

## CHAPTER 13

# The Rising Sea: Grothendieck on Simplicity and Generality

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In 1949, André Weil published striking conjectures linking number theory to topology and a striking strategy for a proof [Weil, 1949]. 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. Some have complained of this style, but Miles Reid, for example, says “Grothendieck himself can’t necessarily be blamed” for the growth of category theory “since his own use of categories was very successful in solving problems” [Reid, 1990, p. 116].<sup>1</sup> This chapter describes the methods Grothendieck made standard in algebraic geometry by 1958, pursuing the Weil conjectures: Abelian categories, derived functor cohomology, and schemes. It touches on Grothendieck topologies, toposes, and étale cohomology, which arose around 1958, to see how Grothendieck himself relates them to the earlier ideas.<sup>2</sup>

Grothendieck describes two styles in mathematics. When he thinks 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 goes on to say:<sup>3</sup>

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!

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<sup>1</sup>Grothendieck began using category theory in functional analysis; see, for example, [Grothendieck, 1952]. Dieudonné punned on the categorical idea of natural transformation to praise Grothendieck’s functional analysis as a “constant search for ‘natural’ definitions and ‘functorial’ properties” [Dieudonné, 1990, p. 2].

<sup>2</sup>For a recent survey of Grothendieck’s life, work, and influence, see [Jackson, 2004].

<sup>3</sup>The abbreviation *ReS* refers to the memoir [Grothendieck, 1985–1987]. Pierre Deligne points out that Grothendieck’s mastery of language in *Récoltes et Semailles* parallels the serious responsibility he took for naming new concepts (e-mail to the author, 13 May, 2003). All translations in this paper are my own.

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 [*ReS*, pp. 552-553].<sup>4</sup>

He writes of a "rising sea [la mer qui monte]," which in French most often means a rising tide but can mean other things.<sup>5</sup> The theorem is "submerged and dissolved by some more or less vast theory, going well beyond the results originally to be established" [*ReS*, p. 555].<sup>6</sup> Grothendieck says this is his approach as well as Bourbaki's, thus comparing his research with Bourbaki writing the *Elements of Mathematics*. He often writes as if research, exposition, and teaching are all the same.

Deligne describes a typical 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 [rien ne semble se passer et pourtant à la fin de l'exposé un théorème clairement non trivial est là]" [Deligne, 1998, p. 12]. Grothendieck makes this simplicity an extreme form of Cantor's stand: "the *essence* of *mathematics* lies precisely in its *freedom* [das *Wesen* der *Mathematik* liegt gerade in ihrer *Freiheit*]" [Cantor, 1932, p. 182]. Grothendieck claims the freedom not only to build a world of set theory for mathematics but also to build an entire world—as large as the universe of all sets—adapted to any single problem such as counting the solutions to a given polynomial equation.

Grothendieck describes himself as creating "new worlds [mondes nouveaux]," but he means what he elsewhere calls "building beautiful houses [belles maisons]," that is, framing theories and methods that become a heritage others can use.<sup>7</sup> He has certainly done that. So has Jean-Pierre Serre in a very different way. Grothendieck says Serre generally uses the hammer and chisel [*ReS*, p. 558]. He calls Serre "Super Yang" against his own "Yin"—but not at all in the sense of being

<sup>4</sup>"... la chair nourricière protégée par la coque ... on pose le tranchant du burin contre la coque, et on tape fort. Au besoin, on recommence en plusieurs endroits différents, jusqu'à ce que la coque se casse—et on est content. ... Je pourrais illustrer la deuxième approche, en gardant l'image de la noix qu'il s'agit d'ouvrir. La première parabole qui m'est venue à l'esprit tantôt, c'est qu'on plonge la noix dans un liquide émollient, de l'eau simplement pourquoi pas, de temps en temps on frotte pour qu'elle pénètre mieux, pour le reste on laisse faire le temps. La coque s'assouplit au fil des semaines et des mois—quand le temps est mûr, une pression de la main suffit, la coque s'ouvre comme celle d'un avocat mûr à point! Ou encore, on laisse mûrir la noix sous le soleil et sous la pluie et peut-être aussi sous les gelées de l'hiver. Quand le temps est mûr c'est une pousse délicate sortie de la substantifique chair qui aura percé la coque, comme en se jouant—ou pour mieux dire, la coque se sera ouverte d'elle-même, pour lui laisser passage.

L'image qui m'était venue il y a quelques semaines était différente encore, la chose inconnue qu'il s'agit de connaître m'apparaissait comme quelque étendue de terre ou de marnes compactes, réticente à se laisser pénétrer. ... La mer s'avance insensiblement et sans bruit. ... Pourtant elle finit par entourer la substance rétive."

<sup>5</sup>It can mean waves rising in a storm, or crashing against rocks, or a generally rising sea level as from global warming. The sometimes psychoanalytic tone of the memoir makes us notice the pun "l'amère qui monte", a rising bitterness. Wordplay on "la mer/l'amère/la mère (the mother)" is familiar in French.

<sup>6</sup>"... submergé et dissous par quelque plus ou moins vaste théorie, allant bien au delà des résultats qu'il était d'abord question d'établir."

<sup>7</sup>See [*ReS*, pp. 554 and P27, resp.].

heavy-handed—rather Serre is the “incarnation of elegance [l’incarnation justement de l’élégance]” [*ReS*, p. 969]. That is the difference. Serre cuts elegantly to an answer. Grothendieck creates truly massive multi-volume books with numerous coauthors, offering set-theoretically vast yet conceptually simple mathematical systems adapted to express the heart of each matter and to dissolve the problems.<sup>8</sup> This is the sense of world-building I mean.

Across the difference in working style, Grothendieck says that from 1955 to 1970 Serre was at the origin of most of his ideas [*ReS*, p. 982]. This includes *every* major step towards the Weil conjectures. Their collaboration is comparable to that of Richard Dedekind and Emmy Noether. One difference is that Serre and Grothendieck talked with each other. Another is that Dedekind and Noether shared much the same style. Such collaboration deserves more historical attention. The most important and challenging remark ever made about twentieth-century mathematics was Noether’s watchword “it is all already in Dedekind.”

### The Weil Conjectures

Solving a Diophantine equation, that is, finding integer or rational solutions to an integer polynomial, can be unapproachably difficult. Weil describes one indirect strategy in a letter dated 26 March, 1940 to his sister, the philosopher Simone Weil: first look for solutions in richer fields than the rationals, 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 [Weil, 1979, 1:252].

A solution modulo 5 to a polynomial equation  $P(X, Y, \dots, Z) = 0$  is a list of integers  $X, Y, \dots, Z$  making the value  $P(X, Y, \dots, Z)$  divisible by 5 or, in other words, equal to  $0 \pmod{5}$ . For example,  $X^2 + Y^2 - 3 = 0$  has no integer solutions. That is clear since  $X$  and  $Y$  would both have to be 0 or  $\pm 1$ , to keep their squares below 3, and no combination of those works. But it has solutions mod 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 mod  $p$ .

A polynomial equation  $P(X, Y, \dots, Z) = 0$  can be exhaustively checked for solutions mod  $p$ , by just checking  $p$  different values for each variable. Even if  $p$  is impractically large, equations are more manageable modulo  $p$ . Going farther, we might look at equations mod  $p$ , but ask how the number of solutions grows as

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<sup>8</sup>Deligne emphasizes (in an e-mail to the author, 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.

