Saunders Mac Lane studied at the University of Göttingen, where he received his Ph.D. in 1934 under the supervision of Paul Bernays and Hermann Weyl. After early positions at Harvard University, he moved to the University of Chicago in 1947. His research has ranged through algebra, logic, algebraic topology, and category theory. Among his books are Homology *and (with Garrett Birkhoff) the influential text* A Survey of Modern Algebra. *His numerous honors include a Chauvenet Prize and a Distinguished Service Award from the MAA and a Steele Prize from the AMS.*

Concepts and Categories in Perspective

SAUNDERS MAC LANE

In the fall of 1933, I joined the American Mathematical Society; that December I attended my first AMS meeting in Cambridge, Massachusetts. At the meetings then there was usually one session of 10-minute papers at a time. Everybody (almost) attended the sessions. J. D. Tamarkin, J. R. Kline, George D. Birkhoff, and other senior members sat in the first row and often offered comments on the papers presented.

I then held a one-year Sterling Research Fellowship at Yale University, where I was working on my own on questions of mathematical logic and, under the direction of Øystein Ore (Sterling Professor of Mathematics at Yale), on questions of algebraic number theory, having to do with the explicit calculation of the prime ideal decomposition of a rational prime in a given algebraic number field. Thus then and now I was interested both in conceptual (logical) and computational (algebraic) issues. But my results on prime ideal composition **[1936]** were not yet ready; I needed a job for the next year, so I announced and gave a 10-minute paper on logic, entitled "Abbreviated proofs in logic calculus" **[1934]**. (References to the bibliography are by author and year.) As soon as my 10 minutes were over and the chairman had asked for questions, Øystein Ore rose and spent the next 10 minutes denouncing my work. Mathematical logic (and even more, philosophical considerations) did not in his view belong in meetings of the AMS, and he made this point very clearly. It was not really at my expense, since George Birkhoff and other Harvard professors were in the audience, and voted a few months later to offer me for the following year an appointment as Benjamin Peirce Instructor at Harvard (I accepted with alacrity). The paper on logic which I had then presented was later published [**1935**] and soon forgotten; it was not profound and may well have deserved Ore's criticism. My research on algebra prospered, especially under the stimulus of giving a graduate course at Harvard on van der Waerden's *Moderne Algebra.* I relate this story to emphasize the then and now continuing role of the American Mathematical Society in providing a forum in which beginning mathematicians can find a hearing. The story also suggests that algebra (and related branches of mathematics) has two opposite aspects: Calculations and Conceptions, *both* of which matter. In my own research work, both have been present: calculations in the study of Eilenberg-Mac Lane spaces (Eilenberg-Mac Lane [**1986**]) and conceptions in the work with Eilenberg in unveiling the notions of category, functor, and natural transformation. This essay will aim to summarize some of the high points in the development of the conceptual approach in the last 60 years of American mathematics, with particular attention to category theory and my own part in this development; it is thus history from the partial viewpoint of a participant.

1. Mathematical Logic

Initially, Aristotelean logic belonged to Philosophy departments, and not to Mathematics. The discovery of Boolean algebra in the 19th century did not change this situation in any substantial way. There were papers by B. A. Bernstein and E. V. Huntington in the 1920s and 1930s giving alternative systems of axioms for Boolean algebra, but they were of no real consequence. The first substantial connection of Boolean algebra with the mainstream of mathematics came in 1936–37 with Marshall Stone's representation theorem for Boolean algebras, and their identification with Boolean rings (Stone [**1936**]).

The publication of *Principia Mathematica*, by Whitehead and Russell, (in 1910–13) was a landmark; it showed in pedantic detail how one could in principle derive all of mathematics from a single system of axioms —axioms for logic and type theory, plus an axiom of infinity. Russell apparently thought that it proved that mathematics *is* a branch of logic, but it is now generally considered that this assertion fails, in part because of the necessity of using that axiom of infinity. The paradoxes, such as the Russell paradox of the set of all sets not members of themselves, were avoided by the use of type theory, but type theory (then and now) seems cumbersome and formal. It is interesting to note that Russell's first publication of type theory came in the same year (1908) as Zermelo's first publication of axioms for set theory. At first, type theory seemed more prominent, but with the improvements in set axiomatics by Skolem and Fraenkel in the 1920s type theory gradually lost out to Zermelo-Fraenkel set theory as the foundation of choice for mathematics.

Nevertheless, *Principia Mathematica* (P.M.) was a massive and impressively monumental attempt to give a conceptual organization for mathematics: Gödel's famous incompleteness theory of 1931 in its title refers to "Principia Mathematica und verwandter Systeme." Hilbert and others cleared up its ambiguities by insisting that a formal system of logic had both axioms and rules of inference (not clearly separated in P.M.); this made it clear that there was (and despite Lakatos, still is) a precise definition of "proof." Carnap built on P.M. in his *Logische Aufbau der Welt*, and it was influential in the Vienna circle (logical positivism). On a much smaller scale, I recall that in 1927 I discovered a copy of P.M. in a dusty library at Yale University. I found this massive book fascinating, and I soon proposed to Professor Wallace A. Wilson that I take a junior honors course to read P.M.

But P.M., though it was famous and influential, fell flat with most mathematicians. It did *not* get new mathematical results; it was unbearably clumsy; it did not help them understand what a rigorous proof really was, because they had already learned that from Weierstrass or from his pupils. And most mathematicians were just not interested in the conceptual organization of mathematics. I was, but I bought only volume I of P.M. and never got more than half way through it. And Professor Wilson told me to study Hausdorff's *Mengenlehre* **[1914]** instead of P.M. From Hausdorff, I learned to calculate with ordinal numbers.

Despite the disinterest in logic, it is fortunate that Oswald Veblen at Princeton saw that there was a future in mathematical logic; he supported the appointment of Alonzo Church in the Department of Mathematics at Princeton; in turn Church had Ph.D. students such as S. C. Kleene and Barkley Rosser; with their work, the presence of logicians in American Mathematics departments really began.

But in the early 1930s most departments of mathematics (except Princeton and Göttingen) felt that logic was not part of their business. It was this attitude that led to the formation of the Association for Symbolic Logic, to cover both mathematical and philosophical logic, and to the publication of the Journal of Symbolic Logic. Under the remarkable (and knowledgeable) editorial guidance of Alonzo Church this provided a vehicle for publications in logic. It is surely the first scholarly American journal specializing in a subfield of mathematics. (There are now, in may view, too many such.)

This development of a separate society and separate journal was a visible mark of the separation of mathematical logic from the mainstream of American mathematics. Many aspects of this separation have continued to this day — as I have argued elsewhere, in a polemical article on *The Health of Mathematics* [*MR* 86b#00006].

NOTE: The present article will involve many minor references to the literature; to simplify matters, they will not be included in the Bibliography (restricted to major items) but will be made as references to *Mathematical Reviews* (*MR*) in the style above. In early volumes of *MR*, reviews were not numbered and reference will be made to the page on which citation appears, e.g., *MR* 14-525.

2. INCOMPLETENESS

Hilbert had quite early **[1904]** set himself the task of proving the consistency of mathematics. The plan for such a demonstration required a careful analysis of the nature of proof and a clear specification of a form of logic —the first order predicate calculus; this was essentially accomplished in the Hilbert-Ackermann book **[1928]**. The explicit formulation there of the problem of completeness came to the attention of Kurt Gödel in Vienna; in his thesis he proved the completeness of the first order predicate calculus, and then soon **[1931]** went on to prove his famous incompleteness theorem. I do not believe that at that time I understood its importance. I was studying logic and mathematics in Göttingen, 1931–33. There I listened to Hilbert's lectures (then on general cultural subjects) and knew the logicians around Hilbert: I talked extensively with Paul Bernays and with Gerhard Gentzen; I knew Arnold Schmidt (then an assistant to Hilbert) and Kurt Schütte. At that time the Hilbert school seemed to hold that Gödel's demonstration that systems like P.M. could not prove their own consistency could be evaded by Hilbert's program, which aimed to get consistency by "finite" methods — and was flexible as to what "finite" might mean. (See Hilbert-Bernays **[1968]**.)

In 1933–34, when visiting Princeton, I met Gödel; I imagine that I must have studied his famous paper by that time. In Princeton, von Neumann, Church, Kleene, and Rosser clearly understood the importance of the incompleteness theorem. In retrospect, it is now clear that Gödel should have received one of the Fields medals in 1936; he did not. Subsequently, he was elected in 1955 to membership in the National Academy of Sciences, on nomination by the Council of the Academy. That is not the normal route; in my own experience all other mathematicians who are members have been elected upon nomination by the section of mathematics, NAS. These observations indicate that the significance of Gödel's contributions was at first not fully understood or appreciated by the mathematical community.

This is not now the case. Much later, in 1975, when I was a member of the National Science Board, I explained to the Board that Gödel was perhaps the greatest logician since Aristotle. The Board then made recommendations to the appointments office of the President, and Gödel was awarded the National Medal of Science. Because of his health he was unable to attend the subsequent ceremonies in the White House. I acted as his representative, and the next day I went to Princeton to bring Gödel the medal and President Ford's greetings to him.

Gödel's incompleteness theorem made use of recursive functions; from this basis Kleene [*MR* 14-525] and others developed the general study of recursion. There were also decisive contributions from Emil Post. He had previously developed his Post systems, but he taught at the City College of New York and he was somewhat isolated. In 1942 I was a member of the AMS committee to invite speakers for Eastern Sectional Meetings; I recommended Post. His resulting hour talk led to the publication of his **[1944]** paper, which formulated Post's problem, and so had a major influence on the development of recursion theory. This is just one illustration of the influence of the invited hour talks of the AMS in making important mathematical work accessible.

From this point recursion theory grew rapidly, and was extensively generalized, chiefly in technical and computational directions (perhaps) in keeping with a desire by logicians to solve hard mathematical problems. Since the development is technical, it falls outside the restriction of this essay to conceptual developments.

However, a major such conceptual development was the formulation of Church's thesis and the important result asserting the equivalence of three definitions of computable functions: recursive functions, λ-computable functions, and functions computable by a Turing machine. This development has had little connection with category theory until recently (Lambok-Scott [1986]).

3. Axiomatics

David Hilbert's 1899 book *Grundlagen der Geometrie* was influential (also in an English translation, though in 1928 I personally read it first in the 6th German edition). Euclidean geometry was definitely and rigorously reduced to five sets of axioms, and it was clear that other subjects would so reduce — as was soon exhibited in the axiomatic theory of fields, carried out in 1910 by Steinitz (*MR* 12-238) so as to include p-adic fields as well as number and function fields and fields of characteristic a prime. The axioms for vector spaces were known (at least over the reals) to Grassmann and to Peano, but their work was little noticed. The decisive change came when Hermann Weyl, in presenting relativity theory in **[1918]**, needed affine spaces and so needed vector spaces — and therefore stated the axioms explicitly. As a graduate student in Chicago, 1930–31, I had carefully learned that a vector was an n-tuple and a vector space a suitable set of n-tuples. When I came to Göttingen in the fall of 1931 I was finally enlightened, in a seminar conducted by Hermann Weyl himself, to discover the axiomatic treatment of vector spaces. I hold that these axioms (then and now) belong in the undergraduate curriculum.

The development of Banach space theory in the 1920s by S. Banach **[1922]** and N. Wiener also exhibits a use of axiomatic ideas. F. Riesz had known that useful properties of functions could be derived from a small list of such properties, but it was only after the first world war that these were consciously called axioms; thus functional analysis originated in part in a conceptual thrust.

4. Modern Algebra

As a student at Erlangen, Emmy Noether had written a thesis in invariant theory — a subject then full of elaborate calculations. But her interest soon shifted to more conceptual issues. She came to Göttingen after World War II as an assistant to Hilbert. She was immediately active in pressing the idea that suitable axioms could be used effectively to understand better the manipulations of algebra (cf. Mac Lane **[1981, 1982]**). She cultivated students and friends, and soon had a massive influence on the direction and character of algebra in Germany — through others such as Max Deuring, Hans Fitting, Wolfgang Krull, Heinrich Grell, Werner Schmiedler, F. K. Schmidt, Oswald Teichmuller, and Ernst Witt.

