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Infinity in Mathematics: Is Cantor Necessary?

Dedicated to the memory of my friend
and colleague Jean van Heijenoort

Infinity is up on trial . . .
(Bob Dylan, *Visions of Johanna*)

Introduction

Since the rise of abstract mathematics in Greek times, mathematicians have had to grapple with the problems of infinity in many guises. When mathematics became an integral part of physical science it could be used to formulate precise answers to age-old questions: Is space infinite? Did time have a beginning? Will it have an end? Modern cosmological theories now marshal considerable physical evidence to support the finitude of space and time. But whether or not (or how) infinity is manifested in the physical universe, mathematics requires at its base the use of various infinite arithmetical and geometrical structures. Without these no coherent system of mathematics is possible, and since mathematics is essential for the formulation of physical theories, there is also no science without these uses of the infinite at the base.

Beginning in the 1870s, Georg Cantor came to realize that one must distinguish different *orders* or *sizes of infinity* of the underlying sets of objects in these basic mathematical structures. As he continued to work out

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the implications of his ideas, he was led to the introduction of a *series of transfinite cardinal numbers* for measuring these sizes. Many mathematicians of Cantor's time were disturbed by his work, partly due to the novelty of his concepts and partly due to the uncertain grounds on which his computations and arguments with the scale of cardinals rested. But some reacted in direct opposition to Cantor's work, for his reintroduction of the "actual infinite" into mathematics (ironically, after that seemed to have been finally eliminated from analysis by the previous foundational work of the nineteenth century). One of Cantor's most vigorous and severe critics was his former teacher Leopold Kronecker, who would admit only "potentially infinite" sets to mathematics and, indeed, only those reducible to the natural number sequence 0, 1, 2, 3, ...

Worries about the Cantorian approach were compounded when, around the turn of the century, *paradoxes* appeared in the theory of sets by taking its ideas to what appeared to be their logical conclusion. The most famous and simplest of these contradictions was due to Bertrand Russell, but earlier ones had already been discovered for Cantor's transfinite numbers by Cesare Burali-Forti[1] and even by Cantor himself. While these paradoxes did not worry Cantor, the vague distinctions between the transfinite and the "absolute" infinite that he made in order to avoid them could not at first be made precise. But the contradiction plagued Russell and he attempted many solutions to escape them; that is, he sought precise systematic grounds for accepting substantial portions of Cantor's theory while excluding the paradoxes. The means at which Russell finally arrived is called the *theory of types*, and though it proved to be very cumbersome as a framework for set theory, it did restore a measure of confidence in Cantor's work. Independently, Ernst Zermelo introduced an *axiomatic theory of sets*, which featured a simple device for limiting the size of sets so as to avoid the paradoxes while providing a very flexible and ready means for the precise development of a considerable portion of Cantor's theory of higher infinities. Extensions of Zermelo's axiom system, which are described below, allow one to develop Cantorian theory in full while avoiding all known paradoxical constructions.

Parallel to the work by logicians providing an axiomatic foundation for higher set theory, Cantor's ideas were put to use more and more in mathematics, so that these days they are largely taken for granted and permeate the whole of its fabric. But there are still a number of thinkers on the subject who, in continuation of Kronecker's attack, object to the panoply of transfinite set theory in mathematics. The reasons for doing so are no longer the paradoxes, which have apparently been blocked in an effective way by means of the axiomatic theories devised by Zermelo and his succes-

[1] Actually, Burali-Forti's paradox was only implicitly contained in his work. Incidentally, Russell's paradox was found independently by Zermelo in 1902. While the work was not published, Zermelo claimed this earlier discovery in his 1908 paper; that has subsequently been confirmed by a variety of evidence in Rang and Thomas (1981).

sors. Rather, the objections to the Cantorian ideas reside in fundamentally differing views as to the nature of mathematics and the objects (numbers, points, sets, functions, ...) with which it deals. In particular, these opposing points of view reject the assumption of the actual infinite (at least in its nondenumerable forms). Following this up, alternative schemes for the foundations of mathematics have been pursued with the aim to demonstrate that everyday mathematics can be accounted for in a direct and straightforward way on philosophically acceptable non-Cantorian grounds. While genuine progress has been made along these lines, the successes obtained are not widely known and the alternative approaches have attracted relatively few adherents among working mathematicians. The general impression is that non-Cantorian mathematics is too restrictive for the needs of mathematical practice, regardless of the merits of the guiding non-Cantorian philosophies.

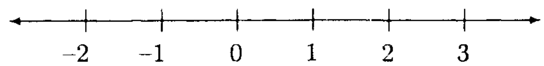
Some logicians would now go farther to bolster this impression by giving it a theoretical underpinning; their aim is to demonstrate that Cantor's higher infinities are in fact *necessary* for mathematics, even for its most finitary parts. The results that have been obtained in this direction do, at first sight, appear to justify such a reading. Nevertheless, it is argued below [now in chapter 12] that *the necessary use of higher set theory in the mathematics of the finite has yet to be established.* Furthermore, a case can be made that *higher set theory is dispensable in scientifically applicable mathematics*, that is, in that part of everyday mathematics which finds its applications in the other sciences. Put in other terms: the *actual infinite is not required for the mathematics of the physical world.* The reasons for this depend on other recent developments in mathematical logic, the description of which is the final aim of this essay [now chapter 12; cf. also chapter 14].

In order to explain the objections to Cantor's ideas in mathematics that lead one to search for viable alternatives, one must first provide some understanding of their nature and use. This will be done here more or less historically though necessarily in outline; we start with a rather innocent looking problem about the existence of certain special kinds of numbers.[2]

From Transcendental Numbers to Transfinite Numbers

There are two basic number systems (having ancient origins), the set **N** of *natural numbers* $0, 1, 2, 3, \ldots$ used for counting, and the set **R** of *real numbers* use for measuring. The latter represent positions of points on a two-way infinite straight line, relative to any initial point 0 (the "origin") and any unit of measurement 1. **R** is pictured as follows:

[2]This is not where Cantor himself started, though he came to it soon enough. For a good detailed introduction to the history of the development of Cantor's ideas, see Grattan-Guinness (1980), chapters 5, 6 (by J. W. Dauben and R. Bunn, resp.). For a deeper pursuit I would recommend most highly the books of Moore (1982) and Hallett (1984). [Cf. also Lavine (1994).]



$\mathbf{N}$ can thus be identified with a subset of $\mathbf{R}$. Other subsets of use are $\mathbf{Z} = \{\ldots, -3, -2, -1, 0, 1, 2, 3, \ldots\}$ (the set of all *integers*) and $\mathbf{Q} = \{n/m : n, m \in \mathbf{Z} \text{ and } m \neq 0\}$, the set of *rational numbers*, consisting of all *quotients* or *ratios* of integers with nonzero denominators. We here write "$x \in S$" for the relation of membership of an object x to a set S, and "$\{x : P(x)\}$" for the set of all objects x satisfying a determinate property $P(x)$. If x does not belong to S we write "$x \notin S$." Finite sets S can be denoted directly by a listing of their elements as $S = \{a_0, a_1, a_2, \ldots, a_n\}$. This notation is extended to certain infinite sets such as $\mathbf{N} = \{0, 1, 2, \ldots, n, \ldots\}$ and $\mathbf{Z}$ (as above), when we have a complete survey of their elements.

If S_1 and S_2 are sets, then S_1 is a *subset* of S_2, in symbols $S_1 \subseteq S_2$, if for every $x \in S_1$ we have $x \in S_2$; $S_2 - S_1 = \{x : x \in S_2 \text{ and } x \notin S_1\}$ then denotes the *difference* of these two sets. $S_2 - S_1$ might be empty, when $S_1 = S_2$; the *empty set* is denoted by $\emptyset$.

The set $\mathbf{Q}$ is densely dispersed throughout $\mathbf{R}$ and cannot be distinguished from $\mathbf{R}$ by a simple picture as above. A basic realization from Greek geometry was the existence of *irrational magnitudes*, that is, elements of $\mathbf{R} - \mathbf{Q}$. For, Pythagoras' law giving the hypotenuse c of a right triangle in terms of its legs a, b by $c^2 = a^2 + b^2$, or equivalently $c = \sqrt{a^2 + b^2}$, leads directly to quantities such as $\sqrt{2} = \sqrt{1^2 + 1^2}$ and $\sqrt{5} = \sqrt{1^2 + 2^2}$, which are easily proved to be irrational. Other kinds of irrational numbers also arise naturally in geometry, for example, $\sqrt[3]{2}$ in the classical problem of the duplication of the cube (that is, construction of a cube with double the volume of a given one), and $\pi (= 3.14159\ldots)$, the ratio of the circumference of a circle to its diameter. However, the proofs of the irrationality of numbers like π only came much later.

Other irrational numbers arise from the solution of algebraic equations; for example, one solution of $x^4 - 7 = 0$ is $x = \sqrt[4]{7}$ and of $x^6 - x^3 - 1 = 0$ is $x = \sqrt[3]{(1 + \sqrt{5})/2}$. Some equations, such as $x^2 + 1 = 0$, have no solutions in real numbers, although we can treat their solutions in the extension of the real number system by the *imaginary numbers* such as $\sqrt{-1}$; however, those will not concern us here. A real number is called *algebraic* if it is the solution x of a nontrivial polynomial equation $p(x) = 0$ with integer coefficients; that is, $p(x) = k_n x^n + k_{n-1} x^{n-1} + \ldots + k_1 x + k_0$ with $n > 0$ and $k_n \neq 0$, and each $k_i \in \mathbf{Z}$. We use "$\mathbf{A}$" to denote the set of all (real) algebraic numbers. Thus $\mathbf{Q} \subseteq \mathbf{A}$ and $\mathbf{A}$ contains all the irrational numbers shown above, except possibly π. However, it is natural to suspect that $\pi \notin \mathbf{A}$, since there is no known polynomial equation of which it is a root. This was in fact first conjectured by Legendre in the eighteenth century, but a proof did not come until a century later.

A number is called *transcendental* if it is in the set $\mathbf{T} = \mathbf{R} - \mathbf{A}$. Another specific number which, like π, is ubiquitous in mathematics and was conjectured to be transcendental is the base e $(= 2.71828\ldots)$ of "natural" logarithms. The first proof that there *exist* any transcendental numbers *at all*, that is, that $\mathbf{T} \neq \emptyset$, was given by Liouville in 1844. His method was to find a property $P(x)$ which applies to all algebraic numbers x and which says something (technical) about how well x can be approximated by rational numbers. Liouville then cooked up a real number ℓ which does not satisfy the property P, so ℓ must be transcendental. Infinitely many other transcendental numbers can also be produced in this way, but Liouville's method did not help answer the specific questions as to whether e and π are transcendental. Those results were not obtained until somewhat later, by Hermite (in 1873) and Lindemann (in 1882), respectively, using rather special methods. In the meantime, Cantor published in 1874 a new and extremely simple but striking argument to prove the existence of transcendental numbers. Cantor's *result* in this respect was no better than Liouville's, but the methodology of his proof turned out to be one of the main starting points for his novel conception of infinity in mathematics.

