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Developments in Algebraic Geometry

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I should say at the outset that I have no claim to any particular insight into the future of algebraic geometry. What I thought I would do, accordingly, is to talk a little bit about the history of the subject, leading up to its present incarnations, and leave it to you to extrapolate.

Algebraic geometry is a subject whose development has been marked by fundamental changes in the basic objects studied, and in the approach to their study. For example, one possible definition of the subject—admittedly an extreme one—would be to say that algebraic geometry is "the study of the geometry of those loci defined by polynomial equations." If we adopt this point of view, we could say that the subject is over two millennia old: the conic sections and quadric surfaces studied by the ancient Greeks happen to be such objects.

A more balanced definition of the subject might be to say that it is the study of the relations between the algebra of polynomials and the geometry of the loci that they define. In this sense, the subject is much younger; it traces its origins back to the introduction by Descartes of the notion of coordinates in the plane, making it possible to describe a conic as the zero locus of a quadratic polynomial $f(x, y)$, and relate the algebraic manipulation of that polynomial to geometric operations on the curve itself.

Of course, to Descartes and to mathematicians for some time afterward, "polynomial" meant polynomials $f_\alpha(x_1, \dots, x_n)$ with real coefficients, and "locus" meant the set of real solutions, that is, the subset X of $\mathbb{R}^n$ of vectors $x = (x_1, \dots, x_n)$ such that $f_\alpha(x) = 0$. The basic set-up of algebraic geometry from the time of Descartes until the early nineteenth century was this: one had a collection of polynomials $f_\alpha(x_1, \dots, x_n) \in \mathbb{R}[x_1, \dots, x_n]$ with real coefficients, and one studied their common zeros in n-space $\mathbb{R}^n$. This was a time when the techniques of the subject were pretty rudimentary, but the problems studied were completely intelligible, even to nonexperts.

For example, consider the simplest type of algebraic variety: a plane curve,

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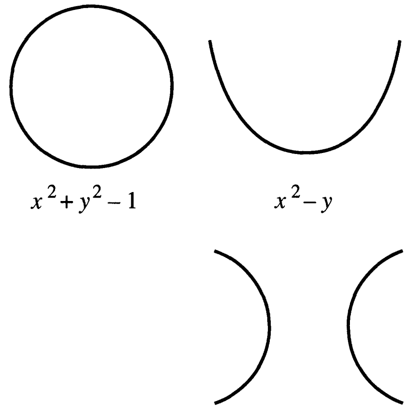


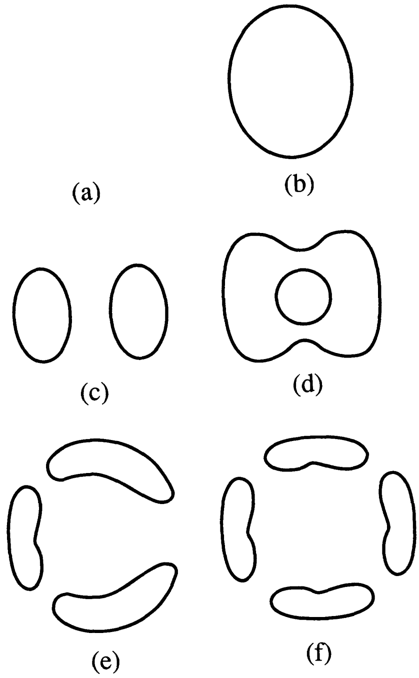
$x^2 + y^2 - 1$ $x^2 - y$

$x^2 + y^2 + 1$ $x^2 - y^2 - 1$

or in other words the zero locus X of a single polynomial $f(x, y)$ of degree d in two variables. If we assume the curve C is smooth, in the sense that f does not vanish simultaneously with its two partial derivatives, then X will be a real 1-manifold, that is, a disjoint union of copies of $\mathbb{R}$ and S^1, the latter of which were called "ovals" in the classical language. We may then ask how many arcs and ovals a plane curve may have; and what sort of configuration they may form—that is, which pairs of ovals may be nested. For example, a plane quartic—that is, the zero locus of a fourth-degree polynomial in x and y—without arcs may have any number of ovals from none to four; if there are two, they may be nested or not, as in diagrams (c) and (d). The main tool here is simply the fact that no line may meet a quartic curve more than four times, and more generally that another plane curve of degree e may meet it in at most $4e$ points; thus, if a quartic contains two nested ovals it can contain no other points, since a line joining such a point to a point interior to the inner of the two nested ovals would meet the curve at least five times.

