Gödel's Proof

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Revised Edition

by
Ernest Nagel
and
James R. Newman

Edited and with a New Foreword by Douglas R. Hofstadter

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Foreword to the New Edition

by Douglas R. Hofstadter

In August 1959, my family returned to Stanford, California, after a year in Geneva. I was fourteen, newly fluent in French, in love with languages, entranced by writing systems, symbols, and the mystery of meaning, and brimming with curiosity about mathematics and how the mind works.

One evening, my father and I went to a bookstore where I chanced upon a little book with the enigmatic title *Gödel's Proof.* Flipping through it, I saw many intriguing figures and formulas, and was particularly struck by a footnote about quotation marks, symbols, and symbols symbolizing other symbols. Intuitively sensing that *Gödel's Proof* and I were fated for each other, I knew I had to buy it.

As we walked out, my dad remarked that he had taken a philosophy course at City College of New York from one of its authors, Ernest Nagel, after which they had become good friends. This coincidence added to the book's mystique, and once home, I voraciously gobbled up its every word. From start to finish, *Gödel's Proof* resonated with my passions; suddenly I found myself obsessed with truth and falsity, paradoxes and proofs, mappings and mirrorings, symbol manipulation and symbolic logic, mathematics and metamathematics, the mystery of creative leaps in human thinking, and the mechanization of mentality.

Soon thereafter, my dad informed me that by chance he had run into Ernest Nagel on campus. Professor Nagel, normally at Columbia, happened to be spending a year at Stanford. Within a few days, our families got together, and I was charmed by all four Nagels—Ernest and Edith, and their two sons, Sandy and Bobby, both close to my age. I was thrilled to meet the author of a book I so loved, and I found Ernest and Edith to be enormously welcoming of my adolescent enthusiasms for science, philosophy, music, and art.

All too soon, the Nagels' sabbatical year had nearly drawn to a close, but before they left, they warmly invited me to spend a week that summer at their cabin in Vermont. During that idyllic stay, Ernest and Edith came to represent for me the acme of civility, generosity, and modesty; thus they remain in my memory, all these years later. The high point for me was a pair of sunny afternoons when Sandy and I sat outdoors in a verdant meadow and I read aloud to him the entirety of *Gödel's Proof.* What a twisty delight to read this book to the son of one of its authors!

By mail over the next few years, Sandy and I explored number patterns in a way that had a profound impact upon the rest of my life, and perhaps on his as well. He went on—known as Alex—to become a mathematics professor at the University of Wisconsin. Bobby, too, remained a friend and today he—known as Sidney—is a physics professor at the University of Chicago, and we see each other with great pleasure from time to time.

I wish I could say that I had met James Newman. I was given as a high-school graduation present his magnificent four-volume set, *The World of Mathematics,* and I always admired his writing style and his love for mathematics, but sad to say, we never crossed paths.

At Stanford I majored in mathematics, and my love for the ideas in Nagel and Newman's book inspired me to take a couple of courses in logic and metamathematics, but I was terribly disappointed by their aridity. Shortly thereafter, I entered graduate school in math and the same disillusionment recurred. I dropped out of math and turned to physics, but after a few years I found myself once again in a quagmire of abstractness and confusion.

One day in 1972, seeking some relief, I was browsing in the university bookstore and ran across *A Profile of Mathematical Logic* by Howard DeLong—a book that had nearly the same electrifying effect on me as *Gödel's Proof* did in 1959. This lucid treatise rekindled in me the long-dormant embers of my love for logic, metamathematics, and that wondrous tangle of issues I had connected with Gödel's theorem and its proof. Having long since lost my original copy of Nagel and Newman's magical booklet, I bought another one—luckily, it was still in print!—and reread it with renewed fascination.

That summer, taking a break from graduate school and driving across the continent, I camped out and religiously read about Gödel's work, the nature of reasoning, and the dream of mechanizing thought and consciousness. Without planning it, I wound up in New York City, and the first people I contacted were my old friends Ernest and Edith Nagel, who served as intellectual and emotional mentors for me. Over the next several months, I spent countless evenings in their apartment, and we ardently discussed many topics, including, of course, Gödel's proof and its ramifications.

