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ABSTRACT

This is one in a series of SMSG supplementary and enrichment pamphlets for high school students. This series makes available expository articles which appeared in a variety of mathematical periodicals. Topics covered include: (1) the nature of mathematics: (2) mathematical inutility and the advance of science: and (3) logic. (MP)

Financial support for the School Mathematics Study Group has been provided by the National Science Foundation.

Mathematics is such a vast and rapidly expanding field of study that there are inevitably many important and fascinating aspects of the subject which do not find a place in the curriculum simply because of lack of time, even though they are well within the grasp of secondary school students.

Some classes and many individual students, however, may find time to pursue mathematical topics of special interest to them. The School Mathematics Study Group is preparing pamphlets designed to make material for such study readily accessible. Some of the pamphlets deal with material found in the regular curriculum but in a more extended manner or from a novel point of view. Others deal with topics not usually found at all in the standard curriculum.

This particular series of pamphlets, the Reprint Series, makes available expository articles which appeared in a variety of mathematical periodicals. Even if the periodicals were available to all schools, there is convenience in having articles on one topic collected and reprinted as is done here.

This series was prepared for the Panel on Supplementary Publications by Professor William L. Schaaf. His judgment, background, bibliographic skills, and editorial efficiency were major factors in the design and successful completion of the pamphlets.

PREFACE

Mathematics is a magnificent cultural heritage. Although its origins are obscure, it is presumably one of the oldest concerns of man, reaching back into time by some six thousand years. Next to the invention of language itself, the creation of mathematics is without doubt the most subtle, powerful, and significant achievement of the human mind.

Just what *is* mathematics? This is more easily asked than answered, at least in any brief sort of way. Mathematics has been described as the "science of self-evident truths"; it has been called the "science which draws necessary conclusions"; it has been defined as the "universal art apodeictic." Bertrand Russell's celebrated characterization of mathematics as the "subject in which we do not know what we are talking about or whether what we say is true" is illuminating if it is not misinterpreted. The late Edward Kasner, in his whimsical way, called mathematics the science "that uses easy words for hard ideas."

A sophisticated observer might say that mathematics is what a mathematician does. But what *does* a mathematician do? Or we might ask: How does he go about doing whatever it is that he does? Why does he do these things? Who really *cares* what a mathematician does? You will find some answers to these questions in the essays that follow. But we should like to give you a bit of perspective first.

You must realize that mathematics has had a long history and that it is a far cry from pre-Babylonian mathematics to the mathematics of today. Furthermore, during this long span there were many "ups and downs." There were barren gaps and intervals of stagnation, as well as brilliant break-throughs and lucid periods of creativity. In this connection, there are several ways of regarding the development of mathematics. The conventional interpretation suggests that this development has been an evolutionary process in which successive generations of mathematicians built upon the contributions of their predecessors without tearing down what had gone before (Hankel). A less conventional viewpoint, suggested by Spengler in his *Decline of the West*, holds that there never was a "continuous growth" of a single discipline called mathematics; what seems to be the development of a single broad subject actually is a plurality of independent mathematic(s). Each new and independent mathematic follows another, not by building upon its predecessor, but by repudiating it and reflecting a subsequent alien culture.

Perhaps a more realistic approach than either of these is that suggested by E. T. Bell, namely, a series of rather distinct epochs, each of which grew by expansion or extension out of the "residue" of the previous epoch. Professor Bell has indicated seven such major epochs, together with newer concepts or more powerful tools which survived from the earlier period and heralded the beginning of the next epoch.

Be that as it may, we are here concerned with the nature of mathematics as we know it today. Stated very briefly and by no means completely, contemporary mathematics is characterized by its sweeping generalizations; its pure abstractions; its succinct symbolism; its precision of language; its ideals of rigorous thinking; and above all, its prime concern with patterns of ideas, with the structure of forms, and with the qualities of relationships. The essence of mathematics is that, unlike the physical sciences, it is wholly "man-made", being essentially independent of the external world. In this sense, it is an art and not a science.

What is described in these essays is the mathematics of today. What of the mathematics of tomorrow? It has been suggested by Bell that we may find it necessary to give up the idea of "one all-inclusive kind of mathematics." We may have to abandon the concept of the continuous and embrace the discrete in mathematics. What such mathematics will be like, nobody can foretell: "the mathematics of the twenty-first century may be so different from that of the twentieth century that it will scarcely be recognized as mathematics."

—William L. Schaaf

Contents

THE NATURE OF MATHEMATICS 3

MATHEMATICAL INUTILITY AND THE ADVANCE OF SCIENCE . . 13

ARE LOGIC AND MATHEMATICS IDENTICAL? 23

BIBLIOGRAPHY 35

FOR FURTHER READING AND STUDY 36

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LEON HENKIN, "Are Mathematics and Logic Identical?", vol. 138 (Nov. 16, 1962), pp. 788-794.

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FOREWORD

If one may be allowed a slight oversimplification, modern mathematics is characterized largely by its concern for abstract structures, while mathematics prior to the nineteenth century leaned somewhat more heavily on intuition. Yet, as pointed out in the present essay, the interplay of intuitive considerations and abstract formulations has proved exceedingly fruitful.

In this discussion of the nature of mathematics, the author appropriately comments upon the relation of mathematics to science (a matter which is again discussed in the second essay). The question has often been raised: How is it that mathematics so "aptly" describes Nature? Some years ago, Heisenberg gave an illuminating answer:

> "On the one hand, mathematics is a study of certain aspects of the human thinking process; on the other hand, when we make ourselves master of a physical situation, we so arrange the data as to conform to the demands of our thinking process. It would seem probable, therefore, that merely in arranging the subject in a form suitable for discussion we have already introduced the mathematics — the mathematics is unavoidably introduced by our treatment, and it is inevitable that mathematical principles appear to rule nature."

Perhaps one of the most perceptive observations made in this connection was made over a quarter of a century ago by J. W. N. Sullivan in the following words:

> The significance of mathematics resides precisely in the fact that it is an art; by informing us of the nature of our own minds it informs us of much that depends on our minds. It does not enable us to explore some remote region of the eternally existent; it helps to show us how far what exists depends upon the way in which we exist. We are the law-givers of the universe; it is even possible that we can experience nothing but what we have created, and that the greatest of our mathematical creations is the material universe itself.
>
> We return thus to a sort of inverted Pythagorean outlook. Mathematics is of profound significance in the universe, not because it exhibits principles that we obey but because it exhibits principles that we impose.

The Nature Of Mathematics

Both Constructive Intuition and the Study of Abstract Structures Characterize the Growth of Mathematics.

MINA REES

Some of the most noted mathematicians and philosophers have addressed themselves to a discussion of the nature of mathematics, and I can hope to add very little to the ideas they have expressed and the insights they have given; but I shall attempt to draw together some of their ideas and to view the issues in the perspective that seems to me appropriate to the present state of mathematical scholarship, taking account of the great increases that have been taking place in the body of mathematical learning, and of the changes in viewpoint toward the old and basic knowledge that grow out of deeper understandings brought about by generations of mathematical research.

In discussions of this subject we find a sharp difference in the views of able mathematicians. This reflects the concern of some that the trend toward abstraction has gone too far, and the insistence of others that this trend is the essence of the great vitality of present-day mathematics. On one thing, however, mathematicians would probably agree: that there are and have been, at least since the time of Euclid, two antithetical forces at work in mathematics. These may be viewed in the great periods of mathematical development, one of them moving in the direction of "constructive invention, of directing and motivating intuition" *(1)*, the other adhering to the ideal of precision and rigorous proof that made its appearance in Greek mathematics and has been extensively developed during the 19th and 20th centuries.

The first position, that the emphasis on abstraction has gone too far, is presented by Courant and Robbins in *What Is Mathematics?* though their position is modified by their recognition of the power of the axiomatic method and the deep insights it has made possible. They say, in part *(1)*: "A serious threat to the very life of science is implied in the assertion that mathematics is nothing but a system of conclusions drawn from definitions and postulates that must be consistent but otherwise may be created by the free will of the mathematician. If this description were accurate, mathematics could not attract any intelligent person. It would be a game with definitions, rules and syllogisms, without motivation or goal. The notion that the intellect can create meaningful postulational systems at its whim is a deceptive half-truth. Only under the discipline of responsibility to the organic whole, only guided by intrinsic necessity, can the free mind achieve results of scientific value."

The second point of view is represented classically by Bertrand Russell's famous definition of mathematics as the "subject in which we do not know what we are talking about or whether what we say is true." Of this view Marshall Stone has this to say *(2)*: "A modern mathematician would prefer the positive characterization of his subject as the study of general abstract systems, each one of which is an edifice built of specified abstract elements and structured by the presence of arbitrary but unambiguously specified relations among them." Stone says in two other passages *(3)*: "While several important changes have taken place since 1900 in our conception of mathematics or in our points of view concerning it, the one which truly involves a revolution in ideas is the discovery that mathematics is entirely independent of the physical world . . . At the same time . . . mathematical systems can often usefully serve as models for portions of reality, thus providing the basis for a theoretical analysis of relations observed in the phenomenal world." "Indeed, it is becoming clearer and clearer every day that mathematics has to be regarded as the corner-stone of all scientific thinking and hence of the intricately articulated technological society we are busily engaged in building."