Emil Artin also had a major influence on abstract algebra. He had studied at Leipzig with Gustav Herglotz. Herglotz (later Professor at Göttingen, from 1930) had a remarkably polished lecturing style. His courses ranged over the whole of classical pure and applied mathematics; they aimed to display the essential features of each subject — as I vividly recall from his lectures in Göttingen on Lie groups and on geometrical optics: The main facts came on a central blackboard, the computations were put on the side.

This style was modified in Artin's magnificent lectures, given with dynamic impatience, leaving the (probably false) impression that Artin at the blackboard was again thinking everything through from first principles, so as to really understand why things were so. I failed to understand his brilliant lectures (1932) on class field theory in Göttingen. But I recall well his two hour colloquium lecture (1937–38) in which he set forth his now more conceptual understanding of Galois theory — later presented in his book with Milgram [*MR* 4-66], and also reflected in the treatment of Galois theory in Birkhoff-Mac Lane's *Survey of Modern Algebra* (1941). B. L. van der Waerden wished to use these ideas and ideals from abstract algebra to reorganize algebraic geometry (the Italian version was not rigorous, and hard to understand outside Italy). From Noether's and Artin's lectures came van der Waerden's magnificent two-volume book *Moderne Algebra* — wonderfully clear, and written in a simple German which made it easy for all to read. It was a strikingly successful presentation of the conceptual view of algebra. I suggest that this book may well be the single most influential mathematics text in pure mathematics in the 20th Century — as a book which clearly established a new and fruitful direction of teaching and research. When I ask for other books of comparable influence, I think also of the Gibbs-Wilson vector analysis [**1901**], which clearly has had the effect of standardizing the notation for vectors, vector products, scalar products and the like in all of American theoretical physics. That book also had conceptual aspects, later neglected by the physicists, such as a discussion of dyads which gives a clear (and abstract) definition of the tensor product of two three-dimensional vector spaces. Other decisive books are Banach's *Linear Operators* [**1932**] and Hausdorff's *Mengenlehre* [**1914**].

Modern algebra was a good discovery for me, when I learned about it in the academic years 1929–30 from lectures of Øystein Ore on group theory and on Galois theory. At his suggestion, I bought and studied carefully the two volume text [**1929**] by Otto Haupt on abstract algebra. It gave an abstract, clear but complicated exposition, as in the case of Galois theory; it is a book which lost out as soon as van der Waerden's text appeared. This experience indicates again the outstanding importance of a crystal-clear presentation.

This study of abstract algebra and of the Steinitz axiomatic theory of fields so excited my interest that I wrote a master's thesis (University of Chicago [**1931**]) in this direction. Since fields (and rings) had two binary operations, I thought that there should be a similar abstract treatment of systems with three binary operations: addition, multiplication and exponentiation. This led me to study a clumsy sort of universal algebra, for several sets with several binary operations. At best my efforts were of no consequence whatever; perhaps I was trying to discover universal algebra. I proved only that such a structure (with its axioms) could be translated along a one-one correspondence. This meager result indicates that just doing something abstractly may well not give the right level of generalization. At that time, I did learn a great deal about axiomatic methods from Professor E. H. Moore (then in his last year of teaching at Chicago). I was much impressed by his dictum that "The existence of analogies between central features of various theories implies the existence of a general theory which underlies the particular theories and unifies them with respect to these central features" (Moore's 1905 Colloquium lectures). This dictum is valuable in both directions: it describes conditions which make it useful to introduce axiomatically a new concept — and it indicates that such new concepts are not likely to be effective when they do not have a variety of possible applications. Parts of the mathematical literature are littered with such failed abstractions.

5. Hilbert Space

The effectiveness of axiomatic treatment is well illustrated by the development of the axiomatic treatment of Hilbert space (cf. the book by M. H. Stone

[**1932**]). In his study of integral equations, David Hilbert had used the space l^2, consisting of all sequences $\{z_n\}$ of complex numbers with $\sum |z_n|^2 < \infty$ — of course with the corresponding inner product. Then in the later 1920s came the exciting discovery that such spaces could be used to understand quantum mechanics. To carry this out, J. von Neumann in 1927 introduced the axiomatic description of a Hilbert space, and used it in his work on quantum mechanics. There is a story of the time he came to Göttingen in 1929 to lecture on these ideas. The lecture started "A Hilbert space is a linear vector space over the complex numbers, complete in the convergence defined by an inner product (a product $\langle a, b\rangle$ of two vectors a, b) and separable." At the end of the lecture, David Hilbert (by custom sitting in the first row of the lecture hall of the Mathematische Gesellschaft), who was then evidently thinking about his definition and not about the axiomatic description, is said to have asked, "Dr. von Neumann, ich möchte gern wissen, was ist dann eigentlich ein Hilbertscher Raum?"

Two of von Neumann's papers on this topic had been accepted in the Mathematische Annalen, a journal of Springer Verlag. Marshall Stone had seen the manuscripts, and urged von Neumann to observe that his treatment of linear operators T on a Hilbert space could be much more effective if he were to use the notion of an adjoint T^* to the linear transformation T — one for which the now familiar equation

$$\langle Ta, b\rangle = \langle a, T^* b\rangle \qquad (1)$$

would hold for all suitable a and b. Von Neumann saw the point immediately, as was his wont, and wished to withdraw the papers before publication. They were already set up in type; Springer finally agreed to cancel them on the condition that von Neumann write for them a book on the subject — which he soon did [**1932**] (see [*MR* 5-165] or [*MR* 16-654]).

This story (told to me by Marshall Stone) illustrates the important conceptual advance represented by the definition of adjoint operator. Stone (a student of George D. Birkhoff) had been studying linear differential equations, and so knew the idea of an adjoint differential operator used there, hence was well able to see how to transfer this adjoint notion to the context of Hilbert space. Subsequently, when I had read the older rather convoluted descriptions of adjoint differential equations, I have found these descriptions hard to understand; the conceptual formulation (1) above represents a marked advance. I have written elsewhere [**1970**] that it is a step toward the subsequent description of a functor G right adjoint to a functor F, in terms of a natural isomorphism

$$\hom(Fa, b) \cong \hom(a, Gb)$$

between hom-sets in suitable categories. But as we will see this general concept did not appear until 1957! This observation illustrates the way in which new and important concepts develop in stages, slowly, and usually at the hands of a succession of people, as in the case Hilbert-von Neumann-Stone.

6. Universal Algebra

Modern algebra for the Noether school dealt with the axiomatic treatment of properties of familiar objects: groups, rings, modules, and fields. This conceptual approach might be described as a way of getting deeper understanding of known special results by deriving them from suitable general axioms. A sample case is the decomposition of ideals into primary ideals in a commutative ring with ACC, a result containing both a decomposition theorem for algebraic manifolds (polynomial ideal rings) and the ideal decomposition in rings of algebraic integers (Noether **[1921]**). The very success of this approach inevitably suggested similar study of many more types of algebraic systems. (My own abortive 1931 master's thesis goes to show that this idea was "in the air".) The suggestion was brilliantly realized by Garrett Birkhoff's 1933 paper, where he introduced general algebras. The type of such an algebra is given by a list of the arities of its operations (unary, binary, ternary, etc.); all the algebras of a given type which satisfy specified equations (between composite operations) form a variety; Birkhoff's theorem states that such a variety may also be characterized by closure under quotient, subalgebra and (possibly infinite) products. This result was an important step in showing that there are indeed theorems about general classes of algebras. It represents a natural development of the German idea of modern algebra, and is the starting point of the whole field of "universal" algebra and its relation to model theory. Its currently active relation to combinatorics (as with Steiner triple systems, quasigroups and the like) is, however, far removed from conceptual issues.

7. Lattice Theory

The subalgebras of any given abstract algebra form a *lattice*: A partially ordered set with largest and smallest elements and with greatest lower and least upper bound for any two of its elements. This concept arises inevitably from the study of universal algebra; it was described in almost simultaneous papers by Garrett Birkhoff and Øystein Ore in 1935. It turned out that the same concept had been introduced by Dedekind in 1900, under the name "dual group". His version had apparently been lost to view, but was noted later by Ore, when he served as one of the editors of Dedekind's collected works. Ore spoke not of a "lattice" but of a "*structure*," clearly conveying the idea that the collection of subobjects (say, of all subgroups of a group) depicted algebraic structure. Birkhoff evidently had the same view, since that 1935 paper of his is entitled "On the structure of abstract algebras." Independently, Karl Menger and collaborators **[1931, 1935, 1936]** observed that projective n-space could be described by the lattice of its projective subspaces. In the ensuing five years, lattice theory was an active and fashionable subject, as for example in John von Neumann's 1937 Colloquium lectures on continuous geometries [*MR* 22#10931], which extended the projective space lattice to cases with continuous dimension function, with decisive examples drawn from rings of operators on a Hilbert space. Thus there was a real impression that lattice theory was the indicated way of describing structure, both algebraic and analytic.

Subsequently, this view was modified. On the one hand, the impact of the second world war with its emphasis on applications cut back on the enthusiasm for lattice theory. Then Suzuki [*MR* 12-586] studied the extent to which a finite group G is determined by the lattice of its subgroups and so documented the limitation of this approach. It also became clear that subgroups alone do not account for properties of homomorphisms or quotient groups. Lattice theory continued as a branch of algebra, with a number of sharp results, but it was no longer viewed as the preferred way to describe algebraic structure.

8. Homomorphism

Emmy Noether's lectures emphasized the importance of homomorphisms onto quotient groups or quotient rings, and the corresponding role of her so-called first and second isomorphism theorems for such quotients. At that time, a homomorphism in algebra always meant a surjective homomorphism (a mapping onto). Now homomorphisms also arise for homology groups of spaces; in such cases they are not necessarily onto — the familiar map $x \mapsto e^{2\pi i x}$ of the real line to the circle is onto the circle, but the induced homomorphism in homology is *not* onto. Moreover, the homotopy classification of maps f between given spaces X and Y was a central topological question, as in Brouwer's classification of maps $S^n \to S^n$ by their degree (for $n = 1$, by their winding number). The problems of topology forced on us the consideration of homomorphisms (and other maps) which are not necessarily surjective or injective.

At first the vivid arrow notation $f: X \to Y$ for a map was not available, and homomorphisms of homology groups (or rings) were always expressed in terms of the corresponding quotient group or rings. Thus the familiar long exact sequence of the homotopy groups of a fibration was originally described in terms of subgroups and quotient groups; this is the style used by all three discoveries of the sequence and of the covering homotopy theorem (Hurewicz-Steenrod [*MR* 2-323], Ehresmann-Feldbau [*MR* 3-58], Eckmann [*MR* 3-317]). The occurrence of exact sequences of homology groups (though not the name "exact") was first noted by W. Hurewicz in 1941; the idea was vigorously exploited by Eilenberg and Steenrod in their axiomatic homology theory [*MR* 14-398] (announced 1945), and it was they who chose the name "exact." The name stuck.

The practice of using an arrow to represent a map $f\colon X \to Y$ arose at almost the same time. I have not been able to determine who first introduced this convenient notation; it may well have appeared first on the blackboard, perhaps in lectures by Hurewicz and it is used in the Hurewicz-Steenrod paper, submitted November 1940 [*MR* 2-323]. At almost this time others used a notation for a map with the same intent as the arrow. The first joint Eilenberg-Mac Lane paper [**1942**] uses arrows and a few commuting diagrams, but does not use exact sequences — though the main result of that paper is the universal coefficient theorem for cohomology, now always expressed as a short exact sequence. This paper also used the now-standard notation $\hom(H, G)$ for the set of homomorphisms of H into G (that may not be the first such usage). Observe that the use of these notational devices preceded the definition of a category; I suggest that this precedence was a necessary first step. I suggest also that abstract algebra, lattice theory, and universal algebra were also necessary precursors for category theory; it is at any rate clear that I personally was familiar with all three of these subjects before taking part in the discovery of categories. Such cumulative developments are, in my view, a frequent phenomenon in conceptual mathematics.