Grothendieck posited *universes*, now often called *Grothendieck universes*, as a set-theoretic fix to gain the conceptual unity of toposes over topologies [Artin, Grothendieck, and Verdier, 1972, pp. 185ff.]. Serre suggests Grothendieck got the idea from Dieudonné or Chevalley, who got it from earlier set-theorists (e-mail to the author, 21 June, 2004). But it was only a fix. He later faulted Bourbaki for focusing on set theory instead of simple categorical properties [*ReS*, p. PU22]. See below for the vision of unity in toposes, and categorical versus set-theoretic properties of schemes. William Lawvere and Myles Tierney’s elementary topos axioms sought to formalize the unity directly, with far less set-theoretic strength than universes [Lawvere, 1979].

we allow irrationals of higher degree as solutions—roots of quadratic polynomials, roots of cubic polynomials, and so on. In each case, there are still only finitely many different potential solutions to check. Doing this for all primes  $p$  and for all degrees  $n$  means looking for solutions in all finite fields, as in Weil's letter.

Answering this question in finite fields does not itself answer those about integer or rational solutions. It might help. It is interesting. And it has surprising applications such as planning efficient networks [Li, 1996]. Building on 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 points about finite fields are: for each prime number  $p$ , the integers mod  $p$  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, denoted  $\mathbb{F}_{p^r}$  or  $\mathbb{F}_q$  with  $q = p^r$ . This comes from  $\mathbb{F}_p$  by adjoining the roots of a degree  $r$  polynomial. So for any natural number  $s > 0$ , there is just one field with  $q^s$  elements, namely,  $\mathbb{F}_{p^{(r+s)}} = \mathbb{F}_{q^s}$ . These fields for all prime numbers  $p$  are all the finite fields. The union for all  $r$  of the  $\mathbb{F}_{p^r}$  is the algebraic closure  $\overline{\mathbb{F}_p}$ . The Frobenius morphism on the algebraic closure takes each  $x \in \overline{\mathbb{F}_p}$  to  $x^p$ . By Galois theory,  $\mathbb{F}_{p^r}$  consists of just the fixed points for the  $r$ th iterate of the Frobenius morphism, so  $x \in \mathbb{F}_{p^r}$  if and only if  $x = x^{p^r}$ .<sup>9</sup>

Take any nice enough  $n$ -dimensional space defined by polynomials on a finite field  $\mathbb{F}_q$ .<sup>10</sup> For each  $s \in \mathbb{N}$ , let  $N_s$  be the number of points defined in  $\mathbb{F}_{q^s}$ . Define the zeta function as an exponential

$$Z(t) = \exp \left( \sum_{s=1}^{\infty} N_s \frac{t^s}{s} \right).$$

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

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

for some integer polynomials  $P(t)$  and  $Q(t)$ . This means there are complex algebraic numbers  $a_1, \dots, a_i$  and  $b_1, \dots, b_j$ , with each algebraic conjugate of an  $a$  (respectively,  $b$ ) also an  $a$  (respectively,  $b$ ), such that for every  $s$ ,

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

The second conjecture is a functional equation:

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

This says for each  $a_i$  in the list of  $as$  (respectively,  $b_j$  in the list of  $bs$ ), the quotient  $q^n/a_i$  is also in the list (respectively,  $q^n/b_j$ ).

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  $0 \leq k \leq n$ . This puts bounds on the numbers  $N_s$ .

<sup>9</sup>See, for example, [Serre, 1973].

<sup>10</sup>More specifically, take a smooth projective variety over  $\mathbb{F}_q$  which lifts to some algebraic number ring.

Over it all is the conjectured link to topology. Since the polynomials are nice, they define a complex manifold  $M$ , a continuous space of complex number solutions. Since  $M$  is algebraically  $n$ -dimensional on the complex numbers, it is topologically  $2n$ -dimensional. Let  $B_0, B_1, \dots, B_{2n}$  be its Betti numbers so each  $B_k$  gives the number of topologically  $k$ -dimensional holes or handles on  $M$ . Then Weil conjectured that each polynomial  $P_k$  in the Riemann hypothesis has degree  $B_k$ , and the constant  $E$  in the functional equation is the Euler number of the manifold, namely, the alternating sum of the Betti numbers

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

It is a fascinating, astonishing link between finite arithmetic and the topology of continuous manifolds. The topology of  $M$  tells how many  $a$ s and  $b$ s there are with each absolute value. This implies useful approximations to the numbers  $N_s$ .

Special cases of these conjectures were known, and Weil proved more. All dealt with curves (algebraically 1-dimensional) or hypersurfaces (defined by a single polynomial). Proofs of these minus the topology make up five chapters in Ireland and Rosen's *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 and Rosen, 1992, p. 151].

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 theorems on topological manifolds would apply to the spaces of finite field solutions. The Lefschetz fixed point theorem would reduce the conjectures to textbook exercises in linear algebra—now literally exercises 24 and 25 of chapter fourteen in Serge Lang's *Algebra* [Lang, 1993, p. 570].<sup>11</sup>

The conjectures were the intuition of an encyclopedic mathematician, drawn to classical problems, skilled at calculation, and informed on the latest methods. The topological strategy was powerfully seductive but seriously remote from existing tools. Even Weil did not name Lefschetz or cohomology in his publication. Serre says that "[a]t that time, Weil was explaining things (in conversations) in terms of cohomology and Lefschetz's fixed point formula" yet "did not want to predict the existence" of a cohomology theory that would actually work. "Indeed, in 1949–50, nobody thought that it could be possible to apply topology" to geometry over finite fields.<sup>12</sup> Weil's arithmetic spaces were not even precisely defined.

### Abelian Categories

Serre gave a thoroughly *cohomological* turn to the conjectures. As Grothendieck put it, "[a]nyway 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."<sup>13</sup> Serre approached the problem through *sheaves*, a new method in topology. Grothendieck would later describe each

<sup>11</sup>See also [Houzel, 1994], [Dieudonné, 1988], [Mumford and Tate, 1978], [Hartshorne, 1997, Appendix C], and [Weil, 1979, 1:568; 2:180–188; 3:279–302].

<sup>12</sup>Jean-Pierre Serre in an e-mail to the author of 1 July, 2004.

<sup>13</sup>"C'est en termes cohomologiques, en tous cas, que Serre m'a expliqué les conjectures de Weil, vers les années 1955—et ce n'est qu'en ces termes qu'elles étaient susceptibles de

sheaf on a space  $T$  as a "meter stick" measuring  $T$ . The cohomology of a sheaf gives a very coarse summary of the information. In the best case, it highlights just what is needed. Certain sheaves on  $T$  produce the Betti numbers. If such "meter sticks" applied to Weil's arithmetic spaces, and proved standard topological theorems for them, 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 discouraging for the Weil conjectures because the topology of arithmetic spaces in the then-existing sense was pretty clearly hopeless for Weil's proof strategy.

At the École Normale Supérieure, Henri Cartan's seminar spent 1948–1949 and 1950–1951 working on sheaf cohomology. Serre, in his early twenties, ran some sessions. Grothendieck, two years younger, attended, perhaps only as an occasional visitor: "I sat in the 'Séminaire Cartan', as a stupefied witness to his discussions with Serre, loaded with 'Spectral Sequences' (brr!) and drawings (called 'diagrams') full of arrows covering the blackboard. It was the heroic age of the theory of 'sheaves' and 'carapaces' and of a whole arsenal whose sense entirely escaped me" [*ReS*, p. 19].<sup>14</sup> The seminar was aware of de Rham cohomology on differentiable manifolds, which related topology to differential analysis. It was easily expressed in terms of sheaves. During the time of the seminar, Cartan saw how to define sheaves on a complex analytic variety reflecting not only its topology but also complex analysis on it. He and Serre would develop this over the coming years [Fasanelli, 1981].

Perhaps Weil cohomology could use sheaves reflecting algebra, but the applications to differential analysis and complex analysis used sheaves and cohomology in the usual topological sense. Their innovation was only to find new sheaves capturing analytic or algebraic information. It was amazing how well that worked. It seemed smaller, however, than the innovation needed for the Weil conjectures.

