Alexandre Grothendieck 1928–2014, Part 1

Michael Artin, Allyn Jackson, David Mumford, and John Tate, Coordinating Editors

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.

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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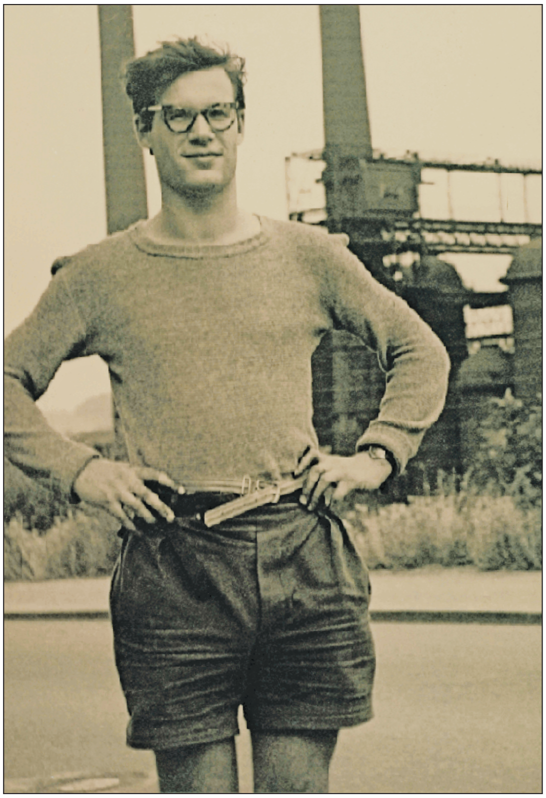
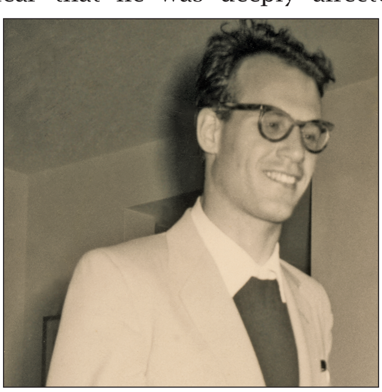


Photo courtesy of Paulo Rihenhoim.

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.



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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.

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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 syzygy 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 $\overline{k}$? 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 $S = \mathrm{Spec}(\overline{k})$. 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.

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[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 *prorepresenting* a functor, F: fix an element a of $F(\mathrm{Spec}(k))$ 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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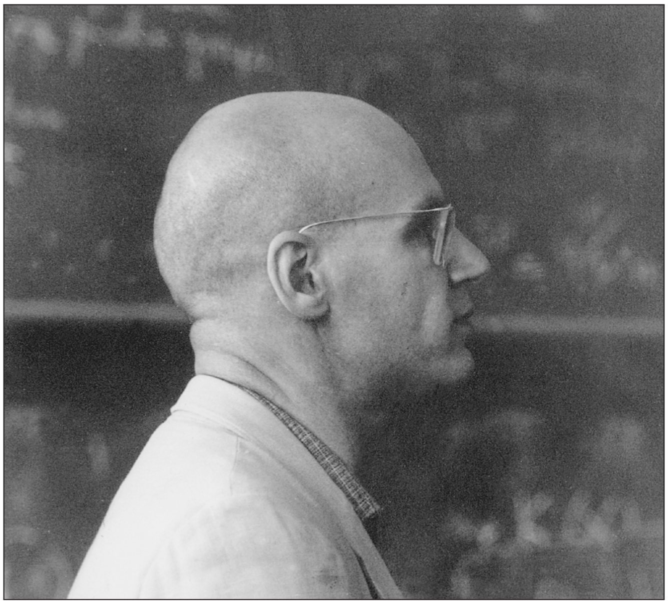

Photo courtesy of the estate of Friedrich Hirzebruch.

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.

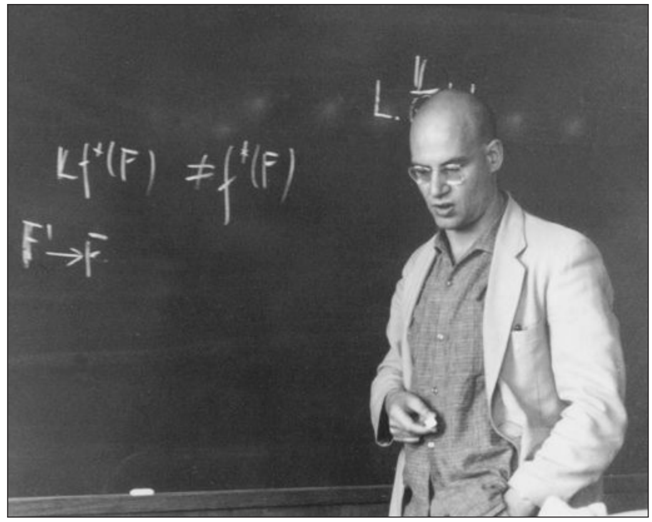
I learned more of Grothendieck's innovations in David Mumford's course, spring 1964, published as [7]. It was devoted to Grothendieck's proof of completeness of the characteristic system of a good complete algebraic system of curves on a smooth projective complex surface F. It is the *first algebraic 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 said, "I would appreciate knowing a simple proof," of the key ingredient, the Nakai–Moishezon criterion.



Grothendieck around 1965.

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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