The year 1972 marked the beginning of my own intense personal involvement with Gödel's theorem and the rich sphere of ideas surrounding it. Over the next few years, I developed an idiosyncratic set of explorations on this nexus of ideas, and wound up calling it *Gödel, Escher, Bach: an Eternal Golden Braid.* There is no doubt that the parents of my sprawling volume were Nagel and Newman's book, on the one hand, and Howard DeLong's book, on the other.

What is Gödel's work about? Kurt Gödel, an Austrian logician born in 1906, was steeped in the mathematical atmosphere of his time, which was characterized by a relentless drive toward formalization. People were convinced that mathematical thinking could be captured by laws of pure symbol manipulation. From a fixed set of axioms and a fixed set of typographical rules, one could shunt symbols around and produce new strings of symbols, called "theorems." The pinnacle of this movement was a monumental three-volume work by Bertrand Russell and Alfred North Whitehead called *Principia Mathematica,* which came out in the years 1910–1913. Russell and Whitehead believed that they had grounded all of mathematics in pure logic, and that their work would form the solid foundation for all of mathematics forevermore.

A couple of decades later, Gödel began to doubt this noble vision, and one day, while studying the extremely austere patterns of symbols in these volumes, he had a flash that those patterns were so much like number patterns that he could in fact replace each symbol by a number and reperceive all of *Principia Mathematica* not as symbol shunting but as number crunching (to borrow a modern term). This new way of looking at things had an astounding wraparound effect: since the subject matter of *Principia Mathematica* was numbers, and since Gödel had turned the medium of the volumes also into numbers, this showed that *Principia Mathematica* was its own subject matter, or in other words, that the patterned formulas of Russell and Whitehead's system could be seen as saying things about each other, or possibly even about themselves.

This wraparound was a truly unexpected turn of events, for it inevitably brought ancient paradoxes of self-reference to Gödel's mind—above all, "This statement is false." Using this type of paradox as his guide, Gödel realized that, in principle, he could write down a formula of *Principia Mathematica* that perversely said about itself, "This formula is unprovable by the rules of *Principia Mathematica.*" The very existence of such a twisted formula was a huge threat to the edifice of Russell and Whitehead, for they had made the absolute elimination of "vicious circularity" a sacred goal, and had been convinced they had won the battle. But now it seemed that vicious circles had entered their pristine world through the back door, and Pandora's box was wide open.

The self-undermining Gödelian formula had to be dealt with, and Gödel did so most astutely, showing that although it resembled a paradox, it differed subtly from one. In particular, it was revealed to be a true statement that could not be proven using the rules of the system—indeed, a true statement whose unprovability resulted precisely from its truth.

In this shockingly bold manner, Gödel stormed the fortress of *Principia Mathematica* and brought it tumbling down in ruins. He also showed that his method applied to any system whatsoever that tried to accomplish the goals of *Principia Mathematica.* In effect, then, Gödel destroyed the hopes of those who believed that mathematical thinking is capturable by the rigidity of axiomatic systems, and he thereby forced mathematicians, logicians, and philosophers to explore the mysterious newly found chasm irrevocably separating provability from truth.

Ever since Gödel, it has been realized how subtle and deep the art of mathematical thinking is, and the once-bright hope of mechanizing human mathematical thought starts to seem shaky, if not utterly quixotic. What, then, after Gödel, is mathematical thinking believed to be? What, after Gödel, is mathematical truth? Indeed, what is truth at all? These are the central issues that still lie unresolved, seventy years after Gödel's epoch-making paper appeared.

