Alexandre Grothendieck 1928–2014, Part 1

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In the eyes of many, Alexandre Grothendieck was the most original and most powerful mathematician of the twentieth century. He was also a man with many other passions who did all things his own way, without regard to convention.

This is the first part of a two-part obituary; the second part will appear in the April 2016 *Notices*. The obituary begins here with a brief sketch of Grothendieck's life, followed by a description of some of his most outstanding work in mathematics. After that, and continuing into the April issue, comes a set of reminiscences by some of the many mathematicians who knew Grothendieck and were influenced by him.

Biographical Sketch

Alexandre Grothendieck was born on March 28, 1928, in Berlin. His father, a Russian Jew named Alexander Shapiro, was a militant anarchist who devoted his life to the struggle against oppression by the powerful. His mother, Hanka Grothendieck, came from a Lutheran family in Hamburg and was a talented writer. The upheavals of World War II, as well as the idealistic paths chosen by his parents, marked his early childhood with dislocation and deprivation. When he was five years old, his mother left him with a family she knew in Hamburg, where he remained until age eleven. He was then reunited with his parents in France, but before long his father was deported to Auschwitz and perished there.

By the war's end the young Alexandre and his mother were living in Montpellier, where he was able to attend the university. In 1948 he made contact with leading mathematicians in Paris, who recognized both his brilliance and his meager background. A year later, on the advice of Henri Cartan and André Weil, he went to the Université Nancy, where he solved several outstanding problems in the area of topological vector spaces. He earned his doctoral degree in 1953, under the direction of Laurent Schwartz and Jean Dieudonné.


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Grothendieck as a child.

Because Grothendieck was stateless at the time, obtaining a regular position in France was difficult. He held visiting positions in Brazil and the United States before returning to France in 1956, where he obtained a position in the Centre National de la Recherche Scientifique (CNRS). In 1958, at the International Congress of Mathematicians in Edinburgh, he gave an invited address that proved to be a prescient outline of many of the mathematical themes that would occupy him in the coming years.

That same year he was approached by a French mathematician businessman, Léon Motchane, who planned to launch a new research institute. This was the start of the Institut des Hautes Études Scientifiques (IHES), now located in Bures sur Yvette, just outside Paris. Grothendieck and

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Photo courtesy of Paulo Rihenhoim.

Grothendieck as a student in Nancy, 1950.

Dieudonné were the institute's first two professors. While he was at the IHES, Grothendieck devoted himself completely to mathematics, running a now-legendary seminar and collecting around him a dedicated group of students and colleagues who helped carry out his extraordinary mathematical ideas. Much of the resulting work from this era is contained in two foundational series, known by the acronyms EGA and SGA: *Éléments de Géométrie Algébrique* and *Séminaire de Géométrie Algébrique du Bois Marie*.

In 1970 Grothendieck abruptly resigned from the IHES and changed his life completely. The reasons for this change are complex and difficult to summarize, but it is clear that he was deeply affected by the student unrest that seized France in 1968 and became convinced that he should focus his energy on pressing social issues, such as environmental degradation and the proliferation of weapons. He began to lecture on these subjects and founded an international group called Survivre et Vivre (called simply Survival in English). While this effort was not a political success, Grothendieck did have, at the grassroots level, a significant influence on others sharing his concerns. After his death leaders in the "back to the land" movement wrote tributes to him. He briefly held positions at the Collège de France and the Université de Paris Orsay before leaving Paris in 1973. He then took a position at the Université de Montpellier and lived in the French countryside.

Photo by Paul R. Halmos. Paul R. Halmos Photograph Collection, e_ph_0113_01, Dolph Briscoe Center for American History, University of Texas at Austin.

Grothendieck in Chicago, around 1955.

In 1984 Grothendieck applied to the CNRS for a research position. His application consisted of his now-famous manuscript *Esquisse d'un Programme (Sketch of a Program)*, which contained the seeds for many new mathematical ideas subsequently developed by others. This marked his first public foray into mathematics after his break with the IHES, but not his last. While he never again returned to producing mathematics in a formal, theorem-and-proof style, he went on to write several unpublished manuscripts that had deep influence on the field, in particular *La Longue Marche à Travers la Théorie de Galois (The Long March through Galois Theory)* and *Pursuing Stacks*.

Selected Works About Grothendieck

Pierre Cartier, Alexander Grothendieck: A Country Known Only By Name. *Notices*, April 2015.

Luc Illusie (with Alexander Beilinson, Spencer Bloch, Vladimir Drinfeld et al.), Reminiscences of Grothendieck and His School. *Notices*, October 2010.

Allyn Jackson, Comme Appelé du Néant—As if summoned from the void: The life of Alexandre Grothendieck. *Notices*, October 2004 and November 2004.

Allyn Jackson, Grothendieck at 80, IHES at 50. *Notices*, September 2008.

Valentin Poénaru, Memories of Shourik. *Notices*, September 2008.

Michel de Pracontal, A la recherche de Grothendieck, cerveau mathématicien (In search of Grothendieck, mathematical brain). *Mediapart*, three-part series published in 2015, `www.mediapart.fr`.

Winfried Scharlau, Who is Alexander Grothendieck? *Notices*, September 2008. (Translation from the German "Wer ist Alexander Grothendieck?", published in the *Annual Report 2006* of the Mathematisches Forschungsinstitut Oberwolfach.)

Winfried Scharlau, *Wer ist Alexander Grothendieck?*, by Winfried Scharlau. Available on Amazon.com through Books on Demand, 2010. ISBN-13: 978-3-8423-7147-7 (volume 1), 978-3-8391-4939-3 (volume 3), 978-3-8423-4092-3 (English translation of volume 1).

Leila Schneps, editor, *Alexandre Grothendieck: A Mathematical Portrait*. International Press, 2014.

Photo by Erika Ifang.

Grothendieck in 1988.

With his CNRS position he remained attached to the Université de Montpellier but no longer taught. From 1983 to 1986 he wrote another widely circulated piece, *Récoltes et Semailles (Reaping and Sowing)*, which is in part an analysis of his time as a mandarin of the mathematical world. *Récoltes et Semailles* became notorious for its harsh attacks on his former colleagues and students.

Grothendieck's severance from the mathematical community meant that he received far fewer prizes and awards compared to other mathematicians of his stature. He received the Fields Medal in 1966 while he was still at the IHES and still active in mathematics. Much later, in 1988, he and Pierre Deligne were awarded the Crafoord Prize from the Royal Swedish Academy of Sciences; Grothendieck declined to accept it.

Grothendieck retired in 1988. He devoted himself to his writing, which focused increasingly on spiritual themes. Around this time he had episodes of deep psychological trauma. In 1991 he went to live in complete isolation in Lasserre, a small village in the French Pyrénées, where he continued to write prodigiously. When he died on November 13, 2014, he left behind thousands of pages of writings.

Grothendieck's Mathematical Work

The greatest accomplishments in Grothendieck's mathematical life were in algebraic geometry and took place in a twelve-year period of the most intense concentration from roughly 1956 to 1968. Before this he had done major work in functional analysis in the period 1950–54, and later, at Montpellier, he worked on many ideas, some of which are summarized in his *Esquisse d'un programme* but which remain mostly unpublished. To cover all this work would require many experts, and in this review we will only sketch what we believe to be his four most outstanding contributions to algebraic geometry. What is most stunning is that in each of them he created a major new abstract theory that then led to the solution of a major problem in algebraic geometry as it stood when he started. Thus we omit many important parts of his work, notably the early work on topological vector spaces, then the theories of duality, flat descent, crystalline cohomology, motives, and topoi done at the IHES, and finally his "dessin d'enfants" and much more from the Montpellier period.

K-theory and the Grothendieck-Riemann-Roch Theorem for Morphisms $f : X \to Y$

The first stunning innovation of Grothendieck was his generalization of the Riemann-Roch theorem that he proved in 1956. In 1954 Hirzebruch had generalized the classical Riemann-Roch theorem for curves and surfaces. His theorem calculated the Euler characteristic of any vector bundle $\mathbb{E}$ on a smooth projective variety X over $\mathbb{C}$ in terms of the Chern classes of $\mathbb{E}$ and of the tangent bundle of X. Following his philosophy that theorems will always fall out naturally when the appropriate level of generality is found, Grothendieck did three things:

(a) he replaced the bundle by an arbitrary coherent sheaf,

(b) he replaced the smooth complex variety X by a proper morphism $f : X \to Y$ between smooth quasi-projective varieties over any field, and

(c) he defined a group $K(X)$ and treated the sheaf as a member of this group.

What then is $K(X)$? It is the free abelian group generated by elements $[A]$, one for each coherent sheaf A, with the relation $[B] - [A] - [C] = 0$ for all exact sequences $0 \to A \to B \to C \to 0$. This seemingly simple definition has led to the development of the major field known as K-theory.

An element $[A]$ of $K(X)$ can be viewed as an abstract "Euler characteristic". Thus, using the higher direct images of the sheaf with respect to the morphism f, the classical Euler characteristic $\sum_k (-1)^k \dim H^k(X, \mathbb{E}) \in \mathbb{Z}$ is replaced by $\sum (-1)^k [R^k f_*(\mathcal{F})]$ in $K(Y)$. The amazing power of treating all sheaves and all morphisms and not just vector bundles on a fixed variety is that, by playing with compositions, products and blow-ups, the result for a general morphism f can be reduced to two cases: the injection of a smooth codimension-one subvariety X into Y and the projection from $\mathbb{P}^n$ to a point. The value of using the K group appears because Hilbert's syzyzgy theorem shows immediately that $K(\mathbb{P}^n)$ is generated by the powers of its basic line bundle $\mathcal{O}(1)$.

Formal Schemes, Nilpotents and the Fundamental Group

The problem of describing the fundamental group of a curve in characteristic p had attracted a lot of attention in the 1950s, and this was the next major problem in algebraic geometry on which Grothendieck made huge progress. To make this progress, he required schemes that went beyond varieties in two essential ways: schemes with nilpotents and schemes of mixed characteristic. This application showed clearly that schemes were the correct setting in which to do algebraic geometry.

In algebraic geometry paths cannot be defined algebraically, so the fundamental group is described in terms of finite coverings. It was known that abelian coverings of degree prime to the characteristic behave in the same way as in characteristic zero, and though not the same as in characteristic zero, coverings of degree p were understood. The nonabelian coverings were a complete mystery. Grothendieck proved a stunning theorem, that the Galois coverings of degree prime to the characteristic are the same as those in characteristic zero and that the fundamental group of a curve in characteristic p is a quotient of the group in characteristic zero. The techniques that he developed for the proof seem amazing still today.

Grothendieck first discovered that if two schemes are given by structure sheaves on the same underlying space, differing only in their nilpotent ideals, they have the same fundamental group. To apply this observation, he considered what Weil had called a "specialization" of a characteristic-zero variety to characteristic p. Suppose, for instance, that a family X of curves is given over the p-adic integers $\mathbb{Z}_p$. Then the fibre X_0 obtained by working modulo p will be a curve over the prime field $\mathbb{F}_p$, and the fibre X_η over the p-adic field $\mathbb{Q}_p$ will have characteristic zero. In this situation one can also consider the scheme X_n obtained by working modulo p^{n+1}, a family of curves over the ring $\mathbb{Z}/p^{n+1}\mathbb{Z}$. The schemes X_n form a sequence $X_0 \subset X_1 \subset \cdots$, and they differ only in their nilpotent elements. So if a covering of X_0 is given, one can extend it to every X_n.

This approach was revolutionary, though nothing technically difficult was needed up to this point. Grothendieck's biggest step was to go from a family of coverings of the sequence $\{X_n\}$ to a covering of the scheme X itself. Once this was done, standard methods related the covering of the curve X_0 in characteristic p to a covering of the characteristic-zero curve X_η.

It was while studying this last step that Grothendieck found a key Existence Theorem. To state that theorem, we begin with a scheme X projective (or proper) over a complete local ring R. It might be a curve over the ring of p-adic integers. Let R_n denote the truncation of R modulo a power of the maximal ideal, and let X_n be the corresponding truncation of X. The schemes X_n form a sequence $X_0 \subset X_1 \subset \cdots$ that Grothendieck calls a *formal scheme*. Given a coherent sheaf M on X, one obtains a sequence of coherent sheaves $\cdots \longrightarrow M_1 \longrightarrow M_0$ on the schemes X_n by truncation: $M_n = M \otimes_R R_n$. The Grothendieck Existence Theorem allows one to go the other way. It states that there is an equivalence of categories between coherent sheaves M on X and sequences of sheaves M_n on X_n such that $M_{n-1} = M_n \otimes_{R_n} R_{n-1}$. Grothendieck then stated the covering problem in terms of coherent sheaves and was able to complete his proof.

Grothendieck's Existence Theorem is a cornerstone of modern algebraic geometry, and the categorical properties that are necessary for a theorem of that type are still not understood.