The great challenge to the Séminaire Cartan in 1950–1951 was to relate the cohomology of topological spaces to the cohomology of groups. Instead of sheaves, the cohomology of a group  $G$  uses  $G$ -modules.<sup>15</sup> This was formally quite different from topology, yet it had grown from topology and was tightly tied to it by applications. Indeed, Samuel Eilenberg and Saunders Mac Lane had created category theory in large part simultaneously with group cohomology in an effort to explain both kinds of cohomology by clarifying the links between them. Eilenberg was in Paris that year at Cartan's invitation and joined the seminar. They aimed to find what was common to the two kinds of cohomology, and they found it in a pattern of functors.

A cohomology theory on a topological space  $X$  assigns each sheaf  $\mathcal{F}$  on  $X$  a series of Abelian groups  $H^n \mathcal{F}$ , and it assigns each sheaf map  $f: \mathcal{F} \rightarrow \mathcal{F}'$  a series

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ma "accrocher"... je ne suis pas sûr que personne d'autre que Serre et moi, pas même Weil si ça se trouve, avait seulement l'intime conviction que ça devait exister" [*ReS*, p. 840].

<sup>14</sup>"J'ai été l'hôte... du 'Séminaire Cartan', en témoin ébahi des discussions entre lui et Serre, à grands coups de 'Suites Spectrales' (brr!) et de dessins (appelés 'diagrammes') pleins de flèches recouvrant tout le tableau. C'était l'époque héroïque de la théorie des 'faisceaux', 'carapaces' et de tout un arsenal dont le sens m'échappait totalement." See the spectral sequences below. Carapaces were similar to sheaves [Cartan, 1948–].

<sup>15</sup>A  $G$ -module is an Abelian group  $M$  (which might also be a module over some ring) plus an action of  $G$  on  $M$ . See, for example, [Brown, 1982].

of group homomorphisms  $H^n f: H^n \mathcal{F} \rightarrow H^n \mathcal{F}'$ . Each  $H^n$  is a functor, the so-called  $n$ -dimensional cohomology functor, with properties we need not know in detail. A crucial one is that, for  $n > 0$ ,  $H^n \mathcal{F} = 0$ , for any *fine* sheaf  $\mathcal{F}$ , where a sheaf is *fine* if it meets a condition borrowed from differential geometry by way of Cartan's complex analytic geometry.<sup>16</sup>

The cohomology of a group  $G$  is a series of functors  $H^n$  from  $G$ -modules to Abelian groups. These functors have the same categorical properties as topological cohomology except that  $H^n M = 0$ , for  $n > 0$ , and for any *injective* module  $M$ . A  $G$ -module  $I$  is *injective* if, for every  $G$ -module inclusion  $N \hookrightarrow M$  and homomorphism  $f: N \rightarrow I$ , there is at least one  $g: M \rightarrow I$  making this commute

$$\begin{array}{ccc} N & \hookrightarrow & M \\ & \searrow f & \downarrow g \\ & & I \end{array}$$

The Séminaire took the analogy no further, and this became a serious problem. One great offshoot was the book *Homological Algebra* usually called “Cartan-Eilenberg.” The book never mentions Emmy Noether but opens by declaring victory for her outlook. “During the last decade,” it proclaimed, “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 and Eilenberg, 1956, p. v]. Yet, this unified account had to exclude its own beginnings.

They could treat the cohomology of several 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 others. Topological cohomology rested on completely different resolutions. In the search for Weil cohomology, this left two questions. What would Weil cohomology use in place of topological sheaves or  $G$ -modules? And what resolutions would it use for them?

Cartan and Eilenberg defined group cohomology as a *derived functor*, which, in turn, they defined using injective resolutions. So the cohomology of a topological space was not a derived functor in their sense. But a looser sense was current. Grothendieck wrote to Serre on 26 February, 1955:

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.<sup>17</sup> One gets the fundamental spectral sequences as special cases of delectable and useful general spectral sequences. But I am

<sup>16</sup>A sheaf is *fine* if it admits partitions of unity in the following sense: for every locally finite cover of  $X$  by open subsets  $U^i$ , there are endomorphisms  $\ell^i$  of  $\mathcal{F}$  such that: (1) for each  $i$ , the endomorphism  $\ell^i$  is zero outside of some closed set contained in  $U^i$ ; and (2) the sum  $\sum_i \ell^i$  is the identity. See [Cartan, 1948–, exp. 15].

<sup>17</sup>This is to say that Grothendieck looked at what is now called *effaceability* before injective sheaves. He saw that fine sheaves are acyclic, and each sheaf embeds in one of them [Colmez and Serre, 2001, p. 12]. This answers Serre's question below about which properties of fine sheaves are needed. It also explains why basically every kind of resolution that works at all, works the

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  $\geq 2$ . Besides this is probably all more or less explicit in Cartan-Eilenberg's book which I have not yet had the pleasure to see [Colmez and Serre, 2001, pp. 13-14].<sup>18</sup>

Here, he lays out the whole paper commonly called *Tôhoku* for the journal that published it [Grothendieck, 1957]. There are several issues. For one thing, fine resolutions do not work for all topological spaces. They only work for the paracompact spaces—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 problem for most of analysis. But it was discouraging for the Weil conjectures since non-trivial algebraic varieties are never Hausdorff.

Serre replied on 12 March, 1955 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. Cartan was aware of it and told Buchsbaum to work on it, but he seems not to have done it.<sup>19</sup> 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!) [Colmez and Serre, 2001, p. 15].<sup>20</sup>

So Grothendieck began rewriting Cartan-Eilenberg before seeing 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].

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same way: all give the (unique up to isomorphism) universal delta functor over the global section functor.

<sup>18</sup>“Je me suis aperçu qu'en formulant la théorie des foncteurs dérivés pour des catégories plus générales que les modules, on obtient à peu de frais en même temps la cohomologie des espaces à coefficients dans un faisceau .... L'existence résulte d'un critère général, les faisceaux fins joueront le rôle des modules 'injectifs'. On obtient aussi les suites spectrales fondamentales comme cas particuliers de délectables et utiles suites spectrales générales. Mais je ne suis pas encore sûr si tout marche aussi bien dans le cas d'un espace non séparé, et je me rapelle tes doutes sur l'existence d'une suite exact en cohomologie en dimensions  $\geq 2$ . D'ailleurs, probablement tout ça se trouve plus ou moins explicitement dans le bouquin Cartan-Eilenberg, que je n'ai pas encore eu l'heur de voir.

<sup>19</sup>David Buchsbaum's problem was not posed by Cartan (e-mail to the author from Buchsbaum, 1 June, 2003). Buchsbaum had given categorical axioms for derived functors using generalized injectives. Compare Theorems 5.1 in [Buchsbaum, 1955] and [Cartan and Eilenberg, 1956]. Then he tried to show that sheaves on any topological space have enough injectives. He sent Cartan an incomplete proof outline. Cartan encouraged him in it. Buchsbaum dropped it when he too noticed *effaceability*. He later found that a weaker condition suffices to define a cohomology functor. Roughly, each cocycle  $\alpha$  of an object  $A$  must have some embedding  $A \rightarrow B$  which kills it.

<sup>20</sup>“Le fait que la cohomologie d'un faisceau soit un cas particulier des foncteurs dérivés (au moins dans le cas paracompact) n'est pas dans le Cartan-Sammy. Cartan en avait conscience, et avait dit à Buchsbaum de s'en occuper, mais il ne semble pas que celui-ci l'ait fait. L'intérêt de ceci serait de voir quelles sont au juste les propriétés des faisceaux fins qu'il faut utiliser; ainsi on pourrait peut-être se rendre compte si, oui or non, il y a suffisamment de faisceaux fins dans le cas non séparé (je pense que la réponse est négative, mais je n'en suis nullement sûr!).

