The Crisis in the Foundations of Mathematics

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The foundational crisis is a celebrated affair among mathematicians and it has also reached a large nonmathematical audience. A well-trained mathematician is supposed to know something about the three viewpoints called "logicism," "formalism," and "intuitionism" (to be explained below), and about what GÖDELs incompleteness results tell us about the status of mathematical knowledge. Professional mathematicians tend to be rather opinionated about such topics, either dismissing the foundational discussion as irrelevant—and thus siding with the winning party—or defending, in principle or as an intriguing option, some form of revisionist approach to mathematics. But the real outlines of the historical debate are not well-known and the subtler philosophical issues at stake are often ignored. Here we shall mainly discuss the former, in the hope that this will help bring the main conceptual issues into sharper focus.

Usually, the foundational crisis is understood as a relatively localized event in the 1920s, a heated debate between the partisans of "classical" (meaning late-nineteenth-century) mathematics, led by HILBERT, and their critics, led by BROUWER, who advocated strong revision of the received doctrines. There is, however, a second, and in my opinion very important, sense in which the "crisis" was a long and global process, indistinguishable from the rise of modern mathematics and the philosophical/methodological issues it created. This is the standpoint from which the present account has been written.

Within this longer process one can still pick out some noteworthy intervals. Around 1870 there were many discussions about the acceptability of non-Euclidean geometries, and also about the proper foundations of complex analysis and even the real numbers. Early in the twentieth century there were debates about set theory, about the concept of the continuum, and about the role of logic and the axiomatic method versus the role of intuition. By about 1925 there was a crisis in the proper sense, during which the main opinions in these debates were developed and turned into detailed mathematical research projects. And in the 1930s Gödel proved his incompleteness results, which could not be assimilated without some cherished beliefs being abandoned. Let us analyse some of these events and issues in greater detail.

1 Early Foundational Questions

There is evidence that in 1899 Hilbert endorsed the viewpoint that came to be known as *logicism*. Logicism was the thesis that the basic concepts of mathematics are definable by means of logical notions, and that the key principles of mathematics are deducible from logical principles alone.

Over time this thesis has become unclear, based as it seems to be on a fuzzy and immature conception of the scope of logical theory. But historically speaking logicism was a neat intellectual reaction to the rise of modern mathematics, and particularly to the set-theoretic approach and methods. Since the majority opinion was that set theory is just a part of (refined) logic,[1] this thesis was thought to be supported by the fact that the theories of natural and real numbers can be derived from set theory, and also by the increasingly important role of set-theoretic methods in algebra, and real and complex analysis.

Hilbert was following Dedekind in the way he understood mathematics. For us, the essence of Hilbert's and Dedekind's early logicism is their self-conscious endorsement of certain modern methods, however daring they seemed at the time. These methods had emerged gradually during the nineteenth century, and were particularly associated with Göttingen mathematics (GAUSS, DIRICHLET); they experienced a crucial turning point with Riemann's novel ideas, and were developed further by Dedekind, Cantor, Hilbert, and other lesser figures. Meanwhile, the influential Berlin school of mathematics had opposed this new trend, KRONECKER head-on and WEIERSTRASS more subtly. (The name of Weierstrass is synonymous with the introduction of rigor in real analysis, but in fact, as will be indicated below, he did not favor the more modern methods elaborated in his time.) Mathematicians in Paris and elsewhere also harbored doubts about these new and radical ideas.

The most characteristic traits of the modern approach were:

(i) acceptance of the notion of an "arbitrary" function proposed by Dirichlet;
(ii) a wholehearted acceptance of infinite sets and the higher infinite;
(iii) a preference "to put thoughts in the place of calculations" (Dirichlet), and to concentrate on "structures" characterized axiomatically; and

1. One should mention that key figures like RIEMANN and CANTOR disagreed (see Ferreirós 1999). The "majority" included DEDEKIND, PEANO, Hilbert, RUSSELL, and others.

(iv) a reliance on "purely existential" methods of proof.

An early and influential example of these traits was Dedekind's approach (1871) to ALGEBRAIC NUMBER THEORY—his set-theoretic definition of NUMBER FIELDS and IDEALS, and the methods by which he proved results such as the fundamental theorem of unique decomposition. In a remarkable departure from the number-theoretic tradition, Dedekind studied the factorization properties of algebraic integers in terms of ideals, which are certain infinite sets of algebraic integers. Using this new abstract concept, together with a suitable definition of the product of two ideals, Dedekind was able to prove in full generality that, within any ring of algebraic integers, ideals possess a unique decomposition into prime ideals.

The influential algebraist Kronecker complained that Dedekind's proofs do not enable us to calculate, in a particular case, the relevant divisors or ideals: that is, the proof was *purely existential*. Kronecker's view was that this abstract way of working, made possible by the set-theoretic methods and by a concentration on the algebraic properties of the structures involved, was too remote from an algorithmic treatment—that is, from so-called *constructive* methods. But for Dedekind this complaint was misguided: it merely showed that he had succeeded in elaborating the principle "to put thoughts in the place of calculations," a principle that was also emphasized in Riemann's theory of complex functions. Obviously, concrete problems would require the development of more delicate computational techniques, and Dedekind contributed to this in several papers. But he also insisted on the importance of a general, conceptual theory.

The ideas and methods of Riemann and Dedekind became better known through publications of the period 1867–72. These were found particularly shocking because of their very explicit defence of the view that mathematical theories *ought not* to be based upon formulas and calculations—they should always be based on clearly formulated *general concepts*, with analytical expressions or calculating devices relegated to the further development of the theory.

To explain the contrast, let us consider the particularly clear case of the opposition between Riemann's and Weierstrass's approaches to function theory. Weierstrass opted systematically for explicit representations of analytic (or HOLOMORPHIC) functions by means of power series of the form $\sum_{n=0}^{\infty} a_n(z-a)^n$, connected with each other by ANALYTIC CONTINUATION. Riemann chose a very different and more abstract approach, defining a function to be analytic if it satisfies the CAUCHY–RIEMANN differentiability conditions.[2] This neat conceptual definition appeared objectionable to Weierstrass, as the class of differentiable functions had never been carefully characterized (in terms of series representations, for example). Exercising his famous critical abilities, Weierstrass offered examples of continuous functions that were nowhere differentiable.

It is worth mentioning that, in preferring infinite series as the key means for research in analysis and function theory, Weierstrass remained closer to the old eighteenth-century idea of a function as an analytical expression. On the other hand, Riemann and Dedekind were always in favor of Dirichlet's abstract idea of a function f as an "arbitrary" way of associating with each x some $y = f(x)$. (Previously it had been required that y should be expressed in terms of x by means of an explicit formula.) In his letters, Weierstrass criticized this conception of Dirichlet's as too general and vague to constitute the starting point for any interesting mathematical development. He seems to have missed the point that it was in fact just the right framework in which to define and analyze general concepts such as CONTINUITY and INTEGRATION. This framework came to be called the *conceptual approach* in nineteenth-century mathematics.

