A biographical reading of the Grothendieck-Serre Correspondence [1]

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The Grothendieck-Serre Correspondence[2] is a very unusual book: one might call it a living math book. To retrace the contents and history of the rich plethora of mathematical events discussed in these letters over many years in any complete manner would require many more pages than permitted by the notion of a book review, and far more expertise than the present author possesses. More modestly, what we hope to accomplish here is to render the flavour of the most important results and notions via short and informal explanations, while placing the letters in the context of the personalities and the lives of the two unforgettable epistolarians.

The exchange of letters started at the beginning of the year 1955 and continued through to 1969 (with a sudden short burst in the 1980's), mostly written on the occasion of the travels of one or the other of the writers. Every mathematician is familiar with the names of these two mathematicians, and has most probably studied at least some of their foundational papers – Grothendieck's "Tohoku" article on homological algebra, Serre's FAC and GAGA, or the volumes of EGA and SGA. It is well-known that the work of these two mathematicians profoundly renewed the entire domain of algebraic geometry in its language, in its concepts, in its methods and of course in its results. The 1950's, 1960's and early 1970's saw a kind of heyday of algebraic geometry, in which the successive articles, seminars, books and of course the important results proven by other mathematicians as consequences of their foundational work – perhaps above all Deligne's finishing the proof of the Weil conjectures – fell like so many bombshells into what had previously been a well-established classical domain, shattering its concepts to reintroduce them in new and deeper forms. But the articles themselves do not reveal anything of the actual creative process that went into them. That, miraculously, is exactly what the correspondence does do: it sheds light on the *development* of this renewal in the minds of its creators. Here, unlike in any mathematics article, the reader will see how Grothendieck proceeds and what he does when he is stuck on a point of his proof (first step: ask Serre), share his difficulties with writing up his results, participate with Serre as he answers questions, provides counterexamples, shakes his finger, complains about his own writing tasks and describes some of his theorems. The letters of the two are very different in character and Grothendieck's are the more revealing of the actual creative process of mathematics, and the most surprising for the questions he asks and for their difference with the style of his articles, whereas Serre's letters for the most part are finished products which closely resemble his other mathematical writings, a fact which in itself is almost as surprising, for

[1] A very short version of this article appeared as a book review in the *Mathematical Intelligencer*, Vol. 29 No. 4, 2007.

[2] published by the Société Mathématique de France in 2001, then translated and published in a bilingual version by the American Mathematical Society in 2003.

it seems that Serre reflects directly in final terms. Even when Grothendieck surprises him with a new result, Serre responds with an accurate explanation of what he had previously known about the question and what Grothendieck's observation adds to it.

They tell each other their results as they prove them, and the responses are of two types. If the result fits directly into their current thoughts, they absorb it instantly and, for the most part, add to it. Otherwise, there is a polite acknowledgement ("That sounds good"), sometimes joined to a confession that they have had no time to look more closely. The whole of the correspondence yields an extraordinary impression of speed, depth and incredible fertility. Most of the letters, especially at first, are signed off with the accepted Bourbaki expression "Salut et fraternité".

At the time the correspondence began, in early 1955, Jean-Pierre Serre was twenty-eight years old. A young man from the countryside, the son of two pharmacists, he had come up to the Ecole Normale Supérieure in 1945 at the age of 19, then defended an extraordinary thesis under the direction of Henri Cartan in 1951, in which he applied Leray's spectral sequences, created as a tool to express the homology groups of a fibration in terms of those of its fibre space and base space, to study the relations between homology groups and homotopy groups, in particular the homotopy groups of the sphere. After his thesis, Serre held a position in the Centre National des Recherches Scientifiques (CNRS) in France before being appointed to the University of Nancy in 1954, the same year in which he won the Fields Medal. He wrote many papers during this time, of which the most important one, largely inspired by Cartan's work and the extraordinary atmosphere of his famous seminar, was the influential "FAC" (Faisceaux Algébriques Cohérents, published in 1955), developing the sheaf theoretic viewpoint (sheaves had been introduced some years earlier by Leray in a very different context) in abstract algebraic geometry. Married in 1948 to a brilliant chemist who had been a student at the Ecole Normale Supérieure for girls, Serre was the father of a small daughter, Claudine, born in 1949.

In January 1955, Alexandre Grothendieck had just arrived in Kansas to spend a year on an NSF grant. Aged twenty-six, his personal situation was chaotic and lawless, the opposite of Serre's in almost every possible way. His earliest childhood was spent in inconceivable poverty with his anarchist parents in Berlin; he then spent five or six years with a foster family in Germany, but in 1939 the situation became too hot to hold a half-Jewish child, and he was sent to join his parents in France. The war broke out almost immediately and he spent the war years interned with his mother in a camp for "undesirables" in the south of France; his father, interned in a different camp, was deported to Auschwitz in 1942 and never returned. After the war, Grothendieck lived in a small village near Montpellier with his mother, who was already seriously ill with tuberculosis contracted in the camp; they lived on his modest university scholarship, complemented by his occasional participation in the local grape harvest. He, too, was the father of a child: an illegitimate son from an older woman who had been his landlady. His family relations – with his mother, the child, the child's mother, and his half-sister who had come to France to join them after a twelve-year separation, was wracked with passion and conflict. He was stateless, with no permanent job and the legal impossibility to hold a university position in France, so that he was compelled to accept temporary positions in foreign countries, while hoping that some suitable research position in France might eventually be created. After

Montpellier, he spent a year at the Ecole Normale in Paris, where he met Cartan, Serre, and the group that surrounded them, and then, on their advice, he went to do a thesis under Laurent Schwartz in Nancy. His friends from his time in Nancy and after, such as Paulo Ribenboim, remember a young man deeply concentrated on mathematics, spending his (very small amount of) spare time taking long walks or playing the piano, working and studying all night long. During his whole life, Grothendieck would keep his mathematical activities sharply separate from his private affairs, of which next to nothing appear in the letters. And during his whole life, he would spend his nights working and writing.

At the time of his visit to Kansas, Grothendieck already had his dissertation and nearly twenty publications to his credit, on the subject of topological vector spaces, their tensor products, and nuclear spaces. To summarize Grothendieck's pre-Kansas research in brief, Laurent Schwartz had given him the following subject as a thesis topic: put a good topology on the tensor product of two locally convex spaces. Schwartz was at that time studying $\mathcal{D}$, the space of smooth real-valued functions with compact support, and the dual space of distributions $\mathcal{D}'$. Wanting to extend the space to functions with values in any locally convex space F, Schwartz was trying to put a topology on $\mathcal{D}' \otimes F$ whose completion would naturally be the topological space $\mathcal{L}(\mathcal{D}, F)$ of linear maps from $\mathcal{D}$ to F. Grothendieck began by discovering two natural topologies on a tensor product of locally convex spaces, which discouraged him greatly at the start[3] because of its depressing lack of canonicity. He overcame this difficulty by restricting attention to the important class of spaces for which the two topologies coincide, which he dubbed nuclear spaces. Grothendieck's discovery that $\mathcal{D}'$ was a nuclear space, and his development of the general theory of nuclear spaces, set the whole subject into its wider context. He completed his thesis in 1953 and then spent the years 1953-1955 in São Paulo where he continued to work on the subject. His move to Kansas marked the beginning of the first of several major shifts in his mathematical interests.

1955-1957: Two mathematicians in their twenties

From the very first letter of the correspondence with Serre, dated January 1955, the words homology, cohomology and sheaf make their appearance, as well as a plethora of inductive and projective limits. These limits and their duality to each other, now a more-than-familiar concept even for students, were extremely new at the time. Although projective limits of topological groups had been studied and the notions of Krull topology and profinite groups were known, projective limits were not yet considered as a general "procedure" which could be applied to projective systems of any type. As for inductive limits, they were studied only in the early 1950s. Their introduction into homological algebra, together with the notion that the two types of limit are dual to each other, dates to very shortly before the exchange of the earliest letters of the correspondence.

From the naturalness with which Grothendieck and Serre juggle inductive and projective limits of homology and cohomology groups in the earliest letters, it is not easy to

[3] Laurent Schwartz, *Un mathématicien aux prises avec le siècle*, Odile Jacob, 1997, English translation *A mathematician grappling with his century*, Birkhäuser, 2001.

realise that they are largely creating, not just learning, the elements of the theory. In the letter of February 26, 1955, for example, Grothendieck states a theorem which, as he observes, has already been proved "umpteen times in all kinds of special cases" and which is now considered a basic result:

Theorem: *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[...] The theorem then says that condition (A) ("approximation") implies h_1.

At the time of these early letters, Grothendieck considers that he is merely learning (as opposed to creating) homological algebra: "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." This remark is the first reference to a text which will grow into his famous Tohoku article. He wants to study and teach a course on Cartan and Eilenberg's new book but cannot get hold of a copy, so that he is compelled to work everything out for himself, following what he "presumes" to be their outline.

The Tohoku paper gives an introduction to abelian categories, extracting the main defining features of some much-studied categories such as abelian groups or modules. Grothendieck introduces the essential notion of having "enough injectives", generalizing Baer's 1940 construction of injectives so that it works as well for the cateogry of sheaves on any space, and the criterion for them to exist. This allows him to extend Cartan-Eilenberg's notion of derived functors of functors on the category of modules to a completely general notion of derived functors. If $F : \mathcal{A} \to \mathcal{B}$ is a left-exact functor between two abelian categories and $0 \to A \to B \to C \to 0$ is a short exact sequence of objects of $\mathcal{A}$, then $0 \to F(A) \to F(B) \to F(C)$ is exact in $\mathcal{B}$. What Grothendieck showed is that if $\mathcal{A}$ has enough injectives, then there is a canonical, repeatable derivation operation $F \to R^1F$ giving a new functor such that we can continue this exact sequence to a long exact sequence

$$0 \to F(A) \to F(B) \to F(C) \to R^1F(A) \to R^1F(B) \to R^1F(C) \to R^2F(A) \to \cdots$$

as was already known to Cartan and Eilenberg for modules.