In the history of mathematics the emphasis in research is sometimes on constructive intuition and the acquisition of results without too much concern for the strict demands of logic, sometimes on the insights gained by the identification and study of abstract systems within a carefully designed logical framework. But over the years the body of mathematics moves forward inevitably with growth in both directions. An individual mathematician chooses to work on one frontier or the other and the emphasis changes from one period to another, but mathematics as a whole and the community of mathematicians have their obligation to the total spectrum. For mathematics is the servant as well as the queen of the sciences, and she weaves a rich fabric of creative theory, which is often inspired by observations in the phenomenal world but is also inspired often by a creative insight that recognizes identical mathematical structures in dissimilar realizations by stripping the realizations of their substance and concerning itself only with undefined objects and the rules governing their relations.

As von Neumann has said *(4)*: "It is a relatively good approximation to truth . . . that mathematical ideas originate in empirics, although the genealogy is sometimes long and obscure. But, once the, are conceived, the subject begins to live a peculiar life of its own and is better compared to a creative one, governed by almost entirely aesthetical motivations."

Euclid and the Parallel Postulate

With this introduction, it will be useful to consider briefly those episodes in the history of mathematics that play a decisive role in the development and understanding of this dichotomy. The Greeks made fundamental contributions in parts of mathematics other than geometry; in addition to Archimede's wide-ranging interest in applications, I cite only Euclid in number theory and Eudoxus in analysis. But the failure of the Greeks to develop adequate symbols with which to express many of their ideas made their treatment of these subjects cumbersome. Through Euclid's *Elements,* however, they contributed to mathematics the ideal of the development of a body of knowledge proved by logical deduction on the basis of a limited number of axioms, a concept that has exercised enormous influence.

One of the greatest of Euclid's contributions to geometry was his recognition that the parallel postulate could not be derived from the others. For 2000 years after Euclid, the development of geometry is characterized by attempts to prove the parallel postulate. At last, in the time of Gauss at the beginning of the 19th century, the problem was solved. And what a solution! A geometry developed independently in Germany by Gauss, in Hungary by the Bolyais, and in Russia by Lobatchevski in which this postulate does not hold, and in which the sum of the angles of a triangle is less than 180 degrees. Interestingly enough, Gauss's impulse was to check to determine whether our physical world (and here he meant only the earth on which we live) was described by Euclidean or by this new non-Euclidean geometry. He found that his instruments were not good enough to discriminate; but it is of some interest to recall that the non-Euclidean geometry developed later by Riemann, in which the sum of the angles of a triangle is greater than 180 degrees, was found by Einstein to provide a satisfactory framework within which to develop his ideas of the physical universe. In passing, it should be noted that the parallel postulate, unlike the others, deals with lines that cannot be described by finite considerations. Infinity early raised difficulties for mathematicians, and the subsequent development of our subject sees infinity introducing new and exciting vistas, which, however, are recurrently accompanied by logical problems that have caused an upheaval in mathematical thought.

The successful denial of the parallel postulate — the recognition that the assumption of a contradictory postulate could be used as the basis for the description of a consistent geometry, one which in fact proved later to be useful in describing the physical universe — opened up a whole new world to mathematicians. The requirement that axioms be self-evident became meaningless, and in its stead were substituted the requirements of consistency and completeness. Exploration of this new-found freedom in the choice of axioms led to the development of many other kinds of abstract geometry, and, in algebra, there was a veritable feast of new ideas, as new number systems were explored by varying one axiom after another, or by recognizing, after the discovery of new systems, that their essential structure could be described in terms of an axiom system closely related to one that was well known but different from it in one or more of its axioms. The axiomatic method has provided deep insights into mathematics, disclosing identities where none had been suspected. In the hands of mathematicians of genius this method has been used to strip away exterior details that seem to distinguish two subjects and to disclose an identical structure whose properties can be studied once for all and applied to the separate subjects. Thus, if we consider three familiar ideas — the addition of real numbers, the multiplication of the numbers in a finite number field, and the result of performing in succession two displacements in Euclidean space — and, for all three, study only the skeleton remaining when each is thought of as a set of abstract elements with an appropriate law of combination, we quickly see that each can be described as a group. And properties of the three may be studied together by the axiomatic theory of groups. The nature of the elements is irrelevant to the study of the properties that follow from the axioms.

The group is an example of one of the three basic mathematical structures that we now recognize. It is one kind of so-called "algebraic" structure. The other two basic structures are called "ordered" and "topological," and each can be described abstractly, the first concerning itself with a generalization of the usual "less than or equal to" relation, the second with the notion of continuity. Modern mathematics is increasingly concerned with systems that satisfy at once the axioms for two different kinds of structure. An example of this is given by the complex numbers. When at the beginning of the 19th century the great discovery was made that complex numbers could be represented geometrically in the Euclidean plane (a familiar topological space), all the available insights about the plane could be used to gain familiarity with the nature of complex numbers.

Many systems, such as the complex numbers, can be characterized by a conjunction of the properties of two of the three kinds of basic structure. And there are many contemporary mathematicians who are interested in the study of known mathematical systems in terms of algebras, ordered systems, and topological spaces.

From Euclid to Gauss

In moving into a discussion of the axiomatic method, I omitted any mention of the great eras of mathematical development from the time of Euclid to the time of Gauss. But it was in this intervening period that a domain wide-flung and vastly influential was conquered by mathematicians whose driving force was intuition and construction, who ignored the axiomatic approach of the Greeks and made brilliant leaps on the basis of intuition, analogy, and guesswork. One need only mention the names of Descartes, Fermat, Pascal, Newton, Leibnitz, and Euler to indicate the vast scientific territories that were conquered in the 16th and 17th centuries. Analytic geometry, many facets of analysis and number theory, probability theory, and the calculus were initially developed in these centuries (5). And later centuries have seen this kind of mathematical discovery continue and expand. It is of interest that the contemporary French mathematician Hadamard takes the position that "the object of mathematical rigor is to sanction and legitimize the conquests of intuition." As we emphasize the deductive structure of our science and of acceptable proof, let us not lose sight of the fact that many of the most significant results that we prove were arrived at by guesswork, by intuition, by brilliant insight.

Role of the Unsolved Problem

The role of the mathematical conjecture, of the unsolved problem in the development of mathematical ideas, should be pursued further. In periods of great mathematical activity there has always been a lively interchange among mathematicians. The long attempt to prove the parallel postulate and the revolutionary impact of the discovery that it was independent of the others have already been mentioned. Other great problems whose solutions were decisive milestones in the history of mathematics are well known. The early assumption that all ratios of lines are rational was disproved when the Pythagoreans established that the ratio of the diagonal of a square to its side is irrational, or, as we would say, that the square root of 2 is irrational. With this discovery the Pythagoreans introduced some of the basic problems of modern mathematical analysis — the concept f the infinite, of limits and continuity. The pursuit of nonalgebraic irrationals has been carried on for centuries; many aspects of the treatment of the infinite remain unresolved.

Another famous unsolved problem is the one usually referred as to Fermat's last theorem. Actually Fermat, who was a mathematical genius of the 17th century although he was professionally a lawyer and public official, had an intriguing way of announcing his results without stating his full proof, particularly in the theory of numbers. Fermat's last theorem is stated on a margin of his copy of the second book of Diophantus' *Arithmetica*, where he wrote, after noting the solution in integers of the familiar equation $x^2 + y^2 = a^2$, "On the contrary it is impossible to separate a cube into two cubes, a fourth power into fourth powers, or, generally, any power above the second into two powers of the same degree. (In other words, the equation $x^n + y^n = a^n$ has no solution in integers if n is greater than 2). I have discovered a truly marvelous demonstration which this margin is too narrow to contain."

This is the famous last theorem which he stated in 1637. Mathematicians have been at work on this problem ever since that time. The attempts have not been successful, but they have led to important advances in mathematical knowledge. It was his work on this theorem that led Kummer in the 19th century to the introduction of ideals, with the consequent reestablishment for algebraic integers of the fundamental theorem of arithmetic, the theorem that assures the unique factorization of integers into primes, without which our concept of integer sits most uncomfortably. The extension of Kummer's work by Dedekind and Kronecker has been central to the development of modern algebra. Nowadays we are apt to read in the newspaper about the solution of a famous unsolved mathematical problem. For example, the New York *Times* of 27 April 1959 carried an editorial called "The mathematical age" that began: "Mathematicians made news twice last week as the solution of two historic problems was announced at a meeting in this city. For most of us, no doubt, the subjects of these two problems, automorphic finite groups and Latin squares, are rather remote. But we are willing to take the word of professional mathematicians that two important new steps have been taken across the mathematical frontiers."

One of the most famous sets of mathematical problems was formulated by David Hilbert, the eminent German mathematician who died in the 1940's. In his lecture at the International Congress of Mathematicians held in Paris in 1900 he described his now famous problems. Before stating his problems, Hilbert had this to say *(6)*: "The great significance of specific problems for the advancement of mathematics in general, and the substantial role that such problems play in the work of the individual mathematician are undeniable. As long as a branch of science has an abundance of problems, it is full of life; the lack of problems indicates atrophy or the cessation of independent development. As with every human enterprise, so mathematical research needs problems. Through the solution of problems, the ability of the researcher is strengthened. He finds new methods and new points of view; he discovers wider and clearer horizons."