Similar methodological debates emerged in other areas too. In a letter of 1870, Kronecker went as far as saying that the Bolzano–Weierstrass theorem was an "obvious sophism," promising that he would offer counterexamples. The Bolzano–Weierstrass theorem, which states that an infinite bounded set of real numbers has an accumulation point, was a cornerstone of classical analysis, and was emphasized as such by Weierstrass in his famous Berlin lectures. The problem for Kronecker was that this theorem rests entirely on the completeness axiom for the real numbers (which, in one version, states that every sequence of nonempty nested closed intervals in $\mathbb{R}$ has a nonempty intersection). The real numbers cannot be constructed in an elementary way from the rational numbers: one has to make heavy use of infinite sets (such as the set of all possible "Dedekind cuts," which are subsets $C \subset \mathbb{Q}$ such that $p \in C$ whenever p and q are rational numbers such that $p < q$ and $q \in C$). To put it another way: Kronecker was drawing attention to the problem that, very often, the accumulation point in the Bolzano–Weierstrass theorem cannot be constructed by elementary operations from the rational

2. Riemann determined particular functions by a series of *independent* traits such as the associated RIEMANN SURFACE and the behavior at singular points. These traits determined the function via a certain variational principle (the "Dirichlet principle"), which was also criticized by Weierstrass, who gave a counterexample to it. Hilbert and Kneser would later reformulate and justify the principle.

numbers. The classical idea of the set of real numbers, or "the continuum," already contained the seeds of the *non-constructive* ingredient in modern mathematics.

Later on, in around 1890, Hilbert's work on invariant theory led to a debate about his purely existential proof of another basic result, the "basis theorem," which states (in modern terminology) that every ideal in a polynomial ring is finitely generated. Paul Gordan, famous as the "king" of invariants for his heavily algorithmic work on the topic, remarked humorously that this was "theology," not mathematics! (He apparently meant that, because the proof was purely existential, rather than constructive, it was comparable with philosophical proofs of the existence of God.)

This early foundational debate led to a gradual clarification of the opposing viewpoints. Cantor's proofs in set theory also became quintessential examples of the modern methodology of existential proof. He offered an explicit defence of the higher infinite and modern methods in a paper of 1883, which was peppered with hidden attacks on Kronecker's views. Kronecker in turn criticized Dedekind's methods publicly in 1882, spoke privately against Cantor, and in 1887 published an attempt to spell out his foundational views. Dedekind replied with a detailed set-theoretic (and "thus," for him, logicistic) theory of the natural numbers in 1888.

The early round of criticism ended with an apparent victory for the modern camp, which enrolled new and powerful allies such as Hurwitz, Minkowski, Hilbert, Volterra, Peano, and Hadamard, and which was defended by influential figures such as Klein. Although Riemannian function theory was still in need of further refinement, recent developments in real analysis, number theory, and other fields were showing the power and promise of the modern methods. During the 1890s, the modern viewpoint in general, and logicism in particular, enjoyed great expansion. Hilbert developed the new methodology into the axiomatic method, which he used to good effect in his treatment of geometry (1899 and subsequent editions) and of the real number system.

Then, dramatically, came the so-called "logical" paradoxes, discovered by Cantor, Russell, Zermelo, and others, which will be discussed below. These were of two kinds. On the one hand, there were arguments showing that assumptions that certain sets exist lead to contradictions. These were later called the *set-theoretic* paradoxes. On the other, there were arguments, later known as the *semantic* paradoxes, which showed up difficulties with the notions of truth and definability. These paradoxes completely destroyed the attractive view of recent developments in mathematics that had been proposed by logicism. Indeed, the heyday of logicism came *before* the paradoxes, that is, before 1900; it subsequently enjoyed a revival with Russell and his "theory of types," but by 1920 logicism was of interest more to philosophers than to mathematicians. However, the divide between advocates of the modern methods and constructivist critics of these methods was there to stay.

2 Around 1900

Hilbert opened his famous list of mathematical problems at the Paris International Congress of Mathematics of 1900 with Cantor's continuum problem, a key question in set theory, and with the problem of whether every set can be well-ordered. His second problem amounted to establishing the consistency of the notion of the set $\mathbb{R}$ of real numbers. It was not by chance that he began with these problems: rather, it was a way of making a clear statement about how mathematics should be in the twentieth century. Those two problems, and the axiom of choice employed by Hilbert's young colleague Zermelo to show that $\mathbb{R}$ (the *continuum*) can be well-ordered, are quintessential examples of the traits (i)–(iv) that were listed above. It is little wonder that less daring minds objected and revived Kronecker's doubts, as can be seen in many publications of 1905–06. This brings us to the next stage of the debate.

2.1 Paradoxes, Consistency

In a remarkable turn of events, the champions of modern mathematics stumbled upon arguments that cast new doubts on its cogency. In around 1896, Cantor discovered that the seemingly harmless concepts of the set of all ordinals and the set of all cardinals led to contradictions. In the former case the contradiction is usually called the *Burali–Forti paradox*; the latter is the *Cantor paradox*. The assumption that all transfinite ordinals form a set leads, by Cantor's previous results, to the result that there is an ordinal that is less than itself—and similarly for cardinals. Upon learning of these paradoxes, Dedekind began to doubt whether human thought is completely rational. Even worse, in 1901–02 Zermelo and Russell discovered a very elementary contradiction, now known as the *Zermelo–Russell paradox*, which will be discussed in a moment. The untenability of the previous understanding of set theory as logic became clear, and there began a new period of instability. But it should be said that only logicists were seriously upset by these arguments: they were presented with contradictions in their theories.

Let us explain the importance of the Zermelo–Russell paradox. From Riemann to Hilbert, many authors accepted the principle that, given any well-defined logical or mathematical property, there exists a set of *all* objects satisfying that property. In symbols: given a well-defined property p, there exists another object, the set $\{x : p(x)\}$. For example, corresponding to the property of "being a real number" (which is expressed formally by Hilbert's axioms) there is the set of all real numbers; corresponding to the property of "being an ordinal" there is the set of all ordinals; and so on. This is called the *comprehension principle*, and it constitutes the basis for the logicistic understanding of set theory, often called naive set theory, although its naivete is only clear with hindsight. The principle was thought of as a basic logical law, so that all of set theory was merely a part of elementary logic.

The Zermelo–Russell paradox shows that the comprehension principle is contradictory, and it does so by formulating a property that seems to be as basic and purely logical as possible. Let $p(x)$ be the property $x \notin x$ (bearing in mind that negation and membership were assumed to be purely logical concepts). The comprehension principle yields the existence of the set $R = \{x : x \notin x\}$, but this leads quickly to a contradiction: if $R \in R$, then $R \notin R$ (by the definition of R), and similarly, if $R \notin R$, then $R \in R$. Hilbert (like his older colleague FREGE) was led to abandon logicism, and even wondered whether Kronecker might have been right all along. Eventually he concluded that set theory had shown the need to refine logical theory. It was also necessary to establish set theory axiomatically, as a basic *mathematical* theory based on mathematical (not logical) axioms, and Zermelo undertook this task.

