ARRANGE YOUR OWN SABBATICAL EXCHANGE: USE SEIS

AMERICAN MATHEMATICS FROM 1940 TO THE DAY BEFORE YESTERDAY*

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Preface. What is the best way to present the small fragment of history described by the title above? Should this report occupy itself mainly with the statistics of the growth of Mathematical Reviews?, with the lives of mathematicians?, with lists of books and papers?, or with retracing the influences and implications that led from the bridges of Königsberg first to *analysis situs* and then to homological algebra? We decided to do none of these things, but, instead, to tell as much as possible about mathematics, the live mathematics of today. To do so within prescribed boundaries of time and space, we present the subject in the traditional "battles and kings" style of history. We try to describe some major victories of American mathematics since 1940, and mention the names of the winners, with, we hope, enough explanation (but just) to show who the enemy was. The descriptions usually get as far as statements only. We omit all proofs, but we sometimes give a brief sketch of how a proof might go. A sketch can be one sentence, or two or three paragraphs; its purpose is more to illuminate than to convince.

Progress in mathematics means the discovery of new concepts, new examples, new methods, or new facts. Schwartz's concept of distribution, Milnor's example of an exotic sphere, Cohen's method of forcing, and the Feit–Thompson theorem about simple groups are surely major by any standards. It was no trouble to find such victories to include in our list; the difficulty was to decide what to exclude. We formulated some rough rules (e.g., theorems, not theories); since at least some aspects of applied mathematics were covered by other presentations, we restricted our attention to pure mathematics; we excluded work that had neither root, nor branch, nor flower in the U.S.; and, in deciding which of two candidates to keep, we leaned toward the one of greater general interest. ("Of general interest" is not quite the same as "famous", but it's close.)

We ended up with ten "battles and kings", and we think that they draw a fair picture of what's been happening. We do not say that our ten are greater than any others, nor that they are necessarily maximal in the mathematical sense of not being lesser than any others. We do say that they would all appear, and would be discussed with respect, in any responsible history of our place and time. The total number of such "non-omittable" victories is certainly greater than ten; it may be twenty or even forty. The choice of our ten was influenced by the limits of our competence and by our personal preferences; that could not be helped. Anyone else would very likely have selected a different set of

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ten. We hope and think, however, that everyone's list would have a large overlap with ours, and that the local differences would not essentially alter the global picture.

In history, every moment influences its successors; to restrict attention to a time interval may be often necessary, and sometimes possible, but it is rarely natural. In the same way, every place influences all others. Since the topology of the surface of our globe is much more intricate than that of the time line, to restrict attention to one country is almost impossible. The history of mathematics is no exception: trying to describe what happened *here*, we frequently yield to the pressure of distant influences and discuss what happened *there*. We were able to stay reasonably close to our original charge just the same; if fractional credits are assigned, something like 8.25 of the ten accomplishments described below can be called American. It might be of interest to observe also that over half of the original papers we refer to appeared in the *Annals of Mathematics*.

The order of the presentations might have been based on any of several principles (e.g., what actually happened first?, what is a prerequisite for what?). We decided to arrange them in order of complexity of the underlying category, or, in other words, very roughly speaking, in order of distance from the foundations of mathematics. At the end of each section there is a small list of pertinent references. The list is intentionally incomplete. All it contains is one (or, if necessary, two or three) of the earliest papers in which the discovery appears, and a more recent exposition of the discovery whenever we could find one.

Continuum Hypothesis. All mathematics is derived from set theory (or, in any event, many of us believe it is) and the manipulation of sets is a simple, natural exercise (or, in any event, students have very little trouble catching on to it). Everything that any working mathematician ever needs to know about sets (and a few extra things that he never thought he needed to know) could be summarized on one printed page (or three or four printed pages, if motivation is wanted along with the formalism). Such a page would state the basic ways of making new sets out of old (e.g., the formation of sets consisting of specified elements, the formation of unions of sets of sets, and the formation of the power set, i.e., the set of all subsets, of a set); it would describe the basic properties of sets (e.g., that two sets are equal if and only if each is a subset of the other, and that no set has elements that are themselves sets that have elements continued on downwards *ad infinitum*); and it would state (as an assumption or as a conclusion, but in either case as a description of the universe that sets live in) that infinite sets exist. These basic set-theoretic statements might be regarded either as obvious factual observations or as an axiomatic description of the ZF (Zermelo–Fraenkel) structure. In either case it would be a simple matter to code them in the language of a suitable (not very complicated) computer. Such a machine could easily be taught all the rules of inference that mathematicians ever use. If, in addition, its basic data were increased by two more statements, it could, in principle, easily print out all known mathematics (and a lot that is not yet known).

The two statements that history has subjected to extra scrutiny are AC (the axiom of choice) and GCH (the generalized continuum hypothesis). AC says that, for each set X, there is a function f from the power set of X into X itself such that $f(A) \in A$ for each non-empty subset A of X; GCH says that each subset of the power set of an infinite set X is in one-to-one correspondence either with some subset of X or with the entire power set — there is nothing in between.

Is AC true? The question has often been likened to a similar one about Euclid's parallel postulate. In both cases there is a more or less pleasant axiom system and a less pleasant, more complicated, non-obvious additional axiom. If the extra axiom is a consequence of the basic ones, it is true, and all is well; if its negation is a consequence of the basic ones, it is false, and, for better or for worse, the question is definitively answered. The same question can, of course, be asked about GCH. It has long been known that GCH implies AC; in view of this there is an obvious connection between the two answers.

The answers are subtle and profound intellectual achievements. Gödel proved (1940) that AC and GCH are not false (i.e., that they are consistent with the axioms of ZF), and Paul Cohen proved (1964) that they are not true (i.e., that they are independent of ZF).

