follows for example that if $\mathrm{Ext}^1(J^*, \mathbf{G}_a) \longrightarrow H^1(X, \mathcal{O}_X)$ is surjective (which is the case if X is affine but in many other cases as well), then for any *unipotent* algebraic group G defined over k, the map $\mathrm{Ext}^1(J^*, G) \longrightarrow H^1(X, G)$ is bijective. If in addition the map $\mathrm{Ext}^1(J^*, \mathbf{G}_m) \longrightarrow H^1(X, \mathbf{G}_m)$ is surjective (after passing to the algebraic closure of k if necessary) then the map $\mathrm{Ext}^1(J^*, G) \longrightarrow H^1(X, G)$ is bijective for any *affine* commutative algebraic group (prove this result first for connected groups by dévissage, then deduce the finite case by embedding G in a connected group, and finally get the general case). In particular, taking G to be an ordinary finite group, one finds that the unramified coverings of X are classified by separable isogenies of the Jacobian. In particular, this always works when $X = \mathrm{Spec}(K)$, since $H^1(K, \mathbf{G}_a) = H^1(K, \mathbf{G}_m) = 0$, or for a non-singular affine algebraic curve, and it *partially* works for a non-singular complete algebraic curve (here, one does not get the whole of $H^1(C, \mathbf{G}_m)$ by extensions of the Jacobian, but for any G it is possible to specify exactly which

part of $H^1(C, G)$ comes from extensions of the Jacobian). I have restricted myself to affine groups, but the case where there is an abelian component can be reduced to this case if X is non-singular, since, as you know, there always exists an *affine* subgroup G' of G such that a given element of $H^1(X, G)$ comes from $H^1(X, G')$.

Of course, $\mathrm{Ext}^1(\mathrm{J}^*, G)$ should be compared with the $\mathrm{Ext}^1(\mathrm{J}^0, G)$ which are usually studied, thanks to the cohomology exact sequence derived from $0 \longrightarrow \mathrm{J}^0 \longrightarrow \mathrm{J}^* \longrightarrow \mathbf{Z} \longrightarrow 0$:

$$0 \longrightarrow G(k) \longrightarrow \mathrm{Hom}(\mathrm{J}^*, G) \longrightarrow \mathrm{Hom}(\mathrm{J}^0, G)$$

$$\mathrm{Ext}^1(\mathbf{Z}, G) \longrightarrow \mathrm{Ext}^1(\mathrm{J}^*, G) \longrightarrow \mathrm{Ext}^1(\mathrm{J}^0, G)$$

$$\mathrm{Ext}^2(\mathbf{Z}, G),$$

taking into account the fact that

$$\mathrm{Ext}^i(\mathbf{Z}, G) = H^i(k, G)$$

which in particular is trivial if $i > 0$ and k is algebraically closed (in this case $\mathrm{Ext}^1(\mathrm{J}^*, G) = \mathrm{Ext}^1(\mathrm{J}^0, G)$), so that in this case one may identify J^* and J^0. When J^1 is trivial (i.e. has a rational point over k), the exact sequence above breaks up into exact sequences

$$0 \longrightarrow \mathrm{Ext}^i(\mathbf{Z}, G) \longrightarrow \mathrm{Ext}^i(\mathrm{J}^*, G) \longrightarrow \mathrm{Ext}^i(\mathrm{J}^0, G) \longrightarrow 0.$$

This is the case, in particular, if J^0 is connected (for example if X is connected — by which I mean relatively connected) and k finite, by Lang's theorem. One recovers Lang's class field theory by defining a canonical isomorphism

$$\mathrm{Ext}^1(\mathrm{J}^*, G) \cong \mathrm{Hom}(\mathrm{J}^*(k), G)$$

for a finite field k and an ordinary finite group G in the following way: by Yoneda, an element of the left-hand side gives a homomorphism

$$\mathrm{J}^*(k) = H^0(k, \mathrm{J}^*) \longrightarrow H^1(k, G);$$

however, since the Galois group of $\overline{k}$ over k is the completion of $\mathbf{Z}$ and acts trivially on G, one has

$$H^1(k, G) = \mathrm{Hom}(\mathbf{Z}, G(\overline{k})) = G,$$

giving the homomorphism above, which is easily checked to be an isomorphism.

It seems to me that the most comprehensible way of presenting Rosenlicht's theory is to start with the Picard $P^*_{C/k}$ of complete singular curves C (whose structure is determined quite easily once the *existence* of Picard is established), and consider the natural morphism $C' \longrightarrow P^1_{C/k}$ which maps the non-singular part C' of C to the Picard group, induced by the diagonal divisor in $C \otimes_k C'$. Rosenlicht's theorem can then be formulated as saying that for fixed C', taking the projective system of Picard groups of C_i with more and more singularities yields a generalized Jacobian for C'. A bunch of short computations have convinced me that the theorem in this form remains valid for curve schemes over a Noetherian base S. J.-P. Serre : "the theorem in this form remains valid for curve schemes". See:

C. Contou-Carrère, *Jacobiennes généralisées globales relatives*, in *The Grothendieck Festschrift*, vol.II, Birkhäuser-Boston 1990, 70–109. I have worked out a precise statement which seems reasonable, and I even have an idea of a proof. But I will of course have to start by proving the existence of Picard, which I intend to do during the holidays, with the tools I have available.

The proof I have in mind is linked to a direct definition of local symbols, obtained as follows. For simplicity, let us work over a field k; let A be a regular complete local ring of dimension 1 which is an algebra over k with residue field k (hence isomorphic to $k[[t]]$). It is then natural to define a proalgebraic group $\mathrm{J}^*_{A/k}$ augmented towards $\mathbf{Z}$, whose k-rational points are the elements of K^* where K is the fraction field of A. For any algebra k' over k, $\mathrm{J}^*_{A/k}(k')$ is thus a group augmented towards $\mathbf{Z}$, which moreover can be identified (once a uniformizing parameter t of A is chosen) with the multiplicative group of series $\sum_i c_i t^i$, where the $c_i \in k'$ vanish for small i, and the first non-zero c_i is invertible. Set $k' = K$, and consider the formal series

$$\sum_{i \geq 1} (1/T^i) t^i,$$

where $T = t$ viewed as an element of the ring of coefficients K (and not as an indeterminate.) This gives an element of $\mathrm{J}^1(K)$; note that on changing the uniformizing parameter, this element gets multiplied by an element of $\mathrm{J}^0(A)^{(1)}$, where $\mathrm{J}^0(A)^{(1)}$ is the subgroup of $\mathrm{J}^0(A)$ which is the kernel of the specialization homomorphism $\mathrm{J}^0(A) \longrightarrow \mathrm{J}^0(k)$. This gives a canonical element

$$\xi_{A/k} \in \mathrm{J}^1(K)/\mathrm{J}^0(A)^{(1)}.$$

It can still be defined without the assumption that A is complete. This element has the following universal property: for any principal homogeneous space P^1 under a commutative algebraic group G over k, and any element of $P^1(K)/G(A)^{(1)}$, there is a unique homomorphism from $J^*_{A/k}$ to $P^*_{A/k}$ such that the given element is the image of ξ under this homomorphism. It would be good to find a direct proof of this result, which in any case follows by global arguments from Rosenlicht's theory.

Once this local result has been obtained, local class field theory can certainly be developed, for equal characteristic, just as in the global case; in particular, it is possible to define an abelian extension of K directly by inverse image, using a separable isogeny of $J^*_{A/k}$ (where this extension is defined modulo an abelian extension unramified over A, i.e. modulo nothing at all in the complete "geometric" case). As you yourself remarked in your course this year, it is easily checked that this map is the inverse of the one you defined (in the geometric case) which goes in the other direction; this gives a simple proof of the existence theorem in local class field theory, in equal characteristic. Naturally, as in the global case, there is no reason to restrict to abelian extensions; one should consider the classification of extensions of $J^*_{A/k}$ by arbitrary commutative algebraic groups, and principal homogeneous bundles defined over K. I have not gone into this in detail.

Unfortunately, my understanding of unequal characteristics has still not improved, except that I have a clearer idea of what I do not understand. It would be enormously helpful to have a direct description of the extension of K corresponding to an isogeny of the Jacobian, as well as an interpretation of the extensions of the Jacobian by algebraic groups which are not ordinary finite groups.

On the other hand, I have some very precise ideas about generalized Jacobians of local rings of any dimension, in equal characteristic. If A is such a local ring, which is regular and is also a k-algebra (whose residue field can have any transcendence degree over k), I can associate to it a proalgebraic group $J_{A/k}$ which deserves to be called a generalized Jacobian. If A is a field, this is the $J^*_{K/k}$ considered above, which represents the functor $G(K)$. If A is 1-dimensional, consider the functor $G(K)/G(A)$ (where K is the fraction field of A), i.e. the G-divisors of the local ring A; this is left exact in G and corresponds to a proalgebraic group $J_{A/k}$ (with the notation above, this would

be $J^0_{A/k}$ if the residue field were k). This is a relatively connected proalge-braic group, which no longer has an abelian part, but a multiplicative part isomorphic to $\mathbf{G}_m$. For $\dim A \geq 2$, one obtains relatively connected *unipotent* proalgebraic groups, which pro-represent local cohomology functors, namely $H^{n-1}(\mathrm{Spec}(A) - \mathrm{origin}, G)$; cohomology is taken to mean ordinary sheaf coho-mology, and one would obtain a uniform definition (independent of $\dim(A)$) of the functors defining the local Jacobians by using *relative* cohomology of $\mathrm{Spec}(A)$ modulo the complement of the origin; this relative cohomology vanishes for all G except in dimension n (an analogous local result proves the duality for topological varieties!) so the relative H^n with values in G is a left exact functor of G... If X is now a non-singular scheme over k, of finite type over k for simplicity, consider the pro-group J_i, product of the $J_{A/k}$ for places A of X of Krull dimension i; the J_i form a complex J_*:

$$0 \leftarrow J_0 \leftarrow J_1 \leftarrow J_2 \leftarrow \cdots \leftarrow J_n \leftarrow 0$$

whose 0-th homology group, for example, is the Jacobian $J^*_{X/k}$. For any algebraic group G, the cohomology of $\mathrm{Hom}(J_*, G)$ is canonically isomorphic to $H^*(X, \mathcal{O}_X(G))$, where the cohomology of X is taken in the usual sheaf-theoretic way. The J_i are actually projective relative to exact sequences of algebraic groups defining locally trivial bundles (i.e. for which the sequence of corresponding sheaves on X is exact). One may possibly find another, more satisfying, theory (with J_i which are truly projective) by working with some Weil cohomology.

There are some slightly more complicated results when X is not assumed to be regular; the complex J is replaced by a double complex (whose Hom into G is the first term of a spectral sequence which abuts at the sheaf-theoretic cohomology on X with values in G); this double complex can be derived from a certain simplicial ring, which deserves to be called the adelic simplicial ring of X, which provides a flasque resolution of $\mathcal{O}_X$ by sheaves of flat algebras.

I can provide the details on demand — it is probably not worthwhile to give the rigorous definition here.

I am coming back to Paris at the beginning of September; will you be there? Have you received chapter I of the hyperplodocus? J.-P. Serre : hyperplodocus = multiplodocus = EGA.

Yours,
A. Grothendieck

September 20, 1961 Jean-Pierre Serre

Dear Grothendieck,

Auslander has just found J.-P. Serre : Cf. M. Auslander, *On the purity of the branch locus*, Amer. J. Math. **84** (1962), 116–125. a "homological" proof of Nagata's purity theorem. Here is an outline of the idea of the proof:

Let A be regular and let B be a finite normal Galois extension of A, unramified outside the origin. Assume that $\dim A \geq 2$; the aim is to prove that B is free (which will imply that B is unramified).

[The general ("quasi-finite", non-Galois) case can trivially be reduced to this one without even having to pass to completions.]

Let G be the Galois group and n its order. Associate to $s \in G$ and $b \in B$ the endomorphism $x \mapsto b \cdot s(x)$ of B. In this way, $B \times \cdots \times B$ (n times) maps to $\operatorname{Hom}_A(B, B)$; as these two modules are reflexive and the map is a localized isomorphism in codimension 1, it is an isomorphism. It follows that $\operatorname{Hom}_A(B, B) = B^n$. From this point on, it is a problem about modules; indeed, Auslander shows that:

THEOREM: *Let A be regular and let M be a reflexive A-module which is free outside of the origin and such that $\mathrm{Hom}(M, M)$ is isomorphic to M^n. Then M is free.*

This is trivial in dimension 2. In dimension 3, he gives a very ingenious proof, which I am too lazy to reproduce here (but I have checked it); the dimension $n \geq 4$ case can be reduced fairly easily (yes indeed!) to the dimension $n - 1$ case.

(Note that the theorem above, applied to a rank 1 module, gives an alternative proof that A is factorial.) Yours,

J-P. Serre

P-S. I will be taking the boat for New York the day after tomorrow.

October 1, 1961 ALEXANDRE GROTHENDIECK

My dear Serre,

Thank you for your letter. Auslander's proof is indeed ingenious, but I do not find it that exciting any more, since I can now prove the purity theorem and various generalizations in a much more natural way.J.-P. Serre : For the proof of the *purity theorem*, see SGA 2, exposé X, th. 3.4. In fact, I have translated all the stuff on formal completions along a hyperplane section into local algebra, and have obtained its exact analog with respect to the relation between a local ring A and a quotient ring A/fA. (Actually, I have not yet written up the main theorem in detail, but I have no doubt at all that it works.) To tell the truth, it seems that the local theorems one obtains this way are always stronger than the global theorems which were known before, in the sense that the projective statements can be deduced from the local algebra statements by standard arguments. Here are two products derived from the general stuff:

A denotes a complete Noetherian local ring, f an element of its radical, assumed for simplicity not to be a zero divisor, $X = \operatorname{Spec}(A)$, $Y = V(f) = \operatorname{Spec}(A/fA)$, x is the closed point of X, $X' = X - x$, $Y' = Y - y$. Then

1) If X is of depth ≥ 2 at the closed points of X' (for instance if A is of depth ≥ 3), then $\pi_1(Y') \longrightarrow \pi_1(X')$ is surjective ("Bertini's theorem" which implies the "purity theorem" for a local regular ring starting from the fact that it is known in dimension 2). If X is of depth ≥ 3 at the closed points of X' (for instance if A is of depth ≥ 4), then every etale covering of Y' can be extended to a covering of X' which is etale outside of a finite subset of $X' - Y'$; thus under the additional assumption that the local rings of the $x' \in X' - Y'$ satisfy the purity theorem, $\pi_1(Y') \longrightarrow \pi_1(X')$ is actually bijective.

N.B. I say that a local Noetherian ring B satisfies the purity theorem if, on setting $Z = \operatorname{Spec}(B)$ and $Z' = Z - z$, where z is the closed point, the restriction functor induces an equivalence of categories between the category of etale coverings of Z and the category of etale coverings of Z'. Thus, using the second part of 1) and the purity theorem, one easily obtains: *If B is a complete intersection of* $\dim \geq 3$, *then B satisfies the purity theorem* (B not necessarily complete).

2) If depth $A \geq 4$, i.e. depth $A/fA \geq 3$, then $\operatorname{Pic}(X') \longrightarrow \operatorname{Pic}(Y')$ is injective; if depth $A \geq 5$, i.e. depth $A/fA \geq 4$, then any invertible Module over Y' can be extended to a Module over X' which is invertible except at a finite subset

of $X' - Y'$; consequently, if in addition the local rings of $x' \in X' - Y'$ are quasi-factorial, then $\text{Pic}(X') \longrightarrow \text{Pic}(Y')$ is bijective.

N.B. A local ring B is said to be quasi-factorial if $\text{Pic}(Z') = 0$. Thus, using the second part of 2) and the unique factorization theorem for regular local rings, one easily obtains: *If B is a complete intersection of* $\dim \geq 4$ *(B not necessarily complete), then B is quasi-factorial.* It follows that if B is a complete intersection which is factorial in codimension ≤ 3 (for instance if B is regular in codim ≤ 3), then B is a factorial ring: this is Samuel's conjecture.

These local results immediately yield global results of the following kind: let X be a locally Noetherian prescheme and Y a closed subset of X, and assume that X is a complete intersection at all points of Y. If Y is of codim ≥ 3 (resp. of codim ≥ 4), then the restriction functor from etale coverings of X (resp. invertible Modules on X) to etale coverings (resp. invertible Modules) on $X - Y = U$ is an equivalence of categories, and in particular there are isomorphisms $\pi_1(U) \cong \pi_1(X)$ (resp. $\mathrm{Pic}(X) \longrightarrow \mathrm{Pic}(U)$). On this topic, let me mention an analogous result one degree lower, which was just proved by a student here, Hartshorne: J.-P. Serre : Cf. R. Hartshorne, *Complete intersections and connectedness*, Amer. J. Math. **84** (1962), 497–508. if Y is of codim ≥ 2, then $\pi_0(U) \longrightarrow \pi_0(X)$ is bijective. (Even better, it is enough for the local rings of X at the points of Y to be of depth ≥ 2. This proves that if X satisfies your condition S_2 and is connected, then it is in fact "connected in codim 1", i.e. two irreducible components of X can be joined by a chain of components of which any two consecutive members meet in codimension 1. I do not believe that this was known even for complete intersections in projective space. The proof of Hartshorne's theorem is actually practically trivial using the relative cohomology sorites and writing the desired relation in the stronger form that $H^0(X, \mathcal{O}_X) \longrightarrow H^0(Y, \mathcal{O}_Y)$ is bijective.)

One obvious moral of this story is that it is necessary to make a systematic study of the topology of local schemes, and more precisely of differences of local schemes, especially in the "geometric" case, i.e. with respect to a complete local ring whose residue field is algebraically closed. For example, I am convinced that if A is a complete local ring with algebraically closed residue field, such that the components of $X = \mathrm{Spec}(A)$ are all of dim ≥ 2, then $\pi_1(X')$ is topologically finitely generated. J.-P. Serre : "$\pi_1(X')$ is topologically finitely generated". This statement was given as a conjecture in SGA 2, exposé XIII, p. 181. I do not know whether any progress has been made on it since. In fact, Lefschetz ideas and descent techniques can be used to reduce the problem to the case where X is normal and 2-dimensional, i.e. to the case studied by Mumford in the analytic complex case, where desingularization gives some fairly precise ideas as to the probable shape of the fundamental group. Actually, in the case of equal characteristic, I have just proved a conjecture from Mumford's paper J.-P. Serre : "Mumford's paper". Reference:

D. Mumford, *Topology of normal singularities and a criterion for simplicity*, Publ. Math. IHES **9** (1960), 1–22. which says that $\mathrm{Pic}(X')$ should be the set of rational points of a group scheme of finite type over the residue field.

Indeed, if X_1 is a regular projective scheme over X and E_1 denotes the fiber over the closed point x, equipped with an induced structure defined by a sufficiently small ideal over X_1, i.e. E_1 is sufficiently large, then it suffices to take the Picard scheme of E_1/k and to divide it by the discrete subgroup induced by the invertible Modules over X_1 defined by irreducible components of E_1 (considered as prime divisors on X_1) — a group whose rank is exactly the Néron-Severi rank of E, so what is left is an extension of a *finite* group (the $H^2(X', \mathbf{Z})$ of Mumford's article) by the connected Picard of E_1/k. It is quite amusing to note that this algebraic structure actually depends on the way in which the residue field k of A has been lifted; however, the very fact of its existence gives finiteness information on the "Kummerian" part of $\pi_1(X')$. It is not out of the question that a suitably adapted version of the technique of specialization of the fundamental group might make it possible to prove that $\pi_1(X')$ is topologically finitely generated.

These results suggest, in topology as well, the existence of "local" versions of the Lefschetz theorem: Let X be a complex analytic space, Y an analytic subset of X, and assume that X is locally a complete intersection at the points of Y, and codim $Y \geq r$. Show that for the homology, homotopy, cohomology groups etc. of X and $U = X - Y$. there are isomorphisms in dimension $\geq r - 2$ and epimorphisms resp. monomorphisms (according to the situation) in the critical dimension $r - 1$.J.-P. Serre : See lectures XII (Grothendieck) and XIV (Michèle Raynaud) from SGA 2. This can be expressed, at least for cohomology, by the vanishing of relative local cohomology groups defined by the local embedding of Y into X, and analogous invariants can probably be defined for homotopy. Likewise, the global Lefschetz theorems for projective varieties X should remain true when replacing the non-singularity condition on X by much weaker local conditions, which are satisfied for instance whenever X is locally a complete intersection.

The mathematical atmosphere at Harvard is absolutely terrific, a real breath of fresh air compared with Paris which is gloomier every year. There is a good number of intelligent students here, who are beginning to be familiar with the language of schemes, and ask for nothing more than to work on interesting problems, of which there is obviously no lack. I am even selling (the little I know of) Weil sheaves and Weil cohomology with the greatest of ease, including to Tate, who has just seriously started work on the "global" analytic structures which have been bothering him for two years, and which appear to be easiest to express in terms of "Weil ringed spaces". On this topic,

it seems more and more obvious to me that the concept of a formal scheme will need to be completely reworked, together with that of an analytic space (or a "rigid-analytic" space as Tate and I call his "global" structures), in order to unite them into a single framework, which has yet to be found. Meanwhile, Mike Artin is getting excited about global degeneracy phenomena for elliptic curves (linked to the work of Kodaira, Néron and Ogg), which he wants to understand in terms of Weil cohomology. In principle, he will start a seminar with Hironaka on the subject in the near future. In addition, there is a course by Zariski on desingularization of surfaces (which I am following) and a kind of course by Kodaira, who will prove the grand theorem on the characterization of ruled surfaces by $P_{12} = 0$, starting next week. What with my course, and my seminar, it will almost be too much!

I am pushing ahead with the writing of chapter IV, which I hope to finish by the end of the year. (Chap.III, on the other hand, has temporarily stopped progress, since I intend to add a load of local stuff of the kind I have sketched in this letter). We are going to put a paragraph into Chap 0_{IV} in which the right theorems for the "good rings" will finally appear all together; they were turning into a little nightmare for me. I have the impression that in the end it will be easy [...]. One of the recent "incentives" for writing up something for Chap 0_{IV} came from Tate, who needed the finiteness theorem for the normalization, for a quotient ring of a ring of restricted formal series over a complete discrete valuation ring or its fraction field. He has worked out the following theorem on this subject, which generalizes Nagata's theorem (for a complete local ring) with a very elegant direct proof: let A be Noetherian and normal, $x \in A$; assume that A is separated and complete for the x-adic topology, and that A/xA is a domain whose normalization in any finite extension of its fraction field is finite over A/xA. Then A has the same property.

Would you like to come and pay a visit over here in the autumn?
Cordially,
A Grothendieck

October 19, 1961 ALEXANDRE GROTHENDIECK

Dear Serre,

Thank you for your letter. The theory of which I gave you several subproducts asserts that under certain conditions, the study of coherent sheaves over certain (projective, or local) schemes is equivalent to the study of coherent sheaves on the formal completions along "hyperplane sections" ... It is probably useless to give you the results in detail; I have already repeated them n times here, there and everywhere, and they will be carefully developed in my Paris seminar. What I am saying will be enough to convince you that you have played a mean trick on poor Schwarzenberger by giving him non-existent results. J.-P. Serre : I had given Schwarzenberger some false statements, claiming that Grothendieck had proved them. Maybe you could let him know this yourself, which would save me the trouble of writing to him. My program is a little upset at the moment, as I just spent a few days in California (where besides Berkeley, there is also my sister. . .).

As promised, I have indeed worked out the theory of good rings, local or otherwise, J.-P. Serre : The theory of "good rings" can be found in EGA 0.13 and EGA IV.7. and will write them up shortly, as much for my course as for Chap. IV. I suggest that Bourbaki use them as inspiration for a nice Chapter on Commutative Algebra! I have read Bourbaki's general comments on Commutative Algebra in the last Tribu. Here are my personal impressions, suggested in part by my recent reflections on Chap. III and IV of the Elements. As far as I can remember, Chaps. I to V of Commutative Algebra are satisfactory and well-adapted to users. On the other hand, Chaps. VI and VII appear to me to be unworthy of Bourbaki. I have proposed several times in vain that VI (Valuations) should be purely and simply thrown out; although since then I have come to understand why resolution of singularities is useful, I nevertheless remain of the opinion that VI should be removed, or at the very least moved from its current position to the end of the book, among the things "not to be read". Its current central position will mislead the reader as to the right ideas and methods. Even concerning the resolution of singularities, which to be perfectly honest is not yet finished even in dimension 2 (because of unequal characteristics), I really have the impression that the whole question will have to be taken up again with an approach entirely different from Zariski's, and it would be premature to predict whether and in what form valuations will be needed; I suspect not at all. In these circumstances, the importance given to valuations in the current plan, for the sole reason that this concept was

central to Krull and Zariski (who have also done better things) is anti-Bourbaki.
Equally, Chap VII is essentially copied from Krull, and appears to me to be
currently far removed both from geometric intuition (which is a good guide)
and from actual practice — starting with the very definition of a Krull ring
via the sempiternal valuation families, and the misleading symmetry between
Dedekind and factorial rings, which have been proudly thrust beneath the
Krullish hat. I confess I did not manage to understand this Chapter throughout
its N successive drafts, except for an ancient draft on factorial rings dating
from before it started getting so complicated. It is a fact that one can only
understand *properly* if a geometric language is available, including non-affine
schemes. A striking example of this fact is your proof of the fact that regu-
lar $\implies$ factorial (which I have written up in detail); J.-P. Serre : "... Your
proof of the fact that regular $\implies$ factorial". In fact, this is a reference to a
proof of Auslander and Buchsbaum's (P.N.A.S. **45** (1959), 733–734), which I
had explained to Grothendieck in a slightly different form, cf. Bourbaki A.C.
VII.68 and A.C. X.53. such proofs ought to be written up by Bourbaki. On
the same subject, such properties as "factorialness" also exist for non-normal,
and even non-reduced, local rings, as you know ("complete intersections", for
example). Even if Bourbaki does not launch into such things, it would be nice
if he at least had the right "yoga". At the moment, Chap. VII reeks of dusty
academics. As for your paper on modules over normal rings, I am critical of the
fact that the "lattice" point of view sometimes obscures the more important
"modules" point of view. It seems to me that in fact most of the properties you
give follow from Serre's S_2 propertyJ.-P. Serre : "Serre's S_2 property". This
is the property: "$\mathrm{depth}(A_{\mathfrak{p}}) \geq \inf(2, \dim A_{\mathfrak{p}})$ for every prime ideal $\mathfrak{p}$ of A", cf
EGA IV.5.7.

One day, Grothendieck asked me "What does it mean for a ring to be
normal?" I pointed out to him the criterion "$R_1 + S_2$", cf. EGA IV, th. 5.8.6. I
had made up this criterion (which for integral rings is due to Krull) in order to
understand Seidenberg's theorem which says that a general hyperplane section
of a normal variety is normal. (which holds for reflexive modules over a ring
satisfying S_2 and many others besides). I encounter this property frequently;
it is equivalent to the following, whose geometric significance is clear: For any
open subset U in $X = \mathrm{Spec}(A)$, and for every closed subset Y in U, consider
the homomorphism $\Gamma(U, F) \xrightarrow{u} \Gamma(U - Y, F)$, where F is the sheaf associated
to the module M; assume for simplicity that $\mathrm{supp}\, F = X$. Then u is injective
if Y is of codimension ≥ 1, and bijective if Y is of codimension ≥ 2. (Here it
is enough to have U run over a basis of open sets.)

Nothing actually compels us to put normal rings before the two Chapters on dimension, depth, Cohen-Macaulay and regular. Those chapters are essentially complete, and do not depend in any way on the current Chapter VII (and even less on Chapter VI!). Note that the most useful characterization of normal rings, apart from their definition as Noetherian + integrally closed, is your characterization by $S_2 + R_1$, which relies on the two Chapters that I just mentioned. In fact, I did not really understand what normal rings or Dedekind rings were, until after understanding dimension and depth of modules. Why should Bourbaki suddenly require himself to follow the historical route?

As for Cohen's theorems, they come out quite nicely without using Witt vectors. Here, again, I was uncomfortable to see such conceptually simple theorems emerging from extremely off-putting formulas on Witt vectors. All this will be covered in detail in Chapter 0_{IV} of the "Elements", which will also absorb the idea (from my IHES seminar) of an algebra which is "formally simple" over another. Naturally, nothing stops us after that from defining Witt vectors over a ring, and noticing with pleasure that when the latter is a perfect field, one gets a "Cohen ring" whose residue field is the field in question. But it still strikes me as more reasonable to put it in with the algebraic groups, or the Greenberg stuff — that is to say, where it is certain to be useful.

I have no definite opinion on non-ramification and differential calculus. Here, again, one understands everything so much better in geometric terms!

On the subject of the Bourbaki Seminars 61-62: my second talk "on Grothendieck" will in fact be on modules, i.e. on Mumford. In addition, there are some extremely interesting results by Hochschild-Kostant-Mostow, which state that for any homogeneous affine space under an affine algebraic group over $\mathbf{C}$, the "algebraic" de Rham cohomology is isomorphic to the classical de Rham cohomology — and various other related results. Kostant goes as far as conjecturing that the same result holds for any non-singular affine variety over $\mathbf{C}$, and it actually is the case for a non-singular affine curve! J.-P. Serre : The result conjectured by Kostant was proved a little later by Grothendieck: *On the de Rham cohomology of algebraic varieties*, Publ. Math. IHES **29** (1966), 95–103 (extract from a letter to M.F. Atiyah dated 10/14/1963). — I do not think it is very smart to let Néron talk about himself: we will be no better off afterwards than we were before. Couldn't we try to find someone courageous enough to try to understand what Néron is doing? Maybe we could have a series of talks by Cartier or someone else, on Néron-Kodaira-Ogg-Tate, since

all this is linked, and should be understood together. Here is another potential lecture topic: Tate's rigid-analytic spaces (unless you are including them in your course at the Collège). Maybe this will incite some Normalien[6] (I am thinking of Houzel) to work on the subject; there would be good reasons for rewriting at least a large part of the theory of complex analytic spaces (Stein spaces, Grauert's finiteness theorem, Remmert-Grauert GAGA, maybe also Rothstein-type theorems...) in this context. I am convinced that sooner or later it will be necessary to subsume ordinary analytic spaces, rigid analytic spaces, formal schemes and maybe even schemes themselves into a single kind of structure, for which all these usual theorems will hold: there is certainly some fun to be had doing it.

I have also read the draft on the fundamental group and covering spaces. J.-P. Serre : "Fundamental group and covering spaces". This Chapter of Bourbaki's *General Topology* has now been a work in progress for more than forty years. It is rather nicely written. The weak point seems to me to be §5 on covering spaces, where Cartan has to deal separately with coverings of simplicial spaces, groupoids and topological spaces, despite which the sorites do not apply to covering of schemes, for example. There is a way to unify all this — I even believe it can be done rather simply — using the concept of a Weil space (which comes up whenever one has any kind of "localization"). In this context, the fundamental group via coverings can be defined as a progroup, which is a group if and only if a universal covering exists. This holds for all the spaces considered by Cartan, but not in the case of general preschemes. Some time, I will have to write up the basic sorites on Weil spaces in the style of "results"; Bourbaki will probably back away in horror, but they could be useful to less inhibited people! Another suggestion for Bourbaki: try to include a generalized van Kampen theorem like SGA IX 5.1 and its corollaries, notably 5.8, which among other things yields the fact that the fundamental group of a symmetric power of X is the abelianization of the fundamental group of X. Of course, such descent results are basically sorites (although they are not yet well known) and can probably be easily expressed in the general context of Weil spaces which I recommend. Even if Bourbaki does not adopt this context, he could make an effort to obtain a van Kampen theorem (which is in fact a descent theorem) which is as general as possible in the usual topological setting. Another criticism: the emphasis on *connected* coverings is excessive; it

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[6]Normalien = student at the Ecole Normale Supérieure in Paris.

must be said explicitly that arbitrary coverings correspond to sets on which the fundamental group acts.

Tate has just given me the manuscript of Nagata's book on local rings. It is vintage Nagata — however, I spotted a few interesting results, notably the following, which answers a question Zariski asked me recently and which I did not know how to answer: Let A be a local ring, and $\mathfrak{p}$ a prime ideal of A such that $\dim A/\mathfrak{p} + \dim A_\mathfrak{p} = \dim A$, and the completion of $A/\mathfrak{p}$ does not have nilpotent elements (clearly harmless conditions). Then $\operatorname{mult} A_\mathfrak{p} \leq \operatorname{mult} A$. Applying this to a quotient ring B/xB, where B is regular, it follows that for any prime ideal $\mathfrak{q}$ of B, one has $\mathfrak{q}^{(n)} \subset \mathfrak{m}^n$, where $\mathfrak{m}$ is the maximal ideal of B. (N.B. On localizing, it also follows that $\mathfrak{q} \subset \mathfrak{r}$ implies that $\mathfrak{q}^{(n)} \subset \mathfrak{r}^{(n)}$). This was Zariski's question. — I thought some more about local intersections, but with no success: I understand that I don't understand, which is at least something.

I have found a relatively quick proof of the theorem of resolution of singularities for a locally Noetherian 2-dimensional prescheme whose local rings are (let us say) formally normal *with residue field of characteristic 0*. J.-P. Serre : This is a reference to Jung's method for resolving the singularities of a surface. I had explained it to Grothendieck some years previously; he had forgotten it and then rediscovered it. The problem reduces to the case of a formally normal local 2-dimensional ring A; it is easy to see that A may be assumed to be complete (since a desingularization can always be obtained by blowing up an ideal which is primary for the maximal ideal); hence by Cohen, A is finite over a regular 2-dimensional B. Let $X = \operatorname{Spec}(A)$, $Y = \operatorname{Spec}(B)$; the claim is that A can be resolved by constructing a series of Y_i (projective over Y) one at a time by successive blow-ups of Y, and taking X_i to be the normalization of $X \times_Y Y_i$: in this procedure, Y_{i+1} is constructed as the blow-up of Y_i at all points over which there is a non-regular point of X_i. Here is the proof: let C be the ramification curve of X over Y, let C_i be its inverse image in Y_i, and use the fact (which is certainly easy, since Y is regular) that over the bad points of Y_i, C_i will either be non-singular or have ordinary double points for i large. The problem is then reduced to an exercise in Galois theory: let a regular 2-dimensional B be finite and normal over A, such that the ramification set is contained in the curve $xy = 0$ (x, y being a regular sytem of parameters); then the base Y can be desingularized by a finite number of blow-ups of Y. Obviously, this is where one makes essential use of the characteristic 0 condition, which allows one to say (using Abhyankar via purity) that X is a quotient of a

covering $B(x^{1/m}, y^{1/n})$, so that the situation is under control. But "for all one knows", it is not clear that the same straightforward procedure wouldn't work for a characteristic-p residue field, by making ingenious use of local class field theory. Do you have any ideas? I am now convinced that resolution of singularities is a good thing, for instance for (Mumford-style) topological studies of a singularity, and apparently also for (Néron- or Kodaira-type) minimal model questions for curves over the field of a valuation ring.

Yours,

A. Grothendieck

P.S. Could I have a copy of the new draft of Hensel?

October 22, 1961 ALEXANDRE GROTHENDIECK

My dear Cartan, J.-P. Serre : This letter from Grothendieck to Cartan is included here (with Cartan's permission) because it is the subject of the two following letters.

I am writing this letter to submit to you some general thoughts concerning the mathematical situation in Paris. I know you have been worrying for years about the "ageing" of Bourbaki, and have been preaching the virtues of rejuvenation. This issue did not worry me at the time, but I have come to the realization that something is indeed going wrong, not so much with Bourbaki, who in my opinion needs nothing more than an infusion of fresh blood, but through a dearth of talented young researchers united around some subject or other, and the absence of the stimulating atmosphere produced by strong shared scientific interests. There used to be such an atmosphere in Nancy in the old days; I also found it in Paris up until around 1959 (the year that Lang and Tate were there), and I find it to a very strong degree here at Harvard, whereas Paris seems to be getting more gloomy every year as far as the scientific atmosphere goes. Everyone incubates his own ideas in his own corner, and the seminars are turning into some kind of social ritual, after which everyone runs home as fast as possible... The young researchers maintain a respectful distance from their hierarchical seniors, and moreover, since they often force themselves to follow about ten lecture courses and seminars a week, they do not necessarily have a lot of time left for thinking, and they therefore do not always have much to say, or even to ask.

Obviously, foreign mathematicians have started to notice this state of affairs, even if they are too tactful to say anything without being asked. I have talked to several of them about it, such as Zariski, Tate and Chern, because it was starting to embarrass me. There are obviously a number of factors, of which the most important is perhaps a question of "genes": no striking talent has appeared on the horizon in Paris for years. There is also Paris's geographical sprawl, which is less suited to scientific life and discussion than the university towns in the USA or small towns such as Nancy and Strasbourg, where everyone lives very near the University. But I think that neither of these explains the near sterility of recent years. On the other hand, I am starting to realize that the long military service has a disastrous influence. Surely it is not necessary for me to explain to you that an enormous effort and a continual tension are necessary for the beginner to be able to absorb a mass of very diverse technical

ideas in order to get to the point where he may be able to do something useful, maybe even original. For our part, we use up enough chalk and saliva until the moment finally arrives when the fellow can pull his own weight. Alas, that is precisely the moment when he is called upon to serve his country, as they say, and the beautiful enthusiasm and subtle cerebral reflexes acquired by years of studying and meditation will be put aside for two years, provided the General consents not to keep him at the shooting range for even longer. With such a prospect in view, I quite understand that a budding Mathematician is inhibited before he starts, and his natural enthusiasm is blunted. Whether he manages to hastily cobble a thesis together before his military service, or plays it smart and enrolls early, he will be useless for several years as an "insider" or at least as a "Parisian" i.e. someone who contributes to the fertility of the scientific atmosphere of Paris. Cartier has not been a Parisian for ages, and even his lectures at the Collège have not changed this, being nothing but the fugitive appearances of a fellow on leave carrying out a social duty of no importance. Just when Gabriel is beginning to be interesting, off he goes to the army, J.-P. Serre : The French term is "bonvoust", ENS slang for military service. and when he comes back he will go to Strasbourg, which I feel as a serious loss for Paris. Apparently we cannot even invite him to IHES immediately, as this would not go down well with the University, which does not have enough professors! The situation is absolutely grotesque. With some difficulty, I have managed to scrape together four or five ex-Normaliens for my algebraic geometry seminar at IHES, who are just beginning to have some vague glimmers of understanding, and one or two of whom even appeared to be about to start on some useful and even urgent work, namely Verdier and Giraud. Nothing doing: unless I am mistaken, both of them, and certainly Verdier, are enrolling early, and in the end someone else (myself if necessary) may end up doing the work for them. If I do not actually have the impression of preaching in the wilderness in Paris, I am at least certain of building on sand.

This situation does not exist in the USA, where at least the State is intelligent enough not to waste its "brain-power" on military exercises. There is no difficulty for a talented student to get exempted from the draft on the grounds of being "indispensable to the defense of the nation", a euphemism which has probably never fooled a single American civil servant. This is exactly the point I wanted to make in this letter. We cannot require the soldiers or the politicians or the princes that govern us to be aware of the psychological subtleties of

scientific research, or to realize that it affects the scientific level of a country when the development of their young researchers is halted or put on hold for two critical years of their training. If they need to be informed of this fact, the only people who can do so with a certain degree of authority are yourself and our colleagues. (I personally am in any case completely out of it.) I am thinking particularly of you, because of your position at the Ecole Normale, which does after all potentially carry with it non-standard duties towards your present and former students. What is more, as you are not suspected of any political "partiality", you are in a better position to do something about it than Schwartz would be, for example: something like writing a series of articles in "Le Monde", or a personal letter to the President, or whatever. In any case, if you do not speak out, I really wonder who will.

