The Grothendieck-Serre correspondence

Toulouse, 6 December 2024

This is a short survey of the Grothendieck-Serre correspondance.

This correspondance was published jointly by the SMF and the AMS in 2004. The AMS version is bilingual.

I refer to Leila Schneps' long review of the correspondance in the volume

Alexandre Grothendieck: a mathematical portrait. Edited by Leila Schneps. International Press, Somerville, MA, 2014. viii+307 pp.

for a much more complete presentation of the correspondence and its scientific context.

Tôhoku

The initial drafts of the article

Grothendieck, A.; *Sur quelques points d'algèbre homologique.* Tôhoku Math. J. (2) **9**, 119–221 (1957).

are discussed several times at the beginning of the correspondence (June 1955, July 1955, Sept. 1956 . . . , the "multiplodoque d'algèbre homologique").

Grothendieck first considered publishing this text in *Trans. Amer. Math. Soc.* However, Eilenberg (who was one of the editors) turned down the paper, in part because of its length.

Grothendieck then tried to publish it in *American J. of Math.*, where it was also rejected, on the argument that he had already published an article there about fibre bundles on the projective line.

After that, he turned to the *Mémoires de la SMF*, but no joy there either.

In the end, Tannaka accepted to publish it in *Tohoku Math. J.* (cf. letter dated Nov. 1956).

The article mentioned above is now often referred to as the "Tôhoku" paper. It builds on the book

Cartan, Henri; Eilenberg, Samuel; *Homological algebra.* Princeton University Press, Princeton, N. J., 1956.

The main contribution of the article is to develop a categorical formalism in which cohomology groups can be defined. This formalism can in particular be used to define the cohomology of sheaves, singular cohomology, group cohomology, and also the Ext and Tor functors.

A (small) part of the results and notions introduced in the article (in particular the notion of *abelian category*) can already be found in an earlier article of Buchsbaum, which was published in *Trans. Amer. Math. Soc.* (**80**, 1–34 (1955)). This overlap is another reason why Eilenberg opposed its publication in this journal.

The Riemann-Roch theorem

This theorem asserts that for a projective morphism $f : X \to Y$ between smooth varieties over a field, the formula

$$\mathrm{ch}(\mathrm{R}^{\bullet} f_*(F)) = f_*(\mathrm{Td}(f)\mathrm{ch}(F)) \quad (\,*\,)$$

holds for any coherent sheaf F on X.

Here $\mathrm{ch}(\cdot)$ is the *Chern character*, and $\mathrm{Td}(\cdot)$ is the *Todd class*. Both are cohomological invariants of vector bundles.

In the formula $(*)$, the Chern character of the linear combination of coherent sheaves $\mathrm{R}f_*(F)$ is computed using resolutions by vector bundles (such resolutions were still called "syzygetic" by Serre, cf. letter dated 26 Feb. 1955), a technique which is generalised in the Tôhoku paper.

The formula $(*)$ generalises a formula of Hirzebruch (which corresponds to the case where Y is a point and the field has char. 0), which he proved in his book *Neue topologische Methoden in der algebraischen Geometrie* (Springer 1956), using topological methods.

A simplified proof of $(*)$ is outlined in the letters dated 1 and 12 Nov. 1957. Borel and Serre later published the results of Grothendieck in the article

Borel, A.; Serre, J.-P.; Le théorème de Riemann-Roch. *Bull. Soc. Math. France* **86** 97–136 (1958)

after running a seminar on the material in Princeton in 1957, which was based on earlier notes by Grothendieck. These notes were later published in the seminar proceedings SGA6.

Serre-Grothendieck duality

It is probably in the letters sent in July and in Dec. 1955 that the modern form of the duality theorem for coherent sheaves makes its first appearance.

This theorem asserts the existence of an isomorphism

$$H^{n-p}(X, F)^{\vee} \simeq \mathrm{Ext}^{p}(X, F, \omega_X)$$

for any coherent sheaf F on a smooth and projective variety over a field.

It is Grothendieck's attempt to generalise this formula to a relative setting that led him later to introduce the notion of *derived category*, which became the object of the dissertation of Verdier.

Although the text of Verdier's dissertation was available to many mathematicians at the time, it was only formally published and properly edited 40 years later (by G. Maltsiniotis (*Des catégories dérivées des catégories abéliennes, Astérisque* No. 239 (1996)).