Gödel argued by the construction of a suitable model. If, he said, ZF is consistent, so that there is a universe V of sets satisfying the basic axioms of ZF, then, he proved, there is a "sub-universe" that also satisfies them, and in which, moreover, both AC and GCH are true. The sub-universe Gödel constructed was the class L of "constructible" sets. (The word is given a very liberal but completely precise meaning; roughly speaking, the constructible sets are the ones that can be obtained from the empty set by a transfinite sequence of elementary set-theoretic constructions.) The class L is a substructure of V in the familiar mathematical sense of that word: the objects of L are some of the objects of V, and the relation $\in$ among them is the restriction of the set-theoretic $\in$ in V to the objects of L. The existence of a model such as L (constructed out of a hypothetically consistent model V) proves the consistency of AC and GCH the same way as the existence of the Euclidean plane proves the consistency of the parallel postulate.

Cohen's argument was similar but harder. It is reminiscent of Felix Klein's construction of a Lobachevskian plane by endowing a Euclidean disk with a new metric. Cohen started with a suitable model of ZF and adjoined new objects to it. The new objects are "classes" (but not sets) in the old model. The adjunctions proceed by a new method called "forcing", which, once it was discovered, was found to be applicable in many parts of set theory. Cohen's proof constructs an infinite sequence of better and better finite approximations to the new objects. Roughly speaking, each property of the new model is "forced" by properties of the old model and one of the approximations. Depending on how the details are adjusted, the end result can be a model of ZF in which AC is false, or a model of ZF in which AC is true but even the classical un-generalized continuum hypothesis CH is false. (CH is GCH for a countably infinite set.) Conclusion: AC and CH are independent of ZF.

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Diophantine Equations. The continuum hypothesis was the subject of Hilbert's first problem (in the famous list of 23 problems that he proposed in 1900); Hilbert's tenth problem concerned the solvability of Diophantine equations. The problem was to design an algorithm, a computational procedure, for determining whether an arbitrarily prescribed polynomial equation with integer coefficients has integer solutions. It is in some respects more natural and sometimes technically easier to discuss the *positive* integer solutions (solutions in $\mathbb{Z}_+$) of polynomial equations with *positive* integer coefficients. Caution: that does not mean equations such as $p(x) = 0$ only. The problem includes the search for x's such that $p(x) = q(x)$; more generally, it includes the search for n-tuples $(x_1, \ldots, x_n)$ such that $p(x_1, \ldots, x_n) = q(x_1, \ldots, x_n)$; and, in complete generality, it means the search for n-tuples $(x_1, \ldots, x_n)$ for which there exist m-tuples $(y_1, \ldots, y_m)$ such that

$$p(x_1, \ldots, x_n, y_1, \ldots, y_m) = q(x_1, \ldots, x_n, y_1, \ldots, y_m).$$

For each p and q (in $n + m$ variables) the solution set, in the latter sense, is called a "Diophantine set" in $\mathbb{Z}_+^n$.

What does it mean to say that there is an algorithm for deciding solvability? A reasonable way to answer the question is to offer a definition of computability for sets and functions, and then to define an algorithm in terms of computability.

When does a function from $\mathbb{Z}_+$ to $\mathbb{Z}_+$, or, more generally, a function from $\mathbb{Z}_+^n$ to $\mathbb{Z}_+$ deserve to be called "computable"? There is general agreement on the definition nowadays: computable functions (also called "recursive" functions) are the ones obtained from certain easy functions (constant, successor, coordinate) by three procedures (composition, minimalization, primitive recursion). The details do not matter here (they won't be used anyway); it might be comforting to know, however, that they are not at all difficult. A set (in $\mathbb{Z}_+$, or, more generally, in $\mathbb{Z}_+^n$) will be called computable in case its characteristic function is computable. Consequence: a set (in $\mathbb{Z}_+^n$) is computable if and only if its complement is computable.

Consider now all polynomial equations (in the sense described above), and let $\{E_1, E_2, E_3, \ldots\}$ be an enumeration of them. (In order for what follows to be in accord with the intuitive concept of an algorithm, the enumeration should be "effective" in some sense. That can be done, and it is relatively easy.) The indices k for which E_k has a solution (in the sense described above) form a subset S of $\mathbb{Z}_+$. The Hilbert problem (is there an algorithm?) can be expressed as follows: is S a computable set? The answer is no. The answer was a long time coming; it is the result of the cumulative efforts of J. Robinson (1952), M. Davis (1953), H. Putnam (1961), and Y. Matijasevič (1970).

The central concept in the proof is that of a Diophantine set, and the major step proves that every computable set is Diophantine. The techniques make ingenious use of elementary number theory (e.g., the Chinese remainder theorem, and a part of the theory of Fibonacci numbers, or, alternatively, of Pell's equation). The proof exhibits some interesting Diophantine sets whose Diophantine character is not at all obvious (e.g., the powers of 2, the factorials, and the primes).

One way to prove that S (the index set of the solvable equations) is not computable is by contradiction. If S were computable, then it would follow (by a slight bit of additional argument) that each particular Diophantine set (i.e., the solution set of each particular equation) is computable, and hence (by the "major step" of the preceding paragraph) that the complement of every Diophantine set is Diophantine. The contradiction is derived by exhibiting a Diophantine set whose complement is not Diophantine.

This last step uses a version of the familiar Cantor diagonal argument. The idea is "effectively" to enumerate all Diophantine subsets of $\mathbb{Z}_+$, as $\{D_1, D_2, D_3, \ldots\}$, say, prove that the set $D^* = \{n\colon n \in D_n\}$ is Diophantine (that takes some argument), and, finally, to prove that the complement $\mathbb{Z}_+ - D^* = \{n\colon n \notin D_n\}$ is not Diophantine (that's where Cantor comes in).

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Simple Groups. So much for the foundations. The next subject up the ladder is algebra; in the present instance, group theory.

Every group G has two obvious normal subgroups, namely G itself and, at the other extreme, the subgroup 1. A group is called "simple" if these are all the normal subgroups it has.