Search for Consistency

One of the problems that Hilbert enunciated on this occasion was disposed of in 1931 by Kurt Gödel, now at the Institute for Advanced Study at Princeton. Gödel's paper has been called one of the century's main contributions to science, and something should be said of it. But first, let me put this problem of Hilbert in its setting. The 19th century saw a great surge forward in mathematical research. Gauss, one of the giants of all mathematical history, began to change the whole appearance of mathematics. A fertile intuition, and inspired mathematical inventiveness, combined with a concern for rigor, made Gauss's contributions to mathematics of first importance in all the branches of mathematics studied in his time — in arithmetic or number theory (which he called the Queen of Mathematics), in geometry, in analysis, in algebra. Indeed, Gauss's work is an ornament of the whole of mathematics. In the 19th century mathematics moved on many fronts, but one, in particular, was to introduce problems that have even now not been solved. At the end of the 19th century George Cantor introduced the notion of sets, a powerful new tool which, however, in its 20th-century development has brought with it paradoxes and so-called antinomies that have undermined the confidence of mathematicians in classical logical processes as they affect the infinite. A series of paradoxes produced by the type of reasoning used by Cantor in his theory of infinite sets led to a critical examination of all mathematical reasoning. Whitehead and Russell, at the beginning of the 20th century, tried to show that, by proper methods, we can avoid the set-theoretic contradictions, and that all of mathematics can be derived from logic. In this they failed, but their work has had tremendous influence. At about the same time the intuitionists, of whom the Dutch mathematician Brouwer was a leader, tried to avoid the contradictions introduced by the use of classical logic by insisting that all proofs be constructive, that we avoid the law of the Excluded Middle. This law is the basis for the method of proof, familiar in high school geometry, that begins by assuming that the desired result is not so and shows that this assumption leads to a contradiction. The new methods avoided logical paradoxes, to be sure, but a great portion of the mathematical results that had been found during the preceding centuries could not be proved by the new constructive methods. Hilbert, who had achieved eminence through the astonishing variety of his contributions to many fields of mathematics, including algebra, analytic number theory, analysis, and the foundations of geometry, himself began the search for a rigorous proof of the consistency and completeness of one substantial part of mathematics such as arithmetic. He sought to show that no two theorems deducible from the postulates can be mutually contradictory, and that every theorem of the system is deducible from the postulates. In 1931 Gödel proved that Hilbert's search was hopeless — that it is impossible, within a system broad enough to encompass ordinary arithmetic, ever to prove the consistency of the system in question, and that there is always a proposition of arithmetic which can be formulated within the system that can neither be proved nor disproved by a finite number of logical deductions made in accordance with the procedures of the system.

The hazards in using much of classical mathematics have never been removed. But there are certain results and concepts that mathematicians feel must be kept, either, as R. L. Wilder says (7), "for application to physical problems or, at the other extreme, for the building up of mathematical theory itself . . . We find that in order to study the properties [of these concepts] — which is . . . necessary in order to improve their utility as mathematical tools — we have to augment the older methods of proof with new methods. And at this point the old bugaboo of the mathematician rears its ugly head — the fear that the new methods may introduce contradictions. Here is where the mathematical logician gets to work . . . whenever we find that new concepts and methods engender inconsistency, we shall, if the concepts seem to make for progress, try to patch up our methods before we reject the concepts." The late E. H. Moore is quoted as having said, "Sufficient unto the day is the rigor thereof."

Language of the Sciences

Standards of logic change as mathematical research progresses, and we are bound by the standards of our time. It is in the study of the properties of new concepts, in the deeper understanding and mastery of older concepts, in the development of technical facility in handling those that have been solidified into theories that the enrichment of mathematics as the language of the sciences lies. Such understanding and mastery constitute the distinctive contribution that the mathematician brings to the increasingly many fields of physical and social science and engineering in which mathematics is being used, and this mastery must include the ability to recognize a mathematical concept in a concrete situation and trim it of its attributes so that it may be studied with mathematical techniques. For mathematical concepts and techniques, derived solely because of their interest and quite independently of possible use, have repeatedly proved their usefulness. There is, for example, the application of matrix theory to quantum mechanics, of topological results to nonlinear mechanics; there is the use by Einstein in the general theory of relativity of the concepts developed by Riemann in his treatment of non-Euclidean geometry; and there are other instances too numerous to mention. The fact is that there is no field of mathematics clearly marked as the only one appropriate for applications, and it is true that the most unexpected applications of seemingly abstract and remote fields have been found and are being found repeatedly. Moreover, problems arising in the natural and social sciences continue to enrich the fabric of mathematics. Seemingly all mathematics is the language of science. The critical facility, for conversing in this language, is the ability to think of the problem, which is usually presented in many frills like a lady in her Easter finery, in mathematical form, to "construct the mathematical model," as we say. Once the trimmings have been removed, the machinery of mathematics comes into play. This makes it possible to derive mathematical theorems, results that can be translated back into the original natural situation, so that their predictions can be checked against experience. The final test of the suitability of the model is this checking against the real world.

When the same procedure is used to study purely mathematical problems, the jump to the theorems is often made by intuition, by analogy, by guess, without the process of abstraction and model building. In practical problems it is when such an intuitive guess cannot be made by the engineer or physicist that the mathematician is consulted.

And now, as I conclude, let me state the major positions that seem to me to emerge from considerations such as those I have set forth. They are these:

— That mathematics is a language which must be learned and that the arsenal of techniques of mathematics must be mastered if we are to speak this language.

— That mathematics grows by the addition of new theorems, and that the discovery of new theorems is made sometimes by insights furnished by intuition, sometimes by insights provided by abstraction and the identification of patterns.

— That the proofs of theorems rely on the logic of their day, but that mathematicians are constantly concerned to find the logic that makes the proofs of needed theorems adequate.

— That mathematics is both inductive and deductive, needing, like poetry, persons who are creative and have a sense of the beautiful for its surest progress.

— That many of the problems of mathematics come from mathematics itself, but that many more, at least in their earliest genesis, come from the realities of the world in which we live.

— That realms conquered by mathematics solely because of their intrinsic interest to mathematicians have provided in the past, and continue to provide, parts of the conceptual framework in which other scientists view their worlds.

— That the process of abstraction and axiomatization has provided simplification and a deep understanding of the body of mathematical results and a powerful tool for conquering new mathematical worlds.

References and Notes

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5. A number of examples, taken from the records of this period, of mathematical results originally conjectured on the basis of observation and daring guess, and subsequently proved, are described by George Polya in his two-volume work *Mathematics and Plausible Reasoning.* Princeton, N. J.: Princeton Univ. Press, 1954.
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FOREWORD

Since the turn of the century an embarrassing question persistently recurs: Where is the borderline between "pure mathematics" and "applied mathematics." As suggested in the preceding essay, there are times when the question is academic, as when, in some instances, these two aspects of mathematics can scarcely be disentangled: for example, in the theory of relativity.

Many writers have commented upon the unexpected usefulness of the seemingly useless. Michael Faraday observed, "There is nothing so prolific in utilities as abstractions." Elsewhere, A. N. Whitehead has suggested that "It is no paradox to say that in our most theoretical moods we may be nearest to our most practical applications."

More recently, Professor Billy Goetz of the Massachusetts Institute of Technology, in an article titled the "Usefulness of the Impossible", suggested that "All mathematics is a gigantic tussle with nonexistent impossibilities." Admitting at once that mathematics is "useful", Goetz goes on to say that it is a "great and respected discipline where all is impossible and yet much is useful. The usefulness largely flows from the impossibility."

The present essay provides a brief, illuminating historical sketch of the relation of mathematics to science, concluding with the plea that "applications" will take care of themselves if mathematicians will only allow their creative imaginations complete freedom.

Mathematical Inutility And The Advance Of Science

Should Science Entice the Mathematician From His Ivory Tower Into Solomon's House?

CARL B. BOYER

A few years ago institutions of learning were cutting requirements in mathematics and foreign language, and Phi Beta Kappa, worried about the survival of the liberal arts, took the drastic step of establishing for initiates minimum requirements in language and mathematics. Today the attitude has changed; but if the contemporary return to favor of mathematics results from a panicky concern for defense, the revival may be short-lived. Thus it is that mathematicians find themselves in the equivocal position of endorsing the demands for increased mathematical training at the same time that they look askance at the motives. Training in mathematics is just as appropriate for philosophers and statesmen as for sputnik-builders; but we shall argue here a more modest thesis concerning the role of mathematics in science, raising a voice in protest against two extreme views. One of these was forcefully expressed in 1941 by G. H. Hardy in *A Mathematician's Apology (1)*: "It is not possible to justify the life of any genuine professional mathematician on the ground of the 'utility' of his work. . . . I have never done anything 'useful.' No discovery of mine has made, or is likely to make, directly or indirectly, for good or ill, the least difference to the amenity of the world."

The only usefulness he granted mathematics was as an "incomparable anodyne." Hardy went so far as to distinguish between what he called "real" mathematics and "trivial" mathematics — the former being nonuseful, the latter "useful, repulsively ugly and intolerably dull."

Where Hardy rejoiced that the remoteness of mathematics from ordinary human activities keeps it "gentle and clean," Lancelot Hogben, at the other extreme, in 1937 wrote in *Mathematics for the Million* (*2*, p. 36) that "mathematics has advanced when there has been real work for the mathematician to do, and . . . it has stagnated whenever it has become the plaything of a class which is isolated from the common life of mankind." Both of these extreme views do violence to the history of mathematics and science. History indicates on the one hand that the growth of mathematics and the concomitant advance of science are not chiefly the result of ultilitarian pressures, but it teaches also that activities of mathematicians which once appeared to be inconsequential have in the end been of far-reaching significance in the growth of science. Paradoxically, the mathematician seems to have been most useful to science when the apparent inutility of his activity was especially marked. Today, especially, surrounded as we are by pressures of immediacy and expediency, it is necessary to look beyond the caricature of the mathematician as a glorified calculator and to appreciate the part that pure mathematics has played in the long-range growth of science.