This long exact sequence was a grand generalization of the familiar long exact sequence of cohomology groups associated to an exact sequence of modules, so that cohomology now comes under the "heading of derived functors". Grothendieck's use of abelian categories, injective resolutions and derived functors allowed him to extend Serre duality – a relation between the sheaf cohomology groups H^i and H^{n-i} associated to a non-singular n-dimensional projective variety – to more general situations, including possibly singular algebraic varieties (Grothendieck duality).

In his own words (letter to Serre of Feb. 26, 1955): "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 $\Gamma(F)$ which takes values in the category of abelian groups, and consider its derived functors. Their existence follows from a general criterion, in which fine sheaves will 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[...] 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." (At the end of this letter, Grothendieck hopefully mentions that he has heard that some associate professorships open to foreigners are going to be created in France: does Serre know anything about it?) A couple of weeks later, the answer from Serre arrives: "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[...] The fact that sheaf cohomology is a special case of derived functors (at least in the paracompact case) is not in Cartan-Sammy. Cartan was aware of it, and had told Buchsbaum to do it, but it appears that he never did." (And in the same letter: "We have no details of the associate professorships. How many will there be in the whole of France? A mystery[...] In any case, you may be sure that if there is an opening for you we will jump at it...")

Grothendieck goes on working by himself throughout the ensuing months. In June, he sends his write-up to Serre: "You will find enclosed a neat draft of the outcome of my initial reflections on the foundations of homological algebra." Serre takes the draft to Bourbaki with him, and answers Grothendieck in July: "Your paper on homological algebra was read carefully and converted everyone (even Dieudonné, who seems to be completely functorised) to your point of view..." but it "raises a totally disjoint question, namely that of publishing it in a journal," because Buchsbaum had betweentimes added an appendix to Cartan-Eilenberg containing some overlap with Grothendieck's work. Serre and the others have obviously thought about the best solution to the problem, and tactfully suggest that Grothendieck cut out some parts: "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..."

It is really striking to see how some of the most typical features of Grothendieck's style over the coming decade and a half are already totally visible in the early work discussed in these exchanges: his view of the most general situations, explaining the many "special cases" others have worked on, his independence from (and sometimes ignorance of) other people's written work, and above all, his visionary aptitude for rephrasing classical problems on varieties or other objects in terms of morphisms between them, thus obtaining incredible generalizations and simplifications of various theories.

Six months later, in December, Grothendieck is back in Paris with a temporary job at the CNRS, while Serre is on leave from the University of Nancy, spending some time in Princeton and working on his "analytic=algebraic diplodocus", which would become the famous GAGA, in which he proved the equivalence of the categories of algebraic and analytic coherent sheaves, obtaining as applications several general comparison theorems englobing earlier partial results such as Chow's theorem (a closed analytic subspace of projective space is algebraic). The comparison between algebraic and analytic structures in any or every context is at this point one of the richest topics of reflection for both Grothendieck and Serre. Grothendieck finds a more general form for the major duality theorem from Serre's FAC, which he expresses as a canonical bijection between the dual of $H^p(X, F)$, where X is an n-dimensional projective algebraic variety and F is a coherent algebraic sheaf on X, and $\mathrm{Ext}_{\mathcal{O}}^{n-p}(X, F, \Omega^n)$, where Ω^n is the sheaf of germs of differential n-forms on X. Serre's delighted response: "I find your formula 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..." And in mid-January 1956, after explaining that recent results of Cartan have allowed him to prove that $H^i(X, k^*) = 0$ for $i \geq 2$ if X is a algebraic variety without singularities, Grothendieck observes: "This proves in particular that a projective algebraic bundle 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..." only to rectify humbly two weeks later, no doubt in response to an objection by Serre, "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, 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[...]and it would be very sad if you already had a counterexample for the projective group." And Serre, the expert, soon responds: "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" (and he turned out to be right). The letter goes on: "I have no comments on the rest of your letter...because I have hardly had the time to study it in detail. I am busy with my blasted analytic paper (I write horribly slowly)..." GAGA would be published during the course of that same year.

The following month finds Grothendieck eagerly telling Serre about some recent ideas of Cartier's on coalgebras, which Cartan adapted to compute the homological structure of the loop space and the loop space of the loop space of the sphere – but remarks that this formalism is not yet refined enough to yield $\pi_6(S^3)$, an allusion to one of the most difficult results obtained in Serre's doctoral thesis.

A year earlier, Grothendieck was working on topological vector spaces; in Kansas in 1955-56, he began his synthesis of homological algebra, and in March 1956, we find that he has "gone back to the classification of analytic bundles over the Riemann sphere with semi-simple structural group, and I have more or less proved my conjecture...Do your constructions also show that over a complex algebraic projective variety, the analytic and algebraic classification of bundles, for example with structural group $\mathrm{SO}(n)$, are not the same?" Grothendieck's work on classification of analytic bundles over the Riemann sphere can be described as his first foray into algebraic geometry: he showed that every such bundle is the direct sum of a certain number of tensor powers of the tautological line bundle. Serre, always concerned with analytic=algebraic theories, replies: "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?" Both young men, more involved in creating new than in studying older mathematics, were apparently unaware of the fact that others (Segre, Birkhoff, Hilbert, Dedekind, Weber) had already considered various versions of the question of classifying bundles over the Riemann sphere.

Serre has finished writing up GAGA by this time ("For information on the relationship between the algebraic and analytic classifications, I refer you to my paper...") and by early April, Grothendieck has finished writing up his classification of analytic bundles on the Riemann sphere. In July, Serre receives a small letter in which Grothendieck proposes to write a supplement to the analytic=algebraic diplodocus (GAGA) showing that "algebraic=analytic" for algebraic coherent sheaves on a complete compact algebraic variety not assumed to be projective – providing that someone does not in the meantime discover how to embed a complete algebraic variety into projective space, in which case the supplement would be useless! As it happens, no one did; Nagata[4] showed later that on the contrary, one can find complete algebraic varieties which cannot be embedded into projective space.

September 1956 finds Grothendieck trying to finish his enormous article on homological algebra and puzzled about where to publish something so long – not in France, where he recently published another long article, not in the American Journal which just accepted the bundles over the Riemann sphere, not in the Transactions, because Sammy (Eilenberg) demands further work on the presentation which Grothendieck is reluctant to comply with. Serre scolds him for this ("I find your objections to publishing in the Transactions idiotic...armed with a little patience and a little glue, it would surely take you no more than a day...") but instead, Grothendieck tells him that he has proposed it to Tannaka, who accepted it for the Tohoku Journal.

In November, Serre is working on stability of cohomology groups of algebraic varieties with values in a coherent algebraic sheaf under blowups at a simple point, and Grothendieck writes to ask him a "stupid question on which I am stuck: let X be an algebraic 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 $\mathbb{C}^{*n}$. Is it true that this bundle will almost never be an algebraic variety? Obviously, I am looking for an algebraic definition of the fundamental group, and I want to be sure that my idea cannot give anything..." This striking sentence reveals Grothendieck's very earliest thoughts on a subject which was to become one of the essential topics of SGA. Serre's answer: "I have no idea about your holomorphic bundles with fiber $\mathbb{C}^{*n}$. I do not see how to prove that they are 'almost never' algebraic." Grothendieck later answered his own question (SGA

[4] On the embedding problem of abstract varieties in projective varieties, *Mem. Coll. Sci. Kyoto (A)* **30** (1956), 71-82

3).

Questions and answers are one of the most lively aspects of the correspondence; the one that makes reading it such a different experience from reading a mathematical article. The most common situation is that Grothendieck asks a question, and Serre either answers or provides a counterexample, though of course there are also questions whose answer he doesn't know. Grothendieck's questions are sometimes quite easy and the answers apparently well-known: "Is a finitely generated projective module over the ring in question (a local Noetherian ring) free? Is this easy to see in special cases?" (Serre answers that it is always free, giving a ten-line proof); "This would mean 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 realize that I did not give a proof in my papers, and I can't seem to improvise one..." (Serre answers "Set your mind at rest, it is indeed true...", sketching a five-line proof); "What is the Pontryagin square?" (Serre admits that it is something of a mystery to him and refers him to Cartan). Some of Grothendieck's remarks about his own ignorance of mathematics are most refreshing: "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..." or "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!"

During the period covered by these early letters, the notion of a scheme was just beginning to make its appearance (see below). It does not seem that Grothendieck paid particular attention to it at the time, but a scattering of early remarks turns up here and there. Already at the beginning of 1955 Grothendieck wrote of FAC: "You wrote that the theory of coherent sheaves on affine varieties also works for spectra 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." Later, of course, he would be the one to explain that one can and should consider spectra of *all* commutative rings. A year later, in January 1956, Grothendieck mentions "Cartier-Serre type ring spectra," which are nothing other than affine schemes, and just one month after that he is cheerfully proving results for "arithmetic varieties obtained by gluing together spectra of commutative Noetherian rings" – schemes! A chatty letter from November 1956 gives a brief description of the goings-on on the Paris mathematical scene, containing the casual remark "Cartier has made the link between schemes and varieties," referring to Cartier's formulation of an idea then only just beginning to make the rounds: *The proper generalization of the notion of a classical algebraic variety is that of a ringed space* $(X, \mathcal{O}_X)$ *locally isomorphic to spectra of rings.* Over the coming years, Grothendieck would make this notion his own.