Simple groups are like prime numbers in two ways: they have no proper parts, and every finite group can be constructed out of them. (By general agreement the trivial positive integer 1 is not called a prime, but the trivial group 1 is called simple. Too bad, but that's how it is.)

Suppose, indeed, that G is finite, and let G_1 be a maximal normal subgroup of G. (To say that G_1 is maximal means that G_1 is a proper normal subgroup of G that is not included in any other proper normal subgroup of G.) If G is simple, then $G_1 = 1$; in any event, the maximality of G_1 implies that the quotient group G/G_1 is simple. The relation between G, G_1, and G/G_1 (group, normal subgroup, quotient group) is sometimes expressed by saying that G is an extension of G/G_1 by G_1. In this language, every finite group (except the trivial group 1) is an extension of a simple group by a group of strictly smaller order. The statement is a group-theoretic analogue of the number-theoretic one that says that every positive integer (except 1) is the product of a prime by a strictly smaller positive integer.

If G_1 is not trivial, the preceding paragraph can be applied to it; the result is a maximal normal subgroup G_2 in G_1, such that G_1 is an extension of the simple group G_1/G_2 by G_2. The procedure can be repeated so long as it produces non-trivial subgroups; the end-product is a chain

$$G = G_0 \supset G_1 \supset G_2 \supset \cdots \supset G_n = 1$$

(a "composition series") with the property that each G_i/G_{i+1} is simple ($i = 0, \ldots, n-1$). A great part of the problem of getting to know all finite groups reduces in this way to the determination of all finite simple groups. (The celebrated Jordan-Hölder-Schreier theorem is the comforting reassurance that, to within isomorphism, the composition factors G_i/G_{i+1} are uniquely determined by G, except for the order in which they occur.)

The abelian ones among the finite simple groups are easy to determine: they are just the cyclic groups of prime order. That's easy. What's hard is to find all non-abelian ones. Some examples of simple groups are easy to come by; among permutation groups, for instance, the most famous ones are the alternating groups of degree 5 or more. The known simple groups did not exhibit any pattern, and even the simplest questions about them resisted attack. Burnside conjectured, for instance, that every non-abelian simple group has even order, but that conjecture stood as an open problem for more than 50 years.

In a spectacular display of group-theoretic power, Feit and Thompson (1963) settled Burnside's conjecture (it is true). The proof occupies an entire issue (over 250 pages) of the *Pacific Journal.* It is technical group theory and character theory. Some reductions in it have been made since it appeared, but no short or easy proof has been discovered. The result has many consequences, and the methods also have been used to attack many other problems in the theory of finite groups; a subject that was once pronounced dead by many has shown itself capable of a vigorous new life.

REFERENCE. W. Feit and J. G. Thompson, Solvability of groups of odd order, Pac. J. Math., 13(1963) 775–1029 (MR29 # 3538).

Resolution of Singularities. Algebra becomes richer, and harder, when it is mixed with and applied to geometry; one of the richest mixtures is the old but very vigorous subject known as algebraic geometry. This section reports the solution of an old and famous problem in that subject.

Let k be an algebraically closed field, and let k^n be, as usual, the n-dimensional coordinate space over k. (The heart of the matter in what follows will be visible to those who insist on sticking to the field of complex numbers in the role of k.) An "affine algebraic variety" V in k^n is the locus of common zeros of a collection of polynomials in n variables with coefficients in k. Since only the zeros matter, the collection itself is not important; it can be replaced by any other collection that yields the same locus. Thus, if R is the ring of *all* polynomials in n variables with coefficients in k, and if I is the ideal in R generated by the prescribed collection, then I will define the same variety; there is, therefore, no loss of generality in assuming that the collection was an ideal to begin with.

The objects of interest on varieties are their "singular points". Intuitively, these are points where the "tangent vectors" are not as they should be. Consider, for example, the curves defined by

$$y^2 = x^3 + x^2 \quad \text{and} \quad y^2 = x^3.$$

(Since the ground field was restricted to be algebraically closed, the *real* planar curves with these equations are not the right things to look at, but they are more lookable at than the complex curves, which lie in the complex plane. Warning: the complex plane has four real dimensions. To the algebraic geometer, the familiar "complex plane" of analysis is the complex *line.*) The first of these comes in to the origin from the first quadrant with slope 2, has a loop in the left half plane, and goes out from the origin to the fourth quadrant with slope -2; it has the origin as a double point. The other one comes in to the origin from the first quadrant with slope 0, and goes out the same way to the fourth quadrant; it has the origin as a cusp.

The effective way to deal with singular points begins by giving a purely algebraic description of them. Consider, for this purpose, the ring R_V of polynomial functions on V (i.e., the restrictions of the polynomials in R to V). If N_V is the ideal of R consisting of the polynomials that vanish on V, then, clearly, $R_V = R/N_V$. Each point $\alpha = (\alpha_1, \ldots, \alpha_n)$ of V induces a maximal ideal N_α in R (consisting of the set of polynomials that vanish at α); clearly $N_V \subset N_\alpha$.

The next step (in the program of defining singular points algebraically) is to form a new ring that studies the local behavior of functions near α. The idea is (very roughly) this. (i) Consider pairs (U, f), where U is a "neighborhood" of α and f is a rational function with no poles in U. (ii) Define an equivalence relation for pairs by writing $(U, f) \sim (U', f')$ exactly when there is a neighborhood U'' of α, included in $U \cap U'$, such that $f = f'$ on U''. (iii) The equivalence classes ("germs") form a ring (with, for example,

$$[(U, f)] + [(U', f')] = [(U \cap U', f + f')]),$$

called the "local ring" of V at α.