My book, despite owing a large debt to Nagel and Newman, does not agree with all of their philosophical conclusions, and here I would like to point out one key difference. In their "Concluding Reflections," Nagel and Newman argue that from Gödel's discoveries it follows that computers—"calculating machines," as they call them—are in principle incapable of reasoning as flexibly as we humans reason, a result that supposedly ensues from the fact that computers follow "a fixed set of directives" (i.e., a program). To Nagel and Newman, this notion corresponds to a fixed set of axioms and rules of inference—and the computer's behavior, as it executes its program, amounts to that of a machine systematically churning out proofs of theorems in a formal system. This mapping of computer onto formal system takes the term "calculating machine" very literally—that is, a machine built to deal with numbers and arithmetical facts alone. The idea that such machines by their very nature should churn out sets of true statements about mathematics is seductive and certainly has a grain of truth to it, but it is far from the full vision of the power and versatility of computers.

Although computers, as their name implies, are built of rigidly arithmetic-respecting hardware, nothing in their design links them inseparably to mathematical truth. It is no harder to get a computer to print out scads of false calculations ("2 + 2 = 5; 0/0 = 43," etc.) than to print out theorems in a formal system. A subtler challenge would be to devise "a fixed set of directives" by which a computer might explore the world of mathematical ideas (not just strings of mathematical symbols), guided by visual imagery, the associative patterns linking concepts, and the intuitive processes of guesswork, analogy, and esthetic choice that every mathematician uses.

When Nagel and Newman were composing *Gödel's Proof,* the goal of getting computers to think like people—in other words, artificial intelligence—was very new and its potential was unclear. The main thrust in those early days used computers as mechanical instantiations of axiomatic systems, and as such, they did nothing but churn out proofs of theorems. Now admittedly, if this approach represented the full scope of how computers might ever in principle be used to model cognition, then, indeed, Nagel and Newman would be wholly justified in arguing, based on Gödel's discoveries, that computers, no matter how rapid their calculations or how capacious their memories, are necessarily less flexible and insightful than the human mind.

But theorem-proving is among the least subtle of ways of trying to get computers to think. Consider the program "AM," written in the mid-1970s by Douglas Lenat. Instead of mathematical statements, AM dealt with concepts; its goal was to seek "interesting" ones, using a rudimentary model of esthetics and simplicity. Starting from scratch, AM discovered many concepts of number theory. Rather than logically proving theorems, AM wandered around the world of numbers, following its primitive esthetic nose, sniffing out patterns, and making guesses about them. As with a bright human, most of AM's guesses were right, some were wrong, and, for a few, the jury is still out.

For another way of modeling mental processes computationally, take neural nets—as far from the theorem-proving paradigm as one could imagine. Since the cells of the brain are wired together in certain patterns, and since one can imitate any such pattern in software—that is, in a "fixed set of directives"—a calculating engine's power can be harnessed to imitate microscopic brain circuitry and its behavior. Such models been studied now for many years by cognitive scientists, who have found that many patterns of human learning, including error making as an automatic by-product, are faithfully replicated.

The point of these two examples (and I could give many more) is that human thinking in all its flexible and fallible glory can in principle be modeled by a "fixed set of directives," provided one is liberated from the preconception that computers, built on arithmetical operations, can do nothing but slavishly produce truth, the whole truth, and nothing but the truth. That idea, admittedly, lies at the core of formal axiomatic reasoning systems, but today no one takes such systems seriously as a model of what the human mind does, even when it is at its most logical. We now understand that the human mind is fundamentally not a logic engine but an analogy engine, a learning engine, a guessing engine, an esthetics-driven engine, a self-correcting engine. And having profoundly understood this lesson, we are perfectly able to make "fixed sets of directives" that have some of these qualities.

To be sure, we have not yet come close to producing a computer program that has anything remotely resembling the flexibility of the human mind, and in this sense Ernest Nagel and James Newman were exactly on the mark in declaring, in their poetic fashion, that Gödel's theorem "is an occasion, not for dejection, but for a renewed appreciation of the powers of creative reason." It could not be said better.