Verdier published a summary of his dissertation in the volume SGA$\frac{1}{2}$, which was published in 1977, after Grothendieck's departure from mathematics.

The notion of derived category is not discussed in the Grothendieck-Serre correspondence.

Algebraic Geometry and Analytic Geometry (GAGA)

The main result of the article

Serre, J.-P.; *Géométrie algébrique et géométrie analytique.* Ann. Inst. Fourier, Grenoble **6**, 1–42 (1955–1956).

asserts that on a projective variety over $\mathbb{C}$, the category of analytic coherent sheaves is equivalent to the category of algebraic coherent sheaves. This generalises an earlier result of Chow (obtained in 1949).

A special case of this theorem is the well-known statement that a meromorphic function with at most a pole at ∞ is rational.

This article is discussed in several letters (Serre-Grothendieck on 22 Dec. 1955, Grothendieck-Serre on 12 Jan. 1956,...).

The techniques of Grothendieck's "Tôhoku" paper already play an important role in this text.

In the Grothendieck-Serre letter of 5 Nov. 1958, the analogue of Serre's theorem in formal geometry is already mentioned.

This is the following statement. Let $\widehat{S}$ be a proper formal scheme over a complete noetherian ring, which is the completion of a scheme S, which is proper over A. Then the category of formal coherent sheaves on $\widehat{S}$ is equivalent to the category of coherent sheaves on S.

It is interesting that this theorem was known to Grothendieck before Dieudonné started writing the *Éléments de Géométrie Algébrique* (on the basis of a detailed draft by Grothendieck).

In the letter mentioned above, a formal scheme is called "variété holomorphe" rather than *schéma formel*, probably to mirror the terminology used by Serre.

Military service and Algerian war

"...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..."
(Grothendieck-Cartan, 22 oct. 1961)

"I do not agree with you that nothing should be done against the military service for gifted scientists in particular before the end of the Algerian war. To start with, as far as injustice is concerned 'when lives are at stake', if it is an injustice to exempt certain people from national service, then the difference between doing so during or after a time of guerilla war is one of degree and not of essence. I do not think that the danger of losing one's life is such, at this point, that it has become more important than the loss of two years of training (for any young person, scientist or otherwise), leaving aside entirely the moral question (to which most people are apparently indifferent). . . ." (Grothendieck-Serre, 31 Oct. 1961).

In this letter, Grothendieck responds to a letter by Serre in which he commented on the letter sent by Grothendieck to Cartan. In particular, he addresses the passage of Serre's letter

". . . an exemption for scientists would be a truly shocking inequality when lives are at stake. . ."

The argument is about exemption from military service, which existed in the USA but not in France.

Grothendieck's pacifist convictions are already visible in the following other passage of the same letter:

"... 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."

EGA & SGA

In several letters, Grothendieck writes about his work on the *Éléments de Géométrie Algébrique* (EGA). He started with this task in Sept. 1958, and in his letter of 18 August 1958, he explained that he hoped to complete his treatise in at most three years (!).

Serre proofread and annotated part of the text. A seminar on (or a part of) the first volume of EGA was run in Princeton in the autumn of 1959.

The proceedings of the *Séminaire de Géométrie Algébrique* (SGA) are also mentioned in several letters (eg Grothendieck-Serre, 24 Sept. 1964). This seminar started in 1960 with a session on the algebraic fundamental group.

In the letter dated 18 Aug. 1959 letter, Grothendieck sketches the table of contents of the treatise that he wanted to write. It is worth noting that all the subjects mentioned in this outline were studied in the SGA seminars. The proceedings of the SGA seminars were understood by him as a first draft for the treatise EGA .

"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. In between, I don't know when, the "big existence theorem" with Picard etc., some algebraic curves, and abelian schemes. Unless there are unexpected difficulties or I get bogged down, the multiplodocus should be ready in 3 years time, or 4 at the outside. We will then be able to start doing algebraic geometry!"

(Grothendieck-Serre, letter dated 18 August 1959)

Weil restriction

Let $T \to S$ be a morphism of schemes. Let X/T be a scheme over T.