9. Categories

The initial discovery of categories came directly from a problem of calculation in topology. For a prime p, the p-adic solenoid Σ is the intersection $\bigcap T_i$ of an infinite sequence of solid tori T_i, where T_{i+1} winds p times around inside T_i. In 1937, Borsuk and Eilenberg asked for the homotopy classes of all continuous mappings $(S^3 - \Sigma) \to S^2$. In 1939, Eilenberg showed that those classes could be represented as the elements of a suitable 1-dimensional cohomology group $H^1(S^3 - \Sigma, Z)$. By using regular cycles, Steenrod [**1940**] partially computed some of these groups. Mac Lane, starting from computational questions in class-field theory (cf. Mac Lane [**1988**]) had independently (unpublished) computed the group $\operatorname{Ext}(\Sigma^*, Z)$ of abelian group extensions of Z by the (discrete) dual Σ^* of Σ. Eilenberg then saw a connection to Steenrod's questions; then Eilenberg and Mac Lane jointly found that this group of group extensions is isomorphic to $H^1(S^3 - \Sigma, Z)$ and that this result comes from the (now familiar) short exact sequence

$$0 \to \operatorname{Ext}(H_{n-1}(K), G) \xrightarrow{\beta} H^n(K, G) \xrightarrow{\alpha} \operatorname{Hom}(H_n(K), G) \to 0$$

(the universal coefficient theorem for cohomology) which "determines" the cohomology groups of a chain complex K in terms of its integral homology groups H_n and H_{n-1}. Actually, to handle Steenrod's regular cycles it was necessary to take a limit of such sequences over an infinite sequence of maps $f: K \to K'$ of complexes; for this it was necessary in turn to know what happens to this short exact sequence under the action of such a chain transformation f. This leads to the diagram

$$\begin{array}{ccccccccc} 0 & \to & \mathrm{Ext}(H_{n-1}(K',G)) & \xrightarrow{\beta} & H^n(K',G) & \xrightarrow{\alpha} & \mathrm{Hom}(H_n(K'),G) & \to & 0 \\ & & f^*\downarrow & & f^*\downarrow & & f^*\downarrow & & \\ 0 & \to & \mathrm{Ext}(H_{n-1}(K),G) & \xrightarrow{\beta} & H^n(K,G) & \xrightarrow{\alpha} & \mathrm{Hom}(H_n(K),G) & \to & 0 \end{array} \quad (2)$$

where α is the operator which evaluates each G-cocycle on homology classes of K, and where the vertical maps are (as was then said) the maps "induced" by f. What this means is that, for G fixed, $H^n(-,G)$ is a functor of K; this functor turns each chain complex into an abelian group, the cohomology $H^n(K,G)$, and also turns each map $K \to K'$ of chain complexes into the "induced" map f^* of cohomology groups. Moreover, if $g: K' \to K''$ is another such chain transformation, the induced map for the composite $g \circ f$ is the composite f^*g^*. In the then new language, this means that $H^n(-,G)$ is a (contravariant) functor, turning complexes into abelian groups and maps of complexes into homomorphisms of groups, and this in such a way as to preserve (better, invert) composition — and also to preserve identities. Thus the geometric situation forces the consideration of a functor, and at the same time compels one to introduce the category (of chain complexes) on which this functor is defined. (The covariant functor $H_n(-)$ is also involved here.)

This is not all; in order to take the necessary limits, one needs to know that both square diagrams in (2) are commutative; i.e.; $f^*\alpha = \alpha f^*$, and similarly with β. This property of α (and of β) means that α is what is called a *natural transformation* between functors (or, for the French, who rename things to suit their own culture, a morphism of functors).

For the purposes of that first paper [*MR* 4-88], Eilenberg and Mac Lane defined only the induced maps (like f^*) and the notion "natural homomorphism". But, given the conceptual background which I have been describing, we took the next following step of defining category and functor in our next joint paper [**1945**] which we entitled simply "General theory of natural equivalences" — although it really began with categories and functors. It was perhaps a rash step to introduce so quickly such a sweeping generality — an evident piece of what was soon to be called "general abstract nonsense." One of our good friends (an admirer of Eilenberg) read the paper and told us privately that he thought that the paper was without any content. Eilenberg took care to see to it that the editor of the *Transactions* sent the manuscript to a young referee (perhaps one who might be gently bullied). The paper was accepted by the *Transactions*; I have sometimes wondered what could have happened had the same paper been submitted by a couple of wholly unknown authors. At any rate, we did think that it was good, and that it provided a handy language to be used by topologists and others, and that it also offered a conceptual view of parts of mathematics, in some way analogous to Felix Klein's "Erlanger Program". We did not then regard it as a field for further research efforts, but just as a language and an orientation — a limitation which we followed for a dozen years or so, till the advent of adjoint functors.

A category is not an algebraic system in the sense of Birkhoff's universal algebra, because the primitive operation of composition $g \circ f$ is defined *only* when the domain of g is the codomain of f; indeed, it is this circumstance that forces the arrows in a category to have both source and target specified. Actually, this is already forced by the topological situation, since the effect of a map for homology depends vitally on the target of f. But I note that the algebra of composition in this sense had already appeared in the studies of the German algebraist H. Brandt [**1925**], whose work on composition of quadratic forms had forced him to consider groupoids (categories in which every arrow is invertible). Incidentally, Brandt is one of the German algebraists who thought that Emmy Noether's view of algebra was too abstract!

Subsequently, Charles Ehresmann's extensive study of the foundations of differential geometry led him to consider groupoids of local isomorphisms transporting geometric structure from one coordinate patch to another; in time this led him to an extensive study of categories, often in an idiosyncratic notation. His example indicates that the discovery of categories was inevitable — if not forced by problems in algebraic topology, it would have been forced by problems in differential geometry.

The use of categories as a language is well illustrated by the development of axiomatic homology theory. About 1940, the multiplicity of homology theories (simplicial, singular, Čech, Vietoris, Alexander,...) seemed confusing. Then Eilenberg and Steenrod introduced their axioms, including the central one asserting that homology is a functor on (a category of) topological spaces to abelian groups. This could have been stated without the words or language "functor" and "category," but Steenrod in conversation emphasized the importance of these concepts. He said that the Eilenberg-Mac Lane paper on categories had a more significant impact on him than any other research paper; other papers contributed results, while this paper changed his way of thinking. Thus, the use of categories formulates the way in which algebraic topology pictures geometric situations by algebraic relations, and in this way has repeatedly appeared in the study of various extraordinary homology theories and in current research on algebraic K-theory.

The initial uses of category theory in computer science (for automata, i.e., machines, minimal realization is left adjoint to behavior) were not so sweeping, though currently categorical techniques appear in the study of data types, of polymorphic types, and, more generally, of the semantics of programming languages.

10. Acyclic Models

This topic represents another shift from computation to concept. In algebraic topology, many necessary comparisons appear to require elaborate formulas backwards and forwards, as in the passage from simplicial singular homology to cubical and as in that from simplicial products to tensor products of chain complexes (the Eilenberg-Zilber theorem). It then turned out that representable functors and the categorical language allowed one to get these comparisons (and many others, bar to $\overline{W}$ in $K(\pi, n)$) without any explicit formulas, by the methods of Acyclic Models (Eilenberg-Mac Lane [**1953**, *MR* 14-670]) which was in effect a "general nonsense" version of an earlier geometric method of "acyclic carriers" — the basic concept is that one triangulates spaces because the resulting pieces (the simplices) are themselves acyclic — they have (reduced) homology zero, so that the fashion in which the simplices are connected together gives all the homology.

One of these explicit comparisons (of the simplicial $\overline{W}$ construction) to the (tensor-product like) bar construction arises in very elaborate calculations of the homology of Eilenberg-Mac Lane spaces $K(\pi, n)$ (those spaces with just one nonvanishing homotopy group $\pi_n \cong \pi$.) These calculations involve repeated manipulation of iterated faces F_i and degeneracies D_j of singular simplices, so Eilenberg-Mac Lane codified these identities (for composites $F_i D_j$) and called the result an F-D complex: I thought we were just organizing algebraic calculations. Instead, we were introducing simplicial sets and groups, now described not by identities but as contravariant functors to sets from a certain small category Δ of model simplices. Today the category of simplicial sets is for many purposes a replacement for (and for homotopy types, equivalent to) the category of spaces. Grothendieck, in a massive unpublished manuscript [**1985**], has pushed for other alternative categories to simplicial sets; the fact remains that what started as a tool for computation has been categorized to become a different approach to the concept of space — notably useful in the application of algebraic K-theory to the study of topological manifolds.

11. Bourbaki

In the period 1930–60, almost all new French mathematicians had studied at the École Normale Supérieure in Paris; when the students there wanted to start a riot, the cry went up for "Bourbaki" (who had been an unsuccessful French general in the Franco-Prussian war). Legend has it that in the 1930s several young mathematical normaliens wandered through Montmartre and observed a bearded clochard at the table of a café, mumbling into his absinthe "compact space, measure, integration." They sat at his feet, followed his many insights, and went on to publish a many-volume treatise which organized mathematics, starting from the most general down to the particular.

They were deliberately carrying further the modern algebra approach of the German school; they were also revolting against certain mathematical trends then dominant in Paris: careless details in proofs and a predominant emphasis on the theory of one complex variable. The resulting Bourbaki treatise (which paid no heed to applied mathematics and never did get so far as to treat one complex variable) was systematic, austere and clear. It started from a definite conceptual background, and had a widespread influence. Here are two examples. Before Bourbaki, a topological space was "compact" if every infinite sequence of points had a convergent subsequence, and "bicompact" if every open cover had a finite subcover. Bourbaki noted that it was the second concept which had sweep and general force; they changed the names: "bicompact" to "compact"; "compact" to "sequentially compact" — and this change was universally adopted (a rare event!). Second, their ideas penetrated the whole mathematical community. I vividly remember a visit in 1950 to Ole Miss (The University of Mississippi), where I was served a rich diet of channel catfish and Bourbaki's concepts.

Bourbaki dealt with mathematical structure, and in one of his very first volumes [**1939**; *MR* 3-55] gave a cumbersome definition of a mathematical structure in terms of what he called an "echelle d'ensembles"; though he did not say so, this is close to the notion of a type theory in the sense of Bertrand Russell, and has the same cumbersome characteristics. By now cartesian closed categories provide a different possible formulation of types (in which the objects of the category are the types); this is presented for example in Lambek-Scott [**1986**]. Early in the 1950s some members of Bourbaki, seeing the promise of category theory, may have considered the possibility of using it as a context for the description of mathematical structure. It was about this time (1954) that I was invited to attend one of Bourbaki's private meetings — not the Bourbaki seminar, but a meeting where draft volumes are torn apart and redesigned. Bourbaki did not then or later admit categories to their volumes; perhaps my command of the French language was inadequate to the task of persuasion. Subsequently, Eilenberg was for a period a regular member of Bourbaki.

Debate at Bourbaki meetings could be vigorous. For example, in one such meeting (about 1952) a text on homological algebra was under consideration. Cartan observed that it repeated three times the phrase "kernel equal image" and proposed the use there of the exact sequence terminology. A. Weil objected violently, apparently on the grounds that just saying "exact sequence" did not convey an understanding as to why that kernel was exactly this image. In the event, the exact sequence terminology won — not just in Bourbaki, but everywhere, probably because it gives such an effective capsule summary.

