The Rising Sea:
Grothendieck on simplicity and generality I

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In 1949 André Weil published striking conjectures linking number theory to topology, and sketched a topological strategy for a proof. Around 1953 Jean-Pierre Serre took on the project and soon recruited Alexander Grothendieck. Serre created a series of concise elegant tools which Grothendieck and coworkers simplified into thousands of pages of category theory. Miles Reid, for example, says "Grothendieck himself can't necessarily be blamed for this since his own use of categories was very successful in solving problems" [Reid 1990, p. 116]. Here we focus on methods Grothendieck established by 1958: Abelian categories for derived functor cohomology, and schemes. We touch on toposes and étale cohomology which arose around 1958 as the context for the work.

Grothendieck describes two styles in mathematics. If you think of a theorem to be proved as a nut to be opened, so as to reach "the nourishing flesh protected by the shell", then the *hammer and chisel* principle is: "put the cutting edge of the chisel against the shell and strike hard. If needed, begin again at many different points until the shell cracks—and you are satisfied". He says:

> I can illustrate the second approach with the same image of a nut to be opened. The first analogy that came to my mind is of immersing the nut in some softening liquid, and why not simply water? From time to time you rub so the liquid penetrates better, and otherwise you let time pass. The shell becomes more flexible through weeks and months—when the time is ripe, hand pressure is enough, the shell opens like a perfectly ripened avocado!
>
> A different image came to me a few weeks ago. The unknown thing to be known appeared to me as some stretch of earth or hard marl, resisting penetration... the sea advances insensibly in silence, nothing seems to happen, nothing moves, the water is so far off you hardly hear it... yet it finally surrounds the resistant substance. [Grothendieck 1985–1987, pp. 552-3][1]

In this "rising sea" the theorem is "submerged and dissolved by some more or less vast theory, going well beyond the results originally to be established" [Grothendieck 1985–1987, p. 555].[2] Grothendieck calls this his approach and Bourbaki's. Here as so often he sees math research, exposition, and teaching as all the same. He

[1]All translations in this paper are my own. Pierre Deligne points out that Grothendieck's mastery of language in some of *Récoltes et Semailles* parallels the serious responsibility he took for naming the various concepts he created (e-mail, 13 May 2002).

[2]Here "rising sea" translates "la mer qui monte". The primary meaning in French is a rising tide though it often means waves rising against rocks. It can mean a generally rising sea level as after an ice age, or in global warming. The sometimes psychoanalytic tone of [Grothendieck 1985–1987] makes us notice the pun "l'amère qui monte", a rising bitterness. Wordplays on "la mer/l'amère/la mère (the mother)" are familiar in French but Grothendieck may not be interested in them.

compares his or Serre's research with Bourbaki sessions planning the *Elements of Mathematics*.

Deligne describes a characteristic Grothendieck proof as a long series of trivial steps where "nothing seems to happen, and yet at the end a highly non-trivial theorem is there" [Deligne 1998, p. 12]. I want to look at this style in Grothendieck's work and what it means philosophically. In Grothendieck it is an extreme form of Cantor's freedom of mathematics. It is not only the freedom to build a world of set theory for mathematics but to build an entire world—specifically a "topos", as large as the universe of all sets—adapted to any single problem such as a single polynomial equation on a finite field.

In fact Grothendieck describes himself as creating "new 'worlds' " [Grothendieck 1985–1987, p. 554]. But he means what he elsewhere calls "building beautiful houses", that is, framing theories and methods that become a heritage others can use [Grothendieck 1985–1987, p. P27]. He has certainly done that. So has Jean-Pierre Serre in a very different way.

Grothendieck says Serre generally uses the hammer and chisel [Grothendieck 1985–1987, p. 558]. He finds Serre "Super Yang" against his own "Yin"—but not at all in the sense of being heavy handed—rather Serre is the "incarnation of elegance" [Grothendieck 1985–1987, p. 969]. That is the difference. Serre concisely cuts to an answer. Grothendieck creates truly massive books with numerous coauthors, offering set-theoretically vast yet conceptually simple mathematical systems adapted to express the heart of each matter and dissolve the problems.[3] This is the sense of world building that I mean.

The example of Serre and Grothendieck highlights another issue: Grothendieck says that from 1955 to 1970 Serre was at the origin of most of his ideas [Grothendieck 1985–1987, p. 982]. This includes *every* major step towards the Weil conjectures. It is a deep collaboration comparable to Dedekind and Emmy Noether. One difference of course is that Serre and Grothendieck knew each other. Another is that Dedekind and Noether are much the same in style. Serre and Grothendieck are quite opposite. Truly deep collaboration needs more attention. The most important and challenging remark ever made about 20th century mathematics was Noether's watchword "it is all already in Dedekind".

The Weil Conjectures

Solving Diophantine equations, that is giving integer solutions to polynomials, is often unapproachably difficult. Weil describes one strategy in a letter to his sister, the philosopher Simone Weil: Look for solutions in richer fields than the rationals,

[3]Deligne emphasizes (e-mail, 13 May 2003) that the set theoretic size of toposes never fazed Grothendieck but was never the point either; and it is inessential in that the same technical work can be done by small Grothendieck topologies.

Indeed Grothendieck posited the very large *Grothendieck universes* as a technical fix to gain the greater conceptual unity of toposes over Grothendieck topologies [Artin, Grothendieck & Verdier 1972, pp. 185ff.]. But the set theory was so far from his real point that he expressly faulted Bourbaki's concept of structure for focusing on set theoretic apparatus, rather than on simple categorical properties [Grothendieck 1985–1987, p. P22]. We return to this topic below. One goal of William Lawvere and Myles Tierney's axioms for elementary toposes, and much work on categorical foundations since, is to formalize the topos unity directly, with far less set-theoretic or proof-theoretic strength than Grothendieck universes. See Lawvere [Lawvere 1979] or [Lawvere 1975].

perhaps fields of rational functions over the complex numbers. But these are quite different from the integers:

> We would be badly blocked if there were no bridge between the two.
>
> And *voilà* god carries the day against the devil: this bridge exists; it is the theory of algebraic function fields over a finite field of constants. (Letter of 26 March 1940, in [Weil 1979, vol.1, p. 252])

A solution modulo 5 to a polynomial $P(X, Y, ..Z)$ is a list of integers $X, Y, ..Z$ making the value $P(X, Y, ..Z)$ divisible by 5, or in other words equal to 0 modulo 5. For example, $X^2 + Y^2 - 3$ has no integer solutions. That is clear since X and Y would both have to be 0 or ± 1, to keep their squares below 3, and no combination of those works. But it has solutions modulo 5 since, among others, $3^2 + 3^2 - 3 = 15$ is divisible by 5. Solutions modulo a given prime p are easier to find than integer solutions and they amount to the same thing as solutions in the finite field of integers modulo p.

To see if a list of polynomial equations $P_i(X, Y, ..Z) = 0$ have a solution modulo p we need only check p different values for each variable. Even if p is impractically large, equations are more manageable modulo p. Going farther, we might look at equations modulo p, but allow some irrationals, and ask how the number of solutions grows as we allow irrationals of higher and higher degree—roots of quadratic polynomials, roots of cubic polynomials, and so on. This is looking for solutions in all finite fields, as in Weil's letter.

An answer to this question about finite fields does not directly answer the questions about integer or rational solutions. It might help. It is interesting in itself. And it turns out to have surprising applications such as planning optimally efficient networks [Li 1996]. Building on work by earlier number theorists, Weil conjectured a penetrating form for the exact answer and some useful approximations. More than that, he conjectured an amazing link with topology.

The key technical points about finite fields are: For each prime number p, the field of integers modulo p form a field, written $\mathbb{F}_p$. For each natural number $r > 0$ there is (up to isomorphism) just one field with p^r elements, written as $\mathbb{F}_{p^r}$ or as $\mathbb{F}_q$ with $q = p^r$. This comes from $\mathbb{F}_p$ by adjoining the roots of a degree r polynomial.[4] These are all the finite fields. Trivially, then, for any natural number $s > 0$ there is just one field with q^s elements, namely $\mathbb{F}_{p^{(r+s)}}$ which we may write $\mathbb{F}_{q^s}$. The union for all r of the $\mathbb{F}_{p^r}$ is the algebraic closure $\overline{\mathbb{F}_p}$. By Galois theory, roots for polynomials in $\mathbb{F}_{p^r}$, are fixed points for the r-th iterate of the *Frobenius morphism*, that is for the map taking each $x \in \overline{\mathbb{F}_p}$ to x^{p^r} (see e.g. [Serre 1973]).