Among other things, he preempted 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<sup>21</sup> of all sheaves on a given topological space or, if you like, the prodigious arsenal of all "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 [*ReS*, p. P38].<sup>22</sup>

The Séminaire Cartan had shown that this structure suffices for much of cohomology. Definitions and proofs can be given by commutative diagrams without asking, most of the time, what these are diagrams of. Grothendieck and Buchsbaum pursued the idea independently, both extending earlier work in [Mac Lane, 1948].<sup>23</sup>

Grothendieck went the farthest, 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  $N \rightarrow M$  and arrow  $f: N \rightarrow I$ , there is at least one  $g: M \rightarrow I$  such that

$$\begin{array}{ccc} N & \xrightarrow{\quad} & M \\ & \searrow f & \downarrow g \\ & & I \end{array}$$

Fine sheaves are not so diagrammatic. Grothendieck saw that Reinhold Baer's original proof that modules have injective resolutions was largely diagrammatic itself.<sup>24</sup> Grothendieck thus gave diagrammatic axioms for the basic properties used

<sup>21</sup>Grothendieck knew this "set" is a proper class, the size of the universe of all sets. This is one reason for his Grothendieck universes [Artin *et al.*, 1972, pp. 185ff].

<sup>22</sup>"Considérons l'ensemble formé de tous les faisceaux sur un espace (topologique) donné, ou, si on veut, cet arsenal prodigieux formé de tous ces 'mètres' servant à l'arpenter. Nous considérons cet 'ensemble' ou 'arsenal' comme muni de sa structure la plus évidente, laquelle y apparaît, si on peut dire, 'à vue de nez'; à savoir, une structure dite de 'catégorie' .... C'est cette sorte de 'superstructure d'arpentage', appelée 'catégorie des faisceaux' (sur l'espace envisagé), qui sera dorénavant considérée comme 'incarnant' ce qui est le plus essentiel à l'espace."

<sup>23</sup>Grothendieck has said he did not know Mac Lane's work. Mac Lane has told me that when Grothendieck spoke on Abelian categories in Chicago around 1958 he mentioned no sources. 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 traveling in Mac Lane's circles, he was in Kansas in the midwest United States 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.

<sup>24</sup>See [Baer, 1940]. Cartan and Eilenberg, and Grothendieck, repair a set-theoretic error. Baer mistakenly says, for any infinite cardinals  $\Lambda \leq \Omega$ , every function  $\Lambda \rightarrow \Omega$  is contained in some initial segment of  $\Omega$  (that is,  $\Lambda$  is not cofinal in  $\Omega$ ). Cartan and Eilenberg correctly say this holds when  $\Omega$  is the next cardinal above  $\Lambda$  [Cartan and Eilenberg, 1956, p. 10]. Grothendieck gives a proof [Grothendieck, 1957, p. 137]. He was probably reading manuscripts for [Bourbaki, 1958].

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*, although 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.<sup>25</sup> Weibel showed how this proof is easily implicit in methods of the Séminaire Cartan [Weibel, 1999, p. 812]. 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 definition of derived functor beyond injectives, and not in terms of finding enough injective sheaves [Colmez and Serre, 2001, p. 15 (quoted above)].

Grothendieck showed that 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. Those resolutions remain available and useful for many cases. But Grothendieck treats paracompactness as a "restrictive condition" well removed from the basic theory, and he mentions the Weil conjectures [Grothendieck, 1957, p. 120]. His axioms also unify topological cohomology with group cohomology. They are clearly far more general in principle, but there were few, if any, known examples outside that framework.

Grothendieck's axioms also simplified homological algebra by focusing on just the relevant features. Textbooks today rarely use the generality. They rarely even discuss sheaves on topological spaces. Yet, they are generally organized in terms of Abelian categories.

Eisenbud's *Commutative Algebra* [Eisenbud, 1995] takes one common strategy, where Abelian categories are not defined but are referred to, and the definitions and proofs are quite diagrammatic. The proofs are effectively in Abelian category terms, although stated only for categories of modules. Hartshorne's *Algebraic Geometry* gives the Abelian category axioms and relies on several kinds of Abelian categories other than module categories. It does not prove the theorems but describes several ways to do it including directly from the axioms [Hartshorne, 1977, p. 202]. The first and second editions of Lang's *Algebra* 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" [Lang, 1993, p. 105]. He dropped that from the third edition because today's homological algebra books are already organized around this axiomatic viewpoint.

Serre's key contribution to the Séminaire Cartan by 1951, taken from work on his dissertation, was to clarify *spectral sequences* and to extend their range and power [Serre, 1951].<sup>26</sup> Spectral sequences were and still are the standard tool for non-trivial calculations in cohomology. At the time, a spectral sequence was an infinite series of infinite two-dimensional arrays of Abelian groups and group

<sup>25</sup>Compare Grothendieck's letter to Serre of 16 January, 1956 in [Colmez and Serre, p. 27]. See also [Godement, 1958, p. 260]. Godement cites heavily the Séminaire Cartan and [Grothendieck, 1957].

<sup>26</sup>The dissertation introduced many constructions in topology, especially those for calculating higher homotopy groups, and especially of spheres.

homomorphisms, with each successive array gotten from the homology of the one before. Of course, the groups might also be modules over some ring. No single point about spectral sequences is difficult. They are imposing from sheer mass. Grothendieck would simplify the theory by reconceiving spectral sequences as arrays of objects from any Abelian category.

As Grothendieck wrote to Serre, “I am rid of my horror of spectral sequences [Je suis débarrassé de mon horreur de la suite spectrale]” [Colmez and Serre, 2001, p. 7]. The point of spectral sequences had been to calculate in an orderly way, passing over many details of the Abelian groups involved. He could prove the general spectral sequence theorems while positing no details of the objects in the first place—although applications would depend on using suitably detailed objects. He derived most of the important spectral sequences as special cases of one “delectable and useful general spectral sequence” today called the Grothendieck spectral sequence [Colmez and Serre, 2001, p. 14].<sup>27</sup>

A few pages of definitions of sheaves, resolutions, and spectral sequences 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, although this legend may be a bit overstated. Eilenberg was ready to put it in the *Transactions of the American Mathematical Society* in 1956 subject to what Grothendieck called “severe editorial taboos.” In a letter to Serre dated 19 September, 1956, Grothendieck said he would do it only if someone else would retype the manuscript [Colmez and Serre, 2001, p. 45]. At any rate, Abelian categories did simplify and extend the theory so that they became and remain the standard setting for (co)homology.

This was a major step. “Grothendieck had shown that, given a category of sheaves, a notion of cohomology groups results” [Deligne, 1998, p. 16].<sup>28</sup> And he radically generalized categories of sheaves to any Abelian category with a generator and enough injectives. Any such category has an intrinsic cohomology theory. It remained to find which Abelian categories give Weil cohomology.

### The Larger Vision

Grothendieck never mentions Abelian categories by name in *Récoltes et Semailles*. He focuses on more controversial ideas. He does cite *Tôhoku* in an enlightening way, namely, 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” . . . above all the properties I introduced into category theory under the name “exactness properties” [*ReS*, p. P39].<sup>29</sup>

<sup>27</sup>See [Grothendieck, 1957], [Eisenbud, 1995, p. 677], and [Lang, 1993, p. 821].

<sup>28</sup>Grothendieck avait montré que, une catégorie de faisceaux étant donnée, une notion de groupes de cohomologie en résulte.”

<sup>29</sup>“Un ‘espace nouveau style’ (ou *topos*), généralisant les espaces topologiques traditionnels, sera décrit tout simplement comme une ‘catégorie’ qui, sans provenir forcément d’un espace ordinaire, possède néanmoins toutes ces bonnes propriétés (explicitement désignées une fois pour toutes, bien sûr) d’une telle ‘catégorie de faisceaux’ . . . Il s’agit ici surtout de propriétés que j’ai

He introduced those properties in [Grothendieck, 1957]. The specific properties of a topos are very different from those of an Abelian category, but both are expressed in part as 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. The cohomology of that topos may solve the problem. In outline:

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

In Grothendieck's own 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 (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 [ReS, p. P41].<sup>30</sup>

The unity sought in the Séminaire Cartan was complete. Cohomology gives algebraic invariants of a topos as it used to give invariants of a topological space. Each topological space determines a topos with sheaf cohomology. Each group determines a topos with group cohomology. This would work for cases yet unimagined. Grothendieck notes that his Abelian categories were exactly suited to the cohomology of any topos, although toposes were entirely unforeseen as he wrote *Tôhoku* in 1955. He takes this as one more proof that it is the right idea of cohomology [ReS, p. P41n].