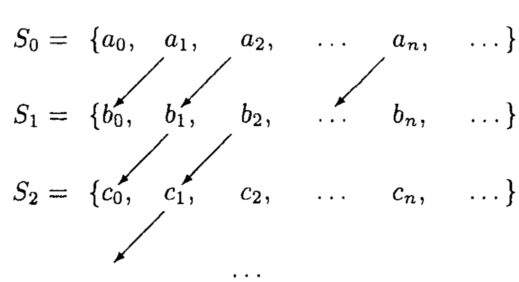
First of all, Cantor defined two sets S_1 and S_2 to be *equinumerous* if their elements can be placed in one-to-one correspondence with each other; symbolically this is indicated by $S_1 \sim S_2$. A set S is *finite* if it is equivalent to some initial segment of $\mathbf{N}$, possibly empty, that is, $S \sim \{0, \ldots, n-1\}$, where $n \geq 0$. A set S is called *denumerable* if S is finite or $S \sim \mathbf{N}$. Every nonempty denumerable set S can be listed as $S = \{a_0, a_1, \ldots, a_n, \ldots\}$, possibly with repetitions, and conversely. From this follows directly several basic facts:

(1) a denumerable union of denumerable sets is denumerable;

(2) the set $\mathbf{Q}$ is denumerable;

(3) the set $\mathbf{A}$ is denumerable.

In (1) we are considering sets $S_0, S_1, \ldots, S_n, \ldots$, each of which is denumerable and may be assumed nonempty; these are listed as



The arrows have been added to show that the union S can be listed following the indicated arrows as

$$S = \{a_0, a_1, b_0, a_2, b_1, c_0, \dots\}.$$

Now, for (2), take S_n to be the set of all multiples m/n for $n \neq 0$ and m in $\mathbf{Z}$; each S_n is denumerable and their union is $\mathbf{Q}$, so (2) follows. To prove (3), one shows first of all that all equations of the form $k_m x^m + \dots + k_1 x + k_0 = 0$ with integer coefficients and $k_m \neq 0$ can be enumerated. If $p_n(x) = 0$ is the n^{th} such equation, take S_n to be the set of its solutions. This set is finite (possibly empty), hence certainly denumerable. But the set $\mathbf{A}$ of algebraic numbers is the union of these S_n's, so it also is denumerable; that is, (3) holds.

Now, in contrast, Cantor showed that

(4) $\mathbf{R}$ is non-denumerable.

In other words there is no way to list $\mathbf{R}$ as a simple sequence of real numbers, $\{x_0, x_1, x_2 \dots\}$. Cantor's first proof of (4) made use of special properties of $\mathbf{R}$. Later he gave a simpler proof that could be generalized to other sets; this used his famous *diagonal argument.* It is sufficient to show that the set of reals x between 0 and 1 cannot be enumerated. Indeed, given any enumeration $\{x_0, x_1, x_2 \dots\}$ of a subset of $\mathbf{R}$, we shall construct a number x that is not in the enumeration. First write each member of $\{x_0, x_1, x_2 \dots\}$ as an infinite decimal:

$$\begin{aligned} x_0 &= 0.a_1a_2a_3 \dots a_n \dots \\ x_1 &= 0.b_1b_2b_3 \dots b_n \dots \\ x_2 &= 0.c_1c_2c_3 \dots c_n \dots \\ &\quad\dots \end{aligned}$$

Now form $x = 0.k_1k_2k_3 \dots k_n \dots$ not in $\{x_0, x_1, x_2 \dots\}$ by choosing $k_1 \neq a_1, k_2 \neq b_2, k_3 \neq c_3$, etc. For example, take $k_1 = 0$ if $a_1 \neq 0$ and $k_1 = 1$ if $a_1 = 0$, etc. This proves (4); it then follows immediately from (3) that $\mathbf{A} \neq \mathbf{R}$. Hence $\mathbf{T} = \mathbf{R} - \mathbf{A}$ must be nonempty, and thus the existence of transcendentals is newly established. In fact, $\mathbf{T}$ must also be nondenumerable, for otherwise by (1) and (3) we would have $\mathbf{R}$ denumerable. This is a stronger conclusion than Liouville's, which was merely that $\mathbf{T}$ is infinite.

Clearly there are two senses of the *size* of a set involved here. If S_1 is a proper subset of S_2, then it is smaller than S_2 in the sense of containing fewer elements. But it may well be possible for S_1 to be a proper subset of S_2 and still have S_1 and S_2 being of the same size in the sense that $S_1 \sim S_2$. For example, $\mathbf{N} = \{0, 1, 2, \dots, n, \dots\}$ is equinumerous with the set $\mathbf{E} = \{0, 2, 4, \dots, 2n, \dots\}$ of even integers, though $\mathbf{E}$ is a proper subset of $\mathbf{N}$. Similarly, $\mathbf{Q} \sim \mathbf{A}$ by the above, though $\mathbf{Q}$ is a proper subset of

A. Intuitively, any infinite set S is of the same size as some proper subset, while finite sets are just those which are not equinumerous with any proper subset.

So if S_1 is a subset of S_2 and we do not know whether S_1 is a proper subset of S_2, one way to establish that is to show that S_1 and S_2 are *not* equinumerous, in symbols, $S_1 \not\sim S_2$; however, to do so would require a special argument, since by the preceding $S_1 \sim S_2$ is well possible for proper subsets. In the case of $S_1 = \mathbf{A}$ and $S_2 = \mathbf{R}$, that is accomplished by the results (3) and (4) above. Cantor's argument is novel not only in the concepts (set, equinumerosity, denumerability, nondenumerability) and results (1)–(4) involved but also in a basic point of methodology: Cantor finds a property $P^\star$ of *sets* and shows two sets S_1, S_2 to be distinct by showing that one of them has the property $P^\star$ while the other does not. Moreover, the *existence of elements of a special kind* follows by a purely logical argument: if S_1 is a subset of S_2 and $P^\star(S_1)$ holds while $P^\star(S_2)$ does not hold, then S_1 must be a *proper* subset of S_2; that is, *there exists an element x of $S_2 - S_1$*.

Since two finite sets $\{a_1, a_2, \ldots, a_n\}$ and $\{b_1, b_2, \ldots, b_m\}$ (of distinct elements) are equinumerous just in case they have the same number of elements ($n = m$), Cantor was led to say in general that two sets S_1 and S_2 have the *same number of elements* if $S_1 \sim S_2$. One may regard the *number of elements in S* as an abstraction from its specific nature which isolates just what it has in common with all equinumerous sets. Thus, for example, $\{1,2,3\} \sim \{1,3,2\} \sim \{\sqrt{2}, \sqrt[3]{5}, \pi\}$ all have the same number 3. For Cantor this was a process of double abstraction; the first level $\bar{S}$ abstracts away all that is distinctive about the elements of S except how they are placed in a certain order, and the second level $\bar{\bar{S}}$ abstracts away the order as well; $\bar{\bar{S}}$ is called the *cardinal number of S.* Here instead we shall write *card(S)* for $\bar{\bar{S}}$. For example $card(\{\sqrt{2}, \sqrt[3]{5}, \pi\}) = 3, card(\{5\}) = 1$, and $card(\emptyset) = 0$. To identify the cardinal number of infinite sets we need new names. Cantor used the Hebrew letter $\aleph$ (aleph) with subscripts to name various infinite cardinal numbers, beginning with $card(\mathbf{N}) = \aleph_0$. Thus also $card(\mathbf{Z}) = card(\mathbf{Q}) = card(\mathbf{A}) = \aleph_0$, but $card(\mathbf{R}) \neq \aleph_0$. To name the cardinal number of the continuum $\mathbf{R}$, a new symbol $\mathbf{c}$ is introduced by definition as $\mathbf{c} = card(\mathbf{R})$.

There is another way of naming $card(\mathbf{R})$ that comes from the extension of the arithmetic operations of addition, multiplication, and exponentiation to infinite cardinals, as follows. Suppose $n = card(S_1)$ and $m = card(S_2)$; we can assume, without loss of generality, that S_1 and S_2 are disjoint. Then define $n + m = card(S_1 + S_2)$, where $S_1 + S_2$ is the union of S_1 and S_2. Next, define $n \times m = card(S_1 \times S_2)$ where $S_1 \times S_2$ is the set of all possible ways (x, y) of combining an element x of S_1 with an element y of S_2. Finally, define $n^m = card(S_1^{S_2})$ where $S_1^{S_2} = \underbrace{\ldots S_1 \times \ldots \times S_1 \times \ldots}_{S_2}$ consists of all possible combinations of elements of S_1, one for each position in S_2. For S_1, S_2 finite these definitions of $+$, $\times$, and exponentiation agree with the usual ones, but for S_1 or S_2 infinite we obtain new results. As examples of calculations with the latter, one has

$$n + \aleph_0 = \aleph_0 + \aleph_0 = \aleph_0$$

for any finite n. For this reflects the relations

$$\{0, 1, \ldots, n-1\} + \{n, n+1, \ldots\} \sim \{0, 2, 4, \ldots\} + \{1, 3, 5, 7, \ldots\} \sim \{0, 1, 2, \ldots\}.$$

Thus unlike the case for finite m where $n + m$ is greater than m (for $n \neq 0$), we have $m + m = m$ for $m = \aleph_0$. Similarly, for the same m we have $m \times m = m$; this corresponds to the fact that a denumerable union of denumerable sets is denumerable.

In order to represent the cardinal number $\mathbf{c}$ of $\mathbf{R}$ in terms of these operations, we return to the relationship of $\mathbf{R}$ with $\mathbf{Q}$. Each element x of $\mathbf{R}$ is approximated by sequences or sets of rationals in various ways. One way is to associate with x the subset $\mathbf{Q}_x$ of $\mathbf{Q}$ consisting of all rationals r with $r < x$; then x is uniquely determined by $\mathbf{Q}_x$. Let $\mathcal{P}(\mathbf{Q})$ be *the set of all subsets of* $\mathbf{Q}$, in symbols $\mathcal{P}(\mathbf{Q}) = \{S : S \subseteq \mathbf{Q}\}$; thus $card(\mathbf{R}) \leq card(\mathcal{P}(\mathbf{Q}))$.[3] Now for any set S the set $\mathcal{P}(S)$ of all subsets S' of S is equinumerous with $\{0,1\}^S$, the set of all possible ways of associating a 0 or 1 with each element i of S (the correspondence puts 1 if the element i belongs to S' and 0 otherwise). Hence $card(\mathcal{P}(S)) = 2^{card(S)}$ (and for this reason, $\mathcal{P}(S)$ is called the *power set of S*). In particular, $card(\mathcal{P}(\mathbf{Q})) = 2^{\aleph_0}$, so, by the above, $\mathbf{c} \leq 2^{\aleph_0}$. On the other hand, it is not difficult to produce $2^{\aleph_0}$ distinct members of $\mathbf{R}$, by looking for example at real numbers $x = 0.a_1a_2a_3, \ldots$ in binary expansion, that is, where each $a_i = 0$ or 1. The conclusion is thus that

(5) $\mathbf{c} = 2^{\aleph_0}$.

Obviously $\aleph_0 \leq 2^{\aleph_0}$ since $\mathbf{N}$ is a subset of $\mathbf{R}$; but since $\mathbf{R}$ is nondenumerable, we must have

(6) $\aleph_0 < 2^{\aleph_0}$.

Thus there are at least two transfinite numbers $\aleph_0$ and $\mathbf{c}$; can these be all?

A Plethora of Transfinite Numbers

Cantor was thus led directly to the following questions:

I. Are there any infinite cardinal numbers besides $\aleph_0$ and $\mathbf{c}$?

[3] For $n = card(S_1), m = card(S_2)$ define $n \leq m$ to hold if $S_1 \sim S_1'$ for some $S_1' \subseteq S_2$ and $n < m$ if $n \leq m$ but $n \neq m$.