The operation of "Weil restriction" refers to the construction of a scheme $R_{T/S}(X)$ over S, which represents the functor $\mathrm{Sch}/S \to \mathrm{Ens}$

$$\mathrm{Hom}_S((\bullet), R_{T/S}(X)) \simeq \mathrm{Hom}_T((\bullet) \times_S T, X).$$

In the letter dated 31 Oct. 1959, Grothendieck establishes the existence of $R_{T/S}(X)$ when $T \to S$ is finite and flat, and he also describes the main properties of $R_{T/S}(X)$.

Around the same time, Greenberg proved a similar results in an arithmetic situation, eg when $S = \mathrm{Spec}\ \mathbb{F}_p$ and $T = \mathrm{Spec}\ \mathbb{Z}/p^n\mathbb{Z}$ (see *Schemata Over Local Rings*, Annals of Math., 2nd Ser., Vol. 73, No. 3 (1961)).

The geometric proof of the local monodromy theorem

Let $f : X \to S$ be a proper morphism of complex analytic spaces. Suppose that S is an open disk and that f is smooth outside of 0.

For any $t \in S\backslash\{0\}$ and all $k \geqslant 0$, one then has a representation $\pi_1(S\backslash\{0\}) \simeq \mathbb{Z} \to H^k(X_t, \mathbb{Z})$. Let $T : H^k(X_t, \mathbb{Z}) \to H^k(X_t, \mathbb{Z})$ the image of 1.

The *local monodromy theorem* asserts that there exists $a \geqslant 0$ such that $(T^a - 1)^{k+1} = 0$.

In the letter dated 30 Oct. 1964, Grothendieck gives a geometric proof of this theorem (maybe the first? See L. Illusie, par. 2.1 dans *Autour du théorème de la monodromie locale*, Astérisque **223** (1994))

His proof is based on a calculation involving *vanishing cycles* and resolution of singularities.

The formula of Woods Hole*

In July 1964, a "Summer Institute" on algebraic geometry was organised in the small town of Woods Hole (in the state of Massachusetts, USA). The mathematicians Atiyah, Bott, Serre, Verdier and Mumford attended this conference. In the letter dated 2 August 1964, Serre describes the following result, which had apparently already been known to Shimura, and which had been of the main subjects of the meeting.

Let X be a smooth and projective variety over a field, and let f be an endomorphism, whose fixed points are assumed to be isolated. Let F/X be a vector bundle and let $f^*F \to F$ be a morphism of coherent sheaves.

The "Woods Hole" formula asserts that

$$\sum_{k\geqslant 0}(-1)^k \mathrm{Tr}(f^*, H^k(X,F)) = \sum_{P\in \mathrm{Fixed}(f)} \mathrm{Tr}(f_P)/\mathrm{det}(1-\mathrm{d}f_P)$$

if the quantities $\mathrm{det}(1-\mathrm{d}f_P)$ do not vanish.

The Woods Hole formula has several consequences, among which the *Weyl character formula* in the theory of representations of Lie groups.

A generalisation of this formula, called the formula of Lefschetz-Verdier, is proven in SGA 5 (Appendix to Exp. III).

A different kind of generalisation was given by Atiyah and Bott and their article *A Lefschetz Fixed Point Formula for Elliptic Complexes: I*, Annals of Math., 2nd Series, 86 (2): 374–407 (1967).

However, in his answer to Serre's letter (dated 8 August 1964), Grothendieck does not seem very taken by this result:

"I do not find no. 1 very exciting, despite the pretty applications; the fixed-point theorem itself seems to me nothing more than a variation on a well-worn theme!"

Tate and the beginnings of rigid analytic geometry

Around 1960, Tate started with his attempts to define *rigid analytic geometry*, which he wanted to be a non-archimedean analogue of complex analytic geometry.

Using an ad hoc construction (based on Weierstrass equations) he first defined what is now known as the *Tate curve*, which gives an analytic description of certain elliptic curves over $\mathbb{Q}_p$.

More precisely, let $E/\mathbb{Q}_p$ be an elliptic curve with multiplicative reduction. Tate described a surjection $\bar{\mathbb{Q}}_p^* \to E(\bar{\mathbb{Q}}_p)$ which is compatible with the action of the Galois group $\mathrm{Gal}(\bar{\mathbb{Q}}_p|\mathbb{Q}_p)$.