Functors and the Hilbert, Picard, and Moduli Schemes

Prior to Grothendieck's work, both Weil and Zariski had struggled with deciding what should be called the *points* of a variety when it was defined over a nonalgebraically closed ground field k: should these be the maximal ideals in their affine coordinate rings, or should they be the solutions of the defining equations in the algebraic closure $\bar{k}$? And they needed some concept of *generic points*; they were first defined by van der Waerden in his classical series of papers on algebraic geometry, in the same way as Weil and Zariski. This confusion disappeared when Grothendieck took the radical step of defining two sorts of points on a scheme X: on the one hand, all *prime* ideals in the affine coordinate rings of X became the points of the scheme, but on the other, morphisms from any scheme S to X were called *S-valued points* of X. What was traditionally thought to be the underlying point set is the case that $S = \mathrm{Spec}(\bar{k})$. If $X(S)$ is the set of S-valued points of X and $S \to T$ is a morphism, composition defines a map from $X(T)$ to $X(S)$. Thus $S \mapsto X(S)$ is a *functor* from the category of schemes to the category of sets.

Grothendieck introduced the term *representable functor*, a functor that is isomorphic to $\mathrm{Hom}(\cdot, X)$ for some object X. Moreover, he insisted on the systematic use of fibred products, using them to define the concept of relative representability. A morphism of functors $F \longrightarrow G$ is relatively representable if, given a morphism $\mathrm{Hom}(\cdot, X) \longrightarrow G$, i.e., an element of $G(X)$, the fibred product $X \times_G F$ is representable. For example, F is an open subfunctor of G if for every such morphism, the fibred product is represented by an open subset of X.

There had been substantial work at this time defining varieties parametrizing certain structures; that is, their points were in one-to-one correspondence with the set of all such structures. Chow had defined a union of varieties whose points parametrize subvarieties of projective space of given degree and dimension, Weil had defined varieties whose points parametrize divisor classes of degree zero on a curve, and Baily had defined a variety whose points correspond to isomorphism classes of curves of fixed genus over the complex numbers. Grothendieck immediately realized that in each of these constructions, one should look for a suitable representable functor. Instead of Chow's formulation, he considered *subschemes* of a given projective space $\mathbb{P}^n$ with fixed Hilbert polynomial P, made it into a functor by looking at flat families of subschemes, and proved that this functor was represented by a scheme that he named the Hilbert scheme $\mathrm{Hilb}_P(\mathbb{P}^n)$. He described these ideas in a series of Bourbaki talks in 1959–62 and in a seminar at Harvard in 1961.

Once again, recasting old problems in their natural more abstract settings solved old problems. Going back to the first decades of the twentieth century, a central problem in the theory of algebraic surfaces F over the complex numbers had been showing that the *irregularity* that we now call $\dim H^1(\mathcal{O}_F)$ was the dimension of the Picard variety that classifies topologically trivial divisor classes. This had been proven by complex analytic methods by Poincaré, but despite multiple attempts by Enriques and Severi, had not been proven algebraically. Grothendieck's approach was to define a Picard *scheme* whose S-valued points correspond to the set of line bundles[1] on $F \times S$. Taking $S = \mathrm{Spec}\, k[x]/(x^2)$, he saw that $H^1(\mathcal{O}_F)$ was the tangent space to the Picard scheme at the origin. Thus the old problem became: show that the Picard scheme is reduced, i.e. has no nilpotent elements in its structure sheaf. But the Picard scheme is a group, and in characteristic zero algebraic groups have an exponential map, hence no nilpotents. In characteristic p this need not be true, and life is richer.

[1]*Technical point: the line bundles should be trivialized on* $\{x\} \times S$ *for some rational point* $x \in F$.

In the case of moduli spaces, the functorial approach first solved their local structure using the idea of *pro-representing* a functor, F: fix an element a of $F(\operatorname{Spec}(k))$ and seek a complete local ring whose Spec defines the subfunctor of F of all nilpotent extensions of a. Criteria for prorepresentability were established by Grothendieck, Lichtenbaum, and Schlessinger, and for moduli in particular this led to the concept of the cotangent complex due to Grothendieck and Illusie.

The global theory of the moduli space, however, went in two directions. One sought quasi-projective moduli schemes and was pursued by Mumford. Grothendieck's idea, however, was to find simple general properties of a functor that characterized those that were representable, solving special cases like moduli as a corollary. But Hironaka found a simple 3-dimensional scheme with an involution whose quotient by this involution fails to be a scheme; hence schemes themselves need to be further generalized if there is to be a nice characterization of the functors they represent. This led to the concept of an *algebraic space*, a more general type of object. A remarkable "approximation" theorem discovered by M. Artin in 1969 led to his characterization of the functors represented by these spaces in 1971, fully vindicating Grothendieck's vision.

Étale Cohomology

Interest in a cohomology theory for varieties in characteristic p was stimulated by André Weil's talk in 1954 at the International Congress of Mathematicians (see also his earlier paper "Numbers of Solutions of Equations in Finite Fields" (*AMS Bulletin*, vol. 55 (1949), 497–508)). In this talk, he compared analytic and algebraic methods in algebraic geometry. The problem of defining cohomology algebraically hadn't attracted much interest before, because the classical topology was available for varieties over the complex numbers. But the culmination of Weil's talk was his explanation that, because rational points on a variety V over a finite field were the fixed points of a Frobenius automorphism, one might be able to count them by the Lefschetz Fixed Point Formula, which asserts that the number of fixed points of an automorphism φ is equal to the alternating sum $\sum_i (-1)^i \mathrm{Trace}_{H^i(V)}(\varphi^*)$ of traces of the maps induced by φ on the cohomology. However, a definition of the cohomology groups was required, and the Zariski topology was useless for this. That a definition should exist with the properties Weil predicted became known as the Weil Conjectures.

There was no problem with cohomology in dimension 1, because $H^1(V, \mathbb{Z}/n)$ can be constructed from the group of n-torsion divisor classes. Therefore the cohomology of curves was understood. In fact, Weil's conjectures were based on the known case of curves, for which the zeta function had been analyzed and for which the analogue of the Riemann Hypothesis had been proved by E. Artin, H. Hasse, and Weil himself.

Grothendieck's idea for defining cohomology was to replace open sets of a topology by unramified coverings of Zariski open sets. There were some hints that this might work. Previously, Serre had defined what he called *local isotriviality*. A bundle B over a variety X is locally isotrivial if for every point p of X there is a finite covering U' of a Zariski open neighborhood U of p such that the pullback of B to U' is trivial. Moreover, Kawada and Tate had shown that one could recover the cohomology groups of a curve in terms of the cohomology of its fundamental group.

M. Artin took up this idea in 1961 when Grothendieck visited Harvard. Using unramified coverings that were not finite, i.e. all étale maps, he succeeded in showing that, over the complex numbers, one did indeed obtain the same cohomology with torsion coefficients as with the classical topology. In retrospect, the étale topology was a natural thing to try, since it is stronger than the Zariski topology and weaker than the classical topology. It wasn't at all obvious at the time, because the étale topology isn't a topology in the usual sense. Open sets are replaced by étale maps, which aren't mapped injectively to the base space. The thought that one could do sheaf theory in such a setting was novel. And one needs to work with torsion coefficients to have a reasonable theory. Cohomology with nontorsion coefficients, which is needed for the Fixed Point Theorem, is defined by an inverse limit as ℓ-adic cohomology.

Then Grothendieck proved a series of theorems, notably the Proper Base Change Theorem, which allows one to control the cohomology of varieties by induction on the dimension, using successive fibrations and beginning with the known case of dimension 1. The Proper Base Change Theorem concerns a proper map $X \longrightarrow S$ and a point s of S. The theorem asserts that the cohomology of the fibre X_s over s, $H^q(X_s, A)$, is isomorphic to the limit of the cohomology $H^q(X', A)$ of pullbacks X' of X to the étale neighborhoods S' of s. To prove the theorem, Grothendieck adapted a method that had been introduced by Serre. Artin, Grothendieck, and Verdier developed the full theory jointly at the IHES in 1963–64.

Grothendieck then defined L-series for cohomology of arbitrary constructible sheaves. This allowed him in 1964 to prove rationality of L-series and to find a functional equation, using the Base Change Theorem and Verdier's duality theorem to reduce to the case of dimension 1. The Riemann Hypothesis for varieties over finite fields was proved by Deligne in 1974.

Michael Atiyah

Grothendieck As I Knew Him

My first encounter with the whirlwind that was Grothendieck occurred at the very first, and very small, Bonn Arbeitstagung in July 1957. I have vivid memories of Grothendieck talking for hours every day, expounding his new K-theory generalization of the Hirzebruch-Riemann-Roch Theorem (HRR). According to Don Zagier, Arbeitstagung records show that he spoke for a total of twelve hours spread over four days. It was

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an exhilarating experience: brilliant ideas, delivered with verve and conviction. Fortunately I was young at the time, almost exactly the same age as Grothendieck, and so able to absorb and eventually utilize his great work.

In retrospect we can see that he was the right man at the right time. Serre had laid the new foundations of algebraic geometry, including sheaf cohomology, and Hirzebruch had developed the full cohomological formalism based on the Chern classes, which he and Borel had streamlined. To many it seemed that HRR was the culmination of centuries of algebraic geometry, the pinnacle of the subject. But Grothendieck, standing on the shoulders of Bourbaki, looked to the future, where abstract structural ideas of universality and functoriality would become dominant. His introduction and development of K-theory rested on his mastery of homological algebra and his technical virtuosity, which steamrollered its way through where mere mortals feared to tread. The outcome, the Grothendieck-Riemann-Roch Theorem, was a brilliant functorialization of HRR, which reduced the proof to an exercise left to Borel and Serre!

Grothendieck, standing on the shoulders of Bourbaki, looked to the future…

This great triumph, following his earlier work in functional analysis, established Grothendieck as a mathematician and led to his receiving a Fields Medal (protesting the Soviet regime, he famously did not attend the 1966 Moscow Congress, where the medal was awarded). His new philosophy attracted a host of disciples, who together developed grand new ideas beyond my powers to describe.

For me personally his K-theory, together with more topological ideas germinating in subsequent Arbeitstagungen, led in the end to topological K-theory as developed by Hirzebruch and me, resting on the famous Bott periodicity theorems. Subsequently, through ideas of Quillen and others, algebraic K-theory emerged as a major framework that linked topology, algebraic geometry, and number theory in a deep and beautiful way with great promise and daunting problems for the future. This is part of the Grothendieck legacy.

The first Arbeitstagung also had an educational aspect for me. At the Institute for Advanced Study in Princeton in the fall of 1959, Saturday mornings were devoted to a detailed technical seminar run by Borel, Serre, and Tate, which expounded the algebraic foundations of schemes à la Grothendieck. I was a diligent student and learned enough commutative algebra to deliver a short course of lectures in Oxford, which ended up as my joint textbook with Ian Macdonald. It was not quite a best seller, but read by students worldwide, mainly because of its slim size and affordability. It also gave the mistaken impression that I was an expert on commutative algebra, and I still get emails asking me tricky questions on the subject!

Photo courtesy of the estate of Friedrich Hirzebruch.

Grothendieck (left) and Michael Atiyah, on a boat trip during the Arbeitstagung, around 1961.

I continued to meet Grothendieck frequently in subsequent years in Bonn, Paris, and elsewhere, and we had friendly relations. He liked one of my early papers, which derived Chern classes in a sheaf-theoretic framework, based on what became known as "the Atiyah class". On the other hand, he rather dismissed the Atiyah-Bott fixed point formula, which led to the Hermann Weyl formula for the characters of representations of compact Lie groups, as a routine consequence of his general theories. Technically he was right, but neither he nor anyone else had ever made the connection with the Weyl formula.

These two reactions to my own work are illuminating. He was impressed by my early paper because it was not part of his general theory, but the Atiyah-Bott result, which I consider much more significant, was only part of his big machine and hence not surprising or interesting.

There are two episodes in my memory that deserve to be recorded. The first occurred on one of the famous boat trips on the Rhine, which were central to the Bonn Arbeitstagung. Grothendieck and I were sitting together on a bench on the upper deck, and he had his feet up on the opposite bench. A sailor came up and told him, quite reasonably, to take his feet off the bench. Grothendieck literally dug his heels in and refused. The sailor returned with a senior officer who repeated the request, but Grothendieck again refused. This process then escalated right to the top. The captain came and threatened to return the boat to harbour, and it took all Hirzebruch's diplomatic skills to prevent a major international incident. This story shows how uncompromising Grothendieck could be in his personal life and parallels I think his uncompromising attitude in mathematics. The difference is that in mathematics he was, in the main, successful, but in the real world his uncompromising nature led inevitably to disaster and tragedy.

My second personal recollection is of Grothendieck confiding to me that, when he was forty, he would quit mathematics and become a businessman. He sounded quite serious, though I took it with a grain of salt. In fact he did essentially leave mathematics around that age, and he became an unconventional businessman, operating not in the narrow mercantile world but, as befitted such a visionary man, on the grand scale of world affairs. Unfortunately the talent that had stood him in good stead in the academic world of mathematics was totally inadequate or inappropriate in the broader world. The compromises that make politics the "art of the possible" were anathema to Grothendieck.