Hilbert famously advocated that to claim that a set of mathematical objects exists is tantamount to proving that the corresponding axiom system is consistent—that is, free of contradictions. The documentary evidence suggests that Hilbert came to this celebrated principle in reaction to Cantor's paradoxes. His reasoning may have been that, instead of jumping directly from well-defined concepts to their corresponding sets, one had first to prove that the concepts are logically consistent. For example, before one could accept the set of all real numbers, one should prove the consistency of Hilbert's axiom system for them. Hilbert's principle was a way of removing any metaphysical content from the notion of mathematical existence. This view, that mathematical objects had a sort of "ideal existence" in the realm of thought rather than a metaphysical existence in the real world, had been anticipated by Dedekind and Cantor.

The "logical" paradoxes included not only the ones that go by the names of Burali–Forti, Cantor, and Russell, but also many semantic paradoxes formulated by Russell, Richard, König, Grelling, etc. (Richard's paradox will be discussed below.) Much confusion emerged from the abundance of different paradoxes, but one thing is clear: they played an important role in promoting the development of modern logic and convincing mathematicians of the need for strictly formal presentation of their theories. Only when a theory has been stated within a precise formal language can one disregard the semantic paradoxes, and even formulate the distinction between these and the set-theoretic ones.

2.2 Predicativity

When the books of Frege and Russell made the paradoxes of set theory widely known to the mathematical community in 1903, Poincaré used them to advance his critical points against logicism and formalism.

His analysis of the paradoxes led him to coin an important new notion, *predicativity*, and maintain that impredicative definitions should be avoided in mathematics. Informally, a definition is impredicative when it introduces an element by reference to a totality that already contains that element. A typical example is the following: Dedekind defines the set $\mathbb{N}$ of natural numbers as the intersection of all sets that contain 1 and are closed under an injective function σ such that $1 \notin \sigma(\mathbb{N})$. (The function σ is called the *successor function*.) His idea was to characterize $\mathbb{N}$ as minimal, but in his procedure the set $\mathbb{N}$ is first introduced by appeal to a totality of sets that should already include $\mathbb{N}$ itself. This kind of procedure appeared unacceptable to Poincaré (and also to Russell), especially when the relevant object can be specified *only* by reference to the more embracing totality. Poincaré found examples of impredicative procedures in each of the paradoxes he studied.

Take, for instance, Richard's paradox, which is one of the linguistic or semantic paradoxes (where, as we said, the notions of truth and definability are prominent). One begins with the idea of *definable* real numbers. Because definitions must be expressed in a certain language by finite expressions, there are only countably many definable numbers. Indeed, we can explicitly count the definable real numbers by listing them in alphabetical order of their definitions. (This is known as the *lexicographic order*.) Richard's idea was to apply to this list a diagonal process, of the kind used by Cantor to prove that $\mathbb{R}$ is not COUNTABLE. Let the definable numbers be $a_1, a_2, a_3, \dots$. Define a new number r in a systematic way, making sure that the nth decimal digit of r is different from the nth decimal digit of a_n. (For example, let the nth digit of r be 2 unless the nth digit of a_n is 2, in which case let the nth digit of r

be 4.) Then r cannot belong to the set of definable numbers. But in the course of this construction, the number r has just been defined in finitely many words! Poincaré would ban impredicative definitions and would therefore prevent the introduction of the number r, since it was defined with reference to the totality of all definable numbers.[3]

In this kind of approach to the foundations of mathematics, all mathematical objects (beyond the natural numbers) must be introduced by explicit definitions. If a definition refers to a presumed totality of which the object being defined is itself a member, we are involved in a circle: the object itself is then a constituent of its own definition. In this view, "definitions" must be predicative: one refers only to totalities that have already been established before the object one is defining. Important authors such as Russell and Weyl accepted this point of view and developed it.

Zermelo was not convinced, arguing that impredicative definitions were often used unproblematically, not only in set theory (as in Dedekind's definition of $\mathbb{N}$, for example), but also in classical analysis. As a particular example, he cited Cauchy's proof of the fundamental theorem of algebra,[4] but a simpler example of impredicative definition is the least upper bound in real analysis. The real numbers are not introduced separately, by explicit predicative definitions of each one of them; rather, they are introduced as a completed whole, and the particular way in which the least upper bound of an infinite bounded set of reals is singled out becomes impredicative. But Zermelo insisted that these definitions are innocuous, because the object being defined is not "created" by the definition; it is merely singled out (see his paper of 1908 in van Heijenoort (1967, pp. 183–98)).

Poincaré's idea of abolishing impredicative definitions became important for Russell, who incorporated it as the "vicious circle principle" in his influential *theory of types.* Type theory is a system of higher-order logic, with quantification over properties or sets, over relations, over sets of sets, and so on. Roughly speaking, it is based on the idea that the elements of any set should always be objects of a certain homogeneous type. For instance, we can have sets of "individuals," such as $\{a, b\}$, or sets of *sets* of individuals, such as $\{\{a\}, \{a, b\}\}$, but never a "mixed" set like $\{a, \{a, b\}\}$. Russell's version of type theory became rather complicated because of the so-called "ramification" he adopted in order to avoid impredicativity. This system, together with axioms of infinity, choice, and "reducibility" (a surprisingly ad hoc means to "collapse" the ramification), sufficed for the development of set theory and the number systems. Thus it became the logical basis for the renowned *Principia Mathematica* by Whitehead and Russell (1910–13), in which they carefully developed a foundation for mathematics.

Type theory remained the main logical system until about 1930, but under the form of *simple* type theory (that is, without ramification), which, as Chwistek, Ramsey, and others realized, suffices for a foundation in the style of *Principia*. Ramsey proposed arguments that were aimed at eliminating worries about impredicativity, and he tried to justify the other existence axioms of *Principia*—the axiom of infinity and the axiom of choice—as logical principles. But his arguments were inconclusive. Russell's attempt to rescue logicism from the paradoxes remained unconvincing, except to some philosophers (especially members of the Vienna Circle).

Poincaré's suggestions also became a key principle for the interesting foundational approach proposed by Weyl in his book *Das Kontinuum* (1918). The main idea was to accept the theory of the natural numbers as they were conventionally developed using classical logic, but to work predicatively from there on. Thus, unlike Brouwer, Weyl accepted the principle of the excluded middle. (This, and Brouwer's views, will be discussed in the next section.) However, the full system of the real numbers was not available to him: in his system the set $\mathbb{R}$ was not complete and the Bolzano–Weierstrass theorem failed, which meant that he had to devise sophisticated replacements for the usual derivations of results in analysis.

Another go at getting this sentence to suit everyone. OK now José?

The idea of *predicative foundations* for mathematics, in the style of Weyl, has been carefully developed in recent decades with noteworthy results (see Feferman 1998). Predicative systems lie between those that countenance all of the modern methodology and the more stringent constructivistic systems. This is one of several foundational approaches that do not fit into the conventional but by now outdated triad of logicism, formalism, and intuitionism.

2.3 Choices

As important as the paradoxes were, their impact on the foundational debate has often been overstated. One frequently finds accounts that take the paradoxes as the real

3. The modern solution is to establish mathematical definitions within a well-determined formal theory, whose language and expressions are fixed to begin with. Richard's paradox takes advantage of an ambiguity as to what the available means of definition are.