PRE-HELLENIC MATHEMATICS

It was customary, a generation ago, to argue that pre-Hellenic mathematics was entirely practical; but it is obvious now that this picture was overdrawn. Some of the problems in the Ahmes papyrus, for example, are far from utilitarian in nature; and the mathematical inutility in the Egypt of almost four thousand years ago is matched in the Mesopotamian valley of the same period by an instance recently uncovered by Neugebauer. Indefinitely many right triangles with integral sides were known to the Babylonians, for they had the equivalent of a formula for such Pythagorean triads. If p and q are arbitrary integers, with $p>q$, then p^2-q^2, $2pq$, and p^2+q^2 form such a triple of numbers. This result, one of the most remarkable from Old-Babylonian mathematics, is a sophisticated bit of number theory far removed from the hope of immediate utility.

It becomes clearer all the time that mathematical inutility was not unknown in the pre-Hellenic period; but with the Greeks it seems to have become a passion. Greek mathematics started out soberly enough with an eye to the practical. Geometry took its name from the measurement of the earth, and soon it was projected into the heavens; arithmetic promptly found applicability in the Pythagorean discovery that music is "number in motion." But then, probably toward the beginning of the last third of the 5th century B.C., came a discovery which was poles removed from the world of the practical man, and this left a deeper mark on mathematics than has any other single event in its history. Two line segments, it was found, might be such that the ratio of their lengths is not expressible as a ratio of integers. That the diagonal of a square, for example, is incommensurable with its side is of no consequence for the engineer with his slide rule, but in Greece this devasting discovery paved the way for the classical deductive development of mathematics. Ultimately, of course, the deductive method spilled over into the sciences, for it was found to have practical, as well as esthetic, value.

The 5th century B.C. bequeathed also to mathematics the three famous problems of antiquity—the duplication of the cube, the squaring of the circle, and the trisection of the angle—and the better half of later Greek developments centered about these. Inasmuch as craftsmen of the time could solve each of these with a precision that would challenge the keenest senses to find a flaw, the problems made sense only to the impractical geometer, and, as was discovered in modern times, all three of them are, as presented, impossible of solution. Could anything be more futile than to tackle problems which are meaningless to the practitioner and beyond the power of the scholar? History here has amply vindicated the activities of the ivory-tower mathematician, for the search for solutions led to discoveries without which modern science as we know it would have been unthinkable. Conic sections, for example, seem to have been discovered by Menaechmus, tutor of Alexander the Great, in the course of his efforts to duplicate the cube, and although the utility of the ellipse, parabola, and hyperbola escaped Greek scientists, we know that without the speculations of Menaechmus there might have been no laws of Kepler, no law of gravitation, and no Lunik.

Eudoxus and the Great Triumvirate

At the Academy of Plato, as among the Pythagoreans, mathematics was a class-related subject far removed from the common life of mankind, and yet the subject flourished exceedingly. The chief contribution of Eudoxus, the outstanding mathematician associated with Plato, was a theory of proportion which is the equivalent of modern definitions of real number, and it is to be doubted that any practical scientist has had occasion to use the principle of Eudoxus or can tell what a real number is. Eudoxus also had a hand in the method of exhaustion, and this was about as impractical a forerunner of the calculus as could be imagined. Nevertheless, without Plato, the "maker of mathematicians," and the work of Eudoxus, the bulk of what we think of as Greek mathematics would not have developed.

The last century of the Hellenic period might be called the "heroic age," for it was then that the characteristically Greek problems and principals were formulated. During the "golden age" which followed, these were elaborated by the great triumvirate of Euclid, Apollonius, and Archimedes. The earlier sections of Euclid's *Elements*—those included in modern elementary textbooks—have a flavor of practicality, but the deeper one goes, the further the material departs from the ordinary world; one finds a proof of the infinity of primes, a formula for perfect numbers, and the crowning Platonic theorem that there are but five regular solids. In the *Conics* of Apollonius are elaborated the properties of curves, which at the time were beautiful and impractical, for the ellipses which we see in the heavens, the hyperbolas which are formed by our lamp shades, and the parabolas we descry in our suspension bridges were not there for the Greeks. Even the quadratures of Archimedes, which anticipated the now indispensable integral calculus, had at the time little utility; and Archmedes' most sophisticated treatise, *On Spirals*, was largely a mental exercise in circle-squaring and angle-trisecting.

Sharp Decline

Conflicting conjectures have been advanced to account for the sharp decline in mathematics following the great triumvirate, but there is general agreement on one aspect—an admitted transfer of interest from pure to applied mathematics. Under the practicalist theory the shift in interest to the popular fields of astronomy and mensurational geography should have been a catalyst for rapid mathematical development, not the herald of centuries of doldrums. Let this be a warning to those who would equate mathematics and measurement, or who would espouse the fragile thesis of Tobias Dantzig, in *Number, the Language of Science* (1930), quoted (with approval) by Hogben (*2*, p. vii): "It is a remarkable fact that the mathematical inventions which have proved to be the most accessible to the masses are also those which exercised the greatest influence on the development of pure mathematics."

I have mentioned above the mathematical inventions of greatest influence in the pure mathematics of the Greeks, and these inventions were neither accessible nor of interest to the masses. There was in ancient Greece another type of mathematics which had wide appeal. Computation and arithmetic methods, stemming from Babylonian views, were what concerned the vast majority—not axiomatics—and the place of Heron and Diophantus becomes clearer when one regards them as representatives of a tradition which always was present in Greece but which shows through only rarely because of the loss of ancient works. Occasionally both traditions—the higher axiomatic or nonutilitarian stream and the lower arithmetic or utilitarian current—appear in one and the same individual. Ptolemy's *Almagest*, for example, is akin to classical geometry, while his astrological *Tetrabiblos* adopts the Babylonian arithmetical devices, and the verdict of history has been that the theoretical *Almagest* was more influential in the advance of science than the pragmatical *Tetrabiblos*.

No better illustration of the baneful effect of the cold breath of utility upon the ardor of the mathematician can be found than in ancient Rome, where the consequence for science of the Roman contempt for mathematical inutility is too well known to require repetition here. Let us hope that history will not repeat itself in this respect and that a tough-minded concern today for the immediate and obvious needs of national defense—just such as the Romans had in mind—may not stifle the legitimate interests of the pure mathematician. Administrative agencies in this country (and apparently in Russia also) thus far have been very far-seeing in this respect and have generously supported basic research, but if the public clamor for more mathematics in the schools were to result merely in fostering development of expedient techniques, the results could be tragic indeed.

The consequences of a lack of interest in the principles of mathematics, as distinct from a concern with practical outcomes, can be seen in the medieval civilizations—Latin, Greek, Chinese, Hindu, and Arabic. Not one of them had a vigorous tradition of pure mathematics and, interestingly enough, none was strong in science. Much has been made of the so-called Hindu-Arabic system of numeration, but even granted th. · it was an invention of the Hindus (which is not definitively established), it should be noted that the system involved no principles not known in antiquity, and that with it the Hindus and Arabs were able to do but little. Only later, in 16th-century Europe, was a significant mathematical advance made.

The Renaissance and Mathematics

A facile explanation of the opening of the new age sometimes is found in the rise of a merchant class with practical computational needs, or in the explorations which posed geographical problems, or in the establishment of closer relations between the scholar and the artisan, but the revival in mathematics does not fit neatly into any of these. Apart from the recovery of the Greek treatises in pure geometry, the event which marked the opening of a new era was the publication of the algebraic solution of the cubic equation. On the surface this looks like an eminently practical result, but nothing could be more deceptive. The formula which Del Ferro and Tartaglia discovered and which Cardan published in 1545, just two years after the epoch-making treatises of Copernicus and Vesalius, was not then, and is not now, of use to the applied mathematician or the practicing scientist. It gave a strong fillip to the pure mathematician's pursuit of algebra, but it did not satisfy the practitioner's need for a practical device for getting approximations to the roots.

Nevertheless, the radical solution of the cubic did in the end stimulate the advance of science—indirectly, and in a rather curious way which well illustrates the unexpected role that mathematical inutility plays. The new formula called attention to imaginary numbers, for in some mysterious way they were bound up with the real roots in the so-called irreducible case. Cardan said of the arithmetic in this case that it is "as subtle as it is useless," and Bombelli, his contemporary, described it as "a wild thought, in the judgment of many; and I too was for a long time of the same opinion." Today any electrical engineer can attest to the ultimate utility of such useless wild thoughts on imaginary numbers; but these numbers at first were rejected by practical men, and even by some not generally regarded as excessively utilitarian. Of them Simon Stevin wrote, "There are enough legitimate things to work on without need to get busy on incertain matter"; and only occasionally were men bold enough to handle these quantities which Leibniz regarded as a sort of amphibian, halfway between existence and nonexistence.

Contemporary with Stevin was Francois Viete, an inadequately appreciated mathematician who likewise valued mathematical inutility. Trigonometry in its infancy had been so unfortunate as to be immediately applicable to astronomy and navigation, and hence, as a science of indirect measurement, it had had a limited growth. By subordinating the practical art of solving triangles to the liberal study of relationships among the trigonometric functions, Viete did much to convert the subject into a branch of pure mathematics, sometimes known as goniometry, or analytical trigonometry. Today in secondary schools the solution of triangles is giving way to increased emphasis upon the analytic side of trigonometry, and every electrical engineer, every student of optics and acoustics, knows through the work of Viete that the immediately practical is not in the end necessarily the most useful.