Take any good n-dimensional algebraic space (any smooth projective variety of dimension n) defined by integer polynomials on a finite field $\mathbb{F}_q$. For each $s \in \mathbb{N}$, let N_s be the number of points defined on the extension field $\mathbb{F}_{(q^s)}$. Define the zeta function $Z(t)$ as an exponential using a formal variable t:[5]

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[4] The polynomial may be reducible but must not have a kind of redundancy in its roots.

[5] That is, you can think of the exponential function as a formal power series, $\exp(t) = \sum_0^\infty (t^n/n!)$, rather than as a complex valued function of a complex number argument.

$$Z(t) \;=\; \exp\,(\sum_{s=1}^{\infty} N_s \frac{t^s}{s})$$

The first Weil conjecture says $Z(t)$ is a rational function:

$$Z(t) = \frac{P(t)}{Q(t)}$$

for integer polynomials $P(t)$ and $Q(t)$. This is a strong constraint on the numbers of solutions N_s. It means there are complex algebraic numbers $a_1 \ldots a_i$ and $b_1 \ldots b_j$ such that

$$N_s = (a_1^s + \ldots + a_i^s) - (b_1^s + \ldots + b_j^s)$$

And each algebraic conjugate of an a (resp. b) also an a (resp. b).

The second conjecture is a functional equation:

$$Z(\frac{1}{q^n t}) \;=\; \pm q^{nE/2} t^E Z(t)$$

This says the operation $x \mapsto q^n/x$ permutes the a's (resp. the b's).

The third is a Riemann Hypothesis

$$Z(t) \;=\; \frac{P_1(t)P_3(t)\cdots P_{2n-1}(t)}{P_0(t)P_2(t)\cdots P_{2n}(t)}$$

where each P_k is an integer polynomial with all roots of absolute value $q^{-k/2}$. That means each a has absolute value q^k for some $0 \leq k \leq n$. Each b has absolute value $q^{(2k-1)/2}$ for some $0 \leq k \leq n$.

Over it all is the conjectured link to topology. Let $B_0, B_1, \ldots B_{2n}$ be the Betti numbers of the complex manifold defined by the same polynomials. That is, each B_k gives the number of k-dimensional holes or handles on the continuous space of complex number solutions to the equations. And recall an algebraically n-dimensional complex manifold is topologically $2n$-dimensional. Then each P_k has degree B_k. And E is the Euler number of the manifold, the alternating sum

$$\sum_{k=0}^{2n} (-1)^k B_k$$

On its face the topology of a continuous manifold is worlds apart from arithmetic over finite fields. But the topology of this manifold tells how many a's and b's there are with each absolute value. This implies useful numerical approximations to the numbers of roots N_s.

Special cases of these conjectures, with aspects of the topology, were proved before Weil, and he proved more. All dealt with curves (1-dimensional) or hypersurfaces (defined by a single polynomial). Those proofs, omitting all reference to topology, make the focus of five chapters in Ireland and Rosen *A Classical Introduction to Modern Number Theory.* The book never mentions Grothendieck but calls Deligne's completion of the proof "one of the most remarkable achievements of this century" [Ireland & Rosen 1992, p. 151].[6]

Weil presented the topology as motivating the conjectures for higher dimensional varieties [Weil 1949, p. 507]. He especially pointed out how the whole series of conjectures would follow quickly if we could treat the spaces of finite field solutions as topological manifolds. The Lefschetz fixed point theorem would reduce them to a pair of graduate exercises in linear algebra—now literally exercises 24 and 25 of Chapter XIV in Serge Lang's *Algebra* [Lang 1993]—trivial calculations with Jordan canonical forms of matrices.[7]

The conjectures were the intuition of an encyclopedic mathematician, drawn to classical problems, skilled at calculation, informed on the latest methods, and unafraid to pursue connections that others might think far-fetched, coincidental, or hopeless. The topological strategy was powerfully seductive but seriously remote from existing tools. Weil's arithmetic spaces were not even precisely defined. To all appearances they would be finite or (over the algebraic closures of the finite fields) countable and so everywhere discontinuous. Topological manifold methods could hardly apply.

Abelian Categories

Serre gave a more thoroughly *cohomological* turn to the conjectures than Weil had. Grothendieck says

> Anyway Serre explained the Weil conjectures to me in cohomological terms around 1955—and it was only in these terms that they could possibly 'hook' me . . . I am not sure anyone but Serre and I, not even Weil if that is possible, was deeply convinced such [a cohomology] must exist. [Grothendieck 1985–1987, p. 840]

Specifically Serre approached the problem through *sheaves*, a new method in topology that he and others were exploring. Grothendieck would later describe each sheaf on a space T as a "meter stick" measuring T. The *cohomology* of a given sheaf gives a very coarse summary of the information in it—and in the best case it highlights just the information you want. Certain sheaves on T produced the Betti numbers. If you could put such "meter sticks" on Weil's arithmetic spaces, and prove standard topological theorems in this form, the conjectures would follow.

By the nuts and bolts definition, a sheaf $\mathcal{F}$ on a topological space T is an assignment of Abelian groups to open subsets of T, plus group homomorphisms among them, all meeting a certain covering condition. Precisely these nuts and bolts were unavailable for the Weil conjectures because the arithmetic spaces had no useful topology in the then-existing sense.

At the École Normale Supérieure, Henri Cartan's seminar spent 1948-49 and 1950-51 focussing on sheaf cohomology. Samuel Eilenberg presented some sessions. Serre then in his early 20s also ran sessions. Grothendieck, two years younger,

[6] For more on the history of the conjectures see [Houzel 1994]. See [Mumford & Tate 1978] for an expertly gentle sketch of the conjectures and Grothendieck's and Deligne's contributions, and [Hartshorne 1977] Appendix C for the clearest possible full statement of the conjectures in current terms.

[7] Weil expanded on this at the 1954 International Congress of Mathematicians, in a 1974 historical talk, and again commenting on his collected works [Weil 1979, vol. 1, pp. 568f, vol. 2, pp. 180-88, vol. 3, pp. 279-302].

attended. As one motive, there was already de Rham cohomology on differentiable manifolds, which not only described their topology but also described differential analysis on manifolds. And during the time of the seminar Cartan saw how to modify sheaf cohomology as a tool in complex analysis. Given a complex analytic variety V Cartan could define sheaves that reflected not only the topology of V but also complex analysis on V. He and Serre would develop this over the coming years (see [Fasanelli 1981]). What could sheaves do?

These were promising for the Weil conjectures since Weil cohomology would need sheaves reflecting algebra on those spaces. But understand, this differential analysis and complex analysis used sheaves and cohomology in the usual topological sense. Their innovation was to find particular new sheaves which capture analytic or algebraic information that a pure topologist might not focus on.

The greater challenge to the Séminaire Cartan was, that along with the cohomology of topological spaces, the seminar looked at the cohomology of groups. Here sheaves are replaced by G-modules.[8] This was formally quite different from topology yet it had grown from topology and was tightly tied to it. Indeed Eilenberg and Mac Lane created category theory in large part to explain both kinds of cohomology by clarifying the links between them. The seminar aimed to find what was common to the two kinds of cohomology and they found it in a pattern of functors.

The cohomology of a topological space X assigns to each sheaf $\mathcal{F}$ on X a series of Abelian groups $\mathrm{H}^n\mathcal{F}$ and to each sheaf map $f\!:\!\mathcal{F}\!\to\!\mathcal{F}'$ a series of group homomorphisms $\mathrm{H}^n f\!:\!\mathrm{H}^n\mathcal{F}\!\to\!\mathrm{H}^n\mathcal{F}'$. The definition requires that each H^n is a functor, from sheaves on X to Abelian groups. These functors have certain properties which we need not know in detail. A crucial one is:

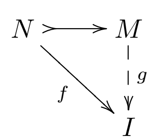
$$\mathrm{H}^n\mathcal{F} = 0 \text{ for } n > 0$$

for any *fine* sheaf $\mathcal{F}$ where a sheaf is *fine* if it meets a certain condition borrowed from differential geometry by way of Cartan's complex analytic geometry.[9]

The cohomology of a group G assigns to each G-module M a series of Abelian groups $\mathrm{H}^n M$ and to each homomorphism $f\!:\!M\!\to\!M'$ a series of homomorphisms $\mathrm{H}^n f\!:\!\mathrm{H}^n M\!\to\!\mathrm{H}^n M'$. Each H^n is a functor, from G-modules to Abelian groups. These functors have the same properties as topological cohomology except that:

$$\mathrm{H}^n M = 0 \text{ for } n > 0$$

for any injective module M. A G-module I is *injective* if: For every G-module inclusion $N\!\rightarrowtail\!M$ and homomorphism $f\!:\!N\!\to\!I$ there is at least one $g\!:\!M\!\to\!I$ making this commute



[8]For any group G, a G-module is an Abelian group M plus an action of G on M. That is for each $g \in G$ a group isomorphism $\alpha_g\!:\!M\!\to\!M$ such that for any $g, h \in G$, $\alpha_g\alpha_h = \alpha_{(gh)}$.