II. In particular, are there any cardinal numbers n strictly between $\aleph_0$ and $\mathbf{c}$?

Cantor first computed the cardinal numbers of other familiar structures besides $\mathbf{N}$ and $\mathbf{R}$. For example, the Euclidean plane when considered from the point of view of analytic (coordinate) geometry may be represented as $\mathbf{R} \times \mathbf{R}$, and three-dimensional space as $(\mathbf{R} \times \mathbf{R}) \times \mathbf{R}$. But Cantor succeeded in showing (to his own surprise) that

$$(1) \qquad \mathbf{c} \times \mathbf{c} = (\mathbf{c} \times \mathbf{c}) \times \mathbf{c} = \mathbf{c}.$$

The fact $\mathbf{c} \times \mathbf{c} = \mathbf{c}$ is analogous to the fact $\aleph_0 \times \aleph_0 = \aleph_0$ noted above. Initially, Cantor indexCantor, Georg proved this by considering arbitrary pairs (x, y) of elements x of $\mathbf{R}$ and y of $\mathbf{R}$. Expanding the decimal parts of each as $0.a_1a_2a_3\ldots$ and $0.b_1b_2b_3\ldots$, one obtains a corresponding new decimal number by alternating their terms: $0.a_1b_1a_2b_2a_3b_3\ldots$. That $(\mathbf{c} \times \mathbf{c}) \times \mathbf{c} = \mathbf{c}$ then follows immediately from $\mathbf{c} \times \mathbf{c} = \mathbf{c}$. For a while, Cantor found no other infinite cardinal numbers besides $\aleph_0$ and $\mathbf{c}$. But eventually he proved that $\mathbf{c} < 2^{\mathbf{c}}$ and in fact that for any cardinal number n,

$$(2) \qquad n < 2^n.$$

The proof of this makes use again of his diagonal argument, via the representation of 2^n as $card(\{0,1\}^S)$ for $n = card(S)$.

Now starting with $n = \aleph_0$ we can form the sequence of larger and larger infinite cardinals

$$(3) \qquad n < 2^n < 2^{2^n} < \ldots .$$

But we can go still further beyond these by forming the infinite sum $n + 2^n + 2^{2^n} + \ldots$ (suitably defined), so that

$$(4) \qquad n < 2^n < 2^{2^n} < \ldots < (n + 2^n + 2^{2^n} + \ldots).$$

Then one can continue still higher by exponentiating and, at limits, taking sums.

One has here the beginnings of what appears to be a *scale* for representing infinite cardinal numbers. But in order to obtain a *complete scale* or *system of representations*, every cardinal number must appear somewhere in the list. This brings us back to question II above: is there any cardinal number n between $\aleph_0$ and $2^{\aleph_0}$? Cantor conjectured that there is *not*, and he expended considerable effort over many years trying to prove just that. This conjecture has come to be called the *Continuum Hypothesis*, abbreviated CH. If CH is true, then $\mathbf{c}$ is the first cardinal larger than $\aleph_0$, but independently of whether or not CH is true, it is natural to ask whether there is a first such cardinal. Cantor argued that must be so by invoking what has come to be called the *Well-Ordering Principle* (WO). A set S is said to be *well-ordered* by a relation $<$ of ordering between its elements if every nonempty subset S' of S has a first element. Thus, for example, the rearrangements of $\mathbf{N}$

$$\text{(5)} \qquad \begin{aligned} &\text{(i)} \quad 1, 2, 3, \ldots, 0, \\ &\text{(ii)} \quad 0, 2, 4, \ldots, 1, 3, 5, \ldots \\ &\text{(iii)} \quad \ldots, 5, 3, 1, 0, 2, 4 \ldots \end{aligned}$$

determine three different orderings $<_1, <_2$, and $<_3$ of $\mathbf{N}$. Under the first two of these, $\mathbf{N}$ is well-ordered (as it is under its standard order $0, 1, 2 \ldots$), but for the third, $<_3$ is not a well-ordering of $\mathbf{N}$ since the subset $\{\ldots 5, 3, 1\}$ does not contain a first element. Note that $<_3$ is the same as the natural ordering of the set $\mathbf{Z}$ of all integers.

Now Cantor claimed WO is true by the following kind of argument[4]: if S is any nonempty set, choose an element x_0 of S arbitrarily. Either $S = \{x_0\}$ or $S - \{x_0\}$ is nonempty; in the latter case choose an element x_1 of $S - \{x_0\}$ arbitrarily. Now either $S = \{x_0, x_1\}$ or $S - \{x_0, x_1\}$ is nonempty; in the latter case we proceed by choosing an element x_2 of $S - \{x_0, x_1\}$. If S is finite, then at some stage n, $S = \{x_0, x_1, \ldots, x_n\}$; otherwise $\{x_0, x_1, \ldots, x_n \ldots\}$ is an infinite subset of S. If $S = \{x_0, x_1, \ldots, x_n \ldots\}$ we are through; otherwise choose an element x_ω of $S - \{x_0, x_1, \ldots, x_n, \ldots\}$. We proceed on, as necessary, to choose distinct elements $x_{\omega+1}, x_{\omega+2} \ldots$ of S; eventually we must exhaust all the elements of S. Then we arrive at

$$\text{(6)} \qquad S = \{x_0, x_1, \ldots, x_n, \ldots, x_\omega, x_{\omega+1}, \ldots, x_\alpha, \ldots\}$$

where at each stage in the procedure of generating this transfinite sequence, there is a *first* element beyond all those already chosen. This determines a well-ordering of S by the order of generation,

$$\text{(7)} \qquad x_0 < x_1 < \ldots < x_n < \ldots < x_\omega < x_{\omega+1} < \ldots < x_\alpha < \ldots.$$

The problem with this argument is that it assumes there is a method for making an unlimited number of successive arbitrary choices, so that for each subset $S' = \{x_0, x_1, \ldots, x_\beta, \ldots\}_{\beta<\alpha}$ of S, if $S' \neq S$, the method chooses an element x_α of $S - S'$. If one takes S to be the set $\mathbf{N}$, there is no problem since the standard ordering of $\mathbf{N}$ already provides a well-ordering. But if one takes S to be the set $\mathbf{R}$, there in no known method to make the required choices. The assumption of the existence of such a sequence of choices may thus be considered to be unjustified, and that was one cause of opposition to Cantor's theory.

[4]Actually, Cantor initially asserted WO to be a "law of thought" when he first stated it in 1883. Around 1895 he began to recognize the need for an argument and indicated such in letters to Hilbert and Dedekind between 1896 and 1898; see Moore (1982), pp. 51–53.

If one assumes the Well-Ordering Principle WO, then a complete scale for the infinite cardinal numbers follows as a consequence. For if $n = card(S_1)$ and $m = card(S_2)$ and $n < m$, we have $S_1 \sim S_1'$, where S_1' is a subset of S_2. Well-order S_1', and then extend that to a well-ordering of S_2. Thus S_1 is equinumerous with an initial segment of the well-ordering of S_2. But since $S_1 \not\sim S_2$, there is a first initial segment S_2' of S_2 with $S_1 \not\sim S_2'$. Let $p = card(S_2')$; then p is larger than the cardinal number n of S_1 and, by the way it is defined, there is no cardinal number between n and p; furthermore, $p \leq m$. Hence, beyond any cardinal number n there is a next larger one, n^+, and for any m with $n < m$ we have $n^+ \leq m$.

Cantor now defined his complete scale of infinite cardinals (which he labeled using the Hebrew aleph with subscripts), as follows:

$$(8) \qquad \aleph_0 < \aleph_1 < \aleph_2 < \ldots < \aleph_n < \ldots < \aleph_\omega < \aleph_{\omega+1} < \ldots < \aleph_\alpha \ldots,$$

where at each successor stage $\aleph_{\alpha+1}$ is taken to be $(\aleph_\alpha)^+$ (the first cardinal number greater than $\aleph_\alpha$), and at limit stages $\aleph_\alpha$ is taken to be the first cardinal number larger than all preceding $\aleph_\gamma$ for $\gamma < \alpha$. The Continuum Hypothesis can now be reexamined in terms of the relationship of $\mathbf{c}$ to $\aleph_0$: since $\aleph_0 < \mathbf{c}$ and $\aleph_1$ is the first cardinal larger than $\aleph_0$, we have $\aleph_0 < \aleph_1 \leq \mathbf{c}$. If there is any cardinal number between $\aleph_0$ and $\mathbf{c}$, then $\aleph_1$ would certainly be such a number. Hence the question as to whether CH is true is equivalent to the question whether $\mathbf{c} = \aleph_1$; in other words,

$$(9) \qquad \text{is } 2^{\aleph_0} = \aleph_1?$$

To summarize: Cantor achieved his complete representation (8) of infinite cardinal numbers at the cost of assuming a problematic principle (WO), and even with this he was left with the fundamental open question (9) about the location of $\mathbf{c}$ in the resulting scale.

Justifying Cantor: Enter Zermelo

At the (Second) International Congress of Mathematicians, held in Paris in 1900, David Hilbert gave a famous lecture entitled "Mathematische Probleme," in which he presented twenty-three major unsolved problems.[5] First in Hilbert's list, which began with problems in the foundations of mathematics, was *Cantor's problem of the cardinal number of the continuum.* In his discussion of this, Hilbert states CH in the form that for every subset S of $\mathbf{R}$, either S is denumerable or $S \sim \mathbf{R}$, that is, that there are no cardinal numbers n between $\aleph_0$ and $2^{\aleph_0}$. Hilbert found CH plausible, but he also raised a question in this connection about Cantor's principle WO and, in

[5] Hilbert's article on his talk was published in English translation in 1902 and is reproduced in Browder (1976), which also contains separate articles detailing the developments arising from the various problems. Hilbert is here quoted in translation. [See also the Appendix to chapter 1 in this volume.]

particular, its consequence that **R** can be well-ordered. Concerning the latter Hilbert said, "It appears to me most desirable to obtain a direct proof of this remarkable statement, perhaps by actually giving an arrangement of [real] numbers such that in every partial system a first number can be pointed out." (Hilbert (1902), in Browder (1976), p. 9) Clearly, Hilbert was not convinced by the argument for WO that Cantor had communicated to him in 1896 or 1897—presumably in its use of a transfinite succession of arbitrary choices—since he is here calling for an *explicit* construction of a well-ordering of **R**.[6]

Hilbert was not the only one to recognize the significance of CH or to question the grounds for accepting the WO principle, despite his favorability to Cantor's ideas. Since Kronecker, Cantor had received criticism from many sides, though he had also gained adherents to his new mathematics. But as one of the leading mathematicians of the time, Hilbert's selection of the Continuum Hypothesis and the Well-Ordering Principle to be the first in his entire list of questions gave added prominence to both the importance of Cantor's ideas and their problematic aspects.

Ernst Zermelo was one of the first to take up Hilbert's challenge. Zermelo had initially worked on topics in analysis and mathematical physics, but in 1899 he became a Privatdozent at Göttingen, where he came under Hilbert's influence and began pursuing set theory. Zermelo's first main contribution to the subject was achieved in 1904 with his proof of the Well-Ordering Theorem on the basis of a new principle, which has come to be called the *Axiom of Choice*, or AC in abbreviated form.[7] Zermelo's original statement of AC is to the effect that if S is any nonempty set, then there is a function f which is defined on the collection C of all nonempty subsets X of S such that for each X in C we have $f(X)$ in X. In other words, f provides for the *simultaneous choice* from each nonempty subset X of S of a distinguished element (namely, $f(X)$) of X. By proving WO from AC, Zermelo was able to reduce the construction of a transfinite sequence of successive choices (which appear to proceed through time) needed in Cantor's attempt to prove WO, to the assumption of a single simultaneous collection of choices. AC itself is an immediate consequence of WO, for if S is well-ordered by a relation $<$, one can define $f(X)$ to be the least element of X under $<$ for each nonempty subset X of S. Since WO and AC are thus equivalent, Zermelo's proof can only be considered to have achieved progress if AC is evident in a way that WO is not.