It is strange that the French, with their reputation of rebelliousness, are in fact less politically-minded than the Americans. I am also thinking of the fact that it is impossible for a foreigner to teach in a State institution in France, despite a dearth of teachers which is becoming more acute every year. To the best of my knowledge, although all academics personally agree that this law is out-dated and absurd, none of them have ever raised the question with public opinion and public authority, and the timid changes to this law (foreigners in the CNRS, temporary associate professorships for foreigners) were not even initiated by academics "who should know".

I will stop my cogitations here, since I am not very comfortable preaching action and virtue when I am not in a position to act myself. All told, the only practical question arising from this for me is a strictly private one, namely whether I will stay in France or emigrate as other mathematicians have done before me, even if the actual working conditions here do not compare.

Thank you for your letter, and the trouble you have taken to correct my text. How is "expression latine" going?

Regards,

A. Grothendieck

October 26 1961 Jean-Pierre Serre

Dear Grothendieck,

Your letters raise a number of questions, to which I will try to give short answers.

(1) I was very interested by your letter to Cartan, and I more or less agree with what you say. However, you may have some illusions about what Paris used to be; it was never a "hothouse" like Göttingen in the old days, then Princeton, and now probably also Harvard, where people continually exchange ideas and see each other every day. In Paris, everyone works at home, and is more independent of the "latest fashion", and most collaborations happen by telephone... I suppose that geography has a lot to do with this, but I am not sure that it is such a bad state of affairs.

What is certainly more serious is the rather low level of the current generation ("orphans", etc.) and I agree with you that the military service is largely responsible. But it is almost certain we will get nowhere with this as long as the war in Algeria continues: an exemption for scientists would be a truly shocking inequality when lives are at stake. The only reasonable action at the moment — we always come back to this — is campaigning against the war in Algeria itself (and secondarily, against a military government). It is impossible to "stay out of politics".

(2) I have tried to clear things up with Schwarzenberger.

(3) Bourbaki's Commutative Algebra. You are very harsh on Valuations! I persist nonetheless in keeping them, for several reasons, of which the first is practical: n people have sweated over them, there is nothing wrong with the result, and it should not be thrown out without very serious reasons (which you do not have). Of course, if it were proved to be of no use and misleading, this first argument would not hold water. But that is not the case. Even an unrepentant Noetherian needs discrete valuations and their extensions; in fact, Tate, Dwork and all the p-adic people will tell you that one cannot restrict oneself to the discrete case and the rank 1 case is indispensable; Noetherian methods then become a burden, and one understands much better if one considers the general case and not only the rank 1 case. Finally, the general theorem of extension of specializations is a very beautiful and aesthetically satisfying result, which has the advantage (for example) of making it clear that a valuation has an extension to an overfield. It is not worth making a mountain out of it, of course, which is why I energetically fought Weil's original plan to make it the central theorem of Commutative Algebra, but on the other hand it must be kept. [As for the fact that Zariski and Krull have done better things

than valuations, that is certainly true, but I do not see how it is relevant to the discussion.]

Nor do I really agree with your objections to the Krull chapter. You must remember that on several occasions we wrote drafts following each of the two points of view: Noetherian and integrally closed, or general Krull. The comparison between them, and (whatever you may say) the factorial case where discrete valuations fit in perfectly well, ended up deciding in favor of the general (Krull) case, and I do not regret it. There is only one thing I really do not like in the draft, namely the definition using discrete valuation systems, when it would be so simple to say that an integral domain A is Krull if $A_{\mathfrak{p}}$ is a discrete valuation ring for any prime ideal $\mathfrak{p}$ of height 1, $A = \bigcap A_{\mathfrak{p}}$, and every $x \in A$, $x \neq 0$, is only contained in a finite number of $\mathfrak{p}$. I tried to get the congress to change its mind about this, but it would have then been necessary to rewrite the chapter, which was contrary to decisions that had already been made. In any case, it is only a minor detail.

[It is clear that these two chapters are basically an insertion into Bourbaki of "Papa's Commutative Algebra", as de Gaulle would say. But I am much less "fundamentalist" than you on such questions (I have no pretension to know "the essence" of things) and this does not shock me at all.]

In any case, these chapters are as good as printed J.-P. Serre : "These chapters are as good as printed...". Chapter 7 was only printed four years later. and discussing them is now a moot question. I am much more interested by the next part of Commutative Algebra, and there the outlook is rather gloomy: Bourbaki showed little enthusiasm for embarking on further chapters, and I myself had great difficulty in making coherent propositions, largely because I do not know what should fit into the algebraic context or the scheme-theoretic one. We agreed on hardly anything except a single chapter, which does look very nice:J.-P. Serre : The publication of the end of Bourbaki's *Commutative Algebra* took a long time: Chapters 8 and 9 came out in 1983 and chapter 10 in 1998. It is a pity; these chapters are well written, and would have been very useful if they had appeared twenty or thirty years earlier. Fortunately, provisional drafts circulated, and Grothendieck himself occasionally used them for the EGA's. the one on dimension that Tate is supposed to write. After that comes chaos: what will we say about homology theory? The risk, as I clearly saw when writing exterior algebra, is of being immediately dragged into abelian categories and ending up doing nothing at all; the other risk, of course, is of being tempted to wait for the geometric language. I would very much like to know how you view these things. Idem for Cohen.

(4) Factoriality of regular local rings. You have probably noticed that the proof of Kaplansky's I sold you proves the following: if A is local and integrally closed and $\mathfrak{a}$ is a reflexive module of rank 1 over A which is of finite homological dimension, then $\mathfrak{a}$ is free. In fact, it seems that a student of Kaplansky's has managed to replace the condition "$\mathfrak{a}$ of rank 1" by "$\mathrm{Hom}(\mathfrak{a},\mathfrak{a})$ is free", but I guarantee nothing.

(5) Desingularization of surfaces in characteristic 0. It is funny you should have rediscovered "Jung's" old method, which I myself actually explained to you (having more or less rediscovered it myself) quite some time ago. Abhyankar, who includes it at the end of one of his papers on coverings (I think it is in "Ramification of algebraic functions" in the American Journal) explains why it does not work in characteristic p, essentially because the ramification is not "tame". Perhaps it is somehow possible to make it so? J.-P. Serre : "possible to make it so". No: it is not that easy to get rid of wild ramification.

On this topic, note that when you blow up a non-singular point P on a surface S, then a point $P^{(1)}$ of the line obtained in this way, and so on indefinitely, you get an increasing sequence of local rings whose union $\mathcal{O}$ *is a valuation ring*; this is another way of saying that eventually this sequence of blow-ups ends up "separating" distinct branches. This is one of the most interesting examples of a non-banal valuation ring.

Still on the subject of Jung's method: you may have noticed that when a cover contained in $B(x^{1/m}, y^{1/n})$ appears, it is not generally possible to simultaneously desingularize the base and the cover. J.-P. Serre : "Simultaneously desingularize the base and the cover ...". According to Shephard-Todd, this is only possible if the local inertia groups are generated by pseudo-reflections. (The first example of this, if I remember rightly, involves a cyclic covering of degree 5.) In particular, one needs general blow-ups on the base, and not just blow-ups with the maximal ideal; have you ever carefully investigated what the right thing to do is? It is classical, of course, but I have always been too lazy to look into it.

(6) What you say about rational cohomology of affine varieties does not surprise me much. It is basically a return to the viewpoint of our fathers, who were so fond of their integrals of the n-th kind, $n = 1, 2, 3$. One would have to check in Atiyah-Hodge, for example, but I think what is already known should be enough to prove Kostant's conjecture for affine varieties of the form $X - H$, where X is projective and non-singular, and H is not too badly behaved.

(On the subject of affine varieties, can you decompose (filter or whatever) their cohomology in such a way as to highlight the parts that come from the completed variety? I cannot express what I mean J.-P. Serre : "I cannot

express what I mean ... ". The language of motives was missing., but you will understand what I want if I put it in the following form: what conjectures should be made on the zeta function of a non-singular affine variety? I find it scandalous that it should be necessary to embed it (if that is even possible!) in a horrible non-singular projective variety, which is not at all unique; on the other hand, I have not been able to formulate anything. Have you any kind of homology to hand, besides than the usual one (for example, "with closed support" or God knows what) J.-P. Serre : "with closed support or God knows what": with proper support. which could be of some use?)

I would have liked to talk to you about Dwork J.-P. Serre : "About Dwork and Fredholm theory": see [**Se62a**]. and Fredholm theory J.-P. Serre : "Your paper on the subject" (cf. footnote): [**Gr56b**]. [7] and complete valuation fields, but I will keep it for another time. Yours, J-P. Serre

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[7] I am aware of your paper on the subject where you say that "everything works". But you had a fair number of illusions on valuation fields at the time, and you tended to use bounds of the type $\left|\frac{1}{n!}x\right| \leq \frac{1}{n!}|x|$; horror of horrors! It has to be looked into more closely.

October 31, 1961 ALEXANDRE GROTHENDIECK

My dear Serre,

I do not agree with you that nothing should be done against the military service — for gifted scientists in particular — before the end of the Algerian war. To start with, as far as injustice is concerned "when lives are at stake", if it is an injustice to exempt certain people from national service, then the difference between doing so during or after a time of guerilla war is one of degree and not of essence. I do not think that the danger of losing one's life is such, at this point, that it has become more important than the loss of two years of training (for any young person, scientist or otherwise), leaving aside entirely the moral question (to which most people are apparently indifferent). The minimal probability of being killed does not seem to me to make a big difference. On the other hand, if certain Academics brought the effects of military service (and, by implication, of the Algerian war) on the scientific level of the country to the attention of the public and the authorities, and required some reforms, it would not exclude the possibility of classical scholars, technicians, firemen and lamp-lighters grouping together to require analogous reforms for themselves, on analogous and to my mind equally valid grounds. Any action in this direction, even if very limited, will contribute to making people realize the consequences of the militarization of the country, and might create a precedent for analogous and vaster actions. But in this case it is obvious that it is only by limiting the problem and the proposals to a restricted situation which from many points of view is "ideal" that there is any chance for rapid success, especially if it is done by Academics without political affiliations, such as Cartan. Note that the arguments being put forward are just as valid in wartime, if not more so, I mean from the government's point of view, as it is quite obvious that the Americans, for example, are even more careful to keep from removing their scientists and their high-class technicians from their laboratories in wartime than in peacetime. — And finally, I have a very down to earth point of view on the military service, namely catch as catch can, and the more people there are who, by whatever means, be it conscientious objection, desertion, fraud or even knowing the right people, manage to extricate themselves from this idiocy, the better.

Your argument in favor of valuations is pretty funny: " n people have sweated over it, there is nothing wrong with it, and it should not be thrown out without very serious reasons". The principle generally respected by Bourbaki is rather that there should be very good reasons for including a huge mess, especially in a central position; the fact that n people have sweated over it is

certainly not a good reason, since these n people had no idea of the role of the mess in commutative algebra, but had simply received an order to figure out, Bourbakically, some stuff that they unfortunately did not bother to examine critically as part of a whole. Your comments on rank zero valuations constitute an argument for removing it from where it is now. Indeed, the right point of view for this is not commutative algebra at all, but absolute values of fields (archimedean or not). The p-adic analysts do not care any more than the algebraic geometers (or even Zariski himself, I have the impression, as he seems disenchanted with his former loves, who still cause Our Master to swoon) for endless scales and arpeggios on compositions of valuations, baroque ordered groups, full subgroups of the above and whatever. These scales deserve at most to adorn Bourbaki's exercise section, as long as no one uses them. A solution which seems reasonable to me (but it is probably too late for the first edition) would be as follows: a) Two or three pages on general valuation rings with the integers: definition, extension, application to the characterization of the normal closure, and that's it. b) Discrete valuation rings anywhere after that, for example (if the Krullians remain inexorable) as paragraph 1 of Krull rings. [On the subject of Krull, it is funny to notice that the term "Krull ring" probably does not appear in any paper by a disciple, and will probably not appear in the future either, so certain are the disciples that Bourbaki is one thing and Mathematics is another . . .]. Of course, we can have them whenever we want. c) Absolute values (which were defined ages ago by Bourbaki) to be put in a final chapter if need be or kept in reserve for "the part of our Treatise devoted to the Theory of Analytic Spaces, also known as p-adic Analysis." This last solution is perhaps the most reasonable.

I have the impression that after the dimension, there are a couple of beautiful Chapters which could be written in a fairly down-to-earth way, hardly using any more Homological Algebra than Chapter 1 on flatness, and to which geometric language would not add very much, namely 1°) Depth, Cohen-Macaulay, Serre's S_k property, regular rings and the R_k property (rings which are regular in codimension $\leq k$). 2°) Good rings. It is true that depth and S_k are notions which mean something for an abelian sheaf on any Noetherian space, and can be better understood from this point of view via relative cohomology. (This will be written up in Elements III.9.) Nevertheless, the definition of depth via M-sequences is perfectly adequate, and is indeed the one best adapted to the immediate use we have in mind for it: there will be a short report on this in Elements IV with the properties that are specific to the situation $A \to B$, M, N, where $A \to B$ is a local homomorphism

of Noetherian local rings, M (N) a finitely generated B (A)-module, and one wants to determine the properties of $M \otimes_A N$, for M flat over A. We have $\dim_B M \otimes_A N = \dim_A N + \dim_{B \otimes k} M \otimes k$; the same formulas hold for depth, codepth, cohomological dimension, and there are variants for R_k and S_k. These results will arrive just in time for 2º, which explores the conditions under which the fibers of $\operatorname{Spec} \widehat{A}$ over $\operatorname{Spec} A$ are regular or even "geometrically" regular schemes. It is true that one is often tempted to go off into schemes or abelian categories, but I do not believe it is morally impossible to prevent this. — There might also be a 3º) on Cohen algebras, a kind of variation on Hensel, disjoint from the preceding ones, except that the Cohen structure theorem may be needed in 2º), leading to the exchange of 2º and 3º. Anyway, 3º could be put in anywhere earlier, for instance it would have been very good as a paragraph in "filtrations and topologies": too late! Apart from ramification theory, about which I have no ideas in any case, it does not seem to me necessary to add anything else to Commutative Algebra, since all the rest (including intersections, as long as everything necessary about *multiplicities* of local rings is there) will be better expressed in geometric terms. By the way, the result on a flat extension of a Noetherian local ring corresponding to a given residue extension (which can often advantageously replace the method of extension of a valuation) fits very well in the context of Cohen Algebras.

I have not really understood your question on (Weil, I presume) cohomology of affine varieties, and moreover I know nothing about a potential natural filtration, even on a complete variety. We will talk about it again when you come to Harvard. Regards,

A. Grothendieck

September 6, 1962 ALEXANDRE GROTHENDIECK

My dear Serre,

I have been thinking again about your stuff on Lie algebras associated to groups of Galois type. It seems quite obvious to me that to be usable, everything will have to be expressed in terms of *representations* of pro-Lie algebras (otherwise you are screwed for expressing even the most obvious functorialities). More precisely, if ℓ is a fixed prime number, associate to any group G of Galois type a "pro-Lie algebra" over $\mathbf{Q}_\ell$, $\mathfrak{g} = (\mathfrak{g}_i)$, where the $\mathfrak{g}_i$ are finite dimensional and the map $\mathfrak{g}_j \to \mathfrak{g}_i$ is surjective. Then $\mathfrak{g} = L_\ell(G)$ depends functorially on G; in particular, if G acts continuously on a module of finite type over $\mathbf{Z}_\ell$ (or even over $\mathbf{Q}_\ell$), then $\mathfrak{g}$ acts on $M \otimes_{\mathbf{Z}_\ell} \mathbf{Q}_\ell$ (resp. on M) via one of the $\mathfrak{g}_i$. This $\mathfrak{g}$ only depends on the *germ* of G. It would be nice to have a dictionary allowing us to pass from $\mathfrak{g}$ to G (or rather to the germ of G); maybe there are some tangible results to be proved when the group G is a germ of ℓ-*groups*, i.e. when there is an open subgroup U which is an ℓ-group. Thus:

QUESTION 1. J.-P. Serre : Questions 1 and 2: no, cf. the letters of September 7, 1962 and September 12, 1962. Let $H \subset G$: is it true that $\mathfrak{h} = L_\ell(H) \to \mathfrak{g} = L_\ell(G)$ is injective?

(N.B. The corresponding statement for an epimorphism is trivially true.)

It would be enough to be able to solve the following question: let $u : H \to \Gamma$ be a continuous homomorphism, with Γ of Galois type (it is enough to take $\Gamma = \mathrm{GL}(n, \mathbf{Z}_\ell)$); can u then be extended to a homomorphism of an open subgroup of G?

QUESTION 2. Let H_1, H_2 be closed in G, with Lie algebras $\mathfrak{h}_1$, $\mathfrak{h}_2 \subset \mathfrak{g}$. If $\mathfrak{h}_1 \subset \mathfrak{h}_2$, is it true that $\mathrm{germ}(H_1) \subset \mathrm{germ}(H_2)$ (thus $\mathfrak{h}_1 = \mathfrak{h}_2$ implies $\mathrm{germ}(H_1) = \mathrm{germ}(H_2)$)?

It would probably be enough to prove this when $H_1 = \mathbf{Z}_\ell$. If the answer were positive, then a homomorphism of germs of Galois ℓ-groups would be determined by the homomorphism on the Lie algebras (but it is probably impossible to start with an arbitrary Lie algebra homomorphism and define a homomorphism of germs of groups from it).

Another remark: There is a *canonical map* $\log : G \to \mathfrak{g}$ [N.B. Write also $\mathfrak{g}$ for $\varprojlim \mathfrak{g}_i$], as can be deduced from the case where G is an analytic group, by forming $\frac{1}{n} \cdot \log x^n$ for large n (log is defined in a neighborhood of e in any case, and y is such that $\log(z^m) = m \cdot \log z$). (If the answer to question 2 is positive,

then $\log x = \log y$ if and only if there is an n such that $x^n = y^n$). Thus, in all the situations you consider with abelian varieties, for instance, which can be assumed to be defined over *rings A of finite type over* $\mathbf{Z}$, to any closed element of $\mathrm{Spec}(A)$, there corresponds via Frobenius, taking the log, a well defined element of $\mathfrak{g}$ (not just a line in $\mathfrak{g}$) and hence a well-defined endomorphism (modulo G actions) of $V_\ell = T \otimes_{\mathbf{Z}_\ell} \mathbf{Q}_\ell$, not just a 1-dimensional subalgebra of $\mathfrak{gl}(V_\ell)$. If one desires a construction invariant under base change $A \to B$, then $\log F$ can be replaced by $\frac{1}{n} \log F$, where $n = [k(x) : \mathbf{F}_p]$. Moreover, knowing these operations on V completely determines the representation of $\mathfrak{g}$ on V by ...'s density theorem, J.-P. Serre : "...'s density theorem...". Grothendieck had forgotten the name Chebotarev. at least if A is a subring of a number field. On the same subject, you must know the answer to:

QUESTION 3. J.-P. Serre : The answer to Question 3 is "almost yes", cf. the letter of September 7, 1962. Let S be a normal scheme of finite type over $\mathbf{Z}$, and let S' be a Galois etale covering with group G: does the density theorem hold, i.e. for any $g \in G$, do there exist closed points of S whose Frobenius lies in the class of g?

Yours,

A. Grothendieck

September 7, 1962 Jean-Pierre Serre

Dear Grothendieck,

Here is a remark on your letter received this morning, even though I cannot answer all your questions:

Question 1 (does $G \subset H$ imply that $\mathfrak{g} \subset \mathfrak{h}$?) resists my efforts. A positive answer would in particular imply the following: if C is a subgroup of G isomorphic to $\mathbf{Z}_\ell$ (i.e. "a 1-parameter group") there is a linear representation of G (over $\mathbf{Q}_\ell$) which is injective on C. This does not look easy to me, but on the other hand I cannot find a counterexample.

The answer to Question 2 is negative. In fact, this would imply that if $\mathfrak{g} = 0$, then the germ of G is trivial, i.e. G is finite. However this is obviously false (take G to be an infinite product of copies of $\mathbf{Z}/\ell\mathbf{Z}$).

The answer to question 3 (density) is positive, provided it is stated more carefully:

Let $T \to S$ be a Galois etale covering, with Galois group G, where S is of finite type over $\mathbf{Z}$. Assume T is irreducible and of dimension ≥ 1. Then for any $s \in G$, there are infinitely many closed points of T whose Frobenius is equal to s (and something can be said about their density, but in general one doesn't care).

This is proved using Artin's L-functions (Lang had done it in the "geometric" case, but in my course in 1961, I did it in the general case).

I will try to look at Question 1 some more, even though I do not really have much hope: it would imply a characterization of analytic groups (the one I told you about in your office) which would really be too nice. I am hindered, when dealing with this kind of question, by my unfamiliarity with p-groups, particularly free p-groups; I may try to get Lazard interested in this kind of question one of these days: it is right up his street.J.-P. Serre : "I may try to get Lazard interested in this kind of question". I succeeded. The result was his grand memoir:

M. Lazard, *Groupes analytiques p-adiques*, Publ. Math. IHES **26** (1965), 389–603. Yours,

J-P. Serre

Tuesday evening, September 1962 Jean-Pierre Serre

Dear Grothendieck,

The answer to your first question is also negative. There is even a *commutative* counterexample — I should have spotted it earlier! Here it is:

Take G to be the product, for $n = 1, 2, \ldots$, of cyclic groups $G_n = \mathbf{Z}/p^n\mathbf{Z}$ (where p is the prime number which interests me). It is easy to prove (by dualizing, for example), that $\mathrm{Hom}(G, \mathbf{Z}_p)$ is trivial; it follows that any morphism from G into an analytic group over $\mathbf{Q}_p$ has *finite* image; in other words, the Lie algebra $\mathfrak{g}$ of G is trivial. But on the other hand G contains elements which are not of finite order; such an element topologically generates a subgroup H of G which is isomorphic to $\mathbf{Z}_p$, i.e. a 1-parameter subgroup. The Lie algebra $\mathfrak{h}$ of H is 1-dimensional, and this gives the desired counterexample.

Yours,

J-P. Serre

September 12, 1962 Alexandre Grothendieck

My dear Serre,

I have the impression that the answer to my Question 1 is also false. Take for example

$$G = \prod_n \mathbf{Z}/\ell^n \mathbf{Z}.$$

Then for any linear representation of G the image of the group is finite (since by a remark of Serre-Borel, for any linear group over a field of characteristic 0 there is an upper bound for the order of elements of finite order. On the other hand, an analytic group over a field of characteristic 0, all of whose elements have m-th power equal to e where m is fixed, has trivial Lie algebra and is hence discrete...). Thus, the Lie algebra $\mathfrak{g}$ of G is trivial, but G contains elements which are not of finite order, so subgroups which are isomorphic to $\mathbf{Z}_\ell$ (1-parameter subgroups).

[...]

Yours,

A. Grothendieck

Friday morning, September 1962 Jean-Pierre Serre

Dear Grothendieck,

I had a look at Mike Artin's argument which proves that any non-singular variety can be covered with "good" open sets ; it is less trivial than you seemed to believe, and I would like to point out some of the difficulties (which to be frank are specific to characteristic p): Start with a normal V^n embedded in a projective space P^N. Take a hypersurface W in V^n containing the singular points S (of dimension $\leq n-2$), and a point $x \in V - W$. The aim is to prove that there is a "good" neighborhood of x contained in $V - W$. The method you pointed out to me consists in slicing V (and therefore also $V - W$) with a suitable family of linear varieties L^{N-n+1} in such a way that the intersections $L \cap V$ are curves. Let L_x be the L that contains x. There are (at least) two precautions to take:

(i) $V \cap L_x$ must be a non-singular curve

(ii) L_x must cut W transversally (indeed, one wants W to define an etale multisection of the base).

Note that x is given: no choice there; thus one cannot invoke (for (i)) the fact that the intersection $V \cap L_t$ is non-singular for generic t; one must restrict oneself to those L passing through x, i.e. show that $V \cap L$ is non-singular if L is generic *among* the linear varieties of codimension $n+1$ passing through x. However, I am not at all sure that this is true; it could be possible (in characteristic p) that all the linear varieties tangent to V pass through x (without V being linear), in which case we are screwed. Fortunately, it can be shown that this hitch does not arise *if one considers a projective embedding of V which is the double of another one*J.-P. Serre : It is indeed by means of hypersurfaces of degree > 1 that M. Artin proves that his "good open sets" exist, cf. SGA 4 III, exposé XI, th. 2.1, p. 65. (this is a harmless condition); the proof is not difficult: one needs to check that the generic quadric passing through x is not tangent to V, which can be done by a dimension count. I feel no desire to give you the details. (Another method: show that if all linear varieties tangent to V pass through x, then the projective degree of V is $\equiv 1$ mod p; but one can always find an embedding whose projective degree is $\equiv 0$ mod p.)

Analogous precautions have to be taken for condition (ii): starting with a stupid embedding of V, it is possible that every hyperplane passing through x is tangent to W; once again one can get around the problem by taking the double of a projective embedding.

I think these are the only hitches, i.e. the construction you explained to me works after this with no other problems.

Yours,

J-P. Serre

P.S. To convince you, here is a numerical example: in the space P^3 parametrized by homogeneous coordinates (x, y, z, t), let V be the surface of equation

$$t^p z + x^{p+1} + y^{p+1} = 0.$$

V is normal, and its unique singular point is $(0, 0, 1, 0)$. Set $x = (0, 0, 0, 1)$, which is a simple point. The generic plane L_x passing through x can be written as $uX + vY + wZ = 0$, and you can easily check that it is tangent to V at the point (x, y, z, t) such that $x^p = u$, $y^p = v$, $t^p = w$, $z =$ whatever. The intersection $L_x \cap V$ therefore *always* has a singular point.

P.S.2. In case you receive this before Monday: Hyman Bass will be speaking on Monday morning at 10:00 at IHP on his construction of an algebraic K^1, inspired by J.H.C. Whitehead's "simple homotopy types".

Friday evening, September 1962 JEAN-PIERRE SERRE

Dear Grothendieck,

Here is another little paper, inspired by Mike Artin's method. J.-P. Serre : See the exercises in *Galois Cohomology*, LN 5, Chap. I, §2.6. It deals with the relation between the cohomology of "discrete" groups and of "Galois" groups (i.e. "ordinary" cohomology and "Grothendian" cohomology). Let G be a group and let $u : G \to K$ be a homomorphism from G to a totally disconnected compact group K. Assume that the image of G is dense in K (the most interesting case is when K is the separated completion of G with respect to the finite-index subgroups topology). It is clear that a topological K-module M is also a G-module, whence $H^q(K, M) \to H^q(G, M)$ for any $q \geq 0$. Consider the following properties:

C_n — For any finite K-module M, $H^q(K, M) \to H^q(G, M)$ is bijective for $i \leq n$ and injective for $i \leq n + 1$.

C_n' — $H^q(K, M) \to H^q(G, M)$ is surjective for $i \leq n$.

E_n — For any $x \in H^q(G, M)$, $1 \leq q \leq n$, there is a finite M' containing M such that x becomes 0 in $H^q(G, M')$.

F_n — For any $x \in H^q(G, M)$, $1 \leq q \leq n$, there is a subgroup G_0 of G, the inverse image of an open subgroup of K, such that x induces 0 in $H^q(G_0, M)$.

These four properties are equivalent. $C_n \Rightarrow C_n'$ is trivial, $C_n' \Rightarrow E_n$, and $C_n' \Rightarrow F_n$ are easy, $E_n \Rightarrow C_n$ follows by a standard little dévissage, and $F_n \Rightarrow E_n$ thanks to "induced" modules.

Note that $C_0, \ldots, F_0$ always hold and if $K = \widehat{G}$, then so do $C_1, \ldots, F_1$ (C_1' — or else F_1 — is the easiest to prove). I will say a group is *good* if it satisfies all the C_n (for $K = \widehat{G}$, of course). I should immediately say that I do not know any "bad" groups, but they surely exist (I have a candidate in mind, but I have not yet looked into it in detail). I am no less certain that all interesting discrete groups are "good"; J.-P. Serre : "I am no less certain that all interesting discrete groups are good" . I was entirely wrong. I quickly realized that most arithmetic groups of rank > 1 (such as $SL_n(\mathbf{Z})$, $n > 2$) are not "good" in the sense given here. in particular, this should be true for unit groups of arithmetic groups — and probably also for fundamental groups of algebraic varieties.

Here is another property of good groups: let $E/N = G$ be an extension, and assume that N is finitely generated. If G is good (it is enough that just C_2 hold), then any subgroup of finite index of N contains a subgroup $N \cap E_0$,

where E_0 is of finite index in E; in other words, there is an exact sequence:

$$0 \to \widehat{N} \to \widehat{E} \to \widehat{G} \to 0.$$

(This is easy to prove when N is abelian, and then standard methods of group extensions reduce the general problem to this case.)

This yields the following:

Let $E/N = G$ be an extension with N and G good, N finitely generated, and such that $H^q(N, M)$ is finite for any finite E-module M. Then E is good.

This is trivial upon mapping the spectral sequence of $\widehat{E}/\widehat{N} = \widehat{G}$ to that of $E/N = G$.

Corollary: a successive extension of finitely generated free groups is a "good" group.

Indeed, a finitely generated free group is "good".

This corollary is the point I wanted to get to; the π_1's of Mike Artin's good neighborhoods are precisely such groups, and their π_i's are trivial; their cohomology can therefore be identified with $H^q(G, M)$, and property F_n shows that this cohomology is killed by passage to a suitable finite etale extension, whence etc.

I also checked that the fundamental group of an algebraic curve is good — as it should be!

Yours,

J-P. Serre

April 10, 1963 Alexandre Grothendieck

My dear Serre,

Could you shed some light on a question on local fields (which crops up in some integrality questions in cohomology). The motivation is the fundamental Ogg-Shafarevich formula, which it is a good idea to write in terms of etale cohomology as follows. Let C be a connected complete simple algebraic curve over k, where k is algebraically closed, and let F be a "constructible" torsion sheaf on C, i.e. having finite fibers and "locally constant" (in the etale topology) over some suitable open set $U = C - S$ (i.e. defined over U by an etale group scheme over U, or alternatively by a finite etale group G over $K = k(C)$ which is unramified at the points of U). Let us say that F is *tamely ramified* at points of S if this holds for G. Then Ogg's formula, for F *tamely ramified* and of course relatively prime to the characteristic, can be written

$$(0) \qquad\qquad E(C, F) = nE(C) - \sum_{x \in C} \epsilon_x(F)$$

where $E(C, F)$ is the Euler-Poincaré characteristic of C with coefficients in F:

$$(1) \qquad\qquad E(C, F) = \sum_{0 \le i \le 2} (-1)^i \mathrm{length}\, H^i(C, F),$$

$E(C)$ is the usual Euler-Poincaré characteristic of C, namely $2 - 2g$, and the local correction terms are given by

$$\epsilon_x(F) = n - n_x,$$

where n_x is the length of F_x and n is the value of n_x for general x, i.e. the length of G considered as a Galois module. As you are not supposed to know what the right-hand side of (1) means, I shall make it explicit by noting that if S is any finite subset of C then

$$(2) \qquad\qquad E(C, F) = E(C - S, F) + \sum_{x \in S} n_x$$

(which holds without the tamely ramified condition), and if S is non-empty then $H^2(C - S, F) = 0$, so that

$$(3) \qquad E(C - S, F) = \mathrm{length}\, H^0(C - S, F) - \mathrm{length}\, H^1(C - S, F)$$

and finally, if S contains the singular set of F, one can interpret the H^i of the right-hand side of (3) in terms of the Galois cohomology of the fundamental group of $C - S$. Bearing (2) in mind, (0) is equivalent to the same formula for $C - S$ (where of course

$$E(C - S) = 2 - 2g - s),$$

and taking S large enough to contain all singular points of F, Ogg's formula becomes simply

$$(4) \qquad\qquad E(C - S, F) = nE(C - S).$$

The proof is easy using the structure of the "tame" fundamental group of $C - S$, which is more or less free on r generators, with $E(C - S) = 1 - r$.

Unfortunately, by a counterexample due to Tate and quoted by Ogg, this formula is false if F is no longer assumed to be tamely ramified. The natural question is whether or not (0) remains valid provided a suitable definition of the $\epsilon_x(F)$ is given, it being understood that they must be "local" invariants in an obvious sense. There is a very tempting analogy with Weil's formula involving the Artin representations; one would like a formula containing both Weil's theorem and Ogg's formula in the not necessarily tame case. I have tried this, using K-formalism as a guide, but have not found a reasonable formulation and am not even convinced that one exists. In any case, the existence of local invariants ϵ_x which would make Ogg's formula valid is a perfectly precise mathematical question, and if the answer is negative we will have to find a counterexample (two sheaves with the same numerical invariants n and $E(C)$ and the same kinds of local singularities, but different Euler-Poincaré characteristics). Otherwise, the search for local invariants with reasonable variance behavior leads naturally to the following problem, for which you may have some feeling:J.-P. Serre : I had no difficulty defining the invariants Grothendieck was asking for (cf. the following letter of April 10, 1963) and he immediately deduced from them the Euler-Poincaré formula that he wanted (which is known as the "Ogg-Shafarevich-Grothendieck" formula). For more details, see Raynaud's presentation to the Bourbaki seminar 1964-1965, no286, and SGA 5, exposé X (by I. Bucur), and J. Milne *Étale Cohomology*, Princeton Univ. Press, 1980, p. 190, th. 2.12.

For any "geometric local" field K (i.e. fraction field of a complete discrete valuation ring with algebraically closed residue field), and any finite etale group F defined over K (i.e. a finite Galois module over the Galois group $G(K_s/K)$) whose order is relatively prime to the residue characteristic, define an integer $\epsilon(F)$ satisfying the following conditions:

   a) $\epsilon(F) + \text{length } H^0(K, F)$ is additive in F (for exact sequences).
   b) $\epsilon(F) + \text{length } H^0(K, F) = \text{length } F$ if F is tamely ramified.

c) Let L be a finite separable extension of K, G a finite etale group defined over L (of order relatively prime to the residue characteristic), then

$$\epsilon(\prod_{L/K} G) = \epsilon(G) + \theta(L/K)\text{length}(G),$$

where $\theta(L/K)$ is the degree of the discriminant of L/K.

Finally, if there is such an invariant one hopes it will satisfy the following condition:

d) Let A be an algebraic connected group defined over K, and let ℓ be a number which is relatively prime to the residue characteristic. Then $\epsilon(_\ell A)$ is independent[8] of ℓ, where $_\ell A = \text{Ker}\,\ell\,\text{id}_A$.

Note that it may be more practical to work with

$$\lambda(F) = \epsilon(F) + \text{length}\,H^0(K, F);$$

a) and b) are then simpler and c) and d) do not change. It is actually not out of the question that there is no such invariant for finite groups, but that there is one when working with the rank of cohomology groups with ℓ-adic coefficients (instead of lengths of finite groups).

Another question: working over the complex numbers, assume that an abelian variety defined over $K = k(C)$ is given, hence by Néron a group scheme G over C which is simple over C. Following Kodaira, consider the group subscheme G^0 made up of the connected components of the fibers of G, and the corresponding analytic bundle G'^0, considered as a quotient V/Γ, where V is the tangent bundle along the zero section, and Γ is made up of the fundamental groups of fibers of G'^0. Then $H^1(C, G^0)$ can be interpreted as the subgroup of points of finite order in $H^1(C', G'^0)$ (the classes of analytic principal homogeneous G'^0-bundles); on the other hand, by the exact cohomology sequence, $H^1(C, G^0)$ has a "connected component" isomorphic to

$$(*) \qquad\qquad H^1(C', V')/\text{Im}\,H^1(C', \Gamma),$$

where $\text{Im}\,H^1(C', \Gamma) = H^1(C', \Gamma)/\text{Im}\,H^0(C', G'^0)$.

Note also that by GAGA,

$$H^i(C', V') = H^i(C, V), \quad H^0(C', G'^0) = H^0(C, G^0).$$

Given this, is it true that $\text{Im}\,H^1(C', \Gamma)$ is a *discrete* subgroup of $H^1(C', V')$? Might the quotient by this subgroup be an algebraic group, or even an abelian variety? In any case, using duality, one can define an alternating bilinear

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[8]note in the margin (Serre) "No : λ instead of ϵ"

form on $H^1(C', \Gamma)$ (coming from a polarization of A) which suggests it may be possible to polarize $(*)$. If it is true that $\text{Im } H^1(C', \Gamma)$ is discrete, it would follow that

$$r \leq 2 \dim H^1(C, V)$$

where r is the "rank" of $H^1(C, G)$, given by Ogg's formula, and $\dim H^1(C, V)$ is given, if $\dim(A) = 1$, by Riemann-Roch as a function of the degree of V, which Kodaira expresses uniquely in terms of the "kinds" of reductions A has at points of C. This inequality would be an equality if the quotient $(*)$ were an abelian variety. But the experts must know if such an equality or inequality is reasonable: it may be viewed as a relation:

$$r_0 \geq 2(g-1) + \sum_p \epsilon_p - 2\delta,$$

where $\begin{cases} g = \text{genus of } C \\ \epsilon_p = \text{the Ogg invariant} \\ \delta = \sum_p \delta_p \text{ as in Kodaira p.133 prop.4} \end{cases}$

linking r_0 (the rank of the group $A(K)$), the genus of C and numerical invariants depending only on the "types" of the reductions of A at the points of C; A is of course assumed to be "non-constant".

Are you coming to eat here next Tuesday?
Yours,
A. Grothendieck

Thank you for Shafarevich's manuscript, which I have received and passed on to Motchane. J.-P. Serre : "Shafarevich's manuscript". This is a reference to a text by Shafarevich, published in Russian (with a French abstract which I had written) in Publ. Math. IHES **18** (1964), 295–319 (English translation: Transl. A.M.S., Series II, **59** (1966), 128–149).

April, 1963 JEAN-PIERRE SERRE

Dear Grothendieck,

As promised, here is a short summary on your ϵ and λ. Let G denote the Galois group of a finite extension of the given local field, and let

$$G = G_0 \supset G_1 \supset \cdots$$

be the ramification groups of G; let me remind you that G_1 is a p-group and that G/G_1 is of order relatively prime to p.

Consider a prime number ℓ and a finite G-module A which is an ℓ-group. Assume of course that $\ell \neq p$.

Let g_i denote the order of G_i and write $g = g_0$ for that of G. Set

$$\epsilon(A) = (d(A) - d(A^G)) + \frac{g_1}{g_0}(d(A) - d(A^{G_1})) + \frac{g_2}{g_0}(d(A) - d(A^{G_2})) + \cdots$$

[$d(A)$ denotes the length of A].

Then $\lambda(A) = \epsilon(A) + d(A^G) =$ linear comb. of $d(A^{G_i})$. As the G_i, $i \geq 1$, are of order relatively prime to ℓ, the A^{G_i} depend additively on A, and it follows that $\lambda(A)$ is additive in A. This makes it possible, in what follows, to restrict to those A which are annihilated by ℓ, or more generally, which are vector spaces over a field of characteristic ℓ.

Let χ be the modular character associated to A. It is elementary that $d(A^{G_i})$ is equal to $\frac{1}{g_i} \sum_{s \in G_i} \chi(s)$. Applying this expression to $\lambda(A)$ and denoting the character of the Artin representation of G by a_G yields

$$\lambda(A) = \langle a_G + 1, \chi \rangle,$$

with the usual notation for the scalar product [note that χ is only defined for ℓ-regular elements of G, but the others lie in $G - G_1$ and $a_G + 1$ is in fact zero on $G - G_1$].

Let us now check that $\lambda(A)$ has the desired properties. This would be horrible with the direct definition, but luckily one can use the formal properties of $a_G + 1$.

(i) Independence of G. One must check that if a normal subgroup N of G acts trivially on A, then $\lambda(A)$ is the same relative to G and to G/N. But this follows easily from the formula

$$a_{G/N} = (a_G)^{\natural}, \text{ cf. Corps Locaux, p.108.}$$

(ii) Formula for induced modules. As above, using the cor. to Prop. 4 in Corps Locaux, p.109.

(iii) Integrality. It is known (by Brauer) that any modular character is a combination with integral coefficients of restrictions of ordinary characters.