4. Cauchy's reasoning was clearly nonconstructive, or "purely existential" as we have been saying. In order to show that the polynomial must have one root, Cauchy studied the absolute value of the polynomial, which has a global minimum σ. This global minimum is impredicatively defined. Cauchy assumed that it was positive, and from this he derived a contradiction (see Gilain 1991).

starting point of the debate, in strong contrast with our discussion in Section 1. But even if we restrict our attention to the first decade of the twentieth century, there was another controversy of equal, if not greater, importance: the arguments that surrounded the axiom of choice and Zermelo's proof of the well-ordering theorem.

Recall from Section 2.1 that the association between sets and their defining properties was at the time deeply ingrained in the minds of mathematicians and logicians (via the contradictory principle of comprehension). The axiom of choice (AC), is the principle that, given any infinite family of disjoint nonempty sets, there is a set, known as a *choice set*, that contains exactly one element from each set in the family. The problem with this, said the critics, is that it merely stipulates the existence of the choice set and does not give a defining property for it. Indeed, when it *is* possible to characterize the choice set explicitly, then the use of AC is avoidable! But in the case of Zermelo's well-ordering theorem it is essential to employ AC. The required well-ordering of $\mathbb{R}$ "exists" in the ideal sense of Cantor, Dedekind, and Hilbert, but it seemed clear that it was completely out of reach from any constructivist perspective.

Thus, the axiom of choice exacerbated obscurities in previous conceptions of set theory, forcing mathematicians to introduce much-needed clarifications. On the one hand, AC was nothing but an explicit statement of previous views about *arbitrary* subsets, and yet, on the other, it obviously clashed with strongly held views about the need to explicitly define infinite sets by properties. The stage was set for deep debate. The discussions about this particular topic contributed more than anything else to a clarification of the existential implications of modern mathematical methods. It is instructive to know that Borel, Baire, and Lebesgue, who became critics, had all relied on AC in less obvious ways in order to prove theorems of analysis. Not by chance, the axiom was suggested to Zermelo by an analyst, Erhard Schmidt, who was a student of Hilbert.[5]

After the publication of Zermelo's proof, an intense debate developed throughout Europe. Zermelo was spurred on to work out the foundations of set theory in an attempt to show that his proof could be developed within an unexceptionable axiom system. The outcome was his famous axiom system (see the Zermelo–Fraenkel axioms), a masterpiece that emerged from careful analysis of set theory as it was historically given in the contributions of Cantor and Dedekind and in Zermelo's own theorem. With some additions due to Fraenkel and von Neumann (the axioms of replacement and regularity) and the major innovation proposed by Weyl and Skolem (to formulate it within first-order logic, i.e., quantifying over individuals, the sets, but not over their properties), the axiom system became in the 1920s the one that we now know.

The ZFC system (this stands for "Zermelo–Fraenkel with choice") codifies the key traits of modern mathematical methodology, offering a satisfactory framework for the development of mathematical theories and the conduct of proofs. In particular, it includes strong existence principles, allows impredicative definitions and arbitrary functions, warrants purely existential proofs, and makes it possible to define the main mathematical structures. It thus exhibits all the tendencies (i)–(iv) mentioned in Section 1. Zermelo's own work was completely in line with Hilbert's informal axiomatizations of about 1900, and he did not forget to promise a proof of consistency. Axiomatic set theory, whether in the Zermelo–Fraenkel presentation or the von Neumann–Bernays–Gödel version, is the system that most mathematicians regard as the working foundation for their discipline.

As of 1910, the contrast between Russell's type theory and Zermelo's set theory was strong. The former system was developed within formal logic, and its point of departure (albeit later compromised for pragmatic reasons) was in line with predicativism; in order to derive mathematics, the system needed the existential assumptions of infinity and choice, but these were rhetorically treated as tentative hypotheses rather than outright axioms. The latter system was presented informally, adopted the impredicative standpoint wholeheartedly, and asserted as axioms strong existential assumptions that were sufficient to derive all of classical mathematics and Cantor's theory of the higher infinite. In the 1920s the separation diminished greatly, especially with respect to the first two traits just indicated. Zermelo's system was perfected and formulated within the language of modern formal logic. And the Russellians adopted simple type theory, thus accepting the impredicative and "existential" methodology of modern mathematics. This is often given the (potentially confusing) term "Platonism": the objects that the theory refers to are treated *as if* they were independent of what the mathematician can actually and explicitly define.

Meanwhile, back in the first decade of the twentieth century, a young mathematician in the Netherlands was beginning to find his way toward a philosophically colored version of constructivism. Brouwer presented his strikingly peculiar metaphysical and ethical views in 1905, and

5. One may still gain much insight by reading the letters exchanged by the French analysts in 1905 (see Moore 1982; Ewald 1996) and Zermelo's clever arguments in his second 1908 proof of well-ordering (van Heijenoort 1967).

started to elaborate a corresponding foundation for mathematics in his thesis of 1907. His philosophy of "intuitionism" derived from the old metaphysical view that individual consciousness is the one and only source of knowledge. This philosophy is perhaps of little interest in itself, so we shall concentrate here on Brouwer's constructivistic principles. In the years around 1910, Brouwer became a renowned mathematician, with crucial contributions to topology such as his fixed-point theorem. By the end of World War I, he started to publish detailed elaborations of his foundational ideas, helping to create the famous "crisis," to which we now turn. He was also successful in establishing the customary (but misleading) distinction between formalism and intuitionism.

3 The *Crisis* in a Strict Sense

In 1921, the *Mathematische Zeitschrift* published a paper by Weyl in which the famous mathematician, who was a disciple of Hilbert, openly espoused intuitionism and diagnosed a "crisis in the foundations" of mathematics. The crisis pointed towards a "dissolution" of the old state of analysis, by means of Brouwer's "revolution." Weyl's paper was meant as a propaganda pamphlet to rouse the sleepers, and it certainly did. Hilbert answered in the same year, accusing Brouwer and Weyl of attempting a "putsch" aimed at establishing "dictatorship à la Kronecker" (see the relevant papers in Mancosu (1998) and van Heijenoort (1967)). The foundational debate shifted dramatically toward the battle between Hilbert's attempts to justify "classical" mathematics and Brouwer's developing reconstruction of a much-reformed intuitionistic mathematics.

Why was Brouwer "revolutionary"? Up to 1920 the key foundational issues had been the acceptability of the real numbers and, more fundamentally, of the impredicativity and strong existential assumptions of set theory, which supported the higher infinite and the unrestricted use of existential proofs. Set theory and, by implication, classical analysis had been criticized for their reliance on impredicative definitions and for their strong existential assumptions (in particular, the axiom of choice, whose extensive use in classical analysis was demonstrated by Sierpinski in 1918). Thus, the debate in the first two decades of the twentieth century was mainly about which principles to accept when it came to defining and establishing the existence of sets and subsets. A key question was, can one make rigorous the vague idea behind talk of "arbitrary subsets"? The most coherent reactions had been Zermelo's axiomatization of set theory and Weyl's predicative system in *Das Kontinuum*. (The *Principia Mathematica* of Whitehead and Russell was an unsuccessful compromise between predicativism and classical mathematics.)