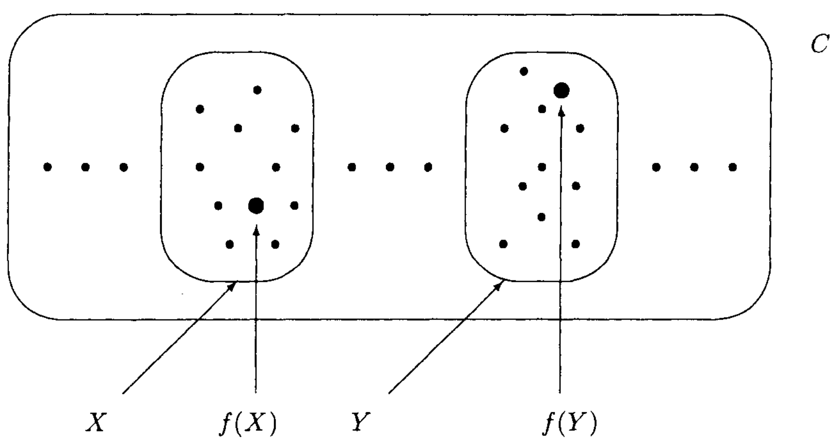
In the years directly following Zermelo's 1904 publication, his proof provoked considerable controversy, centering on both the assumption of AC

[6] Modern logical work on set theory has shown that Hilbert's hope is impossible to realize; the precise sense of this is explained in a later section.

[7] Moore (1982) gives the most comprehensive history available concerning the background, development, and importance for set-theoretical mathematics of Zermelo's Axiom, as well as the extensive controversies to which it has given rise. It was subsequently proved that many important set-theoretical propositions are equivalent to AC. One which is closest to it is the *Multiplicative Axiom*, formulated independently by Russell, which emerged from work of Whitehead and Russell.

as well as his use of other set-theoretical concepts and principles. (Some critics mistakenly believed that a form of the Burali-Forti paradox was involved in Zermelo's argument.) The main criticism of AC was that a function f is asserted to *exist* without any means being provided to define it uniquely. In this, AC violates a view of mathematics according to which existence of an object satisfying a certain condition can only be asserted when such an object has been *explicitly constructed* or *defined.* In contrast, Zermelo's principle asserts the outright existence of an object, f, independent of any means of definition.

Later, Zermelo formulated another principle AC′, which may be considered intuitively clearer than AC, but which can be shown equivalent to it, namely, that if C is any collection of nonempty *disjoint* sets,[8] then there is a function f defined on C such that for each X in C we have $f(X)$ in X. AC′ can be pictured as follows:



However, the basic objection to AC on the grounds that it asserts existence without explicit definability applies equally well to AC′. On the other hand, if one conceives sets to be *arbitrary collections* of entities existing independently of human constructions and definitions, then AC′ is immediately intuitively evident. Thus the question whether to accept or reject AC comes down to a fundamental difference of viewpoints as to the nature of mathematics.[9]

In order to disentangle the various objections to his proof of the Well-Ordering Theorem and shore up his defense of it, Zermelo arrived in 1908

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[8]This means that if X, Y are in C and $X \neq Y$, then the intersection of X and Y is empty.

[9]AC was by no means the first example in mathematics of the assertion of existence of an object without any means being known to construct it. However, it was the first in which this difference is so blatantly revealed.

at an explicit, systematic presentation of basic notions and principles for sets, in the form of an *axiomatic system of set theory*. Two of Zermelo's axioms assert the outright existence of certain sets (namely, the empty set and an infinite set representing the natural numbers **N**), while further axioms state that if certain sets exist then other sets related to them also exist. The latter include the nonconstructive existence statement AC but also include axioms for the usual constructions in set theory such as those of pairing, union, intersection, difference, product, set-of-all subsets, etc. A principal general means of construction is guaranteed by Zermelo's *Axiom of Separation* according to which if S is any set and P is any *definite property* of elements of S, then the set $S' = \{x \in S : P(x)\}$ (that is, $\{x : x \in S \text{ and } P(x)\}$) exists. By this means Zermelo finessed the paradoxes which had arisen from the unrestricted formation of sets $\{x : P(x)\}$. The latter had led to what Cantor called *inconsistent* or *absolutely infinite* sets such as the set C of all cardinal numbers, and the set V of all sets. While not all sets $\{x : P(x)\}$ are necessarily inconsistent in the logical sense, some among them are, as for example Russell's set $R = \{x : x \notin x\}$ consisting of all sets which are not members of themselves (the paradox results when one asks whether $R \in R$ or not). Absolutely infinite or inconsistent sets in Cantor's sense cannot be constructed in Zermelo's axiom system,[10] since there is *no universal set* V from which to separate $\{x : P(x)\}$.

Aside from the existence axioms of Zermelo's system, there is one further axiom which reflects the underlying philosophical view as to the nature of mathematics on which the system rests. This is the *Axiom of Extensionality*, according to which if S_1 and S_2 are sets having exactly the same members, then $S_1 = S_2$; in other words, a set is determined entirely by its members and is to be regarded independently of any specific means of determining just what those members are.[11]

Zermelo showed how the proof of WO from AC could be carried out on the basis of his other axioms. Furthermore, the Well-Ordering Theorem has the following three basic consequences, as Zermelo had already shown in his 1904 article: (i) for any cardinal numbers n and m, exactly one of the three possibilities: $n < m, n = m, m < n$, must hold; (ii) every infinite cardinal n is of the form $\aleph_\alpha$ for some (ordinal) α; and (iii) for any infinite cardinal number m, we have $m = m + m = m \times m$. The first of these is called the *Trichotomy Principle* and says that the cardinal numbers are linearly ordered; in other words, any two of them can be compared as in a scale. The second result gives the complete system of representation for infinite cardinals in terms of the "alephs." The latter are defined in such a way that each $\aleph_\alpha$ represents a well-ordered set; hence (ii) implies WO

[10] Unless the system is logically inconsistent for some other, less obvious reason; then every statement would be derivable according to the usual laws of logic.

[11] For more detail on Zermelo's axiom system and its development see Moore (1982), pp. 153 ff., and, more extensively, Fraenkel, Bar-Hillel, and Levy (1973). Translations of Zermelo's 1904 and 1908 papers referred to above are in van Heijenoort (1967).

and thus AC. It was shown later (by Hartogs) that (i) implies AC as well. Still later Alfred Tarski proved that the statement $m = m \times m$ for all infinite cardinals m also implies AC. Hence even this special fact from the arithmetic of infinite cardinal numbers is as basic as the general principles (i) and (ii) concerning their scale of representation.

Zermelo's axiomatization of set theory did not succeed in satisfying his critics as he hoped. Indeed, it supplied even more fuel for criticism, particularly concerning Zermelo's use of the vague notion of *definite property* in his formulation of the Axiom of Separation. It was some years before this was given a satisfactory explanation by means of formal languages which had been developed for symbolic logic. This was done first by Hermann Weyl and then in a simpler, improved way by Thoralf Skolem (in 1922). In Skolem's account, "definite properties" are replaced by (*well-formed*) *formulas* in a language of the *first-order predicate calculus*, whose basic predicates are $\in$ and $=$. The basic formulas are thus of the form $x \in y$ and $x = y$; formulas in general are built up using symbols for propositional connectives including negation ($\neg$), disjunction ($\vee$), conjunction ($\wedge$), implication ($\rightarrow$), and by existential and universal quantification, $(\exists x)(\dots)$ ("there exists $x, \dots$") and $(\forall x)(\dots)$ ("for all $x, \dots$"). The reasoning employed in making derivations from the axioms is that of first-order logic, that is, the logic of propositional connectives and quantifiers with respect to variables ranging over the domain of discourse. This formal version of *Zermelo Set Theory*, without the Axiom of Choice, is often simply denoted Z. The full system with AC added is denoted Z + AC, or ZC in abbreviated form.

Though basic facts about the arithmetic and ordering of cardinal numbers can be derived in ZC, it turns out that it is insufficient to prove the existence of the limit of the increasing sequence of cardinals $\aleph_0, 2^{\aleph_0}, 2^{2^{\aleph_0}}, \dots$, or equivalently of the set $M = \{\aleph_0, 2^{\aleph_0}, 2^{2^{\aleph_0}}, \dots\}$. We have here a correspondence R between the elements of a set whose existence is already given, namely $\mathbf{N}$, and those of a set M whose existence should be provable but is not; moreover, this correspondence is definable in the first-order language of set theory. In order to overcome this limitation in Zermelo's system, Abraham Fraenkel introduced (in 1922) a new axiom called *Replacement*. This says that if we have a first-order definable relation $R(x, y)$ which associates with each element x of an already existing set A a unique element y, then the set B of all the associated y's exists; intuitively each element x of A is replaced by the unique y such that $R(x, y)$ holds. The system Z with Fraenkel's Axiom of Replacement added is called *Zermelo-FraenkelSet Theory* and is denoted ZF.[12] Again, the adjunction of AC to ZF is indi-

[12] Actually, the need for the new axiom was realized independently by Skolem, and his formulation of it was even superior to Fraenkel's. Thus, properly speaking, the system should be called *Zermelo-Skolem Set Theory* and denoted ZS, or to be more generous, *Zermelo-Fraenkel-Skolem Set Theory* (ZFS). However, we are stuck with ZF as the standard designation these days. Translations of Skolem's and Fraenkel's 1922

cated by ZF + AC or simply ZFC. All of Cantor's theory of infinite sets and numbers can be formalized in ZFC, that is, can be developed in a logically exact manner within that system.

While the logicians were improving and extending Zermelo's axiom system, mathematicians were employing its basic principles more and more and with increasing assurance in the areas of algebra, analysis, and topology. It would take us too far afield here to outline this development, and we must refer the reader to other sources.[13] Though concern about AC never died down, set-theoretical methods and ideas were widely applied in mathematical practice by 1940 and eventually took over in textbook expositions of many parts of mathematics following the Second World War. Simultaneously, a growing mild acquaintanceship with logic gave mathematicians a better (though not deep) understanding of what Zermelo, Skolem, and Fraenkel had accomplished; in this superficial sense, then, Zermelo finally received his due.

The Foundational Issue (Part 1): Cantorian Set Theory as Mathematical Platonism

Zermelo's axiomatization and its extensions diminished one area of concern about Cantorian set theory, namely, that it led to paradoxical constructions; at least there is no obvious way to carry those out in systems such as ZC and ZFC. But the objections based on more fundamental grounds as to the nature of mathematics remained, and fueled a vigorous and continuing controversy between the opponents and defenders of higher set theory.

The history of mathematics has been punctuated repeatedly by the introduction of problematic concepts (for example, imaginary numbers, infinitesimals, and points at infinity), problematic principles (for example, the parallel postulate), and problematic methods (for example, Fourier series expansions of arbitrary functions, and the operational calculus). These have been dealt with in each case by either complete or partial (perhaps modified) acceptance or outright rejection. Each instance raised anew the question: *What is it justified to say and do in mathematics?* In the past, such questions were dealt with by mathematicians sensitive to the issue of justification on a case-by-case or "local" basis. Moreover, this tradition of mathematics taking good care of its own house has continued to operate right up to the present.[14]

papers are to be found in van Heijenoort (1967).

[13]See, to begin with, Moore (1982), chapters 3 and 4. Of interest for the sociology of the subject is that set-theoretical mathematics was given a great boost in Poland after the First World War, largely under the leadership of Wacław Sierpiński.