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 [ReS, p. P31 and passim].<sup>31</sup>

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<sup>30</sup>“La chose cruciale ici, dans l’optique des conjectures de Weil, c’est que la nouvelle notion est assez vaste en effet, pour nous permettre d’associer à tout ‘schéma’ un tel ‘espace généralisé’ ou ‘topos’ (appelé le ‘topos étale’ au schéma envisagé). Certains ‘invariants cohomologiques’ de ce topos (tout ce qu’il y a de ‘bébêtes’!) semblaient alors avoir une bonne chance de fournir ‘ce dont on avait besoin’ pour donner tout leur sens à ces conjectures, et (qui sait!) de fournir peut-être les moyens de les démontrer.”

<sup>31</sup>Deligne's 1972 proof, completing the Weil conjectures, was not as simple as Grothendieck hoped [Deligne, 1974]. Weil's proposed trivial calculation assumed cohomology with ordinary integer coefficients, but étale cohomology gives  $p$ -adic integer coefficients which are more general. In fact, the relevant coefficients are ordinary integers. Grothendieck conjectured general theorems on étale cohomology to prove that and more, called the *standard conjectures*. See [Grothendieck,

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# Anticipations of Schemes

Serre has put it well: no one invented schemes.<sup>32</sup> The brilliant step was to recognize their value. Grothendieck says that “[t]he very idea of scheme is of childish simplicity—so simple, so humble, that no one before me thought of stooping so low. So ‘babyish,’ in short, that for years, despite all the evidence, for many of my erudite colleagues, it was really ‘not serious’!” [ReS, p. P32].<sup>33</sup> The question is, why did Grothendieck believe he should use such an idea to simplify an eighty-page paper by Serre into some 1000 pages of *Éléments de géométrie algébrique*? In fact, others did think of the idea. A look at how they thought of it, and at some of the reasons why they might have dropped it, highlights Grothendieck’s achievement.

Basic algebraic geometry studies *varieties* defined by polynomial equations. Varieties have polynomial coordinate functions on them. One stock example is the complex number plane  $\mathbb{C}^2$ . Its classical points are the pairs  $(\alpha, \beta)$  of complex numbers, subject to no equation. Its coordinate functions are all polynomials  $P(x, y)$  with complex number coefficients and variables  $x, y$  over the complex numbers. The coordinate ring is then just the ring  $\mathbb{C}[x, y]$  of all these polynomials.

Another stock example is the unit circle  $S^1$  in  $\mathbb{C}^2$  defined by the equation  $x^2 + y^2 = 1$ . Its classical points are the pairs of complex numbers  $(\alpha, \beta)$  with  $\alpha^2 + \beta^2 = 1$ . It is thus the *subvariety* of  $\mathbb{C}^2$  defined by that equation. In more systematic terms, it is defined by the ideal in  $\mathbb{C}[x, y]$  of all polynomials divisible by the polynomial  $x^2 + y^2 - 1$  or, geometrically, the ideal of all polynomials which are 0 all over  $S^1$ . The coordinate ring is the quotient of the ring  $\mathbb{C}[x, y]$  by that ideal. A coordinate function on  $S^1$  is thus any complex polynomial in  $x, y$ , regarding polynomials as equal if they take equal values at each point of the circle. There is a natural ring homomorphism from  $\mathbb{C}[x, y]$  to that quotient, which takes each polynomial to its equivalence class. Geometrically, that amounts to restricting each coordinate function on  $\mathbb{C}^2$  to a coordinate function on the subvariety  $S^1 \subseteq \mathbb{C}^2$ .

The subvarieties  $V' \subseteq V$  of a variety  $V$  correspond to prime ideals in the coordinate ring of  $V$ . Since the classical points  $p$  of  $V$  are the minimal subvarieties, they correspond to the maximal ideals. As a hint of things to come, maximal ideals in any ring are all prime.

The early twentieth-century Italian algebraic geometers made deep and subtle use of generic points of a variety—that is, points with no special properties—so that anything proved of a generic point was true of all except maybe some exceptional points on that variety. Bartel van der Waerden used ideas from Emmy Noether to make this more precise [van der Waerden, 1926].<sup>34</sup>

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1969] and [Kleiman, 1994]. They remain unproved. Deligne instead gave a wide-ranging, elegant but difficult geometric argument. See also [Mumford and 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*. One of the two 2002 Fields Medal winners, Vladimir Voevodsky won for “leading us closer to the world of *motives* that Grothendieck was dreaming about in the sixties” [Soulé, 2003, p. 102]. The other, Laurent Lafforgue, relates his own work to “Grothendieck’s conjectural theory of motives” [Lafforgue, 2003, p. 383].

<sup>32</sup>In a conversation with the author in 1995.

<sup>33</sup>“L’idée même de schéma est d’une simplicité enfantine—si simple, si humble, que personne avant moi n’avait songé à se pencher si bas. Si ‘bêbête’ même, pour tout dire, que pendant des années encore et en dépit de l’évidence, pour beaucoup de mes savants collègues, ça faisait vraiment ‘pas sérieux’!”

<sup>34</sup>He later learned she had already done it in her lectures [van der Waerden, 1971].

Skipping the details, his version makes generic points not classical points at all but something else. For example, the “generic point” of the unit circle somehow lies over all of the classical points  $(\alpha, \beta)$ . David Mumford’s famous lecture notes on schemes from the 1960s depict such a point as a blur spread out over the circle [Mumford, 1968]. Anything true of this generic point is true of nearly all the classical points, if stated in the correct form. On this approach, each subvariety of a variety  $V$  has a generic point. Since each classical point is a subvariety, it gets a generic point of its own—a silly doubling of points.

For Emmy Noether’s school, then, it was natural to look at prime ideals instead of classical and generic points. As we would say today, it was natural to identify all points with prime ideals. Her associate Wolfgang Krull did precisely this. He spoke in Paris before the Second World War on algebraic geometry, taking prime ideals as points and implicitly using a Zariski topology (for which see any current textbook on algebraic geometry). The classical points correspond to the maximal ideals, but maximality plays no role in the basic definitions. In fact, all the basic definitions work for prime ideals in any commutative ring, not only for polynomial rings, so Krull did it in that generality. The audience laughed at him, and he abandoned the idea [Neukirch, 1999, p. 49].

Weil based his *Foundations of Algebraic Geometry* on a version of van der Waerden’s generic points [Weil, 1946]. Generic points became a staple of Parisian algebraic geometry. When Serre left them out of his influential rival to Weil foundations, discussed below, people naturally thought about how to add them in. As Pierre Cartier explains:

[André] Martineau remarked to [Serre] that his arguments remained valid for any commutative ring, provided one takes all prime ideals instead of only maximal ideals [that is, provided one takes generic as well as classical points]. 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].

Serre probably already knew it, and certainly found it obvious. Grothendieck and Jean Dieudonné soon wrote “Serre himself has remarked that the cohomology theory of algebraic varieties could be transcribed with no difficulty ... to any commutative ring” [Grothendieck and Dieudonné, 1960, p. 7].

Why did Krull abandon his idea, Martineau leave it as an aside, and Cartier judge his excessively long? Since Krull was a foreigner in Paris, perhaps the audience laughed harder at him than Bourbaki would at Grothendieck. Martineau worked in analysis. Perhaps Cartier would have come back to it, if Grothendieck had not taken it over. I will come back to Cartier.