Bourbaki emphasized that his emphatic use of abstraction and generalization was not as a technique of research, but as an effective way of organizing and presenting mathematics. As carried out, several of his volumes were notably influential; for instance his "Topologie Générale" (Livre III), his "Intégration" (Livre VI) and in particular his "Algèbra multilinéaire (Livre II, Chapter III). In subsequent years, Bourbaki was more interested in further carrying his method to other parts of mathematics, and so was less concerned with underlying questions such as the use of categorical or other conceptual approaches. Here, as elsewhere in the history of mathematics, conceptual advances involve several successive steps, usually subsequent steps by new authors.

The Bourbaki organization of pure mathematics is clearly a further advance on earlier conceptual developments (e.g., modern algebra). It is a remarkable success — perhaps not on the level of Euclid's elements, but far surpassing the efforts of that great German organizer Felix Klein (e.g., in the *Encyclopädie der Mathematik*). A whole generation of graduate students of mathematics were trained to think like Bourbaki. His seminar in Paris presents new results of research; it is a major accolade when a topic is presented there. It may well be that an anonymous group effort in organization like Bourbaki is possible only in a country which is highly centralized (as in Paris) and in which school children are exposed early on to extensive philosophical discussion.

12. Abelian Categories

The next step in the development of category theory was the introduction of categories with structure. About 1947, I noticed that the Eilenberg-Steenrod axiomatic homology theory concerned functors from a category of topological spaces to various categories with an "additive" structure — categories of abelian groups, or of R-modules for various rings R. I consequently set about to describe axiomatically these abelian categories; in doing this I also formulated explicitly the definition of products and coproducts (= sum) by universal mapping properties. The resulting description of abelian categories laid too much emphasis on duality, as in the duality between sum and product. In module categories there is a distinguished class of monomorphism (the inclusion of submodules); I endeavored to force the duality by introducing a distinguished class of epimorphisms (e.g. maps to quotient modules). I failed to note that in "reality" there is no such distinguished class of epimorphisms. To put it in more current terminology: category theory describes certain structures such as products and cokernels, but only up to isomorphism, and that is all that matters. In the event, my initial **[1950]** description of abelian categories was clumsy. I soon had the opportunity to present this description in an invited hour lecture at an AMS meeting. I felt that the ideas involved were important, but the lecture evoked no response at all; for example the *Mathematical Reviews* produced a belated one-line statement [*MR*

14-133]: "This paper is an expanded version of an earlier note [*MR* 10-9]." I was discouraged from pursuing these ideas further.

But abelian categories were there, so the idea did not die. David Buchsbaum, in a thesis stimulated by Eilenberg [**1955**; *MR* 17-579], developed a smoother axiomatic description. Then Grothendieck [**1957**; *MR* 21#1328] made the crucial geometric observation that sheaves of abelian groups or of modules on a space form an abelian category, and proceeded to describe a more specific structure (his AB5). His important discovery was clearly independent of any prior work on abelian categories: He came to Chicago in the spring of 1955 and lectured on this subject; as I heard his lecture, it was amply clear that he had no knowledge of earlier work by Mac Lane or Buchsbaum. This may illustrate the fact that there can be multiple discoveries of a concept, and that the discovery which matters most is that which ties the concept to other parts of mathematics — in this case to sheaf cohomology.

Buchsbaum emphasized the use of abelian categories as the range of axiomatic homology functors, while Grothendieck emphasized their use in homological algebra. Here and below we do not intend to cover the subsequent development of homological algebra or the use there of abelian categories, even though these developments were closely related to those in general category theory.

One may note that the influential 1958 book by Godement, on algebraic topology and the theory of sheaves [*MR* 21#1583] mentions both simplicial sets and abelian categories, but does not make systematic use of these concepts in that general form. New ideas are incorporated in the literature only gradually — if at all.

13. Algebraic Geometry

Algebraic geometry as developed in the early German and the Italian schools was rich in geometric insights but deficient in techniques for rigorous proof. The need for an underpinning of algebraic geometry had played a large role in Emmy Noether's ring theory (polynomial rings) and in extensive research by van der Waerden on ideal theory, by W. Krull on valuation theory, and by Zariski on the resolution of singularities and related matters. André Weil wrote a monumental and influential treatise [*MR* 9-303] on intersection theory in which he reformulated the notion of an algebraic variety. In my own examination of this treatise I did notice a number of points where categorical concepts might be fruitful (I did not develop this observation). There were also important contributions by Serre [*MR* 16-953] and by Chevalley [*MR* 21#7202].

The decisive next step was taken by Grothendieck. To attack certain conjectures of André Weil, he proposed a massive reformulation of all of algebraic geometry; we will note here only two aspects of this reformulation which involve categorical concepts. One aspect was a drastic change in the description of an algebraic variety V. Classically such a V was the locus in affine or better in projective space of a finite number of polynomial equations; in more invariant terms, of the ideal generated by these polynomials. Weil had shown the importance of replacing projective varieties by pasting together several affine pieces. Grothendieck instead shifted the basic notion to that of a scheme, initially described as a suitable topological space carrying a sheaf of local rings. Finally, in the hands of Gabriel and Demazure (*Groups Algébriques* [*MR* 46#1800]) a scheme was defined in simple conceptual terms as a functor from commutative rings to sets; again the categorical formulation made for simplicity, and in this case helped expedite the sheaf concept.

The notion of a sheaf on a space X developed gradually (see Gray's historical article [**1979**]), starting in part from analysis in one or several complex variables. On the one hand, one may consider the set G_x of germs of functions (analytic or continuous, as the case may be) at each point $x \in X$; the totality of these germs then forms a space $\coprod G_x$ with a suitable continuous map p onto X; such a "local homeomorphism" is a sheaf over X. On the other hand, one may consider for each open set U of X the set $F(U)$ of all analytic or of all continuous functions defined on U. Then F is a contravariant functor to sets from a category of open subsets of X. When this functor has a patching property (e.g., a continuous function can be patched together from matching pieces) it is a sheaf. The typical sheaves in analysis are functors to the category of abelian groups or of modules, but for conceptual purposes it suffices to consider sheaves of sets. Serre and others had emphasized the use of sheaves in algebraic geometry, and Grothendieck then used them heavily in his study of the cohomology of his schemes. He observed that the category of sheaves on a space X (or, more generally, on what he called a site) carried the essential information about the topology (and the cohomology) of that space or site. He therefore called such a category a "topos." These ideas were presented in a famous 1962 Harvard seminar of M. Artin and in Grothendieck's Seminaire "Géométrie Algébrique du Bois Marie" — SGA IV, for 1963/64. These seminar notes were later extensively revised and hard to get. (In 1966, I managed to get a copy, but with difficulty). There was a second mimeographed edition in 1969 and then — finally fully public — in a Springer Lecture Notes (Artin [**1972**]). Its presentation involved a great deal of category theory, and soon included a theorem of Giraud characterizing those categories which are topoi (toposes). It was observed that a topos inherits most of the familiar properties of the category of small sets. (This by Verdier in lectures, 1965, and in a copy of Exposé IV in the 1969 edition of SGA IV, by Grothendieck and Verdier, identical with the 1972 edition. (Page 3 there has the famous statement "the authors of the present seminar consider that the object of topology is the study of toposes and not just of topological spaces".) This is the origin of topos theory, a decisive aspect of category theory.

The remarkable and extensive influence of Grothendieck in algebraic geometry does not fall under my subject here. His use of categories is subordinate to his geometric insights, but I note that here (as in the case of algebraic topology and the discovery of categories) geometric questions led inevitably to categorical developments.

14. Adjoint Functors

The notion of a universal construction was developed in stages, well before its formulation in terms of adjoint functors. The description of a construction as "universal" would naturally be used first in cases where a set-theoretic version of the construction is not quite natural. Thus Eilenberg-Mac Lane [**1945**; Thms. 21.1 and 21.2] described direct and inverse limits over directed sets in terms of a version of universality (such limits had appeared first with Čech homology). In [**1948**], Samuel described universal constructions, while representable functors were used by Bourbaki about 1948. As noted, Mac Lane in [**1948**, **1950**] showed that the familiar cartesian product could be described in terms of universal properties of its projections.

Kan [**1958**] took the major step of defining adjoint functors. He then formulated all the related ideas: unit and counit of an adjunction, existence theorems for Kan extensions and tensor product as left adjoint to hom, plus numerous examples from topology. At the same time, he used these adjoints extensively in his study of simplicial sets. At the time I was startled and impressed with his discovery. It represented a major conceptual advance which others from Bourbaki to Samuel to Mac Lane had missed. Kan made this major discovery while he was visiting Columbia University, and Eilenberg suggested the name "adjoint" to Kan. There is evidence that the discovery of adjoint functors was inevitable; other people would have found them.

This discovery soon blossomed. Peter Freyd's basic existence theorem for adjoint functors (the "adjoint functor theorem") appeared in [**1963** *MR* 34#1371] and in his book [**1964**]. Adjoints had arrived. (There are further historical comments in Mac Lane [**1971**; p. 76 and p. 103].)

15. Sets Without Elements

As an undergraduate at Indiana University, F. W. Lawvere had studied continuum mechanics with Clifford Truesdell and Walter Noll; when he gave some lectures in Truesdell's course on functional analysis, he learned some category theory and had occassion to rediscover for himself the notions of adjoint functor and reflective subcategory. He then moved to Columbia University. There he learned more category theory from Samuel Eilenberg, Albrecht Dold, and Peter Freyd, and then conceived the idea of giving a direct axiomatic description of the category of all categories. In particular, he proposed to do set theory without using the elements of a set. His attempt to explain this idea to Eilenberg did not succeed; I happened to be spending a semester in New York (at the Rockefeller University), so Sammy asked me to listen to Lawvere's idea. I did listen, and at the end I told him "Bill, you can't do that. Elements are absolutely essential to set theory." After that year, Lawvere went to California.

I was wrong about Lawvere's idea. In an axiomatic foundation, it is possible to replace the primitive notion "element of" (a set) by the primitive notion "composition of functions" (between sets); this amounts to an axiomatic description of the category of sets. Lawvere did achieve a complete formulation of this idea in his 1963 Columbia thesis, and refined the idea while giving courses at Reed College, 1963–64. By that time I finally understood that it was indeed possible to state axioms (in the first order predicate calculus but not using elements) for the category of sets; I redeemed my earlier lack of understanding by communicating Lawvere's presentation of this idea to the Proceedings of the National Academy of Sciences **[1964]**. This paper established the startling fact that it is possible to give a formal foundation of mathematics different from the standard foundations by axiomatic set theory and by type theory. Since that time this approach has been further improved; one can now describe the elementary theory of the category of sets (ETCS) as the theory of a well pointed (elementary) topos E. Then E has a terminal object 1 and the "elements" of an object X of E appear as the arrows $x\colon 1 \to X$. The equivalence of this theory to a weak form of Zermelo set theory is known (e.g., Johnstone **[1977]** or Hatcher **[1982]**). Moreover, an elementary topos is a cartesian closed category, and the latter concept is closely connected with the typed λ-calculus, while topos theory may be regarded as a version of intuitionistic type theory (Lambek-Scott **[1986]**). The λ-calculus, developed by Church and others in the 1930s, can be described informally as "doing logic without variables"; however, it had little or no connection with the initial developments of elementary topos theory ("doing sets without elements"). J. Lambek and D. Scott were among the first to emphasize the interconnection of these ideas. Also doing set theory without elements does involve much use of commutative diagrams — some rather large and even cumbersome. It is striking that the so-called Mitchell-Benabou language has introduced the idea of using letters in the language which act "as if" they were elements. This effective approach is described, for example, in Boileau-Joyal **[1981]**.