[14]I have analyzed various ways of dealing with such local foundational problems in Feferman (1985), using both classical and modern cases as illustrations. [An updated version of that paper is reproduced as chapter 5 in this volume; cf. also chapter 4 for an abbreviated version.]

The advent of higher set theory gave rise to a complex question of justification of an altogether more fundamental and "global" nature, which again engaged mathematicians first and foremost, but now brought it to the doors of philosophy. Even the most basic modes of reasoning employed in mathematics had to be reconsidered, and here the technical developments of modern logic provided essential new tools for that purpose.

What were the aspects of Cantorian and Zermeloan set theory found to be problematic by its opponents? Here there was no united view, but the following features were of recurrent concern:

(i) *Sets as independent existents.* Sets are conceived to be objects having an existence independent of human thoughts and constructions. Though abstract, they are supposed to be part of an external, objective reality.

(ii) *Actual infinity.* Infinite sets are supposed to exist as completed objects; the most basic of these is the totality of natural numbers.

(iii) *Arbitrary (sub)sets.* For each set, any arbitrary combination of its elements is supposed to exist as a well-determined set in its own right.

(iv) *Power sets.* For each set S, the totality $\mathcal{P}(S)$ of arbitrary subsets of S is supposed to exist as another set.

(v) *The axiom of choice.* For any set S and any subset C of $\mathcal{P}(S)$ consisting of disjoint nonempty sets, there is a choice set, existing as an arbitrary combination of elements, one from each member of C.

(vi) *Relations and functions as sets.* A relation R between elements of a set S_1 and those of a set S_2 is supposed simply to be an arbitrary set of pairs in $S_1 \times S_2$. Functions from S_1 to S_2 are supposed simply to be many-one relations.

(vii) *Objectivity of truth and classical logic.* Each proposition P about sets has a definite truth value (true or false), independent of any means we may have to verify it. The *Law of Excluded Middle* (LEM), that P or $\neg P$ holds, is thus accepted for any such P.

The following remarks expand on these features:

ad(i). Views of mathematical objects as independently existing abstract entities are generally called a form of *platonism.* In its particular set-theoretical manifestation, this reveals itself most obviously in such principles as the Axiom of Extensionality and the Axiom of Choice.

ad(ii). The platonist position *per se* does not necessarily require the existence of infinite sets (or, for that matter, of sets at all), but of course that is essential to Cantorian set theory.

ad(iii). Though the Axiom of Separation—according to which for any set S and well-determined property $P(x)$ (of elements of S) the set $\{x \in S : P(x)\}$ exists—is fundamental and widely applied in set theory, this must not be taken to mean only definable subsets of a set need be assumed to exist.

ad(iv). The formation of power sets leads to the higher infinities by the increase in cardinality of $\mathcal{P}(S)$ over that of S, for any set S. In addition, the existence of power sets justifies *impredicative definition*, whereby a subset S' of S can be singled out by reference (by quantification over $\mathcal{P}(S)$ to arbitrary subsets X of S (for example, as the intersection of all subsets X of S satisfying a given condition $Q(X)$).

ad(v). As already explained at length in the preceding sections, the Axiom of Choice is essential to establish a linear (in fact, well-ordered) scale of infinite cardinals.

ad(vi). The reduction of functions to sets goes far beyond mathematicians' previous conception of functions as given by laws. Since the idea of one-to-one correspondence is explained in terms of one-to-one functions, this is also essential for the theory of equinumerosity.

ad(vii). An immediate consequence of LEM and the other laws of logic is the method of *proof by contradiction*: if the assumption that P is false leads to a contradiction, then P is true; formally,

$$[(\neg P) \to (Q \wedge \neg Q)] \to P.$$

Furthermore, the usual laws of quantification coupled with LEM yield

$$(\exists x)P(x) \leftrightarrow \neg(\forall x)\neg P(x).$$

In other words, to establish an existence result $(\exists x)P(x)$, it is sufficient to assume $(\forall x)\neg P(x)$ and derive a contradiction. In general this will not show how to produce a solution x to satisfy $P(x)$.

According to the platonist picture of set theory, then, statements such as the Continuum Hypothesis CH have a definite truth value, which it would be our aim as mathematicians to determine with all means at our disposal.

The Foundational Issue (Part 2): Brouwer's Way Off, Hilbert's Way Out, and Weyl's Way to Get By

The possible responses to those who found the platonist philosophy of mathematics unacceptable as a justification for higher set theory were either to reject that theory in whole or in part, or attempt some alternative form of justification. Here we review three basically different responses, due respectively to L. E. J. Brouwer, David Hilbert, and Hermann Weyl. To carry out their ideas, Brouwer and Hilbert created substantial foundational programs; these drew much attention but (as we shall see) proved far from successful. Weyl's initiative was more limited in its scope but achieved its aims within that; his basic work and approach is a forerunner of the results to be reported at the end of the essay [in chapter 12].

Brouwer's Way Off

Brouwer's program was the most radical and the most original of the three considered here. He advanced this with passionate conviction and persistence (despite its reception with general incomprehension and/or rejection) from his doctoral dissertation in 1907 through his last paper in 1955.[15] Brouwer strongly criticized platonism and formalism (of which, see below). In his 1908 paper "The unreliability of logical principles," he argued that the Law of Excluded Middle for statements about infinite sets is obtained by an unjustified extension from finite sets, where it is correct. For example, when P is a testable property, we can verify $(\exists x)P(x) \vee (\forall x)\neg P(x)$ for "x" ranging over a finite S, by inspecting each element of S in turn, but this method is evidently inapplicable to infinite S. For Brouwer, questions of truth are restricted to statements that can be verified or disproved. Thus $P \vee \neg P$ cannot be declared true until we have verified one or the other of its disjuncts. Similarly $(\exists x)P(x)$ cannot be recognized as true until we have found an instance a which makes $P(a)$ true. On the other hand, to verify $(\forall x)P(x)$ in an infinite S we cannot check each element of S, and other methods must be found according to the nature of S. Thus, for example, the *Principle of Mathematical Induction*, that $P(0) \wedge (\forall x)[P(x) \rightarrow P(x+1)]$ implies $(\forall x)P(x)$, provides a method of drawing conclusions of the form $(\forall x)P(x)$ in the set $\mathbf{N}$ of natural numbers. Brouwer also rejected the completed infinite, and he called collections S such as $\mathbf{R}$ and Cantor's second number class Ω (the countable ordinals) *denumerably unfinished*: beyond any well-determined denumerable subset S' of such S we can associate an element of $S - S'$. For Brouwer, then, the statement CH, which states the equinumerosity of $\mathbf{R}$ and Ω, has no definite meaning and the question of its truth has no interest for him.

Brouwer thus took a completely *constructivist* stance in his critique of platonism, continuing the way advanced by Kronecker. But Brouwer gave his constructivism a particularly subjectivist stamp which he labeled *intuitionism*, emphasizing the origin of mathematical notions in the human intellect. Moreover, beginning in 1918, Brouwer went on to provide a systematic constructive redevelopment of mathematics for the first time, going far beyond anything actually done by Kronecker.

[15] Brouwer was Dutch and did his dissertation in Amsterdam. All his published papers on philosophy and the foundations of mathematics are to be found in volume 1 of his *Collected Works* (1975); the papers originally in Dutch are there translated into English. Incidentally, the second volume in his *Collected Works* consists of papers in nonconstructive mathematics, mainly topology, to which Brouwer made fundamental contributions during the period 1908–1913.

The theory of real numbers provided the first obstacle to a straightforward constructivist redevelopment. With real numbers identified as convergent sequences of rational numbers, all sorts of classical results would apparently fail to have reasonable constructive versions if one restricted attention only to sequences determined by effective laws. Brouwer introduced instead a novel conception, that of *free choice sequences* (f.c.s.), which might be determined in nonlawlike ways (for example, by random throws of a die), and of which one would have only finite partial information at any stage. Then with the real numbers viewed as convergent f.c.s. of rationals, a function f from $\mathbf{R}$ to $\mathbf{R}$ can be determined using only a finite amount of such information at any given argument. Brouwer used this line of reasoning to conclude that any function from $\mathbf{R}$ to $\mathbf{R}$ must be *continuous*, in direct contradiction to the classical existence of discontinuous functions. With this step Brouwer struck off into increasingly alien territory, and he found few to follow him even among those sympathetic to the constructive position.[16]

Hilbert's Way Out

When Hilbert addressed the International Congress of Mathematicians in 1900, he was nearing the age of forty and was already considered to be one of the world's greatest mathematicians. Hilbert had by that time made his mark in algebra, number theory, geometry, and analysis. He would go on to make further substantial contributions in analysis, mathematical physics, and the foundations of mathematics. At Göttingen, where he was a professor, Hilbert had many first-rate colleagues and students who often helped with the detailed development of his ideas. In particular, Hilbert's program for the foundation of mathematics was taken up by von Neumann, Ackermann, Bernays, Gentzen, and others, beginning in the 1920s. This program was shaped initially by both Hilbert's general tastes and interests as well as his specific experience with axiomatic geometry. Hilbert was noted for his clarity, rigor, and a penchant for systematically organizing subjects; at the same time he had the knack of putting these pedagogical tendencies to work to develop powerful methods for the solution of concrete problems. His work in algebra and algebraic number theory was part of the growing tendency toward abstract methods in the nineteenth century, which then came to dominate twentieth-century mathematics. One advantage of such methods is their generality—any mathematical structure meeting the basic principles must satisfy all the conclusions drawn from them.

In his work on axiomatic geometry, Hilbert returned to the axiom system that had come down from Euclid, for which he gave a superior development meeting modern standards of rigor. In addition, Hilbert moved on

[16]Nowadays, Brouwer's theories of f.c.s. are much better understood and are demonstrably coherent. Dummett (1977) gives an excellent introduction to the development of mathematics based on Brouwer's intuitionistic ideas. [Cf. also Troelstra and van Dalen (1988) and van Stigt (1990).]

to what we would now call "metageometry," through the study of questions such as the *independence* of certain axioms from the remaining ones. This he achieved by a series of constructions of unusual *models* which satisfy all the axioms but the one to be shown independent. Also, Hilbert demonstrated the *consistency* of his axioms by the methods of analytic geometry, which interpret the statements in the Cartesian plane $\mathbf{R} \times \mathbf{R}$ and space $\mathbf{R} \times \mathbf{R} \times \mathbf{R}$. In other words, geometry is shown consistent *relative* to a theory of real numbers (and for weaker combinations of axioms, relative to subsets of $\mathbf{R}$ such as the algebraic numbers). In the second problem of his 1900 address (Hilbert 1900, 1902), the compatibility of the arithmetical axioms, Hilbert proceeded to call for a proof of consistency for a system of axioms for $\mathbf{R}$, which he recognized would, in some sense, have to be *absolute*.