There is, however, an irony to Nagel and Newman's interpretation of Gödel's result. Gödel's great stroke of genius—as readers of Nagel and Newman will see—was to realize that numbers are a universal medium for the embedding of patterns of any sort, and that for that reason, statements seemingly about numbers alone can in fact encode statements about other universes of discourse. In other words, Gödel saw beyond the surface level of number theory, realizing that numbers could represent any kind of structure. The analogous Gödelian leap with respect to computers would be to see that because computers at base manipulate numbers, and because numbers are a universal medium for the embedding of patterns of any sort, computers can deal with arbitrary patterns, whether they are logical or illogical, consistent or inconsistent. In short, when one steps back far enough from myriads of interrelated number patterns, one can make out patterns from other domains, just as the eye looking at a screen of pixels sees a familiar face and nary a 1 or 0. This Gödelian view of computers has exploded on the modern world to such an extent that today the numerical substrate of computers is all but invisible, except to specialists. Ordinary people routinely use computers for word processing, game playing, communication, animation, designing, drawing, and on and on, all without ever thinking about the basic arithmetical operations going on deep down in the hardware. Cognitive scientists, relying on the arithmetical hardware of their computers to be error-free and uncreative, give their computers "fixed sets of directives" to model human error-making and creativity. There is no reason to think that the processes of creative mathematical thinking cannot, at least in principle, be modeled using computers. But back in the 1950s, such visions of the potential of computers were hard to see. Still, it is ironic that in a book devoted to celebrating Gödel's insight that numbers engulf the world of patterns at large, the primary philosophical conclusion would be based on not heeding that insight, and would thereby fail to see that calculating machines can replicate patterns of any imaginable sort—even those of the creative human mind.

I shall close with a few words about why I have taken the liberty of making some technical emendations to this classic text. Although the book received mostly warm accolades from reviewers, there were some critics who felt that in spots it was not sufficiently precise and that it risked misleading its readers. The first time through, I myself was unaware of any such deficiencies, but many years later, when reading *Gödel's Proof* with an eye to explaining these same ideas myself as precisely and clearly as possible, I stumbled over certain passages in Chapter VII and realized, after a while, that the stumbling was not entirely my own fault. It made me sad to realize that this beloved book had a few blemishes, but there was obviously nothing I could do about it. Oddly enough, though, in the margins of my copy I carefully annotated all the glitches that I uncovered, indicating how they might be corrected—almost as if I had foreseen that one day I would receive an email out of the blue from New York University Press asking me if I would consider writing a foreword to a new edition of the book.

I must certainly be among the readers most profoundly affected by the little opus by Ernest Nagel and James Newman, and for that reason, having been given the chance, I owe it to them to polish their gem and to give it a new luster for the new millennium. I would like to believe that in so doing, I am not betraying my respected mentors but am instead paying them homage, as an ardent and faithful disciple.

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Gödel's Proof

I

Introduction

In 1931 there appeared in a German scientific periodical a relatively short paper with the forbidding title "Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme" ("On Formally Undecidable Propositions of Principia Mathematica and Related Systems"). Its author was Kurt Gödel, then a young mathematician of 25 at the University of Vienna and since 1938 a permanent member of the Institute for Advanced Study at Princeton. The paper is a milestone in the history of logic and mathematics. When Harvard University awarded Gödel an honorary degree in 1952, the citation described the work as one of the most important advances in logic in modern times.

At the time of its appearance, however, neither the title of Gödel's paper nor its content was intelligible to most mathematicians. The *Principia Mathematica* mentioned in the title is the monumental three-volume treatise by Alfred North Whitehead and Bertrand Russell on mathematical logic and the foundations of mathematics; and familiarity with that work is not a prerequisite to successful research in most branches of mathematics. Moreover, Gödel's paper deals with a set of questions that has never attracted more than a comparatively small group of students. The reasoning of the proof was so novel at the time of its publication that only those intimately conversant with the technical literature of a highly specialized field could follow the argument with ready comprehension. Nevertheless, the conclusions Gödel established are now widely recognized as being revolutionary in their broad philosophical import. It is the aim of the present essay to make the substance of Gödel's findings and the general character of his proof accessible to the nonspecialist.