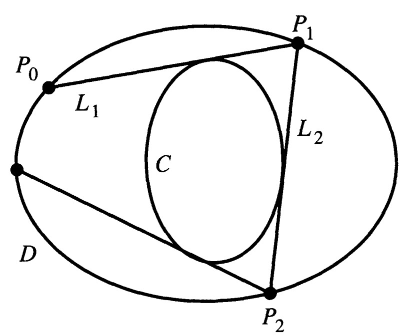
Of course, we may make further distinctions, e.g., between convex and nonconvex ovals; for example, the outer oval of two nested ovals forming a quartic may be either convex or nonconvex, as in figure (d) (the inner one must always be convex; otherwise there would exist a line meeting the curve six times).

The answer to the first of the questions posed above is *Harnack's theorem*, which says that *a plane curve X of degree d may have any number of ovals from none to* $(d-1)(d-2)/2 + 1$. It is proved in elementary fashion using the fact that a curve C passing through a point P lying on an oval of X must meet that oval at least twice. For example, in the case above suppose that a quartic curve had five ovals. We could then choose a point p_i on each of five ovals of C, and then find a conic curve Q passing through each of these points; Q would then have to meet C in at least ten points, violating the fact that a conic and a quartic can meet in at most eight points. In fact, the bound given by Harnack's theorem is sharp, as may readily be seen by example.

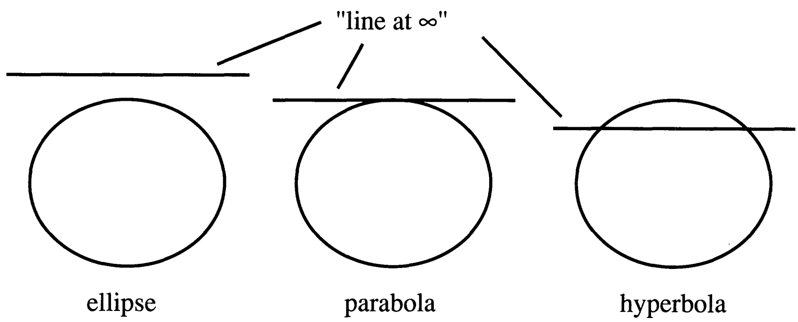


The second question above—what configurations the ovals of a plane curve may form—is, by contrast, still unanswered to this day, even in the case of curves of degree 6, though progress has been made by the Russian school.

To give another example of a problem examined and solved during this period, consider Poncelet's theorem. The original question asks when, given two ellipses C and D in the plane, there is a polygon inscribed in one and circumscribed about the other; the answer is a surprising one. To construct such a polygon, starting with a given vertex P_0 on the outer ellipse D, is easy: we just take the first side L_1 to be one of the two tangent lines to C through P_0; take P_1 to be the other point of intersection of this line with D, L_2 the other tangent line to C through P_1, and so on. The question is then when this process repeats after a finite number of steps; Poncelet's theorem is that it does or does not independently of the choice of initial point P_0, so that the pair (C, D) will either admit a continuous family of inscribed-and-circumscribed polygons or none at all.



The "classical" period. The next transformation of the subject of algebraic geometry occurred around the beginning of the nineteenth century. It consisted of two changes in the basic objects considered: the introduction of projective varieties, and of complex coordinates. The effect of the first was that seemingly different varieties in ordinary Euclidean space, or affine space as it is called, might in fact behave the same when completed in projective space: for example, the three types of smooth conics in $\mathbb{R}^2$ all look like single ovals in $\mathbb{RP}^2$; the difference lies simply in the situation of the "line at infinity" with respect to the projective conic.



Thus, we could say, to understand ordinary plane conics, we should first understand projective conics, whose behavior is more uniform; then consider the different ways in which they may meet a line in $\mathbb{RP}^2$. Somewhat more generally, in relation to the question posed above about arcs and ovals, we may see that a smooth curve of degree d in $\mathbb{RP}^2$ will consist entirely of ovals; the arcs of the curve in $\mathbb{R}^2$ will arise when the line at infinity intersects some of the ovals of the curve.