[9]A sheaf is *fine* if it admits partitions of unity in this sense: For every locally finite cover of X by opens U^i there are endomorphisms ℓ^i of $\mathcal{F}$ such that:

(1) for each i, the endomorphism ℓ^i is zero outside of some closed set contained in U^i.
(2) the sum $\sum_i \ell^i$ is the identity

This is taken from the Séminaire Cartan 1950-51 [Cartan 1948–, exp. 15]

The Séminaire Cartan took the analogy no further. And this caused a serious problem. The great outcome of the seminar was the book [Cartan & Eilenberg 1956], with its declaration (in fact, a great victory for Emmy Noether's outlook):

> During the last decade the methods of algebraic topology have invaded extensively the domain of pure algebra, and initiated a number of internal revolutions. The purpose of this book is to present a unified account of these developments and to lay the foundations for a full-fledged theory. [Cartan & Eilenberg 1956, p. v.]

But the unified account had to exclude its own beginnings.

They could treat the cohomology of several different algebraic structures: groups, Lie groups, associative algebras. These all rest on injective resolutions. They could not include topological spaces, the source of the whole, and still one of the main motives for pursuing the other cohomologies. Topological cohomology rested on the completely different apparatus of fine resolutions (or, in Roger Godement's hands [Godement 1958], flabby and soft resolutions).

As to the search for a Weil cohomology, this left two questions: What would Weil cohomology use in place of topological sheaves or G-modules? And what resolutions would give their cohomology?

Specifically, [Cartan & Eilenberg 1956] defines group cohomology (like several other constructions) as a *derived functor*, which in turn is defined using injective resolutions. So the cohomology of a topological space was not a derived functor in their technical sense. But a looser sense was apparently current.

Grothendieck wrote to Serre while [Cartan & Eilenberg 1956] was in preparation:

> I have realized that by formulating the theory of derived functors for categories more general than modules, one gets the cohomology of spaces at the same time at small cost. The existence follows from a general criterion, and fine sheaves will play the role of *injective* modules.[10] One gets the fundamental spectral sequences as special cases of delectable and useful general spectral sequences. But I am not yet sure if it all works as well for non-separated spaces and I recall your doubts on the existence of an exact sequence in cohomology for dimensions ≥ 2. Besides this is probably all more or less explicit in Cartan-Eilenberg's book which I have not yet had the pleasure to see. (26 Feb. 1955, [Colmez & Serre 2001, pp. 13-14])

Here he lays out the whole paper [Grothendieck 1957], commonly cited as *Tôhoku* for the journal that published it. There are several issues. For one thing, fine resolutions do not work for all topological spaces but only for the paracompact—that is, Hausdorff spaces where every open cover has a locally finite refinement. The Séminaire Cartan called these *separated* spaces. The limitation was no problem for differential geometry. All differential manifolds are paracompact. Nor was it a

[10] Apparently Grothendieck focussed on the idea of an effaceable functor before he looked for injective resolutions in topology. He saw that the role of fine sheaves is: they are acyclic, and each sheaf embeds in one of them [Colmez & Serre 2001, p. 12]. This answers Serre's question below, as to which properties of fine sheaves one actually needs. And it explains why basically every kind of resolution that works at all, gives the same cohomology as every other: all give the (unique up to isomorphism) universal delta functor over the global section functor.

problem for most of analysis. But it was discouraging from the viewpoint of the Weil conjectures since non-trivial algebraic varieties are never Hausdorff.

Serre replied using the same loose sense of derived functor:

> The fact that sheaf cohomology is a special case of derived functors (at least for the paracompact case) is not in Cartan-Sammy.[11] Cartan was aware of it and told [David] Buchsbaum to work on it, but he seems not to have done it. The interest of it would be to show just which properties of fine sheaves we need to use; and so one might be able to figure out whether or not there are enough fine sheaves in the non-separated case (I think the answer is no but I am not at all sure!). (12 March 1955, [Colmez & Serre 2001, p. 15])[12]

So Grothendieck began rewriting Cartan-Eilenberg before he had seen it. To the Séminaire Bourbaki in 1957 he described his work as a form of Cartan-Eilenberg's homological algebra [Bourbaki 1949–, p. 149-01]. Among other things he pre-empted the question of resolutions for Weil cohomology. Before anyone knew what "sheaves" it would use, Grothendieck knew it would use injective resolutions. He did this by asking not what sheaves "are" but how they relate to one another. As he later put it, he set out to:

> consider the set[13] of all sheaves on a given topological space or, if you like, the prodigious arsenal of all the "meter sticks" that measure it. 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"... From here on, this kind of "measuring superstructure" called the "category of sheaves" will be taken as "incarnating" what is most essential to that space. [Grothendieck 1985–1987, p. P38]

The Séminaire Cartan had shown this structure in front of your nose suffices for much of cohomology. Definitions and proofs can be given in terms of commutative diagrams and exact sequences without asking, most of the time, what

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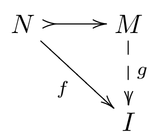
[11] "Sammy" was a familiar name for Samuel Eilenberg.

[12] David Buchsbaum tells me his problem was not posed by Cartan (e-mail 1 June 2003). Buchsbaum's dissertation gave categorical axioms for derived functors, using injectives but also a more general idea based on injectives (see Theorem 5.1 in [Buchsbaum 1955], and compare the theorem of the same number in [Cartan & Eilenberg 1956]). After that he tried to show sheaves on any topological space have enough injectives, which would imply that sheaf cohomology is a derived functor cohomology in the strict Cartan-Eilenberg sense. He sent Cartan an outline of a proof he had attempted, but had not succeeded in completing, and Cartan encouraged him in this project. Buchsbaum dropped the pursuit of injective sheaves when he found what is now called *effaceability*: each sheaf embeds in a fine sheaf, fine sheaves are acyclic, and he saw this was enough to uniquely characterize the sheaf cohomology functor. He later saw that an even weaker condition sufficed to characterize derived functors: Roughly, for each cocycle α of an object A there must be an embedding $A \rightarrowtail B$ which kills α.

[13] Grothendieck was well aware that this "set" is actually a proper class, the size of the universe of all sets.

these are diagrams *of.* Grothendieck and Buchsbaum (in an appendix to Cartan-Eilenberg [Cartan & Eilenberg 1956]) independently pursued this idea, extending work in Mac Lane [Mac Lane 1948].[14]

Grothendieck went farther than any other, insisting that the "formal analogy" between sheaf cohomology and group cohomology should become "a common framework including these theories and others" [Grothendieck 1957, p. 119]. To start with, injectives have a nice categorical sense: An object I in any category is injective if, for every monic $N \rightarrowtail M$ and arrow $f : N \to I$ there is at least one $g : M \to I$ such that



Fine sheaves are not so diagrammatic.

Grothendieck saw that Reinhold Baer's original proof in [Baer 1940] that modules have injective resolutions was largely diagrammatic itself.[15] So Grothendieck gave diagrammatic axioms for the basic properties used in cohomology, and called any category that satisfies them an *Abelian category.* He gave further diagrammatic axioms tailored to Baer's proof: Every category satisfying these axioms has injective resolutions. Such a category is called an AB5 category, and sometimes around the 1960s a *Grothendieck category* though that term has been used in several senses.

These axioms are easily verified for sheaf categories on topological spaces, proving that topological cohomology can use injective resolutions. Grothendieck soon learned a "really trivial" proof of that particular claim from Godement (letter to Serre of 16 January 1956, [Colmez & Serre 2001, p. 27].[16] Weibel [Weibel 1999, p. 812] shows how this proof is implicit in methods of the Séminaire Cartan. It is not explicit. The question seems not to have arisen in the Séminaire. Even when Serre wrote to Grothendieck about topological cohomology as a derived functor, he put it in terms of generalizing the properties of fine sheaves (and so generalizing the definition of derived functor) and not of finding enough injective sheaves [Colmez & Serre 2001, p. 15, quoted above].