More mathematically, it seems Krull’s motive was simply that “it was there.” The algebraic definitions of point and subvariety applied over any ring, so he gave them in that generality. Weil later proved hard theorems using generic points, and this seemed to point to an even more penetrating theory than Weil really provided. Perhaps the general theory of schemes could survive only when it had that much work to do.

Also, Krull lacked sheaves. Cartier knew them well but avoided using them in this context. Serre based algebraic geometry on them. Their easy way of pasting varieties together and their facility for cohomology prompted Grothendieck to say that Serre had “the principle of the right definition” of schemes [Grothendieck, 1958, p. 106]. Finally, Krull, Cartier, and Serre all worked without category theory. Grothendieck and Dieudonné remark this on one central issue: “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 and Dieudonné, 1971, p. 6].<sup>35</sup>

### Schemes in Paris

A story says that in a Paris café around 1955 Grothendieck asked his friends “what is a scheme?” Compare the story of Hilbert asking John von Neumann “but what is a Hilbert space, really?” Hilbert wanted the idea behind von Neumann’s axioms, examples, and theorems. Grothendieck’s question was different. At the time, only an undefined idea of “schéma” was current in Paris, meaning more or less whatever would improve on “Weil foundations.”<sup>36</sup>

One serious challenge was that Weil wanted “algebraic geometry over the integers” and not over a field at all, following Leopold Kronecker [Weil, 1952]. He wanted each integer polynomial  $P(X, Y, \dots, Z)$  to define a space  $X$  over the integers, which would *specialize* to other spaces defined by the same polynomial over the field  $\mathbb{Q}$  of rational numbers and all finite prime fields  $\mathbb{F}_p$ .<sup>37</sup> When Grothendieck described schemes in his memoir years later, this is all he said about them: A single scheme can be a “magical fan [éventail magique]”<sup>38</sup> combining varieties over all those fields [ReS, p. P32].

There were two leading contenders by 1956. One, which did not use the word “scheme,” was Serre’s paper “Coherent Algebraic Sheaves” or, in the original French, “Faisceaux algébriques cohérents” generally cited as FAC [Serre, 1955]. The other was “the Chevalley-Nagata theory of schemes” with a variant of it by Pierre Cartier which he says “closely follows the exposition in Serre [1955] only avoiding the use of sheaves” [Cartier, 1956, p. 1-01].<sup>39</sup>

<sup>35</sup>“L’idée de ‘variation’ de l’anneau de base que nous venons d’introduire s’exprime mathématiquement sans peine grâce au langage fonctoriel (dont l’absence explique sans doute la timidité des tentatives antérieures.”

<sup>36</sup>Otto Schilling’s enthusiastic but awestruck review of Weil’s 1946 book will dispel any thought that the ideas were more accessible and naturally geometric in those days. See *Mathematical Reviews* 9, #303c in 1946.

<sup>37</sup>In effect, Weil wanted geometry over any commutative ring. He later called this “the natural evolution of the subject,” 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, 1:576].

<sup>38</sup>The image is of an oriental hand fan that collapses to a rod. In stories, such a fan works as a magic wand or can extinguish fires or set them by fanning. When a stage magician fans out a deck of cards, this is also an “éventail magique.”

<sup>39</sup>“On a suivi de près l’exposition de Serre (*Ann. de Math.*, 61, 1955, p. 197–278) en évitant seulement l’emploi des faisceaux.” Cartier cites neither Chevalley nor Nagata. Grothendieck mentions them and Cartier in [Grothendieck, 1957, p. 161] and [Grothendieck, 1962, p. 190-01].

Cartier defines a spectrum  $\Omega_A$  for each finite type algebra  $A$  over a field  $k$ , with a Zariski topology.<sup>40</sup> Elements of  $A$  are construed as “functions” from the spectrum to a field extension of  $k$  (as sections of the structure sheaf of a scheme over a field  $k$  are construed as “functions” to  $k$ ). Cartier defines “algebraic sets” by pasting together spectra. He proves various theorems of current scheme theory for spectra 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 so-called separated scheme [Cartier, 1956, p. 1-12]. The Chevalley-Nagata theory is a slight variant [Cartier, 1956, p. 2-18]. The definitions easily generalize to all commutative rings, supporting Cartier’s claim of anticipating Grothendieck’s schemes [Cartier, 2001, p. 398].

The key feature of FAC is missing in all of that. It is the idea of structure sheaves. An affine variety in FAC does not just have a ring of coordinate functions, but a sheaf of rings of functions. For later comparison, note that all of these functions are functions in the set-theoretic sense. The structure sheaf on an affine variety  $V$  gives no more information than a coordinate ring, but it serves two purposes.<sup>41</sup>

First, Weil could only deal with more general, non-affine varieties in terms of their parts. He had to speak of several affine varieties plus a pasting relation, yet the parts and pasting would not be unique. There would be other ways to assemble the same “abstract variety,” but the abstract variety did not exist in itself, only the many ways to assemble it actually exist. Serre could paste several affine varieties into a single space with structure sheaf. That space may also have a natural description without pasting.

Second, the structure sheaf is directly suited to cohomology. Serre’s FAC produced a cohomology for varieties, now standard in algebraic geometry called coherent cohomology. Serre suggested this might give the right Betti numbers of varieties for the Weil conjectures [Serre, 1955, p. 233], but he also knew it could not be the Weil cohomology because it could not give an adequate Lefschetz fixed point theorem.<sup>42</sup> Today, coherent cohomology is generally given in Grothendieck’s form as a derived functor cohomology on any scheme, as in [Hartshorne, 1977].<sup>43</sup> Serre varieties are much less general than schemes. The ring of coordinate functions

<sup>40</sup>In Exposé 1,  $\Omega_A$  is the set of homomorphisms from  $A$  to an algebraically closed extension  $K$  of  $k$ . If  $K$  is the algebraic closure, then (modulo the Galois group of  $K$  over  $k$ ) these amount to the maximal ideals of  $A$  as used in [Serre, 1955]. If  $K$  has infinite transcendence degree over  $k$ , then they amount (modulo the Galois group) to the prime ideals. Exposé 2 uses prime ideals of  $A$  rather than homomorphisms, so the Galois group disappears, leaving spectra in Grothendieck’s sense.

<sup>41</sup>The sheaf assigns each open subset  $U \subseteq V$  a coordinate ring. For all  $p \in U$ ,  $g(p) \neq 0$ , it assigns  $U$  the ring of fractions  $f/g$ , where  $f$  and  $g$  are coordinate functions on  $V$ . For example,  $1/x$  is defined on the open subset  $\{x \in \mathbb{R} \mid x \neq 0\}$ . All is determined by the coordinate ring of  $V$ .

<sup>42</sup>Applied to a variety over any  $\mathbb{F}_p$  or its algebraic closure, this cohomology gives coefficients modulo  $p$ . It could thus count the fixed points modulo  $p$  at best.

<sup>43</sup>Serre used a version of Čech cohomology. A key theorem shows that this cohomology is a derived functor: affine varieties have vanishing cohomology [Serre, 1955, p. 239 (corollary 1)]. Yet, Grothendieck says that “this should be considered an accidental phenomenon” and that “it is important for technical reasons not to take as *definition* of cohomology the Čech cohomology” [Grothendieck, 1958, p. 108]. The derived functor definition trivializes the broad generalities before any substantial theorems.

on a Serre variety has to be an integral algebra of finite type over an algebraically closed field.