From the algebraic point of view, the preceding topological considerations are just heuristic; they will now be replaced by an algebraic construction. The process is, appropriately, called "localization". (i) Consider pairs (f, g), where f and g are in R and $g \notin N_\alpha$. (ii) Define an equivalence relation for pairs by writing $(f, g) \sim (f', g')$ exactly when there is an h not in N_α such that $h \cdot (fg' - gf') = 0$. (iii) Write f/g for the equivalence class of (f, g). The equivalence classes form a ring R_α (with the usual rules of operations for fractions). The ring R_α is indeed a "local ring" in the customary algebraic sense: it has a *unique* maximal ideal, namely the one formed by the elements of R_α that vanish at α.

To motivate the next step, pretend, again, that the subject is not algebraic geometry, but analytic geometry. In that case R_α would consist of Taylor series at α convergent near α, and the ideal N_α of germs vanishing at α would consist of the Taylor series at α with vanishing constant term. The linear terms of a Taylor series are, in some sense, first order differentials. One way to capture just those terms is to "ignore" higher order terms. More precisely: consider the ideal N_α^2, which, in the analytic case, consists of the Taylor series with vanishing constant term and vanishing linear term, and form N_α / N_α^2.

The definition is now easy to formulate. The "dimension" d of V is, by definition, the minimum of the dimensions (over the field k, of course) of all the quotient spaces N_α / N_α^2; a point α is "singular" when $\dim (N_\alpha / N_\alpha^2) > d$. It is not difficult to see that for the two curves mentioned as examples above, the origin is indeed a singular point in the sense of this definition.

One of the main problems of algebraic geometry is to "get rid of" singular points. For this purpose the discussion is restricted to "irreducible" varieties, i.e., to the ones for which R_V is an integral domain, or, equivalently, N_V is a prime ideal. In that case, form the field of fractions F_V of R_V. Two varieties V and W are "birationally equivalent" if F_V and F_W are isomorphic. This means roughly that V and W parametrize one another by rational mappings at all but finitely many places. The problem of "resolution of singularities" is that of finding a non-singular variety birationally equivalent to V.

The subject has a long history. Curves were handled by Max Noether in the 19th century. Surfaces were the subject of much geometric discussion by the Italian school; a rigorous proof was found by R. J. Walker (1935). For varieties of arbitrary dimension, over fields of characteristic 0, the final victory was inspired by Zariski's work; it was won by Hironaka (1964).

REFERENCE. H. Hironaka, Resolution of singularities of an algebraic variety over a field of characteristic zero, Ann. Math., 79(1964) 109–326 (MR 33 # 7333).

Weil Conjectures. The mathematician's work is often most difficult (and most rewarding) when he reasons by analogy, when he guesses that *this* situation ought to be just like *that* one: In 1949 A. Weil, reasoning in this way, proposed three conjectures that have profoundly influenced the development of algebraic geometry over the past 25 years.

The conjectures appeared in a paper entitled "Numbers of solutions of equations in finite fields", which was ostensibly a survey of previous work. Counting the number of solutions of a polynomial equation in several variables over a finite field was a classical problem, investigated by Gauss, Jacobi, Legendre, and others, but Weil took a new point of view. To understand his approach, consider the special case of the homogeneous equation

$$a_0x_0^n + a_1x_1^n + \cdots + a_rx_r^n = 0, \tag{†}$$

where the coefficients a_i are in the prime field F of p elements. The basic problem is to count the number of solutions in F, but, to number theorists, it is just as important to count the number of solutions in any finite extension field of F. Recall that, for every positive integer k, there is a unique extension field F_k of F with p^k elements. What Weil did was to count the number of solutions of (†) in each field F_k, and then code that information in a generating function.

To do this economically, examine the solution set of an equation such as (†). There is, of course, always the trivial solution, where all the x_i are zero; that one is justly regarded as trivial. If $(x_0, x_1, \ldots, x_r)$ is a non-trivial solution, and if $0 \neq c \in F_k$, then $(cx_0, cx_1, \ldots, cx_r)$ is also a non-trivial solution. Each non-trivial solution generates in this way $p^k - 1$ others, and there is no virtue in counting them all separately. It is natural, therefore, to consider the r-dimensional "projective space" $P^r(F_k)$, i.e., the set of non-trivial ordered $(r+1)$-tuples of elements of F_k, where two are identified if one is a scalar multiple of the other. (This is exactly analogous to the familiar real and complex projective spaces.) The problem in these terms is to count the number of "points" in $P^r(F_k)$ that are "solutions" of (†).

That is precisely what Weil did. He let N_k be the number of solutions of (†) in $P^r(F_k)$, considered the generating function G,

$$G(u) = \sum_{k=1}^{\infty} N_k u^{k-1},$$

and proved a remarkable statement: G is the logarithmic derivative of a *rational* function. That is: there exists a rational function Z such that

$$\sum_{k=1}^{\infty} N_k u^{k-1} = \frac{d}{du} \log Z(u),$$

or, in other words, if

$$Z(u) = \exp\left(\sum_{k=1}^{\infty} \frac{N_k}{k} u^k \right),$$

then Z is rational. The function Z satisfies a functional equation analogous to the one satisfied by the Riemann zeta function, and it is appropriate to refer to Z as the zeta function associated with the equation (†). Motivated by classical problems that the Riemann zeta function gave rise to, Weil studied and was able to determine many properties of the zeros and the poles of Z.

Here is where Weil's paper reaches its climax. Weil wanted to extend the results about (†) to algebraic varieties in $P^r(F_k)$, i.e., to the solution sets of *systems* of homogeneous equations in r variables. The notion of a zeta function, originally defined by Riemann, was extended by Dedekind to algebraic number fields, by Artin to function fields, and now, by Weil, to algebraic varieties. (The varieties to be considered should be non-singular. It doesn't matter here what the general definition of that condition is; for most fields it can be defined as usual by requiring that the Jacobian of the system of equations have maximal rank at every point.) Given a system of equations, with coefficients in F, let N_k be, as before, the number of solutions in $P^r(F_k)$. Weil advanced the following conjectures. One: the function Z, defined as before by

$$Z(u) = \exp\left(\sum_{k=1}^{\infty} \frac{N_k}{k} u^k \right),$$

is rational. Two: Z satisfies a particular functional equation, which, as before, bears a striking resemblance to the one satisfied by the Riemann zeta function. Three: the reciprocals of the zeros and the poles of Z are algebraic integers and their absolute values are powers of $\sqrt{p}$. (This is called the generalized Riemann hypothesis.)