He was a tragic figure in the Shakespearian mould, the hero who is undone by his own internal failings. The very characteristics that made Grothendieck a great mathematician, with enormous influence, were also those that unfitted him for the very different role that he chose for himself in later life.

Hyman Bass

Bearing Witness to Grothendieck

Grothendieck had a big, but mostly indirect, impact on my mathematical life. I had only limited personal contact with him, but during the late 1960s I was a fairly close witness to the fundamental transformation of algebraic geometry that he led and inspired. He was a visionary, bigger-than-life figure. Though prodigiously creative, his massive agenda needed distributed effort, and his stable enlisted some of the best young mathematical talent in France—Verdier, Raynaud, Illusie, Demazure,…—with whom I had closer contact. My main intermediary and mentor in that environment was Serre, another universal mathematician but of a totally different style and accessibility. If Serre was a Mozart, Grothendieck was a Wagner. Serre seemed to know the most significant and strategic problems to be addressed across a broad expanse of mathematics, and he had an uncanny sense of exactly where to productively direct the attention of other individual mathematicians, of whatever stature, myself included.

The Grothendieck seminar at IHES, though small in numbers, was intense, almost operatic. On one occasion, Cartier was presenting and struggling with Grothendieck's questioning of the proof of a lemma. At one point, Grothendieck said, "Si tu n'as pas ça, tu n'as rien!" I remember feeling that the events of this period were an important human as well as mathematical story and that it was sad that there was no historian with the technical competence to capture its intellectual and human dimensions in depth.

Grothendieck's influence on my own work began with the exposition, by Borel and Serre, of Grothendieck's proof of his generalized Riemann-Roch Theorem. This seminal paper sowed the birth of both topological (Atiyah and Hirzebruch) and algebraic K-theory. The latter occupied more than two decades of my ensuing work, mostly at the periphery of the Grothendieck revolution.

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Pierre Cartier

Some Youth Recollections about Grothendieck

The scientific birth of Grothendieck occurred in October 1948 at age twenty. After getting his *licence* degree (equivalent to a BS) from the University of Montpellier, he obtained a fellowship for doctoral studies in Paris. This year was the beginning of the famous Cartan Seminar. Grothendieck attended it but was not really attracted. He then moved to Nancy to begin his work on functional analysis, leading to his famous thesis.

My scientific birth occurred in October 1950, when I was accepted as a student at the École Normale Supérieure. I was really eager to learn everything, and there I started a lifelong interest in algebraic topology and homological algebra, joined with a lasting friendship with H. Cartan and S. Eilenberg.

During this time, Grothendieck's fame at Nancy developed rapidly, and even in Paris (!!) we took notice of it. I don't remember exactly when he and I met for the first time, probably around 1953, at the occasion of some Bourbaki Seminar.[2] My first acquaintance with his work came through L. Schwartz. When Schwartz left Nancy for Paris, we had another mathematical father (the first one was H. Cartan). He was very famous for his invention of "distributions" and taught functional analysis to an enthusiastic following (J.-L. Lions, B. Malgrange, A. Martineau, F. Bruhat, me). His first seminar in Paris was devoted to Grothendieck's thesis, and I participated actively, taking a special interest in the "theorem of kernels" and the topological version of Künneth's theorem. Two rather unexpected developments came from Grothendieck's thesis. First, in France, there was a fruitful collaboration between H. Cartan, J.-P. Serre, and L. Schwartz using deep analytical methods to put the finishing touch on the cohomology theory of complex-analytic functions. Then, on the other side, Gelfand, in the then-Soviet Union, used topological tensor products and nuclear spaces for applications to probability theory (Minlos's theorem and random distributions) and mathematical physics (quantum field theory). It would be interesting to trace the transition in Grothendieck's work from functional analysis to algebraic geometry. I plan to develop this some day, but this is not the proper place.

The period in which we were very close is approximately from 1955 to 1961, and there Bourbaki plays a major role. I vividly remember one of our first encounters, which took place at the Institut Henri Poincaré. It was in March 1955 at the Bourbaki seminar after a special lecture that Grothendieck gave about convexity inequalities. He told me: "Very soon, both of us will join Bourbaki." I began

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[2] *At the time, this was the general meeting, three times a year, of all French mathematicians. The French Mathematical Society was then a charming sleeping old lady!*

regularly attending Bourbaki meetings in June 1955. Grothendieck joined soon and participated actively from 1956 to 1960. In June 1955 one of the most interesting pieces to read during our meeting was a first draft of his famous *Tôhoku* paper, where he gives a new birth to homological algebra. One of the major challenges at the time, especially after the appearance of Serre's paper "Faisceaux algébriques cohérents" in 1955, was to devise a theory of sheaf cohomology valid for the most general topological spaces (especially not Hausdorff and not locally compact). What was required was the construction of an injective resolution, but no one knew how to make it for sheaves.[3] In what later became his favorite method, Grothendieck solved the problem from above: looking for the axiomatic properties required of a category to admit injective resolutions, and then checking that the category of sheaves on the most general topological space satisfied these properties.

Let us come back to Bourbaki. There was a turning point, a change of generation. The so-called first part (in six series) was devoted to the foundations, basing everything on set theory and the pervasive notions of isomorphisms and structures. At that time, the publication of this first part was well under way,[4] but what should come after? Among many other projects, it was felt that geometry—both differential geometry, heritage of Elie Cartan,[5] and algebraic geometry, dear to Chevalley, Lang, Samuel, Serre, and Weil—was a cornerstone. We wanted to give a unified presentation of all kinds of manifolds, and there were three competing proposals: ringed spaces (Cartan, Serre), local categories of "charts" (Eilenberg), and a more algebraic version of differential calculus (Weil, Godement, Grothendieck). None was finally accepted, but Grothendieck used them all in his theory of schemes.

Let me add a few personal recollections. All these summer meetings of Bourbaki took place in the Alps, first near Die[6] in Etablissement Thermo-résineux de Salières-les-Bains, a kind of elaborate sauna, then in Pelvoux-le-Poët, in a quiet inn in the mountains. In Die, I remember the late arrival of Grothendieck; having missed an appointment with Serre, who wanted to bring both of them by car, he missed another appointment with us for a night train, then took the wrong train and ended up hitchhiking from Valence to Die! Serre was not especially happy. Another time, he handed me a document to read, where, between the pages, was a letter (in German!) from an unhappy Brazilian girlfriend.

I remember a less exotic event. In the vicinity of Die, deep in the mountains, lived Marcel Légaut, who was an old friend of H. Cartan and A. Weil. Weil's autobiography refers to Légaut as an author of "works of piety," and in the 1970s Grothendieck referred repeatedly to those books. Légaut had left mathematics to raise sheep and became the guru of a kind of phalanstery, long before the wave of hippy communes. With the proper instructions of H. Cartan, Grothendieck and I walked a long way together to visit this guru. On the way, he confessed to me that mathematics was 99 percent labor and 1 percent excitement and that he wanted to leave mathematics to write novels and poetry. Which he did in the end! This was around the time of his mother's death, and it is known that his mother wanted to be a writer.[7] At one of our meetings, he brought his mother, who remained shy.

He was always a rebel.

During a Bourbaki meeting in the summer of 1960, there was a clash between Weil and Grothendieck. It started rather unexpectedly during our reading of a report by Grothendieck about differential calculus. Weil made one of his familiar unpleasant remarks that no one took seriously, except Grothendieck, who immediately left the room and did not come back for a couple of days. Both were uneasy characters, and we didn't understand what was especially at stake. Despite diplomatic efforts of S. Lang and J. Tate, Grothendieck didn't reconsider his self-imposed exile from Bourbaki.

I would need much more space to tell the long tale of the political activities of Grothendieck in the 1970s. He was always a dissident among the dissidents (think of the Vietnam War). Even if your political line was rather close to his own, it was often a painful experience to be on his side, because he wanted to refuse any kind of compromise—and this was the way he always lived his life. He was always a *rebel*.

Pierre Deligne

The first time I attended Grothendieck's seminar, early in 1965, I followed his lecture tenuously. I knew what cohomology groups were but could not understand the expression "*objet de cohomologie*," which kept recurring. After the lecture, I asked him what it meant. Very gently, he explained that if in an abelian category the composite fg is zero, the kernel of f, divided by the image of g, is the cohomology object.

I view his tolerance of what appeared to be crass ignorance and his lack of condescension as typical of him. It encouraged me to not refrain from asking "stupid" questions.

He taught me my trade by asking me to write up, using his notes, the talks XVII and XVIII of SGA4. By "trade" I mean both a feeling for the cohomology of algebraic varieties and how to write. My first draft was returned with comments and injunctions: "never use both sides of a page," "keep ample empty space between lines," as well as

[3] *After the publication of Grothendieck's* Tôhoku *paper, Godement gave an elementary construction of injective sheaves in his well-known textbook.*

[4] *A number of years were still required to finish it, revise it, and produce a so-called "final version".*

[5] *The then-deceased father of Henri Cartan.*

[6] *Site of the family summer house of H. Cartan, where he was the regular organ player in the Huguenot church.*

[7] *She wrote a kind of autobiography entitled* Eine Frau (A Woman), *in German.*

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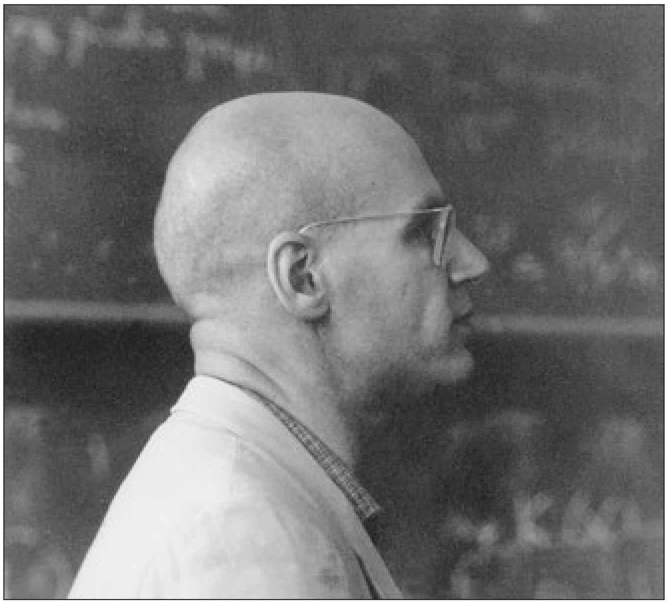

Photo courtesy of the estate of Friedrich Hirzebruch.

Grothendieck in 1965.

“proofs, as well as statements of compatibilities, should be complete.” A key rule was: “One is not allowed to make a false assertion.” Where sign questions in homological algebra are concerned, this rule is very hard to follow.

To use an image from *Récoltes et Semailles*, at that time Grothendieck and those of us around him were building a house. He was the architect-builder. We were helping as we could and bringing a few stones.

I feel extremely fortunate that he was my Master. What I learned from him, especially the philosophy of motives, has been a guiding thread in the works of mine I like the most, such as the formalism of mixed Hodge structures.

From him and his example, I have also learned not to take glory in the difficulty of a proof: difficulty means we have not understood. The ideal is to be able to paint a landscape in which the proof is obvious. I admire how often he succeeded in reaching this ideal.

In *Récoltes et Semailles* Grothendieck criticized me harshly. I always considered this to be a sign of affection. My task was to decide for myself what in these criticisms was true to be able to profit from them.

I am deeply grateful for his helping me to become a mathematician and for sharing his visions.

Michel Demazure

In 1985, I received a heavy parcel. It was Grothendieck’s *Récoltes et Semailles.* On the first page, opposite a photo of the young Shurik, was this dedication, in his well-known and characteristic handwriting: “Pour Michel Demazure—cette réflexion sur un passé et sur un présent, qui nous impliquent l’un et l’autre. Amicalement,—October 1985, Alexandre Grothendieck.” As usual with him, every word was carefully weighed, from the dual meaning of “reflection”, the balanced “past/present”, and the choice of “l’un et l’autre” instead of the obvious “tous les deux” (both).

And the strong “impliquent”:[8] yes, I am “implicated” by a common past, between my twentieth and my thirtieth year. I first met him through his two heavy and hard-to-read monographs, “EVT” (*Espaces vectoriels topologiques*) and “PTT” (*Produits tensoriels topologiques*), and then followed his talks, watching with enthusiasm the infancy of the “new” algebraic geometry (new, and obviously the “right” one, to those of us of the younger generation). I spent the academic year 1959–60 in Princeton at the graduate college, and I remember a seminar at the Institute for Advanced Study where I gave a talk following the manuscript of EGA I. My English was very primitive, and I lost the listeners by pronouncing “jay sub jee” instead of “jee sub jay”. After I returned to France and completed two years of military service, Grothendieck was my thesis adviser (1962–64), and I assisted him in the production of SGA3.

[He] happily climbed levels of abstraction as if he had already been there.