However, it is also known that $\langle a_G + 1, \chi \rangle$ is integral for ordinary characters (Artin's theorem — non-trivial!) Whence etc.

Searching through Brauer, I noticed a curious thing, which I will initially state incorrectly in order to sell it better: using the fact that $a_G + 1$ is zero on the ℓ-singular elements, one can find a *projective* $\mathbb{Z}_\ell[G]$-module L of character $a_G + 1$, and the above expression for $\lambda(A)$ can then be written simply as:
$$\lambda(A) = d(\mathrm{Hom}_G(L, A)),$$
which beautifully highlights its integrality and additivity.

There are two corrections to be made to this: a) it may be necessary to use a "virtual" L, i.e. an element of the corresponding Grothendieck group (although I don't think so), b) it may (very probably) be necessary to act on a larger field of characteristic ℓ than the first one. These are details — and probably even useless details. J.-P. Serre : These details are indeed useless. Using results of Fong and Swan, one can construct a projective $\mathbf{Z}_\ell[G]$-module L having the desired properties, cf. *Représentations Linéaires des Groupes Finis*, §19.2. [9]

Analogous things are possible for infinite A (free of finite type over $\mathbb{Z}_\ell$, say), but I have not yet clarified this properly; one needs to beware of ramification in the infinite case.

I have no idea about the algebraic groups question. It looks strange.
Yours,
J-P. Serre

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[9] note in the margin : "exists by Swan!"

March 31, 1964 Jean-Pierre Serre

Dear Grothendieck,

I have constructed J.-P. Serre : Grothendieck had hoped to prove the Weil conjectures by showing that every variety is birationally a quotient of a product of curves.

In the present letter, I construct a counterexample in dimension 2. There are certainly simpler ones! an example of a surface whose function field is not contained in that of a product of two curves (nor, of course, of a product of n curves, since the case of n can be trivially reduced to that of 2).

I start with an abelian variety with origin 0 and an irreducible subvariety S of A of dimension 2, passing through 0, non-singular at this point and having the following bizarre property:

(*) If C and C' are two irreducible curves passing through 0 contained in S, then the sum $C + C'$ (given by the composition law on A) is not contained in S.

Assume for the moment that such a pair (A, S) exists. Then I claim that S is the desired example. Indeed, suppose there were a dominating rational map $X \times X' \xrightarrow{f} S$, where X and X' are two non-singular curves; by a well-known theorem of Weil, f is in fact a morphism (since it takes values in an abelian variety). It follows that f is surjective, so there is a point (x, x') of $X \times X'$ which is mapped to 0. Let C be the image under f of the curve $X \times x'$, and define C' in the same way; by another theorem of Weil, the image of f is $C + C'$ (any morphism from a product of varieties to an abelian variety is a sum of morphisms); as the image is S, this contradicts (*) (since neither C nor C' can be reduced to a point, because $C + C'$ is of dimension 2).

It remains to find an example of a pair (A, S), and this is not much fun (even though *a priori* one expects any sufficiently "general" surface embedded in A to work). To simplify things, I will work over $\mathbf{C}$.

Lemma: Let $V = \mathbf{C}^2 \times \mathbf{C}^3 = \mathbf{C}^5$; consider the local analytic germ given by the equations:

$x_3 = x_1^2 + \varphi(x_1, x_2),$
$x_4 = x_1 x_2 + \chi(x_1, x_2),$
$x_5 = x_2^2 + \psi(x_1, x_2),$

where φ, χ, ψ are convergent series (or formal series if one wants to go beyond $\mathbf{C}$) beginning with terms of degree at least 3. Let S be the germ of surface thus defined. Consider two systems of 1-variable series $(x_i(t))$, $(x_i'(\theta))$, i.e. two germs of curves, passing through the origin at $t = 0$ (resp. $\theta = 0$), and contained

in S. Assume the point $x(t) + x'(\theta)$ belongs to S for any sufficiently small pair (t, θ). Then one of the systems $x(t)$, $x'(\theta)$ is identically zero.

This is just a stupid computation. Look at the first terms in the expansions of $x_1(t)$, $x_2(t)$, $x'_1(\theta)$, $x'_2(\theta)$, substitute $x(t) + x'(\theta)$ in the equations for S, and see that this cannot vanish. I am too lazy to copy it out; I suppose that you are convinced anyway.

Now take A to be a product $E \times F$, where E is a 2-dimensional abelian variety and F is a 3-dimensional abelian variety. From a "formal" (or even convergent!) point of view, it is possible to choose local coordinates such that the composition law on A is *addition*; moreover, it is trivial to construct a surface in A whose local equations are of the type in the lemma (they need only be given up to order 3 — it is not too hard). It is now clear that this germ (or more precisely, its Zariski closure) answers the question.

Yours,

J-P. Serre

April 1, 1964 Jean-Pierre Serre

Dear Grothendieck,

I have ended up by sending you the books by Hodge and Wallace by post. Give them back to me reasonably soon (in a month, say); in fact, why not have them bought by Motchane; it would be convenient to have them around in Bures (along with Weil's Variétés Kählériennes and Zariski's Algebraic Surfaces).

I would like to show you an example of *non* complete reducibility for the Galois group of a number field; I don't really know whether this example fits into your general yoga; you will see for yourself. J.-P. Serre : No, the example given here does not really fit into Grothendieck's general yoga, since it comes from a "mixed" motive, and not from a "pure" one.

Let A be an elliptic curve defined over a number field k, and let E_x be an extension of A by $\mathbf{G}_m$; it is known that such an extension is classified by an element of $A(k)$; I denote this element by x. Consider the Tate module $T_p(E_x)$; there is an exact sequence

$$0 \longrightarrow T_p(\mathbf{G}_m) \longrightarrow T_p(E_x) \longrightarrow T_p(A) \longrightarrow 0,$$

which is an exact sequence of modules over the Galois group $\mathfrak{g}$ of $\overline{k}/k$. If I have correctly understood your terminology, $T_p(\mathbf{G}_m)$ is a $\mathfrak{g}$-module of *weight* 2 (the eigenvalues of Frobenius have absolute value ℓ), and $T_p(A)$ is of *weight* 1. However, I can construct *an example where the preceding exact sequence does not split* (even up to isogeny, i.e. after tensoring with $\mathbf{Q}_p$). In Lie algebra terms, the Lie algebra of the group defined by $\mathfrak{g}$ is an algebra of dimension 6, explicitly:

$$\begin{pmatrix} a+d & e & f \\ 0 & a & b \\ 0 & c & d \end{pmatrix}$$

with $a, b, \ldots, f$ arbitrary.

Note that E_x can be considered as a generalized Jacobian (that of the curve A with respect to the "modulus" $\mathfrak{m} = P + Q$, where P and Q are two points such that $P - Q = x$); it follows that $T_p(E_x)$ is the first p-adic homology group of the affine curve $A - \{P\} - \{Q\}$. [I forgot to say what I had taken E_x to be: I take A to be an elliptic curve whose j-invariant is not p-integral, and I take x to be a point of $A(k)$ which is not of finite order.]

I have just received a note from Šafarevič; he cannot come to the Clermont conference. Not surprising!

Otherwise, my Izvestia paperJ.-P. Serre : "my Izvestia paper": this is a reference to [**Se64a**]. has appeared, without my even having seen the proofs!

Fortunately, it is not too badly printed, and at first glance I did not see many errors. Šafarevič tells me I will get 25 copies, which is not much.

Manin's paper on Mordell — function fields has also appeared. (On this subject, what should I do with Grauert's manuscript? For the moment, I do not intend to talk about it at Bourbaki; shouldn't it be sent back to him?)J.-P. Serre : It was Samuel who presented to the Bourbaki seminar the work of Manin and Grauert proving the Mordell conjectures for function fields (Séminaire Bourbaki 1964/65, no287).

Yours,

J-P. Serre

P.S. Good luck for your "second attack" on the Weil conjectures. I may have been a bit too pessimistic on the telephone; it is not entirely out of the question that it works. J.-P. Serre : This "second attack" on the Weil conjectures was quickly abandoned by Grothendieck, cf. the letter of April 2, 1964.

April 2, 1964 ALEXANDRE GROTHENDIECK

Dear Serre,

Thank you for your two letters and the books; you will explain your example of non complete reducibility to me when the opportunity arises. Can you construct an analogous example in characteristic $p > 0$? Probably not over a finite ground field, since unless I am mistaken that would contradict the Weil conjectures + complete reducibility in the projective non-singular case + resolution of singularities!

... I have convinced myself that my second approach to the Weil conjectures cannot work in its original form, i.e. that it would not suffice to use only the Riemann hypothesis for curves. One idea which is probably worth keeping, and which fits in very well with your yoga, is as follows. Let π be the fundamental group $\pi_1(\overline{X}, a)$, where X is a scheme of finite type over the finite field $k = \mathbf{F}_q$, and let a be an element of $X(k)$. (N.B. it is enough to assume that $X =$ a rational line minus a finite number of rational points). Consider the $\mathbb{Q}_\ell$-analytic quotient groups $G = \pi/R$, where R is invariant under the Frobenius ϕ, so that ϕ acts on G, and hence on its Lie algebra $\mathfrak{g}$. What can be said about the eigenvalues of ϕ on $\mathfrak{g}$? Ideally, they should be algebraic numbers whose absolute values are all of the form $q^{-\frac{i}{2}}$ where $i \in \mathbf{N}$. The Riemann hypothesis for curves says that this is the case when $\mathfrak{g}$ is abelian (and i is then 1 or 2); unless I am mistaken, it follows formally that the same holds when $\mathfrak{g}$ is solvable (and i is then ≥ 1). In the situation I am now reduced to, letting $\mathfrak{r}$ be the radical of $\mathfrak{g}$, it so happens that $\mathfrak{r}$ is abelian and ϕ, acting on $\mathfrak{g}/\mathfrak{r}$, has eigenvalues which are algebraic numbers of absolute value 1, and everything boils down to showing that the eigenvalues of ϕ acting on $\mathfrak{r}$ are algebraic numbers whose absolute values are of the form $q^{-\frac{i}{2}}$, where $i \geq 2$. Is this a general fact? I am not very optimistic, since it is highly likely that the quotients G coming from the cohomology of algebraic varieties satisfy rather subtle conditions which need to be taken into account. In any case, it is certainly true that (from the Galois point of view) the cohomology spaces of algebraic varieties are successive extensions of spaces which are (up to twisting by a suitable $\mathbb{Q}_\ell(N)$) tensor products of spaces such as $\mathfrak{r}$; one can even restrict to the case where π is the tame fundamental group, so as to obtain a group which is essentially free ...

By the way, have you ever investigated the general relation between the ℓ-adic Galois cohomology of a Galois-type group π and that of the projective system of its analytic quotients?

Regards,

A. Grothendieck

August 2-3, 1964 JEAN-PIERRE SERRE

Dear Grothendieck,

I am just back from the "Summer Institute" at Woods Hole, which was quite interesting. In order to clarify my ideas, I would like to tell you what went on there. Actually, you will soon receive the text of the main talks (in several copies, so that you can distribute them to your "stable"); however, none of the seminars has been written up, and they were the most interesting part.

1. A generalized Lefschetz formula.

You must already know this formula — at least partially. Shimura mentioned it as a conjecture at the beginning of the "Institute", and more or less everyone got down to it, J.-P. Serre : See Atiyah's comments in his *Coll. Works*, vol. III, no61, 62, 63.

The algebraic case (in arbitrary characteristic) was dealt with by P. Donovan: *The Lefschetz-Riemann-Roch formula*, Bull. S.M.F. **97** (1969), 257–273; see also SGA 5, exposé III (L. Illusie), Appendix. particularly Atiyah, Bott (who both lectured on it), Verdier and Mumford. At any rate, by the end of their stay they had proved the "elementary" version Shimura wanted, namely:

Let X be non-singular, projective and defined over k (of any characteristic — but I assume it is algebraically closed, since the question is "geometric"). Let F be a locally free sheaf on X (coherent, of course) and let $f : X \longrightarrow X$ be a morphism; also, assume there is a map of sheaves

$$f' \ : \ f^*F \longrightarrow F,$$

so that (f, f') can be made to act on $H^{\cdot}(X, F)$. Set:

$$\mathrm{Tr}^{\cdot}(f) \ = \ \Sigma_{q=0}^{\infty}(-1)^q \mathrm{Tr}^q(f) \ ,$$

where $\mathrm{Tr}^q(f)$ denotes the trace of the endomorphism on $H^q(X, F)$ defined by (f, f'). The aim is to compute the "Lefschetz number" $\mathrm{Tr}^{\cdot}(f)$.

For this, assume that the fixed points of f are *isolated*. The formula can then be written:

$$\mathrm{Tr}^{\cdot}(f) \ = \ \Sigma_{P \in S} \ L_P(f) \ ,$$

where the sum is taken over the set S of fixed points, and $L_P(f)$ is defined by a local formula which is rather complicated in general. The "elementary" case, which is Shimura's case, is the one in which the diagonal Δ cuts the graph of f *transversally*. In this case,

$$L_P(f) \ = \ t_P(f')/\det(1 - df_P) \ ,$$

where the symbols have the following meaning:

df_P = the tangent map to f at the fixed point P; 1 is not an eigenvalue of this linear map (by the transversality condition) and $\det(1 - df_P)$ is therefore indeed invertible.

$t_P(f')$ = the trace of the endomorphism of $F(P) = F_P/\mathfrak{m}_P F_P$ defined by f'.

The general case (non-transverse intersection) gives an expression for $L_P(f)$ (this is not guaranteed, as I am not sure that I really understand everything) connected to your higher residues. More precisely, let $x_1, \ldots, x_n$ be a system of parameters at P ("regular" parameters, of course), and set $y_i = x_i - f(x_i)$; the ideal (y) generated by the y_i is therefore $\mathfrak{m}_P$-primary. The formula J.-P. Serre : I wrote this formula incorrectly. Its correct form is given, and proved, in SGA 5, *loc cit.*, p. 133. is then

$$L_P(f) = \mathrm{Res}_P\Big(\frac{\mathrm{Tr}\ F/(y)\ dx_1 \wedge \cdots \wedge dx_n}{(y_1, \ldots, y_n)}\Big),$$

using notation that you probably understand better than I do.

In any case, this is not general enough, since there is surely a formula for $L_p(f)$ which is valid without the non-singularity condition, and for an arbitrary coherent sheaf. Perhaps it is already somewhere in your papers?

Atiyah gave an (algebraic) proof of the above result, using elementary facts (duality theory + some local Ext). For a compact complex analytic variety, Bott gave an analytic proof involving differential forms of the type introduced by Leray in his own residue theory. Atiyah and Bott have some ideas for a potential extension of this formula to more general elliptic systems than the wretched $\bar{d}$ operator.

Applications.

a) The case of a finite group acting on X with isolated fixed points: the results obtained were more or less known (to you and me at least) and were not very surprising.

b) The *Hermann-Weyl formula* giving the character of an irreducible representation of a semi-simple group (in characteristic zero).

This is surprisingly straightforward: Let G be semi-simple over $\mathbf{C}$ (for simplicity), let B be a Borel of G, let T be a maximal torus in B and let $X = G/B$ be identified with the Borel variety. Assume that a character

$$\lambda : B \to \mathbf{G}_m$$

is given; then one has a rank 1 bundle $F(\lambda)$ over X on which G acts. This yields an action of G on each $H^q(X, F(\lambda))$, and thence a trace:

$$\text{Tr}^{\cdot}(g) = \sum_{q=0}^{\infty} (-1)^q \text{Tr}^q(g),$$

defined in an obvious way. The H. Weyl formula boils down to giving $\text{Tr}^{\cdot}(g)$ explicitly. [One recovers the usual point of view upon assuming that λ is "positive"; J.-P. Serre : One should pay attention to sign conventions. See:

M.F.Atiyah and R.Bott, *A Lefschetz fixed point formula for elliptic complexes*: II. *Applications*, Ann. Math. **88** (1968), 451–491, §5 (= M.F.Atiyah, *Coll. Works*, vol.3, 129–169),

and, for the algebraic case:

J.C.Jantzen, *Representations of Algebraic Groups*, Acad. Press 1985, §II.5. then $H^q = 0$ for any $q \geq 1$, and H^0 is the irreducible representation of highest weight λ.]. It is enough to do this for $g \in T$, since every semi-simple element has a conjugate in T. One may even assume that g is regular in T, since these elements are dense. But then the action of the element g on the Borel variety X has only a finite number of fixed points, corresponding to elements $w \in W$, the Weyl group. Each of these fixed points is of the "elementary" type described above. The computation of the two terms $t_P(f')$ and $\det(1 - df_P)$ is trivial. Unless I am mistaken, one finds:

$$t_P(f') = w(\lambda)(g) \quad (\text{if } P \to w \quad - \text{ or } w^{-1}!)$$
$$\det(1 - df_P) = \prod_{\alpha > 0} (1 - w(\alpha)(g)) .$$

The desired formula follows:

$$\text{Tr}^{\cdot}(g) = \Sigma_{w \in W} \frac{w(\lambda)(g)}{\prod_{\alpha > 0}(1 - w(\alpha)(g))} ,$$

which is equivalent to the classical Weyl formula. (Note that I have used the multiplicative convention for weights and roots: in "Lie algebra" terms, one should replace my λ by e^{λ}, etc.) Isn't this magnificent?

c) Algebraic varieties having *rational points* over a finite field. Here *I assume* that $\text{Tr}^{\cdot}(f) = 0$ has already been proved for any projective variety X and any *fixed-point free* morphism $f : X \longrightarrow X$ (for the moment, this has been proved only for non-singular X). I assume that X is defined over $\mathbf{F}_q$ and has the following properties:

$H^0(X, \mathcal{O}_X)$ is 1-dimensional, $H^q(X, \mathcal{O}_X) = 0$ for $q \geq 1$.

Applying the formula $\mathrm{Tr}^{\cdot}(f) = 0$ to $f =$ Frobenius yields a contradiction.

COROLLARY: *Such a variety always has at least one $\mathbf{F}_q$-rational point.* (If one had a more precise formula, one could deduce that the number of such points is $\equiv 1 \bmod p$ — in any case, this holds if X is non-singular.) J.-P. Serre : "If one had a more precise formula, one could deduce that the number of such points is $\equiv 1 \bmod p$". This was proved by N. Katz without any smoothness condition, cf. SGA 7 II, exposé XXII, 410–411.

This statement is very reasonable. Indeed, if for example X is a complete intersection of hypersurfaces of degrees $m_1, \ldots, m_k$ in projective space $\mathbf{P}_r$, the triviality of the cohomology of X is equivalent to the condition $m_1 + \cdots + m_k < r + 1$, which is exactly what appears when writing that $\mathbf{F}_q$ is (C_1)! Note further that all known (non-singular) rational varieties satisfy the condition $H^q = 0$ — although no one can yet prove that this is always the case.

Playing the usual little game, J.-P. Serre : This "usual little game" consists of replacing "finite field" by "field of cohomological dimension 1". It is a dangerous game! For instance, the result conjectured here is false; it is easy to give counterexamples using fields of cohomological dimension 1 which are not C_1 (see the examples constructed by J. Ax and reproduced as exercises in *Cohomologie Galoisienne*, Chap.II, §3.2).

On replacing "cohomological dimension 1" by "C_1", the question becomes a bit more reasonable. This is actually the form in which it is mentioned in SGA 5, p. 134. An interesting special case (which is not yet settled) is that where $k = \mathbf{C}(T)$ and the variety is $\bar{k}$-rational, cf. J. Kollàr, *Rational Curves on Algebraic Varieties* (Ergebn. der Math. **32**, Springer-Verlag, 1996), §IV.6 (this reference was pointed out to me by J-L. Colliot-Thélène.) I am tempted to conjecture the following: Let k be a (perfect) field of cohomological dimension 1. If a projective variety X over k is such that $H^0(X, \mathcal{O}_X) = k$, $H^q(X, \mathcal{O}_X) = 0$ for $q \geq 1$, then X contains a k-rational point.

This is probably too optimistic; in any case, let us say that the above claim should hold for all fields which are "clearly" of dimension 1, such as the field of functions in one variable over an algebraically closed field. But even this case does not look easy. (Note for example that if this theorem could be applied to the Borel variety, it would yield my beloved "conjecture I", proved by Steinberg.)

The Lefschetz formula(s) in question have been christened the "Woods Hole fixed point formula". If you have any comments to make on them, I advise you to send photocopies to Atiyah and Bott, who are both at Harvard.

2. Ogg's results plus a beautiful conjecture on ℓ-adic cohomology.

Ogg is interested in elliptic curves defined over a local field k whose residue field k_0 is algebraically closed (for simplicity). Denote the residue characteristic by p, and consider prime numbers $\ell \neq p$ (déjà vu!). The points of order ℓ on E form a Galois module E_ℓ, and this Galois module has an invariant $\beta(E_\ell)$ — the one whose existence you once asked me to prove. One of Ogg's results is that — as you yourself conjectured — $\beta(E_\ell)$ *is independent of* ℓ.

This, plus other (partial) results of Ogg's, led Tate and myself to conjecture heaps of beautiful things:

With the same conditions on k, consider a non-singular projective variety V over k; let $\overline{V}$ denote the variety $V \otimes_k \overline{k}$, where $\overline{k}$ is the algebraic closure of k. Let $H_\ell^i(\overline{V})$ be the ℓ-adic cohomology of $\overline{V}$; it is a module over the Galois group $G = G(\overline{k}/k)$.

CONJECTURE: J.-P. Serre : This conjecture has not yet been proved, except for $i = 1$, in which case it follows from the existence of semi-stable models. *There exists an open normal subgroup U of G such that for any $g \in G$:*

(a) *The trace of g acting on $H_\ell^i(\overline{V})$ is an integer, denoted by $\mathrm{Tr}^i(g)$, which is independent of ℓ and depends only on the image of g in G/U.*

Note that this statement is trivial if V has "good reduction" V_0, since $H_\ell^i(\overline{V}) = H_\ell^i(\overline{V_0})$, on which the Galois group acts trivially.

Note further that (a) does not assert that the elements of U act trivially on $H_\ell^i(\overline{V})$ (there are simple examples which show that this is impossible); however, the elements of U give rise to automorphisms of $H_\ell^i(\overline{V})$ of which 1 is the only eigenvalue. It is clear that the conjecture could be rewritten saying that the ℓ-adic representation $H_\ell^i(\overline{V})$ "does not depend on ℓ" in a suitable sense (i.e. in a specially created Grothendieck group); I preferred to start by giving it to you in down-to-earth terms.

Ogg's results can be stated (and generalized) in the following way:

THEOREM: (i) *If the conjecture above is true for an abelian variety A, then the invariant $\beta(A_\ell)$ is independent of ℓ, and its value is given by* :

$$\beta(A_\ell) = \langle b_{G/U}, \mathrm{Tr}^1 \rangle,$$

where $b_{G/U} = $ Artin char. of $(G/U) - $ augm. char.(G/U)

(ii) *The conjecture above is true for an elliptic curve.*

The first part is easy. For the second part, consider two separate cases:

ii_a — The j-invariant of the elliptic curve is not integral. In this case, the curve becomes "of Tate type" (i.e. admits a description using theta functions) over a quadratic extension of k. The situation is so well under control that the result can simply be read off (cf. my Clermont lecture notes).

ii_b The j-invariant is integral. There is a finite Galois extension k'/k over which E has good reduction. Let U be the corresponding open subgroup of G. Letting E' denote the corresponding elliptic curve scheme over the ring $\mathfrak{o}$ of integers of k', the uniqueness of this scheme shows that it is stable under G/U; hence, G/U acts as a group of k_0 automorphisms of the reduced curve E'_0 (note that the residue field of k' is equal to that of k). Thus, there is a G/U-action on $H^1(\overline{V}) = H^1(E'_0)$, and Weil's theorems on abelian varieties then show that the conjecture holds. (Of course, the same argument can be applied to any abelian variety with good reduction over a finite extension of k.)

The proof Ogg gave for a special case of (ii) was substantially less straightforward; he had to examine the case $p = 2, 3$ in detail using the Kodaira-Néron classification. From the point of view taken here, these cases correspond to the case where $\mathrm{Aut}(E'_0)$ is very large (of order 24 at most). J.-P. Serre : "of order 24 at most". This only happens if $p = 2$ and the group $\mathrm{Aut}(E'_0)$ is isomorphic to $SL_2(\mathbf{F}_3)$.

3. Another beautiful conjecture.

Keep the same conditions and notation as in no2, but assume now that the residue field k_0 is a *finite field* $\mathbf{F}_q$. There is therefore an inertia subgroup I of the Galois group $G = G(\overline{k}/k)$ which is the kernel of the canonical homomorphism

$$G \to \widehat{\mathbf{Z}} = G(\overline{k}_0/k_0).$$

As previously, let V be a non-singular projective variety V over k, and consider the Galois module $H^i(\overline{V})$.

CONJECTURE: *There is a normal subgroup U in G, which is open in I, such that property* (a) *on page 5 holds for every $g \in G$ whose image under $G \to \widehat{\mathbf{Z}}$ is an integer ≥ 0.*

For the elements which map to 0 (i.e. which belong to I), this conjecture is equivalent to the one on page 5. Of course, the most interesting elements are *those that map to 1*; they deserve to be called "Frobenius". They are not well

defined, but there are only a finite number of them modulo U; thus one can talk about their *average*, and the characteristic polynomial of this average is well-defined. Denote it by $P_i(V, t)$. When V has "good reduction V_0", $P_i(V, t)$ is the characteristic polynomial of the Frobenius of V_0 acting on degree i cohomology. What has been done here, therefore, is to define a local "zeta function", as it were (or more exactly, the i-th component of the said zeta), even for bad reduction.

Example. The method used to prove the theorem on page 6 (elliptic curves) should also show that the conjecture above holds for an elliptic curve. I do not entirely guarantee this, however, since I have not done the computation in detail.

The point (for me) of the constructions and conjectures above is that they make it possible to define the *missing factors* J.-P. Serre : It took me several years to understand what the local (archimedean or ultrametric) factors of zeta functions should be, cf. [**Se69**]. of the zeta function. Let me explain myself: take a non-singular projective variety V over $\mathbf{Q}$. Over a non-empty open set U in $\mathrm{Spec}(\mathbf{Z})$, V can be replaced by a smooth projective scheme, giving a reasonable zeta function for each of the corresponding fibers V_p (and this function does not depend on the choice made, even if there are several non-isomorphic possibilities — this follows from Galois arguments). But one wants more: a reasonable definition for exceptional p, which should be such that the global zeta function thus obtained no longer needs anything except *factors at infinity* of the type $\prod \Gamma(\frac{s+m_\alpha}{2}) A^s$ in order to satisfy a perfectly clean functional equation, i.e. $f(s) = \pm f(k - s)$.

I hope the P_i I defined above give us such a "reasonable" definition. Unfortunately, there is little material to work with, basically just the elliptic curves dealt with by Deuring and Shimura; Shimura's case seems to work; I do not know Deuring's well enough to be able to figure it out, at least for the moment.

It is clear that the ultimate aim is to be able to *completely* write down the factors $\Phi_i(s)$ of the zeta function, with their exceptional terms and their terms at infinity; this no longer seems too far away. Hopefully, once this has been done, the result will suggest a proof. . . It will certainly be necessary to use cohomological adeles (i.e. integrate over both the space $H_\ell^i(\overline{V})$ and the cohomology at infinity). These questions cannot reasonably be attacked until the elementary ℓ-adic conjectures (i.e. the various positivity conjectures and the Weil conjectures) have been proved.

4. Yet more ℓ-adic cohomology.

Tate has written up a very beautiful talkJ.-P. Serre : Tate's talk appeared: *Algebraic cycles and poles of zeta functions*, in "Arithmetical Algebraic Geometry", Harper and Row, New York (1965), 93–110. (of more than 20 pages), which explains the state of the art, Weil-type conjectures and his own conjectures, and discusses a certain number of applications and examples. Here is a (non-trivial) consequence of his conjectures:

Let X be a regular scheme of finite type over $\mathbf{Z}$. Then:

The order of $\zeta(X, s)$ at the point dim $X - 1 = $ rk $H^0(X, \mathcal{O}_X^*) - $ rk $H^1(X, \mathcal{O}_X^*)$.

(Note that the two groups in question are *finitely generated* over $\mathbf{Z}$, and their rank is well-defined.)

There is a young Italian, Bombieri, who is working on zeta functions. He noticed all by himself that it was necessary to prove in all characteristics that the intersection form on "primitive" algebraic cycles of half dimension is definite; furthermore, he also apparently spotted the conjecture according to which the factors of an algebraic cycle in a "Künneth" decomposition are algebraic. By the way, what are you up to in these directions?

Here is an exercise (inspired by Bombieri): Let X be an n-dimensional projective, non-singular variety defined over a finite field. Assuming the Tate conjectures, prove that the rank of the group of n-dimensional algebraic cycles in $X \times X$ (modulo numerical equivalence) is $\geq \sum_{i=0}^{2n} B_i(X)$, where $B_i(X)$ is the i-th Betti number of X. Example: if X is an elliptic curve, $\sum B_i(X) = 4$, and one recovers the fact that X necessarily has complex multiplication.

5. One-parameter formal groups (Lubin).

Tate and I have organized a seminar on elliptic curves and formal groups; this started with a general talk, which I gave, essentially going over §2 (with hardly any extra details) from my Clermont lecture J.-P. Serre : "my Clermont lecture": [**Se66**]. (of which you surely have a copy, since it was printed by IHES). Lubin then gave three talks on formal groups. The most interesting part is the construction of (formal) *moduli*. J.-P. Serre : For more information on moduli of formal groups, see:

J. Lubin and J. Tate, *Formal moduli for one-parameter formal Lie groups*, Bull. S.M.F. **94** (1966), 49–60.

Let R be local, complete and Noetherian, with residue field k of characteristic $p \neq 0$, and let $F(x,y) = x + y + \cdots$ be a one-parameter formal group law *on the residue field k.* Assume that F is of height $h < \infty$; let me remind you that this means that the homothety of ratio p in the given formal group is of degree p^h, i.e. can be written

$$[p]_F(x) = u.x^{p^h} + \cdots, \quad u \in k^*.$$

Assume that a local Noetherian R-algebra R' is also given: consider the formal group laws F' on R' whose reduction modulo the maximal ideal of R' is F. Two such laws will be considered to be isomorphic if one can pass from one to the other via a change of variable $\varphi(x)$ such that the reduction of $\varphi(x)$ is $x \in k[[x]]$. This gives a set $T(R')$, which is functorial in R'. The theorem (if I have not screwed up by trying to make everything canonical) is that $T(R')$ is representable by a smooth formal scheme of dimension $h - 1$ over R, i.e. there exists a "universal lifting" of F to the local algebra

$$\mathcal{R} = R[[T_1, \ldots, T_{h-1}]]$$

where the T_i are indeterminates.

The nicest case is when k is complete;J.-P. Serre : "complete" should be "perfect". it is then useful to take R to be the ring of Witt vectors $W(k)$ over k; the algebra structure on R' then boils down to giving an injection of k into the residue field k' of R'.

Lubin did not give the proof in detail. He uses very explicit Lazard-style methods of climbing up the degrees of series (and also filtrations of local rings, of course). One key point is the following: if one defines in an obvious way the cohomology groups

$$H^q_{\text{form}}(F, \mathbf{G}_a) \qquad \text{(where F acts trivially on $\mathbf{G}_a$)},$$

one has $H^1_{\text{form}}(F, \mathbf{G}_a) = 0$, and $H^2_{\text{form}}(F, \mathbf{G}_a)$ is of dimension $h - 1$. I suppose you have some general stuff lying around which gives the theorem starting from these two relations.

The case $h = 2$ is particularly interesting. Indeed, F can be interpreted as the formal group of an elliptic curve (at least when the residue field k is algebraically closed); let E be this curve. As above, the formal moduli of F depend on "one parameter" since $h - 1 = 1$; on the other hand, those of an elliptic curve do the same. Here is a naive question: is there an isomorphism between these two moduli varieties (there is in any case an obvious arrow).

Answer: yes. It should also be possible to prove this via "general nonsense" by comparing the H^2; we did not do it this way, since in any case it follows from Tate's theory, which was the subject of the following seminars (see below).

An amusing question: with the assumptions from the beginning of this section, assume that R is a discrete valuation ring of characteristic 0. The Tate module $T_p(F')$ (where F' is a lifting of F to R) is well-defined; it is a free $\mathbf{Z}_p$-module of rank $h - 1$ J.-P. **Serre** : "of rank $h - 1$" should be "of rank h". on which the Galois group $\mathrm{Gal}(\overline{E}/E)$ acts, where $E = \mathrm{Frac}(R)$. The aim is to construct a formal group, of height 1, which would naturally be called $N(F')$, such that $T_p(N(F')) = \bigwedge^h T_p(F')$. The existence of $N(F')$ is obvious when $h = 1$; when $h = 2$ and k is algebraically closed it follows from the fact that by what is already known, F' comes from an elliptic curve. J.-P. **Serre** : $N(F')$ is known to exist, but only indirectly. This follows from a theorem due to Raynaud which says that the action of the inertia group on $\det T_p(F')$ is given by the cyclotomic character (M. Raynaud, *Schémas en groupes de type $(p, \ldots, p)$*), Bull. S.M.F. **102** (1974), 241–280, th.4.2.1.). The general case would be particularly interesting since it would give some information on the Galois group action on $T_p(F')$. If luck is with us, it should be possible to answer this question by *defining* exterior powers of formal commutative groups (first over a field, then over a ring) with the obvious base change properties. This would also be useful for Lubin-Tate style questions, where the local class fields are constructed by means of formal groups; it is clear that this is what is needed to prove the base change theorems (by this method).

6. Tate's funny cohomology. J.-P. **Serre** : Tate's "funny cohomology" was never published.

(A useful aid for the theory of formal moduli of abelian varieties.)

Let S be a scheme, X a group scheme over S, and B a commutative group scheme over S; assume that X acts on B in an obvious sense.

Tate then defines, by means of a rather complicated double complex, cohomology groups $H^q_{lt}(X, B)$, which are a mixture of group cohomology and Zariski cohomology (they are defined by Cech's methods, using open covers.) Denoting by $H^q_{\mathrm{reg}}(X, B)$ the groups defined in your seminar (dropping the Zariski part and keeping only the "group cohomology" part), then there are homomorphisms $H^q_{\mathrm{reg}} \to H^q_{\mathrm{lt}}$. In dimension 1, this is an isomorphism. In

dimension 2, there is an exact sequence

$$0 \to H^2_{\mathrm{reg}}(X, B) \to H^2_{\mathrm{lt}}(X, B) \to H^1(X, \underline{B}_X),$$

whose interpretation in terms of group extensions is as follows:

$H^2_{\mathrm{reg}}(X, B)$ describes the extensions of X by B which are trivial as fiber spaces,

$H^2_{\mathrm{lt}}(X, B)$ describes the extensions of X by B which are locally trivial (in the Zariski sense) as fiber spaces,

$H^1(X, \underline{B}_X)$ is the group of locally trivial principal bundles with structural group B and base space X.

Nobody has any doubt that it is also possible to define an H^2_{flat}, by replacing Zariski by fpqc, or God knows what. But as Tate's explicit formulas are rather ugly, one doesn't want to do it until one is forced to — unless you can see a more straightforward definition of these wretched cohomology groups.

7. Formal moduli of abelian varieties.

This was the subject of Tate's second talk (and mine, later). It is really very pretty.

i/ *Lifting of morphisms.* — Let R be a local Artinian ring with residue field $k = R/\mathfrak{m}$. Let I be an ideal contained in $\mathfrak{m}$ such that $\mathfrak{m}I = 0$. Set $R' = R/I$. The aim is to "lift" certain objects from R' to R.

Let B be a commutative group scheme (commutative is surely an unnecessary assumption, but it doesn't matter for the application to abelian varieties), smooth over R.

Let X be a group scheme over R.

Set $B' = B \otimes_R R'$, $X' = X \otimes_R R'$, $\widetilde{B} = B \otimes_R k$, etc. Let $t(\widetilde{B})$ be the tangent space to the origin in $\widetilde{B}$; the tensor product $t(\widetilde{B}) \otimes I$ is a finite-dimensional k-vector space. Denote the corresponding k-scheme by W (we have tried to use notation compatible with yours).

THEOREM: *There is an exact sequence:*

$$0 \to \mathrm{Hom}(\widetilde{X}, W) \to \mathrm{Hom}_R(X, B) \to \mathrm{Hom}_{R'}(X', B') \xrightarrow{\delta} H^2(\widetilde{X}, W).$$

[It should be mentioned that the Hom are "group" homomorphisms; the H^2 is the one defined in no6. J.-P. Serre : "the H^2 is as defined in no6": H^2_{lt}. Finally, the image of δ is contained in the symmetric part of H^2, i.e. in $\mathrm{Ext}^1(\widetilde{X}, W)$.]

Tate proves this theorem by taking the exact sequence of complexes

$$0 \to C^{\cdot}(\widetilde{X}, W) \to C^{\cdot}(X, B) \to C^{\cdot}(X', B') \to 0,$$

where $C^{\cdot}(\widetilde{X}, W)$ is the complex giving rise to the cohomology in no6. The fact that this sequence is indeed exact comes from the fact that the cochains can be defined over open *affine* covers. Passing to cohomology gives the desired result.

Of course, the interesting thing here is δ: the obstruction to lifting a homomorphism to another homomorphism lies in an $\mathrm{Ext}(\widetilde{X}, W)$. A geometric description of this group extension would be very useful: Tate's complex in no6 would then not be needed. There are also good reasons for examining the relation between this and the obstruction given by the Greenberg functor (assuming that k is perfect for simplicity); the general situation is not clear; when $I = \mathfrak{m}$, I am pretty much certainJ.-P. Serre : "I am pretty much certain": I am still just as certain, but have never written it up. that the Greenberg obstruction can be deduced from the other by applying a suitable power of Frobenius.

ii/ *Formal moduli of abelian varieties* — Keep the same conditions, but *assume now that k is of characteristic $p \neq 0$.*

Tate formulates the theorem as saying that there is an *equivalence of categories* $C_1 \rightarrow C_2$, where

$(C_1)=$ the categories of abelian schemes over R,

$(C_2) =$ the category of pairs (Φ, X), where Φ is an abelian variety over k and X is a lifting of Φ^* over R; I should say what A^* is when A is an abelian variety over R:

$$A^* = \lim_{n \to \infty} A_{p^n}, \quad \text{where } A_{p^n} = \mathrm{Ker}(p^n : A \to A).$$

Beware: the kernel A_{p^n} should be considered as a (finite, flat) *group scheme* over R. As for A^*, it should be considered as an Ind. object; a lifting of Φ^* is therefore equivalent to a sequence of liftings of the Φ_n, with suitable injections. In what follows, I will do as if A^* (or Φ^*) were a genuine group scheme — clearly this assumption cannot lead us too far astray(!).

The functor $C_1 \rightarrow C_2$ is clear; to any abelian variety A over R, it associates its reduction $\widetilde{A}$, which is an abelian variety over k; one takes $X = A^*$, which is indeed a lifting of Φ^*. The wonderful thing is that it is an *equivalence of categories*! J.-P. Serre : "equivalence of categories...". This is sometimes known as the "Serre-Tate theorem", even though my contribution was limited to ordinary abelian varieties. For a proof (different from Tate's), see V. G.

Drinfeld, *Revêtements de domaines symétriques p-adiques* (in Russian), Funk. Anal. i Prilozen. **10** (1976), 29–40; this proof is reproduced in: N. Katz, *Serre-Tate local moduli*, LN **868** (1981), 138–202. In other words, given the reduction $\widetilde{A}$ of an abelian variety A, the only extra thing needed to determine A is a *lifting* of the Ind. group $\widetilde{A}^*$, which is harmless enough (see below).

Proof of the theorem: This follows more or less formally (I am cheating a little) from the lemma:

LEMMA: *For every i,* $\mathrm{Ext}^i(\Phi, \mathbf{G}_a) = \mathrm{Ext}^i(\Phi^*, \mathbf{G}_a)$.

