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Column Editor: **Osmo Pekonen,**
Agora Centre, 40014
University of Jyväskylä, Finland
e-mail: pekonen@mit.jyu.fi

Grothendieck-Serre Correspondence

by Alexandre Grothendieck, Pierre Colmez (editor), and Jean-Pierre Serre

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REVIEWED BY LEILA SCHNEPS

The Grothendieck-Serre correspondence 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 I possess. More modestly, what I 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 burst in the 1980s), 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 their work profoundly renewed the entire domain of algebraic geometry in its language, in its concepts, in its methods and of course in its results. The 1950s, 1960s, and early 1970s 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 men are very different in character. Grothendieck's are the more revealing of the actual creative process of mathematics, and the more surprising for the questions he asks and for their difference with the style of his articles. 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 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 usually add something as well. 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, 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 in abstract algebraic geometry (sheaves had been introduced some years earlier by Leray in a very different context). 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, were wracked with passion and conflict. He was stateless, with no permanent job. As it was legally impossible to hold a university position in France, 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; 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. Throughout his life, Grothendieck would keep his mathematical activities sharply separate from his private affairs, about which next to nothing appears in his letters. He also had a lifelong habit of working and writing through the night.

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, which constituted a real revolution in the theory. He had completed his thesis in 1953, and he 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. 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.

In those letters, Grothendieck explains that he is in the process of 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, which established the basis of many of the notions of modern homological algebra. In fact, he wanted to teach a course on Cartan and Eilenberg's new book, but he couldn't get hold of a copy, and so he was compelled to work everything out for himself, following what he "presumed" to be their outline.

The Tohoku paper introduced abelian categories, extracting the main defining features of some much-studied categories such as abelian groups or modules, introducing notions such as having "enough injectives," and extending Cartan-Eilenberg's notion of derived functors of functors of the category of modules to a completely general notion of derived functors. 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 here: 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 was back in Paris with a temporary job at the CNRS, obtained thanks to help of Serre and others, who were always searching for a way to allow the homeless maverick to remain permanently in France. Serre, at this time, was 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.

During the period covered by these early letters, the notion of a scheme was just beginning to make its appearance. 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, Hirzebruch . . . and Grothendieck

The classical Riemann-Roch theorem, 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 computes a difference of the dimensions of two vector spaces of meromorphic functions on the curve with prescribed behavior at the points of the divisor D (the left-hand side) in terms of an expression in integers associated topologically with the curve (the right-hand side).

In the early 1950s, 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 he generalized this expression to any n-dimensional non-singular projective variety X equipped with a vector bundle E as the alternating sum $\sum(-1)^i \dim H^i(X, E)$.

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.

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 the end, what Grothendieck brought to the Riemann-Roch theorem is one of the basic features of all of his mathematics, and it 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. 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 organized 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 Française* in 1958 [BS]. 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. Fascinated and overwhelmed by his powerful personality, she 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 from 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 . . . it appears that most statements make sense, and are true, if we assume onlyAto be a commutative ring with unit . . . 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. 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—fundamental groups, in particular—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 it 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 would be allowed to visit.

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 immigrant 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, whose progress continues to be seriously overestimated.

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 1950s. Weil himself proved his conjectures for curves and abelian varieties, and he 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 both Serre and Grothendieck; as the latter 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.

Serre used Zariski topology and tried cohomology over the field of definition of the variety; even though this field was in characteristicp, 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. He writes some of his ideas to Grothendieck, but the response is less than enthusiastic: "I have no comments on your attempts . . . 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 short note *Analogues kählériens*, an outcome of those same "attempts" which left Grothendieck cold at the time, was to play a fundamental role in his reflections aiming at a vast generalization of the Weil conjectures.

On November 15, 1959, came the news that Michel Raynaud, a 21-year-old student at the time, describes as a thunderclap. Serre writes to Grothendieck: "First of all, a surprising piece of news: Dwork phoned Tate the evening of the day before yesterday to say he had proved the rationality of zeta functions (in the most general case: arbitrary singularities). He did not say how he did it (Karin took the call, not Tate) . . . It is rather surprising that Dwork was able to do it. Let us wait for confirmation!" 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. He continues to discuss this in the summer of 1960, the very 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 was 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 AU1→ Sasha after Grothendieck's father. 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."

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, and lead to a tense discussion with Serre about their inclusion in the Bourbaki draft for *Commutative Algebra*. Serre defends them for various reasons including the fact that several people had "sweated" over them: "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; I still recall 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. "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 very same letters, as well as a famous one dated October 22, 1961, and adressed 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, but while the two factions awaited a cease-fire, hostilities continued, 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). On October 5, a curfew on 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 preoccupation ten years later, when he left the IHES after a fracas because he discovered that a small percentage of its funding was of military origin, the tone he adopts in criticizing 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.

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, gently insisting that mathematicians should make some effort to avoid military service, not because they should be treated specially, but because each group of people can be responsible for organizing its own exemptions. A true pacifist, he writes: "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 his 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 containing the same ideas cross. 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 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 it 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).

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, probably proving the functional equation by the end of 1966, when the SGA 5 lectures were completed. Rather than attacking the one remaining conjecture directly, he sketched out a vast generalization of the Weil conjectures and stated the difficult 'standard conjectures' 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.

1964–1965: Reduction of Abelian Varieties Over Local Fields—and Motives

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 during 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, with local fields playing a major role. 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 telephone, 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.

These letters contain "independence from ℓ" type results (for instance, the statement that an open subgroup of the inertia group acts unipotently on $T_\ell(A)$ for any given ℓ prime to the residue characteristic if and only if A has a semistable model over a finite extension of its field of definition), reminiscent of 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. This circle of ideas was the initial stimulation for the idea of 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 overk, 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 . . . " But Grothendieck was not deterred from thinking directly in terms of motives in order to motivate and formulate his statements. 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 $\mathbb{Q}_\ell$.

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 with 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 the study so as to be near him, and woke occasionally to see him slapping his head with his hand, trying to get the ideas out faster.

Grothendieck termed the first of these conjectures, called conjecture A, "the 'minimum minimorum' to be able to give a usable rigorous definition of the concept of motive over a field." He also 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: 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 1980s, he had completely abandoned mathematics. 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 1980s and 1990s, 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 (" . . . 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."*)

*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.

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 chapter after chapter of self-observation, writhes at this phrase which—worse than ever—analyzes the analysis, and wonders how Grothendieck could have typed it at all without 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? Pages and pages of self-examination, 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 for 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 1980s and early 1990s, 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 beauty, Grothendieck to the pursuit of truth.

Equipe Analyse Algébrique
Universite de Paris 6
175 rue du Chiraleret
75013 Paris
France
e-mail: Leila@math.jussieu.fr

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The death of his father was noted earlier.