Those who share with me the unique experience of having benefited from his “advice” know how strong and illuminating it was. The weekly half-day sitting at his side and scribbling on parallel or common sheets is something I’ll never forget. I was amazed by the way he discovered (saw!) things as they came along, happily climbing levels of abstraction as if he had already been there. I did not view him as I did other great mathematicians I have met in my career, who I felt were made of the same fabric as I—better fabric, to be sure, as they were brighter, faster, harder workers. Grothendieck always seemed essentially different; he was an “alien”.

After my thesis, in fall 1964, I became a professor at the University of Strasbourg, and with the distance, my relation to Grothendieck weakened. The SGA seminar went its way (actually SGA3 was a parenthesis and did not really belong to the SGA mainstream), and I was geographically unable to follow it. Two years later I joined Université Paris-Sud in Orsay, with new interests and new responsibilities.

I must say I never felt really at ease with his view of mathematics. At the time when I had contact with him, I could not put this uneasiness into words. I understand it better now. There are two components.

Rereading *Récoltes et Semailles* and also his correspondence with Serre, I find the first component of my uneasiness centers on the question: What, after all, is

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[8] *The French verb* impliquer *can be understood in two ways: simply as “imply”, as in “A implies B”, or as “implicate”, as in “A is implicated in the crime against B”.*

mathematics about? Of course, I am really pleased when I see (or in a few cases contribute to) the perfection of a general tool, but the pleasure is much greater when I see what such tools say in specific situations, where there is not enough room for those tools (size, dimension,…) or when they collide. I remember Robert Steinberg saying, "It is a pity there are so few simple Lie groups and that most of them are classical." He would have been happy (and so would I) had the number of exceptional Lie groups been larger! I think this pleasure in exceptions was foreign to Grothendieck.

The second component centers on the question of "computation", which takes a large place in Grothendieck's correspondence with (and controversy about) Ronald Brown. I always liked to compute (I even spent the summer of 1955, before entering École Normale Supérieure, in the first computer company in France). For me, a complete mathematical theory should lead to effective computations. Grothendieck did not like computations (and hated computers!). He wrote to Brown: "The question you raised 'how can such a formulation lead to computations' doesn't bother me in the least!" It is striking to compare this to what Voevodsky, whom I see as Grothendieck's true continuator, wrote thirty years later: "It soon became clear that the only real long-term solution to the problems that I encountered is to start using computers in the verification of mathematical reasoning."

What I have written might give a wrong impression and hide how much I owe to Grothendieck—as well as to Serre and Tits—and how intellectually enriched I have been by him. One cannot get rid of the "Grothendieck way". For years, when I was stuck while struggling with a problem, I used to ask myself, what would Grothendieck say? Most certainly: If you just had stated the problem in the right way, you'd see the answer in the question.

If there is something like a "space of mathematics", I see Grothendieck as an extremal point, and maybe so extremal as to be felt outside. In the "space-time of mathematics" there was a time interval in which I came into contact with that extremal point. That was a crucial period of my life, when I was, to use his wording, "implicated" by him and with him.

Marvin Jay Greenberg

Memories of Alexandre Grothendieck

My 1959 thesis, which proved a conjecture in arithmetic algebraic geometry by Serge Lang, introduced a technique that seemed complicated. When I learned, from notes by Dieudonné, about Grothendieck's new foundation for algebraic geometry based on schemes, I rewrote my thesis for publication using that language. Moreover, that foundation showed that I had discovered a very natural, useful functor (Grothendieck later incorporated that functor as part of his general theory of "descent"). While teaching at Berkeley, I heard that Grothendieck would visit Harvard in fall 1961, so I obtained a fellowship to learn from him there and also at the IHES in Paris in spring 1962.

My first impression on seeing Grothendieck lecture was that he had been transported from an advanced alien civilization in some distant solar system to visit ours in order to speed up our intellectual evolution. His shaven head, his rapid, intense, commanding mode of speaking, plus the new concepts and generality of his view of algebraic geometry conveyed that impression.

I recall a lecture he gave at Harvard about Hilbert schemes, at the end of which he suddenly announced that he could develop a certain topic much more generally. Professor Oscar Zariski, who was in the audience, stopped Grothendieck from speaking overtime, asking him to "please exercise a little self-control."

In Paris I attended Grothendieck's lectures that were later published as SGA. The lectures were overwhelming, and I was also somewhat intimidated by his forceful personality.

I told him about an excellent symphony I had attended that cost me only a few francs. His firm response…"Ah, but it also cost you your time!"

Nevertheless, during an intermission in one of his presentations, I approached him and attempted to informally chat with him. I told him about an excellent symphony concert I had attended the night before that had cost me only a few francs. His firm response was, "Ah, but it also cost you your time!" Grothendieck evidently worked so hard on mathematics that he spent very little time on anything else.

Feeling utterly out of place attempting to relate to such a formidable person, I was subsequently surprised and elated when he invited me to dine with him and his wife at his home. It was a working-class, unpretentious abode. His wife was busy caring for their young baby. Grothendieck wasted very little time making small talk. With paper and pen at hand, he spent nearly the entire time sketching ways to use the functor I had found. I couldn't follow what he was suggesting. He also urged me to work on presenting, within the framework of schemes, A. Neron's important minimal models theory, which had been written in the old language of Weil's foundations. I did begin studying Neron's publications. Three years later I was able to push through a little of what Grothendieck had suggested. With the help of Michael Artin, I took one result in Neron's work, expressed it in the language

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of schemes, and proved a new version of it in much greater generality. Grothendieck arranged to have this work appear in the *Publications IHES*.

I had no further direct interaction with Grothendieck after that publication, but other connections to him did arise. For example, I taught a course at Crown College, UC Santa Cruz, called The Quest for Enlightenment, in part presenting the teachings of J. Krishnamurti. Many years later Grothendieck, in his *Récoltes et Semailles*, listed Krishnamurti as one of eighteen enlightened masters of our age.

Grothendieck's copious output and originality in mathematics demonstrated a level of intellectual achievement I never imagined was possible by one man. I will forever be grateful that he took a little time to kindly inspire me to contribute a bit. There seems to be a consensus that Grothendieck went mad in his later years. I strongly disagree with that consensus. It is the madness of ordinary society that eventually drives geniuses like Grothendieck (and more recently Grigory Perelman) to withdraw.

Robin Hartshorne

Reflections on Grothendieck

After majoring in math at Harvard, I spent a year at the École Normale Supérieure in Paris. I had courses with Cartan, Serre, and Chevalley and learned some general topology and sheaf theory. After becoming a graduate student at Princeton, I started reading Serre's article "Faisceaux algébriques cohérents" and thought I would like to study algebraic geometry. At that time there was no algebraic geometry at Princeton, so the fall of 1961 found me back at Harvard, and there was Grothendieck.

He gave a lecture course on local properties of morphisms, which later became part of EGA IV, and he gave two seminars, one on local cohomology and one on construction techniques—the Hilbert scheme, the Picard scheme, and so forth. I could see that his was "the right way" to do things and jumped headlong into his world. In 1963 I finished my thesis, which was on the connectedness of the Hilbert scheme. While Grothendieck was not my official advisor, nor did I discuss the work in progress with him, I am sure it was the stimulating atmosphere of discovery he created that provided the context for me to be able to do this work.

I sent a draft copy of my thesis to Grothendieck. He responded with a long letter, containing a few sentences of appreciation for the result and then many pages of further questions about the Hilbert scheme, most of which are still unanswered today. Each new result he encountered gave rise to a myriad of further questions to investigate.

A couple of years later I offered to run a seminar at Harvard on his theory of duality, which he had hinted at in his ICM talk in 1958 but had not yet developed. He agreed and sent me about 250 pages of "prenotes" for the seminar. My job was to digest them, fill in details, give the seminar talks, and then write up the notes. This was quite a challenge, as it included the first occurrence of Verdier's theory of the derived category and Grothendieck's use of it in developing the duality theory for a morphism of schemes. I regard this period as my "apprenticeship" with Grothendieck. We had a constant interchange of letters, as I sent him drafts of the seminar talks, and he returned them covered with red ink. In this way I learned the craft of exposition in his style. After the lecture notes were published (*Residues and Duality*, Springer Lecture Notes 20, 1966), I did not see him so often. But some time later he did ask me, "Well, those lecture notes were a good rough account, but when are you going to write the *book* on duality?" I did not answer that because I was already moving in other directions.

The last time I saw Grothendieck in person was in Kingston, Ontario, in 1971. He had so withdrawn from engagement in mathematics that he devoted equal time in his talks to his new brainchild "Survivre". I could appreciate the sincerity of his beliefs but felt he was hopelessly naïve about political action.

When I finished my book *Algebraic Geometry* in 1977, which is basically an introduction to Grothendieck's way of thinking using schemes and cohomology, I sent him a copy together with a note of thanks and appreciation for all that I had learned from him. He sent a polite card in reply, saying, "It looks like a nice book. Perhaps if one day I again teach a course on algebraic geometry, I will look at the inside."

Near the beginning of his rambling reflections *Récoltes et Semailles*, Grothendieck mentions "les héritiers et le bâtisseur," the heirs and the constructor. As an heir of the master builder Grothendieck, I am now happily inhabiting several of the rooms he built and using his tools to refine my understanding of classical geometry. I owe him the inspiration for my life work.

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Luc Illusie

Alexander Grothendieck was a professor at the IHÉS from 1959 until 1970. In the seminars he led—the famous SGA—a team of students coalesced around him, exploring the new territories that "the Master" had discovered. We were many, coming from various corners of the world, to participate in this adventure, which constituted a sort of golden age of algebraic geometry.

The seminars took place at the IHÉS on Tuesday afternoons and spread out over a year or two. They were held in a former music pavilion that had been

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This is a slightly edited translation of a piece, "Grothendieck était d'un dynamisme impressionnant", which appeared in CNRS, Le journal (`https://lejournal.cnrs.fr/billets/grothendieck-etait-dun-dynamisme-impressionnant`)*, and is an excerpt of a longer article "Alexandre Grothendieck, magicien des foncteurs", which appeared in the online publication of the Mathematical Institute of the CNRS* (`www.cnrs.fr/insmi/spip.php?article1093`)*. The* Notices *thanks the editors of both publications for permission to include this piece here.*

transformed into a library and lecture hall, with large picture windows onto the Bois-Marie park. Occasionally before the lectures, the Master took us for a walk in the woods to tell us about his latest ideas. The seminars were mainly about his own work. There were also related results, of which he sometimes entrusted the exposition to students or colleagues. For instance, he asked Deligne, in the seminar SGA7, to transpose into the setting of étale cohomology the classical Picard-Lefschetz formula, whose proof he confessed to me he had not understood. This étale analogue was later to play a key role in the proof, by Deligne, of the Riemann hypothesis over finite fields. At the blackboard Grothendieck had an impressive dynamism but was always clear and methodical. No black boxes, no sketches—everything was explained in detail. Occasionally he omitted a verification that he considered purely routine (but that could turn out to be more delicate than it had appeared). After the lecture, the audience went to have tea in the administrative building. This was an opportunity to discuss various points from the seminar and exchange ideas.

[He] was always clear and methodical. No black boxes, no sketches.

Grothendieck liked to ask his students to write up his lectures. In this way, they learned their craft. When it came to editing, he was tough and demanding. My typed manuscripts, which might reach fifty pages, would be blackened all over with his critiques and suggestions. I remember reviewing them one by one over the course of long afternoons at his home. The results had to be presented in their natural framework, which usually meant the most general possible. Everything had to be proved. Phrases like “it is clear” and “one easily sees” were banished. We discussed the mathematics point by point, but also punctuation and the order of words in a sentence. Length was not an issue. If a digression looked interesting, it was welcome. Very often we were not finished before 8 o’clock in the evening. He would then invite me to share a simple dinner with his wife, Mireille, and their children. After the meal, as a form of recreation, he would explain to me bits of mathematics he had been thinking about lately. He would improvise on a white sheet of paper, with his large pen, in his fine and rapid hand, stopping occasionally at a certain symbol to once again run his pen over it in delight. I can hear his sweet and melodious voice, punctuated from time to time with a sudden “Ah!” when an objection came to mind. Then he would see me off at the station, where I would take the last train back to Paris.

Nicholas Katz

There is no need, I hope, to discuss the mathematical achievements and the mathematical vision of Grothendieck. What is perhaps less known to people who did not interact with Grothendieck personally was his incredible charisma. We thought of him (as he did of himself, as he says in *Récoltes et Semailles*) as the boss (*patron*) of a construction site (*chantier*). When he asked someone to carry out some work that would be part of this, the person asked felt that he or she had been honored to have been asked, was proud to have been asked, and was delighted to undertake the task at hand (which might take many years to complete). Combined with this charisma, Grothendieck had an uncanny sense of whom to ask to do what. One sees this in looking at the long list of people whose work became an essential part of Grothendieck’s *chantier*.

Steven L. Kleiman

The first time I saw Grothendieck was in September 1961 at Harvard. I was an eager new graduate student; he, a second-time visitor teaching a course. He started by explaining he’d cover some preliminaries to appear in [4]: the course would be elementary; the prerequisites, just the basics.