Brouwer, however, brought new and even more basic questions to the fore. No one had questioned the traditional ways of reasoning about the natural numbers: classical logic, in particular the use of quantifiers and the principle of the excluded middle, had been used in this context without hesitation. But Brouwer put forward principled critiques of these assumptions and started developing an alternative theory of analysis that was much more radical than Weyl's. In doing so, he came upon a new theory of the continuum, which finally enticed Weyl and made him announce the coming of a new age.

3.1 Intuitionism

Brouwer began the systematic development of his views with two papers on "intuitionistic set theory," written in German and published in 1918 and 1919 by the *Verhandelingen* of the Dutch Academy of Sciences. These contributions were part of what he regarded as the "Second Act" of intuitionism. The "First Act" (from 1907) had been his emphasis on the intuitive foundations of mathematics. Already Klein and Poincaré had insisted that intuition has an inescapable role to play in mathematical knowledge: as important as logic is in proofs and in the development of mathematical theory, mathematics cannot be reduced to pure logic; theories and proofs are of course organized logically, but their basic principles (axioms) are grounded in intuition. But Brouwer went beyond them and insisted on the absolute independence of mathematics from language and logic.

From 1907, Brouwer rejected the principle of the excluded middle (PEM), which he regarded as equivalent to Hilbert's conviction that all mathematical problems are solvable. PEM is the logical principle that the statement $p \vee \neg p$ (that is, either p or not p) must always be true, whatever the proposition p may be. (For example, it follows from PEM that either the decimal expansion of π contains infinitely many 7s or it contains only finitely many sevens, even though we do not have a proof of which.) Brouwer held that our customary logical principles were abstracted from the way we dealt with subsets of a finite set, and that it was wrong to apply them to infinite sets as well. After World War I he started the systematic reconstruction of mathematics.

The intuitionist position is that one can only state "p or q" when one can give either a constructive proof of p or a constructive proof of q. This standpoint has the consequence that proofs by contradiction (*reductio ad* *absurdum*) are not valid. Consider Hilbert's first proof of his "basis theorem" (Section 1), achieved by *reductio*: he showed that one can derive a contradiction from the assumption that the basis is infinite, and from this he concluded that the basis is finite. The logic behind this procedure is that we start from a concrete instance of PEM, $p \vee \neg p$, show that $\neg p$ is untenable, and conclude that p must be true. But constructive mathematics asks for *explicit* procedures for constructing each object that is assumed to exist, and explicit constructions behind any mathematical statement. Similarly, we have mentioned before (Section 2.1) Cauchy's proof of the fundamental theorem of algebra, as well as many proofs in real analysis that invoke the least upper bound. All of these proofs are invalid for a constructivist, and several mathematicians have tried to save the theorems by finding constructivist proofs for them. For instance, both Weyl and Kneser worked on constructivist proofs for the fundamental theorem of algebra.

It is easy to give instances of the use of PEM that a constructivist will not accept: one just has to apply it to any unsolved mathematical problem. For example, Catalan's constant is the number

$$K = \sum_{n=0}^{\infty} \frac{(-1)^n}{(2n+1)^2}.$$

It is not known whether K is transcendental, so if p is the statement "Catalan's constant is transcendental," then a constructivist will not accept that p is either true or false.

This may seem odd, or even obviously wrong, until one realizes that constructivists have a different view about what truth *is*. For a constructivist, to say that a proposition is true simply *means* that we can prove it in accordance with the stringent methods that we are discussing; to say that it is false *means* that we can actually exhibit a counterexample to it. Since there is no reason to suppose that every existence statement either has a strict constructivist proof or an explicit counterexample, there is no reason to believe PEM (with this notion of truth). Thus, in order to establish the existence of a natural number with a certain property, a proof by *reductio ad absurdum* is not enough. Existence must be shown by explicit determination or construction if you want to persuade a constructivist.

Notice also how this viewpoint implies that mathematics is not timeless or ahistorical. It was only in 1882 that Lindemann proved that π is a TRANSCENDENTAL NUMBER. Since that date, it has been possible to assign a truth value to statements that were neither true nor false before, according to intuitionists. This may seem paradoxical, but it was just right for Brouwer, since in his view mathematical objects are mental constructions and he rejected as "metaphysics" the assumption that they have an independent existence.

In 1918, Brouwer replaced the sets of Cantor and Zermelo by constructive counterparts, which he would later call "spreads" and "species." A *species* is basically a set that has been defined by a characteristic property, but with the proviso that *every* element has been previously and independently defined by an explicit construction. In particular, the definition of any given species will be strictly predicative.

The concept of a *spread* is particularly characteristic of intuitionism, and it forms the basis for Brouwer's definition of the continuum. It is an attempt to avoid idealization and do justice to the temporal nature of mathematical constructions. Suppose, for example, that we wish to define a sequence of rational numbers that gives better and better approximations to the square root of 2. In classical analysis, one conceives of such sequences as existing in their entirety, but Brouwer defined a notion that he called a *choice sequence*, which pays more attention to how they might be produced. One way to produce them is to give a rule, such as the recurrence relation $x_{n+1} = (x_n^2 + 2)/2x_n$ (and the initial condition $x_1 = 2$). But another is to make less rigidly determined choices that obey certain constraints: for instance, one might insist that x_n has denominator n and that x_n^2 differs from 2 by at most $100/n$, which does not determine x_n uniquely but does ensure that the sequence produces better and better approximations to $\sqrt{2}$.

A choice sequence is therefore not required to be completely specified from the outset, and it can involve choices that are freely made by the mathematician at different moments in time. Both these features make choice sequences very different from the sequences of classical analysis: it has been said that intuitionist mathematics is "mathematics in the making." By contrast, classical mathematics is marked by a kind of timeless objectivity, since its objects are assumed to be fully determined in themselves and independent of the thinking processes of mathematicians.

A spread has choice sequences as its elements—it is something like a law that regulates how the sequences are constructed.[6] For instance, one could take a spread that

6. More precisely, a spread is defined by means of two laws; see Heyting (1956), or more recently van Atten (2003), for further details on this and other points. One can picture a spread as a subtree of the universal tree of natural numbers (consisting of all finite sequences of natural numbers), together with an assignment of previously available mathematical objects to the nodes. One law of the spread determines nodes in the tree, the other maps them to objects.

consisted of all choice sequences that began in some particular way, and such a spread would represent a segment—in general, spreads do not represent isolated elements, but continuous domains. By using spreads whose elements satisfy the Cauchy condition, Brouwer offered a new mathematical conception of the *continuum*: rather than being made up of points (or real numbers) with some previous Platonic existence, it was more genuinely "continuous." Interestingly, this view is reminiscent of Aristotle, who, 23 centuries earlier, had emphasized the priority of the continuum and rejected the idea that an extended continuum can be made up of unextended points.

