# 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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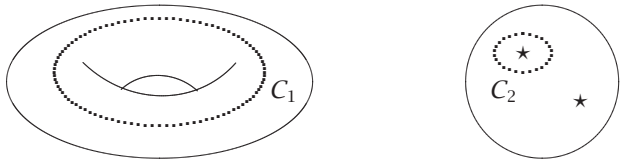
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## The Beginnings of Cohomology

> 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 $\omega$ 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 $\omega$.

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 $\omega$. 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 $\omega$ 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 $\omega$:

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

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:

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

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 $\mathrm{H}_1$ 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 $i$th 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 $i$th 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:

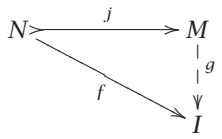
$$\{0\} \longrightarrow A \longrightarrow I_0 \longrightarrow I_1 \longrightarrow \dots .$$

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 $j : N \rightarrowtail M$ and homomorphism

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



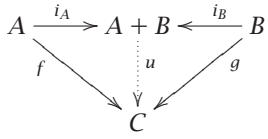

This works because every $R$-module $A$ over any ring $R$ embeds in some injective $R$-module. No one believed anything this simple would work for sheaves. Sheaf cohomology was defined only for sufficiently regular spaces using various, more complicated topological substitutes for injectives. Grothendieck 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.

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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 nonclosed 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 $\nu$ 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 $\nu(0) \in S$.

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:

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

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]

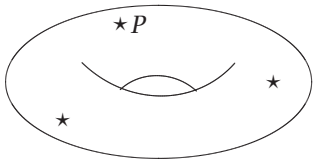
## Étale Cohomology

In the Séminaire Chevalley of April 21, 1958, Serre presented new 1-dimensional cohomology groups $\check{H}^1(X, \underline{G})$ 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 $\check{H}^q(X, \underline{G})$…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.

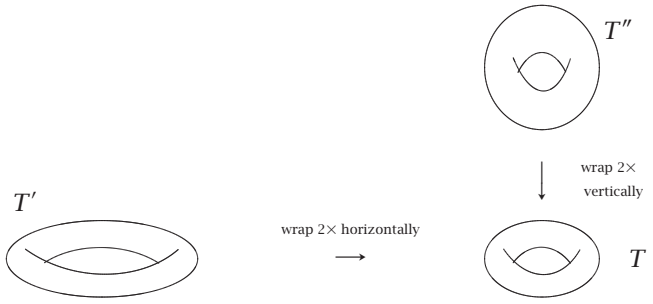



Zariski closed subsets are (locally) the zero-sets of polynomials, so a nonempty Zariski open subset of the torus is the torus minus finitely many punctures (possibly none). Such a subset might or might not be punctured at some point $P$ itself, so the Zariski opens themselves distinguish between having 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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