Soon I found my three terms of graduate algebra as an MIT undergraduate hadn’t prepared me for Grothendieck’s course. So I dropped it and skipped his two weekly seminars developing the Picard scheme and local cohomology. I believe he assigned no homework and gave no exams in the course; at the end he unexpectedly collected the notebooks of the registered students and assigned grades.

Grothendieck began each meeting of the course by erasing the board and then writing $X \xrightarrow{f} Y$ vertically. One time before Grothendieck arrived, John Fogarty, another graduate student, erased the board and wrote $X \xrightarrow{f} Y$ vertically. When Grothendieck arrived, he looked at the board and silently erased it. Then he began his lecture, writing $X \xrightarrow{f} Y$ vertically.

Fogarty had considerable skill as a caricature artist. One day he drew a large, lovingly detailed cartoon on the blackboard in the common room. It showed a side view of Grothendieck with a quiver of arrows on his back, looking ahead where he’d written $X \xrightarrow{f} Y$ vertically.

Thus Fogarty satirized one of Grothendieck’s signature insights: it pays off in better understanding and in greater flexibility to generalize *absolute* properties of objects X to *relative* properties of maps $X \xrightarrow{f} Y$.

Grothendieck’s paper [2] was highly regarded in the student Algebraic Geometry Seminar, which I joined in fall 1962. Grothendieck had upgraded sheaf cohomology: he found enough injectives to resolve sheaves and yield

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their higher cohomology groups as derived functors. Thus he demoted Čech cohomology: taken as the definition in Jean-Pierre Serre's [9], it became just a computational device.

Later Grothendieck went further. He generalized the very notion of topology! In an open covering of U by U_i for $i \in I$, the maps $U_i \to U$ needn't be inclusions, just members of a suitable class. For example, they could be *étale,* his generalization of the local isomorphisms of analytic spaces.

Michael Artin began the systematic development of Grothendieck topology in a Harvard seminar in spring 1962. I didn't attend but did lecture from [1] in a student seminar at Woods Hole, July 1964. The ensuing work of Grothendieck and collaborators (especially Artin, Jean-Louis Verdier, and Pierre Deligne) culminated in the resolution of André Weil's celebrated conjectures, just as Grothendieck [3, p. 104] had predicted.

I learned more of Grothendieck's innovations in David Mumford's course, spring 1964, published as [7]. It was devoted to Grothendieck's proof of completeness of the characteristic system of a good complete algebraic system of curves on a smooth projective complex surface F. It is the *first algebraic proof* of a theorem with a long, rich, and colorful history (see [6]).

"The key," Mumford wrote on p. viii, "… is the systematic use of nilpotent elements" to handle higher-order infinitesimal deformations. That's another of Grothendieck's signature insights. Yet another is to use flatness to formalize the notion of algebraic system. Moreover, Grothendieck proved a complete algebraic system is parameterized by a component H of his Hilbert scheme of F; namely, H classifies all systems via maps into H.

Grothendieck showed the Theorem of Completeness simply provides conditions for H to be smooth at a given point. To prove the conditions work, he used his Picard scheme P and the map $H \to P$. In an ingenious sense, P *classifies families* of line bundles: its *functor of points,* that is, functor of maps $T \to P$, isn't equal to the naive functor of line bundles on $T \times F$, but rather to its associated sheaf in the étale Grothendieck topology.

Mumford sent a preliminary copy of [7] to Grothendieck, who commented in a letter [8, pp. 693–6] dated August 31, 1964. Mumford's numerical characterization of *good* systems reminded Grothendieck about his conjectural *numerical theory of ampleness.* In particular, on an n-fold, just as on a surface, a divisor should be pseudoample if it meets every curve nonnegatively. More generally, the ample divisors should form the interior of the polar cone of the numerical cone of curves. In [8, p. 701], Mumford replied he didn't know if the conjectures are true, even on a 3-fold, but he'd ask me.

Shortly afterwards, I proved Grothendieck's first conjecture; in January, the second. Then I used the second to prove Chevalley's conjecture: a complete smooth variety is projective if any finitely many points lie in some affine subset. In April, Mumford suggested I write to Grothendieck. Grothendieck replied with comments and

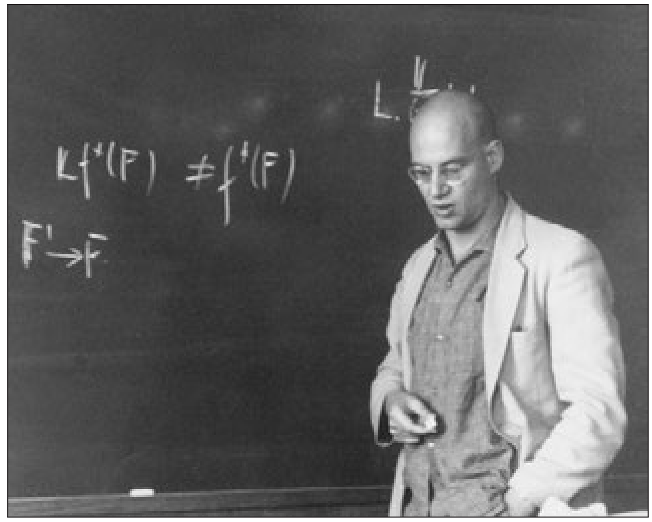

Photo courtesy of the estate of Friedrich Hirzebruch.

Grothendieck around 1965.

said, "I would appreciate knowing a simple proof," of the key ingredient, the Nakai–Moishezon criterion.

I sent Grothendieck a reprint of my first paper [5], where I had simplified Nakai's proof and extended it to a *nonprojective* n-fold as announced by Moishezon. He replied with more comments, and in a PS he gave his opinion on the history of the development of the criterion. The body was typewritten, but the PS, handwritten, showing he'd thought more about it and really wanted everyone to receive proper credit, not because it's due, but to indicate how ideas develop.

Grothendieck's letters show impressive clarity and thoroughness. They pose questions, indicating a wish to continue the discussion. They suggest being generous with ideas while acknowledging their provenance. His letters are complimentary and encouraging. This is the way to do collaborative mathematics!

Grothendieck agreed to supervise my NATO postdoc 1966–67, and I returned to his institute, the IHES, the summers of 1968 and 1969 and the spring of 1970. In [6], I discussed my mathematical experiences.

Socially, Grothendieck had me and others over to his house for dinner several times. The last time, in spring 1970, I brought my new wife. Beverly remembers "a feeling of trepidation, as he was a living legend. However, the minute we entered his home, it was apparent that he was an exceptional person, gracious and attentive. Not for an instant did I feel my deficiency in mathematics and French was something that even occurred to him. His genuine interest and participation in conversation, the general atmosphere of inclusion, is something I've always remembered."

Spring 1970 was hard on Grothendieck, as his era at the IHES ended. Outwardly, he didn't show his feelings, but people did talk about what was happening. I never saw him again and heard from him only once more when he sent me his four volumes of *Récoltes et Semailles,* with this inscription opposite a picture of himself as an adorable six(?)-year-old: "To Steven Kleiman, with my friendly regards, Oct. 1985, Alexander Grothendieck."

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How Grothendieck Simplified Algebraic Geometry

Photo courtesy of the IHES.

Colin McLarty

> The idea of *scheme* is childishly simple—so simple, so humble, no one before me dreamt of stooping so low.…It grew by itself from the sole demands of simplicity and internal coherence.
>
> [A. Grothendieck, *Récoltes et Semailles* (R&S), pp. P32, P28]

Algebraic geometry has never been really simple. It was not simple before or after David Hilbert recast it in his algebra, nor when André Weil brought it into number theory. Grothendieck made key ideas simpler. His schemes give a bare minimal definition of space just glimpsed as early as Emmy Noether. His derived functor cohomology pares insights going back to Bernhard Riemann down to an agile form suited to étale cohomology. To be clear, étale cohomology was no simplification of anything. It was a radically new idea, made feasible by these simplifications.

Grothendieck got this heritage at one remove from the original sources, largely from Jean-Pierre Serre in shared pursuit of the Weil conjectures. Both Weil and Serre drew deeply and directly on the entire heritage. The original ideas lie that close to Grothendieck's swift reformulations.

Generality As the Superficial Aspect

Grothendieck's famous penchant for generality is not enough to explain his results or his influence. Raoul Bott put it better fifty-four years ago describing the Grothendieck-Riemann-Roch theorem.

Riemann-Roch has been a mainstay of analysis for one hundred fifty years, showing how the topology of a Riemann surface affects analysis on it. Mathematicians from Richard Dedekind to Weil generalized it to curves over any field in place of the complex numbers. This makes theorems of arithmetic follow from topological and analytic reasoning over the field $\mathbb{F}_p$ of integers modulo a prime p. Friedrich Hirzebruch generalized the complex version to work in all dimensions.

Grothendieck proved it for all dimensions over all fields, which was already a feat, and he went further in a signature way. Beyond single varieties he proved it for suitably continuous families of varieties. Thus:

> Grothendieck has generalized the theorem to the point where not only is it more generally applicable than Hirzebruch's version but it depends on a simpler and more natural proof. (Bott [6])

This was the first concrete triumph for his new cohomology and nascent scheme theory. Recognizing that many mathematicians distrust generality, he later wrote:

> I prefer to accent "unity" rather than "generality." But for me these are two aspects of one quest. Unity represents the profound aspect, and generality the superficial. [16, p. PU 25]

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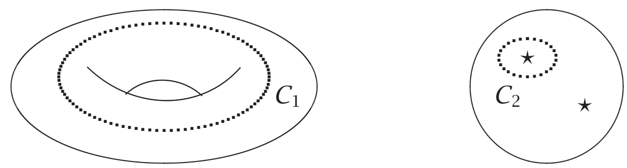
The Beginnings of Cohomology

> Surfaces with holes are not just an amusing pastime but of quite fundamental importance for the theory of equations. (Atiyah [4, p. 293])

The Cauchy Integral Theorem says integrating a holomorphic form ω over the complete boundary of any region of a Riemann surface gives 0. To see its significance look at two closed curves C on Riemann surfaces that are *not* complete boundaries. Each surrounds a hole, and each has $\int_C \omega \neq 0$ for some holomorphic ω.

Cutting the torus along the dotted curve C_1 around the center hole of the torus gives a tube, and the single curve C_1 can only bound one end.



The *punctured sphere* on the right has stars depicting punctures, i.e. holes. The regions on either side of C_2 are unbounded at the punctures.

Riemann used this to calculate integrals. Any curves C and C' surrounding just the same holes the same number of times have $\int_C \omega = \int_{C'} \omega$ for all holomorphic ω. That is because C and the reversal of C' form the complete boundary of a kind of collar avoiding those holes. So $\int_C \omega - \int_{C'} \omega = 0$.

Modern cohomology sees holes as *obstructions* to solving equations. Given ω and a path $P : [0, 1] \to S$ it would be great to calculate the integral $\int_P \omega$ by finding a function f with $\mathrm{d}f = \omega$, so $\int_P \omega = f(P_1) - f(P_0)$. Clearly, there is not always such a function, since that would imply $\int_P \omega = 0$ for every closed curve P. But Cauchy, Riemann, and others saw that if $U \subset S$ surrounds no holes there are functions f_U with $\mathrm{d}f_U = \omega$ all over U. Holes are obstructions to patching local solutions f_U into one solution of $\mathrm{d}f = \omega$ all over S.

This concept has been generalized to algebra and number theory:

> Indeed one now instinctively assumes that all obstructions are best described in terms of cohomology groups. [32, p. 103]

Cohomology Groups

> With homologies, terms compose according to the rules of ordinary addition. [24, pp. 449-50]

Poincaré defined addition for curves so that $C + C'$ is the union of C and C', while $-C$ corresponds to reversing the direction of C. Thus for every form ω:

$$\int_{C+C'} \omega = \int_C \omega + \int_{C'} \omega \qquad \text{and} \qquad \int_{-C} \omega = -\int_C \omega.$$

When curves $C_1, \ldots, C_k$ form the complete boundary of some region, then Poincaré writes $\Sigma_k C_k \sim 0$ and says their sum is *homologous to* 0. In these terms the Cauchy

Integral Theorem says concisely:

$$\text{If } \Sigma_k C_k \sim 0, \text{ then } \int_{\Sigma_k C_k} \omega = 0 \quad \text{for all holomorphic forms } \omega.$$

Poincaré generalized this idea of homology to higher dimensions as the basis of his *analysis situs*, today called topology of manifolds.

Notably, Poincaré published two proofs of *Poincaré duality* using different definitions. His first statement of it was false. His proof mixed wild non sequiturs and astonishing insights. For topological manifolds M of any dimension n and any $0 \le i \le n$, there is a tight relation between the i-dimensional submanifolds of M and the $(n-i)$-dimensional. This relation is hardly expressible without using homology, and Poincaré had to revise his first definition to get it right. Even the second version relied on overly optimistic assumptions about triangulated manifolds.