Similarly, looking at the locus of complex zeros of a polynomial, rather than just the real, has the effect of making uniform their behavior: for example, the polynomials $1 - x^2 - y^2$, $1 + x^2 - y^2$, and $1 + x^2 + y^2$ all have isomorphic zero loci in $\mathbb{C}^2$—after all, they differ only by a complex linear change of variables—even though their zeros in $\mathbb{R}^2$ look different like a circle, a hyperbola, and the empty set, respectively. Again, the implicit idea is to understand conics over $\mathbb{C}$ first, and then to ask what conics over $\mathbb{R}$ may give rise to the same conic over $\mathbb{C}$.

The effect of this change is striking when we consider again the question about the topology of a plane curve X. If we denote by $X(\mathbb{C}) \subset \mathbb{CP}^2$ the closure in $\mathbb{CP}^2$ of the locus of complex solutions of $f(x, y) = 0$—equivalently, the locus of the corresponding homogeneous polynomial—we see that all smooth curves of a given degree d are homeomorphic: they are compact orientable surfaces of genus $(d - 1)(d - 2)/2$, and indeed are isotopically embedded in $\mathbb{CP}^2$.

We can use this information to say something about the real zeros of a real polynomial. The locus $X(\mathbb{R}) \subset \mathbb{RP}^2$ of real points of X in $\mathbb{RP}^2$ is just the set of fixed points of the action of complex conjugation acting on $X(\mathbb{C})$. Thus, if $X(\mathbb{R})$ has δ ovals, the quotient $X(\mathbb{C})/\tau$ will be a 2-manifold with boundary consisting of δ copies of S^1; if we add δ discs D^2 we may complete this to a compact 2-manifold Y. We may then compute the topological Euler characteristic of Y as

$$\chi(Y) = \chi(X(\mathbb{C})/\tau) + \delta = \chi(X(\mathbb{C}))/2 + \delta = -d(d-3)/2 + \delta.$$

But of course $\chi(Y) \leq 2$, and we deduce that

$$\delta \leq \frac{d(d-3)}{2} + 2.$$

Poncelet's theorem similarly appears in a new light when viewed from this vantage point. In fact, it admits a very simple proof, first observed to me by Phillip Griffiths. We look at the incidence correspondence, consisting of pairs:

$$\Gamma = \{(P, L) : P \in D,\ L \text{ is tangent to } C, \text{ and } P \in L\}.$$

This is again an algebraic curve, and when we look at its complex points we find that it is a torus, that is, it is isomorphic to the complex plane $\mathbb{C}$ modulo a lattice Λ. In these terms, we can readily describe the action of passing from one pair (P_i, L_{i+1}) to the next (P_{i+1}, L_{i+2}): it is just a translation in the plane. If this translation has finite order modulo the lattice, every polygon closes up; if not, none do; and so we get Poncelet's theorem.

In this way, the main focus of the subject shifted, in the first half of the 19th century, from varieties in real Euclidean space—real affine varieties—to complex projective ones. It is worth remarking as well that one of the main motivations for this shift was another sort of uniformity of behavior. It was felt by Poncelet, who was instrumental in bringing about both of these changes, that as a general rule intersections of varieties ought to be preserved. Thus, if two lines in general meet in a point, they should continue to do so even if they become parallel; thus the passage to projective space. By the same token, if a line and a conic meet in two points, they should continue to do so, even if we pull them apart; thus the introduction of complex numbers.

Arguments like the ones above about the number of ovals of a plane curve or Poncelet's theorem represent, from one point of view, the completion of a bargain struck when first passing from the fairly natural environment of real plane curves to complex projective ones: you make life easier for yourself by dealing with better-behaved (if less readily visualizable) objects, with the implicit promise of eventually going back and applying what you learn in this way to the original problem. (This bargain has not always been so faithfully kept; new objects tend to suggest new problems, and old ones are easily forgotten. It is embarrassing, for example, when a mathematician working with a hyperbolic PDE in three variables asks a question about real plane curves, how little we know to this day about them.) Without question, these changes opened the door to a new era in algebraic geometry, that culminated in the work of Noether, Segre, Castelnuovo, Enriques, Severi, and others of the Italian school.

"Abstract" algebraic geometry. The basic change from real affine variety to the complex projective one revolutionized the way people thought about algebraic geometry, and there was no going back. One of the reasons these changes stuck was that, while the mental image geometers had of algebraic varieties was altered radically, the formal structure of the subject was much less dramatically altered. Thus, while the words "algebraic curve" conjured up the image of what we would now call a compact Riemann surface, rather than what most people would identify as a curve, many of the old theorems and techniques could still be reproduced word for word in the new context.