The difficulty of understanding that there can be a set theory without elements seems to persist in some quarters. For example, Feferman **[1977]** in responding to a paper of mine, writes "*when explaining* the general notion of structure and of particular kinds of structure such as groups, rings, categories, etc., we implicitly *presume as understood* the idea of *operation* and *collection*" (his italics). This observation fails to make a clear distinction between the prior informal preaxiomatic understanding of notions such as "collection" and their formal presentation, for example in an axiomatization in the first order predicate calculus. More especially, it fails to note that in the elementary theory of the category of sets the objects axiomatize the notion "collection" and the arrows the notion "operation." Feferman goes on to discuss the "operation of *cartesian product* over collections". It might be that he has failed to notice that a finite cartesian product can be axiomatized by universal properties of its projections — and that this gives a more intrinsic understanding of cartesian products in many categories than the usual (artificial) set-theoretic definition of ordered pair. (The categorical treatment of infinite cartesian products requires reference to a slice category, in a more elaborate construction which may well not have then been known to Feferman.) These remarks are not intended as a criticism of Feferman; he is a logician who has indeed examined category theory and made contributions (the use of reflection) to the problem of explaining large constructions such as functor categories. The point rather is that for anyone brought up in the tradition of set theory, it may be very difficult to imagine the viability of alternative approaches which do not take as basic the notion of "set with elements." Also, the pre-formal notion of "collection" may not be well represented by Zermelo-Fraenkel set theory, where the elements of a set are again sets, so that one is dealing with sets of sets of sets, etc. Understanding new conceptual approaches is notoriously hard.

16. The Concept of Set

The discussion of "sets without elements" might well be supplemented by a brief consideration of the earlier origins of the mathematical concept of a set. There seem to be two (related) origins: the notion of a "collection" and the more sweeping notion of "arbitrary" set. By a collection I mean here a collection of some of the elements of an already given totality. Thus the congruence class 3 modulo 11 is the collection of all integers x with $x \equiv 3 \pmod{11}$, a function on the reals to the reals is a collection of pairs of real numbers; a real number is a Dedekind cut; that is, a suitable collection of rational numbers; a rational number is a congruence class (collection) of pairs of integers, and a natural number is an equivalence class of finite sets, where "equivalence" means cardinal equivalence. This notion of collection is the one which appears in Boolean algebra — the algebra of all collections taken from a given universe. Point-set topology dealt originally with point-sets which were usually collections of points from a given Euclidean space.

The wider notion of an arbitrary set came to general attention in the work of Georg Cantor, in his treatment of arbitrary infinities, with the definition of a (possibly) infinite cardinal number as an equivalence class of (arbitrary) sets, and of an ordinal number as an ordinal equivalence class of well-ordered sets. This general idea appears also in the work of G. Frege and of Bertrand Russell, who spoke of "classes" and not "sets" and who formulated the famous paradox of the class of all classes not members of themselves. Then Zermelo's ingenious proof that every set could be well ordered revealed the need to consider the axiom of choice and led him to his axiom system for sets, which also served to avoid the Russell paradox. Hausdorff's famous book "set theory" (first edition, 1914) dealt with naive set theory, though Hausdorff clearly stated that he knew the Zermelo axioms. These axioms for set theory were subsequently improved by Skolem [**1922**], Fraenkel [**1922**], von Neumann [**1928**] and Bernays [**1942**, *MR* 2-210]). But I read the historical record to show that mathematicians generally, up until about 1935, did not regard axiomatic set theory as the foundation of mathematics, but only as a way of explaining Cantor's infinities and of founding the ordinal numbers. At the same time, they thought of collections (in the sense above) as a naive set theory, not requiring any foundations.

At that period, the "foundation" of mathematics was concerned primarily with the rigorous treatment of the calculus. This required expert manipulation of limits by $\varepsilon - \delta$ arguments, in the tradition of Weierstrass, plus proof of basic facts like the mean-value theorem from a definition of real numbers. The standard presentation of this approach was formulated by that vigorous advocate of rigor in proofs, Edmund Landau, in his notable leaflet "Foundations of Analysis (The calculation with whole, rational, irrational, and complex numbers, a complement to the texts on differential and integral calculus)," first published in 1930; cf. [*MR* 12-397] for the English translation. There he started from the Peano postulates for the natural numbers and built up the other number systems in the well-known way by equivalence classes and (eventually) Dedekind cuts.

A Dedekind cut is described (*loc. cit.* p. 43) as a suitable "Menge" of natural numbers. Here the words "Menge" (in Definition 28 for a cut) and "Klasse" (of equivalent fractions, p. 20) appear just like that, with no definition and no apology for the absence of a definition. In the introduction, Landau on page x thanks von Neumann for help — and at this time (1928) von Neumann had already formulated his version of axiomatic set theory.

In other words, Landau built up the reals just from the Peano axioms, presented in his famous austere "Landau style" *Axiom, Definition, Satz, Beweis.* I recall attending his lectures (in Göttingen) and admiring this style and its absolute precision. I also recall that in later years I often explained to other mathematicians that one could not really get the real numbers just from Peano postulates — one also needed assumptions about sets.

In Göttingen in those days (1931–33) Hermann Weyl did not hold much use for set theory: he repeatedly said that set theory "involved too much sand". Then Hermann Weyl was perhaps inclined to intuitionism. But the famous two-volume book by Hilbert-Bernays *Foundations of Mathematics* **[1934]** in its second edition (1968 and 1970) mentions the word "set" only in a wholly incidental way.

On the other hand, we have the current firm belief that ZFC (the Zermelo-Fraenkel axioms with choice) is *the* foundation of mathematics. I have been unable to determine just when the belief became generally accepted. Was it with Gödel's proof of the consistency of the Continuum Hypothesis (1940; *MR* 2-66)? Or with Nicholas Bourbaki's brilliant lecture (actually delivered by A. Weil) to the Association for Symbolic Logic on "Foundations of Mathematics for the Working Mathematician" (1949; *MR* 11-73). Or was it the gradual appreciation of the scholarly quality of Paul Bernays' system of axiomatic set theory (completed 1943; *MR* 5-198). At any rate, by the time of the ascendency of the "new math" in the schools (beginning at 1960), the central role of axiomatic set theory was generally accepted, only to be promptly challenged by Lawvere in 1964.

I conclude that the ZFC axiomatics is a remarkable conceptual triumph, but that the axiom system is far too strong for the task of explaining the role of the elementary notion of a "collection." It is also curious that most mathematicians can readily recite (and use) the Peano axioms for the natural numbers, but would be hard put to it to list all the axioms of ZFC. It is, however, well-known that these axioms do not suffice to settle the continuum hypothesis, in view of Paul Cohen's proof of its independence (1963; *MR* 28#1118 and *MR* 28#2962). But we will soon explain that this independence can be viewed not just as a fact about models of sets, but also as an aspect of sheaf theory for toposes.

17. Research on Categories

Initially, Eilenberg and Mac Lane had written what they thought would perhaps be the only necessary research paper on categories — for the rest, categories and functors would provide a useful language for mathematicians. Then, as noted above in §11, the study of abelian categories became a substantial subject of research, especially in connection with homological algebra. This observation is well exemplified by a famous 1962 thesis of Pierre Gabriel "Des Catégories Abéliennes" [*MR* 38#1144] (Not reviewed till 1969!). In addition, there was a trickle of research papers on general category theory, with some notable items, such as the discovery of adjoint functors (see §14 above).

Another important step came when Peter Freyd, in his 1960 Princeton thesis, showed that there could be substantial theorems about categories by proving his Adjoint functor theorems, which gives conditions for the existence of adjoint functors.

Then in 1963 it suddenly became clear that general category theory (not just abelian categories or applications of categories) was a viable field of mathematical research. It is difficult to understand why so many instances of this development came in just this one year. Some of the major such items in 1963 are:

(i) SGA IV, from the Institut des Hautes Études Scientifiques, first appeared in 1963–64 in a mimeograph edition with 7 Fascicules. The first fascicule has the title "Cohomologie étale des schémas," edited by M. Artin, A. Grothendieck, and J. L. Verdier, while the remaining fascicules 2–7, under the title "Schémas en groupes," are edited by M. Demazure and A. Grothendieck. There was a second mimeographed edition in 1969, edited by Artin, Grothendieck and Verdier, in which the title was deliberately changed to "Théorie des topos et cohomologie étale des schémas" to emphasize the importance of the language of (Grothendieck) topologies and toposes.

(ii) Lawvere's imaginative thesis at Columbia University, 1963, (see *MR* 28#2143) contained his categorical description of algebraic theories, his proposal to treat sets without elements and a number of other ideas. I was stunned when I first saw it; in the spring of 1963, Sammy and I happened to get on the same airplane from Washington to New York. He handed me the just completed thesis, told me that I was the *reader*, and went to sleep. I didn't.

(iii) Peter Freyd's first public presentation of his adjoint functor theorem was at a model-theory conference in Berkeley in 1963 [*MR* 34#1371]. This focused attention on adjoints.

(iv) Ehresmann's big paper on "Catégories Structurées" (in modern terms, on internal categories such as topological categories = category objects in the category Top of topological spaces) appeared in 1963 [*MR* 33#5694].

(v) Mac Lane's first coherence theorem (all canonical diagrams commute in a symmetrical monoidal category) was published in 1963 [*MR* 30#1160], to be sure in an obscure place.

(vi) Mac Lane, as the 1963 Colloquium lecturer for the American Mathematical Society, chose to lecture on Categorical Algebra.

Had I been invited to give Colloquium lectures a year or two earlier, I would have chosen to lecture on homological algebra or an aspect of algebraic topology; I would hardly have ventured to give four one-hour lectures on category theory. But by 1963 I had been stimulated by the enthusiasm of the group of young people at Columbia around Eilenberg (H. Applegate, M. Barr, H. Bass, J. Beck, D. Buchsbaum, P. Freyd, J. Gray, A. Heller, F. W. Lawvere, F. E. J. Linton, B. Mitchell, M. Tierney) and I saw that category theory involved substantial research prospects. My 1963 Colloquium lectures emphasized the equivalence between universal arrows and adjoints, and their effective use in the description of limits and of abelian categories. Other topics included the bar resolution regarded as an adjoint, symmetric monoidal categories and their use to describe higher homotopies, as well as topics in homological algebra. The subsequent article [*MR* 30#2053] was considerably expanded, so does not exactly reflect the content of the lectures. It is clear that at that time my interests in category theory were closely tied to the use of categories in topology and in homological algebra.

It is remarkable that 1963 presented so many developments in category theory, coming from several quite different sources. Possibilities were in the air, because it was at about this time that many mathematicians started to do research in category theory. In the period 1962–67, I estimate that about 60 people started; I document this with a table of "first" research papers in category theory by various authors, arranged (but only approximately) by "schools"; papers primarily in homological algebra and most research announcements are omitted. Many of the papers in this long list are not now of importance, but the list is intended to illustrate the sudden way in which new developments can take off, with widespread participation.