Gödel's famous paper attacked a central problem in the foundations of mathematics. It will be helpful to give a brief preliminary account of the context in which the problem occurs. Everyone who has been exposed to elementary geometry will doubtless recall that it is taught as a *deductive* discipline. It is not presented as an experimental science whose theorems are to be accepted because they are in agreement with observation. This notion, that a proposition may be established as the conclusion of an explicit *logical proof,* goes back to the ancient Greeks, who discovered what is known as the "axiomatic method" and used it to develop geometry in a systematic fashion. The axiomatic method consists in accepting *without* proof certain propositions as axioms or postulates (e.g., the axiom that through two points just one straight line can be drawn), and then deriving from the axioms all other propositions of the system as theorems. The axioms constitute the "foundations" of the system; the theorems are the "superstructure," and are obtained from the axioms with the exclusive help of principles of logic.

The axiomatic development of geometry made a powerful impression upon thinkers throughout the ages; for the relatively small number of axioms carry the whole weight of the inexhaustibly numerous propositions derivable from them. Moreover, if in some way the truth of the axioms can be established—and, indeed, for some two thousand years most students believed without question that they are true of space—both the truth and the mutual consistency of all the theorems are automatically guaranteed. For these reasons the axiomatic form of geometry appeared to many generations of outstanding thinkers as the model of scientific knowledge at its best. It was natural to ask, therefore, whether other branches of thought besides geometry can be placed upon a secure axiomatic foundation. However, although certain parts of physics were given an axiomatic formulation in antiquity (e.g., by Archimedes), until modern times geometry was the only branch of mathematics that had what most students considered a sound axiomatic basis.

But within the past two centuries the axiomatic method has come to be exploited with increasing power and vigor. New as well as old branches of mathematics, including the study of the properties of the familiar cardinal (or "whole") numbers,* were supplied with what appeared to be adequate sets of axioms. A climate of opinion was thus generated in which it was tacitly assumed that each sector of mathematical thought can be supplied with a set of axioms sufficient for developing systematically the endless totality of true propositions about the given area of inquiry.

Gödel's paper showed that this assumption is untenable. He presented mathematicians with the astounding and melancholy conclusion that the axiomatic method has certain inherent limitations, which rule out the possibility that even the properties of the non-negative integers can ever be fully axiomatized. What

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* Number theory is the study, going back to the ancient Greeks, of the properties of the natural numbers 0, 1, 2, 3, . . . —also sometimes called the "cardinal numbers" or "non-negative integers." Such properties include: how many factors a number has; how many different ways a number can be represented as a sum of smaller numbers; whether or not there is a biggest number having some specified property; whether or not certain equations have solutions that are whole numbers; and so on. Although number theory is inexhaustibly rich and full of surprises, its vocabulary is tiny—an alphabet of just a dozen symbols allows any number-theoretical statement to be expressed (although often cumbersomely).

In this book, we shall occasionally use the term "arithmetic" as a synonym for "number theory," but of course what this term entails is the full, rich universe of properties of the natural numbers, and not merely the mechanics of addition, subtraction, multiplication, and long division as taught in elementary schools, and as mechanized in cash registers and adding machines. [—*Ed.*]

is more, he proved that it is impossible to establish the internal logical consistency of a very large class of deductive systems—number theory, for example—unless one adopts principles of reasoning so complex that their internal consistency is as open to doubt as that of the systems themselves. In the light of these conclusions, no final systematization of many important areas of mathematics is attainable, and no absolutely impeccable guarantee can be given that many significant branches of mathematical thought are entirely free from internal contradiction.

Gödel's findings thus undermined deeply rooted preconceptions and demolished ancient hopes that were being freshly nourished by research on the foundations of mathematics. But his paper was not altogether negative. It introduced into the study of foundation questions a new technique of analysis comparable in its nature and fertility with the algebraic method that René Descartes introduced into geometry. This technique suggested and initiated new problems for logical and mathematical investigation. It provoked a reappraisal, still under way, of widely held philosophies of mathematics, and of philosophies of knowledge in general.

The details of Gödel's proofs in his epoch-making paper are too difficult to follow without considerable mathematical training. But the basic structure of his demonstrations and the core of his conclusions can be made intelligible to readers with very limited mathematical and logical preparation. To achieve such an understanding, the reader may find useful a brief account of certain relevant developments in the history of mathematics and of modern formal logic. The next four sections of this essay are devoted to this survey.