Let me explain this in a little more detail, since it is an essential point. Given a collection of polynomials $f_\alpha \in \mathbb{C}[x_1, \dots, x_n]$—or equivalently the ideal $I \subset \mathbb{C}[x_1, \dots, x_n]$ they generate—we associate to them their common zero locus $X = V(I)$. In the other direction, if $X \subset \mathbb{C}^n$ is an algebraic variety we let $I(X) \subset \mathbb{C}[x_1, \dots, x_n]$ be the ideal of polynomials vanishing on X. We thus have a two-way correspondence

$$\{\text{subvarieties of } \mathbb{C}^n\} \underset{V}{\overset{I}{\rightleftarrows}} \{\text{ideals } I \subset \mathbb{C}[x_1, \dots, x_n]\}.$$

Note that this is not by any means bijective: in one direction, the composition of the two is the identity—the definition of a variety $X \subset \mathbb{A}^n$ amounts to the statement that $V(I(X)) = X$—but going the other way the composition is neither injective or surjective. We can fix this up by simply restricting our attention to the image of the map V, and happily there is a nice characterization of this image (and indeed of the composition $I \circ V$); for any ideal $I \subset \mathbb{C}[x_1, \dots, x_n]$, the ideal of functions vanishing on the common zero locus of I is the radical of I, i.e.,

$$I(V(I)) = \operatorname{rad}(I).$$

Thus, there is a bijective correspondence between subvarieties $X \subset \mathbb{C}^n$ and radical ideals $I \subset \mathbb{C}[x_1, \dots, x_n]$.

(Note also that if we replace $\mathbb{C}$ by $\mathbb{R}$, the correspondence breaks down a little further: even a radical ideal in $\mathbb{R}[x_1, \dots, x_n]$ may be nontrivial and still have no common zero locus. Instead, we use this correspondence in effect to *define* the notion of variety over $\mathbb{R}$.)

Now, let $X \subset \mathbb{C}^n$ be a variety and $I(X)$ its ideal. The quotient ring $A = A(X) = \mathbb{C}[x_1, \dots, x_n]/I$ is then called the *ring of regular functions* on X, or the *coordinate ring* of X. (Note that the condition $I = \text{rad}(I)$ is equivalent to saying that the ring $\mathbb{C}[x_1, \dots, x_n]/I$ has no nilpotent elements.) Since X is the common zero locus of the polynomials $f \in I$, X is determined by the ring A; and indeed virtually every property of X may be expressed directly in terms of A rather than of the locus X. For example, a point of X is a maximal ideal in $A(X)$ (in the case of a real variety it is an ideal with residue field $\mathbb{R}$); a map between two such varieties X and Y is exactly a ring homomorphism $A(Y) \to A(X)$ over $\mathbb{C}$; the dimension of X is the transcendence degree of the quotient ring of $A(X)$ over $\mathbb{C}$, and so on. The point is, *pretty much the entire subject can be expressed in terms of the algebra of the rings* $A(X)$. Given that, it is no longer so surprising that the passage from $\mathbb{R}$ to $\mathbb{C}$ involves so little actual reworking of the theory: we would expect homomorphisms between $\mathbb{R}$-algebras A and B to be closely related to homomorphisms between $A \otimes \mathbb{C}$ and $B \otimes \mathbb{C}$, even though the corresponding varieties may be completely different in appearance.

I do not mean, of course, that this passage from the geometric to the algebraic description of algebraic geometry was simply a matter of obvious algebraic analogues of geometric constructions and properties. In fact, it involved a large number of new ideas and techniques. To give you one example, in dealing with compact Riemann surfaces, an object of fundamental importance is its Jacobian variety $J(X)$. This is defined classically as the quotient of complex g-space $\mathbb{C}^g$ by a lattice $\Lambda \subset \mathbb{C}^g$ obtained by integrating a basis of holomorphic 1-forms on X over a collection of cycles forming a basis of the first homology $H_1(X, \mathbb{Z})$. The problem of giving an algebraic construction of this essential object is a serious challenge, and was not solved until Andre Weil. In general, the algebraization of the subject was initiated in earnest in the work of Zariski, starting in the 1920s, and was carried out over a number of decades, reaching in some sense its culmination in the work of Serre.