Topologists spent decades clarifying his definitions and theorems and getting new results in the process. They defined homology groups $\mathrm{H}_i(S)$ for every space S and every dimension $i \in \mathbb{N}$. In each H_1 the group addition is Poincaré's addition of curves modulo the homology relations $\Sigma_k C_k = 0$. They also defined related cohomology groups $\mathrm{H}^i(S)$ such that Poincaré duality says $\mathrm{H}_i(M)$ is isomorphic to $\mathrm{H}^{n-i}(M)$ for every compact orientable n-dimensional manifold M.

Poincaré's two definitions of homology split into many using simplices or open covers or differential forms or metrics, bringing us to the year 1939:

> Algebraic topology is growing and solving problems, but nontopologists are very skeptical. At Harvard, Tucker or perhaps Steenrod gave an expert lecture on cell complexes and their homology, after which one distinguished member of the audience was heard to remark that this subject had reached such algebraic complication that it was not likely to go any further. (MacLane [21, p. 133]

Variable Coefficients and Exact Sequences

> In his Kansas article (1955) and *Tôhoku* article (1957) Grothendieck showed that given any category of sheaves a notion of cohomology groups results. (Deligne [10, p. 16])

Algebraic complication went much further. Methods in topology converged with methods in Galois theory and led to defining cohomology for groups as well as for topological spaces. In the process, what had been a technicality to Poincaré became central to cohomology, namely, the choice of coefficients. Certainly he and others used integers, rational numbers or reals or integers modulo 2 as coefficients:

$$a_1 C_1 + \cdots + a_m C_m \quad a_i \in \mathbb{Z} \text{ or } \mathbb{Q} \text{ or } \mathbb{R} \text{ or } \mathbb{Z}/2\mathbb{Z}.$$

But only these few closely related kinds of coefficients were used, chosen for convenience for a given calculation. So topologists wrote $\mathrm{H}^i(S)$ for the ith cohomology group of space S and left the coefficient group implicit in the context.

In contrast group theorists wrote $\mathrm{H}^i(G, A)$ for the ith cohomology of G with coefficients in A, because many kinds of coefficients were used and they were as interesting as the group G. For example, the famed Hilbert Theorem 90 became

$$\mathrm{H}^1(\mathrm{Gal}(L/k), L^\times) \cong \{0\}.$$

The Galois group $\mathrm{Gal}(L/k)$ of a Galois field extension L/k has trivial 1-dimensional cohomology with coefficients in the multiplicative group $L^\times$ of all nonzero $x \in L$. Olga Taussky[33, p. 807] illustrates Theorem 90 by using it on the Gaussian numbers $\mathbb{Q}[i]$ to show every Pythagorean triple of integers has the form

$$m^2 - n^2, \ 2mn, \ m^2 + n^2.$$

Trivial cohomology means there is no obstruction to solving certain problems, so Theorem 90 shows that some problems on the field L have solutions. Algebraic relations of $\mathrm{H}^1(\mathrm{Gal}(L/k), L^\times)$ to other cohomology groups imply solutions to other problems. Of course Theorem 90 was invented to solve lots of problems decades before group cohomology appeared. Cohomology organized and extended these uses so well that Emil Artin and John Tate made it basic to class field theory.

Also in the 1940s topologists adopted *sheaves* of coefficients. A sheaf of Abelian groups $\mathcal{F}$ on a space S assigns Abelian groups $\mathcal{F}(U)$ to open subsets $U \subseteq S$ and homomorphisms $\mathcal{F}(U) \to \mathcal{F}(V)$ to subset inclusions $V \subseteq U$. So the sheaf of holomorphic functions $\mathcal{O}_M$ assigns the additive group $\mathcal{O}_M(U)$ of holomorphic functions on U to each open subset $U \subseteq M$ of a complex manifold. Cohomology groups like $\mathrm{H}^i(M, \mathcal{O}_M)$ began to organize complex analysis.

Leaders in these fields saw cohomology as a unified idea, but the technical definitions varied widely. In the Séminaire Henri Cartan speakers Cartan, Eilenberg, and Serre organized it all around *resolutions*. A resolution of an Abelian group A (or module or sheaf) is an *exact sequence* of homomorphisms, meaning the image of each homomorphism is the kernel of the next:

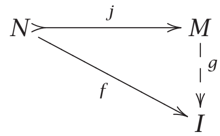
$$\{0\} \longrightarrow A \longrightarrow I_0 \longrightarrow I_1 \longrightarrow \dots .$$

It quickly follows that many sequences of cohomology groups are also exact. That proof rests on the *Snake Lemma* immortalized by Hollywood in a scene widely available online: "A clear proof is given by Jill Clayburgh at the beginning of the movie *It's My Turn*" [35, p. 11].

Fitting a cohomology group $\mathrm{H}^i(X, \mathcal{F})$ into the right exact sequence might show $\mathrm{H}^i(X, \mathcal{F}) \cong \{0\}$, so the obstructions measured by $\mathrm{H}^i(X, \mathcal{F})$ do not exist. Or it may prove some isomorphism, $\mathrm{H}^i(X, \mathcal{F}) \cong \mathrm{H}^k(Y, \mathcal{G})$. Then the obstructions measured by $\mathrm{H}^i(X, \mathcal{F})$ correspond exactly to those measured by $\mathrm{H}^k(Y, \mathcal{G})$.

Group cohomology uses resolution by *injective* modules I_i. A module I over a ring R is injective if for every R-module inclusion $j \colon N \rightarrowtail M$ and homomorphism

$f: N \to I$ there is some $g: M \to I$ with $f = gj$. The diagram is simple:



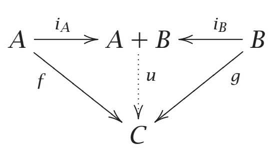
This works because every R-module A over any ring R embeds in some injective R-module. No one believed anything this simple would work for sheaves. Sheaf cohomology was defined only for sufficiently regular spaces using various, more complicated topological substitutes for injectives. Grothendieck found an unprecedented proof that sheaves on all topological spaces have injective embeddings. The same proof later worked for sheaves on any *Grothendieck topology*.

Tôhoku

> Consider the set of all sheaves on a given topological space or, if you like, the prodigious arsenal of all "meter sticks" that measure the space. We consider this "set" or "arsenal" as equipped with its most evident structure, the way it appears so to speak "right in front of your nose"; that is what we call the structure of a "category." [16, p. P38]

We will not fully define sheaves, let alone *spectral sequences* and other "drawings (called "diagrams") full of arrows covering the blackboard" which "totally escaped" Grothendieck at the time of the Séminaire Cartan [R&S, p. 19]. We will see why Grothendieck wrote to Serre on February 18, 1955: "I am rid of my horror of spectral sequences" [7, p. 7].

The Séminaire Cartan emphasized how few specifics about groups or modules go into the basic theorems. Those theorems only use diagrams of homomorphisms. For example, the sum $A + B$ of Abelian groups A, B can be defined, uniquely up to isomorphism, by the facts that it has homomorphisms $i_A: A \to A + B$ and $i_B: B \to A + B$ and any two homomorphisms $f: A \to C$ and $g: B \to C$ give a unique $u: A + B \to C$ with $f = ui_A$ and $g = ui_B$:



The same diagram defines sums of modules or of sheaves of Abelian groups.

Grothendieck [13, p. 127] took the basic patterns used by the Séminaire Cartan as his *Abelian category* axioms. He added a further axiom, AB5, on infinite colimits. Theorem 2.2.2 says if an Abelian category satisfies AB5 plus a set-theoretic axiom, then every object in that category embeds in an injective object. These axioms taken from module categories obviously hold as well for sheaves of Abelian groups on any topological space, so the conclusion applies.

People who thought this was just a technical result on sheaves found the tools disproportionate to the product. They were wrong on both counts. These axioms also simplified proofs of already-known theorems. Most especially they subsumed many useful spectral sequences (*not* all) under the *Grothendieck spectral sequence* so simple as to be Exercise A.3.50 of (Serre [11, p. 683]).

Early editions of Serge Lang's *Algebra* gave the Abelian category axioms with a famous exercise: "Take any book on homological algebra, and prove all the theorems without looking at the proofs given in that book" [20, p. 105]. He dropped that when homological algebra books all began using axiomatic proofs themselves, even if their theorems are stated only for modules. David Eisenbud, for example, says his proofs for modules "generalize with just a little effort to [any] nice Abelian category" [11, p. 620].

Injective resolutions in any Abelian category give *derived functor cohomology* of that category. This was obviously general beyond any proportion to the then-known cases. Grothendieck was sure it was the right generality: For a cohomological solution to any problem, notably the Weil conjectures, find the right Abelian category.

The Weil Conjectures

> This truly revolutionary idea thrilled the mathematicians of the time, as I can testify at first hand. [30, p. 525]

The Weil Conjectures relating arithmetic to topology were immediately recognized as a huge achievement. Weil knew that just conceiving them was a great moment in his career. The cases he proved were impressive. The conjectures were too beautiful not to be true and yet nearly impossible to state fully.

The conjectures were too beautiful not to be true and yet nearly impossible to state fully.

Weil [37] presents the topology using the nineteenth-century terminology of Betti numbers. But he was an established expert on cohomology and in conversations:

> At that time, Weil was explaining things in terms of cohomology and Lefschetz's fixed point formula [yet he] did not want to predict [this could actually work]. Indeed, in 1949–50, nobody thought that it could be possible. (Serre quoted in [22, p. 305].)

Lefschetz used cohomology, relying on the continuity of manifolds, to count fixed points $x = f(x)$ of continuous functions $f: M \to M$ on manifolds. Weil's conjectures deal with spaces defined over finite fields. No known version of those was continuous. Neither Weil nor anyone knew what might work. Grothendieck says:

> Serre explained the Weil conjectures to me in cohomological terms around 1955 and only in these terms could they possibly "hook" me. No one had any idea how to define such a cohomology

> and I am not sure anyone but Serre and I, not even Weil if that is possible, was deeply convinced such a thing must exist. [R&S, p. 840]

Exactly What Scheme Theory Simplified

> Kronecker was, in fact, attempting to describe and to initiate a new branch of mathematics, which would contain both number-theory and algebraic geometry as special cases. (Weil [38, p. 90])

Riemann's treatment of complex curves left much to geometric intuition. So Dedekind and Weber [8, p. 181] proved a Riemann-Roch theorem from "a simple yet rigorous and fully general viewpoint," over any algebraically closed field k containing the rational numbers. They note k can be the field of algebraic numbers. They saw this bears on arithmetic as well as on analysis and saw all too well that their result is "very difficult in exposition and expression" [8, p. 235].

Meromorphic functions on any compact Riemann surface S form a field $M(S)$ of transcendence degree 1 over the complex numbers $\mathbb{C}$. Each point $p \in S$ determines a function e_p from $M(S)$ to $\mathbb{C} + \{\infty\}$: namely $e_p(f) = f(p)$ when f is defined at p, and $e_p(f) = \infty$ when f has a pole at p. Then, if we ignore sums $\infty + \infty$:

$$e_p(f+g) = e_p(f) + e_p(g),$$
$$e_p(f \cdot g) = e_p(f) \cdot e_p(g),$$
$$e_p(\frac{1}{f}) = \frac{1}{e_p(f)}.$$

Dedekind and Weber define a general *field of algebraic functions* as any transcendence degree 1 extension L/k of any algebraically closed field k. They define a *point* p of L to be any function e_p from L to $k + \{\infty\}$ satisfying those equations. Their Riemann-Roch theorem treats L as if it were $M(S)$ for some Riemann surface.

Kronecker [19] achieved some "algebraic geometry over an absolutely algebraic ground-field" [38, p. 92]. These fields are the finite extensions of $\mathbb{Q}$ or of finite fields $\mathbb{F}_p$. They are not algebraically closed. He aimed at "algebraic geometry over the integers" where one variety could be defined over all these fields at once, but this was far too difficult at the time [38, p. 95].

Italian algebraic geometers relied on an idea of *generic points* of a complex variety V, which are ordinary complex points $p \in V$ with no apparent special properties [26]. For example, they are not points of singularity. Noether and Bartel van der Waerden gave abstract generic points which actually have only those properties common to all points of V. Van der Waerden [34] made these rigorous but not so usable as Weil would want. Oscar Zariski, trained in Italy, worked with Noether in Princeton, and later with Weil, to give algebraic geometry a rigorous algebraic basis [23, p. 56].

Weil's bravura *Foundations of Algebraic Geometry* [36] combined all these methods into the most complicated foundation for algebraic geometry ever. To handle varieties of all dimensions over arbitrary fields k, he uses algebraically closed field extensions L/k of infinite transcendence degree. He defines not only points but also subvarieties $V' \subseteq V$ of a variety V purely in terms of fields of rational functions. Raynaud [25] gives an excellent overview. We list three key topics.

(1) Weil has generic points. Indeed, a variety defined by polynomials over a field k has infinitely many generic points with coordinates transcendental over k, all conjugate to each other by Galois actions over k.
(2) Weil defines *abstract algebraic varieties* by data telling how to patch together varieties defined by equations. But these do not exist as single spaces. They only exist as sets of concrete varieties plus patching data.
(3) Weil could not define a variety over the integers, though he could systematically relate varieties over $\mathbb{Q}$ to others over the fields $\mathbb{F}_p$.