All this might seem far removed from what is normally thought of as geometry, and, although several examples were known, it might seem that Weil made his conjectures on strikingly little evidence. What was really behind the conjectures? The answer is contained in the last paragraph of Weil's paper, where he suggests that there is an analogy between the behavior of these varieties (for fields of characteristic p) and that of the classical varieties (for the field of complex numbers).

In 1960 Dwork established the rationality conjecture (without the condition of non-singularity). The final triumph came in 1974: using twenty years' of results of the Grothendieck school, Deligne established all the Weil conjectures, and, perhaps more importantly, proved that there is a beautiful connection between the theory of varieties over fields of characteristic p and classical algebraic geometry. "God ever geometrizes", said Plato, and "God ever arithmetizes", said Jacobi; the Weil conjectures show, better than anything else, how He can do both at once.

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Lie Groups. So much for algebra, with or without geometry. The next subject points toward some of the later analytic ones by mixing algebra with topology. The result, like a few other outstanding results of mathematics, seems to get something for nothing, or, at the very least, to get quite a lot for an astonishingly low price. One of the most famous results of this kind occurs in the early part of courses on complex function theory: it asserts that a differentiable function on an open subset of the complex plane is necessarily analytic.

Hilbert's fifth problem asked for such a something-for-nothing result. The context is the theory of topological groups. A topological group is a set that is both a Hausdorff space and a group, in such a way that the group operations

$$(x, y) \mapsto xy \quad \text{and} \quad x \mapsto x^{-1}$$

are continuous. A typical example is the set of all 2×2 real matrices of the form $\begin{pmatrix} x & y \\ 0 & 1 \end{pmatrix}$ with $x > 0$; the topological structure is that of the right half plane (all (x, y) with $x > 0$), and the multiplicative structure is the usual one associated with matrices. Equivalently: define multiplication in the right half plane by

$$(x, y) \cdot (x', y') = (xx', xy' + y);$$

since

$$(x, y)^{-1} = \left(\frac{1}{x}, \frac{-y}{x}\right),$$

it is clear that both multiplication and inversion are continuous.

This example has an important special property: it is "locally Euclidean" in the sense that every point has a neighborhood that is homeomorphic to an open ball in (2-dimensional) Euclidean space. (Equivalently: every point has a "local coordinate system".) An even more important special property of the example is that the group operations, regarded as functions on the appropriate Euclidean space, are not only continuous but even analytic. If a group is locally Euclidean, i.e., if it can be "coordinatized" at all, then there are many ways of coordinatizing it; if at least one of them is such that the group operations are analytic, the group is called a "Lie group". Hilbert's fifth problem was this: is every locally Euclidean group a Lie group?

The analogy of this problem with the one in complex function theory is quite close. It is relatively elementary that a twice-differentiable function is analytic; it has been known for a long time that if a topological group has sufficiently differentiable coordinates, then it has analytic ones.

Immediately after the discovery of Haar measure, von Neumann (1933) applied it to prove that the answer to Hilbert's question is yes for compact groups. A little later Pontrjagin (1939) solved the abelian case, and Chevalley (1941) solved the solvable case. (Sorry about that, but "solvable" is a technical word here and its use is unavoidable.)

The general case was solved in 1952 by Gleason and, jointly, by Montgomery and Zippin; the answer to Hilbert's question is yes. What Gleason did was to characterize Lie groups. (Definition: a topological group "has no small subgroups" if it has a neighborhood of the identity that includes no subgroups of order greater than 1. Characterization: a finite-dimensional locally compact group with no small subgroups is a Lie group.) Montgomery and Zippin used geometric-topological tools (and Gleason's theorem) to reach the desired conclusion.

Warning: the subject cannot be considered closed. The question can be generalized in ways that are both theoretically and practically valuable. Groups can be replaced by "local groups", and abstract groups can be replaced by groups of transformations acting on manifolds. The best kind of victory is the kind that indicates where to look for new worlds to conquer, and the one over Hilbert's fifth problem was that kind.

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Poincaré Conjecture. A "manifold" is a topological space (a separable Hausdorff space to be exact) that is locally Euclidean. Manifolds have been the central subject of topology for many years, and still are. Hilbert's fifth problem was about group manifolds; the Poincaré conjecture is about the connectedness properties of smooth manifolds. A "differential manifold" is a manifold endowed with local coordinate systems such that the change of coordinates from one coordinate neighborhood to an overlapping one is smooth. "Smooth" in this context is a generally accepted abbreviation for C^∞, i.e., for infinitely differentiable.

The axioms of Euclidean plane geometry characterize the plane. This kind of activity (find the central core of a subject, abstract it, and use the result as an axiomatic characterization) is frequent and useful in mathematics. Since spheres are the principal concept of a large part of topology, it is natural to try to subject them too to the axiomatic approach. The attempt has been made, and, to a large extent, it was successful.

The 1-sphere, for instance (i.e., the circle), is a compact, connected 1-manifold (i.e., a manifold of dimension 1), and that's all it is: to within a homeomorphism every compact connected 1-manifold is a 1-sphere.