1957-1958: Riemann-Roch...and Hirzebruch...and Grothendieck

The classical Riemann-Roch theorem over the complex numbers, stated as the well-known formula

$$\ell(D) - \ell(K - D) = \deg(D) - g + 1$$

concerns a non-singular projective curve over the complex numbers equipped with a divisor D; the formula equates a difference of the dimensions of two vector spaces of meromorphic functions on the curve with prescribed behaviour at the points of the divisor D (the left-hand side) with an integral expression in numbers associated topologically with the curve (the right-hand side).

In the early 1950's, Serre reinterpreted the left-hand side of the Riemann-Roch formula as a difference of the dimensions of the zero-th and first cohomology groups associated to the curve, and generalized this expression to any n-dimensional non-singular projective variety X equipped with a vector bundle E as the alternating sum of dimensions of cohomology groups $\sum(-1)^i \dim H^i(X, E)$.

In 1953, Hirzebruch gave a generalization of the classical Riemann-Roch theorem to this situation, by proving that Serre's alternating sum was equal to an integer which could be expressed in terms of topological invariants of the variety called the Chern class (associated to E) and the Todd class (associated to X).

It seems that the idea of trying to prove a general algebraic version of Riemann-Roch was in Grothendieck's mind from the time he first heard about Hirzebruch's proof. In February 1955, he writes "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." Serre's answer: "No, one should not try to define 'Chern classes' as elements of certain $H^q(X)$ 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 the other classes as equivalence classes of algebraic cycles up to 'numerical' or 'algebraic' equivalence. And this should not even be difficult."

In the end, what Grothendieck brought to the Riemann-Roch theorem is one of the basic features of all of his mathematics, and was already visible in his Tohoku article: the transformation of statements on *objects* (here, varieties) into more general statements on *morphisms* between those objects. He reinterpreted both sides of the formula that Hirzebruch proved in the framework of morphisms $f : X \to Y$ between varieties. The right-hand side (the integer defined in terms of Chern and Todd classes) can naturally be generalized to this situation. However, in order to reinterpret the left-hand side, the alternating sum of dimensions of cohomology groups, in the framework of a morphism between varieties, Grothendieck introduced what came to be called the Grothendieck group $K(Y)$, a quotient of the free abelian group generated by all coherent sheaves on Y (up to isomorphism) which generalizes the abelian monoid of such sheaves under direct sum. The Grothendieck group later gave rise to the entire domain of K-theory. In this framework, Grothendieck was able to prove a statement of the Riemann-Roch theorem which freed it completely from the need to work specifically over the complexes ("You will find enclosed a very simple proof of Riemann-Roch independent of the characteristic", November 1, 1957), and could be expressed as the commutativity of a single square diagram

$$\begin{tikzcd} K(X) \arrow[r] \arrow[d, "\mathrm{ch}_X(-)\mathrm{td}(X)"'] & K(Y) \arrow[d, "\mathrm{ch}_Y(-)\mathrm{td}(Y)"] \\ H^*(X) \arrow[r] & H^*(Y) \end{tikzcd}$$

where the two horizontal maps are derived directly from the given morphism $f : X \to Y$, the symbols ch and td refer to Chern and Todd classes, and Hirzebruch's formula is recovered by taking Y to be a point. Serre sent him several corrections, to which Grothendieck responded in detail in a letter from November 12, 1957 – and then dropped the whole matter! The letter ends with the words "...at the moment I have just dropped research in order to finally start writing up the varieties, of which I hadn't written a single word until the day before yesterday!" The 'varieties' he refers to is one of the anonymous papers written by all the Bourbaki members (this one, as it happens, never attained publication...)

Grothendieck did this work between 1954 and 1957. He wrote up something (RRR – "rapport Riemann-Roch") which he considered a mere preliminary and sent it to Serre, then in Princeton; Serre organised a seminar around it, and then, as Grothendieck was clearly onto other things and not going to publish, Serre wrote the proof up together with Borel and published it in the Bulletin de la Société Mathématique de France in 1958 [BS]. This article begins "What follows consists in the notes taken at a seminar held in Princeton in the autumn of 1957 on the work of GROTHENDIECK; the new results are due to him, our contribution is uniquely for the writing-up. The 'Riemann-Roch' theorem here is valid for (non-singular) algebraic varieties over a field of any characteristic; in the classical case, over the base field **C**, this theorem contains as a special case the theorem proved some years ago by Hirzebruch." Grothendieck finally included his original RRR at the beginning of SGA 6, held in 1966-67 and published only in 1971, at the very end of his established mathematical career.

What is not revealed in the letters is that Grothendieck's mother was dying at the very time of these exchanges. He does add as a postscriptum to the letter of November 1, "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." But Hanka Grothendieck was suffering from more than isolation. She had been nearly bedridden for several years, a victim of tuberculosis and severe depression. After their five-year separation during his childhood, she and Alexander had grown inseparable in the war and post-war years, but during the last months of her life, she was so ill and so bitter that his life had become extremely difficult. She died in December 1957. Shortly before her death, Grothendieck encountered, through a mutual friend, a young woman named Mireille who helped him care for his mother during her last months, and fascinated and overwhelmed by his powerful personality, fell in love with him. At the same time, the Grothendieck-Riemann-Roch theorem propelled him to instant stardom in the world of mathematics.

1958-1960: Schemes and EGA

The idea of schemes, or more generally, the idea of generalizing the classical study of coordinate rings of algebraic varieties defined over a field to larger classes of rings, appeared in the work and in the conversation of various people – Nagata, Serre, Chevalley, Cartier – starting around 1954. It does not appear, either from his articles or his letters to Serre, that Grothendieck paid overmuch attention to this idea at first. However, by the time he gave his famous talk at the ICM in Edinburgh in August 1958, the theory of schemes, past, present and future, was already astonishingly complete in his head. In that talk, he presents his plan for the complete reformulation of classical algebraic geometry in these new terms:

"I would like, however, to emphasize one point[...], namely, that the natural range of the notions dealt with, and the methods used, are not really algebraic varieties. Thus, we know that an *affine* algebraic variety with ground field k is determined by its co-ordinate ring, which is an arbitrary finitely generated k-algebra without nilpotent elements; therefore, any statement concerning affine algebraic varieties can be viewed also as a statement concerning rings A of the previous type. Now it appears that most of such statements make sense, and are true, if we assume only A to be a commutative ring with unit, provide we sometimes submit it to some mild restriction, as being noetherian, for instance[...] Besides, frequently when it seemed at first sight that the statement only made sense when a ground field k was involved,[...] further consideration of the matter showed that this impression was erroneous, and that a better understanding is obtained by replacing k by a ring B such that A is a finitely generated B-algebra. Geometrically, this means that instead of a single affine algebraic variety V (as defined by A) we are considering a 'regular map' or 'morphism' of V into another affine variety W, and properties of the variety V then are generalized to properties of a morphism $V \to W$ (the 'absolute' notion for V being obtained from the more general 'relative' notion by taking W reduced to a point). On the other hand, one should not prevent the rings having nilpotent elements, and by no means exclude them without serious reasons." There follows the full definition of affine pre-schemes and schemes, then general pre-schemes and schemes, then several theorems concerning (quasi-)coherent sheaves on schemes, then several open problems together with a hint that they are open only because the techniques are so new that no one had time to solve them yet, and finally the so-typical conclusion: 'As a quite general fact, it is believed that a better insight in any part of even the most classical Algebraic Geometry will be obtained by trying to re-state all known facts and problems in the context of schemata. This work is now begun, and will be carried on in a treatise on Algebraic Geometry which, it is hoped, will be written in the following years by J. Dieudonné and myself..."

By October 1958, the work is underway, with Grothendieck sending masses of rough – and not so rough – notes to Dieudonné for the final writing-up: "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. I hope that by spring, the first four chapters will be written up..." He goes on to give more or less the same plan for the coming chapters that appears in the introduction of EGA I, with a chronology, however, that turned out to be wildly off target. Indeed, as it turned out, EGA I was published in 1960, EGA II in

1961, EGA III in 1961 (first part) and 1963 (second part), EGA IV in 1964 (first part), 1965 (second part), 1966 (third part) and 1967 (fourth part). As for EGA V, a set of very partial prenotes for this volume were typed up by Grothendieck and are covered with his handwritten corrections, but they lay unused for several years. Grothendieck sent them to Piotr Blass in 1985, and Piotr Blass and Stan Klasa read, translated and gave a final cleaning up to a sizeable section of these notes which they published between 1992 and 1995 in the Ulam Quarterly Journal. It is however clear that this very partial EGA V is far from what Grothendieck had in mind when he called the projected volume "Procédés élémentaires de construction de schémas" in 1958. The rest of EGA was never written at all, although a large portion of the topics it was meant to cover appear in the SGA, and in the FGA (Fondements) which is nothing but the collection of Grothendieck's Bourbaki seminars.

In this period, the exchanges between Serre and Grothendieck become less intense as their interests diverge, yet they continue writing to each other frequently with accounts of their newest ideas, inspiring each other without actually collaborating on the same topic. In the fall of 1958, Zariski invited Grothendieck to visit Harvard. He was pleased to go, but made clear to Zariski that he refused to sign the pledge not to work to overthrow the American government which was necessary at that time to obtain a visa. Zariski warned him that he might find himself in prison; Grothendieck, perhaps mindful of the impressive amount of French mathematics done in prisons (think of Galois, Weil, Leray...) responded that that would be fine, as long as he could have books and students could visit.