The next stage in Brouwer's redevelopment of analysis was to analyze the idea of a function. Brouwer defined a function to be an assignment of values to the elements of a spread. However, because of the nature of spreads, this assignment had to be wholly dependent on an initial segment of the choice sequence in order to be constructively admissible. This threw up a big surprise: all functions that are everywhere defined are continuous (and even uniformly continuous). What, you might wonder, about the function f where $f(x) = 0$ when $x < 0$ and $f(x) = 1$ when $x \geqslant 0$? For Brouwer, this is not a well-defined function, and the underlying reason for this is that one can determine spreads for which we do not know (and may never know) whether they are positive, zero, or negative. For instance, one could let x_n be 1 if all the even numbers between 4 and $2n$ are sums of two primes, and -1 otherwise.

The rejection of PEM has the effect that intuitionistic negation differs in meaning from classical negation. Thus, intuitionistic arithmetic is also different from classical arithmetic. Nevertheless, in 1933 Gödel and Gentzen were able to show that the Dedekind–Peano axioms of arithmetic are consistent *relative to* formalized intuitionistic arithmetic. (That is, they were able to establish a correspondence between the sentences of both formal systems, such that a contradiction in classical arithmetic yields a contradiction in its intuitionistic counterpart; thus, if the latter is consistent, the former must be as well.) This was a small triumph for the Hilbertians, though corresponding proofs for systems of analysis or set theory have never been found.

Initially there had been hopes that the development of intuitionism would end in a simple and elegant presentation of pure mathematics. However, as Brouwer's reconstruction developed in the 1920s, it became more and more clear that intuitionistic analysis was extremely complicated and foreign. Brouwer was not worried, for, as he would say in 1933, "the spheres of truth are less transparent than those of illusion." But Weyl, although convinced that Brouwer had delineated the domain of mathematical intuition in a completely satisfactory way, remarked (1925, p. 534): "the mathematician watches with pain the largest part of his towering theories dissolve into mist before his eyes." Weyl seems to have abandoned intuitionism shortly thereafter. Fortunately, there was an alternative approach that suggested another way of rehabilitating classical mathematics.

3.2 Hilbert's Program

This alternative approach was, of course, Hilbert's program, which promised, in the memorable phrasing of 1928, "to eliminate from the world once and for all the sceptical doubts" as to acceptability of the classical theories of mathematics. The new perspective, which he started to develop in 1904, relied heavily on formal logic and a combinatorial study of the formulas that are provable from given formulas (the axioms). With modern logic, proofs are turned into formal computations that can be checked mechanically, so that the process is purely constructivistic.

In the light of our previous discussion (Section 1), it is interesting that the new project was to employ Kroneckerian means for a justification of modern, anti-Kroneckerian methodology. Hilbert's aim was to show that it is impossible to prove a contradictory formula from the axioms. Once this had been shown combinatorially or constructively (or, as Hilbert also said, finitarily), the argument can be regarded as a justification of the axiom system—even if we read the axioms as talking about non-Kroneckerian objects like the real numbers or transfinite sets.

Still, Hilbert's ideas at the time were marred by a deficient understanding of logical theory.[7] It was only in 1917/18 that Hilbert returned to this topic, now with a refined understanding of logical theory and a greater awareness of the considerable technical difficulties of his project. Other mathematicians played very significant parts in promoting this better understanding. By 1921, helped by his assistant Bernays, Hilbert had arrived at a very refined conception of the formalization of mathematics, and had perceived the need for a deeper and more careful probing into the logical structure of mathematical proofs and theories. His program was first clearly formulated in a talk at Leipzig late in 1922.

Here we will describe the mature form of Hilbert's program, as it was presented for instance in the 1925 paper "On the infinite" (see van Heijenoort 1967). The main goal

7. The logic he presented in 1905 lagged far behind Frege's system of 1879 or Peano's of the 1890s. We do not enter into the development of logical theory in this period (see, for example, Moore 1998; Mancosu et al., forthcoming).

was to establish, by means of syntactic consistency proofs, the logical acceptability of the principles and modes of inference of modern mathematics. Axiomatics, logic, and formalization made it possible to study mathematical theories from a purely mathematical standpoint (hence the name *metamathematics*), and Hilbert hoped to establish the noncontradictoriness of the theories by employing very weak means. In particular, Hilbert hoped to answer all of the criticisms of Weyl and Brouwer, and thereby justify set theory, the classical theory of real numbers, classical analysis, and of course classical logic with its PEM (the basis for indirect proofs by *reductio ad absurdum*).

The whole point of Hilbert's approach was to make mathematical theories fully precise, so that it would become possible to obtain precise results about properties of the theory. The following steps are indispensable for the completion of such a program.

(1) Finding suitable axioms and primitive concepts for a mathematical theory T, such as that of the real numbers.
(2) Finding axioms and inference rules for classical logic, which makes the passage from given propositions to new propositions a purely syntactic, formal procedure.
(3) Formalizing T by means of the formal logical calculus, so that propositions of T are just strings of symbols, and proofs are sequences of such strings that obey the formal rules of inference.
(4) A *finitary* study of the formalized proofs of T that shows that it is impossible for a string of symbols that expresses a contradiction to be the last line of a proof.

In fact, steps (2) and (3) can be solved with rather simple systems formalized in first-order logic, like those studied in any introduction to mathematical logic, such as DEDEKIND–PEANO ARITHMETIC or Zermelo–Fraenkel set theory. It turns out that first-order logic is enough for codifying mathematical proofs, but, interestingly, this realization came rather late—after Gödel's theorems.

Hilbert's main insight was that, when theories are formalized, any proof becomes a *finite* combinatorial object: it is just an array of strings of symbols complying with the formal rules of the system. As Bernays said, this was like "projecting" the deductive structure of a theory T into the number-theoretic domain, and it became possible to express in this domain the consistency of T. These realizations raised hopes that a finitary study of formalized proofs would suffice to establish the consistency of the theory, that is, to prove the sentence expressing the consistency of T. But this hope, not warranted by the previous insights, turned out to be wrong.[8]

Also, a crucial presupposition of the program was that not only the logical calculus but also each of the axiomatic systems would be *complete*. Roughly speaking, this means that they would be sufficiently powerful to allow the derivation of all the relevant results.[9] This assumption turned out to be wrong for systems that contain (primitive recursive) arithmetic, as Gödel showed.

It remains to explain what Hilbert meant by *finitism*. This is one of the points in which his program of the 1920s adopted to some extent the principles of intuitionists such as Poincaré and Brouwer and deviated strongly from the ideas Hilbert himself had considered in 1900. The key idea is that, contrary to the views of logicists like Frege and Dedekind, logic and pure thought require something that is given "intuitively" in our immediate experience: the signs and formulas.

In 1905, Poincaré had put forth the view that a formal consistency proof for arithmetic would be circular, as such a demonstration would have to proceed by induction on the length of formulas and proofs, and thus would rely on the same axiom of induction that it was supposed to establish. Hilbert replied in the 1920s that the form of induction required at the metamathematical level is much weaker than full arithmetical induction, and that this weak form is grounded on the finitary consideration of signs that he took to be intuitively given. Finitary mathematics was not in need of any further justification or reduction.