For the 2-sphere, the facts are more complicated: both the 2-sphere S^2 and the torus T^2 ($= S^1 \times S^1$) are compact connected 2-manifolds, and they are not homeomorphic to each other. To distinguish S^2 from T^2 and, more generally, from a sphere with many handles, it is necessary to observe that, although both S^2 and T^2 are connected, S^2 is more connected. In the appropriate technical language, S^2 is "simply connected" and T^2 is not. The relevant definitions go as follows. Suppose that X and Y are topological spaces and that f and g are continuous functions from X to Y; write I for the unit interval $[0,1]$. The functions f and g are "homotopic" if there exists a continuous function h from $X \times I$ to Y such that $h(x,0) = f(x)$ and $h(x,1) = g(x)$ for all x. (Intuitively: f can be continuously deformed to g.) The space Y is simply connected if every continuous function from S^1 to Y is homotopic to a constant. (Intuitively: every closed curve can be shrunk to a point.) Once this concept is at hand, the characterization of the 2-sphere becomes easy to state: to within a homeomorphism, every compact, connected, simply connected 2-manifold is a 2-sphere.

The discussion of dimensions 1 and 2 does not yet provide a firm basis for guessing the general case, but it does at least make the following concept plausible. There is a way of defining "k-connected" that generalizes "connected" ($k = 0$) and "simply connected" ($k = 1$): just replace S^1 in the definition of simple connectivity by S^j, $j = 0, 1, \ldots, k$. Thus: a space Y is k-connected if, for each j between 0 and k inclusive, every continuous function from S^j to Y is homotopic to a constant.

The general Poincaré conjecture is that if a smooth compact n-manifold is $(n-1)$-connected, then it is homeomorphic to S^n. For $n = 1$ and $n = 2$ the result has been known for a long time; the big recent step was the proof of the assertion for all $n \geqq 5$. The proof was obtained by Smale (1960). Shortly thereafter, having heard of Smale's success, Stallings gave another proof for $n \geqq 7$ (1960) and Zeeman extended it to $n = 5$ and $n = 6$ (1961). For $n = 3$ (the original Poincaré conjecture) and for $n = 4$ the facts are not yet known.

Actually Smale proved a much stronger result. He showed how certain manifolds could be obtained by gluing disks together. His results provide a starting point for a classification of simply connected manifolds.

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Exotic Spheres. A "diffeomorphism" between two differential manifolds is a homeomorphism such that both it and its inverse are smooth. Homeomorphism is an equivalence relation between manifolds; the equivalence classes (homeomorphism classes) consist of manifolds with the same topological properties. Similarly, diffeomorphism is an equivalence relation between differential manifolds, and the equivalence classes (diffeomorphism classes) consist of manifolds with the same differential properties. Are these concepts really different? Is diffeomorphism really more stringent than homeomorphism? The answer is yes, even for topologically very well-behaved manifolds, but that is far from obvious. An example constructed by Milnor in 1956 came as a surprise, and, according to Hassler Whitney, that single, isolated example led to the modern flowering of differential topology.

Milnor's example is the 7-sphere. For every positive integer n, the n-sphere S^n is embedded in Euclidean $(n+1)$-space in a natural way, and thus has a natural differential structure. Milnor showed that there exists a differential manifold that is homeomorphic but not diffeomorphic to S^7; such a manifold has come to be called an "exotic" 7-sphere.

To prove the assertion, there are three problems to solve: (1) find a candidate, (2) prove that it is homeomorphic to S^7, and (3) prove that it is not diffeomorphic to S^7. The first problem was easy (with hindsight); the candidate was a space (a 3-sphere bundle over the 4-sphere) that had been familiar to topologists for a number of years. Milnor solved the second problem using Morse theory. A Morse function on a differential manifold is a real-valued smooth function with only non-degenerate critical points. The n-sphere has a Morse function with exactly two critical points (project onto the last coordinate and consider two poles). A theorem of G. Reeb's goes in the other direction: if a differential manifold has a Morse function with exactly two critical points, then it is homeomorphic to a sphere. Milnor showed that his candidate had such a Morse function. The third problem was the hardest. Here Milnor used two facts: first, that S^7 is the boundary of the unit ball in $\mathbb{R}^8$, and, second, that his candidate was presented as the boundary of an 8-dimensional manifold W. If the candidate were diffeomorphic to S^7, then, using the diffeomorphism, one could glue the unit ball onto W and obtain an 8-dimensional manifold that (as Milnor showed) cannot exist.

Once it was known that exotic 7-spheres could exist, it was natural to ask how many there were, i.e., how many diffeomorphism classes there were. Milnor and Kervaire showed that there are 28.

What about the other spheres? Again Milnor and Kervaire showed that the set of differential n-spheres (modulo diffeomorphism) could be made into a finite abelian group, with the "natural" sphere as the zero element; the group operation is the "connected sum", which is the natural gluing together of manifolds. The group is trivial for $n < 7$; it has order 28 for $n = 7$, order 2 for $n = 8$, order 8 for $n = 9$, order 6 for $n = 10$, and order 992 for $n = 11$. For $n = 31$, there are over sixteen million (diffeomorphism classes of) exotic spheres.

There are two systematic ways of constructing exotic spheres. The first is Milnor's "plumbing" construction (joining holes by tubes), which presents exotic spheres as boundaries of manifolds assembled by cutting and pasting. The other method (due to Brieskorn, Pham, and others) gives preassembled examples. For each finite sequence $(a_1, \ldots, a_n)$ of positive integers, let $\Sigma(a_1, \ldots, a_n)$ be the set of those zeros of the polynomial $z_1^{a_1} + \cdots + z_n^{a_n}$ that lie on the unit sphere in complex n-space. Milnor gave precise criteria on the n-tuple that ensure that this manifold is homeomorphic to a sphere of the appropriate dimension (which is $2n - 3$, by the way). For example, as k runs from 1 to 28, the manifolds $\Sigma(3, 6k - 1, 2, 2, 2)$ provide representatives for the 28 different diffeomorphism classes of 7-spheres.

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Differential Equations. Differential concepts play an important role everywhere, including pure algebra and, as above, topology. Differential equations are what make the world go around, and anyone who wants to predict and perhaps partly to change how the world goes around must know about differential equations and their solutions.