Of course, having reworked the subject of algebraic geometry in this new context, it may be applied over far more fields than just $\mathbb{R}$ and $\mathbb{C}$. Indeed, this is true to an extent that may seem remarkable at first. After all, a variety over a finite field $k = \mathbb{F}_p$ will consist simply of a finite collection of points; you will not see much difference in the picture of a curve in k^3 and the picture of a surface in k^3. You could argue that this is at least in part because the field k is not algebraically closed, but the fact is that a curve in 3-space over the algebraic closure $\bar{k}$ of $\mathbb{F}_p$ still does not look that much different from a surface; both are just countably infinite collections of points.

Nonetheless, geometric statements about real and complex varieties will, for the most part, still be true in this general setting. For example, there is even a Lefschetz fixed point theorem: we can define a cohomology theory (étale cohomology, developed by M. Artin and Grothendieck) for varieties X over $\mathbb{F}_p$ that mimics the ordinary topological cohomology of a variety over $\mathbb{C}$ (albeit with coefficients in the l-adic numbers $\mathbb{Q}_l$); and then it will be the case that the number of fixed points of an automorphism τ of X will be expressed in terms of the traces of the action of τ on the cohomology groups of X.

Indeed, this is fundamentally related to one of the main constructions of number theory, that of the zeta-function. For X a variety over the field $\mathbb{F}_p$ of p elements, we let N_r be the number of points of X over the field $\mathbb{F}_q$ with $q = p^r$ elements. We may then encode this information in the *zeta-function* of X, defined to be the power series in t:

$$Z(X, t) = \exp\left(\sum N_r \cdot \frac{t^r}{r}\right).$$

If we take the special case where τ is the Frobenius endomorphism, sending each coordinate to its p th power, then the number N_r is just the number of fixed points of the r th power of τ. If τ has eigenvalues $\{\lambda_{i,j}\}$ on $H^i(X)$, then,

$$N_r = \sum (-1)^i \mathrm{Tr}(\tau^r | H^i(X)) = \sum (-1)^i (\lambda_{i,j})^r,$$

so

$$\sum_r N_r \cdot \frac{t^r}{r} = \sum_{i,j,r} (-1)^i \frac{(\lambda_{i,j} \cdot t)^r}{r} = \sum_{i,j} (-1)^{i+1} \log(1 - \lambda_{i,j} \cdot t)$$

and

$$Z(X, t) = \prod_{i,j} (1 - \lambda_{i,j} \cdot t)^{(-1)^{i+1}} = \frac{P_1(t) \cdot P_3(t) \cdot \cdots}{P_0(t) \cdot P_2(t) \cdot \cdots},$$

where $P_i(t) = \det(1 - \tau_i \cdot t)$ is the characteristic polynomial of the action τ_i of τ on $H^i(X)$. We may see in this way that the zeta-function Z is a rational function, a theorem first proved by Dwork; Deligne carried this further to prove the analogue of the Riemann hypothesis for varieties over finite fields, that the P_i were polynomials with integer coefficients and roots of absolute value $p^{-i/2}$.

Actually, I may be overstating the extent to which one should feel surprised that the Lefschetz fixed point theorem holds in this context. After all, the one key ingredient of the Lefschetz theorem is the notion of intersection of cycles on a manifold: the essential step in the proof is the calculation of the intersection of the diagonal in a product $X \times X$ with the graph of a map $f : X \to X$, though it is often couched in the language of cup products. At the same time, intersection of cycles is a theory that existed in algebraic geometry some time before it existed in topology; indeed, it was the presence of this notion in algebraic geometry that supposedly motivated Lefschetz to make the definition in the topological setting.

Finally, in all this talk of the algebraization of the subject, I am ignoring another fundamental shift in the subject: the change from consideration of affine or projective varieties—zero loci of polynomials—to abstract algebraic varieties. This was actually a change common to many branches of mathematics in the early twentieth century: for example, while a group in the nineteenth century meant a subset of either the symmetric or general linear group closed under composition and inverse, the twentieth century introduced the notion of abstract group. Group theory was thus split up into the analysis of abstract groups—what we now think of as group theory—and the study of ways in which a given abstract group could be mapped to the general linear group, or in other words representation theory. Similarly, the notion of abstract algebraic variety—an object locally isomorphic, in a suitable sense, to affine varieties—became the basic object of algebraic geometry.