So sheaves on *any* topological space have injective resolutions and thus have derived functor cohomology in the strict sense. For paracompact spaces this agrees with cohomology from fine, flabby, or soft resolutions. So you can still use those,

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[14]Grothendieck has said he did not know Mac Lane's work. Surely he did not recall any article by Mac Lane. He was not careful about sources at that time, and he read less than he heard about from friends, notably Serre. But he was travelling in the Mac Lane's circles, indeed working in Kansas in the midwest US when he did the work, and he used Mac Lane's term "Abelian category", so there was surely an influence. Buchsbaum had seen [Mac Lane 1948] and rather echoed its title in [Buchsbaum 1955] but did not use Mac Lane's terminology.

[15]Cartan and Eilenberg [Cartan & Eilenberg 1956] give Baer's proof. They as well as Grothendieck repair a set-theoretic error in it. Baer says to take any infinite cardinal Ω larger than a given other Λ. Then he reasons as if every function $\Lambda \to \Omega$ is contained in some initial segment of Ω (in other words, he takes it that Λ cannot be cofinal in Ω). Cartan and Eilenberg specify Ω as the next larger than Λ and say "because of the choice of Ω" each function is contained in an initial segment [Cartan & Eilenberg 1956, p. 10]. Grothendieck [Grothendieck 1957, p. 137] takes a few lines to prove the assumption does suffice, probably because he was reading manuscripts for Bourbaki's *Théorie des ensembles* [Bourbaki 1958].

[16]The proof is published in Godement [Godement 1958, p. 260]. Godement cites heavily both the Séminaire Cartan and Grothendieck [Grothendieck 1957].

if you want them, and you will. But Grothendieck treats paracompactness as a "restrictive condition", well removed from the basic theory, and he specifically mentions the Weil conjectures [Grothendieck 1957, p. 120].

Beyond that, Grothendieck's approach works for topology the same way it does for all cohomology. And, much further, the axioms apply to many categories other than categories of sheaves on topological spaces or categories of modules. They go far beyond topological and group cohomology, in principle, though in fact there were few if any known examples outside that framework when they were given.

The generality at the same time simplified homological algebra by focussing on just the relevant features. Textbooks today rarely use the generality in principle. They rarely even discuss sheaves on topological spaces. Yet they are generally organized in Abelian category terms.

Eisenbud's *Commutative Algebra* [Eisenbud 1995] takes one common strategy, where Abelian categories are not even defined but are referred to and the definitions and proofs are quite diagrammatic so that most of them are effectively in Abelian category terms although they are officially stated only for categories of modules. Hartshorne *Algebraic Geometry* [Hartshorne 1977, p. 202] gives the Abelian category axioms and relies on several kinds of Abelian categories other than module categories. He does not prove the theorems, but describes several ways to do it. Lang's *Algebra*, 1st and 2nd edition [Lang 1993, p. 105], famously gave the Abelian category axioms, with an exercise: "Take any book on homological algebra, and prove all the theorems without looking at the proofs given in that book". He dropped that from the latest edition, probably because today's homological algebra textbooks already take the axiomatic viewpoint.

Serre's key contribution to the Séminaire Cartan, and in his 1951 dissertation (published in the *Annals of Mathematics*, [Serre 1951]), was to clarify *spectral sequences* and extend their range and power. A spectral sequence was a tremendously powerful device for computing Abelian groups. They were and still are the standard tool for non-trivial calculations in cohomology. A spectral sequence was an infinite series of infinite two-dimensional arrays of Abelian groups and group homomorphisms, with each successive array gotten from the homology of the one before.

No single point about them is difficult. They are imposing from their sheer mass. As Grothendieck began working on *Tôhoku* [Grothendieck 1957] he wrote to Serre "I am rid of my horror of spectral sequences" [Colmez & Serre 2001, p. 7]. The whole point of spectral sequences is to let you calculate in an orderly way passing over many details of the objects. The Abelian category axioms give the general spectral sequence theorems while positing no details of the objects at all. And Grothendieck derived most of the specific important spectral sequences as special cases of his "delectable and useful general spectral sequence" [Colmez & Serre 2001, p. 14] today called the *Grothendieck spectral sequence.* Besides [Grothendieck 1957], see [Eisenbud 1995, Exercise A3.50].

In effect, a few pages of definitions of sheaves, resolutions, and derived functors, from the Séminaire Cartan (or from Serre's dissertation) were simplified into 102 pages of category theory. Many people found the work completely disproportionate to the problem. It took two years to find a publisher—though this legend may be a bit overstated. Eilenberg was ready to put it in the *Transactions of the* AMS in 1956 subject to what Grothendieck called "severe editorial taboos". Grothendieck said he would do it only if someone else would retype the manuscript (letter of Grothendieck to Serre of 19 September 1956, [Colmez & Serre 2001, p. 45]). Anyway Abelian categories became and remain the standard setting for (co-)homology theories.

This was a major step. In Deligne's words: "Grothendieck had shown that, given a category of sheaves, a notion of cohomology groups results" [Deligne 1998, p. 16]. And he had radically redefined what a category of sheaves was: It was now any Abelian category with a generator and enough injectives. It remained to find which Abelian categories give the Weil cohomology.

The Larger Vision

Grothendieck never mentions Abelian categories by name in *Récoltes et Semailles* [Grothendieck 1985–1987]. He focusses on more controversial ideas. He does cite *Tôhoku* [Grothendieck 1957] in an enlightening way: as an explanation of toposes. He describes a topos as a kind of space. In this sense the category of sets is a one-point space:

> A "space in the *nouveau style*" (or *topos*), generalizing traditional topological spaces, is given by a "category" which, without necessarily coming from an ordinary space, nonetheless has all the good properties (explicitly designated once and for all, of course) of such a "category of sheaves".[17] [Grothendieck 1985–1987, p. P39]

He explains the good properties are "above all the properties I introduced into category theory under the name 'exactness properties'" in [Grothendieck 1957]. We must be clear: the specific properties of a topos are very different from those of an Abelian category. But both are defined in part by exactness properties.

This is the really deep simplification Grothendieck proposed. The way to understand a mathematical problem is to express it in the mathematical world natural to it—that is, in the topos natural to it. Each topos has a natural cohomology, simply taking the category of Abelian groups in that topos as the category of "sheaves". With luck the cohomology of that topos may solve your problem. I would outline this:

(1) Find the natural world for the problem (e.g. the étale topos of an arithmetic scheme).
(2) Express your problem cohomologically (state Weil's conjectures as a Lefschetz fixed point theorem).
(3) The cohomology of that world may solve your problem, like a ripe avocado bursts in your hand.

In Grothendieck's words:

> 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 ("childish" in their simplicity!) seemed to

[17]Here a categories of sheaves means sheaves of sets. I have mostly referred to sheaves of groups.

> 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. [Grothendieck 1985–1987, p. P41]

The unity sought in the Séminaire Cartan is now complete: Cohomology gives algebraic invariants of a topos, just as it used to give invariants of a topological space. Each topological space determines a topos with the sheaf cohomology. Each group determines a topos with the group cohomology.[18] The same, Grothendieck knew, would work for cases yet unimagined. He notes that *Tôhoku* [Grothendieck 1957] already gave foundations for the cohomology of any topos [Grothendieck 1985–1987, p. P41n.]. That context was hardly foreseen as he wrote *Tôhoku* in 1955. This is one more proof that it was the right idea of cohomology.

For the Weil conjectures it only remained to find the natural topos for each arithmetic space—recalling that up to 1956 or so the spaces themselves were not adequately defined. In fact this conception of "toposes" came to Grothendieck as the way to combine his theory of schemes with Serre's idea of isotrivial covers and produce the cohomology [Grothendieck 1985–1987, p. P31 and passim].[19]

Earlier Schemes

On Grothendieck's own view there should be almost no history of schemes, but only a history of the resistance to them:

> The very idea of scheme is of infantile simplicity—so simple, so humble, that no one before me thought of stooping so low. So childish, in short, that for years, despite all the evidence, for many of my erudite colleagues, it was really "not serious"! [Grothendieck 1985–1987, p. P32]

The idea is that simple. But at least two others did think of things rather like it. Neither carried it through. We can look at how and why they thought of it and some reasons why they would drop it.