II

The Problem of Consistency

The nineteenth century witnessed a tremendous expansion and intensification of mathematical research. Many fundamental problems that had long withstood the best efforts of earlier thinkers were solved; new areas of mathematical study were created; and in various branches of the discipline new foundations were laid, or old ones entirely recast with the help of more precise techniques of analysis. To illustrate: the Greeks had proposed three problems in elementary geometry: with compass and straight-edge to trisect any angle, to construct a cube with a volume twice the volume of a given cube, and to construct a square equal in area to that of a given circle. For more than 2,000 years unsuccessful attempts were made to solve these problems; at last, in the nineteenth century it was proved that the desired constructions are logically impossible. There was, moreover, a valuable by-product of these labors. Since the solutions depend essentially upon determining the kind of roots that satisfy certain equations, concern with the celebrated exercises set in antiquity stimulated profound investigations into the nature of number and the structure of the number continuum. Rigorous definitions were eventually supplied for negative, complex, and irrational numbers; a logical basis was constructed for the real number system; and a new branch of mathematics, the theory of infinite numbers, was founded.

But perhaps the most significant development in its long-range effects upon subsequent mathematical history was the solution of another problem that the Greeks raised without answering. One of the axioms Euclid used in systematizing geometry has to do with parallels. The axiom he adopted is logically equivalent to (though not identical with) the assumption that through a point outside a given line only one parallel to the line can be drawn. For various reasons, this axiom did not appear "self-evident" to the ancients. They sought, therefore, to deduce it from the other Euclidean axioms, which they regarded as clearly self evident.[1] Can such a proof of the parallel axiom be

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[1] The chief reason for this alleged lack of self-evidence seems to have been the fact that the parallel axiom makes an assertion about *infinitely remote* regions of space. Euclid defines parallel lines as straight lines in a plane that, "being produced indefinitely in both directions," do not meet. Accordingly, to say that two lines are parallel is to make the claim that the two lines will not meet even "at infinity." But the ancients were familiar with lines that, though they do not intersect each other in any finite region of the plane, do meet "at infinity." Such lines are said to be "asymptotic." Thus, a hyperbola is asymptotic to its axes. It was therefore not intuitively

given? Generations of mathematicians struggled with this question, without avail. But repeated failure to construct a proof does not mean that none can be found any more than repeated failure to find a cure for the common cold establishes beyond doubt that humanity will forever suffer from running noses. It was not until the nineteenth century, chiefly through the work of Gauss, Bolyai, Lobachevsky, and Riemann, that the *impossibility* of deducing the parallel axiom from the others was demonstrated. This outcome was of the greatest intellectual importance. In the first place, it called attention in a most impressive way to the fact that a *proof* can be given of the *impossibility of proving* certain propositions within a given system. As we shall see, Gödel's paper is a proof of the impossibility of formally demonstrating certain important propositions in number theory. In the second place, the resolution of the parallel axiom question forced the realization that Euclid is not the last word on the subject of geometry, since new systems of geometry can be constructed by using a number of axioms different from, and incompatible with, those adopted by Euclid. In particular, as is well known, immensely interesting and fruitful results are obtained when Euclid's parallel axiom is replaced by the assumption that more than one parallel can be drawn to a given line through a given point, or, alter-

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evident to the ancient geometers that from a point outside a given straight line only one straight line can be drawn that will not meet the given line even at infinity.

natively, by the assumption that no parallels can be drawn. The traditional belief that the axioms of geometry (or, for that matter, the axioms of any discipline) can be established by their apparent self-evidence was thus radically undermined. Moreover, it gradually became clear that the proper business of pure mathematicians is to *derive theorems from postulated assumptions,* and that it is not their concern whether the axioms assumed are actually true. And, finally, these successful modifications of orthodox geometry stimulated the revision and completion of the axiomatic bases for many other mathematical systems. Axiomatic foundations were eventually supplied for fields of inquiry that had hitherto been cultivated only in a more or less intuitive manner. (See Appendix, no. 1.)