The school in the USSR was led by A. Kuros, who had worked in group theory:

A. G. Kuroš 1960 Direct decompositions in algebraic categories, *MR* 21#3365,

M. S. Calenko 1960 On the foundations of the theory of categories, *MR* 26#2480,

A. H. Livšic 1960 Direct decompositions with indecomposable components in algebraic categories, *MR* 22#2658a

D. B. Fuks 1962 On the homotopy theory of functors in the category of topological spaces, *MR* 25#572

E. G. Šul'geĭfer 1960 On the general theory of radicals in categories, *MR* 27#2451

A. V. Roĭter 1963 On a category of representations, *MR* 28#3072

A. S. Švarc 1963 Functors in categories of Banach spaces, *MR* 27#4046

H. N. Inasaridze 1963 On the theory of extensions in categories, *MR* 28#3074

O. N. Golovin 1963 Multi-identity relations in groups and operations defined by them on the class of all groups, *MR* 28#3103

V. V. Kuznecov 1964 Duality of functors in the category of sets with a distinguished point, *MR* 32#2456

The school of Grothendieck, in categorical aspects, first came to attention in Harvard lecture notes (1962) by M. Artin on "Grothendieck topologies." Then

J. Giraud 1963 Grothendieck topologies on a category, *MR* 33#1343

I. Bucur 1964 Fonctions définies sur le spectre d'une catégorie et théories de décompositions, *MR* 32#5705

N. Popescu 1964 (with P. Gabriel) Caractérisation des catégories abéliennes avec générateurs et limites inductives exactes, *MR* 29#3518

J. E. Roos 1964 Sur la distributivité des foncteurs $\varprojlim$ par rapport aux $\varinjlim$ dans les catégories des faisceaux (topos), *MR* 32#5714

P.-A. Grillet 1965 Homomorphismes principaux de tas et de groupoïdes, *MR* 35#2989

A. G. Radu 1966 Quelques observations sur les sites, *MR* 37#272

A. Solian 1966 Faisceaux sur un groupe abélien, *MR* 36#245

An especially influential book appeared in 1967:

P. Gabriel and M. Zisman: Calculus of Fractions and Homotopy Theory, *MR* 35#1019

The American School:

John Isbell 1960 Adequate subcategories, *MR* 31#230

John Gray 1962 Category-valued sheaves, *MR* 26#170

J.-M. Maranda 1962 Some remarks on limits in categories, *MR* 29#135

A. Heller 1962 }
K. A. Rowe 1962 } On the category of sheaves, *MR* 26#1887

F. W. Lawvere 1963 Thesis (as already cited), *MR* 28#2143

P. Freyd 1963 The theories of functors and models (already cited), *MR* 34#1371

O. Wyler 1963 Ein Isomorphiesatz, *MR* 26#6096

G. M. Kelly 1964 On the radical of a category, *MR* 30#1157

B. Mitchell 1964 The full embedding theorem, *MR* 29#4783

M. Barr 1965 }
J. Beck 1965 } Acyclic models and triples, *MR* 39#6955

J. Beck 1965 Triples, Algebras and Cohomology (unpublished thesis 1967)

W. Burgess 1965 The meaning of mono and epi in some familiar categories, *MR* 33#161

J. F. Kennison 1965 Reflective functors in general topology and elsewhere, *MR* 30#4812

F. E. J. Linton 1965 Autonomous categories and duality of functors, *MR* 31#4821

J. Lambek 1966 Completions of categories, *MR* 35#228

J. A. Goguen 1967 *L*-fuzzy sets, *MR* 36#7435

J. L. MacDonald 1967 Relative functor representability, *MR* 36#5189

I. S. Pressman 1967 Functors whose domain is a category of morphisms, *MR* 35#4279

Finally one must mention an influential paper not really belonging to this school:

D. G. Quillen 1967 Homotopical algebra, *MR* 36#6480

The Ehresmann school in France was totally separate from the Grothendieck school:

J. Bénabou 1963 Catégories avec multiplication, *MR* 26#6225

M. Hasse 1963 Über die Erzeugung von Kategorien aus Halbgruppen, *MR* 28#152

G. Joubert 1965 Extensions de foncteurs ordonnés et applications, *MR* 33#1344

D. Leborgne 1966 Le foncteur Hom non abélien, *MR* 33#1345

L. Coppey 1967 Existence et construction de sommes finies dans une catégorie d'applications inductives entre classes locales complètes et dans la catégorie des applications "continues" entre paratopologies, *MR* 37#268

There were later many more members of this school.

Other categorists in France:

A. Preller 1966 Une catégorie duale de la catégorie des anneaux idempotents, *MR* 33#169

R. Pupier 1965 Sur les catégories complètes, *MR* 33#170

The low countries (Belgium, The Netherlands; hardly a "school", but just individuals):

P. Dedecker 1964 Sull'hessiano di taluni polinomi (determinanti, pfaffiani, discriminanti, risultanti, hessiani), *MR* 31#2239

F. Oort 1964 Yoneda extensions in abelian categories, *MR* 29#140

R. Lavendhomme 1965 La notion d'idéal dans la théorie des catégories, *MR* 31#3479

P. C. Baayen 1964 Universal morphisms, *MR* 30#3044

J. Mersch 1964 Structures quotients, *MR* 31#2298

P. Antoine 1966 Étude élémentaire des catégories d'ensembles structurés, *MR* 34#220

The Swiss school:

B. Eckmann 1961 }
P. Hilton 1961 } Group-like structures in general categories. I. Multiplications and comultiplications, *MR* 25#108

F. Hofmann 1960 Über eine die Kategorie der Gruppen umfassende Kategorie, *MR* 24#A1300

P. Huber 1961 Homotopy theory in general categories, *MR* 27#187

A. Frei 1965 Freie Gruppen und freie Objekte, *MR* 32#5708

H. Kleisli 1965 Every standard construction is induced by a pair of adjoint functors, *MR* 31#1289

M. André 1966 Categories of functors and adjoint functors, *MR* 33#5693

German categorists (hardly a "school"):

D. Puppe 1962 Korrespondenzen in abelschen Kategorien, *MR* 25#5095

J. Sonner 1962 On the formal definition of categories, *MR* 26#2483

B. Pareigis 1964 Cohomology of groups in arbitrary categories, *MR* 32#136
W. Felscher 1965 Adjungierte Funktoren und primitive Klassen, *MR* 33#2701
H. Brinkmann 1966 Lecture notes, Kategorien und Funktoren, *MR* 33#7388 (with D. Puppe)
D. Pumplün 1967 Das Tensorprodukt als universelles Problem, *MR* 35#2942
H. Herrlich 1967 On the concept of reflections in general topology, *MR* 44#2210

Eastern Europe (Czechoslovakia, D. D. R., Poland)
V. Šedivá-Trnková 1962 On the theory of categories, *MR* 26#3637
Z. Semadeni 1963 Free and direct objects, *MR* 25#5020
K. Drbohlav 1963 Concerning representations of small categories, *MR* 29#3520
H.-J. Hoehnke 1963 Einige Bemerkungen zur Einbettbarkeit von Kategorien in Gruppoïde, *MR* 27#3736
M. Hušek 1964 *S*-categories, *MR* 30#4234
A. Pultr 1964 Concerning universal categories, *MR* 30#3906

Z. Hedrlín 1965 } On the representation of small categories,
A. Pul'tr 1965 } *MR* 30#3123

L. Bukovský *et al.* 1965 On topological representation of semigroups and small categories, *MR* 33#160
A. Suliński 1966 The Brown-McCoy radical in categories, *MR* 34#1378
L. Budach 1967 Quotientenfunktoren und Erweiterungstheorie, *MR* 36#3852
M. Jurchescu, A. Lascu 1966 Strict morphisms, Cantorian categories, completion functors, *MR* 36#3845

Central and South America:
M. Hocquemiller 1963 Problème universel de catégorie, *MR* 34#7610
R. Vázquez García 1965 The category of the triples in a category, *MR* 36#2667

In such a list, the various papers are of quite different strengths; indeed category theory, as a new subject, does offer the possibility of writing papers that appear learned but are really unconsequential. The list does show the variety of interests in different "schools" and the common fact that there was a very active start in this research in the period 1962–67. It may be noted that this was a period when the mathematical community (at least in the USA) was rapidly expanding, and that previous larger "fields" of mathematics may at this time have tended to break up into subfields — a possibility needing more empirical study.

This extensive list of all sorts of contributions to this field is offered as a sample of the way in which a new field (or should we say, a new fashion) nowadays develops rapidly and on a world-wide basis.

The first conference on Category theory, sponsored by the US Air Force Office of Scientific Research (AFOSR), was held in La Jolla, California in June 1965. There the idea of categories with added structure was prominent; Eilenberg and Kelly lectured on closed categories (and enriched categories) *MR* 37#1432 and Lawvere spoke of "The category of categories as a foundation of mathematics," [*MR* 34#7332]. As the review of that paper notes, the axioms there proposed were not adequate but the ideas proposed led to extensive later studied (Benabou, Gray, Street) of 2-categories, bicategories, and related ideas.

At the end of the La Jolla conference the AFOSR representative privately told Eilenberg and Mac Lane that AFOSR could no longer support such research. This was at the beginning of the most fruitful 10 year period in the development of category theory. It may indicate that agency judgments of future prospects are not always on target.

18. Algebraic Theories and Monads

In universal algebra a group G would be described as a set G equipped with three operations: a binary operation $m\colon G\times G \to G$ of multiplication, a unary operation $v\colon G \to G$ giving the inverse, and a nullary operation $e\colon 1 \to G$ giveng the identity element (with 1 the one-point set); the operations are then subject to the usual identities as axioms. Each identity, such as the associative law, involves iterations such as $m(m \times 1) = m(1 \times m) G \times G \times G \to G$ of the three given operations. Lawvere's 1963 thesis took the decisive step of giving an "invariant" description of any such theory, in which all the iterated and composite operations would appear. Thus, in effect, he defined an *algebraic theory* $\mathbb{A}$ to be a category with denumerably many objects $A^0, A^1, \ldots, A^n, \ldots$ with each A^k given as a product of k factors A^1, with explicit projections (and A^0 as terminal object). In such a theory, the morphisms $A^n \to A^1$ are the n-ary operations. An algebra for the theory is a product-preserving function $T\colon \mathbb{A} \to \mathit{Sets}$, and one may similarly define the algebras for this theory in other categories. This elegant description, closely related to P. Hall's clones, certainly does provide the intended invariant description for a theory such as the theory of groups or of rings since it provides in this one category $\mathbb{A}$ all the derived operations of the theory, and of course the identities (such as the associative law) between them. It has been extended by Linton [*MR* 35#233] to include algebras with infinitary operations. Despite the elegant form, it has been neglected by most specialists in universal algebra, but expositions appear in the books by B. Pareigis [*MR* 42#337a,b] and by H. Schubert [*MR* 43#311] and by E. G. Manes [*MR* 54#7578].

Closely related inm the theory of a monad. A functor $F: \mathbb{X} \to \mathbb{E}$ with right adjoint $U: \mathbb{E} \to \mathbb{X}$ defines in the "base" category $\mathbb{X}$ a composite functor $UF = T: \mathbb{X} \to \mathbb{X}$ together with the natural transformations $\eta: I \to T$, the "unit" of the adjunction, and $\mu: T^2 \to T$, defined from the counit. This "triple" $\langle T, \eta, \mu \rangle$ satisfies identities like those for a monoid with multiplication μ and unit element η; such a structure was called a "triple" by Eilenberg-Moore and a monad by Mac Lane. The identities were actually first presented in Godement's rules for the functional calculus in his 1958 book on sheaf theory [*MR* 21#1583]. In 1965 Eilenberg and Moore named triples [*MR* 32#2455] and showed that every such triple in $\mathbb{X}$ arises from a pair of adjoint functors $F: \mathbb{X} \to \mathbb{E}$, $U: \mathbb{E} \to \mathbb{X}$ in which $\mathbb{E}$ is the category of algebras for the triple and F is the functor assigning to each object of $\mathbb{X}$ the corresponding free algebra. This elegant construction of algebras, including the case of algebraic theories, led soon to a considerable study of the structure semantics relation between the triple (the structure) and the semantics — its category of algebras. These relations were developed by Linton [*MR* 39#5655; 40#2730 and 42#6071] in part in a year-long seminar on triples and categorical homology theory, held in 1966–67 in the Forschungsinstitut für Mathematik at the E.T.H. in Zurich. There was also developed the use of triples and their dual, cotriples, in the study of the cohomology of algebraic systems — where the cotriple provided a way of constructing standard resolutions (M. Barr and J. Beck, [*MR* 41#3562]). A. Kock [*MR* 41#5446] and J. Duskin [*MR* 52#14006] developed other related ideas. The properties of monads (= triples) also play an axiomatic role in topos theory, as explained in Johnstone [**1977**] or in the [**1986**] book by Barr and Wells. But in general, this theory of monads is to be regarded as a natural (and inevitable) development of the basic notion of adjoint functor.