At its most basic, algebraic geometry studies *varieties* or spaces defined by polynomial equations, with polynomial coordinate functions on them. One stock example will be the complex number plane $\mathbb{C}^2$, where the coordinate functions are all polynomials $P(x, y)$, with complex number coefficients, and variables x, y over the complex numbers. So the coordinate ring is the ring $\mathbb{C}[x, y]$ of all these polynomials. The classical points of $\mathbb{C}^2$ are the pairs $< \alpha, \beta >$ of complex numbers.

Our other stock example will be the unit circle in $\mathbb{C}^2$ defined by the equation $x^2 + y^2 = 1$. So its classical points are the pairs of complex numbers $< \alpha, \beta >$

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[18]Following another lead Grothendieck would have taken from Serre, each profinite group determines a topos with the Galois cohomology.

[19]Deligne's 1972 proof, completing the Weil conjectures, did not follow this course as simply as Grothendieck hoped. Weil's proposed trivial calculation assumed cohomology with ordinary integer coefficients. But étale cohomology gives p-adic integer coefficients. These include the ordinary integers and more. It takes some proof to show the coefficients in the polynomials of interest are in fact ordinary integers. Grothendieck [Grothendieck 1969] conjectured general theorems on étale cohomology to bridge the gap, called the *standard conjectures*. See also [Kleiman 1994]. No one to date has proved them. Deligne [Deligne 1974] instead gave a wide ranging, elegant but difficult geometric argument. See also [Mumford & Tate 1978] and the review by Nicholas Katz in *Mathematical Reviews* **49** #5013. Deligne, Serre, and others have worked further on Grothendieck's strategy, especially on *motives*. Vladimir Voevodsky's 2002 Fields Medal is related to this [of Mathematicians forthcoming, pages not yet known].

with $\alpha^2 + \beta^2 = 1$. More algebraically we define it by the *ideal* in $\mathbb{C}[x, y]$ of all polynomials divisible by the polynomial $x^2 + y^2 - 1$. In other words that is the ideal of all polynomials which are 0 all over the unit circle. The coordinate ring is the quotient of the ring $\mathbb{C}[x, y]$ by that ideal. In other words a coordinate function on the unit circle is any complex polynomial in x, y, regarding polynomials as equal if their difference is divisible by $x^2 + y^2 - 1$, that is if they take equal values at each point of the circle.

The Italian algebraic geometers made deep and subtle use of *generic points* of a variety, meaning points with no special properties, so that anything proved of a generic point would be true of all except maybe some exceptional points on that variety. Bartel van der Waerden set out to make this more precise. He found an answer based on published ideas of Emmy Noether and then learned she had given that same answer in unpublished lectures (see [van der Waerden 1971]). On this view the generic points are no longer the typical classical points. They are something other than the classical points. For example the unit circle in $\mathbb{C}[x, y]$ has a "generic point" which somehow lies over all of the classical points $< \alpha, \beta >$ with $\alpha^2 + \beta^2 = 1$. David Mumford's famous lecture notes on schemes from the 1960s (reprinted as [Mumford 1988]) draw such a point as a blur spread out over the circle. Anything true of the generic point, is true of nearly all the classical points, if you state it correctly.

We do not need the formal details from [van der Waerden 1926].[20] We only need to know that on this approach each subvariety of a variety V has a generic point. Subvarieties of V correspond to prime ideals in the coordinate ring on V. So generic points correspond to prime ideals. Each classical point of V is itself a subvariety of V and so has a generic point.

From Emmy Noether's viewpoint, then, it was natural to look at prime ideals instead of classical and generic points—or, as we would more likely say today, to identify points with prime ideals. Her associate Wolfgang Krull did this. He gave a lecture in Paris before the Second World War on algebraic geometry taking all prime ideals as points, and using a Zariski topology (for which see any current textbook on algebraic geometry). He did this over any ring, not only polynomial rings like $\mathbb{C}[x, y]$.[21] The generality was obvious from the Noether viewpoint, since all the properties needed for the definition are common to all rings. The expert audience laughed at him and he abandoned the idea. The story is in [Neukirch 1999, p. 49].[22]

Weil made a more systematic treatment of van der Waerden's generic points basic to his *Foundations of Algebraic Geometry* [Weil 1946]. Weil's influence, as well as the technical reasons for using generic points, made generic points a great concern in Parisian algebraic geometry in the 1950s. So when Serre wrote an influential rival to Weil foundations, not using generic points, it is no surprise that people

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[20]The generic point of a variety V over a field k is the n-tuple $< \xi_1, \ldots, \xi_n >$ of images of the generators $x_1, \ldots x_n$ of the polynomial ring $k[x_1, \ldots x_n]$, under the natural homomorphism of $k[x_1, \ldots x_n]$ onto its "determined up to isomorphism" quotient field, by the prime ideal defining V [van der Waerden 1926, pp. 192, 197]. The images of $x_1, \ldots x_n$ in quotients by larger prime ideals are *specializations* of the generic point, and the images under maximal ideals correspond to the classical points of the variety.

[21]In this paper all rings are commutative with unit.

[22]Thanks to Norbert Schappacher for telling me of this.

thought about how to add them in. Everyone knew they corresponded to prime ideals.

The surprise was how easily they fit in, indeed using just the properties of any ring. So Pierre Cartier tells us:

> Martineau remarked to [Serre] that his arguments remained valid for any commutative ring, provided one takes all prime ideals instead of only maximal ideals. I then proposed a definition of schemes equivalent to the definition of Grothendieck. In my dissertation I confined myself to a framework similar to that of Chevalley, so as to avoid an excessively long exposition of the preliminaries! [Cartier 2001, p. 398]

We will see the published part of this in more detail in the next section. Grothendieck and Jean Dieudonné say "Serre himself has remarked that the cohomology theory of algebraic varieties could be transcribed with no difficulty . . . to any commutative ring" [Grothendieck & Dieudonné 1960, p. 7].

Why did Krull abandon his idea, and Cartier judge his excessively long, while Grothendieck went on? Well Krull was a foreigner in Paris. Perhaps the audience laughed harder at him than Bourbaki would at Grothendieck. Perhaps Cartier would have come back to it if Grothendieck had not taken it over.

More importantly, Krull's motivation for the general theory seems to have been that "it was there". When he put the definitions of point and subvariety in the simplest terms, they applied to any ring, not just rings of polynomials, so he gave them in that generality. Weil would later prove hard theorems which needed generic points and a more penetrating theory than [Weil 1946] really provided. Perhaps the general theory could survive only when it had at least that much work to do.

Krull and Cartier both missed the crucial tool of sheaves, which Serre brought to algebraic geometry in FAC. The efficiency of sheaves, the easy way they allow pasting varieties together, and their facility for cohomology theories, made Grothendieck say FAC had "the principle of the right definition" [Grothendieck 1958, p. 106]. Plus, they and Serre all worked without the category theory Grothendieck would use so heavily. Grothendieck and Dieudonné say this explicitly on one issue central to the success 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)" [Grothendieck & Dieudonné 1971, p. 6].

Schemes in Paris

There is no serious historical question of how Grothendieck found his definition of schemes. It was in the air. Serre has well said that no one invented schemes (conversation 1995). The question is, what made Grothendieck believe he should use this definition to simplify an 80 page paper by Serre into some 1000 pages of *Éléments de Géométrie Algébrique*?

There is a story that in some café in Paris in the 1950s Grothendieck asked his friends "what is a 'scheme'?" Compare the story of Hilbert asking John von Neumann "but what is a Hilbert space, really?". The German is more to the point: "was ist aber eigentlich ein Hilbertscher raum?"[23] Apparently von Neumann had

[23] To be painfully literal: "what, I ask, is in own-ness a Hilbert space?"

given the axioms, some examples, and some theorems. Hilbert wanted the idea behind it all. Grothendieck's question was quite different. In Paris at the time there was only an idea behind "schemes", and no accepted definition or axioms.

We have seen Weil wanted algebraic geometry over each finite field $\mathbb{F}_q$. This was not officially sanctioned at the time since finite fields are never algebraically closed. Algebraic geometry before Weil worked with polynomials with complex number coefficients, and points with complex coordinates, or more generally with coefficients and coordinates in an algebraically closed field K. No big problem so far. We can use the algebraic closure of each $\mathbb{F}_q$. In fact, all the $\mathbb{F}_{p^r}$ together form the algebraic closure of $\mathbb{F}_p$.