The over-all conclusion that emerged from these critical studies of the foundations of mathematics is that the age-old conception of mathematics as "the science of quantity" is both inadequate and misleading. For it became evident that mathematics is simply the discipline *par excellence* that draws the conclusions logically implied by any given set of axioms or postulates. In fact, it came to be acknowledged that the validity of a mathematical inference in no sense depends upon any special meaning that may be associated with the terms or expressions contained in the postulates. Mathematics was thus recognized to be much more abstract and formal than had been traditionally supposed: more abstract, because mathematical statements can be construed in principle to be about anything whatsoever rather than about some inherently circumscribed set of objects or traits of objects; and more formal, because the validity of mathematical demonstrations is grounded in the structure of statements, rather than in the nature of a particular subject matter. The postulates of any branch of demonstrative mathematics are not inherently about space, quantity, apples, angles, or budgets; and any special meaning that may be associated with the terms (or "descriptive predicates") in the postulates plays no essential role in the process of deriving theorems. We repeat that the sole question confronting the pure mathematician (as distinct from the scientist who employs mathematics in investigating a special subject matter) is not whether the postulates assumed or the conclusions deduced from them are true, but whether the alleged conclusions are in fact the *necessary logical consequences* of the initial assumptions.

Take this example. Among the undefined (or "primitive") terms employed by the influential German mathematician David Hilbert in his famous axiomatization of geometry (first published in 1899) are 'point', 'line', 'lies on', and 'between'. We may grant that the customary meanings connected with these expressions play a role in the process of discovering and learning theorems. Since the meanings are familiar, we feel we understand their various interrelations, and they motivate the formulation and selection of axioms; moreover, they suggest and facilitate the formulation of the statements we hope to establish as theorems.

Yet, as Hilbert plainly states, insofar as we are concerned with the primary mathematical task of exploring the purely logical relations of dependence between statements, the familiar connotations of the primitive terms are to be ignored, and the sole "meanings" that are to be associated with them are those assigned by the axioms into which they enter.[2] This is the point of Russell's famous epigram: pure mathematics is the subject in which we do not know what we are talking about, or whether what we are saying is true.

A land of rigorous abstraction, empty of all familiar landmarks, is certainly not easy to get around in. But it offers compensations in the form of a new freedom of movement and fresh vistas. The intensified formalization of mathematics emancipated people's minds from the restrictions that the customary interpretation of expressions placed on the construction of novel systems of postulates. New kinds of algebras and geometries were developed which marked significant departures from the mathematics of tradition. As the meanings of certain terms became more general, their use became broader and the inferences that could be drawn from them less confined. Formalization led to a great variety of systems of considerable mathematical interest and value. Some of these systems, it must be

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[2] In more technical language, the primitive terms are "implicitly" defined by the axioms, and whatever is not covered by the implicit definitions is irrelevant to the demonstration of theorems.

admitted, did not lend themselves to interpretations as obviously intuitive (i.e., commonsensical) as those of Euclidean geometry or arithmetic, but this fact caused no alarm. Intuition, for one thing, is an elastic faculty: our children will probably have no difficulty in accepting as intuitively obvious the paradoxes of relativity, just as we do not boggle at ideas that were regarded as wholly unintuitive a couple of generations ago. Moreover, as we all know, intuition is not a safe guide: it cannot properly be used as a criterion of either truth or fruitfulness in scientific explorations.

However, the increased abstractness of mathematics raised a more serious problem. It turned on the question whether a given set of postulates serving as foundation of a system is internally consistent, so that no mutually contradictory theorems can be deduced from the postulates. The problem does not seem pressing when a set of axioms is taken to be about a definite and familiar domain of objects; for then it is not only significant to ask, but it may be possible to ascertain, whether the axioms are indeed true of these objects. Since the Euclidean axioms were generally supposed to be true statements about space (or objects in space), no mathematician prior to the nineteenth century ever considered the question whether a pair of contradictory theorems might some day be deduced from the axioms. The basis for this confidence in the consistency of Euclidean geometry is the sound principle that logically incompatible statements cannot be simultane-