(In fact, these groups vanish for $i \neq 1$, and are k-vector spaces of dimension $\dim A$ for $i = 1$.)

Here is a (not entirely correct!) proof of the lemma: Φ/Φ^* is uniquely divisible by p, so all the Ext with $\mathbf{G}_a$ are trivial.

In fact, one has the feeling that the equivalence theorem should be *completely formal*, given the lemma. Furthermore, the fact that $\mathrm{Ext}^2(\Phi, \mathbf{G}_a)$ vanishes should give *the* right proof of the fact that Φ lifts to R.

iii/ *Application to the case where Φ does not have any point of order p. —* In this case, Φ^* is simply the *formal group* associated to Φ. Hence, *lifting Φ is equivalent to lifting its formal group*. In the case $\dim A = 1$, this is the theorem I alluded to on page 10.

iv/ *Application to the case where Φ has a maximal number of points of order p. —* (This is the case I had already dealt with by the Greenberg method. Tate's theory gives a new proof of this which is more satisfying from certain aspects.)

I will assume (this seems essential — and not just a consequence of my natural taste for Galois theory) that k is *perfect*.

Let $n = \dim \Phi$. The hypothesis on Φ can be translated as saying that Φ_p is a direct sum of an etale k-group of order p^n and an infinitesimal k-group; the former is a Galois twisted $(\mathbf{Z}/p\mathbf{Z})^n$, and the latter is a $(\mu_p)^n$ with an analogous twisting. More generally, there is a canonical decomposition:

$$\Phi^* = \Phi_m^* + \Phi_{\mathrm{et}}^*.$$

Now, it is clear that Φ_{et}^* has only one lifting to all R (Hensel!). The same holds for Φ_m^*, by Cartier duality, for example — or by Dieudonné-style results, I suppose. It is therefore immediate that *there is a canonical lifting of Φ^**, namely the direct sum of the liftings of Φ_m^* and Φ_{et}^*. These are my beloved canonical liftings, which I have often told you about. It is clear that one thus gets a *functor* from the category of Φ to the category C_1, which is the inverse of the reduction functor (warning: it is only defined on the Φ with the maximal number of points of order p). Passing to the limit over R gives something which is *a priori* a *formal scheme* canonically lifting Φ, but Mumford pointed

out to us that one can prove that it is actually an abelian variety. There would be a great deal to say about these canonical liftings (I gave a whole talk on them); I will come back to them a little later on.

I should say something about other liftings. Being lazy, I will assume that k is *algebraically closed*. It is (almost) obvious that any lifting A^* of Φ^* is an extension:

$$0 \to A^*_m \to A^* \to A^*_{\text{et}} \to 0,$$

where A^*_m and A^*_{et} are the canonical liftings over R of Φ^*_m and Φ^*_{et}. Assuming that k is algebraically closed allows me to identify these groups with the groups

$$(\text{Formal-}\,\mathbf{G}_m)^n, \text{ and } (\mathbf{Q}_p/\mathbf{Z}_p)^n,$$

these groups being taken over R in an obvious way. It is then an exercise to show that an R extension of $\mathbf{Q}_p/\mathbf{Z}_p$ by formal-$\mathbf{G}_m$ is characterized by an element of the group $R^*_1 = 1 + \mathfrak{m}(R)$, the multiplicative group of elements of R congruent to 1 modulo the maximal ideal $\mathfrak{m}(R)$.

Passing to the limit over R, one sees that this result still holds in the more general context of a complete local Noetherian ring with residue field k. Of course, in this context there no longer is any guarantee of ending up with actual abelian varieties, but one does in any case get formal schemes. Thus, the points of the *formal moduli variety* are systems of n^2 Einseinheiten; J.-P. Serre : "Einseinheit": element of $R^*_1 = 1 + \mathfrak{m}(R)$. moreover, it has a canonical group structure.

The abelian varieties whose moduli (in the sense given above) are *of finite order* deserve to be called *quasi-canonical*. When R is a discrete valuation ring, such a variety is isogenous to a canonical variety; it is not clear what happens in general.

Still assuming R to be a discrete valuation ring of characteristic zero, there is a simple characterization of quasi-canonical varieties: they are those for which the Tate module T_p, tensored with $\mathbf{Q}_p$, *splits* as a module over my beloved p-adic Lie algebra. This justifies th.1, p.9 of my Clermont lecture notes.

(Of course this is all still very woolly, and much cleaner statements could surely be made: I realize that I am handicapped by my poor knowledge of the basic generalities on group schemes... rejoice!)

v/*Canonical lifting of elliptic curves* — Consider the following problem: let k be perfect, and let E be elliptic with j-invariant $\in k$ and Hasse $\neq 0$J.-P. Serre : "Hasse $\neq 0$". This is a reference to the Hasse invariant of an elliptic curve — an invariant differential form of weight $p-1$. Saying that this invariant is $\neq 0$ means that that the curve is ordinary, i.e. has a point of order p. (i.e. having the maximal number of points of order p); by noiv/, there is a canonical lifting of E to the ring $W(k)$ of Witt vectors. The j of this lifting is thus a function

$$\theta : k-\mathrm{Ker}(\mathrm{Hasse}) \longrightarrow W(k).$$

How can θ be computed?

I almost know how to answer this question. More precisely, set:

a) $s=$ Frobenius acting on $W(k)$ $[= (x_0, x_1, \dots) \mapsto (x_0^p, x_1^p, \dots)]$.

b) $T_p(j, j') =$ the classical equation relating the modular invariants of two elliptic curves related by an isogeny of degree p. This is an equation with coefficients $\in \mathbf{Z}$, which is symmetric in j, j'.

Given this:

THEOREM: (i) *Let λ be in $k-\mathrm{Ker}(\mathrm{Hasse})$ and $x = \theta(\lambda) \in W(k)$. Then*

(∗) $T_p(x, s(x)) = 0$ *and* $x \equiv \lambda \mod p$.

(ii) If $\lambda \in k$ is not contained in $\mathbf{F}_{p^2}$, the system (∗) has one and only one solution.

(Combining (i) and (ii) thus shows that (∗) characterizes $x = \theta(\lambda)$, provided that $\lambda \notin \mathbf{F}_{p^2}$.)

To prove (i), start with the isogeny Frobenius : $E \to E^{(p)}$, and apply the "canonical lifting" functor. The canonical lifting of $E^{(p)}$ can be deduced from that of E by applying the automorphism s; its modular invariant $s(x)$ is thus related to the invariant x of the lifting of E by $T_p(x, s(x)) = 0$, which gives (i). The assertion (ii) can be proved by successive approximation in a standard way; the hypothesis $\lambda \notin \mathbf{F}_{p^2}$, which looks strange, is needed to ensure the non-vanishing of a certain partial derivative of T_p.

Just for fun, here is a numerical example J.-P. Serre : One may find other examples of canonical liftings in my Bourbaki talk 1966/67, no318.: for $p = 2$, $\lambda = 1$, the canonical lifting $\theta(\lambda)$ is equal to $-3^3 5^3$.

I believe this is more or less all I can tell you. Of course, there were several other seminars, but either I hardly followed them, or I am not competent to discuss them. For example: singularities (removal of — , variation of —): speakers, Zariski, Hironaka, Abhyankar; commutative algebra (factorial rings, differentials, by Samuel, Lichtenbaum and another guy from Harvard — I did not go), families of abelian varieties, moduli, class fields (by Kuga and Shimura — on a generalization of Shimura-Taniyama to families of abelian varieties with complex multiplication — it is technically rather complicated and it did not seem to me that anything really new was going on), etale cohomology of number fields, etc. (M. Artin and Verdier; I followed part of it, but you have known all this for ages); formal groups over a field (Barsotti; a not very understandable talk on something which is probably equivalent to results of Manin and Gabriel — still, it will one day be necessary to link this up with Lazard's "naive" point of view, and pass to rings).

Yours,

J-P. Serre

REMORSE. The hopes mentioned at the bottom of page 7 and at the top of page 8 (a "reasonable" definition of the missing factors of the zeta function) now seem to me to be somewhat exaggerated. In any case, peculiar things happen for the elliptic curve considered by ShimuraJ.-P. Serre : "... considered by Shimura". This is a reference to the modular curve $X_0(11)$, which is a remarkable example of the Eichler-Shimura-Igusa theory. This example helped me enormously to understand the structure of the L-function of a motive; I believe it helped Langlands in a similar way., and I don't understand the situation at all. J.-P. Serre : "I don't understand the situation at all..." See note 96.1.

(The case which worried me was that of an elliptic curve E over $\mathbf{Q}$ with bad reduction of multiplicative type at a prime number p. I was certain that the corresponding local factor was of the form $(1 - a \cdot p^{-s})^{-1}$. Should a be the eigenvalue of Frob_p acting on the subspace of $V_\ell E$, $\ell \neq p$, fixed by inertia at p, or acting on the corresponding ("invariant" or "co-invariant") quotient? The first choice yields $a = \pm p$ and the second $a = \pm 1$. It was a long time before I understood that the second choice is the right one.) Perhaps it will be necessary to alter these missing factors a little? Yet I am convinced that this is the right direction.

August 8, 1964 ALEXANDRE GROTHENDIECK

My dear Serre,

Thank you for your detailed report on Woods Hole, which I have just read over briefly. I have hardly any comments to make for the moment. Given my current concerns, I am mostly interested in n°s 2, 3, 4. (I suppose that on p.8, X should be assumed *proper* over Spec $\mathbf{Z}$?). I do not find n° 1 very exciting, despite the pretty applications; the fixed-point theorem itself seems to me nothing more than a variation on a well-worn theme! I did appreciate your diophantine conjecture on page 4, even though I do not know what the "usual little game" is.

I have nothing very interesting to tell you. I have been trying to learn Néron-style height theory, J.-P. Serre : "Néron-style height theory": A. Néron, *Quasi-fonctions et hauteurs sur les variétés abéliennes*, Ann. Math. **82** (1965), 249–331. and intend to try to generalize his local symbols and their interpretation as intersection multiplicities to cycles of arbitrary dimension. I checked the compatibility of the Néron-Tate form on the Jacobian with the intersection form on a non-singular projective surface fibered over a curve with irreducible geometric fibers, using local Néron theory (which Lang avoided in his talk) plus the theory of Picard schemes. Néron's results are terrific, and I believe they will turn out to be very important.

Regards,

A. Grothendieck

I have re-read your n°s 2 to 4, and would like some more enlightenment if possible, since I don't have the impression that I really understand the conjectures. Can you see a transcendental proof of the conjecture on page 5 when V comes from a projective morphism $X \longrightarrow Y$ with smooth generic fiber, with base a smooth algebraic curve over $\mathbf{C}$, by a base change of the form Spec $\widehat{\mathcal{O}}_{Y,y} \longrightarrow Y$? At the bottom of the page, what is "the specially created Grothendieck group"? On page 7 I do not understand what the averaging operation means, given that on page 5 you explain that the $g \in U$ do not act trivially in general! Indeed, the theory of "vanishing cycles" gives some typical examples (the operation of going around the singular fiber defines a transvection in the cohomology of a neighboring fiber which is of infinite order). If V is the generic fiber of a scheme over $Y = \mathrm{Spec}(A)$ which is smooth over Y except perhaps over at most a finite number of points, and possibly imposing some conditions on the nature of these points (non-degeneracy in Morse's sense of the word, for example) which I have no desire to go and fish out of my

notes from last spring, I think a generalized version of the theory of vanishing cycles makes it possible to determine the nature of the Galois operation in the critical dimension $H^n(\overline{V})$ (which would then imply the conjecture); however, the conditions mentioned above are probably very restrictive on V, since they imply for example that for $p \neq n = \dim V$, Galois acts trivially on $H^p(\overline{V})$, so there isn't much hope of a general proof by this route. It might perhaps be possible to get at least the abelian variety case by this method via Jacobians, since it is possible that if V is a curve, one can associate to it a Néron-type model X, projective over $Y = \mathrm{Spec}(A)$ (something much less precise should suffice...). This would at least get us a bit further than the sempiternal elliptic curves via Tate's sempiternal construction... The irritating thing is that one never seems to be able to get past abelian varieties!

I believe that n$^{\mathrm{os}}$ 5,... in your report will more or less form the contents of your course for next year? What is the "canonical group structure" on the formal moduli variety which you mentioned on p. 16?

August 8, 1964 Jean-Pierre Serre

Dear Ogg, J.-P. Serre : This letter to Ogg has been reproduced with his permission.

I have again been looking at your results on "good reduction" of elliptic curves (and abelian varieties, while we are about it). There is no doubt that they become simpler (although less precise) if one considers the whole of the Tate module, and not just its reduction modulo ℓ.

More precisely, let A be an abelian variety over a local field K with algebraically closed residue field k (perfect k can be dealt with in the same way — the case of general k worries me a little). Let ℓ be a prime number different from the characteristic of k, and as usual, let $T_\ell(A)$ be the Tate module of A with respect to ℓ; the Galois group $G(\overline{K}/K)$ acts on $T_\ell(A)$. The (T_ℓ-style) analog of your fundamental lemma is:

Theorem: J.-P. Serre : This theorem is known as the "Ogg-Néron-Shafarevich" criterion, and was published by Tate and myself in [**ST68**]. Grothendieck generalized it, cf. [**Gr66b**]. *For A to have good reduction, it is necessary and sufficient that $G(\overline{K}/K)$ act trivially on $T_\ell(A)$.*

(If k is only assumed to be perfect, one should say that the action of $G(\overline{K}/K)$ is "unramified", i.e. factors through the group $G(\overline{k}/k)$.)

We have to prove that if A has bad reduction, then $G(\overline{K}/K)$ acts non-trivially on $T_\ell(A)$, which boils down to saying that A has *not enough* K-rational points of order ℓ^n. Now, let R be the ring of integers of K, and let N be the Néron model attached to A; here *I assume* all the properties of N given in Néron's Bourbaki seminar (or in my Stockholm talk)J.-P. Serre : "Néron's Bourbaki seminar": Séminaire Bourbaki 1961/62, no227.
"my Stockholm talk": [**Se62b**]., plus the fact that N is quasi-projective over R, which is certain to follow from Néron's construction. Let $\widetilde{N}$ denote the reduction of N, i.e. the group scheme $N \otimes_R k$. Since ℓ is relatively prime to the characteristic of k, the K-rational points of order ℓ^n of A correspond (by reduction) to k-rational points of $\widetilde{N}$ of order ℓ^n. Assuming that $G(\overline{K}/K)$ acts trivially on $T_\ell(A)$, one thus obtains many such points, i.e. $\operatorname{rank} T_\ell(\widetilde{N}) = 2n$, where $n = \dim A = \dim \widetilde{N}$. But this implies that the connected component of $\widetilde{N}$ is an abelian variety (since any other commutative group has fewer points of finite order). Using the fact that N is quasi-projective and has connected generic fiber (namely A), it follows that N is proper (and indeed projective) over R, i.e. A has "good reduction".

COROLLARY: *For A to have good reduction after finite extension of the base field, it is necessary and sufficient for $G(\overline{K}/K)$ to act on $T_\ell(A)$ "via" a finite group.*

This is clear. Moreover, one sees that in this case there exists a smallest finite extension K'/K such that $A \otimes K'$ has good reduction; this extension is Galois over K; its Galois group G acts faithfully on the $T_\ell(A)$, and hence also on the abelian variety $\widetilde{A}$ obtained by reducing $A \otimes K'$. For elliptic curves this is the case where j is integral.

The nice thing about these results is that they more or less trivially imply Deuring's "subtle" results on elliptic curves with complex multiplication. Indeed, for such a curve, which is assumed to be defined over a number field, the action of the Galois group on the T_ℓ can be explicitly defined (as a function of the "Grössencharakter" which gives Frobenius). Using this, one can first prove that the action of the inertia group on T_ℓ is always "finite" (giving yet another proof of the fact that j is integral!) and then that this action is trivial if and only if the given place does not appear in the "conductor" of the Grössencharakter. This is very nice, especially since nothing stops us from doing all this for Shimura-Taniyama's abelian varieties of (CM) type. Thus, such an abelian variety has good reduction everywhere, J.-P. Serre : All this is explained in detail in [**ST68**]. after finite extension of the base field, and this should also give an explicit description of the set of places of a given base field at which the reduction is bad (as a function of the conductor of the Grössencharakter giving Frobenius). Even the first result (which corresponds to the fact that j is integral if dim = 1) was not known.

Let us return to the case of an abelian variety A defined over a local field K with algebraically closed residue field k. One needs to know a little more about the case of "bad reduction". Here is a "naive" question:J.-P. Serre : "Naive question". The issue at stake here is the existence of a semi-stable model (after finite extension of the base field), which was proved shortly afterwards by Mumford (when the residue characteristic $\neq 2$), and by Grothendieck (in the general case).

Does there always exist a *finite* extension K'/K, such that the reduction $\widetilde{N}'$ of the corresponding Néron model N' is an extension of an abelian variety by a torus (and a finite group, of course)?

This may be a stupid question. Do you see a counterexample?

I looked again at the question of "exceptional factors" of zeta functions of elliptic curves. I had already told you about Shimura's curve, where 11 is exceptional (Tate's case); we had seen together that the global zeta function obtained satisfied a clean functional equation (i.e. one having terms only "at infinity"); nevertheless, the fact that one does not get exactly the same function as Shimura bothers me, since the latter appears to be "the right one"; in particular, it is an entire function on the whole complex plane (unless I am mistaken), whereas the one I told you about is not. I do not understand what is going on, and yet I do have the impression the "Galois action on cohomology" point of view is the right one.

Best regards,

J-P. Serre

August 13, 1964 Jean-Pierre Serre

Dear Grothendieck,

Before answering your questions, I want to add a supplement to my "letter to Ogg" of which I recently sent you a copy. The "good reduction" criterion it contains (namely that the Galois group of the local field should have "unramified" action on some T_ℓ, where $\ell \neq$ the residue characteristic) trivially implies the following corollary (due to Shimura plus another Japanese, unless I am mistaken): J.-P. Serre : This is a reference to: S. Koizumi and G. Shimura, *On specializations of abelian varieties*, Sc. Papers Univ. Tokyo **9** (1959), 187–211.

Let A be an abelian variety defined over the local field K (with perfect residue field), with good reduction. Let B be an abelian variety over K equipped with a K-homomorphism $f : B \to A$ with finite kernel. Then B has good reduction over K. (For how else could Galois act on $T_\ell(B)$, given that it is embedded in $T_\ell(A)$?).

I believe I remember that you had a proof of this result in your language. Am I right, and what is it?

More generally, if V is a smooth projective algebraic scheme over the local field K, and the $G(\overline{K}/K)$-action on all the $H_\ell^i(\overline{V})$ is unramified, one can ask whether V does not necessarily have *good reduction*. This is probably a little too optimisticJ.-P. Serre : "A little too optimistic". Of course it is! A curve of genus 2 may have as bad reduction two elliptic curves intersecting in one point. Its Jacobian has good reduction, and the local Galois group acts without ramification on its cohomology., but nevertheless I cannot see any obvious counterexample. In the same vein, I have the impression that it should be possible to prove the *uniqueness* of a putative good reduction of V by imposing ingenious conditions on the cohomology groups $H_\ell^i(\overline{V})$: these groups should not be too trivial! (But I do not have anything precise to propose yet.)

Let me now try to answer your questions — in so far as it is possible, since the situation is far from clear, as you will see:

1) I have no "transcendental" proof of the conjecture on page 5 when V comes from a projective map $X \to Y$ with smooth generic fiber, and base a smooth curve over **C**. It would be necessary to prove that, denoting by s the automorphism of the cohomology of the generic fiber induced by the operation "go once around the hole", there is a power s^n of s which is unipotent (i.e. the eigenvalues of s are roots of unity). Of course, the rest of the conjecture (integrality of the traces and independence of ℓ) is obvious in that case, since

the cohomology over $\mathbf{Z}$ exists. But I really do not see how to prove that s^n is unipotent; this is really a very pretty property of algebraic bundles! There may be a Kähler trick that I have missed. (I have no doubt that the result is true.)

2) Don't worry too much about the "specially created Grothendieck group" at the bottom of page 5. I had simply yielded to the instinct "trace of a representation $\rightarrow$ Grothendieck group," but I would be hard put to tell you what Grothendieck group I meant!

3) On page 7, the operation "taking the average" does not have any intrinsic meaning for the Frobenius (and I did not actually assert that it did), but it does mean something for their characteristic polynomials, which allows me to define my $P_i(V, t)$. If you want an explicit formula, here is one:

$$-\frac{d}{dt}\log P_i(t) = \sum_{n=1}^{\infty} m_n t^{n-1},$$

where m_n is $\mathrm{Tr}(F^n)$, i.e. the average (over G/U) of the traces of the elements of G whose image under $G \rightarrow \widehat{\mathbf{Z}}$ is the integer n.

Moreover, this is a general procedure for defining L-functions: see for example Weil's paper on class field theory, in which he gives a general definition of L-functions (subsuming Artin's definitions and the Grössencharakter definitions).

My feelings about these "missing factors of zeta" given by the P_i are actually rather mixed. On the one hand, I am happy to finally have a general definition which is a little less stupid than the one consisting of simply counting points (and which above all depends only on the generic fiber); I have checked, for example, that for elliptic curves with complex multiplication (cf. Deuring), one does indeed recover exactly Deuring's conventions, i.e. one puts "1" at every place with bad reduction (I left this question hanging in my letter of August 2-3). On the other hand, there is the rather disagreeable example of an elliptic curve considered by Shimura-Eichler-Igusa, for which I do not obtain exactly what I should. It is very vexing.

(By the way, you say that in the context of "vanishing cycles", you can prove the conjecture given on page 5 by proving that "going around" gives a transvection — I believe this is actually one of Lefschetz's formulas? Can you also prove the conjecture on page 7 when the "hole" is over a finite field? I know this works for elliptic curves; in that case, Frobenius actually has two eigenvalues, 1 and q, and I would be curious to see what happens in general.)J.-P. Serre : Yet again my problems of local factors, cf. notes 96.1 and 104.2.

4) Yes, I do intend to lecture on 5, 6 and 7 in my course next year, maybe together with parts of 3 and 4. There is a rather large number of disparate things which all converge (towards elliptic curves, or more generally abelian varieties); for the moment I really do not see how this should be organised.

5) At the bottom of p.8, the consequence of the Tate conjectures which I quote *does not need* X to be proper over Spec $\mathbf{Z}$; this is one of its charms.

One of these days, you will have to explain to me what Néron's local symbols. I understood nothing of what Lang said about them — and neither did I understand Néron's paper, which I once had a look at. What an animal Néron is! Underneath the clumsy airs, everything he proves is fundamental! It is a shame he doesn't know how to present his work better.

On the subject of Néron's theory, I would very much like to understand what happens to his "minimal models" when the local field is enlarged. I would like (cf. my letter to Ogg) the situation to be as follows: the reduced group has a connected component which is stable (starting from some finite extension), which is the extension of an abelian variety by a torus T; the group of connected components itself, on the other hand, should increase with the extension, and finally give something like $(\mathbf{Q}/\mathbf{Z})^r$, where $r = \dim T$. At least, that is what happens in the elliptic case, and I am trying to extrapolate.

Yours,

J-P. Serre

P.S. I have just realized that I forgot to answer one of your questions, namely the one about the canonical group structure on the formal moduli variety considered on page 16: it is — if I may say so — the group structure on $\mathrm{Ext}(A_{\mathrm{et}}^*, A_m^*)$, with the notation from the bottom of page 15. When this Ext is made explicit using Einseinheiten, this simply gives the *product* of those Einseinheiten.

August 16, 1964 ALEXANDRE GROTHENDIECK

My dear Serre,

Thank you for the copy of your letter to Ogg, and your letter of the 13th, to whose questions I will now return.

1) "My" proof of the Shimura-Koizumi theorem (which is actually inspired by theirs). Let B be an abelian variety over K (the fraction field of a discrete valuation ring V), which has good reduction, i.e. comes from an abelian scheme $\overline{B}$ over V, and let A be an abelian variety over K which is isomorphic to a quotient (or to an abelian subvariety, which boils down to the same thing) of B, that is $A = B/N$; then A has good reduction. Indeed, let $\overline{N}$ be the scheme-theoretic closure of N in $\overline{B}$, namely the unique closed flat sub-prescheme of B whose general fiber is N (this is where dim 1 is used); then, since $\overline{B}$ is projective over V (another Japanese theorem, using Weil's ampleness criterion, and valid over a regular base), it follows by the theory of passage to the quotient (written up by Gabriel in the SGAD seminar, for example)J.-P. Serre : SGAD = SGA 3. that $\overline{A} = B/\overline{N}$ is representable by a projective scheme over V, and it is trivial that this is an abelian scheme extending A, qed. Of course, N is not in general smooth over K, i.e. it can have nilpotent elements; moreover, the Japanese did not have a good theory of passage to the quotient, and that is why they are forced to twist and turn every which way (I believe they construct an $\overline{A}$ by generalizing Weil's theorem on the definition of a group by birational data, rather like Mike's SGAD talk). J.-P. Serre : "Mike's SGAD talk": SGA 3, exposé XVIII. For more details on this delicate proof, see chapter 5 of *Néron Models*, by Bosch-Lütkebohmert-Raynaud, Springer-Verlag, 1990.

2) My allusions to "vanishing cycles" were indeed a little vague. To begin with, the only result which appears in the literature (which is probably proved in Igusa's secret papers) is in Igusa's note J.-P. Serre : "Igusa's note": J. Igusa, *Abstract vanishing cycle theory*, Proc. Japan Acad. Sci. **34** (1958), 589–593. in the Proceedings (if I remember rightly) which starts with a regular scheme X and a projective   morphism $f : X \to Y = \mathrm{Spec}(V)$ whose generic fiber is smooth and geometrically connected of dimension 1, and whose special fiber is geometrically integral and has only one singular point which is an ordinary double point. In this case, the Galois action is given by the Poincaré formula. I think it should be possible to analyse what happens for several double points (and perhaps for more complicated points?) and in higher dimensions, but I have not written up anything on this (it is in my short-term program, but has not

yet been done). Hopefully, the information obtained this way will be precise enough to make it possible to prove your conjecture with Tate, in the case of the Jacobian of a curve whose reduction is "not too bad". [10] To pass to arbitrary Jacobians, one would need to construct a "not too bad" model for an arbitrary non-singular projective curve over K, after finite extension of K if necessary. In a letter a few weeks ago, Mumford more or less said that given C, it is possible to find a model X whose special fiber has only ordinary singularities (if the genus is ≥ 2); in any case, he has apparently proved this for a residue field of characteristic 0. Once Jacobians are in the bag, the passage to arbitrary abelian varieties raises a question which I have actually already come across elsewhere, and which looks very interesting to me: does every abelian variety (over an algebraically closed field, say, that will suffice) have a "*finite* resolution" by Jacobians, at least up to isogeny?J.-P. Serre : "Does every abelian variety have a finite resolution by Jacobians, at least up to isogeny?". This is unlikely, but it does not seem easy to construct a counterexample. Alternatively, on forming a "K group" from abelian varieties up to isogeny (a free group generated by simple abelian varieties up to isogeny), is the subgroup generated by the Jacobians the whole group? It would actually suffice if this were true up to torsion, to be able to pass from the result for Jacobians to the case of general abelian varieties, if in your conjecture with Tate we settle for rational and not integral traces. In any case, the case of Jacobians would imply that for an arbitrary abelian variety, the traces you have in mind are algebraic integers — but I do not see how to get any further without using an auxiliary result of the kind mentioned above.

3) Moreover, this question is related to the following one, which is probably far out of reach.J.-P. Serre : "Moreover, this question is related. . .": As far as I know, this text is the first one in which the notion of a *motive* appears. I reproduced it in [**Se91**] along with extracts from *Récoltes et Semailles*. Let k be a field, which for the sake of argument is algebraically closed, and let $L(k)$ be the "K group" defined by schemes of finite type over k with relations coming from decomposition into pieces (the initial L is of course suggested by the link with L-functions). Let $M(k)$ be the "K group" defined by "motives" over k. I will say that something is a "motive" over k if it looks like the ℓ-adic cohomology group of an algebraic scheme over k, but is considered as being independent of ℓ, with its "integral structure", or let us say for the moment its "$\mathbf{Q}$" structure, coming from the theory of algebraic cycles. The sad truth is that

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[10]rather illegible note in the margin "N.B. I have not thought about the case where the residue field is finite."

for the moment I do not know how to define the abelian category of motives, even though I am beginning to have a rather precise yoga for this category, let us call it $\mathbf{M}(k)$. For example, for any prime $\ell \neq p$, there is an exact functor T_ℓ from $\mathbf{M}(k)$ into the category of finite-dimensional vector spaces over $\mathbf{Q}_\ell$ on which the pro-group $(\mathrm{Gal}(\overline{k_i}/k_i))_i$ acts, where k_i runs over subextensions of finite type of k and $\overline{k_i}$ is the algebraic closure of k_i in $\overline{k}$; this functor is faithful but not, of course, fully faithful. If k is of characteristic 0, there is also a functor T_∞ from $\mathbf{M}(k)$ into the category of finite-dimensional vector spaces over k (this is the "de Rham-Hodge functor", whereas T_ℓ is the "Tate functor"). In any case, taking for granted the two ingredients (Hodge and Künneth) of the Riemann-Weil hypothesis that you know about, I can explicitly construct (and indeed I can do this over more or less any base prescheme, not only over a field) the subcategory of *semi-simple* objects of $\mathbf{M}(k)$ (essentially as direct factors defined by classes of algebraic correspondences of some $H^i(X, \mathbf{Z}_\ell)$, where X is a non-singular projective variety). This is all that is needed to construct the group $M(k)$ (and I think it would be possible to give a description of it that would be independent of the conjectures mentioned above, if one wanted to). Hence, for any ℓ, there is a homomorphism from $M(k)$ to the "K group", namely $M_\ell(k)$, defined by the $\mathbf{Q}_\ell$-G-modules of finite type over $\mathbf{Q}_\ell$, where G is the pro-group defined above, or, if you prefer, the associated pro-Lie algebra (which has the advantage over the group of being a *strict* pro-object, i.e. with surjective transition morphisms). This being said, on taking alternating sums of cohomology with compact support, one obtains a natural homomorphism

$$L(k) \to M(k),$$

which is actually a ring homomorphism (with the Cartesian product on the left and the tensor product on the right). The general question which then arises is what can be said about this homomorphism; is it very far from being bijective? Note that the two sides of this homomophism are equipped with natural filtrations, via dimensions of preschemes, and the homomorphism is compatible with these filtrations. The above question on Jacobians can then be formulated as follows: is $L^{(1)} \to M^{(1)}$ surjective? (Indeed, up to a trivial factor of $\mathbf{Z}$ which comes from dimension 0, $M^{(1)}$ is nothing other than the K group defined by the abelian varieties defined over k).

I will not venture to make any general conjecture on the above homomorphism; I simply hope to arrive at an actual construction of the category of motives via this kind of heuristic considerations, and this seems to me to be an essential part of my "long run program". On the other hand, I have not

refrained from making a mass of other conjectures in order to help the yoga take shape; for example, that $M(k) \to M_\ell(k)$ is injective, or more precisely that two simple non-isomorphic (perhaps I should rather say non-isogenous) motives give rise to simple ℓ-adic components which are pairwise distinct. Tate's conjecture can be generalized by saying that for non-singular projective X, the "arithmetic" filtration on the $H^i(X)$ (via the dimension filtration on X) is determined by the filtration on $M(k)$ mentioned above, or alternatively that the filtration on $H^i(X, \mathbf{Z}_\ell)$ is determined by the Galois (or rather pro-Galois) module structure via the corresponding filtration on $M_\ell(k)$. For example, in odd dimension, the maximal filtered part of $H^{2i-1}(X, \mathbf{Z}_\ell(i))$ is also the largest "abelian part", and corresponds to the Tate module of the intermediate Jacobian $J^i(X)$ (defined by the cycles of codimension i on X which are algebraically equivalent to 0).

I should also mention that I do indeed have a construction of such intermediate Jacobians (whose dimension is bounded by $b_{2i-1}/2$ as it should be). Unfortunately, I do not yet even conjecturally understand the link between Hodge-style positivity and the Néron-Tate formula on self-duality of J^i for $\dim X = 2i - 1$, and I would like to discuss this with you some day before you leave. For surfaces, one does indeed get a proof of the Hodge index theorem using the Néron and Tate stuff, essentially by reducing the problem to the positivity of the self-duality of the Jacobian of a curve, and I continue to suspect that this principle of proof by reduction to dimension 1 is actually applicable to more general situations.

4) At the bottom of page 8, I think $\mathcal{O}_X$ should read $\mathbf{Z}_\ell$; it is because of this slip that I had the impression that you had forgotten a properness condition!

5) I have no feeling for your question on the variation of the Néron model under unbounded extensions of the base field. You should ask Néron if he knows anything.

6) The editors of the Bulletin are F. Browder, W. Rudin, E.H. Spanier, 190 Hope Street, Providence (Rhode Island).

Regards,

A. Grothendieck

August, 1964 Jean-Pierre Serre

Dear Grothendieck,

I will be coming back to Paris on Monday, August 31. I would like to come and chat to you one of these days. I propose the following: Wednesday September 2, after lunch (i.e. around 14h), at IHES. If you do not agree, send me a note at avenue Montespan (or call me: KLE 35 63 — I will be there at meal times in any case, and maybe also during the day).

I have received your long letter. Unfortunately I have few (or no) comments to make on the idea of a "motive" and the underlying metaphysics; roughly speaking, I think as you do that zeta functions (or cohomology with Galois action) reflect the scheme one is studying very faithfully. From there to precise conjectures. . .

At the bottom of page 8 of my infinite letter, $\mathcal{O}_X$ should read $\mathcal{O}_X^*$ (the sheaf of invertible elements of $\mathcal{O}_X$, i.e. $\mathbf{G}_m$ underlined as you alone know) and not $\mathbb{Z}_\ell$. This misunderstanding has been hanging around since my first letter! (Note the non-triviality of the rank of H^1: the rational points of a Picard plus Néron-Severi!)

I have written to Néron J.-P. Serre : Néron replied as follows on 28/08/64: ". . . I think, as you do, that the additive groups should disappear on extending the base field K — although I do not know how to prove it. . . ". to ask him if he had any feeling about what happens to his models under extension of the base field. No answer yet.

On the subject of "nice degeneration" of curves, abelian varieties etc.: it seems to me that this is a question for experts on moduli schemes and how to compactify them (such as Igusa or Mumford). There is something rather suggestive about the fact that in order to have moduli schemes in your sense, one needs to rigidify by the m-torsion points ($m \geq 3$), and this is probably also what is needed for the Galois action to be well-behaved. Let me explain myself; it seems reasonable to make the following conjecture: J.-P. Serre : "make the following conjecture". This conjecture is easy to prove, once the semi-stable reduction theorem is known, cf. SGA 7 I, p. 366.

Let k be a local field with algebraically closed residue field, and let A be an abelian variety defined over k. Let m be an integer which is relatively prime to the residue characteristic of k, and assume that the points of A of order m are k-rational (with $m \geq 3$). Then the action of $\mathrm{G}(\overline{k}/k)$ on $T_\ell(A)$ is

"unipotent" for any ℓ (i.e. the eigenvalues are all equal to 1). J.-P. Serre : "for any ℓ": "for any ℓ different from the characteristic of the residue field".

This statement holds in any case when A has good reduction over a finite extension of k (since $\mathrm{G}(\overline{k}/k)$ then acts via a group of automorphisms of a reduced abelian variety $\widetilde{A}$, and one can apply my brilliant theorem J.-P. Serre : "my brilliant theorem": an easy consequence of a theorem of Minkowski's (*Ges. Abh.* I, p.203 and p.213), which says that if an element x of finite order in $GL_n(\mathbf{Z})$ is such that $x \equiv 1 \pmod{N}$, where $N \geq 3$, then $x = 1$. See for example SGA 7 I, p. 367, lemme 4.7.11. on the automorphisms of such abelian varieties). You should be able to transform it into a statement on the fundamental group of the moduli variety of A (rigidified at order m) acting on the T_ℓ of the generic fiber, and this statement should be provable in the classical case by people such as Igusa or Borel, who will tell us that "at infinity, everything is parabolic". . . Wishful thinking!

I have no idea about the fact that every abelian variety should be a product of Jacobians up to isogeny (excuse me: a difference of products of Jacobians). I am somewhat skeptical, but I doubt that anyone is presently capable of settling the question.

A correction to my infinite letter, no3, Conjecture. I think "integer ≥ 0" should be replaced by "integer ≤ 0". The fact is that one needs to be careful with the definition of "Frobenius" acting on cohomology; there are various possible conventions, which differ by an exponent ± 1; for me, Frobenius is an element of the automorphism group of $\overline{k}/k$, but one needs to know how it acts on the scheme $V \otimes_k \overline{k}$ (by transport de structure or by the natural contravariant functor?), and thence on its cohomology (to which the same remarks apply). In short, there are disagreeable conventions to be specified, and it seems to me, alas, that the most natural ones lead to a $-$ sign. But you surely couldn't care less.

See you soon, then. Yours,

J-P. Serre

September 5, 1964 ALEXANDRE GROTHENDIECK

My dear Serre,

I wanted to let you know that my conjecture on the nature of your beloved ℓ-adic Lie algebras is more or less a consequence of the Tate conjectures. Indeed, let M be a motive, identified if you like with the ℓ-adic cohomology space of a smooth projective scheme over the base field K. The Galois group G acts on the ℓ-adic vector space M, hence also on the dual space M' (note also that if M is of weight p, then $M'(p)$ is isomorphic to M as a motive and thus a fortiori as a G-module), and hence on $\mathrm{Sym}(E)$ $[E = M' \otimes M]$. Thanks to Tate, the G-invariants in $\mathrm{Sym}(E)$ are the vector space generated by the classes of algebraic cycles. Since E and the $\mathrm{Sym}^n(E)$ are of weight 0, every semi-invariant under G is in fact invariant (at least *up to sign*;J.-P. Serre : "up to sign". Things are not so simple. It would be better to say "up to a finite group", i.e. after finite extension of the base field. A classical case is that of cubic surfaces in $\mathbf{P}_3$, where the finite group which appears can be equal to the Weyl group of E_6, whose order is 51,840. it may be necessary to pass to a quadratic extension of K), by the Weil conjectures. This being said, since every algebraic subgroup H of $\mathrm{GL}_{\mathbb{Q}_\ell}(M)$ is defined by the fact of giving certain semi-invariants, or more precisely can be described by some $\phi_i \in \mathrm{Sym}^{n_i}(M')$ as the set of $g \in \mathrm{GL}_{\mathbb{Q}_\ell}$ leaving the lines generated by the ϕ_i invariant, it follows that the *algebraic envelope H* J.-P. Serre : "algebraic envelope H". This is the first appearance of motivic Galois groups. of the image G_0 of G in $\mathrm{GL}_{\mathbb{Q}_\ell}$, can be defined in this manner, and by the remark I just made above, it is defined as consisting of all g which leave the ϕ_i themselves invariant. Equivalently, there is a finite set Φ of integers such that H consists of all the $g \in \mathrm{GL}_{\mathbb{Q}_\ell}$ such that g leaves all the elements of $\mathrm{Sym}^n(M')^G$ fixed for $n \in \Phi$. Since $\mathrm{Sym}^n(M')^G$ is generated by the algebraic cycles it contains, it follows that in the above, the ϕ_i can be taken to be classes of algebraic cycles. But then, it follows that H is described by "motivic" equations, i.e. with coefficients in algebraic cycles which are *independent* of ℓ. I think that starting from H, it should be possible to recover at the very least the semi-simple part of G_0 (if this has a precise meaning), by taking something like the commutator of H^0 (the connected component containing e); it is likely that a description of the center of G_0 would escape this method. Nevertheless, one can say that the center of H is nothing but the part of H invariant under G, or alternatively $H \cap \mathrm{GL}_{\mathbb{Q}_\ell}(M)^G$, i.e. the intersection of H with the $\mathbb{Q}_\ell$-algebraic part of $\mathrm{End}_{\mathbb{Q}_\ell}(M) = M' \otimes M$, and as a subset of the latter space it can be described as an algebraic group by equations whose coefficients are *rational and independent of ℓ*. It remains to

be seen what the trace of G_0 = center of G_0 looks like, and in particular its Lie algebra.