The challenge was precisely that Weil's goal was number theory, the arithmetic of the ordinary integers $\mathbb{Z}$. He wanted an "algebraic geometry over the integers", following Leopold Kronecker, as he urged in [Weil 1952]. He wanted to think of an integer polynomial $P(X, Y, ..Z)$ (or a list of polynomials) as defining a space over the integers. Looking at the polynomial modulo a prime p would define a *specialization* of that space to one defined over $\mathbb{F}_p$.[24] When Grothendieck described schemes in *Récoltes et Semailles* in 1986 he focussed on this aspect. Essentially all he says to describe them is that a single scheme can combine "in one magical fan (*éventail magique*[25])" what had been separate varieties over each $\mathbb{F}_p$ and over the rationals [Grothendieck 1985–1987, p. P32].

Weil did much to make geometry work over any field. So he could work with a variety over the rationals, or better over the complex numbers, and then try to find just the points with integer coefficients. But those points were not geometrically distinguished. Arithmetic was a further structure beyond the geometry. It was quite backwards from Kronecker's viewpoint (e.g. [**?**]), and so from Weil's, to start with the complex numbers and then treat rationality and integrality as additional structure.

So an undefined idea of "schéma" was current in Paris. It meant more or less the best generalization or reformulation of the then dominant "Weil foundations". Otto Schilling's enthusiastic but awestruck review of Weil's book [Weil 1946] in *Mathematical Reviews* (**9** #303c in 1946) will dispel any thought that the ideas were more accessible and naturally geometric before Grothendieck.[26] Weil defined *affine varieties* over any field k by taking an algebraically closed infinite transcendence degree extension $k \subset K$ and regarding certain n-tuples $< x_1, \ldots x_n >$ in K^n as generic points of *varieties* on k which can specialize to varieties over other fields .... The definition is distributed over several chapters [Weil 1946, notably pages 26 and 68]. Then you get abstract varieties by pasting together affine varieties.

[24]In effect Weil wanted geometry over any extension of any quotient of the integers and that means over any ring. By 1979 he says "the natural evolution of the subject" led that way. He says it was largely achieved by Goro Shimura and "above all by the theory of schemes as created by Grothendieck and developed by his students and successors" [Weil 1979, vol.1, p. 576].

[25]The image is of an oriental fan, that collapses to a rod and spreads in your hand. In some stories a magical fan works as a magic wand, or can extinguish fires, or set them. When a stage magician fans out a deck of cards, for someone to pick one, this is also an *éventail magique.*

[26]Anyone who thinks the easy intuitive days came before that, with the Italians, should consult Oscar Zariski [Zariski 1935], noted then and now for exceptional clarity on that approach, written before Zariski converted to commutative algebra. To say the Italians worked very intuitively does not mean their intuitions were easily gained.

There were two leading contenders against Weil foundations by 1956. One was Serre [Serre 1955] generally cited as FAC. Serre did not use the word "scheme". We will return to FAC. The other was "the Chevalley-Nagata theory of schemes" (and a variant of it by Pierre Cartier which he says "closely follows the exposition in Serre [FAC] only avoiding the use of sheaves" [Cartier 1956, p. 1-01].[27] Cartier published this theory in the Séminaire Chevalley, sessions of 5 and 12 November 1956. Grothendieck gave four sessions in the same seminar, from 26 November to 14 January 1957, on algebraic geometry, not mentioning "schemes".

Cartier defines a *spectrum* Ω_A for each finite type algebra A over a field k, with a Zariski topology. This comes close to Grothendieck's sense of "spectrum".[28] The elements of the algebra A are construed as "functions" from the spectrum to a field extension of k as in current scheme theory. Cartier parallels Serre's use of sheaves when he defines "algebraic sets" by pasting together spectra. He proves various theorems familiar to anyone who knows current scheme theory, though with the restriction that his spectra are all of finite type over a field. His axiom "EA1" for an algebraic set requires a finite cover by spectra, while "EA2" is the current definition of a *separated* scheme.

An "affine scheme", in Chevalley-Nagata's sense [Cartier 1956, p. 2-18] amounts to what Cartier called the spectrum of a semi-simple algebra A, only reworded in ideal-theoretic terms. That makes a difference. This is Grothendieck's spectrum.[29] Chevalley schemes are gotten by pasting together affines. So they are schemes in Grothendieck's sense, with all the apparatus for the general case, but not stated in all generality. This published record well supports what Cartier says [Cartier 2001, p. 398, quoted above] about anticipating Grothendieck's theory of schemes.

The key feature of FAC, though, is the idea of *structure sheaves*. An affine variety in FAC does not just have a ring of coordinate functions but is equipped with a Zariski topology and a sheaf of rings—each open subset of the variety has a ring of coordinate functions over it. Indeed this sheaf is entirely determined by the coordinate ring on the whole variety. Roughly, a coordinate function over an open subset $U \subseteq V$ is any fraction f/g where f and g are coordinate functions on V, and for all $p \in U$, $g(p) \neq 0$. So $1/x$ is not defined as a function on the whole real line $\mathbb{R}$, but is defined on the open subset $\{x \in \mathbb{R} |\ x \neq 0\}$. For later comparison, note all of these functions are actually functions in the set-theoretic sense.

The structure sheaf on an affine variety gives no more information than a single coordinate ring but it serves two purposes. It gives more uniform means of pasting affine varieties together than Weil had done in [Weil 1946]. Roughly: where Weil would speak of several spaces and a way to paste them together, Serre could

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[27] Cartier has no citation for Claude Chevalley or Masayoshi Nagata. A search of *Mathemtical Reviews* suggests they never published using the term "scheme". Grothendieck mentions Chevalley, Nagata, and Cartier in [Grothendieck 1957, p. 161], and [Grothendieck 1962, p. 190-01].

[28] Exposé 1 defines Ω_A as the set of algebra homomorphisms from A to an algebraically closed extension K of k. Different extensions give different spectra. If K is the algebraic closure then (modulo the Galois group of K over k) Ω_A amounts to the *maximal spectrum* or set of maximal ideals of A. This is the case in Serre [Serre 1955]. If we assume, as in Weil [Weil 1946], that K has infinite transcendence degree over k then Ω_A agrees (again, modulo the Galois group) with the spectrum in Grothendieck's sense of all prime ideals of A. Since the definition of scheme in Exposé 2 uses prime ideals of A, rather than homomorphisms to a specified K, the Galois group disappears and we get the spectrum, and schemes, in Grothendieck's sense.

[29] With the trivial difference of using local rings contained in the field of quotients of an algebra rather than prime ideals of the algebra.

speak of a single structure sheaf gotten by pasting other structure sheaves together. And it presents the algebraic information in just the form needed for cohomology. Serre's FAC produced a cohomology for varieties, now a standard tool in algebraic geometry, called *coherent cohomology.* Serre suggested a possible application to the Weil conjectures [Serre 1955, p. 233], in defining Betti numbers of varieties. But he knew this could not actually be the Weil cohomology because it could not give an adequate Lefschetz fixed point theorem.[30] Today coherent cohomology is generally given in Grothendieck's form as a derived functor cohomology on any scheme, as in [Hartshorne 1977].[31]

Each Serre variety V is defined over some algebraically closed field K and the coordinate functions take values in K. More than that, V must be pasted together from parts each of which is (isomorphic to) some locally closed subset of the affine space K^n with the Zariski topology. This sharply limits which rings can be coordinate rings.[32]

Grothendieck took the simple route through all this apparatus: Every ring R defines a scheme, called the *spectrum* of R or Spec(R). Points are prime ideals. Every ideal of R gives a closed set, i.e. Spec(R) has the Zariski topology. Schemes in general come from pasting together spectra. Everyone in the best circles of Parisian mathematics knew this was the way to give a geometry for every ring. At the 1958 International Congress of Mathematicians Grothendieck called this topology on the set of prime ideals of any ring "classical" [Grothendieck 1958, p. 106]. The next year in the Séminaire Bourbaki he called it "well known" [Grothendieck 1962, p. 182-01].

Grothendieck's originality, according to Serre (conversation 1995), was that no one but him thought it could work in all generality. Serre thought the rings "should meet some conditions, at least be Noetherian".[33] For Grothendieck, as the correct definition of cohomology applies to every topological space, so the right definition of scheme would apply to every ring. Serre had already shown (without drawing the conclusion in print) his cohomology works in this generality.