Hilbert's program proceeded gradually by studying weak theories at first and proceeding to progressively stronger ones. The *metatheory* of a formal system studies properties such as consistency, completeness, and some others ("completeness" in the logical sense means that all true or *valid* formulas that can be represented in the calculus are formally deducible in it). Propositional logic was quickly proved to be consistent and complete. First-order logic, also known as *predicate logic*, was proved complete by Gödel in his dissertation of 1929. For all of the 1920s, the attention of Hilbert and coworkers was set on elementary arithmetic and its subsystems; once this had been settled, the project was to move on to the much more difficult, but crucial, cases of the theory of real numbers and set theory. Ackermann and von Neumann were able to establish consistency results for certain subsystems of arithmetic, but

8. For further details and precisions, see, for example, Sieg (1999).
9. The notion of "relevant result" should of course be made precise: doing so leads to the notion either of syntactic completeness or of semantic completeness.

between 1928 and 1930 Hilbert was convinced that the consistency of arithmetic had already been established. Then came the severe blow of Gödel's incompleteness results (see Section 4).

The name "formalism," as a description of this program, came from the fact that Hilbert's *method* consisted in formalizing each mathematical theory, and formally studying its proof structure. However, this name is rather one-sided and even confusing, especially because it is usually contrasted with intuitionism, a full-blown *philosophy* of mathematics. Like most mathematicians, Hilbert never viewed mathematics as a mere game played with formulas. Indeed, he often emphasized the meaningfulness of (informal) mathematical statements and the depth of conceptual content expressed in them.[10]

3.3 Personal Disputes

The crisis was unfolding not just at an intellectual level but also at a personal level. One should perhaps tell this story as a tragedy, in which the personalities of the main figures and the successive events made the final result quite inescapable.

Hilbert and Brouwer were very different personalities, though they were both extremely wilful and clever men. Brouwer's worldview was idealistic and tended to solipsism. He had an artistic temperament and an eccentric private life. He despised the modern world, looking to the inner life of the self as the only way out (at least in principle, though not always in practice). He preferred to work in isolation, although he had good friends in the mathematical community, especially in the international group of topologists that gathered around him. Hilbert was typically modernist in his views and attitudes; full of optimism and rationalism, he was ready to lead his university, his country, and the international community into a new world. He was absolutely for community work, and felt happy to join Klein's schemes for institutional development and power.

As a consequence of World War I, Germans in the early 1920s were not allowed to attend the International Congresses of Mathematicians. When the opportunity finally arose in 1928, Hilbert was eager to seize on it, but Brouwer was furious because of restrictions that were still imposed on the German delegation and sent a circular letter in order to convince other mathematicians. Their viewpoints were widely known and led to a clash between the two men. On another level, Hilbert had made important concessions to his opponents in the 1920s, hoping that he would succeed in his project of finding a consistency proof. Brouwer emphasized these concessions, accusing him of failing to recognize authorship, and demanded new concessions.[11] Hilbert must have felt insulted and perhaps even threatened by a man whom he regarded as perhaps the greatest mathematician of the younger generation.

The last straw came with an episode in 1928. Brouwer had since 1915 been a member of the editorial board of *Mathematische Annalen*, the most prestigious mathematics journal at the time, of which Hilbert had been the main editor since 1902. Ill with "pernicious anemia," and apparently thinking that he was close to the end, Hilbert feared for the future of his journal and decided it was imperative to remove Brouwer from the editorial board. When he wrote to other members of the board explaining his scheme, which he was already carrying out, Einstein replied saying that his proposal was unwise and that he wanted to have nothing to do with it. Other members, however, did not wish to upset the old and admired Hilbert. Finally, a dubious procedure was adopted, where the whole board was dissolved and created anew. Brouwer was greatly disturbed by this action, and as a result of it the journal lost Einstein and Carathéorody, who had previously been main editors.

After that, Brouwer ceased to publish for some years, leaving some book plans unfinished. With his disappearance from the scene, and with the gradual disappearance of previous political turbulences, the feelings of "crisis" began to fade away (see Hesseling 2003). Hilbert did not intervene much in the subsequent debates and foundational developments.

4 Gödel and the Aftermath

It was not only the *Annalen* war that Hilbert won: the mathematical community as a whole continued to work in the style of modern mathematics. And yet his program suffered a profound blow with the publication of Gödel's famous 1931 article in the *Monatshefte für Mathematik und Physik*. An extremely ingenious development of metamathematical methods—the arithmetization of metamathematics—allowed Gödel to prove that systems like axiomatic set theory or Dedekind–Peano arithmetic are incomplete. That is, there exist propositions P formulated strictly in the language of the system such that neither P nor $\neg P$ is formally provable in the system. (See Gödel's theorems for a further discussion.)

10. This is very explicit, for example, in the lectures of 1919/20 edited by Rowe (1992), and also in the 1930 paper that bears exactly the same title (see *Gesammelte Abhandlungen*, Volume 3).

11. See his 'Intuitionistic reflections on formalism' of 1928 (in Mancosu 1998).

This theorem already presented a deep problem for Hilbert's endeavor, as it shows that formal proof cannot even capture arithmetical truth. But there was more. A close look at Gödel's arguments made it clear that this first metamathematical proof could itself be formalized, which led to "Gödel's second theorem"—that it is impossible to establish the consistency of the systems mentioned above by any proof that can be codified *within* them. Gödel's arithmetization of metamathematics makes it possible to build a sentence, in the language of formal arithmetic, that expresses the consistency of this same formal system. And this sentence turns out to be among those that are unprovable.[12] To express it contrapositively, a finitary formal proof (codifiable in the system of formal arithmetic) of the impossibility of proving $1 = 0$ could be transformed into a contradiction of the system! Thus, if the system is indeed consistent (as most mathematicians are convinced it is), there is no such finitary proof.

According to what Gödel called at the time "the von Neumann conjecture" (namely, that if there is a finitary proof of consistency, then it can be formalized and codified within elementary arithmetic), the second theorem implies the failure of Hilbert's program (see Mancosu (1999, p. 38) and, for the reception, Dawson (1997, pp. 68 ff)). One should emphasize that Gödel's negative results are purely constructivistic and even finitistic, valid for all parties in the foundational debate. They were difficult to digest, but in the end they led to a reestablishment of the basic terms for foundational studies.

Mathematical logic and foundational studies continued to develop brilliantly with Gentzen-style proof theory, with the rise of MODEL THEORY, etc.—all of which had their roots in the foundational studies of the first third of the twentieth century. Although the Zermelo–Fraenkel axioms suffice for giving a rigorous foundation to most of today's mathematics, and have a rather convincing intuitive justification in terms of the "iterative" conception of sets,[13] there is a general feeling that foundational studies, instead of achieving their ambitious goal, "found themselves attracted into the whirl of mathematical activity, and are now enjoying full voting rights in the mathematical senate."[14]

However, this impression is somewhat superficial. Proof theory has developed, leading to noteworthy reductions of classical theories to systems that can be regarded as constructive. A striking example is that analysis can be formalized in *conservative extensions* of arithmetic: that is, in systems that extend the language of arithmetic while including all theorems of arithmetic, but which are "conservative" in the sense that they have no new consequences in the language of arithmetic. Some parts of analysis can even be developed in conservative extensions of primitive recursive arithmetic (see Feferman 1998). This raises questions about the philosophical bases on which the admissibility of the relevant constructive theories can be founded. But for these systems the question is far less simple than it was for Hilbert's finitary mathematics; it seems fair to say that no general consensus has yet been reached.