Serre Varieties and Coherent Sheaves

Then Serre [27] temporarily put generic points and non-closed fields aside to describe the first really penetrating cohomology of algebraic varieties:

> This rests on the use of the famous Zariski topology, in which the closed sets are the algebraic sub-varieties. The remarkable fact that this coarse topology could actually be put to genuine mathematical use was first demonstrated by Serre and it has produced a revolution in language and techniques. (Atiyah [3, p. 66])

Say a *naive variety* over any field k is a subset $V \subseteq k^n$ defined by finitely many polynomials $p_i(x_1, \dots, x_n)$ over k:

$$V = \{\ \vec{x} \in k^n \mid p_1(\vec{x}) = \cdots = p_h(\vec{x}) = 0\ \}.$$

They form the closed sets of a topology on k^n called the Zariski topology. Even their infinite intersections are defined by finitely many polynomials, since the polynomial ring $k[x_1, \dots, x_n]$ is Noetherian. Also, each inherits a Zariski topology where the closed sets are the subsets $V' \subseteq V$ defined by further equations.

These are very coarse topologies. The Zariski closed subsets of any field k are the zero-sets of polynomials over k: that is, the finite subsets and all of k.

Each naive variety has a *structure sheaf* $\mathcal{O}_V$ which assigns to every Zariski open $U \subseteq V$ the ring of regular functions on U. Omitting important details:

$$\mathcal{O}_V(U) = \{\ \frac{f(\vec{x})}{g(\vec{x})} \text{ such that when } \vec{x} \in U \text{ then } g(\vec{x}) \neq 0\ \}.$$

A *Serre variety* is a topological space T plus a sheaf $\mathcal{O}_T$ which is locally isomorphic to the structure sheaf of a naive variety. Compare the sheaf of holomorphic functions $\mathcal{O}_M$ of a complex manifold. The sheaf apparatus lets Serre actually paste varieties together on compatible patches, just as patches of differentiable manifolds are pasted together. Weil could not do this with his abstract varieties.

Certain sheaves related to the structure sheaves $\mathcal{O}_T$ are called *coherent*. Serre makes them the coefficient sheaves of a cohomology theory widely used today with schemes. The close tie of coherent sheaves to structure sheaves makes this cohomology unsuitable for the Weil conjectures. When variety (or scheme) V is defined over a finite field $\mathbb{F}_p$, its coherent cohomology is defined modulo p and can count fixed points of maps $V \to V$ only modulo p. Still:

> The principal, and perhaps only, external inspiration for the sudden vigorous launch of scheme theory in 1958 was Serre's (1955) article known by the acronym FAC. [R&S, p. P28]

Schemes

> The point, *grosso modo*, was to rid algebraic geometry of parasitic hypotheses encumbering it: base fields, irreducibility, finiteness conditions. (Serre [29, p. 201])

Schemes overtly simplify algebraic geometry. Where earlier geometers used complicated extensions of algebraically closed fields, scheme theorists use any ring. Polynomial equations are replaced by ring elements. Generic points become prime ideals. The more intricate concepts come back in when needed, which is fairly often, but not always and not from the start.

In fact this perspective goes back to unpublished work by Noether, van der Waerden, and Wolfgang Krull. Prior to Grothendieck:

> The person who was closest to scheme-thinking (in the affine case) was Krull (around 1930). He used systematically the localization process, and proved most of the nontrivial theorems in Commutative Algebra. (Serre, email of 21/06/2004, Serre's parentheses)

Grothendieck made it work. He made every ring R the coordinate ring of a scheme $\mathrm{Spec}(R)$ called the *spectrum* of R. The points are the prime ideals of R, and the scheme has a structure sheaf $\mathcal{O}_R$ on the Zariski topology for those points, like the structure sheaf on a Serre variety. It follows that the continuous structure preserving maps from $\mathrm{Spec}(R)$ to another affine scheme $\mathrm{Spec}(A)$ correspond exactly to the ring homomorphisms in the other direction:

$$A \xrightarrow{f} R \qquad\qquad \mathrm{Spec}(R) \xrightarrow{\mathrm{Spec}(f)} \mathrm{Spec}(A)\ .$$

The points can be quite intricate: "When one has to construct a scheme one generally does not begin with the set of points" [10, p. 12].

For example, the ring $\mathbb{R}[x]$ of real polynomials in one variable is the natural coordinate ring for the real line, so the spectrum $\mathrm{Spec}(\mathbb{R}[x])$ is the scheme of the real line. Each nonzero prime ideal is generated by a monic irreducible real polynomial. Those polynomials are $x - a$ for $a \in \mathbb{R}$ and $x^2 - 2bx + c$ for $b, c \in \mathbb{R}$ with $b^2 < c$. The first kind correspond to ordinary points $x = a$ of the real line. The second kind correspond to pairs of conjugate complex roots $b \pm \sqrt{b^2 - c}$. The scheme $\mathrm{Spec}(\mathbb{R}[x])$ automatically includes both real and complex points, with the nuance that a single complex point is a conjugate pair of complex roots.

A polynomial equation like $x^2 + y^2 = 1$ has many kinds of solutions. One could think of rational and algebraic solutions as kinds of complex solutions. But solutions modulo a prime p, such as $x = 2$ and $y = 6$ in the finite field $\mathbb{F}_{13}$, are not complex numbers. And solutions modulo one prime are different from those modulo another. All these solutions are organized in the single scheme

$$\mathrm{Spec}(\mathbb{Z}[x,y]/(x^2 + y^2 - 1)).$$

The coordinate functions are simply integer polynomials modulo $x^2 + y^2 - 1$. The nonzero prime ideals are not simple at all. They correspond to solutions of this equation in all the absolutely algebraic fields by which Weil explicated Kronecker's goal, including all finite fields. Indeed, the closest Grothendieck comes to defining schemes in *Récoltes et Semailles* is to call a scheme a "magic fan" (*éventail magique*) folding together varieties defined over all these fields (p. P32). This is algebraic geometry over the integers.

Now consider the ideal $(x^2 + y^2 - 1)$ consisting of all polynomial multiples of $x^2 + y^2 - 1$ in $\mathbb{Z}[x,y]$. It is prime, so it is a point of $\mathrm{Spec}(\mathbb{Z}[x,y])$. And schemes are not Hausdorff spaces: their points are generally not closed in the Zariski topology. The closure of this point is $\mathrm{Spec}(\mathbb{Z}[x,y]/(x^2 + y^2 - 1))$. This ideal is the *generic point* of the closed subscheme

$$\mathrm{Spec}(\mathbb{Z}[x,y]/(x^2 + y^2 - 1)) \rightarrowtail \mathrm{Spec}(\mathbb{Z}[x,y]).$$

The irreducible closed subschemes of any scheme are, roughly speaking, given by equations in the coordinate ring, and each has exactly one generic point.

In the ring $\mathbb{Z}[x,y]/(x^2 + y^2 - 1)$ the ideal $(x^2 + y^2 - 1)$ appears as the zero ideal, since in this ring $x^2 + y^2 - 1 = 0$. So the zero ideal is the generic point for the whole scheme $\mathrm{Spec}(\mathbb{Z}[x,y]/(x^2 + y^2 - 1))$. What happens at this generic point also happens *almost everywhere* on $\mathrm{Spec}(\mathbb{Z}[x,y]/(x^2 + y^2 - 1))$. Generic points like this achieve what earlier algebraic geometers sought from their attempts.

Schemes vindicate more classical intuitions as well. Ancient Greek geometers debated whether a tangent line meets a curve in something more than a point. Scheme theory says yes: a tangency is an infinitesimal segment around a point.

The contact of the parabola $y = x^2$ with the x-axis $y = 0$ in $\mathbb{R}^2$ is plainly given by $x^2 = 0$. As a variety it would just be the one point space $\{0\}$, but it gives a nontrivial scheme $\mathrm{Spec}(\mathbb{R}[x]/(x^2))$. The coordinate functions are real polynomials modulo x^2 or, in other words, real linear polynomials $a + bx$.

Intuitively $\mathrm{Spec}(\mathbb{R}[x]/(x^2))$ is an infinitesimal line segment containing 0 but no other point. This segment is big enough that a function $a + bx$ on it has a slope b but is too small to admit a second derivative. Intuitively a scheme map v from $\mathrm{Spec}(\mathbb{R}[x]/(x^2)$ to any scheme S is an infinitesimal line segment in S, i.e. a tangent vector with base point $v(0) \in S$.

Grothendieck's signature method, called the *relative viewpoint*, also reflects classical ideas. Earlier geometers would speak of, for example, $x^2 + t \cdot y^2 = 1$ as a quadratic equation in x, y with parameter t. So it defines a conic section E_t which is an ellipse or a hyperbola or a pair of lines depending on the parameter. More deeply, this is a cubic equation in x, y, t defining a surface E bundling together all the curves E_t. Over the real numbers this gives a map of varieties

$$E = \{\langle x, y, t\rangle \in \mathbb{R}^3 | x^2 + t \cdot y^2 = 1\} \longrightarrow \mathbb{R} \qquad \langle x, y, t\rangle \mapsto t\ .$$

Each curve E_t is the *fiber* of this map over its parameter $t \in \mathbb{R}$. Classical geometers used the continuity of the family of curves E_t bundled into surface E but generally left the cubic surface implicit as they spoke of the variable quadratic curve E_t.

Grothendieck used rigorous means to treat a scheme map $f : X \to S$ as a single scheme simpler than either one of X and S. He calls f a *relative scheme* and treats it roughly as the single fiber $X_p \subset X$ over some indeterminate $p \in S$.[1]

Grothendieck had this viewpoint even before he had schemes:

> Certainly we're now so used to putting some problem into relative form that we forget how revolutionary it was at the time. Hirzebruch's proof of Riemann-Roch is very complicated, while the proof of the relative version, Grothendieck-Riemann-Roch, is so easy, with the problem shifted to the case of an immersion. This was fantastic. [18, p. 1114]

What Hirzebruch proved for complex varieties Grothendieck proved for suitable maps $f : X \to S$ of varieties over any field k. Among other advantages this allows reducing the proof to the case of maps f, called immersions, with simple fibers.

The method relies on *base change* transforming a relative scheme $f : X \to S$ on one base S to some $f' : X' \to S'$ on some related base S'. Fibers themselves are an example. Given $f : X \to S$ each point $p \in S$ is defined over some field k, and $p \in S$ amounts to a scheme map $p : \mathrm{Spec}(k) \to S$. The fiber X_p is intuitively the part of X lying over p and is precisely the relative scheme $X_p \to \mathrm{Spec}(k)$ given by pullback:

$$\begin{array}{ccc} X_p & \longrightarrow & X \\ \downarrow & & \downarrow{\scriptstyle f} \\ \mathrm{Spec}(k) & \xrightarrow[p]{} & S \end{array}$$

Other examples of base change include extending a scheme $f : Y \to \mathrm{Spec}(\mathbb{R})$ defined over the real numbers into one $f' : Y' \to \mathrm{Spec}(\mathbb{C})$ over the complex numbers by pullback along the unique scheme map from $\mathrm{Spec}(\mathbb{C})$ to $\mathrm{Spec}(\mathbb{R})$:

$$\begin{array}{ccc} Y' & \longrightarrow & Y \\ {\scriptstyle f'}\downarrow & & \downarrow{\scriptstyle f} \\ \mathrm{Spec}(\mathbb{C}) & \longrightarrow & \mathrm{Spec}(\mathbb{R}) \end{array}$$

Other changes of base go along scheme maps $S' \to S$ between schemes S, S' taken as parameter spaces for serious geometric constructions. Each is just a pullback in the sense of category theory, yet they encode intricate information and express operations which earlier geometers had only begun to explore. Grothendieck and Jean Dieudonné took this as a major advantage of scheme theory:

> The idea of "variation" of base ring which we introduce gets easy mathematical expression thanks to the functorial language whose absence no doubt explains the timidity of earlier attempts. [17, p. 6]

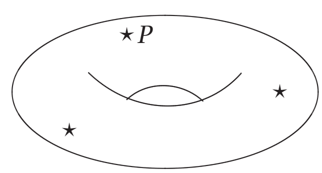
Étale Cohomology

In the Séminaire Chevalley of April 21, 1958, Serre presented new 1-dimensional cohomology groups $\tilde{\mathrm{H}}^1(\mathrm{X}, \underline{\mathrm{G}})$ suitable for the Weil conjectures: "At the end of the oral presentation Grothendieck said this would give the Weil cohomology in all dimensions! I found this very optimistic" [31, p. 255]. That September Serre wrote:

> One may ask if it is possible to define higher cohomology groups $\tilde{\mathrm{H}}^q(\mathrm{X}, \underline{\mathrm{G}})$…in all dimensions. Grothendieck (unpublished) has shown it is, and it seems that when G is finite these furnish "the true cohomology" needed to prove the Weil conjectures. On this see the introduction to [14]. [28, p. 12]

Grothendieck later described that unpublished work of 1958, saying, "The two key ideas crucial in launching and developing the new geometry were those of scheme and of topos. They appeared almost simultaneously and in close symbiosis." Specifically he framed "the notion of *site*, the technical, provisional version of the crucial notion of *topos*" [R&S, pp. P31 and P23n]. But before pursuing this idea into higher-dimensional cohomology he used Serre's idea to define the fundamental group of a variety or scheme in a close analogy with Galois theory.