The Harvard letters show that Grothendieck is mulling over fundamental groups: "I would like to prove the following result: Let X be a scheme over Y, proper over Y, whose 'tangent map is everywhere surjective'. Show that the 'geometric' fundamental group of the fiber $f^{-1}(y)$ is independent of y...Have you ever thought about questions of this flavor?" This eventually turned into Chap. X of SGA 1. However, Serre, back in Paris and now working hard on local fields as well as on write-ups for Bourbaki, replies simply "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..." Undeterred, Grothendieck fires back "I have the impression I am making progress with the π_1. It seems to me that one of the fundamental things to prove is the following: Let X be proper over a *local* ring $\mathcal{O}$, and let F be the geometric fiber of the origin in $\mathrm{Spec}(\mathcal{O})$. Then the homomorphism $\pi_1(F) \to \pi_1(X)$ is *injective*." This result is equivalent to the statement that every covering of F is dominated by the restriction to F of a covering of X.

Serre's response: "Your letter makes me want to take stock of what I am doing with local fields," and he presents an analogue of Grothendieck's injectivity statement for fundamental groups in the framework of class field theory. Namely, fixing a connected commutative algebraic group G defined over an algebraically closed field, he notes that the collection of isogenies $G' \to G$ where G' is also a connected algebraic group form a projective system; he sets $\overline{G}$ to be the projective limit, and defines $\pi_1(G)$ as the kernel of the surjective homomorphism $\overline{G} \to G$. "These constructions yield formal results of the usual kind. The only surprising result is the following: If $G \subset G'$, then $\pi_1(G) \to \pi_1(G')$ is *injective*..." This result proves the exact analog of Grothendieck's desired result stated above, in the situation of commutative algebraic groups, with coverings replaced by isogenies. Serre's ten-page, perfectly written letter goes on to classify isogenies of the groups of units of a discrete valuation field K with algebraically closed residue fields in terms of the abelian extensions of K and study the behaviour of the conductor of a finite extension L/K. The results ended up published in Serre's articles on algebraic groups and local fields.

A break of several months in the letters, due no doubt to the presence of both the correspondents in Paris, brings us to the summer of 1959. During the gap, Grothendieck's job problem had been solved once and for all when he accepted the offer of a permanent research position at the IHES (Institut des Hautes Etudes Scientifiques), newly created in June 1958 by the Russian businessman Léon Motchane as the French answer to Princeton's Institute for Advanced Study. He and Mireille had also become the parents of a little girl, Johanna, born in February 1959. The letters from this period show that Grothendieck was already thinking about a general formulation of Weil cohomology (planned for chapter XIII of EGA, now familiarly referred to as the Multiplodocus), while still working on the fundamental group and on writing the early chapters. The progress of the write-up is still seriously overestimated: "Next year, I hope to get 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... Unless there are unexpected difficulties or I get bogged down, the multiplodocus should be ready in 3 years' time, or 4 at the outside. Then we can start doing algebraic geometry!"

In the fall, Serre writes from Princeton, where under Weil's influence no one can help thinking about the Weil conjectures (see below); he is, however, also participating in a seminar on the part of the multiplodocus that has already been written, and sends detailed letters containing remarks, criticisms, corrections and suggestions. Although he does not naturally talk about his own interests in terms of schemes, he is very willing to do so in order to get a point across to his one-track minded friend: "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 $\mathrm{Morph}_S(U, R_{T/S}(V))$ to $\mathrm{Morph}_T(U \times_S T, V)$ is bijective. This probably doesn't exist except under very strong conditions..." One can imagine that Weil did not express the restriction of scalars – or descent theory, as it is called in his famous 1956 paper on the subject – in these terms! In fact, Weil and Grothendieck appear to have been rather allergic to each other's style (if not to each other); to a later remark in the same letter: "Tate gave a very pretty lecture on adelic points from the point of view of schemes; they are the same as Weil's in the separated case," Serre added a footnote when the Correspondence was published, commenting "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?"

To Serre's addition to the above question: "Lang says he finds all this insufficient, and that one should find a big bag containing all this stuff together with the Greenberg functor...Have you done this?" Grothendieck replies that: "I have indeed fully clarified the Weil and Greenberg operations...as Lang said, they go in the same bag, otherwise the proofs have to be repeated..." This is followed by a lengthy and complete resolution of the question Serre had posed – itself followed by an anxious query from Lang, worried that Grothendieck would publish something on the Greenberg functor before Greenberg himself did! Grothendieck reassures him; he plans to include his result in EGA V which will not be out for what he supposes to be a year or two. At this point, he is still devoted to the foundations of scheme theory. However, as the whole exchange of letters around this time clearly show, the Weil conjectures were the major inspiration for the development of the new language of algebraic geometry, and sketching possible proofs was an irresistible temptation, even if it did run ahead of the inexorable plan for EGA.

1959-1961: The Weil conjectures: first efforts

The Weil conjectures, first formulated by André Weil in 1949, were very present in the minds of both Serre and Grothendieck at least from the early 1950's. Weil himself proved his conjectures for curves and abelian varieties, and reformulated them in terms of an as yet non-existent cohomology theory which, if defined, would yield his conjectures as natural consequences of its properties. This was the approach that attracted Grothendieck; as he explained at the very beginning of his 1958 ICM talk, the precise goal that initially inspired the work on schemes was to define, for algebraic varieties defined over a field of characteristic $p > 0$, a 'Weil cohomology', i.e. a system of cohomology groups with coefficients in a field of characteristic 0 possessing all the properties listed by Weil that would be necessary to prove his conjectures.

As Weil first stated them, the conjectures do not appear overtly cohomological. For a projective variety X defined over the field $k = \mathbb{F}_q$ of q elements, let $\overline{X}$ be the same variety considered to be defined over the algebraic closure $\overline{k}$ of k, and for each $r \geq 1$, let N_r denote the number of points in $\overline{X}$ whose coordinates lie in the field $\mathbb{F}_{q^r}$. Define the zeta function of X by

$$Z(X,t) = \exp\Big(\sum_{r=1}^{\infty} N_r \frac{t^r}{r}\Big).$$

Then a rough statement of the Weil conjectures is as follows: $Z(X,t)$ is a rational function; it satisfies a simple functional equation of the form $Z(X, 1/q^n t) = \pm q^{nE/2} t^E Z(X,t)$ where E is an integer associated to the geometry of X known as the Euler characteristic of X; in fact $Z(X,t)$ has a decomposition as

$$Z(X,t) = \frac{P_1(t)P_3(t)\cdots P_{2n-1}(t)}{P_0(t)P_2(t)\cdots P_{2n}(t)}$$

where $P_0(t) = 1-t$, $P_{2n}(t) = 1-q^n t$ and each of the $P_i(t)$ is a product $P_i(t) = \prod_j (1-\alpha_{ij}t)$ where all of the α_{ij} are algebraic integers with $|\alpha_{ij}| = q^{i/2}$; and finally, the degrees $B_i(X)$ of the $P_i(t)$ have two important properties: $\sum_i (-1)^i B_i(X) = E$ and if X is the reduction mod p of a variety Y defined in characteristic 0, then $B^i(X) = \dim H^i(Y, \mathbb{Z})$.

The idea of reformulating the conjectures in cohomological terms, as simple consequences of the properties of a suitable cohomology with coefficients in characteristic zero, was first initiated by Weil himself, as he tried to prove his conjecture – and succeeded in proving it for curves and abelian varieties – by making use of the Frobenius morphism F which maps a point of $\overline{X}$ given by $P = (a_1, \dots, a_n) \in \overline{k}^n$ to the point $F(P) = (a_1^q, \dots, a_n^q)$. Then P is a fixed point of F if and only if the $a_i \in k$, and more generally P is a fixed point of the iterate f^r if and only if the $a_i \in \mathbb{F}_{q^r}$. Thus the number N_r measures exactly the number of fixed points of f^r on $\overline{X}$.

Suppose we are in possession of a 'Weil cohomology' associated to a scheme of finite type X over an algebraically closed field k of characteristic $p > 0$. This is a set of vector spaces $H^i(X, K)$ over some characteristic 0 field K such that $H^i(X, K) = 0$ unless $0 \leq i \leq 2n$, which satisfy a number of good properties of which we mention only a few here. The $H^i(X, K)$ should be contravariant functors in X, should be equipped with a cup-product. Under the assumption that X is smooth and proper, the $H^i(X, k)$ should be finite-dimensional and satisfy Poincaré duality, and for a morphism $f : X \to X$ with isolated fixed points, they should satisfy a Lefschetz fixed-point formula:

$$\#\text{fixed points of } f = \sum_{i=0}^{2n} (-1)^i \mathrm{Tr}(f^*; H^i(X, K)),$$

where for each i, $f^* : H^i(X, K) \to H^i(X, K)$ is the linear map induced by f, and the fixed points are counted with their multiplicity.

Now, if we have such a cohomology, the Lefschetz fixed-point formula applied to the iterated Frobenius morphism f^r associated to $\overline{X}$ as above says that

$$N_r = \sum_{i=0}^{2n} (-1)^i \mathrm{Tr}\big((f^r)^*; H^i(\overline{X}, K)\big).$$

Thus, Weil's zeta function can be written

$$\begin{aligned} Z(X,t) &= \exp\Big(\sum_{r=1}^{\infty} \sum_{i=0}^{2n} (-1)^i \mathrm{Tr}\big((f^r)^*; H^i(\overline{X}, K)\big) \frac{t^r}{r}\Big) \\ &= \prod_{i=0}^{2n} \Big[\exp\Big(\sum_{r=1}^{\infty} \mathrm{Tr}\big((f^r)^*; H^i(\overline{X}, K)\big) \frac{t^r}{r}\Big)\Big]^{(-1)^i} \end{aligned}. \qquad (*)$$

An elementary lemma shows that in general, if $\phi : V \to V$ is a linear map on a finite-dimensional vector space V defined over a field k, then as power series in t with coefficients in k, we have

$$\exp\big(\sum_{r=1}^{\infty} \mathrm{Tr}(\phi, V) \frac{t^r}{r}\big) = \det(1 - \phi t, V)^{-1}.$$

Since the right-hand side is a rational function in t, we immediately obtain from this and from (*) that

$$Z(X,t) = \frac{P_1(t) P_3(t) \cdots P_{2n-1}(t)}{P_0(t) P_2(t) \cdots P_{2n}(t)}$$

where $P_i(t) = \det(1 - f^*t, H^i(\overline{X}, K))$. This shows that $Z(X, t)$ is a rational function (the first Weil conjecture), although much more work is needed to prove the exact nature of the $P_i(t)$ predicted by Weil. The degrees of the $P_i(t)$ are equal to the dimensions of the $H^i(\overline{X}, K)$ (Betti numbers), and Poincaré duality implies that $Z(X, t)$ satisfies the functional equation (second conjecture).