19. Elementary Topoi

After the discovery and exploitation of the properties of adjoint functors, the next decisive development in category theory was the axiomatization of elementary topoi. This comes from three or four different sources and from the examination of several different sorts of categories: The category $\mathbb{S}$ of sets, as in the *Elementary Theory of the Category of Sets* (ETCS) of §15 above, the category (Grothendieck topos) $\mathbb{E}$ of all sheaves of sets on a topological space or on a "site" (a category equipped with a Grothendieck topology, as in §13 above), functor categories $\mathbb{S}^{\mathbb{C}^{\infty}}$ (the category of all contravariant functors to sets from some small category $\mathbb{C}$) and the categories constructed by Scott and Solovay of Boolean valued models of set theory. We now know that each of these (types of) categories satisfies the axioms for an "elementary topos": A category $\mathbb{E}$ with all finite limits which is cartesian closed (that is, the functor $X \mapsto (--) \times X$ has a right adjoint, the exponential $\mapsto (\ )^X$), and which has a "subobject classifier" Ω. But the development of this formulation took time and many interactions between mathematicians; the following account is based in part on private communications from Lawvere and Tierney.

Thus Lawvere had struggled with the fascinating possibility of axiomatizing the category of all categories as a foundation for mathematics (§17 above); it was closely tied in to his axiomatization of ETCS (§15 above), while this axiomatics was developed by Lawvere under the stimulus of teaching able students at Reed College, 1963–64 (Courses and lectures generally have a lot to do with the articulation and development of mathematical ideas). P. Freyd had suggested to M. Bunge, a graduate student, the problem of axiomatizing functor categories such as $\mathbb{S}^{\mathbb{C}^{\infty}}$; after advice from Lawvere this resulted in her Ph.D. thesis [*MR* 38#4536]. In that magical year 1963, Paul Cohen had invented the process of forcing to prove the independence of the continuum hypothesis from the axioms of ZFC, while in 1966 D. Scott and R. Solovay developed a proof of this independence by the alternative method of Boolean-valued models. (This was presented by Scott in lectures at the 1967 summer institute on axiomatic set theory (UCLA); the mimeograph version of Scott's lectures was not published in the subsequent *Proceedings* of the symposium [*MR* 43#38], but there is a brief published note [*MR* 38#4300] and a published proof of Boolean-valued models of the independence of the continuum hypothesis [*MR* 36#1321]. (See also the book by J. L. Bell, [*MR* 87e#03118].) When Lawvere in 1966 learned of the Boolean-valued models the connection with ETCS and topos theory became clear to him; Gabriel's 1967 lectures at Oberwalfach on Grothendieck topoi also stimulated him.

The subobject classifier Ω in a category $\mathbb{E}$ is an object Ω and an arrow $t\colon 1 \to \Omega$ such that any subobject $S \hookrightarrow X$ of any X can be obtained from t by pullback along a unique map $\chi\colon X \to \Omega$; in the category of sets, this χ is just the characteristic function $\chi_S\colon X \to \{0, 1\}$ of the subset S, while Ω is the set $\{0, 1\}$ of the two classical truth-values. The symbol Ω in this sense apparently first cropped up in the initial IHES edition of SGA IV, where it is noted that the set $\Omega(X)$ of all subobjects of an object X in a Grothendieck topos $\mathbb{E}$ defines a sheaf for the "canonical" topology on $\mathbb{E}$ and so by Giraud's theorem, is representable by some object Ω. Apparently this idea was dropped in later editions of SGA IV, but Lawvere used it to develop the notion of subobject classifier as described above; in a lecture in March 1969, he noted its connection to Grothendieck topologies. In the summer of 1969 Lawvere also lectured on the probable connection between topoi and Boolean-valued models.

Myles Tierney, originally interested in topology, started to study Grothendieck topoi in 1968; with Alex Heller he conducted in 1968–69 a New York seminar on Grothendieck topologies and sheaves. He saw Lawvere at Albrecht Dold's house in Heidelbert in summer 1969, where he and Lawvere decided on a joint research project on "Axiomatic Sheaf Theory".

Then in 1969, Lawvere became for two years the Killam Professor at Dalhousie University in Halifax, and in this connection was able to invite about a dozen people to come to Dalhousie as Killam fellows; they included R. Diaconescu, A. Kock, F. E. J. Linton, E. Manes, B. Mitchell, R. Paré, M. Thiebaud, M. Tierney, and H. Volger. The 1969–70 seminar on axiomatic sheaf theory presented weekly lectures by Tierney with contributions by Lawvere. The intent was to axiomatize the category $\mathbb{E}$ of sheaves (on a site); it was soon clear that the axioms should be stable under passage to a comma category $\mathbb{E}/X$ or to the category $\mathbb{E}^G$ of objects X of E with a G-action, where G is an internal group (or an internal monoid) in $\mathbb{E}$. It was also important that the axioms apply to the "classifying topos" of any "geometric theory". It was especially important that any "Grothendieck topology" J in $\mathbb{E}$ should yield a category $\mathbb{E}_j$ of J-sheaves which would itself be a topos. This involved the classical "sheafification" construction by which a presheaf on a space X is turned into its associated sheaf by a double application of a suitable functor L. This classical use of $L \circ L$, used both for topological spaces and for Grothendieck topologies, did not seem to work under the topos axioms (much later, it was carried out for topoi by P. Johnstone, [*MR* 50#10002]). In this complicated set of requirements, Lawvere found a new method of sheafification which did apply under the axioms, while Tierney showed that the original relatively complicated definition of a Grothendieck topology could be replaced by the simple definition of such a topology as a "modal operator" $j : \Omega \to \Omega$ satisfying just three conditions (idempotent, preserves t and preserves product). This discovery, together with the use of the subobject classifier Ω to define a "partial map" classifier, combined to produce the desired effective axiomatization of an elementary topos: A category $\mathbb{E}$ with all finite limits and colimits, cartesian closed, and with a subobject classifier Ω.

These are the axioms presented by Lawvere in his lecture at the 1970 International Congress of Mathematicians at Nice [*MR* 55#3029]. (By coincidence, it was at this same conference that Grothendieck announced his shift of interest to political questions; other such questions had preoccupied Lawvere at Halifax.) The important connection with forcing and Boolean-valued models was later presented by Tierney in an AMS invited lecture (after a Springer lecture notes publication [*MR* 51#10088]. There, starting with a functor category $\mathbb{S}^{\mathbb{C}^{\infty}}$ with a well chosen $\mathbb{C}$ and an appropriate topology j, the category of j-sheaves essentially provides a model of set theory which shows the independence of the continuum hypothesis from ZFC. The idea was close to Cohen's original forcing technique: Cohen's poset of "conditions" appears as the category $\mathbb{C}$ and the forcing relation is mirrored by sheafification. It is a remarkable connection between geometry (sheaves) and logic.

These two papers, by Lawvere and Tierney, each refer to the other as a collaborator, and thus present striking joint work in developing elementary topos theory. With this opening, many categorists saw the promise of this new development; refinements followed fast. Benabou's seminar in Paris 1970–71 produced under the title "Généralités sure les topos de Lawvere et Tierney" the first available set of lecture notes on the subject.Then Kock and Wraith in 1971 [*MR* 49#7324] provided a much used set of notes, moreover there were notes of Tierney's 1971 lectures at Varenna [*MR* 50#7277]. Julian Cole explained the connection between ETCS and a weak form of Zermelo set theory (at the Bertrand Russell Memorial Logic Conference in 1971 in Denmark [*MR* 49#4747]); see also Osius [*MR* 51#643]. Lawvere contributed a stimulating introduction to a 1972 lecture notes volume on topos theory [*MR* 51#12973] while P. Freyd's influential 1972 paper on *Aspects of Topoi* elucidate many aspects, and in particular contained many embedding theorems. C. J. Mikkelsen at this time simplified the axioms, by using sophisticated methods to deduce the existence of colimits in a topos from the other axioms; his results were published much later [*MR* 55#2572]. Subsequently, R. Paré used properties of the monad given by the iterated power set functor to give a much quicker proof of the existence of colimits [*MR* 48#11245]. There were other expositions, as in lectures by Mac Lane in Chicago, Heidelberg and (1972) in Cambridge, England. Peter Johnstone, who started on the subject from the lectures of Tierney and Mac Lane, subsequently prepared his definitive 1977 book *Topos Theory* [*MR* 57#9791]; there is a more recent exposition *Toposes, Triples and Theories* by M. Barr and C. Wells [**1985**].

Some of the relations of topos theory to logic were explored by M. Makkai and G. E. Reyes in their monograph *First Order Categorical Logic* [*MR* 58#21600]; there is a more recent book by J. Lambek and P. Scott [**1986**]. All told, the development of topos theory provides a remarkable and fruitful connection between geometry and logic.

The principal architects of topos theory are Grothendieck and his associates on the one hand and Lawvere and Tierney on the other. But it is also notable that the rapid development of the subject involved many other mathematicians, and depended on many conferences and meetings, in Oberwolfach, Halifax, Aarhus and elsewhere. With the present large and varied mathematical community, it would seem that here and in other cases new ideas develop rapidly with input from many hands and many lands. This environment seems strikingly different from that at the time of the origination of category theory; the AMS invited lectures may no longer have the same impact they once had.

20. Later Developments

With the rapid exploitation of topos theory there were also other active aspects of categories, some of which we now mention briefly. Most developments since 1973 are omitted, since it may be too soon to judge their historical importance.

At the 1965 La Jolla conference, Eilenberg and Kelly lectured on closed categories. These categories V (such as the category of abelian groups) are equipped with a tensor product $\otimes$, which is associative and commutative up to coherent canonical isomorphisms and closed in the sense that the functor $-\otimes B\colon V \to V$ has a right adjoint $[B, -]$, called the internal hom. For these categories the coherence theorem (all diagrams of canonical isomorphisms are commutative) holds only with limitations, and the proofs involve connection with Gentzen's cut elimination theorems of proof theory (Kelly-Mac Lane, [*MR* 44#278]). A category *enriched* over the closed category V is one with its "hom set" in V; thus an abelian or an additive category is one enriched over the closed category of abelian groups. There are many such enriched categories; for them one can carry over most of the properties of "ordinary" categories including the Yoneda lemma as set forth in the comprehensive book by G. M. Kelly [*MR* 84e#18001].

Cat, the category of all categories, has a murky epistemological existence (the set of all sets?); it also appears as a tentative foundation for mathematics, in Lawvere's talk at the La Jolla conference. It has three kinds of things: its objects are categories, its arrows are functors and its "2-cells" are natural transformations between functors. Other structures of objects, arrows and 2-cells with suitable axioms are the 2-categories, widely studied (with their 2-limits) in Australia (R. Street [*MR* 50#436 and 53#585]); see also the systematic treatise by J. Gray [*MR* 51#8207]. In many related cases the composition of arrows is associative only up to an isomorphism given by a 2-cell; one then speaks of a bicategory (J. Benabou, [*MR* 36#3841]). There are good reasons to consider not just 2-categories but also n-categories and even ($n = \infty$) ∞-categories. On the other hand, Ehresmann early observed that squares (regarded as arrows), have two compositions, horizontal and vertical, and so constitute a *double category*. The corresponding n-fold categories (arrows, such as n-cubes, with n commuting composition structures) have entered into the study of homotopy types of spaces, as in a theorem of Loday using a group object in $\mathbf{Cat}^n$, the category of all n-fold categories (J.-L. Loday [*MR* 83i#55009]).

This process (the contemplation of, say, a group object or a ring object in an ambient category with products) has proved to be conceptually very handy, especially in the study of internal categories and functors in, say, a topos — an idea often hard for a beginner to appreciate.

Beginning in 1970 there was an active school of category theory in Germany, starting with the publication of the systematic treatise "Kategorien" by H. Schubert (*MR* 43#311 and 50#2286). In 1971 P. Gabriel and F. Ulmer published their influential paper "Locally presentable categories" (*MR* 48#6205), with an extensive treatment of categories of models, in particular of algebraic theories.