Descartes, Fermat, and Boyle

It is in the 17th century that one expects to see the other side of the coin—aspects of mathematics which were suggested by experience and which directly promoted the advance of science. Much of this there was, but less, I suspect, than is commonly assumed. Analytic geometry, for example, was not the practical outgrowth of a mundane use of coordinates. Descartes regarded his geometry as a triumph of philosophical method to be appreciated by the elite, and it took form in his mind as a generalization of an impractical locus problem inherited from ancient Greece. Apollonius had considered the locus of points for which the product of the distances to two of four given lines should be proportional to the product of the distances to the other two lines. Pappus had suggested, but was unable to complete, the generalization of this to six, eight, ten, or more lines, hinting at a geometry of more than three dimensions—the height of inutility, one should suppose. About this problem Descartes developed his coordinate geometry, the aim of which at the time was the theoretical geometric construction of the roots of equations that now would be solved by the practical man through successive arithmetical approximations.

Fermat, an independent inventor of analytic geometry, represents an even more striking instance of mathematical inutility, for he was as unconcerned about the practical outcome of his studies as he was about personal fame. And yet Fermat was an inventor in three branches which turned out to be among the most useful of all: he discovered the fundamental principle of analytic geometry; he invented the differential calculus; and he was a founder of the theory of probability. His coordinate geometry was scarcely more practical than Descartes'. It was a study of geometric loci, the "crowning point" of which was the following proposition: Given any number of fixed lines, the locus of a point from which the sum of the squares of the segments drawn from the point to meet the lines at given angles is constant is a solid locus (conic section).

Can this be used in the workaday world? His new infinitesimal analysis did turn out to have tremendous practical implications, but Fermat's thought here, too, was nonutilitarian. Perhaps the best way to describe his calculus is to say that it represented the first satisfactory definition of the tangent to a curve, a bit of theory which Newton and Leibniz developed into an algorithm which made possible the celestial mechanics upon which our hopes for space travel are founded. Even Fermat's theory of numbers, at the time far removed from the market place, has not been entirely without applicability, for his studies in figurate numbers enter into statistics.

Francis Bacon, in his utopian Solomon's House, had valued mathematics solely for its utility, but Robert Boyle, Fermat's Baconian contemporary, put in a good word for mathematical inutility. Boyle realized with regret that in mathematics one cannot in old age atone for the sins of neglect in one's youth, and it was not lack of training in practical mathematics that he regretted. "I confess," he wrote (3), "that after I began . . . to discern how useful mathematics may be made in physicks, I have often wished that I had employed about the *speculative* part of geometry, and the cultivation of the *specious* Algebra . . . a good part of that time and industry, that I had spent about surveying and fortification . . . and other parts of *practick* mathematics" (italics mine).

The *Principia* of Newton probably never would have been written had it not been for the work of Fermat and others like him, and hence it can be regarded as the fruit of earlier mathematical inutility rather than as an inevitable outgrowth from social and economic roots of the time. In fact, there is not so large a proportion of applied mathematics in the book as is commonly supposed. Moreover, the philosophical import of the law of gravitation far transcended any practical significance. It should be noted also that Newton's contribution in this connection was not so much a discovery—some half a dozen men earlier had suggested an inverse square law—as it was a mathematical proof of the validity of the law, and the practical man has no truck with mathematical demonstration. Newton derived as a corollary of the law of gravitation the fact that within the earth the force varies directly as the distance from the center—a bit of knowledge which at the time served no useful end but which carried within it the germs of potential theory and paved the way for the electromagnetic age.

In Time of Crisis

It is in times of crisis akin to our own that the temptation to undervalue mathematical inutility is great, but mathematicians of stature generally have risen above this. Few more striking instances of this can be found than during the French Revolution. Lazare Carnot and Gaspard Monge were key figures in the frantic defense against foreign invasion, yet during the turmoil they did not yield to the exigencies of the moment and divert their efforts to applied mathematics alone. Both men spent much time reviving pure geometry, one of the more beautiful but less immediately useful branches, and their names still are associated with theorems in the subject. Carnot, the "Organizer of Victory," wrote an especially useless work—one on the metaphysics of the calculus, which has gone through many editions down to our time—and Monge was instrumental in the establishment of the Ecole Polytechnique, an institution which might well be taken as a model of balance between pure and applied mathematics.

Lagrange, one of the teachers at the school, spent much of his time looking for a logical foundation for the calculus—a pursuit which scientists of the time regarded as misdirected effort, but which has since led to the theory of functions, a subject which physicists find indispensable. But the theory of functions owed even more to what at the time looked like a fruitless effort. During the Napoleonic era no less than three men were toying with the idea of picturing imaginary numbers, and the result, now known as the Argand or Wessel or Gaussian diagram, became the basis for the theory of functions of a complex variable, with striking consequences for science. It probably is not too much to say that electrodynamics is the gift of the imaginary number, once shunned as useless.

Nineteenth Century Developments

Most ages have produced men who studied mathematics with little regard for its applicability, but the 19th century was a veritable paradise of mathematical inutility. One of the amazing things about this penchant of the century is that it proceeded in the main from *anciens eleves* of the Ecole Polytechnique, a school of technology. In France, pupils of Monge stirred a revival in pure geometry such as had not been seen since the days of Apollonius. Projective geometry, with its concern for ideal elements, and the analytic geometry of imaginary points fascinated the heirs of the French Revolution, inapplicable though these studies might be. Poncelet, an engineer in the French army under Napoleon, reached the epitome of mathematical inutility when he noted that all circles in a given plane have two points in common—not ordinary points, of course, but two points which are both imaginary and at infinity! The two chief mathematical journals of the time both carried in the title the phrase (one in French, the other in German) "Pure and Applied Mathematics," but so obvious was the preponderance of pure mathematics that wags read the title as "Pure Unapplied Mathematics." And treatises of the time showed the same tendency.

The imaginary appeared everywhere in analysis, geometry, and algebra, and especially in the works of Cauchy. And what was the effect upon science of this feast of uselessness? It probably is safe to say that physics, at least, never developed more rapidly than during and immediately following the period we have been describing. Mechanics, optics, thermotics, acoustics felt the effect of Cauchy's theory of functions of a complex variable. But how, one may be inclined to ask, can the theory of the imaginary number have anything to do with the real world? The answer, of course, is that imaginary numbers are not fictitious, despite their name. What one generation labels impossible, another reduces to common sense. After Gauss, Wessel, and Argand had shown that imaginary numbers can be pictured as points in a plane, it was a short step to Sir William Rowan Hamilton's identification of the theory of complex numbers with the properties of couples of real numbers. This led Hamilton to devise a four-dimensional analog—the system of quaternions—and this in turn was later generalized into the theory of tensors, without which the mathematical theory of relativity would be unthinkable.

Relatively is in a real sense a bequest to science of once-useless mathematics. Not only is it an outcome of the imaginary number; it resulted also from some impossible geometries. Gauss, greatest mathematician of all times, played indifferently with useful and useless mathematics. His contributions in probability and statistics found ready application; much of his theory of numbers, which he enjoyed most, still is without palpable use. Among the mathematical toys of Gauss was one called non-Euclidean geometry, of which similar schemes were developed independently by Bolyai and Lobachevski.

The new geometries seemed to be a denial of common sense, but disagreement with sense never has been, and we hope never will be, a bar to mathematical investigation. If it had been, the 19th century would not have pursued the study of geometries of more than three dimensions. As it turned out, both non-Euclidean geometry and multidimensional geometry are applicable to science in the theory of relativity. Bertrand Russell has said that Riemann is logically the immediate predecessor of Einstein, and one might add that Cayley's geometry of *n*-dimensions, developed in 1843 with no inkling of possible applicability, has since found a place in thermodynamics, applied chemistry, and statistical mechanics. Here in America the analysis of Gibbs once was termed a "hermaphrodite monster," but the monster soon was tamed and became the chemist's best friend. Perhaps even today's bizarre mathematics of transfinite numbers eventually may become a scientist's man Friday. Had Hamilton been dissuaded on utilitarian grounds from toying with economically worthless noncommutative algebras, much of the abstract algebra of the 20th century would never have developed, and quantum mechanics would have been the loser.

The history of science seems indeed to support the findings of psychology in the thesis of a great nonagenarian mathematician of our day, Jacques Hadamard, who, on the basis of a study of *The Psychology of Invention in the Mathematical Field* (4), concluded that "practical application is found by not looking for it, and one can say that the whole of civilization rests on that principle."

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FOREWORD

Perhaps the greatest contribution of the ancient Greeks to mathematics was their naive faith in deductive logical reasoning as a basis for creating the geometry of their day. Since the time of Euclid and Aristotle, the science of logic has been the concern alike of scientists, mathematicians, philosophers and theologians. As with other disciplines, logic underwent an evolution until today modern logic, or more precisely, various systems of logic, bear but little resemblance to the relatively simple tenets of Aristotle and Plato.

The inception of modern logic was foreshadowed by Leibniz (1666) when searching for a universal system using symbolic rules which would make thinking unnecessary. The latter half of the nineteenth century saw the development of logic extended quietly by George Boole's *Laws of Thought* (1854); A. De Morgan's formal logic (1874); John Venn's *Symbolic Logic* (1881); C. S. Peirce's introduction of truth tables (c.1880); Frege's symbolic logic applied to the foundations of arithmetic (1880-1900); Hilbert's logical foundations of geometry (c. 1900). In 1910-1913 Whitehead and Russell published their monumental work, *Principia Mathematica*, which is probably the most comprehensive formulation of symbolic logic ever undertaken.

There quickly arose three major schools of opinion in the field of mathematical philosophy: (1) *Logicalism*, identified with Whitehead and Russell; (2) *Formalism*, as typified by Hilbert; and (3) *Intuitionism*, as championed by Brouwer.