### Schemes in Grothendieck

Grothendieck made it simple. As the correct definition of cohomology applied to every topological space, so every commutative ring should be the coordinate ring of a space. Every such space would have cohomology in Serre's sense. The space of a commutative ring  $R$  is its spectrum or  $\text{Spec}(R)$  with the prime ideals  $p$  as points and the Zariski topology. Every ideal  $I$  of  $R$  gives a closed set, where a point  $p$  lies in the closed set of  $I$ , if  $I \subseteq p$  as ideals. Schemes in general come from pasting together spectra. At the 1958 International Congress of Mathematicians, Grothendieck called this topology on the prime ideals of commutative rings "classical" [Grothendieck, 1958, p. 106]. The next year in the Séminaire Bourbaki he called it "well known [bien connu]" [Grothendieck, 1962, p. 182-01].

Grothendieck's originality, according to Serre, was that only he saw it as geometry. Before Grothendieck convinced him, Serre thought geometry would require the rings to "meet some conditions, at least be Noetherian."<sup>44</sup> Indeed, many classical geometric results need further assumptions, but uncanny amounts of geometric intuition are directly expressed in all generality for all commutative rings. The later assumptions are visibly irrelevant to the first steps—that is, now visibly, but visible to few in the 1950s.

Famously, Grothendieck wrote the work in collaboration with Jean Dieudonné. Their original edition published in the series of the Institut des Hautes Études Scientifiques (IHES) is wary of history in two senses. They find any history of schemes "beyond our competence [hors de notre compétence]" [Grothendieck and Dieudonné, 1960, p. 7]. They also warn that prior knowledge of algebraic geometry, "despite 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 and Dieudonné, 1960, p. 5].<sup>45</sup>

Yet, their introduction closes with an 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<sup>46</sup> or of schemes over a given scheme): cartesian products, the laws of a group or a ring or module, fiber bundles,

<sup>44</sup>In a conversation with the author in 1995.

<sup>45</sup>"... malgré ses avantages évidents, pouvait parfois (par l'habitude trop exclusive du point de vue birationnel qu'elle implique) être nuisible à celui qui désire se familiariser avec le point de vue et les techniques exposés ici."

<sup>46</sup>I translate "préschémas" as "schemes" to follow current usage.

principal homogeneous fiber bundles, etc. 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 [Grothendieck and Dieudonné, 1960, p. 9].<sup>47</sup>

In the Springer-Verlag edition, they claim their own historical heritage and trace the basic idea back a hundred years to Dedekind and Heinrich Weber [Grothendieck and Dieudonné, 1971, p. 11].

They also signal evolution in thinking about categories. They drop the warning and add a section on transporting structures into categories.<sup>48</sup> 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, 1998, p. 13 (his emphasis)].<sup>49</sup> The nice properties are those used to transport the various structures listed by Grothendieck and Dieudonné.

The 1960 list of structures highlights how these categorically nice properties are a mess set-theoretically. The product  $X \times Y$  of schemes  $X$  and  $Y$  is a scheme with projection morphisms  $p_1$  and  $p_2$  with the familiar categorical property:

$$\begin{array}{ccccc} & & T & & \\ & f \swarrow & & \searrow g & \\ X & & X \times Y & & Y \\ & p_1 \swarrow & & \searrow p_2 & \end{array}$$

$\langle f, g \rangle$

A morphism from any scheme  $T$  to  $X \times Y$  is given by a pair  $\langle f, g \rangle$  of morphisms to  $X$  and  $Y$ . The set of points of  $X \times Y$ , however, is nothing like the set-theoretic 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.<sup>50</sup> Or  $X$  and  $Y$  may each have a single point, while  $X \times Y$  has many.<sup>51</sup>

An ordinary group is a set  $G$  with a binary operation  $G \times G \rightarrow G$  and a group inverse function  $G \rightarrow G$  which satisfy certain equations, that is, they make certain

<sup>47</sup>"Pour terminer, nous croyons utile de prévenir les lecteurs que, tout comme les auteurs eux-mêmes, ils auront sans doute quelque difficulté avant de s'accoutumer au langage des schémas, et de se convaincre que les constructions habituelles que suggère l'intuition géométrique peuvent se transcrire, essentiellement d'une seule façon raisonnable, dans ce langage. Comme dans beaucoup de parties de la Mathématique moderne, l'intuition première s'éloigne de plus en plus, en apparence, du langage propre à l'exprimer avec toute la précision et la généralité voulues. En l'occurrence, la difficulté psychologique tient à la nécessité de transporter aux objets d'une catégorie déjà assez différente de la catégorie des ensembles (à savoir la catégorie des préschémas, ou la catégorie des préschémas sur un préschéma donné) des notions familières pour les ensembles: produits cartésiens, lois de groupe, d'anneau, de module, fibrés, fibrés principaux homogènes, etc. Il sera sans doute difficile au mathématicien, dans l'avenir, de se dérober à ce nouvel effort d'abstraction, peut-être assez minime, somme toute, en comparaison de celui fourni par nos pères, se familiarisant avec la Théorie des Ensembles."

<sup>48</sup>See the first section of [Grothendieck and Dieudonné, 1971].

<sup>49</sup>"Si permettre que tout anneau commutatif définisse un schéma affine donne droit de cité à des schémas bizarres, le permettre fournit une catégorie de schémas ayant de bonnes propriétés."

<sup>50</sup>This happens, for example, if  $X$  and  $Y$  are schemes over two fields with different characteristics.

<sup>51</sup>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.

diagrams commute. This transfers directly to schemes. A group scheme  $X$  has morphisms  $X \times X \rightarrow X$  and  $X \rightarrow X$  making the same diagrams commute. Set-theoretically, however,  $X \times X$  does not have the elements of a product, and  $X \times X \rightarrow X$  is not a binary operation. A group scheme is not a group from the set-theoretic viewpoint, although it is from the categorical viewpoint. Thus, the slogan “a group scheme is just a group object in the category of schemes.”

The greatest objection to schemes was a certain non-set-theoretic feature.<sup>52</sup> The elements of any commutative ring  $R$  appear as coordinate functions on the spectrum  $\text{Spec}(R)$ . Of course, these are generally not functions in the set-theoretic sense. The scheme context makes them act rather like 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 as in any standard text on schemes). Yet, a “function”  $g \in R$  may have  $g(p) = 0$  at every point  $p$  of the scheme and not be the zero function! In geometric terms, this happens when the scheme has infinitesimal fringe around it, and  $g$  is 0 at each point but has nonzero derivative in some directions through the fringe.<sup>53</sup>

A “function” in this sense is not determined by its values. According to David Mumford, “[i]t is this aspect of schemes which was most scandalous when Grothendieck defined them” [Mumford, 1966, p. 12].<sup>54</sup> It is nevertheless 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 there are subschemes 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  $X$ .

Indeed, the set of points of a scheme is rarely the best handle on it:

The audacity of Grothendieck’s definition is to accept that *every* commutative ring  $A$  (with unit) has a scheme  $\text{Spec}(A)$ . . . . This has a price. The points of  $\text{Spec}(A)$  (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].<sup>55</sup>

Rather, one begins with geometric relations to other schemes. This holds as well for some older notions of space and many newer ones [Cartier, 2001]. None, however, is as important as schemes.

<sup>52</sup>This is not about foundations of mathematics. On any foundation, geometers will treat sections of the structure sheaf as functions (analogous to polynomial functions on classical varieties), while they are not set-theoretic functions.

<sup>53</sup>Algebraically, this means that  $g \neq 0$  has some power  $g^n = 0$  and so belongs to every prime ideal. For a picture, consider a polynomial  $f(x)$  with  $f(0) = 0$  but derivative  $f'(0) \neq 0$ ;  $f$  is not the zero function on a first-order infinitesimal fringe around 0, although 0 is the only point within that fringe. In this case,  $f^2(x)$  has value  $f^2(0) = 0$  and first derivative  $2f(0) \cdot f'(0) = 0$ . So  $f^2$  is the zero function all over that fringe.

<sup>54</sup>Mumford has said Oscar Zariski was particularly put off by this, but Mumford made him waver by describing a scheme-theoretic proof of Zariski’s Main Theorem.