This simplified discussion shows that defining a Weil cohomology would prove at least a large part of the Weil conjectures, and this is the approach that was taken up by both Serre and Grothendieck, who set to work on trying to discover the right cohomology theory. Serre used Zariski topology and tried cohomology over the field of definition of the variety; even though this field was in characteristic p, he hoped at least to find the right Betti numbers, but didn't. Then he tried working with the ring of Witt vectors, so that he was at least in characteristic zero, but this too failed to yield results. At the beginning of his ICM talk, Grothendieck describes these efforts and concludes "Although interesting relations must certainly exist between these cohomology groups [those studied by Serre] and the 'true ones', it seems certain now that the Weil cohomology has to be defined by a completely different approach." In October 1959, Serre writes from Princeton: "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), $X' = \mathrm{transp}(X)$ 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!" Although this definition of X' does not quite work to generalize Weil's formula, Serre soon found a way that did, and published it in a three-page note – an extract from a letter to Weil – called *Analogues kähleriens des conjectures de Weil* (Annals of Math. 1960).

Grothendieck's blunt response to Serre's remarks: "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 of the Weil conjectures based on the curves case, which means I am not that excited about your idea." His mind still running on several simultaneous tracks, he adds: "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!" The impression is that the two friends are thinking along different lines, with an intensity that precludes their looking actively at each other's ideas. Yet it is only a question of time. Just a few years later, Serre's *Analogues kählériens* was to play a fundamental role in Grothendieck's reflections aiming at a vast generalization of the Weil conjectures, making them even more "geometric", extending them to a statement on general endomorphisms rather than the all-pervasive but restrictive Frobenius. Serre's ideas stimulated him to formulate the famous 'standard conjectures' on algebraic cycles which were formulated in order to lead towards an unconditional theory of motives (see below), and whose proof would have fulfilled Grothendieck's plan for proving the third Weil conjecture.

On November 15, 1959 came the news which Michel Raynaud, a 21-year-old student at the time, describes as a thunderclap: "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 took the call, not Tate)...it is rather surprising that Dwork was able to do it. Let us wait for confirmation!" writes Serre. To quote Katz and Tate's memorial article on Dwork in the March 1999 Notices of the AMS: "In 1959 he electrified the mathematical community when he proved the first part of the Weil conjecture in a strong form, namely, that the zeta function of *any* algebraic variety over a finite field was a rational function. What's more, his proof did not at all conform to the then widespread idea that the Weil conjectures would, and should, be solved by the construction of a suitable cohomology theory for varieties over finite fields (a 'Weil cohomology' in later terminology) with a plethora of marvelous properties." Dwork did, however, make use of the Frobenius morphism and detailed p-adic analysis in a large p-adic field.

It is hard to assess the effect this announcement had on Grothendieck, because he did not respond (or his response is missing). However, one thing is absolutely clear: Dwork's work had little or no effect on his own vast research plan to create an algebraic-geometric framework in which a Weil cohomology would appear naturally. In August 1960 he writes: "I 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 expand my planned program by so much, 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..." This was the year in which he began running his famous SGA (Séminaire de Géométrie Algébrique). The first year of the seminar, 1960-61, was devoted to the study of the fundamental group and eventually published as SGA 1.

1961: Valuations – and War

October 1961 finds Grothendieck happily ensconced at Harvard – married, now, to Mireille, as this made it easier for the couple to travel to the US together, and the father of a tiny son born in July, named Alexander and called Sasha after Grothendieck's father, who perished in Auschwitz in 1942. His letters show him to be full of ideas and surrounded by outstanding students and colleagues: John Tate, Mike Artin, Robin Hartshorne, David Mumford. "The mathematical atmosphere at Harvard is absolutely terrific, a real breath of fresh air compared with Paris which becomes 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 better 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 [...] Meanwhile, Mike Artin is getting excited about global degeneracy phenomena for elliptic curves...which he wants to understand in terms of Weil cohomology..." By this time, Grothendieck's vision of the right way to do mathematics is strong and clear, and he is intolerant of other views. Valuations, for some reason, provoke intense annoyance; in his criticism of Bourbaki's draft for *Commutative Algebra*, he expostulates: "Chaps. VI and VII appear to me to be unworthy of Bourbaki. I have proposed several times in vain that Chap. VI (Valuations) should be purely and simply thrown out; even though I have come to understand why resolution of singularities is useful since then, I am still 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 position will mislead the reader as to the right ideas and methods...Chap. VII is also copied from Krull, and appears to me to be far removed from both geometric intuition (which is a good guide) and from actual practice...one can only understand *properly* if a geometric language is available...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." Serre is not impressed: "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...Nor do I really agree with your objections to the Krull chapter...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."

This is the first time that a pinch of annoyance can be felt in Serre's tone, underlying the real divergence between the two approaches to doing mathematics. Serre was the more open-minded of the two; any proof of a good theorem, whatever the style, was liable to enchant him, whereas obtaining even good results 'the wrong way' – using clever tricks to get around deep theoretical obstacles – could infuriate Grothendieck. These features became more pronounced in both mathematicians over the years; the author still recalls Serre's unexpected reaction of spontaneous delight upon being shown a very modest lemma on obstructions to the construction of the cyclic group of order 8 as a Galois group, simply because he had never spotted it himself, whereas Grothendieck could not prevent himself, later, from expressing bitter disapproval of Deligne's method for finishing the proof of the Weil conjecture, which did not follow his own grander and more difficult plan.

Grothendieck, ever the idealist, fires back a response also tinged with irritation and again making use of his favorite word 'right' as well as the picturesque style he uses when he really wants to get a point across: "Your argument in favor of valuations is pretty funny. 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."

These very same letters, as well as a famous one dated October 22, 1961 and addressed to Cartan, contain a fascinating exchange of views on the situation in France connected with the Algerian war and the necessity of military service. By October 1961, the end of the Algerian war of independence was thought to be in sight: already in 1959, de Gaulle had pronounced the fatal words 'self-determination', admitting the possibility that France would eventually have to give up Algeria, and in May 1961 he had negotiated a cease-fire set to begin in March 1962. However, hostilities continued during the interim period, with violent terrorist acts on the part of Algerian independence factions, and even more violent repression from the French police and anti-independence groups such as the OAS (Secret Army Organization), in both France and Algeria. Between August and October 1961, eleven policemen were killed and many more wounded in Algerian bomb attacks in Paris. The French prefect of police responded with "For every blow received, we will respond with ten," and Algerians living in France were subjected to harassment, imprisonment, torture and 'disappearing'. On October 5, a curfew concerning all "French Muslims from Algeria" was announced. On October 17, thousands of Algerians poured into the streets of Paris to protest. The massacre that occurred on that day left dozens of bloody bodies piled in the streets or floating down the Seine, where they were still to be seen days later.

Grothendieck's letter to Cartan was written from Harvard just four days after this event. Surprisingly for a man whose extreme antimilitarist, ecological views were to become his dominating preoccupation ten years later, when he left the IHES with fracas because he discovered that a small percentage of its funding was of military origin, the tone he adopts to criticise the effect of the mandatory two years' military service on budding mathematicians is quite moderate. Rather than lambasting military service on principle, he emits more of a lament at its effect on mathematics students. "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, 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..."

Cartan's response is not included in the Correspondence, but Cartan showed this letter to Serre, who responded to Grothendieck directly, in very typical, simple and pragmatic terms, which probably resonate with the majority: "What is certainly [...] 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'." It is not certain whether Serre himself took any kind of action against the war in Algeria, but other mathematicians, above all Laurent Schwartz – whose apartment building was plastic bombed by the OAS – certainly did.

Grothendieck replied to Serre in the same measured terms as before: "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 guerrilla 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."

Few if any of Grothendieck's French colleagues shared his views, however, and even after the Algerian war wound down, military service remained mandatory in France until 2001.

1962-1964: Weil conjectures more than ever

The letters of 1962 are reduced to a couple of short exchanges in September; they are rather amusing to read, as the questions and answers go so quickly that letters cross containing the same ideas. These letters concern the questions that Serre was working on at the time; Lie algebras associated to groups of Galois type, Galois cohomology and 'good' groups. Many of the results he mentions were eventually published in a form almost identical to the letters (compare for instance the letter dated Friday evening 1962 with the exercises given in *Galois Cohomology*, Chap 1, §2.6). Serre's letter deals with "the relation between the cohomology of 'discrete' groups and of 'Galois' groups (i.e. 'ordinary' cohomology and 'Grothendian' cohomology). He considers homomorphisms $G \to K$ where G is discrete and K totally disconnected compact, the basic case being where K is just the profinite completion $\widehat{G}$ of G, and distinguishes 'good' groups as being those such that $H^i(G, M) \simeq H^i(\widehat{G}, M)$ for all i and all topological $\widehat{G}$-modules M. The expression "Grothendian cohomology" is probably in view of the case where G is a topological fundamental group and its profinite completion the associated geometric fundamental group much studied by Grothendieck.