Since then there has been considerable controversy regarding these points of view, and, indeed concerning the larger matter as to the very nature of mathematics and its relation to logic. In fact, the critical study of the properties possessed by formal abstract postulational systems has become known as *metamathematics*. Interestingly enough, the controversies between the mathematicians and the logicians have not yet been satisfactorily resolved.

Are Logic And Mathematics Identical?

An Old Thesis of Russell's Is Reexamined in the Light Of Subsequent Developments in Mathematical Logic.

LEON HENKIN

It was 24 years ago that I entered Columbia College as a freshman and discovered the subject of logic. I can recall well the particular circumstance which led to this discovery.

One day I was browsing in the library and came across a little volume by Bertrand Russell entitled *Mysticism and Logic.* At that time, barely 16, I fancied myself something of a mystic. Like many young people of that age I was filled with new emotions strongly felt. It was natural that any reflective attention should be largely occupied with these, and that this preoccupation should give a color and poignancy to experience which found sympathetic reflection in the writings of men of mystical bent.

Having heard that Russell was a logician I inferred from the title of his work that his purpose was to contrast mysticism with logic in order to exalt the latter at the expense of the former, and I determined to read the essay in order to refute it. But I discovered something quite different from what I had imagined. Indeed, contrasting aspects of mysticism and logic were delineated by Russell, but his thesis was that each had a proper and important place in the totality of human experience, and his interest was to define these and to exhibit their interdependence rather than to select one as superior to the other. I was disarmed, I was delighted with Russell's lucent and persuasive style, I began avidly to read his other works, and was soon caught up with logical concepts which have continued to occupy at least a portion of my attention ever since.

Bertrand Russell was a great popularizer of ideas, abstract as well as concrete. Probably many of you have been afforded an introduction to mathematical logic through his writings, and perhaps some have even been led to the point of peeping into the formidable *Principia Mathematica* which he wrote with Alfred Whitehead about 1910. You will recall, then, the astonishing contention with which he shocked the mathematical world of that time — namely, that all of mathematics was nothing but logic. Mathematicians were generally puzzled by this radical thesis. Really, very few understood at all what Russell had in mind. Nevertheless, they vehemently opposed the idea.

This is readily understandable when you recall that a companion thesis of Russell's was that logic is purely tautological and has really no content whatever. Mathematicians, being adept at putting 2 and 2 together, quickly inferred that Russell meant to say that all mathematical propositions are completely devoid of content, and from this it was a simple matter to pass to the supposition that he held all mathematics to be entirely without value. *Aux armes, citoyens du monde mathematique!*

Half a century has elapsed since this gross misinterpretation of Russell's provocative enunciation. These 50 years have seen a great acceleration and broadening of logical research. And so it seems to me appropriate to seek a reassessment of Russell's thesis in the light of subsequent development.

Definitions and Proofs

In order to explain how Russell came to hold the view that all of mathematics is nothing but logic, it is necessary to go back and discuss two important complexes of ideas which had been developed in the decades before Russell came into the field. The first of these was a systematic reduction of all the concepts of mathematics to a small number of them. This process of reduction had indeed been going on for a very long time. As far back as the days of Descartes, for example, we can see at least an imperfect reduction of geometric notions to algebraic ones. Subsequently, with the development of set theory initiated by Georg Cantor, the reduction of the system of real numbers to that of natural numbers marked another great step in this process. But perhaps the most daring of these efforts, the culminating one, was the attempt by a German mathematician, Gotlob Frege, to analyze the notion of natural number still further and reduce it to a concept which he considered to be of a purely logical nature.

Frege's work was almost entirely unnoticed in his own time (the last three decades of the 19th century), but when Bertrand Russell came upon Frege's work he realized its great significance and gave these ideas very wide currency through his own brilliant style of exposition. The ultimate elements into which the notion of natural number was analyzed by Frege and by Russell were entities which they called "propositional functions." To this day there persists a controversy among philosophers as to just what these objects are, but at any rate they are connected with certain linguistic expressions which are like sentences except for containing variables. Just as there is a certain *proposition* associated with (or expressed by) the sentence "U Thant is an astronaut," for example, so there is a *propositional function* associated with the expression "x is an astronaut." Since propositions had long been recognized as constituting one of the most basic portions of the domain of investigation of logicians, and since propositional functions are very closely related to propositions, it was natural to consider these, too, to be a proper part of the subject of logic. It is in this sense that Frege seemed able, by a series of definitions, to arrive at the notion of number, as well as at the other notions under study in various parts of mathematics, starting from purely logical notions.

The second important line of development which preceded Russell, and upon which he drew for his ideas, was the systematic study by mathematical means of the laws of logic which entered into mathematical proofs. This development was initiated by George Boole, working in England in the middle of the 19th century. He discovered that certain of the well-known laws of logic could be formulated with the aid of algebraic symbols such as the plus sign, the multiplication sign, and the equality sign and of variables. For example, Boole used the familiar equation $P.Q. = Q.P.$ to express the fact that sentences of the form "P and Q" *and* "Q and P" must be both true or both false (whatever the sentences P and Q may be), while the generally unfamiliar algebraic equation $-(P.Q.) = (-P) + (-Q)$ indicates that the sentence "Not both P and Q" has the same truth value as "Either not P or not Q." Boole demonstrated that through the use of such algebraic notation one can effect a great saving in the effort needed to collate and apply basic laws of logic. Later his work was extended and deepened by the American C. S. Peirce and the German mathematician E. Schröder. And Russell himself, working within this tradition, found it a convenient basis for a systematic development of all mathematics from logic. By combining the symbolic formulation of logical laws with the reduction of mathematical concepts to a logical core, he was able to conceive of a unified development such as was attempted in the *Principia Mathematica.*

From Russell to Gödel

What was the *Principia* like? Well of course the work is still not completed (only three of four projected volumes having appeared); and since Bertrand Russell has most recently seemed to occupy himself with the political effects of certain physical research it may, perhaps, never be completed! Nevertheless, one can see clearly the intended scope of the work. Surprisingly, it reminds one of the present massive undertaking by the Bourbaki group in France. For even though the *Principia* and Bourbaki are very dissimilar in many ways, each attempts to present an encyclopedic account of contemporary mathematical research unified by a coherent point of view.

In the *Principia,* starting from certain axioms expressed in symbolic form which were intended to express basic laws of logic (axioms involving only what Russell conceived to be logical notions), the work systematically proceeds to derive the other laws of logic, to introduce by definition such mathematical notions as the concept of number and of geometric space, and finally to develop the main theorems concerning these concepts as part of a uniform and systemic development.

Viewed in retrospect, the contemporary logician is struck by the willingness of Russell and Whitehead to rest their case on what, for a mathematician, must be considered such flimsy evidence. The world of empirical science, of course, expects to achieve conviction on the basis of empirical evidence, but the quintessence of the mathematician's approach, especially of the mathematical logician's, is the demand always for *proof* before a thesis is accepted. Yet you see that whereas Russell was interested in establishing that in a certain sense all of mathematics could be obtained from his logical axioms and concepts, he never really set out to give a proof of this fact! All he did was to gather the basic ideas that had been developed in a nonformal and unsystematic way by mathematicians before him, and to say, in effect, "You see that I have been able to introduce all this loosely formulated work within the precise framework of my formal system. And it's pretty clear, isn't it, that I have all the tools available to formalize such further work as mathematicians are likely to do?"

In this respect one is reminded of the approach of that first great axiomatizer and geometer, Euclid. Euclid, too, conceived that all propositions of geometry — that is, all the true statements about triangles, circles, and those other figures in which he was interested — could be developed from the simple list of concepts and axioms he gave. But in his case, too, there was never any attempt to prove this fact other than by the empirical process of deriving a large number of geometric propositions from the axioms and then appealing to the good will of the audience, so to speak. "Well," we may imagine him saying, "look how much I have been able to deduce from my axioms. Aren't you pretty well convinced that *all* geometric facts follow from them?"

But of course there were mathematicians and logicians who were *not* convinced. And so the demand for proof was raised.

Actually, the proper formulation of the problem of whether a system of axioms is adequate to establish *all* of the true statements in some domain of investigation requires a mathematically precise formulation of the notion of "true sentence," and it was not until 1935 that Alfred Tarski, in a great pioneering work, made fully evident the form in which semantical notions must be analyzed for mathematical languages. Of course, it is a trivial matter to give the conditions under which any *particular* sentence is true. For example, in the theory of Euclidean geometry the sentence "All triangles have two equal angles" is true if, and only if, all triangles have two equal angles. However, Tarski made it clear that there is no way to utilize this simple technique in order to describe (in a finite number of words) conditions for the truth of all the infinitely many sentences of a language; for this purpose a very different form of definition, structural and recursive in character, is needed.

Even before Tarski's treatment of semantics, indeed as early as 1919, we find the first proof of what we call, in logic, "completeness." The mathematician Emil Post (in his doctoral dissertation published in that year), limiting his attention to a very small fragment of the system created by Whitehead and Russell, was able to show that for any sentence in that fragment which was "true under the intended interpretation of the symbols," one could indeed get a proof by means of the axioms and rules of inference which had been stated for the system. Subsequently, further efforts were made to extend the type of completeness proof which Post initiated, and it was hoped that ultimately the entire system of the *Principia* could be brought within the scope of proofs of this kind.

