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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 mathématique!*

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

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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 is interpreted 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 interpretation 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 method 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 wakening 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 upon 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 Lévy 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 and 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.

Bibliography

Some of the early work of Boole, Frege, and Cantor has been made more available by relatively recent translations into English. Examples are the works cited. The *Principia Mathematica* of Russell and Whitehead, while largely devoted to a formidable formalism, contains many very readable sections of an introductory or summary character. An account of the fundamental theorems of Gödel and Church may be found in Kleene. In describing logical research since 1936 my aim has been not to give a complete history but only to indicate the range of activity by mentioning some of the most actively cultivated areas. Accordingly, I have referred to only a very small sample of the literature, selecting (where possible) works which are largely self-contained. With reference to the following list, for an example of an axiomatic theory of sets and an important contribution toward its metatheory, see Gödel. For an account of the decision problem, see Tarski (1951) for positive solutions and Tarski *et al.* (1953) for negative. The theory of recursive functions and their uses is well described in Kleene. Some historical remarks on algebraic logic, as well as detailed results, may be found in Henkin and Tarski. A very recent and important contribution to the theory of models is that of Keisler, where reference to earlier works may be found. For an account of recent work on infinitely long formulas, see Henkin; an application of such work to a problem on the existence of certain measures appears in Tarski (in press). Applications of logic to algebra are described in Robinson.

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Arthur Holly Compton, Research Physicist

I first met Arthur Compton in 1924, in William Duane's x-ray research laboratory at Harvard University. Compton had come on a visit to attempt to discover why Duane and his associates could not confirm his discovery of the change of wavelength of x-rays on scattering, now known as the Compton effect. I do not know what Compton had been doing just before he arrived, but his appearance late that afternoon was completely nontypical. He was disheveled, unshaven, and obviously overtired. He returned to the laboratory the following morning looking like himself—a well-groomed, energetic, and clear-thinking physicist.

The situation was rather tense, with peculiar overtones. Compton was not the first to perform experiments which indicated that scattered x-rays and gamma rays were more absorbable—that is, of longer wavelength—than their primaries. As far back as 1912 Sadler and Mesham had observed such an effect in x-rays scattered from carbon, and Compton himself, in 1921, had followed others in experiments showing the softening of gamma rays on scattering. But, as has several times happened in physics, the experiment, the interpretation, and the audience were not simultaneously ready, and these prior results attracted little attention. Compton, however, had never completely laid aside those gamma-ray experiments he performed in Rutherford's laboratory, and he turned them over and over in his mind, finally reaching an interpretation based on the transfer of momentum from light quanta to free electrons. Again the "interpretation" was not new; the idea of photons or light quanta had long been in the minds of many physicists. Some had even worked out Compton's equations for the conservation of energy and momentum in the photon-electron collision, ending with the wry remark that this would be a beautifully simple theory of scattering but was of course untenable because everyone knew that scattered light and x-rays were unchanged in wavelength and coherent with the primary radiation. Compton solved the equations independently, however, and was the first to publish the results.

It took Compton to correlate theory and experiment and finally to clinch the matter with a demonstration in which the change in wavelength was precisely measured with a crystal spectrometer and shown to be h/mc or 0.024 angstrom units at 90 degrees, as the calculation had predicted. The audience was ready, because the apparent conflict between the corpuscular and the wave theories of light was in every physicist's mind. A Nobel-prize discovery had been made.

But here at Harvard in 1924, in the laboratory of a highly respected investigator of x-rays, the crystal spectrometer measurements seemed to give different results. The scattered radiation showed, as Compton had found, part of the radiation to be shifted to longer wavelengths, but Duane interpreted this as "tertiary radiation," of the bremsstrahlung type, caused by the deceleration of photoelectrons ejected from the scatterer by the primary radiation. Actually, the shift at 90 degrees, from carbon, of the K x-rays of molybdenum could be quantitatively accounted for by the energy loss in the ejection of carbon K-electrons. The crucial tests of the angular dependence of the shift, and of its independence of the atomic number of the scatterer, had not been decisively performed at Harvard.

A peculiar overtone to the situation was Duane's great resistance to accepting a photon theory of scattering. It was Duane and Hunt who, a few years previously, had quantitatively established the relation between the electron kinetic energy and the maximum frequency of the bremsstrahlung, which, in those pre-wavemechanical days, was considered one of the strongest evid-