Differential equations are classified in a curiously primitive manner according to the number of independent variables that are involved in differentiation, and the way in which the unknown functions enter. The classification is "one" and "many" in the one case, and "good" and "not-so-good" in the other, or, in terms of the corresponding adjectives that apply to the equations, "ordinary" and "partial" in the one case, and "linear" and "non-linear" in the other. This report is concerned with linear equations only, and partial ones at that; ordinary ones make just a brief appearance at the beginning, to set the stage.

The beginnings of the theory of ordinary linear differential equations are simple and satisfactory; they can be found in elementary textbooks. If p is a polynomial

$$p(\xi) = \sum_{j=0}^{k} a_j \xi^j,$$

and if $D = d/dx$, then $P = p(D)$ is a differential operator, and $Pu = g$ (for given g and unknown u) is the typical linear O.D.E. with constant coefficients. If g is continuous (a reasonable, useful, but much too special assumption), then the equation always has a solution. The conclusion remains true even for variable coefficients (i.e., in case the a_j's themselves are functions of x), provided they are subjected to some mild restrictions. It is, for instance, sufficient that the a_j's be continuous and that the "principal" coefficient a_k have no zeros.

For partial differential equations even the beginnings are non-trivial and new, and, for instance, even the theory for constant coefficients belongs to the most recent period of research. The formulation of the problem is easy enough: consider a polynomial in several variables $\xi_1, \ldots, \xi_n$, and obtain a differential operator P by replacing ξ_j by $\partial/\partial x_j$; the problem is to solve $Pu = g$ for u.

To avoid some not especially enlightening and not especially useful epsilontic hairsplitting, it has become customary to take g (and to seek u) in either the most or the least restrictive class of objects in sight. The most restrictive class consists of the smooth (infinitely differentiable) functions on whatever domain is under consideration ($\mathbb{R}^n$, an open set in $\mathbb{R}^n$, a manifold); the other extreme is represented by

Laurent Schwartz's distributions. (The motivation of distribution theory is that functions f induce linear functionals $\phi \mapsto \int \phi(x)f(x)dx$ on C^∞. A "distribution" is a suitably continuous linear functional, not necessarily one induced by a function. The analogy between the generalization and its source suggests an appropriate definition of differentiation for distributions, and with that definition the theory of partial differential equations is off and running.)

Partial differential equations is an old subject and a widely applied one, and it is astonishing that the basic theorem is as recent as it is; it seems only the day before yesterday that Ehrenpreis (1954) and Malgrange (1955) proved that every linear P.D.E. with constant coefficients is solvable. If the right hand side is smooth, there is a smooth solution; even if the right hand side is allowed to be an arbitrary distribution, there is a distribution solution. The subject is exhaustively treated in Ehrenpreis's book (1962) and can be regarded as closed.

So far so good; the proofs are harder than for O.D.E.'s, but the facts are pleasant. The theory for variable (i.e., function) coefficients is much harder, much less known, and nowhere near finished. Two exciting contributions to it in the late 1950's showed that old guesses and old methods were woefully inadequate.

As for old guesses: Hans Lewy produced (1957) an inspired and amazingly simple example of a P.D.E. with variable (but *very* smooth) coefficients that has no solutions at all. Lewy's polynomial is of degree 1,

$$p(x, \xi) = a_1\xi_1 + a_2\xi_2 + a_3\xi_3,$$

where the coefficients a_1, a_2, a_3 are functions of three variables x_1, x_2, x_3, and, in fact, the first two are constants:

$$a_1 = -i, \quad a_2 = 1, \quad a_3 = -2(x_1 + ix_2).$$

The corresponding differential operator is, of course,

$$P = -i\frac{\partial}{\partial x_1} + \frac{\partial}{\partial x_2} - 2(x_1 + ix_2)\frac{\partial}{\partial x_3}.$$

What Lewy proved is that for almost every g in C^∞ (in the sense of Baire category) the equation $Pu = g$ is satisfied by no distribution whatever.

At about the same time (1958) Calderón studied the uniqueness of the solution of certain important partial differential equations (under suitable initial conditions). He showed, in effect, that if $Pu = 0$, with $u = 0$ for $t \leqq 0$ (intuitively, "t" here is time), then, locally u remains 0 for some positive time. Calderón's methods were transplanted from harmonic analysis; they introduced singular integrals into the subject, whence, a little later, came pseudo-differential operators and Fourier integral operators. These ideas have dominated the subject ever since.

Hörmander analyzed and generalized Lewy's example (1960). What makes it work, he pointed out, was that the coefficients are complex; what is fundamental is the behavior of the commutator of P and $\bar{P}$. The operator $\bar{P}$ here is obtained simply by replacing each coefficient by its complex conjugate. (In operator language: $\bar{P}u = \overline{(P\bar{u})}$.) More precisely: consider, for each polynomial in $(\xi_1, \ldots, \xi_n)$ its "principal part", i.e., the part that involves the terms of highest degree only. (For Lewy's example there is no other part.) If $p(x, \xi)$ is the principal part, write $b(x, \xi)$ for the "Poisson bracket",

$$b(x, \xi) = \sum_j \left(\frac{\partial p}{\partial \xi_j}\frac{\partial \bar{p}}{\partial x_j} - \frac{\partial p}{\partial x_j}\frac{\partial \bar{p}}{\partial \xi_j}\right).$$

Assertion: if, for some (x^0, ξ^0), the principal part $p(x^0, \xi^0)$ vanishes but the Poisson bracket $b(x^0, \xi^0)$ does not, then p is, in the sense of Lewy, not solvable in any open set containing x^0. It is easy to see that the Lewy example is covered by the Hörmander umbrella. Indeed: since

$$p = -i\xi_1 + \xi_2 - 2(x_1 + ix_2)\xi_3,$$
$$\bar{p} = i\xi_1 + \xi_2 - 2(x_1 - ix_2)\xi_3,$$

elementary computation yields

$$b = 8i\xi_3,$$

and it becomes clear that for every $x = (x_1, x_2, x_3)$ there is a $\xi = (\xi_1, \xi_2, \xi_3)$ such that $p(x, \xi) = 0$ and $b(x, \xi) \neq 0$.