Hilbert's statement of Problem 2 already reveals some of his key positions, though they would be extended and elaborated later. He says there that the foundations of any science must be provided by setting up an *exact* and *complete* system of axioms. "The axioms so set up are at the same time the definitions of [the] elementary ideas [of that science]; and no statement within the realm of the science whose foundation we are testing is held to be correct unless it can be derived from those axioms by means of a finite number of logical steps" (Hilbert (1902), in Browder (1976), p. 10). After explaining the relative consistency proofs for geometry, he says: "On the other hand a direct method is needed for the proof of the compatibility of the arithmetical axioms" (*ibid.*) (that is, for a theory of real numbers). Hilbert goes on to posit that a mathematical concept *exists* if, and only if, it can be shown to be consistent (noncontradictory); thus, for him "the proof of the compatibility of the axioms [for real numbers] is at the same time the proof of the mathematical existence of the complete system of real numbers" (*ibid.*, p. 11). The real numbers are not to be regarded as all possible convergent sequences of rational numbers, but rather as a structure determined by or governed by certain axioms. Finally, in his statement of Problem 2, Hilbert expresses the conviction that the existence of Cantor's higher number classes will be demonstrated by a consistency proof "just as that of the continuum." Evidently, at that time Hilbert thought the consistency proof of both the theory of real numbers and set theory would be straightforward. But within a few years (Hilbert 1904) he was presenting a less sanguine view: the paradoxes of set theory seemed to him to indicate that the problem of establishing the consistency of set theory presented greater difficulties than he had anticipated. Furthermore, according to Paul Bernays, "although he strongly opposed Leopold Kronecker's tendency to restrict mathematical methods, he nevertheless admitted that Kronecker's criticism of the usual way of dealing with the infinite was partly justified."[17]

[17] Bernays (1967), p. 500. For more information about Hilbert's shifting views concerning foundations see Sieg (1984), pp. 166 and 170ff. [cf. also Hallett (1995)].

In the following decades Hilbert was preoccupied with the theory of integral equations and mathematical physics, and he did not return to problems of foundations until 1917.* In the meantime, Zermelo had laid the axiomatic grounds for Cantorian (platonist) set theory while Brouwer had launched his attacks on mathematical platonism and formalism. Concerning the latter, Brouwer stressed that *consistency* was not enough to justify the use of mathematical principles; what was necessary was to assure their *correctness*.

Hilbert's mature program for the foundations of mathematics via *finitist proof theory* was announced in his 1917 address, "Axiomatic thought" (Hilbert 1918); it was then elaborated in a succession of almost yearly publications through 1931. Briefly, the idea is that a given body of mathematics (such as number theory, analysis, or set theory) is to be treated as formally represented in an axiomatic theory T. Each such T is to be specified by precisely described rules for generating its well-formed formulas (statements) from a finite stock of basic symbols. Certain of these formulas are then selected as axioms (both logical ones and axioms concerning the specific subject matter of T), and rules for drawing inferences from the axioms are specified. For a formal axiomatic theory T presented in this way, the set of *provable formulas* is defined to consist of just those formulas for which there exists a proof (or derivation) in the system, that is, a finite sequence ending with the formula, each term of which is an axiom or is obtained from preceding terms by one of the rules of inference. Then T is *consistent* just in case there is no contradiction $(P \wedge \neg P)$ which is provable in T. Hilbert's *Beweistheorie*, or *theory of proofs*, was developed as a tool to analyze all possible derivations in formal axiomatic systems. With proofs represented as finite sequences of formulas, and formulas as finite sequences of basic symbols, whose structure in both cases is regulated by effective conditions, the question of consistency of T in no way assumes the actual infinite. Now Hilbert's idea was to use entirely *finitary* methods in establishing the consistency of formal systems which otherwise required for their justification the assumption of the actual infinite. Not only the theory of real numbers but already a formal system of elementary number theory would have to be shown consistent. Such a system would be a first-order version PA (Peano Arithmetic) of Peano's axioms for $\mathbf{N}$, formulated by replacing the second-order axioms of induction by the corresponding first-order scheme: $P(0) \wedge \forall x(P(x) \rightarrow P(x+1)) \rightarrow \forall x P(x)$, for all formulas $P(x)$ in the language of PA. Hilbert wanted to justify the use of such a system including classical logic, which leads by LEM to statements like $(\exists x)P(x) \vee (\forall x)\neg P(x)$; in this, he indirectly acknowledged Brouwer's criticism of the assumption of the actual infinite.

In addition to his general program, Hilbert proposed some specific proof-theoretic techniques to carry it out. These methods were shown

*[It has been brought to my attention by W. Sieg that this is not quite correct: Hilbert continued to lecture throughout that period on the foundations of mathematics; cf. Hallett (1995).]

to work in relatively simple cases for theories T much weaker than PA. The success of the program would depend next on extending them to a finitary proof of the consistency of PA, and so on, up to the consistency of set-theoretical systems like ZFC. As we shall see, these hopes were to be dashed by Gödel's incompleteness theorems of 1931.

Hilbert's 1926 paper "On the infinite"[18] is a very readable exposition of the finitist program for the foundations of mathematics and is a typical example of Hilbert's style of heroic optimism, which nowadays may be considered bombastic: "the definitive clarification of the *nature of the infinite* has become necessary, not merely for the special interests of the individual sciences, but rather for the *honor of the human understanding* itself" (Hilbert (1926), in van Heijenoort (1967), pp. 370–371).

The first portion of Hilbert's 1926 paper reviews the general "problem of the infinite" in mathematics, from which he turns to the particular problems raised by Cantor's theory of transfinite numbers. Here "the infinite was enthroned and enjoyed the period of its greatest triumph" (*ibid.*, p. 375). But the paradoxes discovered by Russell and others brought one to an intolerable situation: is there no way to retain what Cantor achieved while escaping the paradoxes? Yes, by careful investigation and proof of the complete reliability everywhere of our inferences, "no one shall drive us from the paradise that Cantor created for us" (*ibid.*, p. 375).

The plan laid out in the mid-portion of Hilbert's paper is that of expressing mathematical propositions formally and representing mathematical inferences by derivations in precisely described formal systems. The formulas of these systems are divided into *finitary propositions* and *ideal propositions*. Finitist proof theory is to be used to show how the latter can be eliminated in terms of the former; this is to be achieved by finitary proofs of consistency. The procedure of elimination here is analogous to that used to justify the introduction of *ideal elements* in mathematics such as imaginary numbers, points at infinity, and the algebraic number ideals introduced by Kummer. Apropos of this last, Hilbert remarks that "it is strange that the modes of inference that Kronecker attacked so passionately are the exact counterpart of what . . . the same Kronecker admires so enthusiastically in Kummer's work and praised as the highest mathematical achievement" (*ibid.*, p. 379). Moving on to more definite proposals, a formal system of elementary number theory is presented which is equivalent to the first-order axioms PA of Peano Arithmetic indicated above. Hilbert claims that the problem of its consistency is "perfectly amenable to treatment"; moreover, what a "pleasant surprise that this gives us the solution also of a problem that became urgent long ago," (*ibid.*, p. 383) namely, the consistency of a system of axioms for the real numbers.

Finally, in the third part of his 1926 paper, Hilbert moves into high gear, leaving even his acolytes standing bewildered in the dust. For here

[18]We refer here to the English translation in van Heijenoort (1967), pp. 369–392.

he claims "to play a last trump" and to show how the continuum problem can be solved by use of proof theory. Hilbert's plan has intriguing aspects roughly related to later work on constructive hierarchies of number-theoretic functions as well as to Gödel's own definitive results on the consistency of CH (to be discussed below), but it has never been worked out in any coherent form.* In his bravado and eagerness to demonstrate the success of his program, Hilbert promised far more than he could deliver. Indeed, even the claims of finitist proofs of the consistency of elementary number theory and real number theory would not survive Gödel's incompleteness theorems of 1931. For one who trumpeted the cause of absolute reliability, how wrong could he be? Nevertheless, Hilbert's influence and program were decisive factors in what followed: as we shall see, the attack on the "problem of the infinite" was shifted to the arena of metamathematics and in that proof theory became one of the primary tools.

One final remark concerning Hilbert vis à vis the constructivists needs to be made. As Paul Bernays tells it, "there was a fundamental opposition in Hilbert's feelings about mathematics . . . namely his resistance to Kronecker's tendency to restrict mathematical methods . . . particularly, set theory . . . [and his] thought that Kronecker had probably been right. . . . It became his goal to do battle with Kronecker with his own weapon of finiteness."[19] But in his efforts to outfight the constructivists, Hilbert was hoist with his own petard.

Weyl's Way to Get By

Hermann Weyl was one of Hilbert's most illustrious students, and his work was almost as broad (and deep) as that of his teacher. After a period as Dozent in Göttingen following his doctoral work there, Weyl took a position in Zürich shortly before World War I. Hilbert made several efforts to bring him back, and Weyl finally returned as Hilbert's successor in 1930, only to leave for Princeton when the Nazis came into power in 1933. All these personal and professional connections made it difficult for Weyl to express his strong differences with Hilbert on foundational matters, but he did so tactfully on a number of occasions.[20]

Weyl evolved his first approach to the foundations of mathematics in the monograph *Das Kontinuum* (1918). In the introduction thereto he criticized axiomatic set theory as a "house built on sand" (though the objects of, and reasons for, his criticism are not made explicit). He proposed to replace this with a solid foundation, but not for all that had come to

*[Cf. the discussion by R. M. Solovay of one of Gödel's proofs of the consistency of CH in Gödel (1995), 114–127.]

[19]Quoted in Reid (1970), p. 173.

[20]Weyl's best known works on logic and the philosophy of his mathematics are his monograph (1918) and book (1949), both discussed below. But there are also many less familiar articles on these subjects scattered through his *Collected Works* (1968), spanning the period 1918–1955. [Chapter 13 in this volume provides a detailed examination of Weyl (1918); see also chapter 14.]

be accepted from set theory; the rest he gave up willingly, not seeing any other alternative. Weyl's main aim in this work was to secure mathematical analysis through a theory of the real number system (the continuum) that would make no basic assumptions beyond that of the structure of natural numbers **N**. Unlike Hilbert, Weyl did not attempt to reduce first-order reasoning about **N** to something supposedly more basic. In this respect Weyl agreed with Henri Poincaré that the natural number system and the associated principle of induction constitute an irreducible minimum of theoretical mathematics, and any effort to "justify" that would implicitly involve its assumption elsewhere (for example, in the metatheory). And, unlike Brouwer, Weyl accepted uncritically the use of classical logic at this stage (though at a later date he was to champion Brouwer's views). Formulated in modern terms, a system like PA was thus accepted by Weyl as basic. However, for a theory of real numbers one would have to provide a means to treat sets or sequences of natural numbers (using the reduction of **Q** to **N**) and then, for analysis, explain how to deal with functions of real numbers as functions from and to such sets. Weyl added axioms for existence of sets of natural numbers which are arithmetically definable; these are of the form $(\exists X \subseteq \mathbf{N})(\forall n \in \mathbf{N})[n \in X \leftrightarrow P(n)]$ where the formula $P(n)$ contains no quantified variables other than those which range over **N**. Using two sorts of variables, lowercase for elements of **N** and uppercase for subsets of **N**, these axioms take the form $(\exists X)(\forall n)[n \in X \leftrightarrow P(n)]$. Weyl deliberately excluded axioms of this type in which P contains quantified variables ranging over subsets of **N**; in particular, he thus excluded statements of the form $(\exists X)(\forall n)[n \in X \leftrightarrow (\forall Y)Q(n,Y)]$, even when Q is an arithmetical formula. Weyl's reason for doing so was that otherwise one would be caught in a *circulus vitiosus*. The matter at issue here requires a lengthy aside.

The *vicious circle principle*, first enunciated by Poincaré, was designed to block certain purported definitions, in which the object introduced is somehow defined in terms of itself. According to Poincaré, all mathematical objects beyond the natural numbers are to be introduced by explicit definitions. But a definition which refers to a presumed totality—of which the object being defined is itself to be a member—involves one in an apparent circle, since the object is then itself ultimately a constituent of it own definition. Such "definitions" are called *impredicative*, while proper definitions are called *predicative*; put in more positive terms, in the latter one refers only to totalities which are established prior to the object being defined. The formal definition of a set X of natural numbers as $X = \{n : (\forall Y)Q(n,Y)\}$, taken from the axiom $\exists X \forall n[n \in X \leftrightarrow (\forall Y)Q(n,Y)]$, is impredicative in Poincaré's sense because it involves the quantified variable "Y" ranging over arbitrary subsets of **N**, of which the object X being defined is one member. Thus in determining whether $(\forall Y)Q(n,Y)$ holds, we shall have to know in particular whether $Q(n,X)$ holds—but that can't be settled until X itself is determined.