This did not make for a change so much in the objects studied as in the way they were studied; analogously to the development of group theory, the study of varieties was "factored" into the study of abstract varieties, and then the ways in which a given abstract variety could be embedded in projective space.

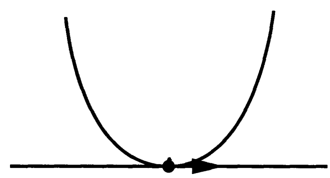
Schemes. We come now to the latest of the revolutions in the subject of algebraic geometry, the introduction of the theory of schemes by Grothendieck in the 1950s and 1960s. The notion of scheme has had tremendous impact, both in a purely geometric and in an arithmetic setting. To a certain extent, it is possible to describe this impact separately in the two settings, and I will try to do this here.

To describe a scheme in the geometric context, recall the basic correspondence introduced earlier between varieties $X \subset \mathbb{C}^n$ and ideals $I \subset \mathbb{C}[x_1, \dots, x_n]$. If one is going to fix up the above correspondence so as to make it bijective, there are naively *two* ways of going about it: we can either restrict the class of objects on the right, or enlarge the class of objects on the left. In classical algebraic geometry, as we have just said, we do the former; in scheme theory, we do the latter. Thus, we more or less *define* an affine scheme $X \subset \mathbb{C}^n$ to be an object associated to an arbitrary ideal $I \subset \mathbb{C}[x_1, \dots, x_n]$. To put it differently, given a finitely generated ring over $\mathbb{C}$—that is, a ring of the form $A = \mathbb{C}[x_1, \dots, x_n]/I$—we create an object, called $\operatorname{Spec} A$, whose ring of functions is the ring A.

What sense can this possibly make? Just as before, this makes sense to the extent that most of the notions that we actually deal with in algebraic geometry may be defined in terms of rings and ideals. For example, if $X \subset \mathbb{C}^n$ is the subscheme with ideal $I = I(X)$, we *define* a function on X to be an element of the ring $A(X) = \mathbb{C}[x_1, \dots, x_n]/I$; the intersection of two such varieties $X, Y \subset \mathbb{C}^n$ is given by the join of their ideals; the data of a map between two such varieties X and Y is equivalent to the data of a map

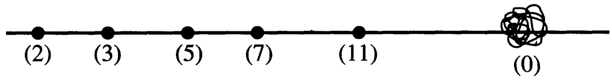
$\varphi: A(Y) \to A(X)$; a point of X is a prime ideal p in $A(X)$; the fiber of the map $X \to Y$ given by $\varphi: A(Y) \to A(X)$ over a point $p \in X$ is the subscheme of Y corresponding to the ring $A(Y)/\varphi^{-1}(p)$, and so on. The point is, all these things make as much sense whether or not I is a radical ideal.

In these circumstances, for example, if we wanted to intersect the line $(y = 0)$ with the conic curve $(y = x^2)$, we would take the intersection to be the object $X = \operatorname{Spec} \mathbb{C}[x, y]/(y, x^2) \subset \mathbb{C}^2$ in the affine plane defined by the ideal $(y, y - x^2) = (y, x^2)$. This object has only one point, but it is not the same as the point defined by the ideal (x, y): we simply declare a function on X to be an element of the quotient ring $\mathbb{C}[x, y]/(y, x^2) = \mathbb{C} \oplus \mathbb{C} \cdot x$—that is, an expression of the form $a + bx$. In other words, we say that a function $f(x, y)$ on the plane vanishes on X if and only if it vanishes at the point $(0, 0)$ and has normal derivative $\partial f/\partial x$ zero at $(0, 0)$ as well.



It is interesting to note that one justification for this generalization of the notion of variety comes from the same source as Poncelet's. Again, consider a line and a conic in the plane, and suppose now that the line becomes tangent to the conic. As before, we would like to say that there are still two points of intersection of the two. Classically, it was just said that the line and the conic intersected at the one point "with multiplicity 2," but this is unsatisfactory from a number of points of view. Scheme theory gives us a way of refining it: we say that the intersection of the line (x) with the conic $(x - y^2)$ is the scheme given by the ideal $(x, x - y^2) = (x, y^2)$. This not only conveys the multiplicity of intersection in the fact that the ideal is not radical, it tells us also the direction from which the two points that coalesced into this one point came.