Notice that Zariski topology registers punctures much more directly than it registers holes like those through the center of the torus or inside the tube.

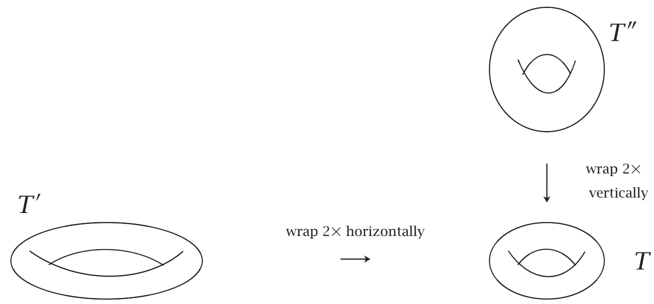


Zariski closed subsets are (locally) the zero-sets of polynomials, so a nonempty Zariski open subset of the torus is the torus minus finitely many punctures (possibly none). Such a subset might or might not be punctured at some point P itself, so the Zariski opens themselves distinguish between having and not having that puncture. But every nonempty Zariski open subset surrounds the hole through the torus center and the one through the torus tube. These subsets by themselves cannot distinguish between having and not having those holes. Coherent cohomology registers those holes by using coherent sheaves, which cannot work for the Weil conjectures, as noted above.

So Serre used many-sheeted covers. Consider two different 2-sheeted covers of one torus T. Let torus T' be

[1] *In 1942 Oscar Zariski urged something like this to Weil* [23, p. 70]. *Weil took the idea much further without finally making it a working method* [38, p. 91ff].

twice as long as T, with the same tube diameter. Wrap T' twice around T along the tube:



Let torus T'' be as long as T with twice the tube diameter. Wrap it twice around the tube. The difference between these two covers, and both of them from T itself, reflects the two holes in T.

Riemann created Riemann surfaces as analogues to number fields. As $\mathbb{Q}[\sqrt{2}]$ is a degree 2 field extension of the rational numbers $\mathbb{Q}$, so $T' \to T$ is a degree 2 cover of T. As $\mathbb{Q}[\sqrt{2}]/\mathbb{Q}$ has a two element Galois group where the nonidentity element interchanges $\sqrt{2}$ with $-\sqrt{2}$, so $T' \to T$ has a two-element symmetry group over T where the nonidentity symmetry interchanges the two sheets of T' over T.

Serre consciously extended Riemann's analogy to a far-reaching identity. He gave a purely algebraic definition of *unramified covers* $S' \to S$ which has the Riemann surfaces above as special cases, as well as Galois field extensions, and much more. Naturally, in this generality some theorems and proofs are a bit technical, but over and over Serre's unramified covers make intuitions taken from Riemann surfaces work for all these cases. Grothendieck used these to give the first useful theory of the fundamental group of a variety or a scheme, that is, the one-dimensional *homotopy*. He also worked with a slight generalization of unramified covers, called *étale maps*, which include all algebraic Riemann covering surfaces.

Serre had not calculated cohomology of sheaves but of *isotrivial fiber spaces*. Over a torus T those are roughly spaces mapped to T which may twist around T but can be untwisted by lifting to some other torus $T''' \to T$ wrapped some number of times around each hole of T. While Grothendieck [12] also used fiber spaces for one-dimensional cohomology, he found his *Tôhoku* methods more promising for higher dimensions. He wanted some notion of sheaf matching Serre's idea.

During 1958 Grothendieck saw that instead of defining sheaves by using open subsets $U \subseteq S$ of some space S, he could use étale maps $U \to S$ to a scheme. He published this idea by spring 1961 [15, §4.8, p. 298]. Instead of inclusions $V \subseteq U \subseteq S$, he could use commutative triangles over S:

In place of intersections $U \cap V \subseteq S$ he could use pullbacks $U \times_S V$ over S. Then an *étale cover* of a scheme S is any set of étale maps $U_i \to S$ such that the union of all the images is the whole of S. Sites today are often called *Grothendieck topologies*, and this site may be called the *étale topology* on S.

There are two basic ways to solve a problem locally in the étale topology on S. You could solve it on each of a set of Zariski open subsets of S whose union is S, or you could solve it in a separable algebraic extension of the coordinate ring of S. The first gives an actual, global solution if the local solutions agree wherever they overlap. The second gives a global solution if the local solution is Galois invariant—like first factoring a real polynomial over the complex numbers, then showing the factors are actually real. Étale cohomology would measure obstructions to patching actual solutions together from combinations of such local solutions.

In 1961 Michael Artin proved the first higher-dimensional geometric theorem in étale cohomology [1, p. 359]. According to David Mumford this was that the plane with origin deleted has nontrivial H^3; in the context of étale cohomology that means the coordinate plane punctured at the origin, $k^2 - \{0\}$, for any field of coordinates k. Weil's conjectures suggest that, when k is absolutely algebraic, this cohomology should largely agree with the classical cohomology of the complex case $\mathbb{C}^2 - \langle 0, 0\rangle$. That space is topologically $\mathbb{R}^4$ punctured at its origin. It has the classical cohomology of the 3-sphere S^3, and that is nontrivial in H^3. So Artin's result needed to hold in any Weil cohomology. Artin proved it does hold in the derived functor cohomology of sheaves on the étale site. Today this is *étale cohomology*.

In short, Artin showed the étale site yields not only some sheaf cohomology but a good usable one. Classical theorems of cohomology survive with little enough change. Grothendieck invited Artin to France to collaborate in the seminar that created *Théorie des topos et cohomologie étale* [2]. The subject exploded, and we will go no further into it.

Toposes are less popular than schemes or sites in geometry today. Deligne expresses his view with care: "The tool of topos theory permitted the construction of étale cohomology" [10, p. 15]. Yet, once constructed, this cohomology is "so close to classical intuition" that for most purposes one needs only some ordinary topology plus "a little faith/*un peu de foi*" [9, p. 5]. Grothendieck would "advise the reader nonetheless to learn the topos language which furnishes an extremely convenient unifying principle" [5, p. VII].

We close with Grothendieck's view of how schemes and his cohomology and toposes all came together in étale cohomology, which indeed in his hands and Deligne's gave the means to prove the Weil conjectures:

> The crucial thing here, from the viewpoint of the Weil conjectures, is that the new notion of space is vast enough, that we can associate to each scheme a "generalized space" or "topos" (called the "étale topos" of the scheme in question). Certain "cohomology invariants" of this topos (as "babyish" as can be!) seemed to have a good chance of offering "what it takes" to give the

conjectures their full meaning, and (who knows!) perhaps to give the means of proving them. [16, p. P41]


Courtesy of Patricia Princehouse.

Colin McLarty with a Great Pyrenees, from the region where Grothendieck spent much of his life.

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COMMUNICATION

Meeting Grothendieck, 2012

Katrina Honigs

Photo courtesy of Michael Moewe.

Katrina Honigs enjoys travel. In addition to having met Alexander Grothendieck, she also once touched the nose of a marmot in the Swiss Alps. Both experiences were very thrilling, though in different ways.

I met Alexander Grothendieck on January 2, 2012. I had read a bit of his mathematical work and felt a sense of connection with it. I was at a point in my third year of graduate school where I was not only not making progress on solving any problems but miserably unengaged by my work. Despite the burnout, Grothendieck's work remained an island of enjoyment in an otherwise featureless sea. So when I was in France for a conference, I sought him out.

The village of Lasserre is small and remote. My appearance in a rental car was a strange enough event that as soon as I parked, a friendly man came out of a nearby house to ask if I needed any help. It turned out Grothendieck's house was not fifty feet away.

I had purchased some *galettes du roi* that morning in preparation for apologizing to Grothendieck for entering his yard. After clearing the fence, I stepped furtively across the slightly ramshackle yard, which had many plants and terra cotta pots in various degrees of wholeness, and walked up the steps. I knocked on the door and then shouted "Monsieur Grothendieck!" and waited, but there was no response.

Suddenly I realized that a figure with a large white beard and a brown robe over his clothes had appeared utterly silently quite nearby on my left. In one hand, he held a short pitchfork loosely at his side. It reminded me of his doodle of devils with pitchforks around the Grothendieck-Riemann-Roch formula. His free hand rose, brandishing an admonitory finger. "Il ne faut pas entrer," he said, advancing slowly toward me. I tried to form some sentences about visiting, but Grothendieck did not react. He continued to walk slowly toward me, wagging his finger, telling me that I shouldn't be in here disturbing him. I tried to give him the galettes, but he told me again to leave.

Once he had seen me leave his yard, we studied each other from opposite sides of the gate for a moment. We were a similar height, and his blue eyes were alert and focused. Grothendieck asked me not angrily, but a bit sternly, in French how I knew his address and how I had gotten there. He told me again that I should not have come in and should not have disturbed him in his "cloître", which reinforced the impression given by the brown robe that he thought of himself, in some sense, as a monk. When I was given the address, I had said I wouldn't tell Grothendieck how I came by it, so I just watched him silently during this monologue, looking shocked.

Then, he asked me my name and explained that he could not hear very well anymore and so I must shout into his ear. After I said my name, I started to spell it, but he stopped me partway, since he had already recognized it: a couple of weeks before I had sent what I now realize was a very enthusiastic fan letter. He then switched to English and, irritably,

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asked me why I had included a French translation. "To be polite? La politesse?" Nothing by him that I had read had been in English. But of course he knew English, and I had offended him a little.

He told me he had responded to my letter, explaining that my reasons for contacting him were not sufficient and that I should not visit. I felt a bit deflated but also couldn't help but be a little amused. Trust a mathematician to tell me that my reasons for writing were "not sufficient."

After Grothendieck discovered that I had not received his response to my letter, he seemed to decide that this partly explained my presence. But he was still dissatisfied and asked again why I had visited. Clearly a bit suspicious that I had some unsavory motive, he said he thought my visit must indicate that I wanted something. I told him that maybe he didn't realize, but he is very famous and I just wanted to meet him. He shrugged and said again that I didn't have any satisfactory reason for visiting, but I could tell that he was a bit amused by being told that he was famous, and he relaxed a bit.

He told me that he could see from my face that I didn't have any bad intentions and that he would never want to harm anyone. I saw then that the pitchfork was no longer in his hand but propped against the fence. If you had received my reply, he said, you would have understood that I am not taking visitors and you should not disturb someone in their retirement. He expected, though, to receive a letter from me soon explaining how I got his address. "C'est la moindre des choses," he intoned, switching back to French for a moment. He used to receive all his visitors, he said, but he had had two very bad experiences and no longer did it, though he was very sorry that I came such a long way to not be invited in and sorry for himself as well that he was not able to invite me in. He took my hand and shook it. He told me that he thought we would meet again, very soon, though not in this life. He told me he thought that he would die within the year, though this prediction was made with a practiced air that suggested this was not the first time he had made it.

After these heavy declarations, he turned his attention back to my visit. For all his bluster about not wishing to be disturbed, a part of him was curious about his visitor. How did I get here? On a train? No, in a car. Am I rich? No. Am I poor? Of course I'm poor! I'm a graduate student! I laughed, and he chuckled good-naturedly. Am I alone? Yes. Didn't I have something for him? The bakery box reemerged, and I opened it to show him the contents. He looked at the pastry inside. What is it? Galettes. What? Galettes! Did you make them? No, I bought them. What? I bought them! Oh, thank you for making them. He took the box from me and said he wanted to get something for me too and then went back into his house. I was glad for my instinct to bring baked goods. They smooth everything over in the American Midwest, where I'm from.

I was not able to discuss math with him at all. At one point, when I tried to make our conversation more detailed by writing on a piece of paper, he waved it away. But we had spoken more than I had thought we might. When he came back out of his house he presented me with a tomato and a packet of almond paste. The tomato was large and fresh and came from his garden—impressive for January—and he told me to eat it in good health. He also said I should remember that it was his friend (likely something was lost in translation). The packet of almond paste was very large. A kilo.

"He told me we would meet again, very soon, though not in this life."

After the exchange of gifts was over, it seemed we were finished. Grothendieck wished me well, shook my hand again, and, after entreating me once more to write a letter telling him how I came to know his address, told me goodbye and walked back to his house. I said goodbye as well, but his back was already turned to me, and I realized right after I spoke that he likely didn't hear me.

My experience of the rest of the day was odd and heightened. The drive back through the countryside. The primary colors of the public transit train in Toulouse. The tomato, when I ate it later that day. As the days and weeks went on, the visit was something I reflected on with enjoyment. My burnout faded, and I got more excited about my work again.

A little while after my visit, I did write Grothendieck again, but my letters were returned. Although my fantasies of having some magical conversation about math with him had to be swept aside, I am grateful to have had the chance to meet him.

A fuller version of this essay is at Katrina Honigs's personal website, `math.utah.edu/~honigs/Grothendieck.pdf`.