Of course, these developments can be translated directly into Lie algebra terms by replacing group representations by algebra representations and K-rational algebraic cycles by $\overline{K}$-rational algebraic cycles in the above. One thus finds that the algebraic envelope $\mathfrak{h}_0$ of your Lie algebra $\mathfrak{g}_0$ does indeed come from a motive which is independent of ℓ, so the same holds for its derived algebra, which is nothing but the derived algebra, i.e. the semi-simple part, of $\mathfrak{g}_0$. As for the center of $\mathfrak{g}_0$, it is contained in the center of $\mathfrak{h}_0$, which is itself defined by an abelian **Q**-Lie subalgebra of the **Q**-algebraic subspace of $M' \otimes M = \mathfrak{gl}(M)$, this space being independent of ℓ. The point which still has to be cleared up is whether the center of $\mathfrak{g}_0$ itself is defined by a **Q**-vector subspace of this type. In any case, from this point of view, the case of elliptic curves is extremely suggestive (and for me, convincing): one then finds, as luck has it, the arithmetic invariant controlling the situation, namely the *quadratic field* of complex multiplication (and not simply an insipid tensor product with $\mathbb{Q}_\ell$). Moreover, I have the impression that your grasp of the Tate conjectures should be good enough to investigate what happens for a product of elliptic curves.

The above considerations show fairly clearly how one can give a complete construction (or at least a construction of the part acting faithfully on the set of semi-simple motives) of the Lie proalgebra associated to K (or at least of its "algebraic envelope", and in particular its semi-simple part), in terms of the category of motives over K (equipped with the structures $\otimes$ and $\underline{\mathrm{Hom}}$, which moreover determine each other, and its functors T_ℓ to the category of finite dimensional vector spaces over $\mathbb{Q}_\ell$). Note that the "right" Lie proalgebra associated to an arbitrary field K (which is not necessarily of finite type over the prime field nor even necessarily a field ...) can be obtained by considering K as the inductive limit of its subrings of finite type over **Z**, of which one takes the projective system of π_1, followed by the projective system of their Lie proalgebras. Even if K is algebraically closed, one obtains a "Galois" Lie proalgebra which is not at all trivial: it is, for example, the one that appears naturally in the statement of the Lie algebra version of the Tate conjectures.

Yours,

A. Grothendieck

September 8 1964 Jean-Pierre Serre

Dear Grothendieck,

I am a little "confusé", as Lang would say. I do see that Tate's conjecture gives the algebraic envelope of the Galois groups (and the corresponding Lie algebra), but I do not see why the Lie algebras $\mathfrak{g}_\ell$ in question would form a *motive*. In fact, I have the impression I have a counterexample. You will tell me if I am talking rubbish:

Let E be an elliptic curve (of course!) defined over a *finite field k*. I assume we are in the "general" case (non-zero Hasse), i.e. its Frobenius π is such that none of its powers is a homothety. The ring of endomorphisms of E is then an imaginary quadratic field K, and π can be identified with an element of K. Passing to ℓ-adics, K defines a Lie algebra K_ℓ (acting on H^1, say); it is clear (!) that this is a motivated Lie algebra (i.e. coming from a motive), and that it is described by its invariants. The unfortunate thing is that the Lie algebra $\mathfrak{g}_\ell$ which interests us is nothing but a wretched 1-dimensional subalgebra of K_ℓ generated by $\log_\ell \pi^N$, where N is such that $\pi^N \in$ the domain of definition of the series $\log_\ell$. It should be noted that log must be taken in the ℓ-adic sense, i.e. is awfully transcendent. I am sure the $\mathfrak{g}_\ell$ do not form a motivated Lie algebra (they would have to be defined by a line in K, which is certainly not true) — and are not algebraic either.

The odd thing is that I haven't been up to constructing an analogous example over a number field. I would not be surprised if in this case the Galois groups were always algebraic and the $\mathfrak{g}_\ell$ always motivated — but this may be somewhat optimistic. I will have to investigate the Shimura-Taniyama case more carefully.

Note also that from this beautiful hypothesis plus your $\mathrm{Sym}(E)$ stuff, one can deduce the following conjecture (still over a number field):

the Lie-algebra $\mathfrak{g}_\ell$ always contains the homotheties. J.-P. Serre : The conjecture "$\mathfrak{g}_\ell$ contains the homotheties" was proved in 1980 by Bogomolov (C.R.A.S. **290**, 701–703).

I had asked this question in my Russian paper.

Yours,

J-P. Serre

September 9, 1964 ALEXANDRE GROTHENDIECK

My dear Serre,

Since my last letter I have also been giving some thought to the finite ground field case, which does seem to give rise to algebras which do not come from motives. You should look into this thoroughly. If this screws up, I do not see any other plausible yogic reason why the rank of the center of your $\mathfrak{g}_\ell$ should be independent of ℓ, in the general case. I believe only that it probably follows from Tate's conjectures that the center of the group G_0 of my letter is topologically generated by a family (independent of ℓ) of "algebraic" automorphisms of M, essentially the central components of the various Frobenius automorphisms. (Perhaps one only gets the center of G_0 up to a finite group, i.e. an open subgroup of this group.) But in general, given a family of automorphisms, is there any reason why the dimension of the group that it generates ℓ-adic-topologically should be independent of ℓ? As for your suggestion that maybe over a number field the $\mathfrak{g}_\ell$ do come from a motive, I have no feeling for it (except of course that it would be nice!). I am actually very annoyed not to have managed to produce any kind of yoga for number fields, i.e. essentially for unequal characteristics, notably in order to get hold of some geometric meaning for the various conjectures involving L-functions and such. To do this, it will probably be necessary to introduce the connected part of the "right" Galois group (which, by the principle of conservation of nuisance, seems to be the counterpart of the equally unexplored "infinitesimal part" of the fundamental group in characteristic $p > 0$).

I thought a little yesterday about Tate's conjectures on his functions φ_i, wanting to understand their link with his grand geometric conjecture. It is true that his conjectures 2 and 3 (from Woods Hole), for characteristic $p > 0$, are contained in his conjecture 1 (of course, I am freely interpreting his conjecture 3 by assuming that the system of representations of the fundamental group he starts with comes from a motive). To see this link, it is enough to re-interpret the function Φ_M defined by a motive M over the prescheme X of finite type over $\mathbf{F}_p$ as being the alternating product of the characteristic polynomials of Frobenius on the cohomology groups *with compact support* $H_!^i(\overline{X}, M)$, where $\overline{X}$ denotes the passage to the algebraic closure of $\mathbf{F}_p$; when X is smooth (which can be assumed for Tate's conjectures 2 and 3), one can use the duality theorem; one finds directly (and therefore essentially without using Tate but only Weil) that the order of the pole of Φ_M at the point $n = \dim(X)$ (the weight of M being 0) is nothing but the rank of the invariant module M^G.

Moreover, I think the "finite" expression in terms of cohomology with compact support given above can already be proved, using neither Tate nor Weil nor even resolution of singularities. In the case where X is not assumed to be of characteristic $p > 0$, one even finds similarly that Φ_M is the alternating product of the Φ_{M_i} relative to motives M_i over $\mathrm{Spec}(\mathbf{Z})$, namely the $R^i_! f(M)$, where $f : X \to \mathrm{Spec}(\mathbf{Z})$ is the canonical projection. In this way, Tate's conjectures 2 and 3 can be reduced to the case $X = \mathrm{Spec}(\mathbf{Z})$, and are equivalent to saying that if $M^G = 0$, then Φ_M is still holomorphic and invertible at the point $s = 1$ (M being assumed of weight 0). — I have also looked at how to interpret the Birch–Swinnerton-Dyer conjecture for X of characteristic $p > 0$; it so happens that (modulo Tate) it is exactly equivalent to the conjecture I mentioned to you on abelian schemes over a scheme of finite type X, namely that $H^1(X, A)(\ell)$ is finite; moreover, this fact itself should follow from the Tate conjectures.

Regards,

A. Grothendieck

September 24, 1964 ALEXANDRE GROTHENDIECK

My dear Serre,

I have just noticed that part of your conjectures with Tate can be proved almost trivially, namely, exactly the part that has no obvious "transcendental" explanation:J.-P. Serre : "your conjectures with Tate": those stated in S§2 and 3 of my letter of August 2-3, 1964.

THEOREM: *Let V be a discrete valuation ring with fraction field K, let ℓ be a prime number other than the residue characteristic, let E be a finite-dimensional vector space over $\mathbb{Q}_\ell$ on which $G = \mathrm{Gal}(\overline{K}/K)$ acts, let I be "the" inertia subgroup of G, and assume that:* a) *I acts on E via a pro-ℓ-group quotient of I, and* b) *There exists a subring A of K of finite type over* **Z**, *such that the action of G on E comes from the action of $G_0 = \pi_1(\mathrm{Spec}(A))$ on E via the natural homomorphism $G \to G_0$.*

Then the action of I on E is unipotent (i.e. the actions of the $g \in I$ on E are unipotent).

COROLLARY 1: *Suppose that instead of* a) *and* b), *one has the condition :*

b′) *E comes from a free module of finite type E_0 over $\mathbb{Z}_\ell$, on which a G_0 as above acts.*

Then there exists an open subgroup U of I whose action on E is unipotent.

Indeed, after taking a finite extension of K if necessary, the problem is reduced to the situation of the theorem.

Instead of proving the theorem as stated, I will restrict myself to proving a variant (the proof of the theorem is essentially identical, modulo the usual little technical exercises):

COROLLARY 2:*Assume that instead of* b) *we have the condition:*

b″) *The residue field of V is finite (assume also* a)).
Then the action of I on E is unipotent.

Indeed, V can obviously be assumed complete, and in this case the structure of the quotient of G by the smallest closed subgroup of I containing the Sylow p-subgroups of G is known: it is a semi-direct product $\widehat{\mathbf{Z}} \cdot \mathbb{Z}_\ell$, where the first factor (generated by the "Frobenius" f) acts on the second (generated by g) via the equation:

$$fgf^{-1} = g^q,$$

where q is the number of elements in k. (Of course, the second factor $\mathbb{Z}_\ell$ could be intrinsically written as $\mathbb{Z}_\ell(1) = T_\ell(\mathbf{G}_m)$; the choice of g is awfully

non-canonical.) Hence, the action of G on E is given by two automorphisms f, g of E satisfying the above relation. This implies that g and g^q are equivalent automorphisms, so the set of eigenvalues of g is stable under the operation $s \mapsto s^q$, and as it is finite, it follows that it consists of q^N-th roots of unity for a suitable N. Furthermore, it follows immediately from the continuity of $\mathbb{Z}_\ell \to \mathrm{Aut}(E)$ that these eigenvalues must be congruent to 1 modulo the maximal ideal of the ring of integers of the finite extension of $\mathbb{Q}_\ell$ containing the eigenvalues. Consequently, all these eigenvalues are equal to 1.

Of course, there is no hope of giving such a trivial proof of your integrality conjectures, which are fundamentally linked to the concept of *motive*; I have the feeling they should follow (like the "global" integrality theorems) from the conjectural "Künneth" formula for algebraic cycles.

These arguments by reduction to schemes of finite type in the absolute sense (of which the first example known to me is Lazard's) J.-P. Serre : "Lazard's": This is a reference to:

M. Lazard, *Sur la nilpotence de certains groupes algébriques*, C.R.A.S. **241** (1955), 1687–1689.

Lazard proves that an algebraic group whose underlying variety is isomorphic to an affine space Aff^n is nilpotent: he reduces the problem to the case where the base field is finite and of characteristic p, and notes that the group of rational points is nilpotent since it is a p-group. are very amusing. Here is another sample, which I am going to include in EGA IV 10 along with Jacobson preschemes (warn me if it is already known, so I can give credit where it is due): let X be finitely presented over a prescheme S, and let g be an endomorphism of X which is purely inseparable (resp. a monomorphism); g is then surjective (resp. an isomorphism). J.-P. Serre : Grothendieck included this result ("every injective endomorphism is bijective") in EGA IV 10.4.11.

I am stuck on the question we talked about on the telephone, of which here is a typical example: take an algebraic curve X over a finite field $\mathbf{F}_q$, and an abelian scheme A over X of relative dimension 1 ("a pencil of elliptic curves"); let E be $T_\ell(A)$. The question is to study $a_n = \chi(E^{\otimes n}) \in K(\mathbb{Q}_\ell[\mathbf{Z}])$ J.-P. Serre : Here $K(\mathbb{Q}_\ell[\mathbf{Z}])$ denotes the Grothendieck group of the category of finite dimensional $\mathbb{Q}_\ell$-vector spaces equipped with an automorphism, cf. h) of the letter of October 3-5, 1964. (also given by the Z-functions with coefficients in $E^{\otimes n}$) for various n, and show in particular that $\sum a_n s^n \in K[[s]]$ is a rational function. If you like, replace the $E^{\otimes n}$ by symmetric powers, or by the virtual sheaves deduced from E via the Adams operations, all of which should be

more or less the same. It seems very plausible to me that results of this type should hold for all *motives* over an X of finite type over $\mathbf{F}_p$, but on the other hand I have not yet managed to clarify whether all this really has anything to do with motives. One might think of trying to construct counterexamples using "very transcendental" representations of the fundamental group of X, but for the moment I have not managed to construct any which do not essentially arise from sheaves on $\mathbf{F}_p$ and for which there is no hitch; this is partly a consequence of the fact that (if X is absolutely irreducible over $\mathbf{F}_p$) the abelianization of the fundamental group of X has an ℓ-adic component isomorphic to the one corresponding to $\mathbf{F}_p$ under the canonical projection. In order to construct examples destined to be pathological, one is therefore forced to consider genuinely non-abelian representations, and to construct those, one would need some idea of the way Frobenius acts on the (*non-abelianized!*) fundamental group of $\overline{X}$. Do you have the slightest conjecture to propose in this direction? I don't.

I should also point out that there is an obvious dictionary which lets us interpret the various results on Z-functions in all genera (rationality, the Riemann hypothesis in the cases where one has it or wants to assume it) in terms of fundamental groups G of schemes of finite type, the system of "Frobenius elements" of G (classes of privileged elements of G modulo inner automorphisms which are in one-to-one correspondence with closed points of the prescheme used to define G) and the canonical homomorphism $G \to \pi_1(\mathbf{F}_p) = \widehat{\mathbf{Z}}$, as properties of more or less arbitrary ℓ-adic representations of such G. When X is a curve, one can even interpret the cohomology of X which arises in the expression of the Z-functions (defined at first as infinite products over Frobenius automorphisms) as the cohomology of the group G; the case where X is a Mike-style "elementary scheme" is much the same. Given this, I wonder whether group theory experts, such as Tate or yourself, might not be up to the effort of extracting more complete information from the theorems resp. conjectures which are already known, in order to get information on the Z-functions associated to tensor powers etc. of a fixed G-module, for example.

Returning to the Lefschetz formula for schemes of finite type: as I had pointed out to you, this can be considered as an identity in a group $K(\mathbf{Z})$, identifying a finite sum with an infinite sum, and because of this, it remains valid when taking into account the existence of a finite group of automorphisms of (X, F), and furthermore an arbitrary ring acting on F as endomorphisms. The question which then arises is whether the formula continues to hold when taking into account arbitrary endomorphisms of (X, F), or at least endomorphisms g which

act as proper endomorphisms on X. This is practically equivalent (at least for a coefficient field of characteristic 0, such as $\mathbb{Q}_\ell$) to the question [of knowing] whether the Lefschetz formula in terms of cohomology with compact support is true not only for f and its powers, but also for the products of the latter with g and its powers. This is certainly true if X is a curve, by the result of Verdier's we assumed. In the general case, the initial proof does not work as it stands, since it will not be possible to find successive *g-invariant* fibrations in order to reduce the problem to the case of a curve. My impression is rather that we should look for a purely geometric theorem of the following type: let X be of finite type over algebraically closed k, let F be a constructible $\mathbb{Q}_\ell$-sheaf on X, g a proper endomorphism of X, and $F \to f_*(F)$ a homomorphism, so that f acts on the $H^i_!(X, F)$. Assume that the Zariski tangent map to f vanishes at every rational point of X (which is a condition which is practically meaningless except in characteristic $p > 0$), which implies in particular that the fixed points of f, and thus also of its powers (which satisfy the same conditions as f), are isolated. This being given, one wants the naive Lefschetz formula to hold J.-P. Serre : For the Lefschetz formula, see SGA 5, exposés III and III.B. (in terms of traces; for coefficients of non-zero characteristic one also needs a Lefschetz formula in terms of characteristic polynomials). It is possible that in order to prove a theorem of this type and be able to reduce for example to the case of curves, it will be necessary to reformulate the general Lefschetz theorems in terms not of endomorphisms of a single X (which are then compared with the identity endomorphism), but in terms of the coincidence of two morphisms $X \rightrightarrows Y$ (in the case of non-singular varieties with constant coefficients, a variant of this type is probably more or less classical; in any case, I wrote one up in the old days).

Please do not forget to ask Verdier if he has received my letters, and to urge him to send me the end of the notes, and his comments on my remarks. I would also like to know if Mike has not dropped the (re-)writing of his talks at our seminar. Finally, you can tell Mumford that I have decided to include a certain number of supplements in EGA IV in a paragraph 20 onJ.-P. Serre : "paragraph 20": this is found in §21. invertible sheaves, divisors, linear systems J.-P. Serre : "linear systems". This classical notion is not easy to translate into the language of schemes; as far as I can see, it does not appear in the published chapters of the EGA's. of divisors, starting with the determination of the invertible sheaves on a projective bundle P on S ($\operatorname{Pic}(P) \cong \operatorname{Pic}(S) \times \mathbf{Z}$) and (as a corollary) the determination of the automorphisms of a projective bundle; we will probably also include the Auslander-Buchsbaum theorem: regular $\Rightarrow$

factorial. I had originally intended to include these kinds of sorites in Chap. III, but as the end of Chap. III does not seem very close to publication, unlike Chap. IV, I have changed my mind.

Regards,

A. Grothendieck

September 30, 1964 Jean-Pierre Serre

Dear Grothendieck,

Your theorem on the action of the inertia group is terrific — if you really have proved it. In fact, I cannot see how you generalized the argument which proves corollary 2; in the general case, you need Frobenius automorphisms which *normalize* the group I, and I really do not see how you go about it.

Note also that the theorem (and even corollary 2) are false[11] as stated; all that you prove (but it is quite sufficient to enchant me) is that there is an *open* subgroup (as in Corollary 1) where everything works; indeed, look at your proof of Corollary 2, you will see that the so-called q^N-th roots of unity you obtain are in fact $(q^N - 1)$-th roots of unity, and $q^N - 1$ can very well be divisible by ℓ.

I do not understand your question on the series $\sum a_n s^n$ (page 3 of your letter) because I do not understand what the ring $K(\mathbb{Q}_\ell[\mathbf{Z}])$ you mention is: could you be more explicit?

I have no idea how Frobenius acts on the fundamental group of a curve; J.-P. Serre : "in his Stockholm talk": see I.R. Shafarevich, *Collected Math. Papers*, Springer-Verlag, 1989, top of page 284. Šafarevič also mentioned this question in his Stockholm talk.

Nor do I have any ideas on the distribution of the Frobenius automorphisms or on Lefschetz formulas, except that the latter himself considered his fixed point theorem as a special case of the coincidence between two maps.

Local news: Atiyah-Bott are continuing in the direction of the Woods-Hole fixed point formula, from the "elliptic systems" side. I don't understand any of it, but it looks beautiful, and rather unrelated to fixed points; they think it should give a direct and elementary proof of Riemann-Roch, for example. In a word, they are creating a new "yoga" for this kind of question.

Mumford-Tate are looking at algebraic cycles on abelian varieties. They would like to finish off the Hodge conjecture (and the fact that every algebraic cycle is a linear combination of products of divisors) for abelian varieties; unless I am mistaken, they have managed to deal with a product of elliptic curves, and it does not look at all trivial. Just now, they told me about a (conjectural)

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[11] easy counterexamples

construction of the algebraic group over $\mathbf{Q}$ which in ℓ-adics should give the Galois group G_ℓ that I like. More precisely:

Let A be an abelian variety over $\mathbf{C}$; let V be its H^1 (with coefficients in $\mathbf{Q}$); the desired group will be an algebraic subgroup of $\mathrm{GL}(V)$ defined over $\mathbf{Q}$. More precisely, decompose $V \otimes \mathbf{C}$ into type $(1,0)$ + type $(0,1)$, and let T be the torus of dimension 2 consisting of homotheties λ on the first factor and μ on the second. This is an algebraic subgroup of $\mathrm{GL}(V \otimes \mathbf{C})$. The desired group G is by definition the smallest algebraic subgroup of $\mathrm{GL}(V)$ whose extension to $\mathbf{C}$ contains T J.-P. Serre : One recognizes the *Mumford-Tate group.* (our forefathers would say: the algebraic group generated by T and its conjugates over $\mathbf{Q}$).

Of course, for the moment they cannot see how to prove this, but it is nevertheless satisfying; it will also be necessary to check that this gives the right result in the few cases for which the question is settled. J.-P. Serre : "the few cases in which the question is settled". There were not many of them in 1964. I was starting to attack elliptic curves, but I did not have complete results except, of course, for the case of complex multiplication.

I have run all the errands you set me, with mixed results:

Tate says that he does not have much to say about heights (but Mumford has some original ideas on Weil "distributions" which he is supposed to explain to us in the seminar, one of these days). I do not know whether Tate will end up writing to you; if I understand correctly, it is as difficult for him as writing a paper!

Mumford is sending a copy of his lecture notes on surfaces to Gabriel, and is sending you 3 copies for your "stable".

Verdier has received your letters; I assume he will reply directly to you. I have not yet been able to explain to him in detail what you wanted from him (a Lefschetz formula for curves); I will see him again one of these days.

Mike Artin does not seem to be writing up at the moment. He says he needs Verdier's lectures to be written before he goes back to his own; my impression is that it will be some time before you get a draft from him.

I have not yet seen Schlessinger or Lichtenbaum.

Phew! I think that is all.

Yours,

J-P. Serre

P.S. It is my turn to set you a task: will you remember to write up a summary of the chapter on dimension for Bourbaki?

P-S.2. Samuel has written up categories: J.-P. Serre : "Samuel has written up categories". This is a reference to a Bourbaki report. 70 pages! But it is only a first draft, do not despair. . .

September 30, 1964 ALEXANDRE GROTHENDIECK

My dear Serre,

I have been thinking about functional equations of Z-functions in the geometric case (base field $\mathbf{F}_p$), and things seem simpler to me than you appeared to believe.J.-P. Serre : This letter, like the next one, contains hand-written corrections which are hard to decipher. The reader wishing to check the exactness of the formulas is invited to either redo the computations himself or refer to [**Gr64**], which is less detailed but clearer.

Let X be a scheme of finite type over $\mathbb{F}$, E a constructible sheaf of vector spaces over $\mathbb{Q}$ or a finite extension of $\mathbb{Q}$. Associate to E a sheaf, or rather a complex of sheaves (considered as an object of a derived category) $D(E)$ (which for the moment is defined at least for smooth or quasi-projective X). In the "good cases", $E \longmapsto D(E)$ is a perfect self-duality in the triangulated category constructed from constructible sheaves, and in particular, $D(E)$ is also constructible (i.e. the sheaves $H^i(D(E))$ are constructible, and all but a finite number of them vanish). For the moment, by "good cases" I mean those for which resolution of singularities is available for finite schemes over X equipped with divisors (this is trivial in dim ≤ 1, for example, and follows from Abhyankar in dim ≤ 2), or else restricting to smooth X with "twisted constant" sheaves on X, i.e. defined by continuous representations of the fundamental group of X. In any case, the global duality thoerem (when $D(E)$ is defined) gives a canonical isomorphism ($g: X \to \mathrm{Spec}(\mathbb{F})$ is projection)

$$(1) \qquad\qquad D(R_!g(E)) \simeq Rg_*(D(E))$$

where the D on the left-hand side is the straightforward duality of $\mathbb{Q}$-vector spaces equipped with an automorphism f (Frobenius). When X is proper over $\mathbb{F}$, one can also write $R_!g$ on the right-hand side, to obtain

$$(1 \text{ bis}) \qquad\qquad D(R_!g(E)) = R_!g(D(E));$$

but in general there are good reasons for writing (1) as a formula on virtual sheaves

$$(2) \qquad\qquad D(R_!g(E)) = R_!g(D(E)) + R_\infty g(D(E))$$

where from the virtual sheaves point of view one sets

$$(2 \text{ bis}) \qquad\qquad R_\infty g(F) = Rg_*(F) - R_!g(F)$$

which makes (2) tautological, but $R_\infty g$ can also be defined as a cohomological functor, or a functor of triangulated categories, by using a compactification $\overline{X}$

of X in the following way: set $Y = \overline{X} - X$, let $i : X \to \overline{X}$ and $j : Y \longrightarrow \overline{X}$ be the canonical immersions, and set

(3) $R_\infty g(F) = R\overline{g}_*(j^*(Ri_*(F)))$. J.-P. Serre : Here $\overline{g}$ denotes the projection $\overline{X} \to \mathrm{Spec}(\mathbf{F}_p)$.

The cohomological functor thus obtained does not depend on the choice of compactification, and plays the role of local cohomology at infinity; of course, the virtual formula (2) actually comes from an exact triangle linking the three complexes appearing in (2), which itself comes from the exact triangle linking $Rg_*, R_!g$ and $R_\infty g$ in general. Translating formulas (1 bis) and (2) in terms of characteristic polynomials, i.e. into Z-functions, yields functional equations:

(A) $$L_{D(E)}(t) = (-t)^{-\chi(E)}\delta(E)L_E(t^{-1}) \qquad (g \text{ proper})$$

(B) $$L_{D(E)}(t) = (-t)^{-\chi(E)}\delta(E)L_E(t^{-1})A_E(t) \qquad (g \text{ arbitrary})$$

where

(4) $$\chi(E) = \mathrm{rank}^* R_!g(E) = \sum_i (-1)^i \mathrm{rank}\, H_!^i(\overline{X}, \overline{E}),$$

(5) $$\delta(E) = \det{}^* R_!g(E) = \prod_i \det(f|H_!^i(\overline{X}, \overline{E}))^{(-1)^i},$$

and finally, the $A_E(t)$ in (B) is a "correction term" which can be explicitly written as an "L-function at infinity"

(6) $$A_E(t) = L_{D(E)}^\infty(t)^{-1},$$

where for all F on X,

(6 bis) $$L_F^\infty(t) = L_{R_\infty g(F)}(t) = \prod_i \det(1 - tf|H_\infty^i(\overline{X}, \overline{E}))^{(-1)}$$

This correction term J.-P. Serre : The exponent (-1) in the right-hand side of (6 bis) should probably be replaced by $(-1)^i$. is equal to 1 when X is proper. These formulas are valid in the case where $D(E)$ can be defined and is known to be constructible. Using (1), $\chi(E)$ and $\delta(E)$ can also be expressed by means of cohomology with *arbitrary* support on $D(E)$ via

(4 bis) $$\chi(E) = \mathrm{rank}^* Rg_*(D(E)) = \sum_i (-1)^i \mathrm{rank}\, H^i(\overline{X}, D(\overline{E})),$$

(5 bis) $$\delta(E) = \det{}^* Rg_*(D(E))^{-1} = \prod_i \det(f|H^i(\overline{X}, \overline{E}))^{(-1)^{i+1}}.$$

Note also the difference in nature between $\chi(E)$ and $\delta(E)$, the first being a geometric invariant, and the second being arithmetic.

Assuming the Weil conjectures and resolution of singularities, the role of (B) as an "approximate" functional equation becomes more precise upon noting that if E comes from an effective motive of weight $\leq \rho$, then $R_\infty g(D(E))$ also comes from a motive over $\mathbb{F}$, not generally pure of weight ρ even if E is, nor even of weight $\leq \rho$, but of "level" $\leq \rho - 1$, which implies that the zeros and poles of the correcting L-function $A(t)$ have inverses which are multiples of integer powers of p by algebraic integers whose absolute value is of the form $p^{i/2}$, where i is integral and $\leq \rho - 1$.

The functional equations (A) and (B) link the L-functions associated to two different sheaves, E and $D(E)$. In certain cases, $D(E)$ can be expressed simply in terms of E, yielding a functional equation of L_E alone. Assume for example that X is smooth and everywhere of dimension n, and that E is "twisted constant"; then (by definition)

$$(7) \qquad\qquad D(E) = \check{\mathbb{E}}(n)[2n],$$

where (n) means tensoring by the Tate sheaf $\mathbb{Q}(n)$, $[m]$ means translation of degrees by m, and finally, $\check{\ }$ denotes the dual sheaf in the naive sense. It follows that

$$(7\text{ bis}) \qquad\qquad L_{D(E)}(t) = L_{\check{\mathbb{E}}}(p^{-n}t),$$

which makes it possible to write the functional equations (A) and (B) as expressing $L_{\check{\mathbb{E}}}$ in terms of L_E. One can go farther by making the additional assumption that there is an isomorphism

$$(8) \qquad\qquad \mathbb{E} \simeq \check{E}(\rho), \quad \text{whence} \quad D(E) = E(\rho + n)[2n]$$

where ρ is an integer (which in the motivic case is morally the "weight" of E). Then one can also write

$$(7\text{ ter}) \qquad\qquad L_{D(E)}(t) = L_E(q^{-1}t), \quad \text{where } q = p^{\rho + n}.$$

The functional equations (A) and (B) are then of the form

$$(B') \qquad\qquad L_E(1/qt) = A_E(t)(-t)^{\chi(E)}\delta(E)L_E(t),$$

where the three extra factors on the right-hand side are as given in (4), (5), (6), and the correction factor $A_E(t)$ is 1 if X is proper (i.e. one then has a simple equation (A$'$) which I will not copy down). Moreover, bearing (8) in

mind, $A_E(t)$ can also be written explicitly in terms of $L_E^\infty(t)$ instead of $L_{D(E)}^\infty(t)$,

$$(6 \text{ ter}) \qquad\qquad A_E(t) = L_E^\infty(1/qt)^{-1},$$

and similarly, (4 bis) and (5 bis) can be simplified to give

$$(4 \text{ ter}) \qquad\qquad \chi(E) = \sum_i (-1)^i \operatorname{rank} H^i(\overline{X}, \overline{E}),$$

$$(5 \text{ ter}) \qquad \delta(E) = q^{\chi(E)} \delta(Rg_*(E))^{-1} = q^{\chi(E)} \prod_i \det(f | H^i(\overline{X}, \overline{E}))^{(-1)^{i+1}}.$$

When X is moreover proper, a comparison of (5) and (5 ter) actually shows that

$$(9) \qquad\qquad \delta(E)^2 = q^{\chi(E)} \quad i.e. \quad \delta(E) = \pm q^{\chi(E)/2}.$$

(N.B. this equality comes out without using any conjecture such as Weil); thus, in this case, knowing $\chi(E)$ determines $\delta(E)$ up to sign. One can specify the sign $\pm$ in (9) a little by proceeding as in your courseJ.-P. Serre : "as in your course". This is a reference to the course of 1960/61, in which I had given the formal properties of L-functions, cf. [**Se61b**].; also, when X is not necessarily proper, but if one assumes the Weil conjectures and resolution of singularities, so as to be able to give a virtual decomposition of $R_! g(E)$ as a sum of pure components, one can give $\chi(E)$ and $\delta(E)$ explicitly in terms of these virtual components.

Now assume that X is a projective smooth curve, which for the sake of argument is connected, i.e. irreducible (but not necessarily geometrically irreducible), with fraction field $K = k(\eta)$. Assume that

$$(10) \qquad\qquad E = i_{\eta*}(M),$$

where M is a constructible sheaf over $\eta = \operatorname{Spec}(K)$ (the generic point of X) and $i_\eta : \eta \to X$ is the canonical injection. (Beware of the fact that the right-hand side of (10) really does say $i_{\eta*}$ and not $Ri_{\eta*}$.) It comes to the same to say that if U is an non-empty open subset of X over which E is "twisted constant", or, as we will say, unramified, then E is nothing else than $i_*(E_{|U})$, where $i : U \to X$ is the canonical injection. Note that one then has

$$(10 \text{ bis}) \qquad\qquad \mathbb{E} = i_{\eta*}(\check{\mathrm{M}}),$$

which is trivial, and moreover

$$(11) \qquad\qquad D(E) = \mathbb{E}(1),$$

which is essentially the local duality theorem on X, already contained in Ogg's thesis. Thus, even though E is not twisted constant, one still obtains a simple functional equation linking L_E and $L_\mathbb{E}$ directly, namely:

$$(C) \qquad L_\mathbb{E}(1/pt) = (-t)^{\chi(E)} \delta(E) L_E(t).$$

When M satisfies

$$(12) \qquad \check{M} = M(\rho), \quad \text{whence} \quad D(E) = \mathbb{E}(\rho + 1),$$

(morally M is a motive of weight ρ), (C) takes the form

$$(C') \qquad L_E(1/qt) = (-t)^{\chi(E)} \delta(E) L_E(t), \quad \text{with } q = p^{1+\rho}.$$

In these formulas, $\chi(E)$ and $\delta(E)$ are again as given by (4) and (5) and their variants bis and ter; in particular, one has formula (9), which determines $\delta(E)$ in terms of $\chi(E)$ up to sign.

Let us write down the global form of L_E explicitly:

$$(13) \qquad L_E(t) = \frac{P_E^1(t)}{P_E^0(t) P_E^2(t)}$$

where

$$P_E^i(t) = \det(1 - tf | H^i(\overline{X}, \overline{E}));$$

condition (12) actually implies the following partial functional equations which make (C′) more precise:

$$(C'') \qquad \begin{cases} P_E^2(t)(1/qt) = (-t)^{-b^0(E)} \delta^0(E)^{-1} P_E^0(t), \\ P_E^1(t)(1/qt) = (-t)^{-b^1(E)} \delta^1(E)^{-1} P_E^1(t). \end{cases}$$

Let us now work in the framework of motives, assuming resolution of singularities and the Weil conjectures. Assume that E comes from a motive M over K, purely of weight ρ (which implies (12) when M is a semi-simple motive; this condition can even be dropped when working with virtual sheaves). One can then check that the inverses of the eigenvalues of the Frobenius action on H^0, H^1, H^2 are respectively of absolute value $p^{\rho/2}, p^{(\rho+1)/2}, p^{(\rho+2)/2}$, and moreover those appearing in H^0 and H^2 come in pairs whose product is $q = p^{\rho+1}$; finally, if M is an effective motive, so that $\rho \geq 0$, H^2 (while being of weight $\rho + 2$) is of *level* $\leq \rho$, and unless I am mistaken there should even be a Lefschetz-type isomorphism (which does not depend on the hypothetical theory of motives) $H^2 = H^0(-1)$, whence

$$(14) \qquad P_E^2(t) = P_E^0(pt).$$

In particular, the zeros of L_E are precisely the eigenvalues of Frobenius on H^1 and have inverse absolute value $p^{(\rho+1)/2} = q^{1/2}$, the poles are exactly the eigenvalues of Frobenius on H^0 (for those of inverse absolute value $p^{\rho/2}$)) and H^2 (for those of inverse absolute value $p \cdot p^{\rho/2}$). When M is an effective motive of weight 0, i.e. essentially for classical L-functions associated to finite groups, these results are valid without any conjectures thanks to the Riemann hypothesis for curves. For an effective motive of weight 1, possibly twisted by $\mathbb{Q}(1)$, i.e. associated to an abelian variety A_η over K, say

$$(15) \qquad\qquad M = T_\ell(A_\eta)$$

(for this choice, therefore, $\rho = -1$), the validity of the above results for absolute values is equivalent to the Weil conjectures for surfaces, given that resolution exists for surfaces. (N.B. One may obviously always assume that A is a Jacobian, which reduces the problem to surface theory.)

To finish up with the good properties of L_E in this situation, let us specify what the local factors

$$L_E^x(t) = \frac{1}{P_E^x(t)} = \frac{1}{\det(1 - t^{d(x)} f_x | E_x)},$$

look like, where f_x is the Frobenius relative to x (morally, the $d(x)$-th power of the absolute Frobenius f), and $d(x) = $ the degree of $k(x)$ over $\mathbb{F}$. For x "unramified" for M, the corresponding local factor is purely of weight ρ; for x "ramified" for M, the corresponding local factor is of weight $\leq \rho$, but is not in general purely of weight ρ. In the case (15), the local factors can be given especially nicely in terms of the Néron model A of A_η; ; indeed (by the very definition of the Néron model via its axiomatic properties) one has

$$(16) \qquad\qquad E = T_\ell(A)$$

whence

$$(16 \text{ bis}) \qquad\qquad E_x = T_\ell(A_x)$$

where A_x is the fiber of the Néron model A at x. (N.B. In formulas (15) and (16 bis), I forgot to tensor the right-hand side by $\mathbb{Q}\ldots$). Thus, the local factor $P_E^x(t)$ splits into a product of two factors at x, respectively of weight $\rho - 1 = -2$ and $\rho = -1$, namely essentially the characteristic polynomials of f_x on $T_\ell(A_x^{\mathrm{mult}})$ and $T_\ell(A_x^{\mathrm{abel}})$, where A_x^{mult} and A_x^{abel} are respectively the multiplicative and abelian parts of A_x (whose formal definition is obvious, given that $k(x)$ is finite and hence perfect).

At first glance, it would seem from these considerations that if the aim is to define an L-function for a sheaf M over $\eta = \mathrm{Spec}(K)$ coming from some E, the right definition would be to take $L_M = L_E$. This gives results which are perfect from the point of view of functional equations, distribution of zeros and poles and relationship with the Néron model. In fact, I forgot to mention in the above the attractive birational interpretation of the multiplicity of the pole at 1 (or p) of L_E when M is a (not necessarily effective) semi-simple motive of weight 0; it is the dimension of $H^0(K, M) = M^\pi$ (where M is considered as a module over $\pi = \mathrm{Gal}(\overline{K}/K)$). I can only see one flaw in this definition of L_M, namely that L_M is not multiplicative with respect to exact sequences in M (and probably, when finite groups G of automorphisms are also introduced in order to define functions $L_{M,\phi}$ with respect to class functions ϕ on G, the variance of the functions $L_{M,\phi}$ is given by formulas that are not as simple as one would like; I have not looked into this question). If one insists on having an L-function which depends multiplicatively on M, it seems to me beyond doubt that the definition you recommend is the best one. From the point of view adopted here, it consists in considering the L-function with coefficients not in the E defined in (10), but with values in a slightly larger sheaf

(10 bis) $$E^\natural = E + \sum_x i_{x*}(\alpha_x(M)),$$

where the sum is taken over an arbitrary set of closed points $x \in X$ containing all the ramification points of M; for a closed point $x \in X$, $\alpha_x(M)$ is a constructible sheaf on $k(x)$, of rank at most $r - \mathrm{rang}\, E_x$ (where r is the rank of M), and hence zero if x is unramified for E; its explicit definition is as follows: consider M as a module over $\pi = \mathrm{Gal}(\overline{K}/K)$, let D_x, I_x, be a decomposition (inertia) group of x in π, with $I_x \subset D_x$; then there is a canonical isomorphism

$$D_x/I_x = \mathrm{Gal}(\overline{k(x)}/k(x)) = \widehat{\mathbf{Z}}.$$

Then one knows (cf. my last letter) that there exists an open subgroup U in I_x, invariant in D_x, which has a unipotent action on M, and one can filter M by the subspaces $M_{x,0} = M^U$, $M_{x,1} =$ inverse image of $(M/M_{x,1})^U$ in M, etc. Then the $\mathrm{gr}_{x,i}(M)$, $i \geq 0$ are modules over D_x/U, and hence the $\mathrm{gr}_{x,i}(M)^{I_x/U}$ are modules over D_x/I_x, so they can be considered as sheaves on $k(x)$. Actually, $\mathrm{gr}_{x,0}(M)^{I_x} = M^{I_x} = E_x$ is nothing but the fiber of E at x. Given this, we set as you do

(17) $$\alpha_x(M) = \sum_{i \geq 1} \mathrm{gr}_{x,i}(M)^{I_x}$$

and

(18) $\qquad L_E^{\natural}(t) = L_{E^{\natural}}(t) = L_E(t) \prod_x L_{\alpha_x(M)} = L_E(t) / \prod_x P_{\alpha_x(M)}(t^{d(x)}),$

where $P_{\alpha_x(M)}(t) = \det(1 - tf_x|\alpha_x(M))$ is the characteristic polynomial of f_x in $\alpha_x(M)$.