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Index Theorem. The Atiyah–Singer index theorem (1963) spans two areas of mathematics, topology and analysis, and that's not an accident of technique but in the nature of the subject: the span is what it's all about. Theorems with such a broad perspective are usually the ones that are the most useful and the most elegant, and the index theorem is no exception. The very breadth of the theorem requires, however, that an expository sketch of it proceed obliquely. In what follows we describe, first and mainly, a historical and conceptual precursor, the Riemann–Roch theorem, and then indicate, briefly, how the Atiyah–Singer theorem generalizes it.

The classical Riemann–Roch theorem deals with the dual nature (topological and analytic) of a Riemann surface. Every compact Riemann surface is homeomorphic to a (two-dimensional) sphere with handles. The number of handles, the "genus", completely determines the topological character of the surface; that part is easy. The analytic structure is more complicated. It consists of a covering by a finite number of open sets and of explicit homeomorphisms from the complex plane $\mathbb{C}$ to each open set, which define holomorphic functions on the overlaps. (It is convenient and harmless to use the homeomorphisms to identify each open set in the covering with an open set in $\mathbb{C}$; that is tacitly done below.) If, for example, the surface is the sphere (with no handles), think of $\mathbb{C}$ as slicing through the equator, and use stereographic projections (toward the north and south poles) as the homeomorphisms. There are two open sets here, the complement of the north pole and the complement of the south pole; the holomorphic function on the overlap is given by $w(z) = 1/z$.

A smooth function on a Riemann surface can be viewed as a set of functions on, say, the open unit disk in $\mathbb{C}$ (one for each of the open sets of the covering) that are smooth (C^∞) and transform into one another under the changes of variables induced by the overlaps. (If, that is, f and g are two of these functions, and w is the transformation on the disk induced by going via the appropriate homeomorphism to the open set corresponding to f and coming back from the overlap with the open set corresponding to g, then $f(z) = g(w(z))$.) The function on the Riemann surface is called holomorphic (or meromorphic) if each of these functions on the disk is holomorphic (or meromorphic). Another necessary concept for the analytic study of a Riemann surface is that of a smooth differential: that is an expression of the form $p(x, y)dx + q(x, y)dy$, where p and q are complex-valued smooth functions that, on the overlaps, satisfy the chain rule for change of variables. A holomorphic differential is one of the form $f(z)dz$, where f is holomorphic and $dz = dx + idy$. (In the notation used above, the overlap relation for these differentials becomes $f(z)dz = g(w)dw = g(w(z))w'(z)dz$; the functions f and g no longer merely transform into one another, but are altered by the contribution of the differentials as well.)

The analytic properties of a Riemann surface are the properties of the holomorphic (and meromorphic) functions and differentials that it possesses. A well-known result is that the only holomorphic functions on a compact Riemann surface are constants: that is essentially what Liouville's theorem says. The Riemann–Roch theorem says much more. In its simplest form it deals with a compact Riemann surface S of genus g, and n points $z_1, \ldots, z_n$ on S. Let F be the vector space of meromorphic functions on S with poles of order not greater than 1 at each z_i (and nowhere else); let D be the vector space of holomorphic differentials with zeros of order not less than 1 at each z_i (and possibly elsewhere). Conclusion:

$$\dim F - \dim D = 1 + n - g.$$

(In the special case of the classical Liouville theorem, $g = 0$, $n = 0$, and $\dim D = 0$.) The important aspect of the conclusion is that a quantity described completely in *analytic* terms can be computed from nothing but *topological* data.

In the special case $n = 0$, F is the vector space of holomorphic functions on S (so that $\dim F = 1$) and D is the space of all holomorphic differentials. There is a linear map, conventionally denoted by $\bar{\partial}$, from the vector space of all smooth functions on S to the vector space of all smooth differentials: write

$$\bar{\partial} f = \frac{\partial f}{\partial \bar{z}} \, d\bar{z}$$

in each of the open sets of the prescribed covering. The map $\bar{\partial}$ is an example of a differential operator. The kernel of $\bar{\partial}$ consists precisely of the functions satisfying the Cauchy–Riemann equations; in other words

$$\ker \bar{\partial} = F.$$

The cokernel of $\bar{\partial}$ (the quotient space of the space of all smooth differentials modulo the image of $\bar{\partial}$) is similarly identifiable with D. The conclusion of the Riemann–Roch theorem takes, in this case, the form

$$\dim \ker \bar{\partial} - \dim \operatorname{coker} \bar{\partial} = 1 - g.$$

The Atiyah–Singer theorem is a generalization of the Riemann–Roch theorem in that it too states that a certain analytically defined number (the "analytic index") can be computed in terms of topological data. Which aspects are generalized? All. To begin with, the Riemann surface is replaced by an arbitrary compact smooth manifold M of arbitrary dimension. The vector spaces of smooth functions and smooth differentials are replaced by vector spaces of smooth sections of complex vector bundles over M (in fact, complexes of vector bundles). The map $\bar{\partial}$, finally, is replaced by a differential operator Δ, which satisfies a certain invertibility condition (called ellipticity). It follows that both $\ker \Delta$ and $\operatorname{coker} \Delta$ are finite-dimensional; the difference of the two dimensions is the analytic index. The conclusion is that the analytic index can be computed in terms of topological invariants (the "topological index"), which are very sophisticated generalizations of the genus.

Even in its relatively short life the Atiyah–Singer index theorem has had important and interesting consequences, and has been proved in at least three enlighteningly different ways. A recent one depends on the study of the heat equation on a manifold.

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Epilogue. Concepts, examples, methods, and facts continue to be discovered; problems get reformulated, placed in new contexts, better understood, and solved every day. We hope that the ten examples above have communicated at least a part of the breadth, depth, excitement, and power of the mathematics of our time. Mathematics is alive, and it's here to stay.

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