In 1930, Kurt Gödel contributed greatly to this development and to this hope when he succeeded in proving the completeness of a deductive system based upon a much larger portion of mathematical language than had been treated by Post. Gödel's proof deals with the so-called "first-order predicate logic," which treats of mathematical sentences containing variables of only one type. When such a sentence in intepreted as referring to some mathematical model, its variables are interpreted as ranging over the elements of the model; in particular, there are no variables ranging over sets of model elements, or over the integers (unless these happen to be the elements of the particular model). Now Gödel shows that if we have any system of axioms of this special kind, then whenever a sentence is true in every model satisfying these axioms there must be a proof of finite length, leading from the axioms to this sentence, each line of the proof following from preceding lines by one of several explicitly listed rules of logic. This result of Gödel's is among the most basic and useful theorems we have in the whole subject of mathematical logic.

But the very next year, in 1931, the hope of further extension of this kind of completeness proof was definitely dashed by Gödel himself in what is certainly the deepest and most famous of all works in mathematical logic. Gödel was able to demonstrate that the system of *Principia Mathematica*, taken as a whole, was *incomplete*. That is, he showed explicitly how to construct a certain sentence, about natural numbers, which mathematicians could recognize as being true under the intended intepretation of the symbolism but which could not be proved from the axioms by the rules of inference which were part of that system.

Now, of course, if Gödel had done nothing more than this, one might simply conclude that Russell and Whitehead had been somewhat careless in formulating their axioms, that they had left out this true but unprovable sentence from among the axioms, and one might hope that by adding it as a new axiom a stronger system which was complete would be achieved. But Gödel's proof shows that this stronger system, too, would contain a sentence which is true but not provable; that, indeed, if this system were further strengthened, by the addition of this new true but unprovable sentence as an axiom, the resulting system would again be incomplete. And indeed, if a whole infinite sequence of sentences were to be obtained by successive applications of Gödel's method, and added simultaneously to the original axioms of *Principia*, the same process could still be applied to find *another* true sentence still unprovable.

Actually, Gödel described a very wide class of formal deductive systems to which his methods applies. And most students of the subject have been convinced that any formal system of axioms and rules of inference which it would be reasonable to consider as a basis for a development of mathematics would fall in this class, and hence would suffer a form of incompleteness. From this viewpoint it appears that one of the basic elements on which Russell rested his thesis that all mathematics could be reduced to logic must be withdrawn and reconsidered.

Consistency and Decision Problems

I have been talking about completeness, which has to do with the adequacy of a formal system of axioms and rules of inference for proving true sentences. But I must mention, also, a second aim of the Russell-Whitehead *Principia* which also fared ill in the subsequent development of mathematical logic. Russell and Whitehead were very much concerned with the question of *consistency*. While they hoped to have a complete system, one containing proofs for all correct statements, they were also concerned that their system should *not* contain proofs of incorrect results. In particular, in a consistent system such as they sought, it would not be possible to prove both a sentence and its negation.

To understand their concern with the question of consistency it is necessary to recall the rude awakening which mathematicians sustained in 1897 in connection with Cantor's theory of transfinite numbers. For centuries before the time of Cantor mathematicians simply assumed that anyone who was properly educated in their subject could distinguish a correct proof from an incorrect one. Those who had trouble in making this distinction were simply "weeded out" in the course of their training and were turned from mathematics to lesser fields of study. And no one took up seriously the question of setting forth, in explicit and mathematical terms, exactly what was meant by a correct proof.

Now when Cantor began his development of set theory he concerned himself with both cardinal and ordinal numbers of transfinite type. (These numbers can be used for infinite sets in very much the same way that we use ordinary numbers for counting and ordering finite sets.) Many of the properties of transfinite numbers are identical to those of ordinary numbers, and in particular Cantor showed that, given any ordinal number b, we can obtain a larger number, $b + 1$. However, in 1897 an Italian mathematician, C. Burali-Forti, demonstrated that there must be a *largest* ordinal number, by considering the set of all ordinal numbers in their natural order. Mathematicians were unable to find any point, either in the argument of Cantor or in that of Burali-Forti, which they intuitively felt rested on incorrect reasoning. Gradually it was realized that mathematicians had a genuine paradox on their hands, and that they would have to grapple at last with the question of just what was meant by a correct proof. Later, Russell himself produced an even simpler paradox in the intuitive theory of sets, based up on the set of all those sets which are not elements of themselves.

This background sketch will make clear why it was that Russell and Whitehead were concerned that no paradox should be demonstrable in their own system. And yet they themselves never attempted a *proof* that their system was consistent! The only evidence they adduced was that a large number of theorems had been obtained within their system without encountering paradox, and that all attempts to reproduce within the system of *Principia Mathematica* the Burali-Forti paradox, and such other paradoxes as were shown, had failed.

As with the question of completeness, mathematicians were not satisfied with an answer in this form, and there arose a demand that an actual proof of the consistency be given for the system of *Principia* (and for other systems then considered). The great and illustrious name of David Hilbert was associated with these efforts to achieve consistency proofs for various portions of mathematics, and under his stimulus and direction important advances were made toward this goal, both by himself and by his students. But as with the efforts to prove completeness, Hilbert's program came to founder upon the brilliant ideas of Kurt Gödel.

Indeed, in that same 1931 paper to which I have previously referred, Gödel was able to show that the questions of consistency and completeness were very closely linked to one another. He was able to show that *if* a system such as the *Principia* were truly consistent, then in fact it would not be possible to produce a sound proof of this fact! Now this result itself sounds paradoxical. Nevertheless, when expressed with the technical apparatus which Gödel developed, it is in fact a precisely established and clearly meaningful mathematical result which has persuaded most, though admittedly not all, logicians that Hilbert's search for a consistency proof must remain unfulfilled.

I should like finally to mention a third respect in which the original aim of mathematical logicians was frustrated. The questions of consistency and completeness clearly concerned the authors of *Principia Mathematica,* but the question of decision procedures seems not to have been treated to any serious extent by Russell and Whitehead. Nevertheless, this is an area of study which interested logicians as far back as the time of Leibniz. Indeed, Leibniz himself had a great dream: He dreamt that it might be possible to devise a systematic procedure for answering questions—not only mathematical questions but even questions of empirical science. Such a procedure was to obviate the need for inspiration and replace this with the automatic carrying out of routine procedure. Had Leibniz been conversant with today's high-speed computing machines he might have formulated his idea by asserting the possibility that one could write a program of such breadth and inclusiveness that any scientific question whatever could be placed on tape and, after the machine had been set to work on it for some finite length of time, a definitive reply would be forthcoming.

Logic After 1936

Leibniz's idea lay dormant for a long time, but it was natural to revive it in connection with the formal deductive systems which were developed by mathematical logicians in the early part of this century. Since these logicians had been interested in formulating mathematical ideas within a symbolic calculus and then manipulating the symbols according to predetermined rules in order to obtain further information about these mathematical concepts, it seemed natural to raise the question of whether one could not devise purely automatic rules of computation which would enable one to reach a decision as to the truth or falsity of any given sentence of the calculus. And while the area of empirical science was pretty well excluded from the consideration of 20th-century logicians seeking such decision procedures, it was perhaps not beyond the hope of some that a system as inclusive as that of the *Principia* could some day be brought within the scope of such a procedure.

Efforts to find decision procedures for various fragments of the *Principia* were vigorous and many. The doctoral dissertation of Post, for example, contained some efforts in this direction, and further work was produced during the succeeding 15 years by logicians of many countries. Then in 1936 Alonzo Church, making use of the newly developed notion of recursive function, was able to demonstrate that for a certain fragment of mathematical language, in fact for that very first-order predicate logic which Gödel, in 1930, had showed to be complete, no decision procedure was possible. And so with decision procedures, as with proofs of completeness and consistency, efforts to establish a close rapport between logic and mathematics came to an unhappy end.

Well, I have brought you down to the year 1936. Probably most mathematicians have heard at least something of the development which I have sketched here. But somehow the education in logic of most mathematicians seems to have been terminated at about that point. The impression is fairly widespread that, with the discoveries of Gödel and Church, the ambitious program of mathematical logicians in effect ground to a halt, and that since then further work in logic has been a sort of helpless faltering by people, unwilling to accept the cruel facts of life, who are still seeking somehow to buttress the advancing frontiers of mathematical research by finding a nonexistent consistency proof.

And yet this image is very far indeed from reality. For in 1936, just at the time when, many suppose, the demise of mathematical logic had been completed, an international scholarly society known as the Association for Symbolic Logic was founded and began publication of the *Journal of Symbolic Logic*. In the ensuing 25 years this has greatly expanded to accommodate a growing volume of research. And at present there are four journals devoted exclusively to publishing material dealing with mathematical logic, while many articles on logic appear in a variety of mathematical journals of a less specialized nature.

In the space remaining I should like to mention very briefly some of the developments in mathematical logic since 1936.

Sets and Decision Methods

I have found it convenient for this exposition to divide research in mathematical logic into seven principal areas. And first I shall mention the area dealing with the foundations of the theory of sets.

To explain the connection of this field with logic it should be mentioned that those objects which Russell and Whitehead had called "propositional functions" are, in fact, largely indistinguishable from what are now called "sets" and "relations" by mathematicians. From a philosophical point of view there is perhaps still room for distinguishing these concepts from one another. But since, in fact, the treatment of propositional functions in *Principia Mathematica* is extensional (so that two functions which are true of exactly the same objects are never distinguished), for mathematical purposes this system is identical to one which treats of sets and relations.