In the framework of motives, one can specify, at least conjecturally, the structure of the supplementary factors $\frac{1}{P_{\alpha_x(M)}(t)}$ that you want to add to L_E, and which a priori might add some poles (or destroy some zeros). Still assuming that M comes from a pure motive of weight ρ, it should be true that all the $\mathrm{gr}_{x,i}(M)^{I_x}$ are in fact motives over $k(x)$, which are effective if M is effective: this is more or less the motivic interpretation of your conjectures with Tate (up to the fact that one should say, more precisely, that the $\mathrm{gr}_{x,i}(M)$ are themselves motives defined over the finite extension of $k(x)$ with group I_x/U...). If your extra factors are not to screw up the zeros and poles of the L-function too badly (precisely, if it is not to acquire extra poles of weight $\neq \rho + 1$), then it must be assumed resp. admitted that the $\alpha_x(M)$ piece is pure of weight $\rho + 1$ (which implies that $E_x = \mathrm{gr}_{x,0}(M)^{I_x}$ is exactly the part of $E_x^{\natural}$ weight $\leq \rho$). When M is defined by an abelian variety A_η by (15), this is exactly equivalent to saying that over a sufficiently large finite extension K' of K, the Néron model of $A_{K'}$ does not have any additive component at the points over x, i.e. to your beloved personal conjecture (to which I will soon return)[12]. [But this can be *false* when M is an effective motive of rank 2, for instance when M is the tensor product $R^1(A_\eta) \otimes R^1(A_\eta)$, A_η some elliptic curve over K! *Then $\alpha_x(M)$ has parts of weight ρ and $\rho + 2$.*] In this case (and only then!), the extra factors essentially satisfy the same functional equation as L_E, up to numerical coefficients, and there is a nice functional equation for $L_M^{\natural}$,

(D) $\qquad L_M^{\natural}(1/qt) = (-t)^{\chi(E) + \sum_x \chi_x(M)} (\delta(E) \prod_x \delta_x(M)) L_M^{\natural}(t),$

with

(19) $\qquad\qquad\qquad \chi_x(M) = d(x) \cdot \mathrm{rank}\, \alpha_x(M)$

(20) $\qquad \delta_x(M) = (\det(f_x|\alpha_x(M)))^{d(x)} = \pm q^{\chi_x(M)/2}, \text{ where } q = p^{\rho+1}.$

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[12] What follows was hand-written at the bottom of the page and was partially erased.

If in addition one wants $L_M^\natural(t)$ not to have poles of weights other than ρ and $\rho + 2$ (which according to you seems to be one of the criteria which should distinguish nice L-functions) then the zeros of the local factors defined by the $\alpha_x(M)$ must be cancelled out by those of the $P_E^1(t)$, which is almost the same thing as saying that $H^1(\overline{X}, \overline{E})$, as a space with automorphism f, "contains" the sum of the $g_{x*}(\alpha_x(M))$ (where $g_x : \operatorname{Spec}(k(x)) \to \operatorname{Spec}(\mathbb{F})$ is the canonical morphism, induced by $g : X \to \operatorname{Spec}(\mathbb{F})$). I have not given this question any serious thought, but at first glance such a cancellation would seem fairly miraculous!

To finish up these general considerations on L-functions, I can say exactly what happens when $M = T_\ell(A)$ and your conjecture holds at x. J.-P. Serre : "Your conjecture": the existence of a semi-stable model after finite extension of the base field, cf. note 108.2. Then the filtration of M by the $M_{x,i}$ has exactly two steps, and (taking a polarization of A which defines a pairing $M \times M \to \mathbb{Q}(1)$), $M_{x,0}$ is the orthogonal complement in M of the "multiplicative type" part, i.e. the weight $\rho - 1 = -2$ part, of $M_{x,0}$. Passing to invariants, one then gets a canonical isomorphism

$$(21) \qquad \alpha_x(M) = (M/M^U)^{I_x/U} = T_\ell(A_x^{\mathrm{mult}})^\vee(1) \quad (\simeq T_\ell(A_x^{\mathrm{mult}})(-1))$$

whence

$$(21 \text{ bis}) \qquad \chi_x(M) = \dim A_x^{\mathrm{mult}}, \quad \delta_x(M) = \pm 1.$$

Precisely, A_x^{mult} is determined by a discrete lattice

$$R_x = \operatorname{Hom}_{\mathrm{gr}}(\mathbf{G}_{m\ k(x)}, A_x^{\mathrm{mult}})$$

of rank $\chi_x(M)$ on which f_x acts as an automorphism of finite order, and one has

$$(22) \qquad L_{\alpha_x(M)}(t) = 1/\det(1 - t^{d(x)} f_x | R_x), \quad \delta_x(M) = (\det f_x | R_x)^{d(x)},$$

so the total local factor of $L_M^\natural$ at x is

$$(23) \qquad L_{M,x}^\natural(t) = P_x^0(t) P_x^1(t) P_x^2(t)$$

with

$$(23 \text{ bis}) \qquad \begin{cases} P_x^0(t) = \det(1 - (p^{-1}t)^{d(x)} f_x | R_x) \\ P_x^2(t) = P_x^0(pt) = \det(1 - t^{d(x)} f_x | R_x) \leftarrow \begin{smallmatrix} \text{this is your extra stuff} \\ \text{coming from } \alpha_x(M) \end{smallmatrix} \\ P_x^1(t) = \det(1 - t^{d(x)} f_x | T_\ell(A_x^{\mathrm{abel}})) \end{cases}$$

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Facsimile of Grothendieck's letter of 9.30.1964, page 222

As this letter has already become rather long, I will stop for the moment, and will return in the near future to the "local" conjectures on abelian varieties, which look provable to me (at least in characteristic 0), modulo the result of Mumford's which I have already mentioned to you in this context.

Yours,

A. Grothendieck

PS. An example. Take an elliptic curve A over K of Néron model A, and assume that $x \in X$ is such that A_x^0 is a group of multiplicative type, say $\mathbf{G}_{m,x}$ itself, so that $M_{x,0} = M_0 = M^{I_x} = E$ is of rank 1 and weight $\rho - 1 = 0$, and $M/M_0 = \mathrm{gr}_{x,1}(M) = \mathrm{gr}_{x,1}(M)^{I_x}$ is of weight $\rho + 1 = 2$, where I_x acts on M by transvection. (N.B. Here $M = R^1 h_*(\mathbb{Q}_A)$, an effective motive of weight 1). The situation is completely under control, and it is possible to analyse completely the filtration at x of a motive of the form $M^\rho = M \otimes \ldots \otimes M$ (ρ factors), which is effective of weight ρ. Unless I am mistaken, one finds that $\alpha_x(M)$ contains parts of weights exactly all the even numbers between ρ and 2ρ (and in any case, the weight 2ρ appears as can be seen immediately). Thus, the function $L^\natural_{M^\rho}$ has screwed-up poles as soon as $\rho \geq 2$, such as the pole $p^{-\rho}$ of weight 2ρ, and it also has a rather unappetizing functional equation. Therefore, it seems to me that it is only for effective motives of weight 1 (possibly with Tate twists) that your function $L^\natural$ is reasonable — and even then, it is not certain that there will be no unwanted poles of weight $\rho = 1$.J.-P. Serre : Replace "of weight $\rho = 1$" by "of weight $\rho + 1 = 2$", cf. the letter of October 3-5, 1964. Of course, for effective motives of weight 0, which essentially correspond to ordinary L-functions, relative to finite groups, the $\alpha_x(M)$ vanish, i.e. $L_E = L^\natural_M$.

October 3-5, 1964 Alexandre Grothendieck

My dear Serre,

Here is the promised sequel to my letter of September 30th, which I would like to start with some comments on the latter.

a) Last page, ligne -3, you will have already corrected the slip, by reading "of weight $\rho + 1 = 2$" for "of weight $\rho = 1$". On the subject of formula (14), which is related to a "Lefschetz type" isomorphism, its validity means exactly (as can be seen by duality) that the invariants of M and $\check{M}$ are dual to each other, which obviously is not always the case, but holds if M is semi-simple under the action of π, so (assuming the motivic yoga) if M comes from a semi-simple motive, such as $H^i(Y, \mathbf{Q}_\ell)$ (for projective smooth Y_η over K).

b) The formulas in the letter can be transposed in an obvious way to the situation where a finite group G acts on (X, E), and one considers the functions $L_{E,G,\phi}$ associated to class functions ϕ on G (with values in the coefficient field of E, a finite extension of $\mathbf{Q}_\ell$). Indeed, the fundamental duality formula (1) obviously holds if the G-action is taken into account (i.e. as a formula in the derived category of sheaves with group of operators G). In (8), the only thing to assume is that the envisaged isomorphism is compatible with the operations of G (where G acts on $\check{E}$ by transport de structure, i.e. by an action contragredient to the action on E); note actually that (accepting the motivic yoga), taking averages of the transforms of a "positive definite" form on a semi-simple motive M of weight ρ under the operations of G, an isomorphism (8) compatible with G should indeed exist for such a motive M. Under these conditions, the functional equation (B′) becomes   1/2

(B′) $L_{E,G,\check{\phi}}(1/qt) = A_{E,G,\phi}(t)(-t)^{\chi(E,G,\phi)}\delta(E,G,\phi)L_{E,G,\phi}(t),$

with the obvious interpretation of the quantities $A_{E,G,\phi}$, $\chi(E,G,\phi)$ and $\delta(E,G,\phi)$, and where $\check{\phi}$ is the contragredient class function of ϕ:

$$\check{\phi}(g) = \phi(g^{-1}),$$

and there are analogous variations of all the other formulas in my letter. An analogous remark can be made if one also takes a fixed algebra over the coefficient field, and restricts to sheaves E which are modules over this algebra, taking L-functions relative to the characters of the latter. Things can also be spiced up by having G act on the said algebra. . .

c) In the middle of writing, I have changed my mind about the point that "if one insists on having an L-function which depends multiplicatively on M,

the definition you suggest is the best". It now seems to me that the simplest solution (which I had initially rejected) is best, namely: take a composition sequence of M with simple quotients M_i (for the group action $\pi = \mathrm{Gal}(\overline{K}/K)$) and define L_M to be the product of the L_{E_i}, where E_i is the direct image of M_i over X. In this way, additivity holds by definition, without losing either the functional equation or the nice distribution of zeros and poles (modulo the Weil conjectures). What is more, in the most interesting cases, M is already semi-simple, and L_M is nothing but L_E, whose theoretical description (via local factors) is particularly simple. Actually, it is good to note that if M comes from a semi-simple motive, then (assuming the general motivic yoga) M is semi-simple as a π-module, hence L_M is nothing but L_E, and moreover this function does not depend on the prime number ℓ chosen to "realize" the given motive. This last remark can be immediately extended to the case of an arbitrary motive which is not necessarily semi-simple, by linearity from the simple case. The argument that initially made me reject this simple definition of L_M was that with this definition, the local factors of L_M appear not to be purely local invariants, i.e. depending only on the sheaf deduced from E by the base change $K \to K_x$ (where K_x is the completion of K at x), because the virtual decomposition of E into simple components is no longer one after the base change $K \to K_x$; precisely, even if F is simple, $F \times_K K_x$ is not necessarily semi-simple, and one does *not* obtain the desired local factor by taking a composition sequence with simple factors of the latter. However, it seems that the only use (if any) of L-functions is in the case of coefficient sheaves coming from *motives*, (which makes conjectures such as Riemann and Artin possible). Now, remaining in the category of motives, semi-simplicity is preserved by base change, and consequently the local factor of L_M at x only depends on the *motivic* pullback of the motive in question over $\mathrm{Spec}(K)$ under $\mathrm{Spec}(K_x) \to \mathrm{Spec}(K)$. Actually, contrary to my inital predictions (influenced by your view of local factors), the L_M (just like the L_E themselves) behave well under restriction and induction of characters when finite groups come into play. Therefore, it seems to me that the functions L_M have nothing but virtues, and not a single true defect.

d) I have taken another look at Weil's article "Sur la théorie du corps de classes", and it seems to me that in the last paragraph of this work, Weil let himself get a little carried away by "the generality of analysis" J.-P. Serre : Grothendieck had misunderstood Weil, see below. (of course, I take it, as Weil does in saying that "there are good reasons for conjecturing, as

Artin does for his non-abelian L-functions, that the L-functions introduced here are entire functions", that he restricts himself to an *irreducible character* *other than the trivial character*). And this in two ways: 1º) By the geometric case (the only one for which I have a precise yoga), I have the feeling that it is crazy to hope for Riemann hypothesis —Artin conjecture type results (which apparently are more or less the same thing) for sheaves, or if you prefer, for characters, which are not associated to *motives*. Obviously, for the moment I do not really know what this means in Weil's context, where he has characters with complex coefficients, but there must be Draconian geometric restrictions on the nature of the characters in question in order to reasonably give rise to conjectures of the envisaged type. 2º) Even assuming these Draconian restrictions, the geometric case seems to indicate that Weil's local factors are not what is needed to get Riemann-Artin to work (cf. the P.S. in my previous letter). — I will try to make this point precise in the next few days, by trying to clarify the link between motives and Weil-type L-functions.

e) Here is a bunch of questions on classical L-functions. Do Artin's L-functions contain Hecke's, i.e. are they associated to a "Grössencharactere" as Weil's are? Are there Riemann hypotheses or Artin conjectures available for either of them? From this point of view, what is the status of the functions introduced in Tate's thesis; are they anything more than a trick for giving a pretty proof of a functional equation for functions of a known type, or do Tate's generalized ζ functions turn up anywhere else? Are there any striking geometric contexts in which Hecke-Artin-Tate or even Weil-type functions turn up? I remember you mentioning Deuring in the context of curves with complex multiplication; are there any other known examples? Finally, is it known whether the Riemann ζ function has infinitely many zeros? J.-P. Serre : "is it known whether the Riemann ζ function has infinitely many zeros?". Despite his work in Analysis (on Fredholm theory, for example), Grothendieck was never interested in analytic number theory.

Having just taken another look at Weil, I find there that Hecke's L-functions are not special cases of Artin's; apparently, finding a common generalization of them was one of Weil's main motivations. Moreover, I have also found the answer to my objections in d), since (contrary to what I thought I remembered), Weil restricts himself to a *finite* Galois extension K of $\mathbf{Q}$ (let us say) and the canonical extension $G_{K/\mathbf{Q}}$ of $\mathrm{Gal}(K/\mathbf{Q})$ by the idele class group C_K, without passing to the projective limit over all K. Under Weil's conditions, and restricting for the sake of argument to a character which is trivial on the connected component, one finds that the character in question is trivial on an open subgroup, i.e. comes from a character of the Galois group of a finite extension of $\mathbf{Q}$, so that it corresponds to an effective motive of weight 0 of $\mathbf{Q}$ (which counters both my objection 1º) and my objection 2º)).

f) I have just received your letters of 9.30 and 10.2. All right, I had confused q^N and $q^N - 1$, and my conclusion thus has to be modified as you say. Here is how one deals with the general case. Let J.-P. Serre : This proof is developed in SGA 7, I, lecture IX. $X_0 = \mathrm{Spec}(V)$, $U_0 = \mathrm{Spec}(K)$, $Y_0 = \mathrm{Spec}(k)$; there is then an exact sequence;

$$1 \to I_0 \to \pi_1(U_0) \to \pi_1(X_0) \to 1,$$

where I_0 is the inertia group (N.B. V is assumed complete, which is legitimate). V is the inductive limit of its finitely generated subrings A, hence K is the inductive limit of the $A[t^{-1}]$ (restricting to A containing t), and by assumption E comes from a representation of a fundamental group of a $\mathrm{Spec}(A[t^{-1}])$. Up to localizing a little, one may assume that $X = \mathrm{Spec}(A)$ is regular, and $Y = V(t)$ (the subscheme defined by t) is regular and irreducible. Finally, replacing A by its completion with respect to the tA-adic topology, one obtains a morphism $X_0 \to X = \mathrm{Spec}(A)$, where A is a regular separated Noetherian ring which is complete for the tA-adic topology; $Y = V(t)$ is a regular irreducible subscheme of X, Y_0 is the inverse image of Y, and U_0 is thus the inverse image of $U = X - Y$; E comes from a module over $\pi_1(U)$, and finally, Y is a scheme of finite type and hence contains a point whose residue field is *finite*. As A is regular and complete, and Y is regular, one finds an exact sequence

$$1 \to I \to \pi_1(U) \to \pi_1(X) \to 1,$$

where I is the image of the inertia group relative to the prime ideal tA, and there is a homomorphism from the first exact sequence into this one, which induces an isomorphism $I_0(\ell) \to I_0'(\ell)$ of the largest quotients of I_0, I which are pro-ℓ groups. This reduces the problem to proving the desired statement,

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forgetting about the initial situation and starting with a module E over $\pi_1(U)$ on which I acts via $I(\ell)$. You can now complete the proof by noting that there is a map from $\pi_1(y) \simeq \widehat{\mathbf{Z}}$ to $\pi_1(X) = \pi_1(Y)$ (A is complete!) where y is the point of Y with finite residue field, and of course the action of $\pi_1(y)$ on $I(\ell)$ is once again the q-th power, where q is the number of elements in $k(y)$. You can also reduce the problem to cor 2. of my letter of September 24, by constructing (this is easy) a homomorphism $\mathrm{Spec}(V) \to X$, where V is a discrete valuation ring whose residue field is $k(y)$, such that the inverse image of y is precisely the closed point of $\mathrm{Spec}(V)$. — It is actually possible that I exaggerated a little in the proof, in claiming that I is the inertia group of tA (to be honest, I   6/7 don't know if the inertia group is really invariant), but in any case it will work if $\pi_1(U)$ is replaced by the quotient $\pi_1(U)'$ classifing Galois coverings tamely ramified at tA (and gives rise to a kernel I' which really is isomorphic to the largest pro-ℓ quotient of the inertia group).

g) Note that the local theorem in my letter of September 24 gives (via "arithmetic") an analogous result for a projective morphism of analytic spaces $f : X \to Y$, when the base is non-singular of dimension 1; for an arbitrary locally constant sheaf of $\mathbf{Z}$-Modules F on X, the sheaves $R^i f_*(F)$ are locally constant outside a discrete set, and modulo passage to a finite-index subgroup $n\mathbf{Z}$ in the local fundamental group $\mathbf{Z}$ on Y, the action of the latter on the cohomology of the fiber is unipotent. I wonder whether an analogous result holds for an arbitrary proper morphism of analytic spaces; in a way, it would be more satisfying if the answer were no — i.e. the reason this holds for a projective morphism really is arithmetic. Note that the local result is also true for a module over the fundamental group of the complement of a divisor with normal crossings at a regular point x of a scheme (or an analytic variety). But, as can already be seen in the case of the projecting cone of an algebraic curve, there is no generalization to much more general local fundamental groups.

h) The ring $K\big(\mathbf{Q}_\ell[\mathbf{Z}]\big)$ is the K-ring constructed from finite-dimensional vector spaces over $\mathbf{Q}_\ell$ equipped with an automorphism f. — It would surprise me J.-P. Serre : "It would surprise me...". Indeed, Mumford constructed a counterexample:

D. Mumford, *A note on Shimura's paper "Discontinuous groups and abelian varieties"*, Math. Ann. **181** (1969), 345–351. if Tate's suggestion (that algebraic cycles on an abelian variety are "decomposable") were true; it had seemed to me that this must be a particularity of products of elliptic curves. — I have no comments to make on the Mumford-Tate algebraic group, as I

haven't been able to think about it. — In principle, I still intend to devote a week this month to writing a summary on dimension for Bourbaki; I will try to keep my word. Let me take advantage of this opportunity to confess that I am a little bothered by the honour Bourbaki grants me in inviting me so regularly to the meetings. I was quite bored at the last one, which is of course not surprising, but in addition I had the impression of having almost nothing to say for a fortnight, and had difficulty keeping myself from dozing off most of the time. It might be better policy if Bourbaki were only to invite me (like the retired members) when it seems to him that my insight could be useful for such and such a subject on the program. Also, don't forget that I am already up to my neck in sorites with the writing of the Elements, which perhaps diminishes my appetite for Bourbaki's. Something like two weeks a year out of the four weeks of Bourbaki meetings would seem like a reasonable dose to me.

i) I have finally come to the point of this letter! Start with a discrete valuation ring V, which you may assume complete with algebraically closed residue field. It gives an inertia group I, thence a quotient $I(\ell)$: assume we have a finite-dimensional vector space M on which $I(\ell)$ acts (so there is an ℓ-adic sheaf on $U = \mathrm{Spec}(K)$). Then the following conditions (which are stronger than the fact that the action of I is unipotent) are trivially equivalent (N.B. g denotes a topological generator of $I(\ell)$):

a) $M' = (1 - g)M \subset M^g$.

a') I acts trivially on $M/M^I = M/M^g$.

a'') $(1 - g)^2 = 0$.

b) $(M^I)^{\mathrm{orth}} \subset M^I$.

b') $(1 - g)M \subset \big((1 - g)M\big)^{\mathrm{orth}}$.

c) rank $M^I \cap (M^I)^{\mathrm{orth}} = d$ (where $d = \mathrm{rank}\, M/M^I = \mathrm{rank}\, M'$).
    (N.B. a priori, we have the inequality $\leq$).

d) The action of g on M is given by a transvection, or intrinsically, by a homomorphism

$$I(\ell) = \mathbf{Z}_\ell(1) \longrightarrow \mathrm{Hom}(M/M^I, M'),$$

d') or alternatively by an element

$$\xi \in M' \otimes M'(\rho - 1) \quad \text{where } M' = (1 - g)M.$$

I forgot to mention that in b), b′), c), and d′) one assumes given a perfect pairing

$$M \times M \longrightarrow \mathbf{Q}_\ell(\rho)$$

compatible with the action of I (where I acts trivially on the right-hand side) and symmetric or alternating (it will practically be one or the other according to whether ρ is even or odd). In practice, this means that M comes from a semi-simple motive of weight ρ on U. Note that in d), the homomorphism $\mathbf{Q}_\ell(1) \otimes M/M^I \to M'$ obtained is necessarily bijective, and therefore in d′) the "form" obtained is necessarily non-degenerate. — Assume now that M comes from an effective semi-simple *motive* of weight $\rho \geq 0$. Let us assume that then M^I also comes from an effective motive over $\mathrm{Spec}(k)$, independent of ℓ (this follows from the general motivic yoga!) which will not generally be pure of weight ρ. This is in any case what can be proved via the Néron model when M comes from an abelian variety, i.e. is effective of weight $\rho = 1$. In general, the motive that M^I comes from is neither semi-simple nor pure of weight ρ, but only of weight $\leq \rho$; let M_0^I be the subspace which corresponds to the largest submotive of weight $\leq \rho - 1$. Then, for reasons of weight (which are entirely justified for an abelian variety) one sees that M_0^I *is orthogonal to* M^I, i.e. is contained in M' (this appears to be the essential point in Igusa's theory of "vanishing cycles"). Using this, one sees that the conditions above are also equivalent to the following: 8/9

e) $M_0^I = (M^I)^{\mathrm{orth}} \quad (= M')$.

e′) rank $M_0^I = d$ (N.B. a priori, we have the inequality $\leq$).

f) M/M^I, considered as a virtual motive over $\mathrm{Spec}(k)$, is of weight $\geq \rho + 1$.

To make f) explicit, I should also have mentioned that the successive factors in the filtration on M obtained by taking the invariants under I, then the invariants modulo these etc., should also (by Serre-Tate) be motives over $\mathrm{Spec}(k)$,

which makes f) meaningful if we assume (as we shall) that the action of I on M is unipotent. If in addition we assume that the action of I on M/M^I is trivial (condition a') above), then M/M^I is also a motive over $\mathrm{Spec}(k)$, dual modulo twist of M' which is a submotive of M^I, and condition f) can be stated as saying that M' is of weight $\leq \rho - 1$, i.e. $M' \subset M_0^I$ i.e. is equivalent to e), which is itself trivially equivalent to e'). But in any case, f) should imply a'), since for reasons of weight, the homomorphism $M' \to M/M^I$ is zero given f). It remains to prove that a) implies e), or alternatively that given a), the weight of M' is $\leq \rho - 1$, but this again follows from the existence of a non-degenerate form, just as for d'), by weight considerations which also show that *under the above conditions, $M' = M_0^I$ is purely of weight $\rho - 1$, and therefore M/M^I is purely of weight $\rho + 1$.* At the same time, one sees that M^I is purely of weight ρ only if $M' = 0$, i.e. $M = M^I$, i.e. in the unramified case.

In the effective case of weight $\rho = 1$, i.e. starting with an abelian variety over K, these arguments can be explained without reference to motivic meta-physics (the weight considerations boil down to reduction to the case of a finite residue field, whose Frobenius will act on the given sheaves on $\mathrm{Spec}(k)\ldots$). Then M_0^I corresponds to the multiplicative part of the reduced Néron variety A_0, M^I being nothing but $T_\ell(A_0)$. The validity of condition e') then means (assuming that the action of I on M is unipotent, which will be the case after finite extension of K) that A_0 has no additive part, i.e. it is nothing else than your conjecture on the "limit" behavior of Néron models under arbitrarily large extension of the base field K. (Note that once I is assumed unipotent, the validity of the conditions above is invariant — and not simply stable — under extension of K, and M^I and M_0^I are not modified by extension of K). The equivalence of this geometric condition with apparently weaker conditions such as a'') is interesting because the latter are clearly inherited under passage to a submodule N of M. This makes it possible, in order to prove your conjecture (along with the Serre-Tate conjectures), to assume that the abelian variety in question is a *Jacobian*. In characteristic 0, it then follows from a theorem of Mumford's on degeneration of curves that this works — since for the Jacobian of a curve whose reduction is a curve with only ordinary double points, the proof of e') is trivial via the Picard scheme. For the general case, it seems to me

that the problem can be reduced to the case of the completion of a number field, but a fat lot of good that does us; *Mumford will have to try to prove his theorem in arbitrary characteristic, possibly unequal.*

10/11

For effective motives of weight $\rho \geq 2$, the example in my last letter shows that the conditions I have just proposed are rather exceptional, and in general one should expect to find parts of all weights between 0 and ρ in M^I. I would like to call your attention to an interesting phenomenon which arises in the "favorable" case, namely that the initial form on M gives rise to *two* non-degenerate forms on motives over $\mathrm{Spec}(k)$, the first one, on M^I/M_0^I, being induced by the one on M, and the less obvious second one being described in d′). It follows from the general yoga of motives that the first one is "motivic", and I do not doubt for a moment that the second one is as well (although I have not yet tried to deduce this from the general yoga). This being said, there are good reasons for conjecturing that if the initial form on M (which has of course been assumed motivic in the above) is positive definite, then the two forms it gives rise to are also definite — the sign which appears may possibly depend on ρ, and should be fairly easy to determine from special cases (an elliptic curve and tensor powers of its T_ℓ). This therefore means that for an abelian variety A, a polarization of A induces a *polarization* of the abelian part of A_0, and a positive definite *quadratic form* J.-P. Serre : See the letter of October 30, 1964, as well as SGA 7, I, exposé IX. with coefficients in $\mathbf{Q}$ on the lattice corresponding to the part of multiplicative type. More intrinsically, introducing the dual A' of A, it should be true that the canonical pairings between the T_ℓ of A_0^{abel} and $A_0'^{\;\mathrm{abel}}$ on the one hand, and A_0^{mult} and $A_0'^{\;\mathrm{mult}}$ on the other, come from a $\mathbf{Q}$-valued pairing between the associated lattices, and every polarization isogeny $A \to A'$ induces a *polarization* isogeny $A_0^{\mathrm{abel}} \to A_0'^{\;\mathrm{abel}}$, and a *positive definite* form on the lattice associated to A_0^{mult} — at least up to a sign which I have not investigated. J.-P. Serre : "up to a sign which I have not investigated". This sign is $+$, cf. SGA 7, I, p. 144, th. 10.4.b). Already in the case of an elliptic curve, the latter would yield an interesting invariant, namely (when there is ramification, i.e. A_0 is of multiplicative type) a positive rational number, which will be multiplied by n (at least when n is relatively prime to the residue characteristic) upon passage to a degree n extension of K. When the elliptic curve has a proper model over $\mathrm{Spec}(V)$ which is regular and whose special fiber has only one ordinary double point, Igusa's theory seems to show that this invariant is 1. What happens in general? Experts on elliptic curves should be able to say.

11/12

Regards,

A. Grothendieck

PS   In i), I forgot to mention that for an abelian variety, the envisaged conditions, i.e. basically your conjecture on Néron models of abelian varieties, immediately give the two Serre-Tate conjectures on abelian varieties in the general case (you have probably already noticed this). Another remark: in my letter of September 24th, I think that for unequal characteristics, the condition "ℓ different from char. k" can be replaced by "I acts on E via the quotient corresponding to extraction of the ℓ^N-th roots, for various N, of a suitable uniformizing parameter"; unless I am mistaken, the argument given in f) works and shows here that if $\ell = $ char. k, the action of I is unipotent (without passage to a finite-index subgroup). Using this fact, I have the impression that the same reduction argument shows that the conclusion is valid (for any ℓ) if the residue characteristic is 0 — by reduction to the case of residue characteristic ℓ: this would imply that in the case char. $k = 0$, if I in fact acts on a finitely generated module over $\mathbf{Z}_\ell$, and if this action becomes trivial after reduction modulo ℓ (the "rigidity condition"), then the action of I is unipotent. Is this compatible with your counterexamples?

October, 1964 JEAN-PIERRE SERRE

Dear Grothendieck,

Verdier tells me I should reply to your "naive questions" on L-functions. On reading your letter (page 3-4, then page 5) I had thought that you had answered the said questions yourself. So here are a few comments:

As you noticed (p. 5) Artin's L-functions do not contain Hecke's L functions; Weil's generalized L-functions (in his paper on class field theory) are probably the smallest reasonable family containing them both. Unless I am mistaken, the Riemann hypothesis and Artin's conjectures are "valid" for these functions: the zeros of $L(s)$ lie on $R(s) = \frac{1}{2}$ (for $R(s) > 0$, let us say, since there are "trivial" zeros on the negative real axis) and if the original representation (the character) does not contain the unit character, then L is everywhere holomorphic.

The L-functions in Tate's thesis are essentially Hecke's L-functions (i.e. correspond to an abelian representation of C_k, if I may say so). By "essentially" I mean first of all that they give Hecke's L-functions, and that the functions they give are probably linear combinations of Hecke's L-functions (ask Godement for more details). I refer you to Godement because this is obviously a question of locally compact groups (in what sense can any function be approximated by a combination of characters?).

There are indeed some geometric questions in which Hecke's L-functions (with Grössencharakter) turn up. Indeed, this is one of Weil's most beautiful results. He noticed it while studying the zeta function of Fermat-style hypersurfaces $\sum x_i^{n_i} = 0$ (cf. his paper in the Bulletin). J.-P. Serre : "his paper in the Bulletin": the one in which he states the "Weil conjectures", Oe. Sci. [1949b]. In this case, Frobenius can be determined explicitly for every p, and *the variation of Frobenius with p is a Grössencharakter*. This is a rather messy way of talking; it should first be specified that a character of C_k can be identified with a function $f(\mathfrak{p})$ on the set of prime ideals $\mathfrak{p}$ of the field not contained in a suitable finite set S. Here is the method: if S is a finite set of prime ideals, let I_S be the subset of the idele group I consisting of (x_v) such that $x_v = 1$ for v at infinity and $v \in S$; let U_S be the product of the units of the local fields k_v, $v \notin S$ (U_S is open in I_S). The character $\chi : C_k \to T$ defines a character χ of I, and thence by restriction a character χ_S of I_S. By continuity of χ_S, one sees that (replacing S by a larger set if necessary) χ_S is equal to 1 on U_S, i.e. corresponds to a character $\overline{\chi}_S$ of the discrete group $I_S/U_S = \mathrm{Id}_S$ of *ideals* prime to S. Moreover, since $k^*.I_S$ is dense in I, knowledge of χ_S (and thus also of $\overline{\chi}_S$) *determines χ itself.*J.-P. Serre : The passage from χ

to χ_S is explained in more detail in *Abelian ℓ-adic Representations and Elliptic Curves*, Benjamin, New York, 1968. See also [**ST68**], §7, th. 10. What is called the Grössencharakter is either the character χ of C_k or the character $\overline{\chi}_S$ of Id_S. (Of course, the $\overline{\chi}_S$ are *very special* characters of the group Id_S; their characterization is an easy and boring exercise; see the result in Weil's talk at the Kyoto symposium, 1st lecture).

Here is an example (due to Deuring, but inspired by Weil) in which a Grössencharakter turns up naturally:J.-P. Serre : Reference: M. Deuring, *Die Zetafunktion einer algebraischen Kurve vom Geschlechte Eins*, I,. . . , IV, Gött. Nach., 1953–1957.

Let E be an elliptic curve defined over a number field k, which has complex multiplication by an imaginary quadratic field L: I assume $L \subset k$ (i.e. the complex multiplication is defined over k). Let S be a sufficiently large finite set of primes of k (unless I am mistaken, it is enough to take S to be the set of points where the curve has bad reduction — but never mind). If $\mathfrak{p}$ is a prime ideal of k, $\mathfrak{p} \notin S$, the Frobenius of the reduced curve $\pi_\mathfrak{p}$ is an endomorphism of the said reduced curve $\overline{E}_\mathfrak{p}$. However the field L can be naturally embedded into $\mathrm{End}(\overline{E}_\mathfrak{p}) \otimes \mathbf{Q}$; it can (easily!) be shown that $\pi_\mathfrak{p} \in L$. Choosing an embedding of L into $\mathbf{C}$, then gives a complex function $\mathfrak{p} \mapsto \pi_\mathfrak{p}$; as $\pi_\mathfrak{p}$ is not of absolute value 1, one can force it to be so by dividing by $\mathrm{N}\mathfrak{p}^{1/2}$. The theorem (which is not very difficult either, once the theory of reduction of elliptic curves is well established) is that

$$\chi : \mathfrak{p} \mapsto \pi_\mathfrak{p}/(\mathrm{N}\mathfrak{p}^{1/2})$$

is a Grössencharakter of k (in fact, up to a character of finite order, this character actually comes from a Grössencharakter *of L*).

REMARKS 1) In fact, it is more convenient not to demand that the Grössencharaktere take values which are complex numbers of absolute value 1; thus, one can say in an obvious sense that $\mathfrak{p} \mapsto \pi_\mathfrak{p}$ is a Grössencharakter *of weight* 1.

2) Of course, the result above can be translated in terms of zeta functions: the zeta factor corresponding to H^1 of the elliptic curve is equal to the product $L(s, \chi) \cdot L(s, \overline{\chi})$, where $\overline{\chi}$ denotes the imaginary conjugate of χ.

Deuring proved (and this is now very easy) that this identity holds *without any fudge factors*: i.e. the right zeta (in your sense) coincides term-by-term with the "right" Hecke L-function (the one that has a perfectly pretty functional equation).

As you know, this fact is what really got Weil excited (and rightly so!) He (+Shimura-Taniyama) generalized all this to abelian varieties. I refer you to Tokyo-Nikko for a presentation of their results. Weil explains there in particular that by this method, one basically only gets holds of "half" of the possible Grössencharaktere J.-P. Serre : "half of the possible Grössencharaktere". I do not understand what "half" means. This refers to characters "of A_0 type" in Weil's sense of the word, i.e. "motivated" (their Hodge type is integral).; one only gets those whose values are contained in a number field. No progress has been made on the question since.

Do not think that this is the only case in which zeta functions of varieties over number fields have been cleverly computed. Eichler, Shimura, Igusa studied zeta functions of what we now know as *moduli varieties* (of polarized abelian varieties, possibly equipped with a fixed ring of endomorphisms). By ingenious methods (of which I know little), they manage to write down the Frobenius automorphisms explicitly by using the natural correspondences of these varieties, and from this they deduce a formula for zeta. In the case where the moduli problem is that of elliptic curves with a certain rigidification, the functions obtained are those that Hecke associated to Spitzenformen of the modular group (for weight 1 — since there is then once again a theory of weights, which very probably coincides with the geometric one). J.-P. Serre : "a theory of weights, which very probably coincides with the geometric one". Not quite: modular weight k corresponds to geometric weight $k - 1$. The case dealt with by Eichler, Shimura and Igusa is $k = 2$. This correspondence J.-P. Serre : We are close to the Taniyama-Weil conjecture! gives information *in both directions*: since these functions are (automorphic) Hecke functions, the functional equation and analytic continuation work (which is not known in general, not even for a curve over $\mathbf{Q}$) — and since they are zeta functions of curves, the absolute values of Frobenius are $(N\mathfrak{p})^{1/2}$ (which Hecke's analytic theory made it natural to conjecture, without being up to providing a proof). That is why all this is interesting.

Your questions have led me a bit too far: moreover, I am sure I have already told you all this many a time.

Regards,

J-P. Serre

October 30, 1964 Alexandre Grothendieck

My dear Serre;

Thank you for your letter of October 24th. I am glad Tate and Mumford have gotten rid of the screwed-up conjecture J.-P. Serre : "the screwed-up conjecture": the one that said that the **Q**-algebra of cycle classes is generated by its degree 1 elements, i.e. by the divisor classes. on algebraic cycles on abelian varieties; as for the Hodge conjectures, I now hold them to be an article of faith on a par with the Tate conjectures; indeed, they are too intimately linked for me to be able to believe that one could be wrong without the other: in other words, they must both be true. I would be very interested to have some detailed information about the seminar on heights and Weil distributions, since I anticipate that I will be using them in the not-too-distant future. Could you perhaps keep me informed? I was disappointed that you didn't answer most of my questions on L-functions (from e) — perhaps you would like to answer me some day when you feel more chatty; the same goes for the last question in my letter, at the end of the P.S.