Poincaré raised his ban on impredicative definition thinking doing so would exclude the paradoxes. However, that succeeds only by taking a very broad reading of what it means for a definition to refer to a totality. For example, in Russell's paradox, derived from $\exists a \forall x[x \in a \leftrightarrow x \notin x]$, or $a = \{x : x \notin x\}$, the formula $P(x) = (x \notin x)$ does not refer to the presumed totality of *all* sets, since it does not contain any *quantified variables* ranging over sets. On the other hand, it is true that in order to determine by its "definition" which members a has, we must already know the answer; in particular, $a \in a$ would be determined true only if we already knew that $a \notin a$. However, this vicious circle is not itself excluded by the principle enunciated by Poincaré in its prima facie reading. On the other hand, Poincaré's principle as it stands would certainly exclude impredicative definitions in analysis of the sort $X = \{n : P(n)\}$ where P contains quantified variables ranging over arbitrary subsets of $\mathbf{N}$; the objection to such definitions is not that they are paradoxical, but rather that they are implicitly circular and hence not proper.

Russell was one of the first to accept Poincaré's ban on impredicative definitions as applied to sets, and he turned to the construction of a formalism for generating predicative definitions. Roughly speaking, instead of talking about arbitrary sets, one may only talk in this formalism about *sets of level 1, sets of level 2*, etc. A set of level 1 is defined by a formula using quantified variables ranging over individuals only. This determines the totality of sets of level 1; then a set of level 2 is defined using just quantified variables ranging over individuals *and* over arbitrary sets of level 1. The problems with this kind of *ramification* (as Russell called it) is that in the resulting theory of real numbers (introduced as sets of rational numbers) one would have reals of level 1, reals of level 2, etc., but analysis with such distinctions would be unworkable. As an ad hoc device, Russell then introduced his *Axiom of Reducibility*, which says that every set of higher level is coextensive with one of lowest level. In effect, this "axiom" nullified the point of ramification and indirectly permitted the use of impredicative definitions in analysis. Russell recognized that this step did not jibe with the initial philosophical outlook stemming from Poincaré's definitionism, but saw no alternative to developing analysis. As it happens, his *theory of types*, which distinguished sets according as to whether they were sets of individuals (type 1), sets of such sets (type 2), etc., and which restricted the membership relation to objects of successive types (with individuals taken to be of type 0), already blocked the paradoxes without in any way forcing one to insist on predicative definitions. This was achieved by the later move (due to F. P. Ramsey) to *Simple Type Theory* as opposed to Russell's original *Ramified Type Theory* (which distinguished sets both as to types, and as to levels of definition).

Now if one accepts the platonist philosophy in set theory, the totality $\mathcal{P}(\mathbf{N})$ of subsets of $\mathbf{N}$ exists independently of how its objects may be defined, if at all. According to this view any formula $P(x)$ involving variables ranging over $\mathbf{N}$ and variables ranging over $\mathcal{P}(\mathbf{N})$, connected by arithmetical relations between individuals and relations of membership between individuals and sets, has a definite meaning, independent of whether we have any means to "determine" it; then the definition $X = \{x : P(x)\}$ simply serves to single out one member of $\mathcal{P}(\mathbf{N})$. According to the platonists, this is entirely analogous to singling out a natural number as the minimum one satisfying a certain (nonempty) property, formally $k = (min\, n)P(n)$, where we may have no way to determine k effectively, even when P only involves numerical quantification. But according to the predicativist (or "definitionist") there *is* a difference: the totality of natural numbers is granted as clear and definite and each of its members can be singled out by a prior representation, while there is no justification in the assumption of a totality of subsets of $\mathbf{N}$ independent of how these may be defined, since sets can be introduced only by definition on the basis of (successively) established totalities.

To return to Weyl, we can say that he positioned himself as follows in *Das Kontinuum*.[21] First, he rejected the platonist philosophy of mathematics as manifested in the impredicative existence principles of axiomatic set theory, though he accepted classical quantifier logic when applied to any established system of objects. Second, he accepted the predicativist viewpoint taking the system of natural numbers as its point of departure. Third, he recognized that, whatever their justification on predicative grounds, ramified theories would not give a viable account of analysis. Weyl's main step, then, was to see what could be accomplished in analysis if one worked just with sets of level 1, in other words, only with the principle of arithmetical definition. Here it is to be understood that such definitions may be *relative*; that is, if $P(x, Y_1, \ldots Y_n)$ contains variables $Y_1, \ldots, Y_n$ but no quantified set variables, then P serves to define $X = \{x : P(x, Y_1, \ldots, Y_n)\}$ relative to $Y_1, \ldots Y_n$. Given any specific definitions of $Y_1, \ldots, Y_n$ this will produce by substitution in P a specific definition of X. Finally, such means of relative arithmetical definition serve to explain which *functions* from sets to sets are to be admitted, namely, just those given as $F(Y_1, \ldots, Y_n) = \{x : P(x, Y_1, \ldots, Y_n)\}$ with P arithmetical.

In this way, Weyl was able to set up a formal axiomatic framework for *arithmetical analysis* in which he could define rational numbers in terms of (pairs of) natural numbers, then real numbers as certain sets (namely, lower Dedekind sections) of rational numbers, and finally functions of real numbers as functions from sets to sets in the way just explained. He then went on to examine which parts of classical analysis could be justified on these grounds. To begin with, the general *least upper bound (l.u.b.) principle for sets of real numbers* could not be accepted in its full generality. According to that principle, any set S of real numbers which is bounded above has a l.u.b. X. In Weyl's framework, S is given as consisting of lower Dedekind sections Y satisfying an arithmetical con-

[21] See also his elaboration of this position in Weyl (1919).

dition $P_1(Y)$; then its supposed l.u.b. X is the union $\bigcup Y[P_1(Y)]$; that is, $X = \{r \in \mathbf{Q} : \exists Y(P_1(Y) \wedge r \in Y)\}$. Translating rational numbers to natural numbers, this gives $X = \{n : (\exists Y)P(n, Y)\}$ for suitable arithmetical P; but this is an impredicative definition which is not justified in Weyl's system. On the other hand, the *l.u.b. principle for sequences of real numbers* does hold in his system. For, a sequence $\langle Y_k \rangle_{k \in \mathbf{N}}$ is given by an arithmetical condition $P_1(r, k)$, which holds just in case $r \in Y_k$. If the sequence is bounded above, its l.u.b. X is the union $\bigcup Y_k[k \in \mathbf{N}]$, that is, $X = \{r \in \mathbf{Q} : (\exists k)P_1(r, k)\}$, and this reduces to an arithmetical definition of the form $X = \{n : (\exists k)P(n, k)\}$ with P arithmetical.[22] Thus Weyl's task was reduced to seeing which parts of classical analysis rest simply on the l.u.b. principle for *sequences* of reals rather on the more general l.u.b. principle for *sets* of reals. Here, in fact, he found that the entire theory of *continuous functions* of reals goes through in a straightforward manner in arithmetical analysis, including for such (i) the intermediate value theorem, (ii) the attainment of maxima and minima on a closed interval, and (iii) uniform continuity on a closed interval.[23] From these it is a direct step to develop the theory of *differentiation and integration* for continuous functions. Here it is Riemann integration that can be treated in a straightforward predicative way. (Weyl remarks[24] that it is less simple to deal with the more modern theories of integration through theories of *measure*, but gives no indications; their treatment would be left for the future developments of predicative analysis [outlined in chapters 12–14 in this volume].)

There is no mention of Brouwer or intuitionism, and no restriction on the logic employed, in *Das Kontinuum*. However, within the next few years Weyl became more familiar with Brouwer's work and somewhat of a convert. He is quoted as saying, during some lectures on Brouwer's ideas in 1920: "I now give up my own attempt and join Brouwer."[25] Over the following years Weyl was to help champion Brouwerian intuitionism as against Hilbert's program in various publications, much to Hilbert's annoyance. However, in later years he became pessimistic about the prospects for the Brouwerian revolution. Moreover, it is not true to say that Weyl ever completely gave up his "own attempt," which he continued to mention over the years in articles on foundational matters, where his criticisms of mathematical platonism in set theory remained constant. A relatively mature expression of Weyl's view is provided by his 1949 book, which is

[22] Actually, Weyl establishes *Cauchy's convergence principle* for sequences of reals, which is equivalent to the l.u.b. principle for sequences. Note also that the l.u.b. principle for sets (sequences) is equivalent to the g.l.b. principle for the same, and with the given representation of real numbers this leads to a definition of the g.l.b. as an intersection rather than a union.

[23] As sketched in Weyl (1918), pp. 61–65. [Cf. also chapters 13 and 14 in this volume.]

[24] *Ibid.*, p. 65.

[25] See van Heijenoort (1967), p. 480; the original statement may be found in Weyl's *Gesammelte Abhandlungen* (1968), vol. 2, p. 158.

readily accessible.[26] The following quotations from this book reveal both his settled convictions as well as his unsettled state of mind about the eventual foundations of mathematics:

> The leap into the beyond occurs when the sequence of numbers that is never complete but remains open toward the infinite is made into a closed aggregate of objects existing in themselves. Giving the numbers the status of ideal objects becomes dangerous only when this is done. . . . The vindication of this transcendental point of view forms the central issue of the violent dispute . . . over the foundations of mathematics.[27]
>
> From an aggregate of individually exhibited objects we may by selection produce all possible subsets and thus make a survey of them one after another. But when one deals with an infinite set like **N**, then the existential absolutism for the subsets becomes still more objectionable than for the elements.[28]

There follows an explanation of the vicious circle principle, of levels of predicative definition, and of the resulting "dilemma" for analysis concerning the l.u.b. principle:

> Russell, in order to extricate himself from the affair, causes reason to commit harakiri, by postulating the above assertion [the Axiom of Reducibility] in spite of its lack of support by any evidence. . . . In a little book *Das Kontinuum*, published in 1918, I have tried to draw the honest consequence and constructed a field of real numbers of the first level, within which the most important operations of analysis can be carried out.[29]
>
> Mathematics with Brouwer gains its highest intuitive clarity. He succeeds in developing the beginnings of analysis in a natural manner, all the time preserving the contact with intuition much more closely than had been done before. It cannot be denied, however, that in advancing to higher and more general theories the inapplicability of the simple laws of classical logic eventually results in an almost unbearable awkwardness. And the mathematician watches with pain the larger part of his towering edifice which he believed to be built of concrete blocks dissolve into mist before his eyes.[30]

Finally, "Hilbert's mathematics may be a pretty game with formulas . . . but what bearing does it have on cognition, since its formulas admittedly

[26] That is a revised and considerably augmented English edition of an article Weyl wrote for the *Handbuch der Philosophie* in 1926.

[27] Weyl (1949), p. 38.

[28] *Ibid.*, p. 49.

[29] *Ibid.*, p. 50.

[30] *Ibid.*, p. 54.

have no material meaning by virtue of which they could express intuitive truths?"[31] In this connection, Weyl says that a consistency proof of arithmetic "would vindicate the standpoint taken by the author in *Das Kontinuum*, that one may safely treat the sequence of natural numbers as a closed sequence of objects."[32]

Incidentally, a famous wager was made in Zürich in 1918 between Weyl and George Pólya, concerning the future status of the following two propositions:

(1) Each bounded set of real numbers has a precise upper bound.
(2) Each infinite set of real numbers has a countable subset.