Famously Grothendieck wrote the work up in collaboration with Jean Dieudonné. In the original IHES edition they were leery of history in two senses. They find any historical sketch of the idea "beyond our competence" [Grothendieck & Dieudonné 1960, p. 7]. And they warn that prior knowledge of algebraic geometry, "despite

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[30]Specifically, applied to a variety over any $\mathbb{F}_{p^r}$ or its algebraic closure, the coefficients have characteristic p, so this cohomology could at best count fixed points modulo p.

[31]Serre used a sheaf version of Čech cohomology. This agrees with the derived functor cohomology, because of a theorem Serre proved in FAC: affine varieties have vanishing cohomology. And the theorem holds for all affine schemes. Grothendieck says "this should be considered an accidental phenomenon" and "it is important for technical reasons not to take as *definition* of cohomology the Čech cohomology" [Grothendieck 1958, p. 108]. His point was, the simple generalities of derived functor cohomology should come prior to the substantial theorem on cohomology of affines. The Čech method remains a tool for calculations in derived functor cohomology.

[32]The coordinate rings on affine parts are integral algebras of finite type over K. They differ from Cartier's algebras notably, for our purposes, in allowing no nilpotents, no elements $x \neq 0$ such that $x^n = 0$ for some n.

[33]Indeed many geometric results need further assumptions, but [Grothendieck & Dieudonné 1960] or [Grothendieck & Dieudonné 1971] or Hartshorne [Hartshorne 1977] show the advantages of the simple definition. An uncanny amount of geometric intuition is directly expressed over any ring. The later restrictions are visibly irrelevant to the first steps—that is, now visibly, but not to most geometers in the 1950s.

its obvious advantages, can sometimes (by the too exclusive habituation to the birational viewpoint implied in it) cause problems for those who wish familiarity with the viewpoint and techniques given here" [Grothendieck & Dieudonné 1960, p. 5].

Yet they close the Introduction with some historic perspective:

> To conclude, we believe it helpful to warn readers that, like the authors themselves, they will no doubt have some trouble before they are accustomed to the language of schemes and before they convince themselves that the usual constructions suggested by geometric intuition can be transcribed, in essentially just one reasonable way, into this language. As in many parts of modern mathematics, the initial intuition, seemingly draws farther and farther away from the language suited to expressing it in all the desired precision and generality. In the present case, the psychological difficulty is in transporting notions familiar from sets into the objects of rather different categories (that is, the category of schemes or of schemes over a given scheme): cartesian products, the laws of a group or a ring or module, fiber bundles, principle homogeneous fiber bundles, et c. No doubt it will be difficult for future mathematicians to do without this new effort at abstraction, which is perhaps quite small, compared to that our fathers faced, familiarizing themselves with set theory.[34] [Grothendieck & Dieudonné 1960, p. 9]

By 1971 in the Springer-Verlag edition they claim their own historical heritage, tracing the basic idea back a hundred years to Dedekind and Heinrich Weber [Grothendieck & Dieudonné 1971, p. 11]. They apparently mean Dedekind and Weber's work, extending a line of thought in Riemann, where a curve $\mathcal{C}$ over the complex numbers is studied via the algebra $\mathrm{M}(\mathcal{C})$ of meromorphic functions on that curve. On this approach the ring (indeed, field) $\mathrm{M}(\mathcal{C})$ of functions is the basic object and the "points" of the curve are defined algebraically from it.

They also signal some evolution in thinking about categories. They drop the warning about it and add a section on algebraic structures in categories taken from [Grothendieck & Dieudonné 1961]. Twenty five years after that Deligne wrote: "if the decision to let every commutative ring define a scheme gives standing to bizarre *schemes*, allowing it gives a *category of* schemes with nice properties" (Deligne's emphasis, [Deligne 1998, p. 13]). What is nice about categories of schemes is, among other things, that you can transport into them the various notions Grothendieck and Dieudonné listed.

Probably the greatest actual objection anyone had to scheme theory was based on a non-set-theoretic feature of schemes.[35] The elements of any ring R appear as coordinate functions on the spectrum $\mathrm{Spec}(R)$. Of course these are generally not functions in the set-theoretic sense. The scheme context makes them act rather like

[34] I translate "préschéma" as "scheme", since what [Grothendieck & Dieudonné 1960] calls "préschéma" is called "schéma" in [Grothendieck & Dieudonné 1971] and "scheme" in English. What was called "schéma" in [Grothendieck & Dieudonné 1960] is today called a "separated scheme".

[35] This has nothing to do with set theoretic foundations for mathematics. You can quite officially found scheme theory on Zermelo Fraenkel set theory. It will remain that the sections of the structure sheaf are treated as coordinate functions, inheriting that role from polynomials in earlier algebraic geometry, while they are not set theoretic functions.

set-theoretic functions. Each one can be evaluated at any point p of the scheme (taking values in the fiber of the scheme at that point, see any standard text on schemes). Yet one scandalous fact was: a "function" $g \in R$ may have $g(p) = 0$ at every point p of the scheme and yet not be the zero function. In geometric terms, this happens when the scheme has infinitesimal fringe around it, and g while 0 at each point has non-zero derivative in some directions through the fringe.[36]

A "function" in this sense is not determined by its values. "It is this aspect of schemes which was most scandalous when Grothendieck defined them" according to Mumford [Mumford 1966, p. 12].[37] But it is tremendously helpful, for example, in describing a singular point x of a scheme X. Looking at "arbitrarily small neighborhoods" of x is not helpful in the very coarse Zariski topology where no neighborhood is small. But you can define a subscheme of X containing just the point x and infinitesimal fringe around it. The contortions of X around x are retained in this fringe with no other complexities of the larger space X. You study those contortions, and thus the nature of the singularity, by looking at the derivatives of functions around x, while there are no other points but x.

Indeed, while a scheme has a set of points, that set is rarely the best handle on it. Deligne says:

> The audacity of Grothendieck's definition is to accept that *every* commutative ring A (with unit) has a scheme Spec(A) ... This has a price. The points of Spec(A) (prime ideals of A) have no ready to hand geometric sense ... When one needs to construct a scheme one generally does not begin by constructing the set of points. [Deligne 1998, p. 12]

Rather one constructs a scheme by its geometric relations to other schemes. The same could be said for some older notions of space and many newer ones. This is a theme of [Cartier 2001]. But none has yet drawn so much attention this way as schemes.

The nice categorical properties are a mess set theoretically. Grothendieck and Dieudonné's list deliberately highlights the issue. The cartesian product of schemes X and Y is the usual product of category theory. It is a scheme $X \times Y$ with projection morphisms $p_1 : X \times Y \to X$ and $p_2 : X \times Y \to Y$ with the familiar property. A morphism from any scheme T to $X \times Y$ is given by a pair $< f, g >: T \to X \times Y$ of morphisms $f : T \to X$ and $g : T \to Y$. But the set of points of $X \times Y$ is nothing like the set theoretic cartesian product of the sets of points of X and Y. It can happen that X and Y each have points, while $X \times Y$ is empty.[38] Or X and Y may each have a single point while $X \times Y$ has many.[39]

In sets, a group is a set G with a binary operation $G \times G \to G$ and a group inverse function $G \to G$ which satisfy certain equations, i.e. they make certain diagrams commutate. This description transposes to schemes: A group scheme is a scheme X with scheme morphisms $X \times X \to X$ and $X \to X$ making the same diagrams commute. Thus the slogan "a group scheme is just a group object in

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[36]In algebraic terms, it happens when $g \neq 0$ is nilpotent and so belongs to every prime ideal.

[37]Mumford has told me Oscar Zariski was particularly put off by this. A scheme theoretic proof of Zariski's Main Theorem was one of the first ways Mumford made him waver.

[38]E.g. if X and Y are schemes over two fields with different characteristics.

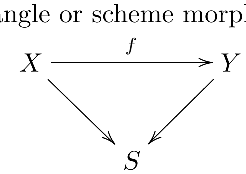
[39]E.g. if $X = Y$ is the spectrum of a field k then $X \times Y$ has as many points as the Galois group of k over its prime field.

the category of schemes". The set theoretic picture inherits the complications of products, aggravated by the binary operation.