I was also disappointed that you did not find an expert on surfaces to solve your conjecture on abelian varieties, and tell you something about the quadratic form I mentioned in j). And therefore, finding myself in a healthy temper, I broke off my current reflections to find a proof myself, which I hand you fresh out of the oven. Let $f : X \to S$ be a projective map, S being the spectrum of a discrete valuation ring V, which may be assumed complete with algebraically closed residue field. Assume that the generic fiber X_1 is smooth and geometrically connected of dimension 1 over the fraction field K of V, choose an ℓ prime to the residue characteristic, and assume that the action of $\pi = \operatorname{Gal}(\overline{K}/K)$ on $H^1(\overline{X}_1, \mathbf{Z}_\ell)$ is unipotent. The result to be proved is that π acts trivially on $H^1/(H^1)^\pi$. By resolution of singularities (Abhyankar), X may be assumed regular, and furthermore $C = X_{0,\mathrm{red}}$ may be assumed to have normal crossings in X. Let C_i be the reduced irreducible components of X_0, and set $X_0 = \sum_i m_i C_i$. Assume first for the sake of simplicity that ℓ is *relatively prime* to the m_i. Let F be a finite ℓ-group (we will take $F = \mathbf{Z}/\ell^N\mathbf{Z}$ and pass to the limit over N). It follows from the conditions imposed on the action of π on $H^1(\overline{X}_1, \mathbf{Z}_\ell)$ that

$$(*) \qquad H^1(\overline{X}_1) = \varinjlim_i H^1(X_1(i)),$$

in which it is implicitly understood that F is everywhere the coefficient sheaf, and

$$X_1(i) = X_1 \times_S S(i), \quad S(i) = \mathrm{Spec}\big(V[z]/(z^{\ell^i} - t)\big),$$

where t is a uniformizing parameter of V. Similarly, let $X(i)$, $X_0(i)$ be deduced from X, X_0 by the same base change; there are then exact sequences

$$\cdots \to H^n_{X_0(i)}(X(i)) \to H^n(X(i)) \to H^n(X_1(i)) \to H^{n+1}_{X_0(i)}(X(i)) \to \cdots$$

J.-P. Serre : The term $H^q_{X_0(i)}(X(i))$ denotes the q-th cohomology group of $X(i)$ with support in $X_0(i)$.

whence, by passage to the limit over i, and taking into account the isomorphisms $(*)$ and

$$H^n(X(i)) \xrightarrow{\sim} H^n(X_0(i)) \xleftarrow{\sim} H^n(X_0),$$

one obtains an exact sequence

$$\cdots \to \Phi^n \to H^n(X_0) \to H^n(\overline{X}_1) \to \Phi^{n+1} \to \cdots,$$

with

$$\Phi^n = \varinjlim_i H^n_{X_0(i)}(X(i)).$$

Hence the comparison of the $H^n(X_0)$ and the $H^n(\overline{X}_1)$ is essentially reduced to the computation of the Φ^n (actually a general fact having nothing to do with relative curves...). We are going to see that here, the Φ^n can be computed as essentially *local* invariants on X. For this, consider the spectral sequence

$$H^n_{X_0(i)}(X(i)) \Longleftarrow H^p(X_0(i), \underline{H}^q_{X_0(i)}(F_{X(i)}))$$

whence by passage to the limit over i, and writing $p_i : X_0(i) \to X_0$ for the projection isomorphism, one gets:

$$\Phi^n \Longleftarrow E_2^{p,q} = H^p(X_0, \underline{\Phi}^q)$$

with $$\underline{\Phi}^n = \varinjlim_i p_{i*} \underline{H}^n_{X_0(i)}(F_{X(i)}).$$

Now, I claim that:

$$(**) \quad \begin{cases} \underline{\Phi}^n = 0 \quad \text{for } n = 0, 1, \\ \underline{\Phi}^n \text{ is concentrated on } T, \text{ the set of double points of } X_0 \\ \qquad \text{for all } n, \end{cases}$$

whence the spectral sequence above degenerates and yields the values

$$\Phi^n = \sum_{x \in T} \underline{\Phi}^n_x$$

which express Φ^n as a sum of local invariants associated to the double points of X_0. The proof of $(**)$ is easy, since the local situation is well under control. The hypothesis that ℓ is relatively prime to the m_i is used only to ensure that the $X(i)$ are geometrically unibranch, which implies that $\underline{\Phi}^0 = \underline{\Phi}^1 = 0$ (the first relation is in any case trivial without this condition). If the hypothesis in question is omitted, $\underline{\Phi}^1$ can have 1-dimensional support, which prevents the spectral sequence from degenerating, so that the Φ^n can no longer be computed purely locally, but it remains true that the other $\underline{\Phi}^n$ ($n \geq 2$) are concentrated on T. Recall the explicit formula

$$\underline{\Phi}^n_x = \varinjlim_i H^{n-1}(U_x(i)) \quad \text{for } n \geq 2,$$

where X_x denotes a strict localization of X at x, U_x is the inverse image of $X - X_0 = X_1$ in U_x, which can therefore be obtained (if $x \in T$) by removing from X_x two divisors corresponding to a regular system of parameters (u, v) for X_x; $U_x(i)$ means the usual thing, and is the inverse image of U_x in the covering $X_x(i)$ of X obtained by adjoining a z_i such that

$$z_i^{\ell^i} = t \quad (= u^p v^q, \text{where } p, q \text{ are two of the } m_i \text{ introduced above}).$$

Knowing the "tame" fundamental group of U_x, one immediately finds

$$\underline{\Phi}^2_x \cong \operatorname{Hom}(N_x, F)$$

where N_x is defined via the exact sequence

$$0 \longrightarrow N_x \longrightarrow \mathbf{Z}_\ell(1) \times \mathbf{Z}_\ell(1) \xrightarrow{\ w_x\ } \mathbf{Z}_\ell(1),$$

with

$$w_x(a, b) = pa + qb.$$

Here, the m_i, and hence in particular q, are assumed relatively prime to ℓ, so that the equation $pa + qb = 0$ defining N_x has the solution $b = -(p/q)a$, so N_x appears to be more or less canonically isomorphic to $\mathbf{Z}_\ell(1)$, and one finds

$$\underline{\Phi}^2_x = \operatorname{Hom}(\mathbf{Z}_\ell(1), F) = F(-1).$$

In fact, one can check that this isomorphism is compatible with the natural action of π, which therefore acts *trivially* on Φ^2. Combining these pieces of information yields an exact sequence

$$0 \to H^1(X_0) \to H^1(\overline{X}_1) \to \Phi^2 \to H^2(X_0) \to H^2(\overline{X}_1) \to \Phi^3 \to 0$$

(since $H^3(X_0) = 0$), in which Φ^3 can actually be replaced by 0 when there is an invertible sheaf of degree 1 on X_1, as this implies that

$$H^2(X_0) = H^2(X) \to H^2(\overline{X}_1) \cong F$$

is indeed surjective. Passing to the limit over the $F = \mathbf{Z}/\ell^N\mathbf{Z}$, one gets the same exact sequence with coefficients in $\mathbf{Z}_\ell$, but this time

$$(* * *) \qquad\qquad \Phi^2 = \mathbf{Z}_\ell(-1)^T,$$

with trivial π-action; Φ^3 is now a finite cyclic group (annihilated by the degree of any invertible sheaf on X_1).

In fact, I have just realized that since K is a Tsen field, J.-P. Serre : "a Tsen field" is a field of dimension ≤ 1, in the sense of *Galois Cohomology*, Chap. II, §3. there is always an invertible sheaf of degree 1 on X_1, so in fact $\Phi^3 = 0$ in any case J.-P. Serre : $\Phi^3 = 0$ in any case: no, see the letter of November 1, 1964. (so $\Phi^n = 0$ for $n \geq 3$). As π acts trivially on the image of $H^1(X_0)$ in $H^1(\overline{X}_1)$, and also on Φ^2, the assertion we want to prove follows from the exact sequence, which in fact gives additional information such as:

$$(* * **) \qquad\qquad \mathrm{rang}(H^1(\overline{X}_1)/H^1(X_0)) = d - c + 1,$$

where d is the number of double points of $X_{0,\mathrm{red}}$, and c is the number of its irreducible components.

If ℓ is no longer assumed relatively prime to the m_i, some small details must be changed. Obviously, the first idea is to simply change the prime number ℓ, since it is known for other reasons that the conclusion (expressed in terms of the Néron model of the Jacobian) is in fact independent of ℓ, but it does not seem obvious to me a priori that the hypothesis that the action of π on $H^1(\overline{X}_1)$ is unipotent will then remain valid. It might be quite easy, however, to prove that up to taking a suitable finite extension over S, and keeping the same ℓ, the problem can be reduced to the favorable situation above. Here is another solution, which consists of repeating the argument above with some slight changes: consider the largest integer i_0 such that ℓ^{i_0} divides one of the m_i, consider only the $i \geq i_0$, and replace $X(i)$ by $X'(i) = X'(i_0) \times_{S_{i_0}} S_i$, where $X'(i_0)$ is the normalization of $X(i_0)$. With this slight change, all the arguments above should work, and in the finished product i.e. the final exact sequence, $H^i(X_0)$ should be replaced by $H^i(X'_0(i_0))$

$$0 \to H^1(X'_0(i_0)) \to H^1(\overline{X}_1) \to \Phi'^2 \to H^2(X'_0(i_0)) \to H^2(\overline{X}_1) \to 0$$

where Φ'^2 is once again the sum of the $\underline{\Phi}'^2_x$, $x \in T$, and $\underline{\Phi}'^2_x$ is given by the general formula $\mathrm{Hom}(N_x, \mathbf{Z}_\ell)^{a_x}$ where $a_x = \mathrm{card.\,coker}\, w_x = $ largest power of ℓ dividing p and q = number of "branches" of $X_x(i_0)\ldots$ (The fact that one can again put zero on the right-hand side, i.e. $\Phi'^3 = 0$, can be seen using the cohomology class on X defined by an invertible sheaf on X which induces a sheaf of degree 1 on X_1 on taking the inverse image of this class on $X(i_0)$.) The conclusion is that if $\pi(i_0)$ denotes the subgroup of π corresponding to $S(i_0)$, then $\pi(i_0)$ acts trivially on $H^1(\overline{X}_1)/H^1(\overline{X}_1)^{\pi(i_0)}$ (since it acts trivially on $H^1(X'_0(i_0)) = H^1(X(i_0))$ and on Φ'^2).

What we have really shown is the following: assuming that X is regular, X_0 has normal crossings, X_1 is smooth and geometrically connected of dimension 1, ℓ is relatively prime to the residue characteristic and $H^1(\overline{X}_1, \mathbf{Z}_\ell)$ is *tamely ramified*, i.e. the image of π in $\mathrm{Aut}(H^1)$ is of supernatural cardinal N which is relatively prime to p, let i_0 be the lcm of the $\gcd(N, m_i)$. Then the subgroup $\pi(i_0)$ of index i_0 in π acts by transvections on $H^1(\overline{X}_1)$, i.e. acts trivially on $H^1(\overline{X}_1)/H^1(\overline{X}_1)^{\pi(i_0)}$. The rank of this quotient is bounded above by $\sum_{x \in T} a_x - \sum b_i + 1$, where for every $x \in T$, we set $a_x = \gcd(m'_x, m''_x, N)$, where m'_x, m''_x are the multiplicities of the two components of X_0 passing through x, and for any component C_i of X_0, we set $b_i = \gcd(m_i, N)$, where m_i is the multiplicity of C_i [N.B. $\sum a_x$ is the rank of Φ'^2, $\sum b_i$ is that of $H^2(X'_0(i_0))$ i.e. the number of irreducible components of $X'_0(i_0)$, and 1 is the rank of $H^2(\overline{X}_1)$]; it is possible this is always an equality, i.e. that $H^1(\overline{X}_1)^{\pi(i_0)} = \mathrm{Im}\, H^1(X'_0(i_0))$ modulo a finite group. In any case, this is what one can check when $i_0 = 1$, and more generally, one can check directly (without any hypotheses on i_0) that

$$H^1(X_0) \longrightarrow H^1(\overline{X}_1)^\pi$$

is an isomorphism on a finite-index subgroup, the cokernel being isomorphic to $\mathbf{Z}/d_\ell\mathbf{Z}(-1)$, where d_ℓ is the largest power of ℓ dividing the gcd of the m_i. (This comes from a comparison of the two Leray spectral sequences for $X_1 \to S$, factored through S on the one hand and through X on the other.)

I confess that I have not checked the computations carefully in the general case with $'$, but I am convinced the method should work. In fact, returning to the case where one assumes beforehand that the action of π on $H^1(\overline{X}_1)$ is unipotent, and as Serre-Tate has now been proved J.-P. Serre : "... as Serre-Tate has now been proved": cf. SGA 7, I, exposé IX, §4. (for abelian varieties), one sees that the unipotence condition is preserved on changing ℓ.

Using the exact sequence:

(X) $\qquad 0 \to H^1(X_0) \to H^1(\overline{X}_1) \to \Phi^2 \to H^2(X_0) \to H^2(\overline{X}_1) \to 0$

for every ℓ (the fact that one can put zeros at both ends can be seen directly and independently of ℓ; on the left this is a simple consequence of the normality of $X\ldots$), the rank of Φ^2 will then be independent of ℓ, and therefore equal to the value obtained for "general" ℓ, namely $d - c + 1$. A more detailed analysis should give a canonical exact sequence:

$$0 \to \mathbb{Z}_\ell/d_\ell\mathbb{Z}_\ell(-1) \to \Phi^2 \to \mathbb{Z}_\ell(-1)^{(T)},$$

in which the cokernel of the last homomorphism is finite (and obviously trivial if ℓ is relatively prime to the m_i). — Note that the duality between $H^1(\overline{X}_1)/H^1(\overline{X}_1)^\pi$ and the part "of weight zero = of multiplicative type" of $H^1(X_0)$ appears in a particularly striking way in (X); indeed (possibly modulo finite groups) the first group is none other than the kernel of $\Phi^2 \to H^2(X_0)$ which can be identified (again modulo finite groups) with $\mathbb{Z}_\ell^{(T)}(-1) \to \mathbb{Z}_\ell^{(I)}(-1)$, where I is the set of irreducible components of X_0, and the second group can be computed by a well-known method using the normalization of $X_{0,\mathrm{red}} = C$, as the cokernel of a homomorphism $\mathbb{Z}_\ell^{(I)} \to \mathbb{Z}_\ell^{(T)}$ which can hardly be anything other than the transpose of the previous one (in any case, the choices which have to be made on each side in order to identify some group with $\mathbb{Z}_\ell^{(T)}$ are exactly the same, namely the choice, for every $x \in T$, of one of the two components of X_0 passing through x). In fact, it is useful to write (X) as a slightly longer *self-dual* sequence

$$0 \to H^0(C) \to H^0(\widetilde{C}) \to \Psi^2 \to H^1(\overline{X}_1) \to \Phi^2 \to H^2(X_0) \to H^2(\overline{X}_1) \to 0,$$

which is exact everywhere except in the middle, where the failure to be exact is $H^1(\widetilde{C})$ (where $\widetilde{C}$ is the normalization of C), i.e. the weight 1 part of $H^1(X_0) = H^1(C)$. What remains to be done is mostly to determine the (probably definite) non-degenerate quadratic form on $\mathrm{Ker}(\Phi^2 \to H^2(X_0)) = \mathrm{Ker}(\mathbb{Z}_\ell^{(T)} \to \mathbb{Z}_\ell^{(I)})$ which I mentioned in my last letter and which contains the same information as the action of π on $H^1(\overline{X}_1)$; I have not had the time to search seriously for a good candidate.

Here is a few other thoughts on abelian varieties. Let X be a scheme of finite type over an algebraically closed field k, X normal and irreducible, let A be an abelian scheme over X, and assume that $\pi_1(X)$ acts on $T_\ell(A)$ (ℓ being a given prime number $\neq$ char. k) via a *commutative* group: this is the case, for example, if there is "a lot" of complex multiplication on A defined over X (the (CM) case), or if $\pi_1(X)$ is commutative (X an abelian variety, for instance), or if $X = \mathrm{P}^1$ minus two points and if $T_\ell(A)$ is tamely ramified... Then A comes from an abelian scheme defined over k, at least after passage to a finite etale covering of X: indeed, this can be deduced from the Tate conjectures (more precisely, from the semi-simplicity of the action of the [absolute] Galois group of a finitely generated field extension of $\mathbf{F}_p$). The arithmetic analog of the geometric statement above is the following: let A be an abelian variety defined over a field K which is finitely generated over the prime field $\mathbf{Q}$, and assume that $\mathrm{Gal}(\overline{K}/K)$ acts commutatively on $T_\ell(A)$; $A_{\overline{K}}$ then comes from an abelian scheme over the ring of integers of a number field; this is another consequence of Tate. In fact, modulo Tate, these claims are also valid for arbitrary motives. Still on the subject of commutative Galois actions, assume that A is defined over a number field K, with commutative Galois action; I have the impression that A then has good reduction everywhere J.-P. Serre : "I have the impression A then has good reduction everywhere": no, see the letter of November 8, 1964. (without passing to a finite extension of K); do you have examples or counterexamples relating to this? Here, finally, is a statement that can be proved J.-P. Serre : "Here, finally, is a statement that can be proved . . . " This is true when the residue characteristic is 0, but it may otherwise be false; see:

A.J. de Jong and F. Oort, *On extending families of curves*, J. Alg. Geom. **6** (1997), 545–562.
(This remark, and the reference, are due to A. Chambert-Loir.) without using any conjectures, but using Mumford's moduli scheme for polarized abelian varieties: let X be the spectrum of a local normal Noetherian ring, and let A be an abelian scheme over $X - a$ (a = closed point) such that $T_\ell(A)$ is trivial, where ℓ is a prime number $\neq$ residue char.; A can then be extended to an abelian scheme over X. Corollary: if X is a regular scheme, and Y is a closed subset of X of codimension ≥ 2, then every abelian scheme over $X - Y$ can be extended to X.

Let me point out a result which shows that the Frobenius action on the π_1 is enormous, and at first glance appears very strange: let X be normal and geometrically connected over the finite field $\mathbf{F}_q$, then for every $\ell \neq p$, $\pi_1(X) \to \pi_1(\mathbf{F}_q)$ induces an isomorphism modulo finite groups on the ℓ-primary components of the abelianized groups, and for almost all ℓ, it induces an isomorphism on the maximal pro-ℓ quotient groups (in particular, for almost all ℓ, the maximal pro-ℓ quotient of $\pi_1(X)$ is commutative, and in fact isomorphic to $\mathbb{Z}_\ell$). The proof is by reduction to curves; the second statement follows on computing $H^1(X, \mathbf{Z}/\ell\mathbf{Z})$. — To have a clearer idea of what the Frobenius action on maximal ℓ-quotients of $\pi_1(\overline{X})$ looks like, I would like to know if one knows how to determine the outer automorphism group of a free group: is it simply the group of automorphisms of the abelianization of this group? The question arises both for discrete groups and for pro-ℓ-groups. Is the answer known at least for the analogous question on Lie algebras?

For the last three weeks I have been getting very excited about the interpretation of Galois and fundamental groups of all kinds in terms of algebraic groups J.-P. Serre : This refers to motivic Galois groups, cf. e.g. [**Se94**]. over number fields, especially over $\mathbf{Q}$, and even in terms of group schemes over the integers. I have convinced myself that these groups, together with the general motivic yoga, are the key to a good understanding of a transcendence conjecture J.-P. Serre : "of a transcendence conjecture". I do not believe Grothendieck ever published a precise version of this conjecture. He settled for alluding to it in [**Gr66a**], footnote at the bottom of page no10. See also S. Lang, *Introduction to Transcendental Numbers*, Addison-Wesley 1966, p. 43. linked to various cohomology lattices (integral cohomology, Hodge-de Rham cohomology), the relations between the latter and the Hodge and Tate conjectures, and a better understanding of "non-commutative" class field theory, which would realize Kronecker's "Jugendtraum", and of a functional equation for L-functions over $\mathbf{Q}$. I will probably send you another infinite letter J.-P. Serre : "I will probably send you another infinite letter ...". He never did. one of these days, but I will need some time to arrive at a coherent set of conjectures.

Regards,

A. Grothendieck

November 1, 1964 ALEXANDRE GROTHENDIECK

Dear Serre,

This is just a note to tell you that my claim, in my last letter, that $\Phi^3 = 0$, allegedly because K is a Tsen field, was obviously idiotic; I was mixed up. In fact, it is easy to check that the homomorphism $H^2(X_0) \to H^2(\overline{X}_1)$ is given by $(\lambda_i) \mapsto \sum m_i \lambda_i$, where the m_i are the multiplicities of the irreducible components of X_0. Hence $\Phi^3 = \mathbf{Z}/d_\ell \mathbf{Z}(-1)$ in the notation of my last letter. As for determining Φ^2 in general, this should more or less follow from the spectral sequence, taking into account the fact that the structure of the sheaf $\underline{\Phi}^1$ is known. I still do not have any ideas on the quadratic form, and I don't actually think I will give it any thought now. Maybe Kleiman, who is interested in numerical questions and seems very clever, might be interested in this question, if you want to discuss it with him.

My seminar will be called "ℓ-adic cohomology and L-functions". There will be a lot of duality and the passage to projective limit sorite, which runs the risk of being long (and rather unexciting).

Who is in charge of selecting the Russian texts translated by AMS? Someone should suggest they translate a certain number of things by Manin and Šafarevič, such as Manin's article on formal groups, which is a systematic exposition that should be made available to users who do not read Russian. Regards,

A. Grothendieck

November 8, 1964 Jean-Pierre Serre

Dear Grothendieck,

It is difficult to reply to your infinite letters: they would require infinite answers, which presupposes I have understood what you are talking about. As this is not the case, I will restrict myself to answering the trivial questions:

1) You can always write to AMS to suggest they translate any paper which you feel should be translated. I am sure they will be delighted. Furthermore, unless I am mistaken, Manin's article on formal groups appeared in Uspekhi Ak. Nauk, which is regularly translated (by an English journal, entitled "Russian Math. Surveys Translations", or something like that). When I left Paris, the nocontaining Manin had not yet appeared; it should have done so by now (but Harvard does not subscribe). So go and look in the IHP library, or even better, get IHES to take out a subscription to this journal. (On the other hand, no journal systematically translates the Izvestia Akad. Nauk, in which Manin and Šafarevič publish regularly — you could mention this to AMS.)

2) In your correction-letter of November 1st, you ask me to discuss your quadratic form with a certain "Kleiman"; the name is hard to decipher. Who is it?

3) Here is a suggestion of Tate's: when an abelian variety has purely multiplicative reduction (the extreme case), your quadratic form should be the one used to write down the (Morikawa-Tate) theta functions. In the elliptic case it should be $-v(j)n^2$, or something similar.

4) I feel too lazy, at least for today, to talk to you about heights. Besides, there is nothing very new to tell you. While I am on the subject, Mumford presented most of the Grauert-Manin proof to us (Grauert-style). Unfortunately, he got rather mixed up toward the end. He is to finish it next week; I hope things will become a little clearer.

5) Your results on the Frobenius action on the ℓ-adic π_1 are really amusing; for once, I had no problem reconstructing them! As for automorphisms of free pro-ℓ groups, and their use, the cleanest method consists of filtering them by the descending central sequence; the graded Lie algebra $\mathrm{gr}(F)$ is a *free* Lie algebra (cf. a recent Bourbaki draft), and since you know how Frobenius acts on the abelianization of the group (i.e. gr^1), you know how it acts on the whole of $\mathrm{gr}(F)$. This gives a little information (I am actually assuming that you know that $\mathrm{gr}^n F$ is motivic of weight $-n$ for all n — I am sure this is meaningful, and that it is obvious in your system). Of course, knowing the

Frobenius action on $\mathrm{gr}(F)$ is not at all equivalent to knowing it on F itself, and there I can no longer help you. In any case, it is false that the automorphisms of F which induce the identity on $F/(F,F) = \mathrm{gr}^1 F$ are inner: divide F by the n-th term F_n of the descending central sequence, so that F/F_n is a Lie group, and count the dimensions of the group of inner automorphisms and the group of automorphisms. It does not add up at all. (Note that finding the automorphisms of F or F/F_n is trivial: map the generators to anything at all that gives an automorphism in $F/(F,F)$).

The analogous question (on automorphisms) for a *discrete* free group is much more subtle. Nielsen has shown that the group of automorphisms of a free group is generated by "obvious" elements of the type $(x,y) \mapsto (x, x^{-1}y)$ and transpositions. I think (an old memory!) that someone has proved that J.-P. Serre : "that someone has proved that for two generators the answer is yes". This someone is none other than Nielsen himself: Math. Ann. **78** (1918), 385–397 and **91** (1924), 169–209. for *two* generators, the answer to your question is yes (any automorphism acting trivially on $F/(F,F)$ is inner) and for 3 or more it is no. I absolutely cannot guarantee this, however. Lazard and Cerf should know, or at least know a reference.

6) Your results on abelian varieties for which the Galois action on T_ℓ is commutative are interesting. Can you explain to me how you use Tate for the "descent" which shows that there are no parameters? For a number field with the (CM) hypothesis, I knew it was possible to find an abelian scheme over a finite extension (cf. page 3 of the letter to Ogg of August 8, of which you have a copy). Nevertheless, it is *false* that A has good reduction everywhere, i.e. that one can manage without enlarging the field. Here is a way to construct an example: take an elliptic curve E with complex multiplication whose only automorphisms are ± 1 (which is the general case!), defined over k. Assume it has good reduction at the place v. Now choose a quadratic extension k'/k of k, ramified at v; thus there is a homomorphism $\mathrm{Gal}(\bar{k}/k) \to \mathrm{Aut}(E)$. Use this homomorphism to *twist* E. You get an elliptic curve E'/k with the same modular invariant j as E, and just as much complex multiplication, and you can check that E' has *bad reduction* at v.

There is, however, an attractive positive statement in this special case: For any given v, you can find an elliptic curve E_1/k with the same j as E such that E_1 has good reduction at v. This was proved by Deuring J.-P. Serre : "proved by Deuring": not quite, cf. the letter of November 9, 1964. and is now very easy, using the criterion for good reduction which you know

(the action of $\mathrm{Gal}(\overline{k}/k)$ on T_ℓ is unramified when ℓ does not divide the residue characteristic). It is probable that this also holds for abelian varieties with Shimura-Taniyama complex multiplication (i.e. *conjecturally* when the action of $\mathrm{Gal}(\overline{k}/k)$ is abelian), but I have not checked this, lacking the necessary courage to plunge once and for all into Shimura-Taniyama.

Another positive special case: if A has enough complex multiplication, and its points of order 3 (say ...) are k-rational (or unramified), then A has good reduction everywhere. This is immediate by the same methods.

7) How do you prove the result on extensions of abelian schemes over $X - a$ when $T_\ell(A)$ is trivial?

Regards,

J-P. Serre

November 9, 1964 JEAN-PIERRE SERRE

Dear Grothendieck,

Let me return to the question of good reduction of elliptic curves with complex multiplication. In my letter of the day before yesterday, I wrote the following: "(If E is such a curve) you can find an elliptic curve E_1/k with the same j as E such that E_1 has good reduction at v (for a given v). This was proved by Deuring and is now very easy ... etc."

After sending the letter, I began to have doubts about the proof I had in mind, and these doubts have crystallized into a counterexample: Deuring's claim (Tokyo-Nikko, p.49, th.3) is therefore *false*.

Here is the situation:

Let $L = \mathrm{End}(E)$ and let $\overline{L}$ be the integral closure of L; set $L_0 = L \otimes \mathbf{Q}$. It is known that L_0 is a quadratic imaginary field; $\overline{L}$ is its ring of integers and L is a subring of $\overline{L}$ of rank 2 over $\mathbf{Z}$. (Incidentally, it is easy to classify all such rings: take an integer $f \geq 1$, and form $L_f = \mathbf{Z} + f\overline{L}$; every L is equal to one and only one L_f; the corresponding f is called the conductor for obvious reasons.) Let me write U (resp. $\overline{U}$) for the group of invertible elements of L (resp. $\overline{L}$). We will see that *Deuring's theorem is true when $U = \overline{U}$*. This is "a lot" of cases. Indeed, let $R_2 = \mathbf{Q}(i)$ and $R_3 = \mathbf{Q}(\rho)$ be the fields of the fourth and sixth roots of unity. If $U \neq \overline{U}$, then $\overline{U} \neq \{\pm 1\}$, so L_0 contains more roots of unity than is usual, and thus necessarily $L_0 = \mathbf{Q}(i)$ or $L_0 = \mathbf{Q}(\rho)$ (exercise!). Thus Deuring's theorem is true whenever L_0 is not one of these two fields, which is quite a few cases!

Assume thus that $U = \overline{U}$; here is the method for proving Deuring: consider the action of $\mathrm{Gal}(\overline{k}/k)$ on T_ℓ (for $(\ell, v) = 1$, where v is the given place); this action is commutative, and works via the group of units of the local (in fact, "semi-local"!) field $L \otimes \mathbf{Q}_\ell$. Let I_v be the corresponding inertia group; as you know, it acts via a *finite* group, independent of ℓ, which can be interpreted as a group of automorphisms of a reduction $\overline{E}_w$ of the curve E (over a suitable extension of k). It easily follows that I_v acts via a homomorphism $\varphi : I_v \to L^*$, or equivalently,

$$\varphi : I_v \longrightarrow \overline{U}.$$

(I am sure that this is just as obvious from your point of view, since the action of I_v must be "motivic". Anyway, it is easy to prove.)

But I assumed that $U = \overline{U}$. Let us thus choose a homomorphism

$$\psi : \mathrm{Gal}(\overline{k}/k) \longrightarrow U$$

whose restriction to I_v coincides with φ (this is possible, as can be seen using Kummer theory — beware of the fact that there is no general theorem saying

that the character of a local field is always induced by a character of the group of idele classes). J.-P. Serre : "beware of the fact that there is no general theorem saying that the character of a local field is always induced by a character of the group of idele classes". Indeed, it is sometimes necessary to slightly enlarge the group of values of the character, cf. Artin-Tate, *Class Field Theory*, Chap. 10, th. 5. If you now *twist* the curve E using ψ (or ψ^{-1} depending on the sign conventions), the action of I_v on the resulting curve becomes trivial, which means that the curve in question has good reduction at v.

Conversely, this shows how to get a counterexample when $U \neq \overline{U}$: namely, construct a curve E for which the homomorphism $\varphi : I_v \longrightarrow \overline{U}$ does not have values in U. But this is not very difficult: I have both a general method for constructing such examples, and numerical examples. (Note that Deuring's theorem can be true when $U \neq \overline{U}$, for instance if the place v does not divide the conductor f — I have not yet entirely worked out the details of the situation, but it does not present any difficulties.)

Abelian varieties of (CM) type can be dealt with in the same way: in any case, Deuring's theorem is true when $U = \overline{U}$ (with the corresponding notation), so it is true in particular if $\mathrm{End}(A)$ is the full ring of integers of the field in question.

For a general abelian variety of (CM) type whose endomorphisms are a subring of the ring of integers, note that in any case there exists an abelian variety A' isogenous to A (over the given field) whose endomorphisms are the integral closure of $\mathrm{End}(A)$. From this one can easily deduce the following *weak form* of Deuring's theorem:

For any A of (CM) type defined over k and any place v there exists B defined over k with good reduction at v which is $\overline{k}$-isogenous to A.

One could even consider a finite number of places if one wanted to.

There is something amusing about this situation: the elements of $\overline{U}$ are "motivic" automorphisms of the set-up, and are not necessarily actual automorphisms. This is the source of the hitch.

Yours,

J-P. Serre

P-S. Deuring's paper containing his proof is no 3 of his series in the Göttingen Nach. The theorem is stated on page 48. Deuring gives a 4 page argument (48-51) to prove that $\mathrm{End}(E)$ can be assumed integrally closed — which is false. I have not had the courage to unravel his notation enough to find out exactly where the error is.

November 14, 1964 Jean-Pierre Serre

Dear Grothendieck,

Let me come back to Deuring's wrong theorem. J.-P. Serre : Cf. [**ST68**], §6. Here is exactly the case where it is wrong:

Notation: $L = \mathrm{End}(E)$, $\overline{L} =$ integral closure of L, $f =$ conductor, $k =$ field over which E is defined, (along with its complex multiplication), $v =$ place of k. The aim is to determine whether there exists a curve E' over k, with the same j as E, which has good reduction at v. Answer; *yes* unless all possible bad luck occurs *at the same time*, i.e.:

   a) the fraction field L_0 of L is the field of fourth or sixth roots of unity,

   b) the conductor f is a power of a prime number p^e (with $e \geq 1$),

   c) the place v divides p (i.e. has residue characteristic p).

Conversely, if these unfavorable conditions are all satisfied, then it is possible to *choose* the field k so that the answer is *no*. (Note that actually, for a given L, there is a minimal field k over which such a curve exists — it is an abelian extension of L_0 whose Galois group can be described by class field theory.)

One of these days, I will have to write to Deuring — or write a note for the CR[13] — or both, on this kind of thing and the extension to abelian varieties of (CM) type.

Yours,

J-P. Serre

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[13] The Comptes Rendus (de l'Académie des Sciences de Paris.)

Wednesday afternoon Jean-Pierre Serre

Dear Grothendieck,

I looked at your problem on obstructions linked to the projective group J.-P. Serre : This is a reference to the topological analog of the Brauer group. On this subject, see Grothendieck's talk in the Bourbaki seminar, 1964/65, no290, th. 1.6. and have come to the — rather surprising — conclusion that there is no counterexample: If X is, let us say, a finite polyhedron, and $x \in H^3(X, \mathbf{Z})$ is a torsion class, there is a projective bundle over X whose invariant is equal to x.

The question comes down to studying the topology of the classifying space of PGL_n, as $n \to \infty$. It is however necessary to pay careful attention to the way in which the PGL_n (or the GL_n) are mapped to each other: take the filtered system of the integers (for divisibility), and map GL_n into GL_{nm} via

$$A \to 1_m \otimes A = (A, \dots, A);$$

passing to the quotient by $\mathbf{G}_m$ gives the desired embedding $\mathrm{PGL}_n \to \mathrm{PGL}_{nm}$.

It follows that the limit of the GL_n's (for example) is not at all the one which arises in Bott. However, the π_i of GL_∞ can be computed using Bott. One gets $\pi_i = \mathbf{Q}$ for odd i and $\pi_i = 0$ for even i (I really do mean "$\mathbf{Q}$" — it is quite rare for a homotopy group!). Those of PGL_∞ can be deduced from this: $\pi_1 = \mathbf{Q}/\mathbf{Z}$ and $\pi_i = \mathbf{Q}$ or 0 for $i \geq 2$ as above. This gives the homotopy groups of the classifying space B of PGL_∞:

$$\pi_2 = \mathbf{Q}/\mathbf{Z}, \quad \pi_4 = \mathbf{Q}, \quad \pi_6 = \mathbf{Q}, \quad \text{etc.}$$

Furthermore, the technique of k-invariants (i.e. the study of well-known successive fibrations) shows that such a space is homotopically equivalent to the *product* $K(\mathbf{Q}/\mathbf{Z}, 2) \times K(\mathbf{Q}, 4) \times \cdots$ Thereafter it is in the bag: if $x \in H^3(X, \mathbf{Z})$ is torsion, x is of the form dy where $y \in H^2(X, \mathbf{Q}/\mathbf{Z})$, which defines a map $X \to K(\mathbf{Q}/\mathbf{Z}, 2)$, and thence a map $X \to B$. Since B can be considered as the inductive limit of the B_{PGL_n}, we are done.

It is a bit strange — in particular, the inductive limit argument is a bit scary. However, I really have the impression that it works. Taking Milnor's construction for B_G (or adopting the semi-simplicial point of view), the inclusions $\mathrm{PGL}_n \to \mathrm{PGL}_{nm} \to \cdots$ give inclusions of the B, and the inductive limit is very harmless. I must admit that I would still be happy to find a more down-to-earth proof of this bizarre result.

Regards,

J-P. Serre

May 2, 1965 JEAN-PIERRE SERRE

Dear Grothendieck,

I am returning to the subject of our last telephone conversation: the Brauer group.

1/ The "descent" argument I told you about goes as follows: let L be a (not necessarily commutative) field, G a group of automorphisms of L, and K the subfield of fixed points. Let V_0 be a vector space over K, and let $V_0 \otimes_K L = V$ be its extension by L (let us say these are right vector spaces). Let W be an L-vector subspace of V. For W to be of the form $W_0 \otimes L$, it is necessary and sufficient for it to be stable under G acting in the obvious way on V.

This result is contained in Bourbaki J.-P. Serre : "This result is contained in Bourbaki". It is not in the new edition, but can be easily deduced from th. 1 of A II, §8. (Algebra II, 3ème éd., p.188 — the theorem stated there is actually more general) and is very easy to prove. Note that even for commutative fields, the statement is much more general than those which limit themselves to simply translating faithfully flat descent: for instance, L and K can have different transcendence degrees. [In terms dear to your heart, there is a functor $V_0 \mapsto V$ from K-vector spaces to L-vector spaces equipped with G actions; the theorem, if rewritten in these terms, says that this functor is fully faithful. But it is not generally an equivalence of categories.]

Application: take L to be a skew field, K its center, and G the group of inner automorphisms. The result I told you about on the telephone then follows easily: if A_0 is a K-algebra, then the two-sided ideals of $A_0 \otimes L$ are those coming from A_0. Note that I have not assumed $[L : K]$ to be finite, so that this result cannot be deduced by descent from the case of matrix algebras.

This trick of thinking of a skew field as being "Galois" over its center is really very nice. As far as I can see, it is due to Emmy Noether (Math. Zeit., 1933). Deuring and van der Waerden copied it piously into their books.

2/ I feel like telling you a bit of the history of the Brauer group and factor systems. J.-P. Serre : This little "historical note" on Brauer cohomology and groups should be corrected and completed.

a/ Factor systems — due to Schur (Crelle, 1904). Schur is interested in the following problem: projective representations of a group. He sees at once that there is an obstruction to lifting to the linear group; he interprets it as a system a_{st} of complex numbers of absolute value 1 (if so desired) modulo the equivalence relation we know. He makes this into a group, shows that it is finite, killed by the order of G (assumed to be finite) and that every element

of this group appears as an obstruction; he observes that this group plays a universal role in classifying the central extensions of G. He computes it in some trivial cases and in some not at all trivial cases (alternating groups, symmetric groups, the group $SL(2, \mathbf{Z}/p^n\mathbf{Z})$ — see Crelle, 1907, 1911). Bibliography: van der Waerden, Gruppen von linearen Transformationen, §21. Note however that in these factor systems the group only acts trivially on the coefficients.

The case of a non-trivial action — and its interpretation by extensions — is, I believe, due to Schreier (Abh. Hamburg, 1926). There may, however, have been some interaction between him, E. Noether and R. Brauer.

b/ Using factor systems for simple algebras. This is due to Brauer; its history is amusing:

Brauer was interested in the following problem: write a given irreducible representation over a given number field K, which of course contains the values of the character. This problem had been considered by Schur and Frobenius, who had obtained very non-trivial results on it. In particular, Schur had recognized that the obstruction comes from the occurrence of skew fields (whose degrees give what are known as the Schur indices). Brauer takes the point of view of Galois descent which is familiar to us: writes the representation over a larger field L/K, and if M is this representation, then $M^\sigma \cong M^\tau$ etc. Actually, theorem 90 was really necessary at this point; it so happens that (in its factor systems form) it had been proved by Speiser in 1919 — it thus seems (contrary to what I said above) that factor systems with non-trivial actions must have been known in 1919; unfortunately, I do not have enough documents at hand to see if they are due to Speiser or to Schur himself (which is what I would tend to think). In any case, it was definitely Brauer (Sitz. Berlin, 1926) who was the first to see Galois factor systems cropping up as obstructions to a typical Galois descent.

In this memoir from 1926, only representations of finite groups are considered, and not simple algebras. But Brauer is perfectly well aware that the obstruction described by his factor systems is also described by a skew field. Comparing the two points of view, he realizes that factor systems allows him to write down the skew field in question (or rather the matrix algebra over it which occurs in the problem), and he finally notices that this has nothing at all to do with finite groups (Crelle, 1932).

Brauer's factor systems are "homogeneous" in three variables and are defined for any separable extension; the passage to the non-homogeneous case in the

Galois situation is due to Emmy Noether — or at least so Deuring says, but I do not know whether she published anything.

Yours,

J-P. Serre

August 27, 1965 ALEXANDRE GROTHENDIECK

My dear Serre,

I am still turning algebraic cycles classes every which way in my head; from a technical point of view, I now see things more clearly, but the final breakthrough is still missing. As I know you are allergic to cohomologyJ.-P. Serre : "As I know you are allergic to cohomology. . . ". Allergy, certainly not. Indigestion, perhaps?, I would like to show you the two key conjectures J.-P. Serre : This is a reference to the "standard conjectures"., which should be proved purely geometrically, i.e. without reference to cohomology. Let X denote a smooth connected projective n-dimensional variety over an algebraically closed field k, and Y a smooth hyperplane section. Let me denote by $C^i(X)$ the group of cycle classes of codimension i modulo algebraic equivalence, tensored with $\mathbf{Q}$, and by $\xi \in C^1(X)$ the class of Y.