The topologically important notion of a fibration has its categorical analog, the fibered categories $p: \mathbb{F} \to \mathbb{E}$ with a category as the inverse image under p over each object of $\mathbb{E}$, with suitable "pullback" along arrows of $\mathbb{E}$. The notion is due to Grothendieck who observed its equivalence to the notion of a "pseudofunctor" from $\mathbb{E}$ to $\mathbb{C}at$, assigning to each object X of $\mathbb{E}$ the fiber over X. The idea has been extensively developed by Benabou ([*MR* 52#13991] and unpublished) and, in the alternative presentation as an indexed category, by R. Paré and D. Schumacher [*MR* 58#16816]; there has been some controversy as to method, perhaps settled by a coherence theorem for indexed categories (Mac Lane-Paré, [*MR*k#18003]).

Categories are now taken for granted in algebraic geometry; when Grothendieck retired from the mathematical scene, the fashion in algebraic geometry shifted dramatically to more concrete problems about specific manifolds. A topos had provided a setting in which one could effectively formulate many cohomology theories, with the objective of finding one for which the Lefschetz fixed point theorem would resolve the famous Weil conjectures. These conjectures were settled by DeLigne [*MR* 49#5013], using only part of the apparatus of SGA IV; this led to his publication of a shorter version, SGA $4\frac{1}{4}$ [*MR* 57#3132]. On the other hand, Falting's famous solution of the Mordell conjecture on diophantine equations made use of the full panoply of techniques of arithmetic algebraic geometry, including many ideas due to Grothendieck [*MR* 85e#11026a,b]. For that matter, an unpublished long manuscript by Grothendieck **[1985]** (starting with a letter to Quillen) studies categories (like the category of simplicial sets) which have suitable categories of fractions equivalent to the category of homotopy types of spaces.

Algebraic K-theory currently makes extensive use of many categories, in particular categories of simplicial sets in order to study manifolds M, both topological and piece-wise linear (PL). A central issue is the use of groups Top(M) or PL(M) of all topological or PL-homeomorphisms of M. Now the category PL does not have exponentials (function spaces), and this may be the basic reason that in this study one shifts from PL manifolds to polyhedra and then to simplicial sets: The category of simplicial sets is a functor category and hence an elementary topos — and so does have exponentials. All told, K-theory displays a remarkably effective use of categories as a language, as in the original intent of Eilenberg-MacLane.

At the same time, the original connections of categories with topology have prospered. The idea of homology and cohomology as functors with axiomatic properties does include many new types of extraordinary homology theories, and categorical techniques such as operads are essential tools in the handling of iterated loop spaces. For many purposes, with R. Brown and N. Steenrod, one carries out topology in a "convenient category" of topological spaces — one where exponentials are possible. For conceptual reasons, the idea of a topological space may be effectively replaced by the notion of a locale (e.g., a lattice such as the lattice of open sets). In homotopy theory, P. Freyd's generating hypothesis for stable homotopy is still an active subject of study; it was originally proposed by Freyd in *MR* 41#2675.

In topos theory, it has gradually become clear that every topos is a set-theoretic universe with its own "internal" logic, which is intuitionistic. Previously elaborate arguments about commutative diagrams in a topos can be formulated expeditiously in the Mitchell-Benabou language, with variables functioning as if they were set-theoretic elements (see Boileau-Joyal (*MR* 82a#03063) or Bruno [**1984**]). In this way, much of mathematics can be carried out in a topos. This point of view has been vigorously advocated by A. Joyal, who showed that in fact every topos can be viewed as a forcing extension in which the site is interpreted as a category of forcing conditions, using the so-called Kripke-Joyal semantics.

Lawvere's original 1960 interest in dynamics reappeared in 1967, in reaction to a Chicago course given by Mac Lane on classical Hamiltonian dynamics, treated with the techniques of modern differential geometry. Then in a seminar, Lawvere lectured on *Categorical Dynamics* — making the proposal that there could be a category containing the C^∞ differentiable manifolds and a real line object R with a suitable subobject $D \subset R$ of infinitesimals of square zero (or, as the case may be, of cube zero, etc.). With these infinitesimals one could carry out rigorously the informal treatment of Lie groups and differential forms in the style of S. Lie and Elie Cartan.

This proposal of Lawvere, made in several different presentations, lay fallow for many years, until it was revived in 1978 by his former student A. Kock [*MR* 58#18529], who renamed the subject "synthetic differential geometry" (SDG) and published, in [*MR* 80i#18002], a version of Lawvere's original 1967 lecture (in my view, this version has been rewritten with hindsight and so is not quite a historical document). This has led to a flurry of activity; E. Dubuc [*MR* 83a#58004] has used the C^∞ analog of the schemes of algebraic geometry to introduce a model topos in which the desired Lawvere axioms on the infinitesimal object D in the line can be realized, and by now there are three texts presenting SDG — a first version by Kock [**1981**] [*MR* 83f#51023], an elementary text by Lavendhomme [**1987**], and a treatise by Moerdijk and Reyes [**1988**]. It is still too early to judge the possible effect of these lively developments on differential geometry and Lie groups. It is also hard to know about the depth of the connection with continuum mechanics, advertised by Lawvere in the lecture notes *Categories in Continuum Physics* [*MR* 87h#73001].

Many other topics have been omitted here: The remarkable presence of intuitionistic logic "internally" in a topos, the semantics of sketches (Ehresmann, Barr-Wells) the use of categorical ideas in describing homotopy limits (Bousfield and Kan [*MR* 51#1825]), the application of props and operads to describe homotopy-everything spaces, and the remarkable work of G. B. Segal on categories and cohomology theories [*MR* 50#5782].

If this account of current work omits many other thrusts and seems to leave many obscurities and loose ends, that is, I think, inevitable. The progress of mathematics is like the difficult exploration of possible trails up a massive infinitely high mountain, shrouded in a heavy mist which will occasionally lift a little to afford new and charming perspectives. This or that route is explored a bit more, and we hope that some will lead on higher up, while indeed many routes may join and reinforce each other. For the present it is hard to know which of many ways is the most promising, or which of many new concepts will illuminate the road up.

21. The Combination of Concepts

This essay has been a tentative exploration of the origins and development of the notions of category theory. This theory exemplifies the conceptual aspects of mathematics, in contrast to the problem-solving aspects. Now the solution of a famous old problem is at once recognizable as a major advance. It is not so with the introduction of a new concept — which may or may not turn out to be useful, or which may later turn out to be really helpful in wholly unanticipated ways, as for example, in the current use of simplicial sets in the study of algebraic K-theory or in the use of categories to handle many-sorted data types. Sometimes a conceptual advance may assist in the solution of an explicit problem, as in the use of Grothendieck's categorical concepts in the solution of the Weil conjecture or (on a small scale) in the use of group extensions to clarify Steenrod's homology of solenoids (§9 above).

Concepts and computations interact. Thus the explicit formulas for the Eilenberg-Zilber theorem are illuminated (and made inevitable) by the notion of acyclic models (§10 above), while the notions of exact sequence facilitated both homotopy and homology computations (and lead on to more complex concepts, such as spectral sequences). The simple notion of a functor made possible axiomatic homology theory and the organization of generalized homology theories. In the long run, the merit of a concept is tested by its use in illuminating and simplifying other studies.

New concepts may be accepted promptly, slowly, or not at all. Thus my own attempt to legislate strict duality between subobjects and quotient objects in abelian categories (§12 above) was mistaken, and has disappeared. Categories were accepted slowly, or dismissed as "general abstract nonsense". Lattices as discovered by Dedekind in 1900 were at once lost from sight, but became immediately popular upon rediscovery in the 30s by Birkhoff and Ore — perhaps because the then new emphasis on modern algebra made them acceptable. Bourbaki enjoyed instant popularity, but is now criticized for lack of attention to applications. For fifteen years after Zermelo, axiomatic set theory was hardly noticed, but is now a firm item of belief. Fashion may play some role in these varied events, but it would seem ultimately that a new concept is really accepted only when it has demonstrated its power: Categories became a subject of research only after the discovery of adjosint functors. And set theory without elements is still unpalatable to those trained from infancy to think of sets with elements: Habit is strong, and new ideas hard to accept.

Major new conceptual development appears to take place slowly, and in stages; it seems to require many hands to bring an array of novel ideas into effective form. Thus integral equations led to the Hilbert space l^2 of sequences and only later to an axiomatically defined Hilbert space. Set theory and its axiomatics travelled a long road from Boole and Schroeder to Cantor and Dedekind, then to Zermelo, Skolem, Fraenkel, von Neumann, Bernays and Gödel — with subsequent changes by Paul Cohen, Scott-Solovay and even by sheaf theory. The basic notions of category theory were perhaps inevitable ones, but they too came in successive stages: maps represented by arrows (Hurewicz), then exact sequences, then categories and functors (Eilenberg-Mac Lane), next universal constructions (many people), adjoint functors (Kan), monads, ETCS (Lawvere), categories of sheaves (Grothendieck and associates) and elementary topoi (Lawvere-Tierney). It may be that each successive advance needs a fresh impetus from a new thinker, courageous and foolhardy enough to evisage and advocate an unpopular idea. The advance of mathematics may depend not just on power and insight, but also on audacity.

In the current circumstances, with a large and international mathematical community, the interaction between many different workers at conferences, seminars, and lectures seems to be vital. For categories, the group around Eilenberg at Columbia University in the early 1960s (§17 above) was important, as were the Grothendieck seminars at the Institute des Hautes Etudes. The La Jolla conference in categories in 1965 was followed by a year long seminar at Zurich, meetings of the "Midwest category seminars" in Chicago [*MR* 36#3840, *MR* 37#6341, 41#8487 and 43#4873], seminars arranged by Tierney in New York, by Kock in Aarhus, meetings at Oberwolfach, and currently by the peripatetic seminars in Europe and meetings in Montreal. The exchange of ideas at such meetings runs in parallel with journal publication in the development of new concepts. (Citation indices miss a good bit!) Beginning at Oberwolfach in 1972, there has been a week-long category meeting in Europe practically every summer. At these meetings, all those in attendance have a chance to give a talk; the resulting stimulus assists the development — and also emphasizes the separation of specialists in category theory from other parts of mathematics. This sort of separate specialization occurs today, to my regret, in many parts of mathematics.

All this bears on the progress of mathematics as a whole; this progress involves not just the solution of old problems and the discovery of remarkable theorems, but also the introduction and testing of new and sometimes shocking concepts — concepts which can illuminate past results and serve — often in unexpected ways — to make possible new advances. As I have argued at length (*Mathematics: Form and Function*, **[1986]**), mathematics presents an elaborate network in which the form (the concepts) organize and illuminate the function (solutions of problems and relations to the real world).

Appendix

Categories in Prague. Dr. J. Adamek has provided me with an interesting sketch of the origins in the 1960s of the extensive study of categories in Czechoslovakia. About 1960, A. G. Kurosh from Moscow lectured at Prague about categories. This continued with a study of the paper of Kurosh, Lifshits, and Sulgeifer (*MR* 22#9526) in the major seminar on general topology conducted by M. Katětov; this led in turn to the early papers of V. Trnkova and M. Hušek. In Amsterdam, J. de Groot was studying topological spaces with prescribed group actions; Z. Hedrlín visited there, and this was the origin (groups to monoids to categories) of the work of Hedrlín and A. Pultr on embeddings of general categories into specific concrete categories. The seminar of E. Čech and the Eilenberg-Steenrod book on axiomatic homology theory was a third source of the interest in categories. This interest has continued and developed since that time.

Categories in Belgium started as reported to me by Francis Borceux, with the work of J. Mersch, P. Dedecker and R. Lavendhomme. Mersch had studied in Paris, where he presented a thesis in 1963, under the supervision of Ehresmann, on the problem of quotients in categories. Lavendhomme first learned about categories in a lecture by Peter Hilton at Leuven, and then studied Kan's paper on adjoint functors. Subsequently, Dedecker interested him in questions of cohomology.

For information and helpful comments on earlier drafts of this article, I would like to thank Jack Duskin, Peter Freyd, John Gray, John Isbell, Max Kelly, Bill Lawvere, Peter May, Ieke Moerdijk and Myles Tierney. They should not be held responsible for the judgments I have expressed.

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