Tate later generalised his theory, and his work was studied by several authors (Kiehl, Bosch, Raynaud…) in the 1970s. His generalisation makes use of the notion of Grothendieck topology.

However, here again, Grothendieck did show much interest for Tate's initial result:

"... Tate has written to me about his elliptic curves stuff, and has asked me if I had any ideas for a global definition of analytic varieties over complete valuation fields. I must admit that I have absolutely not understood why his results might suggest the existence of such a definition, and I remain skeptical. Nor do I have the impression of having understood his theorem at all; it does nothing more than exhibit, via brute formulas, a certain isomorphism of analytic groups; one could conceive that other equally explicit formulas might give another one which would be no worse than his (until proof to the contrary!)" (Grothendieck, letter dated 18 Aug. 1959).

The Grothendieck group of integral linear representations of a linear group scheme

In the letter dated 7 Dec. 1966, Serre describes the structure of his proof of the fact that

$$R_{\mathbb{Z}}(\mathrm{GL}_n) \simeq R_{\mathbb{Q}}(\mathrm{GL}_n) \simeq \mathbb{Z}[\Lambda^1(\mathrm{Id}_n), \ldots, \Lambda^n(\mathrm{Id}_n)]_{\mathrm{det}}$$

which implies that $R_{\mathbb{Z}}(\mathrm{GL}_n)$ is a special λ-ring.

The proof of this theorem was later published in the classic paper

Groupes de Grothendieck des schémas en groupes réductifs déployés. Inst. Hautes Études Sci. Publ. Math. **34**, 37–52 (1968).

This result also implies that the Grothendieck group of vector bundles of a scheme is a special λ-ring, without having to resort to a geometric splitting construction.

Other topics discussed in the correspondence

Here are a few more important topics, which are discussed in the correspondence, but which I can only briefly mention for lack of time:

• The Weil conjectures, the standard conjectures (see eg Grothendieck-Serre, 27 Aug. 1965).

• Motives (see eg Grothendieck-Serre, 16 Aug. 1964)

• The formula of Néron-Ogg-Shafarevich (Grothendieck-Serre, April 1963)

• Geometric class field theory (Serre-Grothendieck, 9 Nov. 1958)

• The theory of the canonical lifting of ordinary abelian varieties (Serre-Grothendieck, 2 Aug. 1964).

• The work of Néron of the models of abelian varieties over discrete valuation rings, the Serre-Tate criterion of good reduction (Grothendieck-Serre, 19 Oct. 1961, . . .).

The end of the correspondance

Grothendieck and Serre corresponded a lot between 1955 and 1961, and also in 1964. There is however a gap of about fifteen years between the letter dated 15 Jan. 1969 and the next one, dated 2 Sept. 1984.

Here are a few highlights of the (published) letters exchanged after 1984.

• Serre's modularity conjecture, outlined in the letter from Serre to Grothendieck dated 31 Dec. 1986. This conjecture was proven in Wintemberger et Khare in 2009 (cf. *Serre's modularity conjecture (I) & (II)*, Invent. Math. **178** (3)). It implies the Taniyama-Weil conjecture (almost completely) proven by Wiles in 1996, and as such it implies Fermat's last theorem, although this was probably not known to Serre.

Serre's reaction to Grothendieck's rejection (in *Récoltes et Semailles*, which Grothendieck had sent to Serre) of the methodology of some of Deligne's work in the 1970s:

• "For example, I found very beautiful what Deligne does in LN 900 (the text you reject with horror...) to get around the problem of Hodge cycles and obtain highly useful results nevertheless (on ℓ-adic representations, for example). I know that the very idea of "getting around a problem" is foreign to you - and maybe that is what shocks you the most in Deligne's work. (Another example: in his proof of the Weil conjectures, he "gets around" the "standard conjectures" - this shocks you, but delights me)."
(Serre-Grothendieck, 23 July 1985)

Serre's attempt to understand why Grothendieck left the Parisian mathematical scene in 1970:

• "...I have the impression that, despite your well-known energy, you were quite simply tired of the enormous job that you had taken on..." (Serre-Grothendieck, 8 Feb. 1986)

• "...Whence the question: did you not come, in fact, around 1968-1970, to realise that the "rising tide" method was powerless against this type of question [number theory and modular forms], and that a different style would be necessary - which you did not like?" (ibid.)