The second, arithmetic, impact of the notion of scheme arises from a further generalization. To put it simply, we may observe that in the construction of the scheme $\operatorname{Spec} A$ *there does not need to be a ground field at all*: the ring A in general need not contain any field. Thus, for example, we have a fundamentally important scheme $\operatorname{Spec} \mathbb{Z}$, whose points (except for (0)) correspond to the prime numbers.



In effect, then, we are treating the integers as variables—as functions on our space $\operatorname{Spec}\mathbb{Z}$. This turns out to be one of the most crucial points in the application of schemes to number theory. For example, a diophantine problem—in other words, a variety defined by polynomials with integer coefficients such as $y^2 = x^3 + 1$—will give rise to a scheme $X = \operatorname{Spec}(\mathbb{Z}[x, y]/(y^2 - x^3 - 1))$. The inclusion $\mathbb{Z} \subset \mathbb{Z}[x, y]/(y^2 - x^3 - 1)$ then gives a map $X \to \operatorname{Spec}(\mathbb{Z})$, whose fibers (as loosely defined above) are exactly the reductions of the original equation modulo the primes. Nor does the ring have to be finitely generated; geometric objects associated to rings such as power series rings are extremely useful as auxiliary objects in algebraic geometry.

Needless to say, for every "why not" I toss off blithely here, a tremendous amount of foundational work is implicit. For example, consider again the Jacobian of a curve: now that we have described a curve as an object fibered over $\operatorname{Spec}\mathbb{Z}$, its Jacobian should be one as well. Actually constructing such an object—showing it exists and has the functorial properties we want—is a project of major proportions (it took Steve Kleiman essentially a semester to describe his solution of this problem in a course I attended). The need for this sort of foundational material has given the subject, unavoidably, a reputation for technical difficulty and inaccessibility. On the other hand, it would be hard to overestimate the power of the ideas implicit in these notions. After all, it is worth bearing in mind, to most mathematicians of the early 19th century the notion of a complex projective variety must have seemed more than a little forbidding as well.

Let me finish by considering what may lie ahead. If you are comfortable with the thesis presented here, that progress in algebraic geometry is reflected as much in its definitions as in its theorems, the natural question to ask is what objects algebraic geometers will be studying in the next century. Currently there are two notions abroad that make a claim to be the natural successor of the notion of scheme (and Manin has even suggested that they should be amalgamated).

The first is the notion of *compactified arithmetic scheme*, developed by Arakelov, Faltings, and others. In this, we take a scheme of finite type over $\mathbb{Z}$ and add additional structure to it: we throw in the data of a Kähler metric on the "fiber at infinity." This additional structure in some sense addresses the problem that there is no "compactification" of $\operatorname{Spec}\mathbb{Z}$ to a projective scheme and allows us to tie together many of the phenomena associated to individual primes. For example, the classical fact that the total degree of a rational function on a projective curve—that is, the same number of zeros minus the number of poles—is zero translates into the product formula, that the product of the valuations of an element of a number field at all primes (including the infinite ones) is one. The notion of arithmetic scheme is, as you might expect, of special interest to number theorists, and indeed played a role in Faltings' proof of the Mordell conjecture.

The second new notion is that of a *superscheme*. This is a generalization of the notion of scheme, in which we loosen still further the strictures on the rings we consider: we no longer require that it be commutative. This is not to say that we look at arbitrary noncommutative rings; rather, we look at rings with a $\mathbb{Z}/2$-grading and require that they be skew-commutative, in the sense that for x and y homogeneous, $x \cdot y = (-1)^{\deg(X)\deg(Y)} y \cdot x$. Thus, a commutative ring represents the special case where the grading is trivial; and in general (if we are not in characteristic 2) the odd graded piece will consist of nilpotents, though nilpotents that behave very differently than those considered in the context of "classical" schemes. Much of the motivation for the study of superschemes comes from physics, though the questions that arise in trying to carry standard algebraic geometry over into this new context seem interesting in their own right.

Does either of these two notions embody the future of algebraic geometry; does it lie in some other direction altogether, or will the future bring about a return to the classical questions of the subject? The only general pattern to the development of the subject thus far seems to be a gradual but consistent trade-off of naive geometric intuition for a formal unity (in each case, met with cries of, "It may be a pretty theory, but it's not geometry!"). Whether this continues, how far and in what direction, is anybody's guess.

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