Whatever its roots and justification may be, mathematics is a human activity. This truism is clear from the subsequent development of our story. The mathematical community refused to abandon "classical" ideas and methods; the constructivist "revolution" was aborted. In spite of its failure, formalism established itself in practice as the avowed ideology of twentieth-century mathematicians. Some have remarked that formalism was less a real faith than a Sunday refuge for those who spent their weekdays working on mathematical objects as something very real. The Platonism of working days was only abandoned, as a BOURBAKI member said, when a ready-made reply was needed to unwelcome philosophical questions concerning mathematical knowledge.

One should note that formalism suited very well the needs of a self-conscious, autonomous community of research mathematicians. It granted them full freedom to choose their topics and to employ modern methods to explore them. However, to reflective mathematical minds it has long been clear that it is not the answer. Epistemological questions about mathematical knowledge have not been "eliminated from the world"; philosophers, historians, cognitive scientists, and others keep looking for more adequate ways of understanding its content and development. Needless to say, this does not threaten the autonomy of mathematical researchers—if autonomy is to be a concern, perhaps we should worry instead about the pressures exerted on us by the market and other powers.

Both (semi-)constructivism and modern mathematics have continued to develop. On the one hand, one may

12. For further details, see, for example, Smullyan (2001), van Heijenoort (1967), and good introductions to mathematical logic. Both theorems were carefully proved in Hilbert and Bernays (1934/39). Bad expositions and faulty interpretations of Gödel's results abound.

13. The basic idea is to view the set-theoretic universe as a product of iterating the following operation: one starts with a basic domain V_0 (possibly finite or even equal to $\emptyset$) and forms all possible *sets of* elements in the domain; this gives a new domain V_1, and one iterates forming sets of $V_0 \cup V_1$, and so on (to infinity and beyond!). This produces an open-ended set-theoretic universe, masterfully described by Zermelo (1930). On the iterative conception, see, for example, the last papers in Bernacerraf and Putnam (1983).

14. To use the words of Gian-Carlo Rota in an essay of 1973.

mention the work of Harvey Friedman, among others; on the other, there is the impressive expansion of CATEGORY THEORY. The contrast between modern and constructivist mathematics has simply been consolidated, though in a very unbalanced way: some 99% of practising mathematicians are "modern." (But do statistics matter when it comes to the correct methods for mathematics?) In 1905, commenting on the French debate, HADAMARD wrote that "there are two conceptions of mathematics, two mentalities, in evidence." It has now come to be recognized that there is value in both approaches: they complement each other and can coexist peacefully. In particular, interest in effective methods, algorithms, and computational mathematics has grown markedly in recent decades—and all of these are closer to the constructivist tradition.

The foundational debate left a rich legacy of ideas and results, key insights and developments, including the formulation of axiomatic set theories and the rise of intuitionism. One of the most important of these developments was the emergence of modern mathematical logic as a refinement of axiomatics, which led to the theories of recursion and computability in around 1936 (see Section **??** of ALGORITHMS). In the process, our understanding of the characteristics, possibilities, and limitations of formal systems was hugely clarified.

One of the hottest issues throughout the whole debate, and probably its main source, was the question of how to understand the continuum. The reader may recall the contrast between the set-theoretic understanding of the real numbers and Brouwer's approach, which rejected the idea that the continuum is "built of" points. That this is a labyrinthine question was further established by results on Cantor's continuum hypothesis (CH), which postulates that the cardinality of the set of real numbers is $\aleph_1$, the second transfinite cardinal, or equivalently that every infinite subset of $\mathbb{R}$ must biject with either $\mathbb{N}$ or with $\mathbb{R}$ itself. Gödel proved in 1939 that CH is consistent with axiomatic set theory, but Paul J. Cohen proved in 1963 that it is independent of its axioms (i.e., Cohen proved that the negation of CH is consistent with axiomatic set theory—see THE INDEPENDENCE OF THE CONTINUUM HYPOTHESIS). The problem is still alive, with a few mathematicians proposing alternative approaches to the continuum and others trying to find new and convincing set-theoretic principles that will settle Cantor's question (see Woodin 2001).

The foundational debate has also contributed in a definitive way to clarifying the peculiar style and methodology of modern mathematics, especially the so-called "Platonism" or "existential character" of modern mathematics (see the classic 1935 paper of Bernays in Benacerraf and Putnam (1983)), by which is meant (here at least) a methodological trait rather than any supposed implications of metaphysical existence. Modern mathematics investigates structures by considering their elements as given independently of human (or mechanical) capabilities of effective definition and construction. This may seem surprising, but perhaps this trait can be explained by broader characteristics of scientific thought and the role played by mathematical structures in the modelling of scientific phenomena.

In the end, the debate made it clear that mathematics and its modern methods are still surrounded by important philosophical problems. When a sizable amount of mathematical knowledge can be taken for granted, theorems can be established and problems can be solved with the certainty and clarity for which mathematics is celebrated. But when it comes to laying out the bare beginnings, philosophical issues cannot be avoided. The reader of these pages may have felt this at several places, especially in the discussion of intuitionism, but also in the basic ideas behind Hilbert's program, and of course in the problem of the relationship between formal mathematics and its informal counterpart, a problem that is brought into sharp focus by Gödel's theorems.

I thank Mark van Atten, Jeremy Gray, Paolo Mancosu, José F. Ruiz, Wilfried Sieg, and the editors for their helpful comments on a previous version of this paper.

Further Reading

It is impossible to list here all the relevant articles by Bernays, Brouwer, Cantor, Dedekind, Gödel, Hilbert, Kronecker, von Neumann, Poincaré, Russell, Weyl, Zermelo, etc. The reader can easily find them in the source books by van Heijenoort (1967), Heinzmann (1986), Ewald (1996), and Mancosu (1998).

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Seminar paper about the foundations of mathematics and the foundational crisis of mathematics ("Grundlagenkrise") https://www21.in.tum.de/teaching/prooâ€¦ mathematics seminar seminar-paper formalism intuitionism logicism philosophy math maths tum logic. 138 commits.Â This work was made as part of the seminar "Formal Proof in Mathematics and Computer Science" offered by the Chair for Logic and Verification at the Technical University of Munich (TUM) 2017 https://www21.in.tum.de/teaching/proof21/SS17/. This work is licensed under CC BY 3.0. Contact. In Foundations of Set Theory by Fraenkel, Bar-Hillel, and Levy (1973), the authors argue that there have been three distinct periods of crisis in the foundations of mathematics. The first was undergone by the Ancient Greeks: [...] two discoveries were made that were extremely paradoxical: the first was that not all geometrical entities of the same kind were commensurable with each other, so that, for instance, the diagonal of a given square could not be measured by an aliquot part of its side (in modern terms, that the square root of 2 is not a rational number).