The next letters date from April 1963. By this time, Grothendieck had already developed many of the main properties of etale and ℓ-adic cohomology, which he would explain completely in his SGA lectures of 1963-64 (etale, SGA 4) and 1964-65 (ℓ-adic, SGA 5). The ℓ-adic cohomology was developed on purpose as a Weil cohomology, and indeed, in his Bourbaki seminar of December 1964, Grothendieck stated that using it, he was able to prove the first and the fourth Weil conjectures in April 1963, although he published nothing on the subject at that time.

Serre must have been aware of this result, so that it is never explicitly mentioned in the letters of April 1963 or in any others, leaving one with the same disappointed feeling an archeologist might have when there is a hole in a newly discovered ancient parchment document which must have contained essential words. But it was one of Grothendieck's distinguishing features as a mathematician, that he was never in a hurry to publish, whether it be for reasons of priority, or credit, or simply to get the word out. Each one of his new results fitted, in his mind, into an exact and proper position in his vast vision, and would be written up only when the write-up of the vision had reached that point and not before (as for actual publishing, this often had to wait for several more years, as there was far too much for Grothendieck to write up himself and he was dependent on the help of a large number of more or less willing and able students and colleagues).

Let us take the time here to sketch out something of Grothendieck's further work on the Weil conjectures even though it does not appear explicitly in the letters, because it sheds light on everything he was thinking about at the time. For this, we need to briefly explain the development of etale and ℓ-adic cohomology. It fits very naturally into Grothendieck's vision in general. To start with, Grothendieck's idea of a scheme S was far from what the commun run of mortals might associate with that notion (for instance, an ordinary algebraic variety defined by polynomial equations over a field). His notion of a scheme carried with it, automatically, the whole range of associated properties and objects: covers, morphisms, mod p versions, cohomology... He uses the words "set of yardsticks" for the cohomology groups and "fan" for the family of mod p versions, but "fan" is a very good image in general: when spread, the fan gives a clear picture of S, whereas the classical attitude of considering just one or two aspects (such as geometric points, for instance) shows only a very tiny slice of the full fan.

Although this quintessentially Grothendieckian point of view may be difficult to adopt for oneself, it is not conceptually out of reach. The next idea, however, is certainly one of Grothendieck's most astonishing contributions to mathematics. Grothendieck's work on generalizing the notion of fundamental group of an algebraic variety, in particular the geometric fundamental group which classifies unramified covers, to scheme theory led him naturally to consider at a single blow the whole collection of etale (flat, unramified – the schematic generalization of the topological notion of being a local homeomorphism, or finite covering map of an open set) morphisms to a given scheme S. The leap consists in considering this collection of maps – or, more generally, *any* collection of maps to a given object S satisfying a few good properties – as a new, more general kind of *topology* on S. In this view, open sets are now replaced by these maps $X \to S$, inclusions of open sets are replaced by compositions of morphisms $Y \to X \to S$, and the basic axioms of a topology are easily generalized. Such a topology is now known as a Grothendieck topology, and the initial example is the etale topology on a scheme S given by the collection of all etale morphisms to S. The scheme equipped with this topology is called the etale site of S, S_{et}. Grothendieck saw that this new type of topology had the power to yield results which the Zariski topology was not fine enough to give, even in Serre's able hands.

The next step was to define sheaves and sheaf cohomology for this new type of topological space, which presented no real problem since Grothendieck topology possesses all the axiomatic properties of ordinary topology. So he was able to simply apply the derived functor definition of sheaf cohomology which had already enchanted him back in 1955, obtaining a definition of etale cohomology groups $H^i(S_{et}, \mathcal{F})$ for a sheaf $\mathcal{F}$ on S_{et}.

The example of elliptic curves already yields some very important information about what kind of sheaves have to be used in order to build up a cohomology theory with the desired properties of a Weil cohomology. First of all, one needs to use *torsion* sheaves, for instance, the constant sheaf on a finite group. Secondly, the order of this group has to be prime to p, the characteristic of the field of definition of the algebraic variety in the Weil conjectures. So Grothendieck considered the simplest case, where the abelian group is $\mathbb{Z}/\ell\mathbb{Z}$ for a prime ℓ different from p, defining the etale cohomology groups $H^i(S_{et}, \mathbb{Z}/\ell\mathbb{Z})$. But the coefficients are in characteristic ℓ, whereas they should be in characteristic 0. Not to worry: Grothendieck defined the *ℓ-adic cohomology group* $H^i_{et}(S, \mathbb{Q}_\ell)$ as the $\mathbb{Q}_\ell$-vector space obtained by taking the inverse limit of the $H^i(S_{et}, \mathbb{Z}/\ell^n\mathbb{Z})$ as $n \to \infty$ to get a $\mathbb{Z}_\ell$-module and tensoring it with $\mathbb{Q}_\ell$.

Instead of writing explicitly about his results on the Weil conjectures, Grothendieck's letters from April 1963 are concerned with recasting the Ogg-Shafarevitch formula expressing the Euler characteristic of an algebraic curve in his own language, and generalizing it to the case of wild ramification. To do this, he looks for "local invariants" generalizing the terms of the Ogg-Shafarevitch formula, but although he sees what properties they should have, he doesn't know how to define them. His letter asking Serre this question bears fruit just days later, as Serre recognized that the desired local invariants can be obtained using the Swan representation, allowing Grothendieck to establish his Euler-Poincaré formula for torsion sheaves on an algebraic curve. Grothendieck did not get around to publishing this result either; it eventually appeared in his student Michel Raynaud's Bourbaki seminar of February 1965 (Raynaud recalls a slight feeling of panic the day before the seminar, when Grothendieck lightheartedly suggested that he talk about a grand generalization of what he had already carefully prepared.)

Grothendieck continued to work on the second and third Weil conjectures throughout 1963 and 1964; at one point he tried to plan a proof based on showing that every variety is birationally a product of curves... until Serre sent him a counterexample (letter of March 31, 1964). He then had another idea, to judge by Serre's letter of April 2, in which Serre writes “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.” But Serre’s intuition (as usual) was right, because the reply arrives the very next day: “I have convinced myself that my second approach to the Weil conjectures cannot work...” followed, undeterred, by yet another possible attack. However, in Grothendieck’s Bourbaki seminar of December 1964, he states that the second and third Weil conjectures are still open. The functional equation was surely proved by the end of 1966, when Grothendieck finished giving the SGA lectures that eventually became the SGA 5 volume on ℓ-adic cohomology. This seminar contained proofs that the ℓ-adic cohomology satisfies all the properties of a Weil cohomology – Poincaré duality in particular – and it is studied there in great depth.

Rather than attacking the remaining conjecture directly, Grothendieck sketched out a vast generalization of the Weil conjectures and stated the difficult ‘standard conjectures’ (see motives, below) which remain unproven to this day. However, in 1975, Deligne managed to get around the standard conjectures and prove the remaining Weil conjecture by using deep and subtle properties of the ℓ-adic cohomology and original, far-from-obvious techniques.

1964: Good and bad reduction of abelian varieties over local fields

The Weil conjectures on algebraic varieties over finite fields, and all of the mathematics that grew up around them, stimulated great interest in the study of algebraic varieties defined over *local* fields, and consequently also the study of the different types of reduction modulo the prime ideal of the local field. The sudden flush of letters exchanged over the fall of 1964 very largely concerns this theme, concentrating especially on elliptic curves and abelian varieties (to Serre’s delight and Grothendieck’s annoyance: “It might perhaps be possible to get at least the abelian variety case by this method [...]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!”)

It all began in August 1964 at the Woods Hole Summer Institute, which Serre attended but Grothendieck didn’t. On his return, Serre went off on vacation to the south of France, and from there, sent Grothendieck a very long letter describing, in detail, the main new ideas from what must have been a very lively meeting. Reading over the interests, conjectures and recent results of the mathematicians he names: Shimura, Atiyah, Bott, Verdier, Mumford, Ogg, Bombieri, Tate makes it abundantly clear how the Weil conjectures motivated much of the work in number theory and algebraic geometry at that time. Local fields, however, now play a major role.

At Woods Hole, Shimura expressed a conjectural Lefschetz-type formula expressing the trace of f in terms of its fixed points rather than the contrary; so many people started working on it that the formula was proved by the end of the Institute and became known as the “Woods Hole fixed-point formula” (Grothendieck’s unenthusiastic reaction: “the fixed point theorem seems to me to be nothing more than a variation on a well-worn theme.” And indeed, using his formalism of six operations, he had simultaneously proved a more general fixed-point formula.) Ogg studied the module of ℓ-torsion points on elliptic curves defined over local fields and showed that a certain invariant was independent of ℓ. Serre and Tate generalized this work by conjecturing the independence from ℓ of ℓ-adic representations given by ℓ-adic cohomology groups. This independence (a constant preoccupation in Grothendieck's mind and one that foreshadows the idea of motives), would generalize Ogg's results to abelian varieties. Tate added his own conjectures on ℓ-adic cohomology, algebraic cycles and poles of zeta functions, and Serre and Tate worked on lifting from characteristic p to characteristic 0.

In the months following this report, the exchange of letters between Serre and Grothendieck is exceptionally rich, with almost twenty letters exchanged over the summer and fall of 1964. Even though both epistolarians were in France, the ideas they wanted to share were too complex to discuss only over the phone, and the twenty or so kilometers separating the Collège de France from the IHES in Bures prevented them from seeing each other on a daily basis.