CONJECTURE A: *For every integer i such that $2i \leq n$, the product with ξ^{n-2i} induces an isomorphism $C^i(X) \cong C^{n-i}(X)$.*

Modulo a verification which I have not written down in detail, to prove this conjecture it would be enough to prove surjectivity, and in fact to do it for $n = 2i + 1$. For the moment, the first case that eludes me is $i = 1$, $n = 3$. This conjecture would imply the analog, for the C^i, of the well-known Lefschetz theorems comparing the cohomologies of X and Y under the direct image and inverse image homomorphisms (to get the correct formulation all you need to remember is that C^i becomes H^{2i}), and it would simultaneously imply the cohomological Lefschetz theorems themselves, whose formally strongest form consists of asserting that ξ^{n-i} induces an isomorphism between H^i and H^{2n-i} for the cohomology with coefficients in $\mathbb{Q}_\ell$; let me point out that even this purely cohomological theorem is not yet proved! J.-P. Serre : "is not yet proved!". It was proved 15 years later by Deligne: *La conjecture de Weil* II, Publ. Math. IHES **52** (1980), 313–428, th. 4.1.1. The only thing that has been proved is the "weak" Lefschetz theorem, which says that the cohomological dimension of an affine variety of dimension n is bounded by n, which, for the affine variety $U = X - Y$ takes the form that $H^i(X) \to H^i(Y)$ is bijective for $i \leq n - 2$, and injective for $i = n - 1$, or alternatively $H^i(Y) \to H^{i+2}(X)$ is bijective for $i \geq n$ and surjective for $i = n - 1$. In fact, unless I am mistaken, I can deduce conjecture A from its following weaker form (which looks like a "weak" Lefschetz theorem):

CONJECTURE A′: *For every integer i such that $2i \geq n - 1$, the direct image map $C^i(Y) \to C^{i+1}(X)$ is surjective, i.e. any algebraic cycle of dimension $j < n/2$ on X is τ-equivalent J.-P. Serre : "τ-equivalent" = equivalent up to torsion, i.e. after tensor product with $\mathbf{Q}$. to the image of an algebraic cycle with rational coefficients on Y.*

Once again, unless I am mistaken, it is enough to work with $n = 2i + 1$. Moreover, in the argument used to reduce A to A′, it seems necessary to prove A′ over a not necessarily algebraically closed base field and for cycle classes which are rational over it, *or* restrict to stating A for cohomological equivalence alone (but A then loses the "purely geometric" nature I promised you!)

As I told you, A implies "the Künneth formula for cycles" (actually, I exaggerated a bit when I claimed that the two statements were equivalent). This then implies all the integrality theorems one could wish for (on coefficients of characteristic polynomials, for example), except that it seems possible to have powers of $p=$ char. k in the denominator. This therefore implies the Weil conjectures, except for the question of the absolute values of eigenvalues, which will be covered by B. Note also that conjecture A appears to be the "minimum minimorum" to be able to give a usable rigorous definition of the concept of a motive over a field.

CONJECTURE B: *Assume that $n = 2m$, and let $P^m(X)$ be the kernel of the multiplication by ξ homomorphism from $C^m(X)$ to $C^{m+1}(X)$. Then the form $(-1)^m \epsilon(xx')$ on $P^m(X)$ is positive definite.*

For the Weil conjectures, it would actually be enough to show that this form is positive. But this stronger formulation also implies other attractive results, such as the fact that τ-equivalence = numerical equivalence (= homological equivalence with $\mathbb{Q}_\ell$ coefficients, since this one is sandwiched in between the other two), and the fact that the $C^m(X)$ are finite-dimensional, — and in fact, that the groups of τ-equivalence classes of cycles are finitely generated. (From this, one can formally deduce that that $C^m(X) \otimes \mathbb{Q}_\ell \to H^{2m}(X)(m)$ is injective, and thus the rank of $C^m(X)$ is bounded by $b_{2m}(X)$.) Furthermore, B also implies that the category of motives constructed from non-singular projective varieties is semi-simple, and in more down-to-earth terms, that the ring of classes of algebraic correspondences on X (modulo τ-equivalence and tensored with $\mathbf{Q}$) is a finite semi-simple algebra over $\mathbf{Q}$ which can in fact be equipped with an involution and a trace satisfying the usual conditions. One also gets the following result, which may be viewed in some sense as a generalization

of A (assuming that the cohomological version of A is already proved): Let $H^{2i}(X)(i) \to H^{2i+2r}(X')(i+r)$ be a homomorphism defined by an algebraic correspondence class, and let $C^i(X) \to C^{i+r}(X')$ be the homomorphism it defines on cycles. Then an element of $C^{i+r}(X')$ lies in the image of $C^i(X)$ if and only if its image in H^{2i+2r} lies in that of H^{2i}. Note also that the semi-simplicity result mentioned above would imply the analogous semi-simplicity result for the Galois group actions in the Tate conjectures. Finally, I have more or less convinced myself that A and B also imply a reasonable theory of the abelian varieties which appear as parameter varieties of continuous families of algebraic cycles, and in particular allow us to obtain the necessary relations between these "intermediate Jacobians" (which can be viewed as "algebraic pieces" in Weil's Jacobians) and the cohomology in odd degree. It is however quite possible that the proof of B will itself be linked to the introduction of these abelian varieties.

In any case, A and A$'$ seem to me to be in some sense preliminary to B, and it seems reasonable to start with them. Let me point out a rather suggestive statement which is equivalent to A$'$:

CONJECTURE A$''$: *Let U be the n-dimensional affine open set $X - Y$, then $C^i(U) = 0$ for every i such that $2i > n$.*

In fact, it is possible that this holds for any smooth affine variety, and even that for any smooth variety (affine or not), every cycle which is cohomologically equivalent to 0 is τ-equivalent to 0 (as I said, this would follow for projective varieties from theorems A and B).

For the moment, what is needed is to invent a process for deforming a cycle whose dimension is not too large, in order to push it to infinity. Perhaps you would like to think about this yourself? I have only just started on it today, and am writing to you because I have no ideas.

Yours,

A. Grothendieck

December 7, 1966 JEAN-PIERRE SERRE

Dear Grothendieck,

You asked me some time ago if the K of representations of GL_n over $\mathbf{Z}$ is "what one expects", i.e. the same as over $\mathbf{Q}$. It seems to me that this is true, and follows easily from J.-P. Serre : I published this result in [**Se68**]. Chevalley's theory, the key point being that representations of GL_n in characteristic p can "almost" be lifted to representations over $\mathbf{Z}$.

More precisely, let G be a split connected reductive group scheme over a principal ring (shame on me!) A with fraction field E (I have to restrain myself from calling this field $K\ldots$). Consider two representations L_1 and L_2 of G over A (given by free modules of finite type over A), corresponding to the same representation of G over E. I have to prove that $[L_1] = [L_2]$ in the K-group of representations of G over A. This will prove that this K is the same as over E (and the latter has essentially been determined by Chevalley).

By dévissage, I may assume there is an extremal element p in A such that $L_1 \supset L_2 \supset pL_1$, and that in addition, the representation of G on L_1/L_2 is irreducible. Let k be the field A/p. I can view L_1/L_2 as a representation of the group $G_{/k}$.

Let us say that a representation of $G_{/k}$ "lifts to A" if it can be written in the form L/pL, where L is a representation of G over A. The non-formal property of G that I need is the following:

LEMMA: *Let M be an irreducible representation of $G_{/k}$. Then M can be written as a quotient $M = M_1/M_2$, where M_1 lifts to A and the only irreducible modules appearing in the Jordan-Hölder sequence of M_2 are of highest weight (strictly) less than that of M.*

(I say that a highest weight is less than another one if their difference is a combination of roots with coefficients ≥ 0. Of course, all this presupposes a preliminary pinning of the group G.)

This lemma must follow from Chevalley's theory, but I must confess I would be a little embarrassed if you were to ask me for a precise reference. The idea is obviously to take the highest weight ω of M, and use it to construct a linear representation of G over A (by taking the invertible sheaf associated to ω over G/B); if M_1' denotes this representation, then define M_1 to be the reduction modulo p of M_1'. There is one problem: is it really true that M_1' is free over A? I am afraid we may find ourselves facing the unresolved problem of the vanishing of the H^1 of G/B. In short, I do not totally guarantee this lemma, but either you or Chevalley will certainly be able to do so.

Admitting this, let me return to my original problem. Here is another lemma, which is now really trivial:

OBVIOUS LEMMA: *Let $L_1 \supset L_2$ and $L'_1 \supset L'_2$ be representations of G over A. If $L_1/L_2 \simeq L'_1/L'_2$, then $[L_1] - [L_2] = [L'_1] - [L'_2]$ in the K-group of representations of G over A.*

Let M be the sub-object of $L_1 \times L'_1$ consisting of elements which have the same image in L_1/L_2 and in L'_1/L'_2. One may view M as an extension of L_1 by L'_2 and also of L'_1 by L_2. Whence etc.

Let me now go back to the original situation, with

$$L_1 \supset L_2 \supset pL_1,$$

and L_1/L_2 irreducible. The aim is to show that $[L_1] = [L_2]$. One may assume this is already known for all analogous situations with irreducible $G_{/k}$-modules with highest weight strictly less than that of $L_1/L_2 = M$. Applying the non-obvious lemma, one sees that there are $L_3 \supset L_4 \supset pL_3$ such that $L_3/L_4 = M$, and L_4/pL_3 is a successive extension of irreducible $G_{/k}$-modules of highest weight strictly less than that of M. Given my induction hypothesis, I then have $[L_4] = [pL_3] = [L_3]$. On the other hand, the obvious lemma says that $[L_1] - [L_2] = [L_3] - [L_4]$. The result is in the bag!

(Note that "A principal" has been used to assert that $[pL_3] = [L_3]$. For Dedekind rings, the ideal class group of A would have appeared.)

Of course, one sees in particular that the K of representations of GL_n over $\mathbf{Z}$ is a special λ-ring (since it is the same as over $\mathbf{Q}$); whence, as you pointed out to me, it follows that all λ-rings in nature are special (I am exaggerating a little!)

As this down-to-earth method of considering K has probably disgusted you, let me try and sell you something prettier. I will keep the notation above, but will weaken my hypotheses by assuming only that:

a) A is Dedekind;

b) G is a flat group scheme[14] of finite type over A (but is not necessarily reductive, nor split).

Let $K_A(G)$ and $K_E(G)$ denote the K-groups that you guess (for A, I take those representations which are finitely generated projective A-modules); if $\mathfrak{p}$ is a maximal ideal of A, I write $K_{\mathfrak{p}}(G)$ for the K-group corresponding to the group $G \otimes A/\mathfrak{p}$. I would like to define, just as in Brauer theory, an exact sequence:

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[14] Note in the margin: Perhaps one should strengthen these hypotheses to "locally free over A"?

$$\coprod K_{\mathfrak{p}}(G) \to K_A(G) \to K_E(G) \to 0.$$

The homomorphism $K_A(G) \to K_E(G)$ is defined in an obvious way. It is certainly surjective (even though I do not see why!).

The homomorphisms $K_{\mathfrak{p}}(G) \to K_A(G)$ are much less obvious. Starting with a $G_{/A/\mathfrak{p}}$-module M, one has to show that it can be written as a quotient L_1/L_2, where L_i is a representation of G over A (i.e. a finitely generated projective module over A on which G acts) such that $L_1 \supset L_2 \supset \mathfrak{p}L_1$. (In other words, every representation of G over $A/\mathfrak{p}$ is a *quotient* of a liftable representation. This seems to me to be a good general result to know. Proof: reduce to the case of representations contained in the Ind-representation given by the algebra of coordinates of $G_{/A/\mathfrak{p}}$, for which it is obvious. Note also that by duality, you can replace "quotient" by "sub-object".) This being so, map M to the difference $[L_1] - [L_2]$, which by the obvious lemma above does not depend on the choice of the L_i. It is immediate that one thus obtains a homomorphism from $K_{\mathfrak{p}}(G)$ to $K_A(G)$. The exactness of the sequence written above is now clear.

Continuing to imitate Brauer: for every $\mathfrak{p}$, there is a reduction homomorphism

$$r_{\mathfrak{p}} : K_E(G) \to K_{\mathfrak{p}}(G)$$

characterized by the fact that the composition $K_A(G) \to K_E \to K_{\mathfrak{p}}(G)$ is the obvious homomorphism (coming from $A \to A/\mathfrak{p}$).

My initial proof can now be reformulated as follows:

THEOREM: *Assume A is principal and all the $r_{\mathfrak{p}}$ are surjective. Then $K_A(G) \to K_E(G)$ is an isomorphism.*

Indeed, it is enough to prove that $K_{\mathfrak{p}}(G) \to K_A(G)$ vanishes. However, by assumption, $K_p(G)$ is generated by classes of elements of the form $L/\mathfrak{p}L$, where L is a representation of G over A; since A is principal, one can choose a generator p of $\mathfrak{p}$; then $[\mathfrak{p}L] = [L]$, whence etc.

One would very much like to know when the $r_{\mathfrak{p}}$ are surjective. Chevalley's theory says they are isomorphisms when G is reductive and split (unless I am wrong — it is a shame this was not clarified in SGAD). This is therefore "geometrically true" when G is reductive non-split. For finite groups, the situation is similar: $r_{\mathfrak{p}}$ is geometrically surjective, i.e. becomes surjective after finite extension of the field E (this is one of Brauer's prettiest results; I gave a talk on it in Bures last year).J.-P. Serre : This talk in Bures became the third part of *Représentations Linéaires des Groupes Finis*, Hermann, Paris, 1968.

Some brave soul should investigate non-connected reductive groups, and prove that $r_{\mathfrak{p}}$ is again geometrically surjective; that would be quite satisfactory.

Take the above with a grain of salt: it is quite possible, given my lack of familiarity with group schemes, that I have screwed up somewhere.

Yours,

J-P. Serre

January 15, 1969 Alexandre Grothendieck

My dear Serre,

I have been thinking about what you told me about Steinberg's theoremJ.-P. Serre : "Steinberg's theorem". This theorem says that $H^1(k, G) = 0$ if k is a perfect field of cohomological dimension 1 and G is a connected linear k-group. See:

R. Steinberg, *Regular elements of semisimple algebraic groups*, Publ. Math. IHES **25** (1965), 49–80 (= R. Steinberg, C.P. no20)., and I have some questions and comments.

1) A priori, what the argument you gave me shows is that (for semi-simple connected G over k) every element of $H^1(k, G)$ comes fromJ.-P. Serre : "every element of $H^1(k, G)$ comes from some $H^1(k, T)$": Here it is assumed that G is quasi-split, i.e. that there exists a k-subgroup of G which is a Borel group, cf. Steinberg, *loc. cit.*, th. 1.8. some $H^1(k, T)$, where T is the *centralizer* of a regular element of $G(k)$. I doubt that this centralizer is necessarily a maximal torus in general (in any case this is false for a non-simply connected group such as J.-P. Serre : It seems that here $GP(1)$ denotes $\mathbf{PGL}_2$, even though Grothendieck usually denotes the group $\mathbf{PGL}_n$ by $GP(n)$. $GP(1)$), but in fact it is true for a "sufficiently general" regular element, which is sufficient for the argument you use. Precisely, if g is a semi-simple regular element of $G(k)$, G smooth and connected (not necessarily semi-simple) over k, then what I have already done in the seminar (the regularity criterion via the Killing polynomial) implies that its centralizer $Z(G)$ is a *smooth* subgroup of G, and of course $Z(g)^0$ is the unique Cartan subgroup C containing g; furthermore $Z(g) \subset N(C)$. Thus, for a given maximal torus T, the elements of $T(k)$ whose centralizer is exactly $C = \mathrm{Centr}(T)$ are the elements of $T(k)$ which are regular in G and which are not fixed points of geometric elements $\neq 1$ in $W(T) = N(T)/T$. In fact, in the case where G is semi-simple, one immediately sees that the fact of not being a fixed point of W already implies regularity (as I said above, the converse is not true in general). One could use the term "strictly regular" to describe the semi-simple points of $G(k)$ whose centralizer is a Cartan subgroup; they form an open subset of the variety of regular semi-simple points, hence an open subset of G when the unipotent rank of G is zero ($T = C$).

Thus, to correct the argument you indicated to me, it seems that "regular" must be replaced by "strictly regular". Apart from this, it is of course not necessary for k to be perfect, provided Steinberg's result has been proved over k (for a quasi-split group).

2) "A priori", the method in question — taking two rational points g and g' in G and in the group G' obtained by twisting G, which are strictly regular and "conjugate" — gives *every* possible way of restricting the structural group of the homogeneous principal G bundle P to a maximal torus. (This remark remains valid whenever G is smooth and connected over k, provided "maximal torus" is replaced by "Cartan subgroup"). Moreover, Steinberg's theorem (every "sufficiently general" rational orbit has a rational point) only holds for G' if G' comes from a class $H^1(k, G)$ which is zero, i.e. if G' is itself quasi-split. Indeed, applying Steinberg's argument backwards, one concludes that the structural group of P can be reduced to any given torus, but there exists such a T (the one contained in the Borel) whose cohomology is trivial, as you brilliantly observed.

3) Is Steinberg's theorem (in the form that says that rational points exist on rational orbits, say) only true for simply connected groups, and is this also true of the corollary which states that the structural group can always be reduced to a maximal torus? What happens for $GP(1)$, for instance? Is there a rational section of G over $I(G)$ ("invariants") in this case? J.-P. Serre : Steinberg's paper quoted in note 183.1 certainly makes it possible to answer the questions asked here by Grothendieck.

4) I have been amusing myself by looking at the construction of a quotient $G/\mathrm{ad}(G)$ for smooth connected G over k. Maybe it always exists: in any case, I have checked it for affine G of trivial unipotent rank; then for any maximal torus T with Weyl group W, one has

$$T/W \overset{\sim}{\to} G/\mathrm{ad}(G).$$

Setting $I(G) = G/\mathrm{ad}(G)$, this gives a canonical morphism

$$I : G \to I(G),$$

and one is very tempted to study this (geometrically) Kostant-style (in the semi-simple case), for instance: if U is the open set of points where I is smooth ("quasi-regular points"), then for every $s \in I(G)$, U_s is the unique open orbit of G in $O_s = I^{-1}(s)$; the orbits of $\mathrm{ad}(G)$ in G are of even dimension, which would already imply that the O_s are normal varieties, whose set of singularities would be $O_s - U_s$, and the quasi-regular points of G will be those whose centralizer is of dimension $r = \mathrm{rank}$. The open set $G^{\mathrm{str\,reg}}$ in G is probably the inverse image of an open set $I(G)^{\mathrm{str\,reg}} = T^{\mathrm{str\,reg}}/W$ in such a way that $G^{\mathrm{str\,reg}}$ becomes a homogeneous space over $I(G)^{\mathrm{str\,reg}}$ of group $G_{I(G)^{\mathrm{str\,reg}}}$. Does the

analogous statement with "strictly regular" replaced by "regular" hold? Are regular (or strictly regular) points characterized by the fact that their orbits under $\mathrm{ad}(G)$ are closed? Can the set O_0 be identified with the set of unipotents of G? When G is an adjoint group, is it possible to generate the affine ring of $I(G)$ with coefficients of the Killing polynomial? In the general case, is it enough to take the coefficients of analogous polynomials for certain linear representations (perhaps arbitrary faithful representations)? Etc.

5) Is it true that $I(G)$ is a rational variety when G is semi-simple over k? It is obviously enough to assume that G is quasi-split; in the split case, one even gets a space $\mathrm{Spec}\, k[t_1, \ldots t_r]$!

I would like to ask Steinberg to come to lunch on Tuesday in Bures. Will you be at Borel's on Monday morning, and will he be there, so that you can introduce us to each other?

I have had a letter from Remmert with a message from Grauert, who (he says) is seriously ill, so that he hasn't been able to put the finishing touches to a manuscript which he promised me for the Publications, containing a proof of geometric Mordell in arbitrary characteristic. I had raised the question of lecturing on his proof in the Bourbaki seminar, and he suggests putting his ("mathematically complete") manuscript at our disposal. As I believe that you wanted to talk about Manin, and Grauert seems to go farther by a purely algebraic method, it might interest you to give this talk. Another possibility would be for Artin to give it J.-P. Serre : It was Samuel who gave this talk, cf. note 87.2. (he was with me when Grauert explained his proof to us, but we didn't think about it enough to understand the key point), but actually he already has a lot of work, and masses of talks to prepare. . .

Yours,

A. Grothendieck

September 2, 1984 Alexandre Grothendieck

J.-P. Serre : My correspondence with Grothendieck was interrupted when he retired from the mathematical community in 1971. This letter was the first one I received from him after that.

Dear Serre, Thank you for sending me the information on the Peccot course and the invitation. J.-P. Serre : "the invitation". The Collège de France organized a *Colloque Peccot* in 1985, to which all the previous Peccot lecturers were invited; this gave me a good opportunity to write to Grothendieck. As you probably know, I no longer leave my home for any mathematical meeting, whatever it may be.

I am going to take advantage of the opportunity to ask you for two pieces of information, if you can give them to me. 1) Where can I write to Leray? (Is he still at the Collège?) 2) Do you know Delsarte's first name?

I have just written a retrospective reflectionJ.-P. Serre : "retrospective reflection": *Récoltes et Semailles*. "on my past life as a mathematician", which is currently being typed, and this is where I would like to inculde Delsarte's first name*. When it is printed, I intend to send a copy to various people, old friends and students, and in particular to the members of Bourbaki I knew (so you are on the list), and also to Leray. As it happens, you are mentioned at several points in the reflection, and you are one of the "elders" to whom it is dedicated (along with my former students), which makes a total of three reasons for sending it to you. . .

Hoping to hear from you soon, cordially yours,

A. Grothendieck

* So, if you know it, it would be good if you could give it to me quickly. Please note my telephone no: (16/90) 61 88 30. Address : Les Aumettes, 84570 Mormoiron.

July 23, 1985 JEAN-PIERRE SERRE

Dear Grothendieck,

Thank you very much for the first chapters of "Récoltes et Semailles" which you sent me. They arrived just before I went to Luminy (for a meeting on numerical methods in number theory — a subject certainly far removed from your concerns) and I took advantage of the trip and my stay there to look at them, in some detail. (I say nevertheless that I "looked" at them, since I could not compel myself to read them in order, and line-by-line. The text is really too repetitive to be readable that way. You wrote it as a kind of "diary", and one can hardly do otherwise than dip into it locally).

There would be much to say about the content. As I told you on the phone (which damaged me in your eyes, cf L. 26), I am sad that you should be so bitter about Deligne, who is one of the most honest mathematicians I know — and one of those who cares most about you. I will not try to change your mind on this subject (nor indeed on any other subject): I know too well the strength and rigidity of your convictions. This is probably what I find most painful in your text. That, and the general tone of recrimination towards both yourself and your former students. Moreover, you must have unconsciously recognized this recriminatory tone, since you tried to exorcise it (L.23):

". . . which has prevented my testimony (I believe) from ever veering into sterile recrimination. . ."

This page is actually remarkable for the next sentence, which is so contradictory that one wonders how you could have typed it without laughing: "This absence of complacency with respect to myself. . ."

How can you?

Among the questions you bring up, there are:

—Mebkhout and perverse sheaves. There must be about a hundred pages on this subject, containing the curious expression "the Good Lord's theorem" which I had great difficulty understanding (I finally realized that "Good Lord's" meant it was a beautiful theorem). As I have never tried to understand what a "perverse sheaf" is (I find the term "perverse" just as unpleasant as you do: this is one point we agree on), I am not competent to comment.

— The publication of SGA 5 and SGA 4 1/2. This publication seemed very useful to me. The existing notes of SGA 5 were both incomplete and unconvincing: I remember a talk by Illusie on the trace formula where he confessed to having been unable to prove compatibility of all the necessary

diagrams. It is not enough to claim that the diagrams one writes "should" commute — especially when things as important (for me...) as Weil's or Ramanujan's conjectures depend on them! (If the natural diagram "algebraic duality= analytic duality" commuted, it would follow that π is algebraic, as you know – and even that $2\pi i = 1$.)

As for the fact that no one wrote up your conjectures lecture, it is perfectly natural: only the author can write such a text. I imagine with horror someone trying to write up a talk in which I had made conjectures (on ℓ-adic representations, for example); I am sure that half of them would be false!

— The fact that your work was not continued by your former students. You are right: they did not continue. This is hardly surprising: it was you who had a global vision of the project, not they (except Deligne, of course). They preferred to do other things. I do not see why you should reproach them with this.

As for Deligne, he moved little by little towards questions that go beyond the framework of algebraic geometry: modular forms, representations, the Langlands program. And he applied his deep understanding of algebraic geometry (including "motives") to various questions — for example the construction with Lusztig of many (not all...) representations of the groups $G(\mathbf{F}_q)$ for reductive G. Why should he not have used the yoga of "motives"? You introduced it, everyone knows that, and everyone has the right to use it — provided one carefully distinguishes what is conjectural (and perhaps even false, until proof of the contrary) from what can be proved. For example, I found very beautiful what Deligne does in LN 900 (the text you reject with horror...) to get around the problem of Hodge cycles and obtain highly useful results nevertheless (on ℓ-adic representations, for example). I know that the very idea of "getting around a problem" is foreign to you — and maybe that is what shocks you the most in Deligne's work. (Another example: in his proof of the Weil conjectures, he "gets around" the "standard conjectures" — this shocks you, but delights me).

(As a matter of fact, despite what you said in L28, my way of thinking is not very different — depth excepted — from Deligne's. And it is also quite distant from yours — which, moreover, explains why we complemented each other so well for 10 or 15 years, as you say very nicely in your first chapter.)

On the topic of nice things, I very much liked what you say about the Bourbaki of your beginnings, about Cartan, Weil and myself, and particularly

about Dieudonné. Perhaps you would like some recent news of these people? Here is some:

Cartan is 81, and has not changed. He is as lively as ever, it is a pleasure to see him. He is not doing any real math, but takes care of what I call "good works" (mathematicians in prison or mental institutions, URSS-style). He has a programmable SHARP calculator with which he plays a great deal; recently he asked me some non-trivial questions on units in real quadratic fields.

Weil is somewhat tired physically (he is literally dragging his feet) but not at all intellectually. He has stopped doing math, but continues to be interested in its history. You may have seen his very beautiful book "Number Theory — An approach through history from Hammurapi to Legendre" which was published in 1983 by Birkhäuser. I was hoping he would write a sequel (on Gauss, Jacobi and the others), but I do not think he will. According to what he tells me, he is presently taking care of editing the collected works (or correspondence?) of one of the Bernoullis. J.-P. Serre : Reference: *Der Briefwechsel von Jacob Bernoulli*, A. Weil. réd., Birkhäuser Basel, 1993.

Dieudonné, as you surely know, underwent several operations some years ago. They tired him a lot, and he has lost that "force of nature" quality he used to have. He finished writing his grand treatise on Analysis (9 volumes, culminating with Lie groups and elementary differential topology), and he is now working on a history of Topology. J.-P. Serre : Reference : J.Dieudonné, *A History of Algebraic and Differential Topology 1900–1960*, Birkhäuser Boston, 1989.

A lot more could be said about your text. I will restrict myself to typographical questions:

— I like the characters of your typewriter (or the secretary's typewriter?) very much. They are really very pretty, and very legible.

— For the parts that you want to emphasize, you use an interval between the letters:

"P u r s u i n g   S t a c k s", for example. This is not very legible. Have you tried half-intervals:

"P u r s u i n g   S t a c k s"?

This is a little better (but harder to produce on a typewriter).

—"cru" only takes a circumflex when it is the past participle of the verb croître "j'ai crû en sagesse", but "j'ai cru en la conjecture de Hodge" (and,

oddly enough, one says "Le cru et le cuit" without a circumflex: it is illogical, but that is how it is.)[15]

— For the word "ça", I find disagreeable your habit of omitting the cedilla underneath a "Ça" with a capital C: why don't you type a comma underneath the "C", as I just did?

— ambiguë and not ambigüe.

— Ramanujan and not Ramanuyam (p.192, 195, 225).

— (p.263) 355/113 and not 344/133 (which would be < 3, contradicting what you just explained!).

Regards,

J-P. Serre

PS — In your letter, you express surprise at the difference in tone between my two letters. Here is the explanation:

In my first letter, I told you about my own results from the last 15 years or so, and I spoke without excessive enthusiasm — which corresponds to what I think of them.

In the second, I talked about what has been done in mathematics recently, and thus I had every right to be extremely enthusiastic: we are living in a remarkably rich period, at least as rich as the one you knew.

Whence the difference in tone.

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[15] The terms in French mean, respectively, to grow, "I grew in wisdom", "I believed in the Hodge conjecture" and "the raw and the cooked"

July 25, 1985 Alexandre Grothendieck

Dear Serre,

I was glad to get your letter this afternoon, in which you gave me your first impressions on reading Récoltes et Semailles. As I might have expected, you rejected everything in the testimony which could be unpleasant for you, but that did not prevent you from reading it (partially, at least) or from "taking" the parts you find pleasant (those that are "nice", as you write!). I was not at all convinced that you would read past the Introductory Letter giving the tone of it. One thing that had already struck me about you in the sixties (or maybe even earlier) was that the very idea of examining oneself gave you the creeps — I still remember how flabbergasted I was to hear you say one day, I no longer remember in what context, that in the very fact of saying something negative about oneself you sensed a "cynicism" (this was the expression you used) on top of the negative thing in question (assumed to be true). I do not believe I discussed the matter with you then — I must have sensed for once (which was not usual for me then) that it was a lost cause. I remained with an impression of surprise, as if faced with a mystery. . . . And yet, at the time, and up until these last (nine) years, it was not usual for me either, to risk taking a close look at myself (or at anyone else, to be frank). But the idea that it is possible to do so, going beyond a "nice" façade, did not inspire any visceral rejection in me like the one I observed in you. This did not, perhaps, change much, since I was fundamentally convinced (without even having ever had to ask myself the question) that in my own case, at least, there was no façade — that my ideas (or rather, my intimate convictions, since that was how I perceived them) about myself were nothing other than the faithful reflection of the real truth of what I actually was. In this, as of course in many other things, I was not at all different from you or anyone else.

The fact remains that I know that even if you were not personally (and by name) implicated in my reflection, it could not do otherwise than inspire very strong reactions of defense and rejection in you, by its very nature. My aim is not to be "nice" or "not nice" any more than it was in math, but to discover a truth, which I "express" as I continue my probings, as a means of discovery. You call my text (or my method?) "complacent", and this is probably another of your intimate convictions, which presents itself to you as "obvious" to such an extent that it would not have occurred to you to specify where, exactly, you perceived complacency. And yet it would interest me a great deal if you would take the trouble to specify it (if in taking this trouble, or in the question I have

just asked, you do not yet again perceive some "complacency"!) Is it in the fact that I express myself without reserve on the beauty of a work (which happens to be mine) and which I feel very deeply, with the "guts" of my mind, if I may say? Such things, however intensely they may be felt, almost always remain unsaid. It took all the impact on my being (last year) of the desecration and derision which struck this work, for this deep feeling linking me to it to find the path of language to express itself clearly, "before those who pretend to disdain it and before indifferent witnesses". It is true that this "act of respect" must call out to you strongly — all the more so since something deep inside you concurs, and finds in it an echo. For you too, while often denying it to yourself, have also sensed this living beauty, this power, in this work of another person, a work which you were closer to, for a long time, then anyone else (apart from myself). It is probably because of all this that it would not be at all surprising if you were to reject this call as a "complacency" of mine — for by general consensus (which comes to the rescue just in time), one is only supposed to feel and express beauty in others and in the work of others (something, therefore, which remains exterior to us), and never that which fills our very selves, or the products of our *own* loves.

It is actually not impossible that there are passages in R.S. where complacency might have slipped in — the contrary would be surprising! This is not what I mean when I talk about "absence of complacency" — but the fundamental tone, the intention that drives the whole reflection. But it is likely that this expression "complacency" (like many others, no doubt) means something very different to you than it does to me — and it could not be otherwise, given that the very aim of the quest pursued in RS inspires a visceral rejection in you! And I imagine it is this very rejection which leads you to make no reference to my rather personal letter before last, in which I told you I was sending you the introductory part of RS. . .

Thank you for the typographical and orthographical comments etc. — the latter will be taken into account in the printed version ("if I find an editor mad enough . . . "). I deserve no praise for the typewriter, which belongs to the university! As for my own, I imagine you are beginning to know it well. . . I was glad to read your news of Cartan, Weil and Dieudonné. If you tell me about them, it probably means that you still meet them from time to time? At the Bourbaki seminars, I suppose?

Your letter crossed mine, in which I told you I was impatient to have your reactions to RS III and IV, which I told you I had sent you, and which you will perhaps receive at the same time as this letter. But I am afraid that in all

this (and you will not be the only one) the blow will hurt in the same place —
because all this is a bit personal!

Very cordially yours,

A. Grothendieck

February 8, 1986 JEAN-PIERRE SERRE
Dear Grothendieck,

I have received the volume of R. and S. which you had sent to me. Thank you very much. I still do not have the second-to-last volume (pages 500 to 800, more or less), of which I have only a few isolated pages which I would like to put back into their context. Could you have it sent to me as well, so that I have the complete set? Thank you in advance.

One thing strikes me in the texts that I have seen: you are surprised and indignant that your former students did not continue the work which you had undertaken and largely completed. But you do not ask the most obvious question, the one every reader expects you to answer:

why did you yourself abandon the work in question?

For example, you talk about Jouanolou and his lack of interest in his own thesis (projective limits arising in ℓ-adic cohomology). This lack of interest is not surprising: these are technical results which are only meaningful in the context of a program which you alone mastered. But you, who had created this program, why did you abandon it? The answer to this question would have deserved at least half of the 1200 or so pages which you devote to Verdier, Deligne, myself, etc. As far as I remember, your interest in your own program declined around 1968-1970. It is true that chapter V of the EGA's (the one you never wrote: hyperplane sections of all kinds) was not very inspiring. Of course it would have been useful (I was pleased to learn recently that P. Blass intended to resurrect itJ.-P. Serre : "P. Blass intended to resurrect it" : see note 24.2 to the letter of October 31, 1959. with your authorization but without your collaboration), but it would not have contained anything as original as chapters I and III, for instance. I understand very well that you may have shrunk from the necessary fine-tuning — but if this is the case, you can hardly reproach other, much less motivated people (motives again, you will say...), for shrinking from it also.

I have the impression that, despite your well-known energy, you were quite simply tired of the enormous job you had taken on. Especially since the SGA's were also getting increasingly behind schedule with the years. I particularly remember the rather disastrous state of SGA 5, where the authors got lost in masses of diagrams whose commutativity they were reduced to asserting without proof (up to sign, with a little optimism...); and these commutations were essential for the sequel. My comment to the Bourbaki seminar: "...the definitive version of SGA 5, which should be more convincing than the existing photocopies" referred to this state of affairs, which was disastrous (and not idyllic, as one is led to believe on reading R. and S.).

One would like to know what you think of all this, even modified by 15 years of "burial", to use your term. One is left unsatisfied.

One might ask oneself, for example, if there is not a deeper explanation than simply being tired of having to bear the burden of so many thousands of pages. Somewhere, you describe your approach to mathematics, in which one does not attack a problem head-on, but one envelopes and dissolves it in a rising tide of general theories. Very good: this is your way of working, and what you have done proves that it does indeed work. For topological vector spaces or algebraic geometry, at least... It is not so clear for number theory (in which the structures involved are far from obvious — or rather, in which all possible structures are involved); I have the same reservation for the theory of modular forms, which is visibly richer than its simple "Lie groups" aspect

or its "algebraic geometry — moduli schemes" aspect. Whence this question: did you not come, in fact, around 1968-1970, to realize that the "rising tide" method was powerless against this type of question, and that a different style would be necessary — which you did not like?

This is the kind of question one asks oneself.
Regards,
J-P. Serre

December 31, 1986 Jean-Pierre Serre

Dear Grothendieck,

One of these days, you should be receiving a copy of "Sur les représentations modulaires de degré 2 de $\mathrm{Gal}(\overline{\mathbf{Q}}/\mathbf{Q})$", J.-P. Serre : "Sur les représentations...": [**Se87**]. a piece of work which I have written up over the last few months, but which has in fact been in progress for a dozen years.

I would like to give you some explanation of what it is about, since you might be put off by the technical aspect of S§1, 2 and 3, and I am not sure you will really like the numerical examples in §5.

You probably remember the conjecture made by Weil in 1966: every elliptic curve over $\mathbf{Q}$ is "modular". We called it the "Weil conjecture"; it is now called the "Taniyama-Weil conjecture" or the " Shimura-Taniyama conjecture", depending on the authors, but never mind. The importance of this conjecture comes from the fact that it describes how to get the simplest possible motives: those of dimension 2, height 1 J.-P. Serre : "height 1" should read "of motivic weight 1" (I probably wanted to avoid the confusion with modular "weight"). and base field $\mathbf{Q}$. In particular, if the conjecture is true (and it has been checked numerically in very many cases), then the zeta function of a motive has the analytic properties (continuation and functional equation) one expects.

More generally, all zeta functions attached to motives should (conjecturally) come from suitable "modular representations"; Langlands and Deligne have made fairly precise conjectures on this subject.

What I tried to do in the text I am sending you is an *analog* (modulo p) of this Weil conjecture. One would like to describe certain Galois representations in terms of modular forms (modulo p). These representations appear to be very special; they are representations

$$\mathrm{Gal}(\overline{\mathbf{Q}}/\mathbf{Q}) \to \mathrm{GL}_2(\mathbf{F}_p)$$

which are irreducible (otherwise it is not very interesting) with odd determinant (complex conjugation must have determinant -1). The conjecture I make is that all such representations are "modular", i.e. come from modular forms modulo p whose weight and level I actually predict (the recipe predicting the level is very natural — the one for the weight is not). Of course, I am not at all sure this conjecture is true! But it is supported by a large number of half-theoretical, half-numerical examples, and I have finally decided to publish it, all the more because it has many applications:

a) it implies the Weil conjecture cited above, along with analogous conjectures on motives of height > 1 J.-P. Serre : Here, once again, "height" should be replaced with "motivic weight". (cf. §4 in my text); this may appear surprising at first: how can a result in characteristic 0 be deduced from a result in

characteristic p? This is much less surprising when one realizes that there is an infinite number of p.

b) it implies Fermat's (big) theorem, along with some rather surprising variations: the non-existence of non-trivial solutions of $x^p + y^p + \ell z^p = 0$, $p \geq 11$, for primes ℓ equal to 3,5,7,11,17,19,... (the method does not apply to $\ell = 31$).J.-P. Serre : The list of possible ℓ is $3, 5, 7, 11, 13, 17, 19, 23, 29, 53, 59$, cf. [**Se87**], §4, th. 2.

c) it implies that any finite flat group scheme over **Z** of type (p, p) is a direct sum of copies of $\mathbf{Z}/p\mathbf{Z}$ and μ_p (for $p \geq 3$). (Beware: this refers only to schemes of rank 2. I don't know how to do anything for higher rank.)

Of course, it would be slightly reassuring to be able to formulate a general conjecture (over an arbitrary global field, for representations of arbitrary dimension). I have thought about this often, but I do not see what to do (and yet I am convinced it is possible, at least in certain cases). We'll see...

Regards — and best wishes for 1987

J-P. Serre

January 25, 1987 Alexandre Grothendieck

Dear Serre,

Thank you for your reprint, and the trouble you took to try to explain the ins and outs of it to me. I realize from your letter that beautiful work is being done in math, but also and especially that such letters and the work they discuss deserve readers and commentators who are more available than I am. My research is taking me farther and farther from what is generally considered as "scientific" work (not that I have the impression of any real "break" in the ardor and the spirit I put into my work) — and anyway, it would be entirely useless for me to tell you about it, even briefly. I will talk about it, however, for those who might be interested (if any. . .). I have taken the decision to retire next year (I will then be sixty), in order to feel freer to pursue my research in a direction which does not fit into any "discipline" of recognized usefulness to society and fundable as such, and in which I will be the only one to go. My interest in mathematics is not dead, but I doubt I will have the spare time to write up the few grand sketches I had still intended to write.

I would like to take advantage of this opportunity to wish you a happy 1987 and all the success you hope for in your work.

Yours,

A. Grothendieck

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