Among systems of set theory which have been put forth by logicians as a basis for the development of mathematics, the principal ones are the theory of types used by Whitehead and Russell themselves, subsequently amplified by L. Chwistek and F. Ramsey, and an alternative line of development initiated by E. Zermelo, to which important contributions were subsequently made by A. Fraenkel and T. Skolem. Still another system, having certain characteristics in common with each of these two principal forms, was advanced and has been studied by W. Quine and, to some extent, by J. B. Rosser. Of these systems the Zermelo-type system has probably received most attention, along with an important variant form suggested and developed by J. von Neumann, P. Bernays, and Gödel.

Among the significant efforts expended on these systems were those directed toward establishing the status of propositions such as the axiom of choice and the continuum hypothesis of Cantor. Here the names of Gödel and A. Mostowski are especially prominent.

Gödel showed that a strong form of the axiom of choice and the generalized continuum hypothesis are simultaneously consistent with the more elementary axioms of set theory—under the assumption that the latter are consistent by themselves. Mostowski showed that the axiom of choice is independent of the more elementary axioms of set theory, provided that a form of these elementary axioms is selected which does not exclude the existence of nondenumerably many *"Urelemente"* (objects which are not sets). The independence of the axiom of choice from systems of axioms such as that used in Gödel's consistency proof, and the independence of the continuum hypothesis in any known system of set theory, remain open questions.

More recently the direction of research in the area of foundations of set theory seems to have shifted from that of formulating specific axiom systems and deriving theorems within them to consideration of the totality of different realizations of such axiom systems. It is perhaps J. Shepherdson who should be given credit for the decisive step in this shift of emphasis, although his work clearly owes much to Gödel's. Subsequent work by Tarski, R. Vaught, and R. Montague has carried this development much further.

An important tool in their work is the concept of the *rank* of a set, which may be defined inductively as the least ordinal number exceeding the rank of all elements of the set. This notion may be used to classify models of set theory according to the least ordinal number which is not the rank of some set of the model. Recently there have been some very interesting contributions by Azriel Levy to these studies. His efforts have been directed toward successively strengthening the axioms of set theory so as to penetrate increasingly far into the realm of the transfinite.

A second area that I would delineate in contemporary logical research is that dealing with the decision problem. While it is true that the work of Church made it clear that there could be no *universal* decision procedure for mathematics, there has remained a strong interest in finding decision procedures for more modest portions of mathematical theory. Of special interest here is Tarski's decision method for elementary algebra and geometry, and an important extension of it which was made by Abraham Robinson. Wanda Szmielew has also given an important decision procedure—namely, one for the so-called "elementary theory" of Abelian groups. By contrast, the elementary theory of *all* groups was shown by Tarski to admit of no decision procedure. In fact, Szmielew and Tarski considered exactly the same set of sentences—roughly, all of those sentences which can be built up by the use of the group operation symbol, and variables ranging over the group elements, with the aid of the equality sign, as well as the usual logical connectives and quantifiers. If we ask whether any given sentence of this kind is true for all *Abelian* groups, it is possible to answer the question in an automatic way by using the method of Szmielew. But if we are interested in which of these sentences are true for *all* groups, then Tarski's proof shows that it is impossible to devise a machine method to separate the true from the false ones.

A result closely related to Tarski's is that of P. Novikov and W. Boone concerning the nonexistence of a decision method which would enable one to solve the word problem for the theory of groups, a problem for which a solution had long been sought by algebraists. In fact it is a simple matter to show that the Novikov-Boone result is equivalent to the nonexistence of a decision method for a certain *subset* of the sentences making up the elementary theory of groups —namely, all those sentences having a special, very simple, form. Hence, this result is stronger than Tarski's.

Recursive Functions

Now the key concept whose development was needed before negative solutions to decision problems could be achieved was the concept of a recursive function. Intuitively speaking this is simply a function from natural numbers to natural numbers which has the property that there is an automatic method for computing its value for any given argument. A satisfactory and explicit mathematical definition of this class of functions was first formulated by J. Herbrand and Gödel. But it remained for S. C. Kleene to develop the concept to such an extent that it now underlies a very large and important part of logical research.

Much of the work with recursive functions has been along the line of classifying sets and functions, a classification similar to that involving projective an analytic sets in descriptive set theory. Kleene himself, his students Addison and Spector, and other logicians, including Post, Mostowski, J. Shoenfield, and G. Kreisel, have contributed largely to this development. Also to be mentioned are the applications which initially Kleene, and subsequently others, have attempted to make of the concept of recursive function by way of explicating the notion of "constructive" mathematical processes. In this connection several attempts have been made to link the notion of recursive function with the mathematical viewpoint known as intuitionism, a radical reinterpretation of mathematical language which was advanced by L. Brouwer and developed by A. Heyting.

Algebra, Logic, and Models

A fourth area of logical research deals with material which has recently been described as algebraic logic. This is actually a development which can be traced back to the very early work of Boole and Schröder. However, interest in the subject has shifted away from the formulation and derivation of algebraic equations which express laws of logic to the consideration of abstract structures which are defined by means of such equations. Thus, the theory of Boolean algebras, of relation algebras, of cylindric and polyadric algebras have all successively received attention; M. Stone, Tarski, and P. Halmos are closely associated with the central development here. The algebraic structures studied in this domain may be associated in a natural way with mathematical theories, and this association permits the use of very strong algebraic methods in the metamathematical analysis of these theories.

A fifth area of modern logical research concerns the so-called theory of models. Here effort is directed toward correlating mathematical properties possessed by a class of structures defined by means of given mathematical sentences with the structural properties of those sentences themselves.

A very early example is Garrett Birkhoff's result that, for a class of structures to be definable by means of a set of equational identities, it is necessary and sufficient that it be closed under formation of substructures, direct products, and homomorphic images. Characterizations of a similar nature were given for classes definable by universal elementary sentences (Tarski) and by any elementary sentences (J. Keisler).

A related type of result is R. Lyndon's theorem that any elementary sentence whose truth is preserved under passage from a model of the sentence to a homomorphic image of that model must be equivalent to a sentence which does not contain negation signs. In a different direction, E. Beth has shown that if a given set symbol or relation symbol is not definable in terms of the other symbols of an elementary axiom system, then there must exist two distinct models of these axioms which are alike in all respects except for the interpretation of the given symbol. (This proves the completeness of A. Padoa's method of demonstrating nondefinability.) A logical interpolation theorem of W. Craig's provides a close link for the results of Lyndon and Beth.

A sixth area which can be discerned in recent work on logic concerns the theory of proof. This is perhaps the oldest and most basic portion of logic, a search for systematic rules of proof, or deduction, by means of which the consequences of any propositions could be identified. In recent work, however, logicians have begun to depart in radical ways from the type of systems for which rules of proof were originally sought. For example, several attempts have been made to provide rules of proof for languages containing infinitely long formulas, such as sentences with infinitely many disjunctions, conjunctions, and quantified variables. Tarski, Scott, C. Karp, W. Hanf, and others have participated in such efforts. Curiously enough, while this direction of research seems at first very far removed from ordinary mathematics, one of the important results was used by Tarski to solve a problem, concerning the existence of measures on certain very large spaces, which had remained unsolved for many years.

The last area of logical research I should like to bring to your attention is a kind of converse study to what we have called algebraic logic. In the latter we are interested in applying methods of algebra to a system of logic. But there are also studies in which results and methods of logic are used to establish theorems of modern algebra. The first to have made such applications seems to have been the Russian mathematician A. Malcev, who in 1941 indicated how the completeness theorem for first-order logic could be used to obtain a result on groups. Subsequently the same technique was used by Tarski to construct various non-Archimedean ordered fields. Perhaps the best-known name in this area is that of Abraham Robinson, who formerly was associated with the University of Toronto in Canada. Among his contributions was the application of logical methods and results to improve a solution, given in 1926 by E. Artin, to Hilbert's 17th problem (17th of the famous list of problems presented in his address to the International Congress of Mathematicians in 1900). Robinson showed that when a real polynomial which takes only nonnegative values is represented as a sum of squares of rational functions, the number of terms needed for the representation depends only on the degree and number of variables of the given polynomial, and that it is independent of the particular coefficients .

Russell's Thesis in Perspective

I hope that this very brief sketch of some of the areas of contemporary logical research will give some idea of the ways in which logicians have reacted to the theorems of Gödel and Church which, in the period 1931 to 1936, dealt so harshly with earlier hopes. Speaking generally, one could describe this reaction as compounded of an acceptance of the impossibility of realizing the original hopes for mathematical logic, a relativization of the original program of seeking completeness and consistency proofs and decision methods, an incorporation of the new methods and constructs which appeared in the impossibility proofs, and the development of quite new interests suggested by generalization of early results.

Now with this background, let us return to Russell's thesis that all of mathematics can be reduced to logic. I would say that if logic is understood clearly to contain the theory of sets (and this seems to be a fair account of what Russell had in mind), then most mathematicians would accept without question the thesis that the basic concepts of all mathematics can be expressed in terms of logic. They would agree, too, that the theorems of all branches of mathematics can be derived from principles of set theory, although they would recognize that no fixed system of axioms for set theory is adequate to comprehend all of those principles which would be regarded as "mathematically correct."

But perhaps of greater significance is the consensus of mathematicians that there is much more to their field than is indicated by such a reduction of mathematics to logic and set theory. The fact that *certain* concepts are selected for investigation, from among all logically possible notions definable in set theory, is of the essence. A true understanding of mathematics must involve an explanation of which set-theory notions have "mathematical content," and this question is manifestly not reducible to a problem of logic, however broadly conceived.

Logic, rather than being all of mathematics, seems to be but one branch. But it is a vigorous and growing branch, and there is reason to hope that it may in time provide an element of unity to oppose the fragmentation which seems to beset contemporary mathematics—and indeed every branch of scholarship.

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