<sup>55</sup>“L’audace de la définition de Grothendieck est d’accepter que tout anneau commutatif (à unité)  $A$  définit un schéma affine  $\text{Spec}(A)$ . . . . Ceci a un prix. Les points de  $\text{Spec}(A)$  (idéaux premiers de  $A$ ) n’ont pas un sens géométrique maniable. . . . Quand on a à construire un schéma, on ne commence pas en général par construire l’ensemble de ses points.”

Another issue was decisive in the success of schemes. 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  $P$ . For example, a complex polynomial  $x^2 + \alpha xy + \beta y^2$  in two variables  $x, y$  with parameters  $\alpha, \beta$  defines a conic section varying over the complex plane  $\mathbb{C}^2$ . For parameter values  $(\alpha, \beta) = (1, 1) \in \mathbb{C}^2$ , it is the conic  $x^2 + xy + y^2 = 0$ . For values  $(\alpha, \beta) = (2, 1) \in \mathbb{C}^2$ , it is the degenerate conic

$$x^2 + 2xy + y^2 = (x + y)^2 = 0.$$

Scheme theory expresses both in the same way. To say a scheme  $X$  varies over the complex plane  $\mathbb{C}^2$  is just to say it has a scheme morphism,  $X \rightarrow \mathbb{C}^2$ , to  $\mathbb{C}^2$ . To say  $X$  is defined over a field  $k$  is just to say it has a scheme morphism,  $X \rightarrow \text{Spec}(k)$ , to the spectrum of  $k$ .

Grothendieck treats a scheme morphism  $X \rightarrow S$  as a single scheme or, more precisely, as a relative scheme over the base  $S$ . A morphism of relative schemes over  $S$  is a commuting triangle of scheme morphisms:

$$\begin{array}{ccc} X & \xrightarrow{\quad} & Y \\ & \searrow & \swarrow \\ & S & \end{array}$$

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

Given  $X \rightarrow S$ , Grothendieck could largely ignore the coefficients or the parameters and let them take care of themselves. The category of schemes over any base scheme  $S$  is very much like the category of schemes *per se*, although with specific differences reflecting the algebraic geometry of  $S$ . Demazure and Grothendieck note the advantages of this, and of infinitesimal fringe, to group schemes [Demazure and Grothendieck, 1970, 1.viii]. They can treat a parameterized family of group schemes as a single group; a group scheme over a base scheme  $S$  with just one point plus infinitesimal fringe is an infinitesimal deformation of one group scheme.

Relative schemes produce the simple and general functorial account of base change that Grothendieck and Dieudonné mentioned [Grothendieck and Dieudonné, 1971, p. 6]. For example, given a scheme  $X \rightarrow \text{Spec}(\mathbb{R})$  over the real numbers, to focus on its complex points means extending it to a scheme  $X' \rightarrow \text{Spec}(\mathbb{C})$  over the complex numbers. This is just a pullback in the category of schemes

$$\begin{array}{ccc} X' & \longrightarrow & X \\ \downarrow & & \downarrow \\ \text{Spec}(\mathbb{C}) & \longrightarrow & \text{Spec}(\mathbb{R}) \end{array}$$

The natural morphism  $\text{Spec}(\mathbb{C}) \rightarrow \text{Spec}(\mathbb{R})$  corresponds to the field extension  $\mathbb{R} \subseteq \mathbb{C}$ . The same works for any extension field  $k \subseteq K$ .

Alternatively, take a family of schemes  $X \rightarrow \mathbb{C}^2$  varying over the complex plane  $\mathbb{C}^2$ . To look at just the part lying over the unit circle  $S^1$  in that plane, take the pullback along the natural inclusion  $S^1 \rightarrow \mathbb{C}^2$ .

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

The formalism makes good, general theorems trivial so as to clear the way to the particulars in any given case. Of course, the particulars of a given case may remain difficult themselves because they can give the solution to some genuine problem in geometry or algebra. In any case, the set-theoretic details are rebarbative and uninteresting.

### Toward the Séminaire de Géométrie Algébrique

We have seen “the sea advance insensibly in silence ... so far off you hardly hear it [La mer s’avance insensiblement et sans bruit ... si loin on l’entend à peine]” towards the Weil conjectures, up to about 1958 [ReS, p. 552]. Along the way, in 1957, Grothendieck found the Grothendieck-Riemann-Roch Theorem, but left it to Armand Borel and Serre to publish a proof [Borel and Serre]. Raoul Bott’s review in the *Mathematical Reviews*<sup>56</sup> notes that “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,” dispelled Bourbaki’s doubts about him, and made him “somewhat feared” by other mathematicians at the 1958 International Congress of Mathematicians. He says he proved it by “the rising sea,” even though it was not a question of his own making. Serre put him onto it. We have seen not one step specifically on the Weil conjectures.<sup>57</sup>

Serre made a crucial step in 1958, actually finding the 1-dimensional Weil cohomology groups, using isotrivial coverings.<sup>58</sup> 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 bundles to yield the  $H^1$ s. “But Grothendieck was always an optimist in those days.”<sup>59</sup> Serre thought higher dimensional Weil cohomology would need to use higher homotopy groups [Serre, 2001, p. 255]. Those seemed far out of reach. The expanded print version of Serre’s talk appeared a few months later. By then, Grothendieck had shown him that it indeed gives cohomology in all dimensions, and convinced him that this was likely the “true cohomology needed to prove the Weil conjectures.”<sup>60</sup>

Grothendieck’s optimism grew from his method. Cohomology is uniquely determined, once we know *what* we want the cohomology *of*. Serre had found that the Weil conjectures need the cohomology of isotrivial covers (soon modified to étale covers). The job was thus finished in principle—from Grothendieck’s viewpoint—but he did not rush to work it all out. That would have been striking at the chisel. Rather, he conceived a larger framework to embrace at once spaces, their sheaves, and cohomology. Technically, this framework is all about “covering.” Conceptually,

<sup>56</sup>See 22, #6817.

<sup>57</sup>Quotes from [ReS, pp. P23, 705, 32, 554-555].

<sup>58</sup>He used locally isotrivial fiber bundles, but the relation to coverings was obvious. See [Serre, 1958], which cites [Grothendieck, 1958] for scheme theory.

<sup>59</sup>Quotes from a conversation with the author in the fall of 1995.

<sup>60</sup>See [Serre, 1958, p. 125] and compare [Grothendieck, 1958, p. 104].

it is about transporting geometric ideas into new categories. It first appeared as Grothendieck topology: "the technical, provisional form of the crucial notion of *topos*" [*ReS*, p. P24 (his emphasis)].

Each sheaf lives in 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'" [*ReS*, p. P38]. To find the right sheaves for a new problem, find a new world: the right *topos*. The sheaves will be the Abelian groups in that world. To work with a scheme, look at 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 Géométrie Algébrique at the IHES along with his proofs of the first and second Weil conjectures.

Categorical world-building is young. Abelian categories are standard in research, although they are not very common in textbooks. Grothendieck topology is entirely accepted in research and rare in textbooks. Toposes are still widely avoided by geometers, although the theory continues to grow.<sup>61</sup> Lafforgue's and Voevodsky's Fields Medals bear testament to the fact that Grothendieck's largest vision is still progressing in algebraic geometry.

As to schemes, Grothendieck and Dieudonné focused 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 and Dieudonné, 1971, p. 1].<sup>62</sup> 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].<sup>63</sup>

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<sup>61</sup>For the current state of the general theory, see [Johnstone, 2002].

<sup>62</sup>"Il convient de donner à la Géométrie algébrique toute la souplesse et la généralité désirables, en la faisant reposer sur la notion de *schéma*."

<sup>63</sup>"Je dois préciser que je prends ce dernier terme au sens qui est devenu le sien depuis quelques années: celui de *théorie des schémas*."

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