In the first letters following the Woods Hole report, Serre writes to Ogg and Grothendieck about one of the major topics of the rest of the correspondence: the reduction of an abelian variety A defined over a local field K with algebraically closed residue field k of characteristic p. He rephrases results of Ogg to show that A has good reduction if and only if the action of the absolute Galois group of K on the associated Tate module $T_\ell(A)$ for $\ell \neq p$ is trivial. This result, known as the Ogg-Néron-Shafarevitch criterion, was published by Serre and Tate, following the conventions of a pre-publish-or-perish time, in which it was occasionally those who strove to understand a given result who published it – often contributing substantially to the original idea – rather than the originator (cf. the case of Borel and Serre's article on Grothendieck's Riemann-Roch theorem). Using this criterion, Serre recovers known results about the reduction of elliptic curves, and shows that in the presence of complex multiplication (CM) abelian varieties have good reduction everywhere after finite extension of the base field. In the following letter, he adds another consequence of the Ogg-Néron-Shafarevitch criterion: if A has good reduction and B is an abelian variety over K equipped with a K-homomorphism $B \to A$ with finite kernel, then B also has good reduction. This result had already been proved by Shimura and Koizumi, and then by Grothendieck, who responds to Serre's request "I believe you had a proof of this result in your language. Am I right, and what is it?" by posting back a ten-line proof indeed in his own language, i.e. full of words like "closed flat pre-scheme", "general fiber" and "representable".

Serre also asks a precise question about bad reduction: can one always find a finite extension of K such that the (Néron model of the) abelian variety now reduces to an extension of an abelian variety by a torus? ("This may be a stupid question. Do you see a counterexample?") This result, which can be re-stated as the existence of a semi-stable model after finite extension of the base field, was soon proved by Mumford when $p \neq 2$, but on September 24, Grothendieck sends Serre a letter proving the general case. In fact, his theorem is quite general, giving conditions on an ℓ-adic vector space E equipped with an action of the Galois group $\pi = \mathrm{Gal}(\overline{K}/K)$ (where K is the fraction field of a discrete valuation ring) under which there exists an open subgroup of the inertia group which acts unipotently on E. But his conditions apply to the case where E is the Tate module $T_\ell(A)$ associated to an abelian variety A defined over K. Since a unipotent Galois action on

$T_\ell(A)$ corresponds to existence of a semi-stable model after finite extension of the base field, just as the trivial action corresponds to good reduction (see above), this answers Serre's question completely.

Serre's response to Grothendieck's letter is simultaneously excited, suspicious and as usual, precise: "Your theorem on the action of the inertia group is terrific – if you really have proved it. In fact, I cannot see how you generalize 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." He adds "Note also that the theorem (and even corollary 2) are false as stated" – in these two statements Grothendieck had spoken of the whole inertia group and not just an open subgroup – "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." Coming from Grothendieck, this kind of error is not in the least surprising – nor is the fact that in spite of it, his insight still yielded a powerful theorem. He answers reassuringly on October 5: "I just received your letter. 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..." and goes on to give full details of the proof. On October 30 – after an interlude devoted to L and Z-functions – he posts off another long letter, proving another result apparently conjectured by Serre ("I was disappointed that you did not find an expert on surfaces to solve your conjecture on abelian varieties...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.") Namely, he shows that in the case of curves, the unipotent action of the Galois group π passes to a trivial action on the quotient $H^1/(H^1)^\pi$; in other words, for curves, the action can be filtered in at most two steps.

Note that since the result that an open subgroup of the inertia group acts unipotently on $T_\ell(A)$ for any given ℓ prime to the residue characteristic is equivalent to saying that A has a semi-stable model over a finite extension of its field of definition, and this latter statement is independent of ℓ, the theorem also proves that if the action is unipotent for one ℓ then it is unipotent for all ℓ. This kind of "independence from ℓ" result – in the same spirit as the original proof of the fourth Weil conjecture (on Betti numbers) saying that the dimensions of the ℓ-adic cohomology groups are given by the degrees of the factors of the rational function $Z(X,t)$, and thus implying that these dimensions are independent of ℓ – was the initial stimulation for the idea of a motive.

1964-65: Motives

Motives made their appearance during the same exceptionally active (in terms of letter-writing) period, the fall of 1964. The first mention of motives in the letters – the first ever written occurrence of the word in this context – occurs in Grothendieck's letter from August 16: "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 '$\mathbb{Q}$' structure, coming from the theory of algebraic cycles. The sad truth is that 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 $\mathbf{M}(k)$." He is quite hopeful about doing this shortly: "I simply hope to arrive at an actual construction of the category of motives via this kind of heuristic consideration, and this seems to me to be an essential part of my 'long run program' [sic: the words 'long run program' are in (a sort of) English in the original]. On the other hand, I have not refrained from making a mass of other conjectures in order to help the yoga take shape."

Serre's answer is unenthusiastic: "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..." This way of expressing Grothendieck's idea underlines the similarity with the *anabelian theory* that he developed in the 1980's, several years after his grand departure from the established world of mathematics. This theory investigates algebraic varieties which are completely determined by their arithmetic fundamental group, i.e. their geometric fundamental group equipped with its canonical Galois action. The anabelian theory, however, represents a break with the linear aspects (cohomology groups=vector spaces) studied in the 1960's.

Grothendieck was not deterred from thinking directly in terms of motives in order to motivate and formulate his statements. On September 5, just days after first mentioning the idea, he calmly writes "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 expression 'if you like' shows how in his own mind he is moving away from considering the actual ℓ-adic cohomology, towards considering only some of its fundamental independent-from-ℓ properties – while respecting the fact that Serre may not want to follow him into so vague a terrain.

The letters of early September constitute the first technical discussion as to whether something is or is not a motive, here taken in the simple sense to mean that a family of ℓ-adic objects forms (or comes from) a motive if each member of the family is obtained by tensoring a fixed object defined over $\mathbb{Q}$ with $\mathbb{Q}_\ell$. The family they consider is the family of ℓ-adic Lie proalgebras associated to a field K. It is in these letters – precisely, in the one from September 5, 1964, that Grothendieck first introduces the motivic Galois group, which is constructed from the absolute Galois group G_K of K as follows: G_K acts on the ℓ-adic cohomology space, which can be viewed as a vector space over $\mathbb{Q}_\ell$, therefore there is a homomorphism $G_K \to \mathrm{GL}_{\mathbb{Q}_\ell}$; let G_0 denote the image, and let H denote the algebraic envelope of G_0. This H is the motivic Galois group associated to K, and Grothendieck's assertion is that H is described by 'motivic' equations, i.e. equations with coefficients in algebraic cycles which are independent of ℓ. From this, he notes that one should be able to recover the semi-simple part of G_0, which is thus motivic, but recovering the center of G_0 seems more difficult; still, Grothendieck seems optimistic that G_0 itself might be motivic. Serre, who prefers to think in terms of the Lie algebra $\mathfrak{g}_\ell$ associated to $G_0 \subset \mathrm{GL}_{\mathbb{Q}_\ell}$, produces a counterexample in his answer three days later, considering an elliptic curve E over a finite field k such that the ring of endomorphisms of E is an imaginary quadratic field K. Then he identifies the Lie algebras $\mathfrak{g}_\ell$ explicitly in terms of the ℓ-adic logarithm of the Frobenius of E, and explains that this family $\mathfrak{g}_\ell$ cannot be motivic. With some surprise, he notes that he is not able to construct a counterexample of this type over a number field.

The situation proves confusing as Grothendieck tries to get a feel, or what he terms a *yoga*, for what is motivic and what isn't: "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...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..."

In November, Grothendieck is continuing to develop the notion of motivic Galois group: "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 over number fields, especially over $\mathbb{Q}$, 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 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 $\mathbb{Q}$. I will probably send you another infinite letter one of these days, but I will need some time to arrive at a coherent set of conjectures."

Several more letters are exchanged, with all of the previous subjects still being touched upon: functional equations, good reduction of abelian varieties, especially elliptic curves. Then there is a silence of several months, interrupted only by a short letter from Serre in May 1965, responding to a phone call and giving an elegant two-page exposition of the theory of the Brauer group and factor systems. Silence again until August 1965, when Grothendieck addressed to Serre one of the key letters in the history of motives: the one containing the *standard conjectures*. This letter – written four months after the birth of Mathieu, his third child from Mireille – exudes an atmosphere of intense creativity in a totally new direction. This is the period in which Mireille described him as working at mathematics all night, by the light of a desk lamp, while she slept on the sofa in order to be near him, and woke occasionally to see him slapping his head with his hand, trying to get the ideas out faster.

In order to understand the origin and motivation of the standard conjectures, it is necessary to understand two basic things: how Grothendieck envisioned his conjectural category of motives, and what Serre had done in his 1960 note *Analogues kählériens des conjectures de Weil*.

Serre noticed that in the case of Kähler varieties, if one considers correspondences which are cohomology classes (rather than cycles) respecting the Hodge structure on both sides, then Weil's proof of the Weil conjecture for curves and abelian varieties, using algebraic correspondences and positivity of a certain form, goes through directly. In 1960, ℓ-adic cohomology was not sufficiently developed to apply this idea to it. But by 1965, Grothendieck was able to see that under certain conditions of algebricity of certain ℓ-adic cohomology classes, the same proof could be transported to any smooth projective variety over any algebraically closed field. This idea fitted into the broader framework of motives.

In 1965, Grothendieck's envisioned category of motives $\mathbf{M}(k)$ concerned smooth projective varieties over a field k. He wanted it to be a semi-simple abelian category, whose objects should correspond to 'motivic cohomology groups' $h(X)$ for the varieties X, equipped with decompositions $h(X) = \oplus_i h^i(X)$, which behave like absolute cohomology groups 'covering' or 'explaining' all specific types of cohomologies of X.