Another issue was probably decisive in the success of scheme theory. Classically there were two different ways a variety V could be "over" something. It could be *defined over* a field k, meaning roughly that it is defined by polynomials with coefficients in k, and the coordinates of its points lie in k. Or it could *vary over* a parameter space P. So for example a polynomial $x^2 + \alpha xy + \beta y^2$ in two variables x, y, with complex parameters $\alpha, \beta \in \mathbb{C}$ defines a conic section varying over the complex plane $\mathbb{C}^2$. When $\alpha = 1 = \beta$ it is the conic $x^2 + xy + y^2 = 0$. When $\alpha = 2$ and $\beta = 1$ it is the degenerate conic $(x + y)^2 = 0$. Weil [Weil 1952] urged a kind of unification of these (I do not claim he was the first).

In scheme theory they are simply the same thing. To say a scheme X is defined over a field k is just to say it has a scheme morphism $X \rightarrow S$ where $S = \mathrm{Spec}(k)$. To say a scheme X varies over the complex plane $\mathbb{C}^2$, for example, is just to say it has a scheme morphism $X \rightarrow S$ where $S = \mathbb{C}^2$.

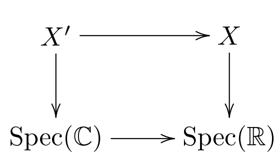
Grothendieck often treats a scheme morphism $X \rightarrow S$ as a single scheme or more precisely a *relative scheme* over the base S. A morphism over S to another relative scheme $Y \rightarrow S$ is a commuting triangle or scheme morphisms:



This could be a morphism of schemes defined over a field k, preserving the coefficients in k. Or it could be a morphism between families X, Y of schemes over, say the complex plane $S = \mathbb{C}^2$, so that the scheme in X with given parameters $< \alpha, \beta >$ is mapped to the scheme in Y with the same parameters.

Grothendieck would treat $X \rightarrow S$ as a scheme. He could largely ignore the coefficients or the parameters, let them take care of themselves, because the category of schemes over any base scheme S is very much like the category of schemes *per se*, though with some specific differences reflecting the algebraic geometry of S. Demazure and Grothendieck [Demazure & Grothendieck 1970, vol.1, p. VIII] note the advantages of this, and infinitesimal fringe, to group schemes. You can treat a parameterized family of group schemes as a single group. And a group scheme over a base scheme S with just one point plus infinitesimal fringe, is a family of infinitesimal deformations of one group scheme.

Relative schemes produce the simple and general functorial account of base change that Grothendieck and Dieudonné mentioned [Grothendieck & Dieudonné 1971, p. 6, quoted above]. For example you might have schemes over the real numbers $\mathbb{R}$ and want to focus on their complex number points. That means taking each scheme $X \rightarrow \mathrm{Spec}(\mathbb{R})$ to a scheme $X' \rightarrow \mathrm{Spec}(\mathbb{C})$. There is a scheme map $\mathrm{Spec}(\mathbb{C}) \rightarrow \mathrm{Spec}(\mathbb{R})$ and the base change is just the pullback



in the category of schemes. The same works for any extension field $k \subseteq K$ and of course even more generally than that.

Or you might have a family of schemes varying over the complex plane $\mathbb{C}^2$ and want to look at just the ones that lie over the unit circle S^1 in that plane. There is a scheme map $S^1 \to \mathbb{C}^2$. You restrict a relative scheme $X \to \mathbb{C}^2$ to its part $X' \to S^1$ by taking the pullback

$$\begin{array}{ccc} X' & \longrightarrow & X \\ \downarrow & & \downarrow \\ S^1 & \longrightarrow & \mathbb{C}^2 \end{array}$$

in the category of schemes.

The categorical formalism could not be simpler. The particulars of some given change of base can be quite hard, and solve a genuinely hard problem in geometry or algebra. Descending far into the set theoretic details is a mess.

Grothendieck came to think of schemes more and more functorially. By the mid 1960's he found that "to obtain a language that 'sticks' easily to geometric intuition, and avoid eventually insupportable circumlocutions" he should identify each scheme (or, relative scheme over a base S) with its *representable functor* from the category of schemes (resp. relative schemes over S) to the category of sets [Demazure & Grothendieck 1970, vol.1, p. VI]. In effect, you identify each scheme with the diagram of all morphisms to it from other schemes. Later he began to stress that you do not need the whole category of schemes but can use its restriction to the category of affine schemes—i.e. to the (dual to the) category of rings. You can define a scheme as a suitable functor to sets from the category of rings. See [Grothendieck & Dieudonné 1971,]. That is beyond this paper.

Towards the Séminaire de Géometrie Algébrique

We have watched the "sea advance insensibly in silence. . . so far off you hardly hear it" [Grothendieck 1985–1987, p. 552] towards the Weil conjectures, up to about 1958. By the way in 1957 Grothendieck found the Grothendieck-Riemann-Roch theorem, but left it to Armand Borel and Serre to publish a proof [Borel & Serre 1958]. Raoul Bott's extremely helpful *Mathematical Reviews* review (**22**, #6817) notes "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". Grothendieck says this theorem "made me a 'big star' overnight", first dispelled Bourbaki's doubts about him, and was his claim to fame at the 1958 International Congress of Mathematicians where he first became "somewhat feared" by other mathematicians [Grothendieck 1985–1987, pp. P23, 705, 32]. He says he proved it by "the rising sea", even though it was not a question of his own making, and he says Serre put him onto it [Grothendieck 1985–1987, pp. 554-5]. We have seen not one step dealing specifically with the Weil conjectures.

Serre made a crucial step in 1958, actually producing the 1-dimensional Weil cohomology groups, using *isotrivial coverings*. See [Serre 1958] which cites [Grothendieck 1958] for scheme theory. Grothendieck was at the talk and immediately told Serre this would produce the cohomology in all dimensions. Serre was "absolutely unconvinced" since he felt he had "brutally forced" the maps to yield the H^1s. "But

Grothendieck was always an optimist in those days" (conversation, fall 1995).[40] The expanded print version of Serre's talk appeared a few months later. By then Grothendieck had showed him it indeed gives a cohomology in all dimensions, and convinced him this was likely the "true cohomology needed to prove the Weil conjectures" [Serre 1958, p. 125], compare [Grothendieck 1958, p. 104]. Again, this leads beyond the scope of this paper.

Grothendieck's optimism grew from his method: Cohomology is uniquely determined, once you know *what* you want the cohomology *of.* Serre had found the Weil conjectures need the cohomology of isotrivial covers (soon modified to étale covers). So the job was finished in principle—from Grothendieck's viewpoint—but he did not rush to work it all out. That would be striking hard at the chisel. Rather he conceived a larger framework to embrace at once spaces, their sheaves, and cohomology. Technically this framework is all about the idea of "covering". Conceptually it is all about transporting geometric ideas into new categories. It first appeared as Grothendieck topology: "the technical, provisional form of the crucial notion of *topos*" [Grothendieck 1985–1987, p. P24].

All is building worlds. To understand a sheaf, look at a world as big as the universe of all sets: the category of all sheaves of that type—"the way it appears so to speak 'right in front of your nose'" [Grothendieck 1985–1987, p. P38]. To find the right kind of sheaves for a new problem, find another kind of equally large world: find the right topos. The "sheaves" you want will be simply the Abelian groups in that world. To work with a scheme, look at it in the category of all schemes—or, more likely, build from that a suitable category of relative schemes. Into each of these worlds transport familiar geometric constructions. The ideas would grow through the 1960s at Grothendieck's Séminaire de Géometrie Algébrique at the IHES along with his specific proofs of the first and second Weil conjectures in their full topological form.

Categorical world building is young. Abelian categories are standard in homology research today, though not yet common in textbooks. Grothendieck topology is entirely accepted, but not exactly standard, in research geometry, and rare in textbooks. Toposes are still widely avoided by geometers though the theory continues to grow (on the general theory, see the compendious [Johnstone 2002-]). Voevodsky's Fields Medal shows Grothendieck's largest vision is still progressing in algebraic geometry.

As to schemes, Grothendieck and Dieudonné focussed on the finally decisive point: "It is fitting to give algebraic geometry all desirable generality and suppleness by resting it on the notion of *scheme*" [Grothendieck & Dieudonné 1971, p. 1]. When Serre spoke at the Stockholm International Congress of Mathematicians in 1962, on algebraic geometry, he said "I must specify that I take this term in the sense it has had for several years now: the theory of schemes" [Serre 1963, p. 190].

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[40] Compare the note in [Serre 2001, p. 255]. There Serre mentions he specifically thought higher dimensional Weil cohomology would need to use higher homotopy groups.

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