The functor $\mathrm{Var}_k \to \mathbf{M}(k)$ given by $X \mapsto h(X)$ should be contravariant, as for any cohomology theory. The $h^i(X)$ should behave more or less like finite-dimensional vector spaces; for instance, for any two objects H, K of $\mathbf{M}(k)$, $\mathrm{Hom}(H, K)$ should be a finite-dimensional $\mathbb{Q}$-vector space. Most importantly – the real meaning of the idea that objects of $\mathbf{M}(k)$ are 'motivic cohomology groups' – is that there should be natural functors from them to the actual cohomology groups associated to X:

- when char. $k = 0$, a functor T_{dR} such that $T_{dR}(h^i(X)) = H^i_{dR}(X)$;
- when $k \subset \mathbb{C}$, a functor T_{Betti} such that $T_{\mathrm{Betti}}(h^i(X)) = H^i_{\mathrm{Betti}}(X)$;
- for all $\ell \neq$ char. k, a functor T_ℓ such that $T_\ell(h^i(X)) = H^i_{et}(X, \mathbb{Q}_\ell)$.

Grothendieck actually sketched out the way to define the category $\mathbf{M}(k)$ of pure motives (corresponding to smooth projective algebraic varieties). Very (very, very) roughly speaking, he used the varieties X themselves as objects and $\mathbb{Q} \otimes C(X, Y)$ for the $\mathbb{Q}$-vector space of morphisms, where $C(X, Y)$ denotes the group of algebraic cycles of $X \times Y$ up to some equivalence relation. Various other objects need to be added to make sure the category possesses some of the desired properties.

In order to prove that $\mathbf{M}(k)$ is a semi-simple abelian category, in his letter of August 27, 1965, Grothendieck formulated the two famous *standard conjectures* inspired by Serre's *Analogues kählériens*.

"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 $\mathbb{Q}$, 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 $C^i(X) \simeq C^{n-i}(X)$."

Then after some discussion of this conjecture, he goes on to state the following conjecture, where ϵ denotes the form given by $\epsilon(xx') = L^{n-2r}(x) \cdot y$, L denoting cup product with ξ.

"**Conjecture B**: Assume that $n = 2m$, and let $P^m(X)$ be the kernel of the homomorphism from $C^m(X)$ to $C^{m+1}(X)$ given by multiplication by ξ. Then the form $(-1)^m \epsilon(xx')$ on $P^m(X)$ is positive definite."

In this letter, Grothendieck explains that conjecture A, which can be reinterpreted as a certain algebricity condition on cohomology classes of ℓ-adic cohomology giving correspondences respecting Hodge structure, makes it possible to apply Weil's original techniques for abelian varieties to prove the Weil conjectures in general (except for the absolute values of the eigenvalues, which are covered by conjecture B), just as Serre had noted in the Kähler case in 1960. He calls conjecture A "the 'minimum minimorum' to be able to give a usable rigorous definition of the concept of motive over a field." As it happens, he was not quite correct. In 1991, U. Jannsen showed that in order for $\mathbf{M}(k)$ to be abelian semi-simple, it is necessary and sufficient to use the equivalence relation of numerical equivalence. Homological equivalence would also work if, as is conjectured, it is actually the same as numerical equivalence, but algebraic equivalence does not work, and using it in the definition would yield a category which is not abelian semi-simple, although conjecture A as Grothendieck stated it may still hold. Jannsen was able to show these results without proving either of the standard conjectures. The generalization of the whole theory to arbitrary varieties, which would yield the yet undefined category of mixed motives, is much more difficult; even the standard conjectures would not suffice to prove that it has the desired properties.

In his letter, Grothendieck makes some initial attempts at sketching out proofs or directions of proofs of the conjectures, which however resisted his attempts and all other attempts to prove them. The letter ends with what constitutes the major obstacle: "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."

Although not the last letter, this letter represents the end of the Grothendieck-Serre correspondence in a rather significant way, expressing as it does the mathematical obstacle which prevented Grothendieck from developing the theory of motives further; the standard conjectures are still open today. Of course he remained incredibly intense and hardworking for several more years, continuing the SGA seminar until 1969, the writing of the EGAs and ever more research. Yet this letter has a final feel to it. Only two more letters date from before the great rupture of 1970: a short one from Serre from December 1966, answering a rather precise question about representations of GL_n, probably in response to a phone conversation, then a letter from Grothendieck to Serre from January 1969, also referring to a previous conversation, about Steinberg's theorem. Then fourteen years of silence.

1984-1987: The last chapter

The six letters from these years included in the Correspondence – a selection from a much larger collection of existing letters – are intriguing and revealing, yet at the same time somewhat misleading. From the tone of some of Grothendieck's comments ("As you probably know, I no longer leave my home for any mathematical meeting, whatever it may be," or "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,") it may seem as though by the 1980's, he had completely abandoned mathematics. But this was in fact far from the case. Quite the contrary, although he did stop working in mathematics for months at a time, there were other months during which he succumbed to a mathematical fever in the course of which he filled thousands of manuscript pages with "grand sketches" for future directions, finally letting his imagination roam, no longer reining himself in with the necessity of advancing slowly and steadily, proving and writing up every detail. A famous text ("Sketch of a Program"), three enormous informally written but more or less complete manuscripts and thousands of unread handwritten pages from his hand date from the 80's and 90's, describing more or less visionary ways of renewing various subjects as concrete as the study of the absolute Galois group over the rationals, or as abstract as the theory of n-categories. And this does not count the many thousands of non-mathematical pages he wrote and still writes.

At the time of the exchange of letters included in the published Correspondence, Grothendieck had just completed his monumental mathematico-autobiographical work *Récoltes et Semailles* (Reaping and Sowing), retracing his life and his work as a mathematician and, over many hundreds of pages, his feelings about the destiny of the mathematical ideas that he had created and then left to others for completion. He sent the successive volumes of this work to his former colleagues and students. The exchanges between Serre and Grothendieck on the topic of this text underscore all of the differences in their personalities already so clearly visible in their different approaches to mathematics.

Serre, a lover as always of all that is pretty ("les jolies choses" is one of his favorite expressions), clean-cut, attractive and economical, viscerally repelled by the darker, messier underside of things, reacts negatively to the negative ("I am sad that you should be so bitter about Deligne..."), positively to the positive ("My way of thinking is...quite distant from yours, which explains why we complemented each other so well for ten or fifteen 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...") and uncomprehendingly to the ironic ("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."[5])

Grothendieck picks up on this at once, and having known Serre for twenty years, is not in the least surprised: "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!)" After all, "One thing that had already struck me about you in the sixties was that the very idea of examining oneself gave you the creeps."

It is true enough that self-analysis in any form strikes Serre as a pursuit fraught with the danger of involuntarily expressing a self-love which to him appears in the poorest of taste. Grothendieck, trying in all honesty to take a closer look at his acts and feelings during the time of his most intense mathematical involvement, speaks of his "absence of complacency with respect to myself." Serre, disbelieving in the very possibility of self-analysis without complacency, and already struggling with the embarrassment of hundreds and hundreds of pages of self-observation, writhes at this phrase which – worse than ever – analyses the analysis, and wonders how Grothendieck could have typed it at all without

[5] A misunderstanding! Grothendieck's sarcastic references to the 'Good Lord's theorem' meant that this theorem was not attributed by name to its author, whom he felt to have been neglected and mistreated by the mathematical establishment.

laughing: "How can you?"

But where, exactly, does he perceive complacency, Grothendieck asks in some surprise. There is no need for Serre to answer. It is obvious that for him, the act of looking at oneself implies self-absorption, which as a corollary necessarily implies a secret self-satisfaction, something which perhaps exists in everyone, but should remain hidden at all costs.

And then, if one is going to do the thing at all, should one not do it completely? Hundreds of pages of self-examination, hundreds of pages of railing because the beautiful mathematical work accomplished in the fifties and sixties met a fate of neglect after the departure of its creator – mainly because basically no one, apart from perhaps Deligne, was able to grasp Grothendieck's vision in its entirety, and therefore perceive how to advance it in the direction it was meant to go – but Serre reproaches him the fact that the major question, "the one every reader expects you to answer," is neither posed nor answered: *"Why did you yourself abandon the work in question?"* Clearly annoyed by this, he goes on to formulate his own guesses as to the answer: "despite your well-known energy, you were quite simply tired of the enormous job you had taken on..." or "one might ask oneself 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...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?"

Grothendieck's answer to this letter and the subsequent exchanges are not included in the present publication, but he did answer in fact, referring to a passage in *Récoltes et Semailles* in which he powerfully expresses the feeling of spiritual stagnation he underwent while devoting twenty years of his life exclusively to mathematics, the growing feeling of suffocation, and the desperate need for complete renewal which drove him to leave everything and strike out in new directions. Reading *Récoltes et Semailles*, it is impossible to believe that Grothendieck felt that his mathematical methods were running into a dead end, whatever their efficacity on certain types of number theoretic problems might or might not have been. His visions both for the continuation of his former program and for new and vast programs are as exuberant as ever; what changed was his desire to devote himself to them entirely. *Récoltes et Semailles* explains much more clearly than his letters how he came to feel that doing mathematics, while in itself a pursuit of extraordinary richness and creativity, was less important than turning towards aspects of the world which he had neglected all his life: the outer world, with all of what he perceived as the dangers of modern life, subject as it is to society's exploitation and violence, and the inner world, with all its layers of infinite complexity to be explored and discovered. And, apart from the sporadic bursts of mathematics of the 1980's and early 90's, he chose to devote the rest of his life to these matters, while Serre continued to work on mathematics, always sensitive to the excitement of new ideas, new areas, and new results. In some sense, the difference between them might be expressed by saying that Serre devoted his life to the pursuit of truth in beauty, Grothendieck to the pursuit of truth in duty.