Weyl predicted that within twenty years either Pólya himself or a majority of leading mathematicians would admit that the concepts of number, set, and countability involved in (1) and (2) are completely vague, and that it is no use to ask whether these propositions are true or false, though any reasonably clear interpretation would make them false (unless the concepts involved were to acquire totally new meanings). The loser was to publish the conditions of the bet and the fact that he lost in the *Jahresberichten der Deutschen Mathematiker Vereinigung*; this never took place as such.[33]

Weyl's viewpoint in making this wager is often mistakenly identified as being that of Brouwer's intuitionism, though it was made at the time of publication of *Das Kontinuum*, prior to Weyl's taking up Brouwer's views. As we have seen, the l.u.b. principle was rejected in his 1918 publication on the grounds of the vicious circle principle and the rejection of impredicative definitions. And (2) requires some form of the Axiom of Choice applied to subsets of $\mathbf{R}$ for its proof, so was rejected by Weyl along with his rejection of the conception of $\mathbf{R}$ as a completed totality. As to the settlement of the wager, there is no question that Weyl lost under its stated conditions. Moreover, the *vast* majority of mathematicians (leading or otherwise) would say then, as they would say now, that Weyl's side of the bet was simply wrong-headed. But Weyl would not be shaken by this: "The motives are clear, but belief in this transcendental world taxes the strength of our faith hardly less than the doctrines of the early Fathers of the Church or of the scholastic philosophers of the Middle Ages."[34] So, the "Middle Ages" are

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[31] Weyl (1949), p. 61.

[32] *Ibid.*, p. 60. For elaboration of Weyl's views, the reader should not overlook Appendix A of Weyl (1949), and see further his 1944 obituary article "David Hilbert and his mathematical work," reproduced in Reid (1970), pp. 245–283, as well as Weyl (1946). [Cf. also chapter 13 in this volume.]

[33] The document spelling out the bet was reproduced by George Pólya in a brief note (1972). According to Pólya, "The outcome of the bet became a subject of discussion between Weyl and me a few years after the final date, around the end of 1940. Weyl thought that he was 49% right and I, 51%; but he also asked me to waive the consequences specified in the bet, and I gladly agreed." Pólya showed the wager to many friends and colleagues and, with one exception, all thought that he had won.

[34] Weyl (1946), p. 6.

simply taking somewhat longer to wane than Weyl expected—but that's the way it goes with Middle Ages.

The Rise of Metamathematics and the Crumbling of Hilbert's Program

Hilbert's emphasis on the axiomatic approach to mathematics and on the metatheoretical questions concerning axiom systems constituted one of the principal sources for the development of metamathematics as a new, distinctive, and coherent subject. Here "meta" has taken the meaning "about"; that is, axiom systems are objects of study examined externally. Such a treatment fits very well with axiomatic mathematics in its traditional sense, as first exemplified in geometry, but also with modern axiomatizations of number theory, algebra, and topology; each of these deals with a restricted or "local" part of mathematics. The metamathematical questions that Hilbert raised about axiom systems concerned their consistency, completeness, categoricity, and independence (of the axioms, one from another). In his program for the foundations of mathematics via finitist proof theory, Hilbert imposed further requirements on how the investigation of such questions was to be carried out, but those restrictions are not essential to metamathematics and only influenced part of its development. What *is* essential is that axiom systems are to be described precisely in formal terms so that results concerning them may be established rigorously. The term "metamathematics" then is suitable for the study in each case of that part of mathematics formalized in a given axiom system. But this is already troublesome, insofar as such study is to be carried out by informal mathematical means which may themselves be formalized. This novel feature is exactly what was capitalized on in the first striking results of metamathematics, namely, the Löwenheim-Skolem theorem and Gödel's incompleteness theorems; these, furthermore, combined to undermine both Hilbert's conception of mathematical existence and his finitist program for establishing "existence" via consistency proofs.

The following is devoted primarily to the highly discomfiting (if not devastating) effects on Hilbert's program of several metamathematical results of a general character (including those just mentioned). At the same time, these results served to undermine the programs for the universal or "global" axiomatization of mathematics initiated by Frege and carried on in the work of Russell, Zermelo, Fraenkel, and others. In contrast to the axiom systems for restricted areas of mathematics of the sort described above, here one aimed at systems of such generality that "all" mathematics could be formalized within them. As such, the idea of investigating these systems from the outside was antithetical to the motives for their creation,[35] but as

[35]This point has been emphasized particularly by van Heijenoort; cf., for example, his 1967 essay reproduced in van Heijenoort (1986), pp. 13–14.

we shall see, the claims to universality could not withstand the onslaught of metamathematics.

The Löwenheim-Skolem Theorem

In 1915 Leopold Löwenheim established a theorem which is now stated as follows: if a formula A of the first-order predicate calculus has a model (is satisfiable in some domain), then it has a denumerable model. This was improved by Thoralf Skolem in 1920 (and again in 1922) both in the proofs and in the statement of results. Skolem's theorem tells us that if S is any set of first-order formulas which has a model, it has a denumerable model. The Löwenheim-Skolem theorem was derived later (1930) as a consequence of Gödel's completeness theorem, by which if S is consistent in first-order predicate logic, then S has a denumerable model. For it is trivial that if S has a model at all, then it must be consistent.[36]

At first sight, Gödel's completeness theorem would seem to support Hilbert's dictum that existence of a mathematical concept is the same as consistency of an axiom system for it. But Hilbert had in mind axiomatizations S like that of Peano for the natural numbers and of Dedekind for the reals (as well as certain of his own for geometry) which are *categorical*, that is, such that the models of the axiom system are *uniquely* determined up to isomorphism. This implies that if M, M' are any two models, then they are equinumerous; that is, $M \sim M'$. But the Löwenheim-Skolem theorem shows that no set of first-order axioms S for the real numbers, that is, for which $\mathbf{R}$ is a model, can be categorical since S has a denumerable model while $\mathbf{R}$ is nondenumerable.

A later result by Skolem showed that no set of first-order axioms for $\mathbf{N}$ can be categorical; he did this by constructing a *nonstandard model* N' satisfying exactly the same first-order statements as $\mathbf{N}$.[37] A still more general theorem of Tarski showed that if a first-order set of statements S has a model M of any infinite cardinality m, then it has a model M' of any other infinite cardinality m'. So *no* set of first-order axioms S with an infinite model can be categorical.

Since Peano's original axioms for $\mathbf{N}$ and Dedekind's for $\mathbf{R}$ are categorical, they must have an essentially non-first-order component. In fact these are just, respectively, the axioms of induction for $\mathbf{N}$ and of completeness

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[36] The various basic papers of Löwenheim, Skolem, and Gödel may be found translated in van Heijenoort (1967). [The paper of Skolem dated 1922 in that source was not published until 1923 and appears as Skolem (1923a) in our bibliography; the reason given by van Heijenoort for the earlier dating is that Skolem had lectured on this material in 1922.] See also Gödel (1986). [Chapter 6 in this volume provides a survey of Gödel's life and work.]

[37] In the language of current model theory, two structures M, M' which provide interpretations of the same language are called *elementary equivalent*, and one writes $M \equiv M'$, if they satisfy the same first-order statements in that language. Then by a nonstandard model of the natural numbers is meant an N' not isomorphic to $\mathbf{N}$ with $\mathbf{N} \equiv N'$. Similarly a nonstandard model of the reals is an R' such that $\mathbf{R} \equiv R'$ but R' is not isomorphic to $\mathbf{R}$.

(or the l.u.b. principle) for **R** expressed set-theoretically. For example, in the case of Peano's axioms this takes the form

$$\forall X[0 \in X \wedge \forall n(n \in X \rightarrow (n+1) \in X) \rightarrow \forall n(n \in X)],$$

where the (set) variable "X" ranges over subsets of the domain. But for categoricity there is a further requirement, called that of *standard second-order logic*: in any interpretation of the axioms in a domain M, the set variables are to be interpreted as ranging over the set $\mathcal{P}(M)$ of arbitrary subsets of M.

Now this takes on a more puzzling aspect when we move to axioms for sets like those of Zermelo-Fraenkel, ZF (or even Zermelo's system Z). Since ZF is first-order, if it has a model M at all then it has a denumerable model M'. But any set A in M' must have an interpretation of its powerset $\mathcal{P}(A)$ in M', say $\mathcal{P}_{M'}(A)$. In ZF it is a theorem that if A is infinite then $\mathcal{P}(A)$ is nondenumerable. But in M', $\mathcal{P}_{M'}(A)$ has only denumerably many members. This peculiar situation is referred to as "Skolem's paradox" though it is not actually paradoxical; the puzzle is resolved by noting that *externally*, $A \sim \mathcal{P}_{M'}(A)$, since both sets are denumerably infinite. But *internally*, that is, within M', there is no function which can establish the one-to-one correspondence between A and the interpretation of $\mathcal{P}(A)$ in M'. In other words, the formal statement $\neg(A \sim \mathcal{P}(A))$ is still true in M'.

Now if we are to follow Hilbert's dictum of mathematical existence through in this context as well, the existence of set-theoretical concepts must be secured by the consistency of axiom systems like ZF. As we see, however, what would thereby be secured is not the intended concept but a variety of possible interpretations in models of different cardinality. The only way to secure the concept of set axiomatically is to add to the axioms of ZF a second-order "completeness" axiom referring to arbitrary subsets of the intended model M. This now puts us in a bind: one can't capture the notion of arbitrary set axiomatically without somehow already assuming the notion of arbitrary set to be understood.

There is still a final problem for Hilbert's conception of mathematical existence when combined with his finitist program. According to the latter, each axiomatic system must be prescribed finitarily: the syntax, axioms, and rules of inference are all to be determined by finitary effective procedures. For example, this is achieved in the axiom system PA by replacing Peano's second-order axiom of induction by the axiom scheme consisting of all formulas of the form

$$P(0) \wedge \forall n(P(n) \rightarrow P(n+1)) \rightarrow \forall n P(n),$$

where $P(n)$ is any formula in the first-order language of arithmetic using the basic symbols $(0, 1, +, \cdot,$ and $=)$. Now from a purely formal point of view, the statement of Peano's original axiom,

$$\forall X[0 \in X \wedge \forall n(n \in X \rightarrow (n+1) \in X) \rightarrow \forall n(n \in X)],$$

appears to be equally finitary. Indeed this is so, but being so does not suffice for categoricity; there is nothing in such a formal axiomatization within a second-order language that can force the system to be categorical. Only the *semantic* requirement that we are dealing with standard second-order logic, that is, that in any interpretation M, "X" ranges over $\mathcal{P}(M)$, will ensure categoricity. But that requirement is on its face nonfinitary; in fact, this is demonstrated as a consequence of the results with which we deal next.

Gödel's Incompleteness Theorems

In his famous paper "On formally undecidable propositions of *Principia Mathematica* and related systems," Gödel (1931) proved that for a wide class of effectively presented consistent extensions S of a formal system PM containing PA, there are elementary statements A such that neither A nor $\neg A$ is provable in S. For his proof, Gödel introduced a class of finitarily defined functions which are obtained from the zero and successor functions by explicit definition and inductive definition on $\mathbf{N}$.[38] The class of functions so generated is now called the *primitive recursive functions.* All such are effectively computable; through later work of Church, Kleene, Turing, and Post,[39] one arrived at a definition of the most general class of effectively computable functions on $\mathbf{N}$, which is now called the *recursive functions.*

Gödel's incompleteness results were achieved by first coding up syntactic objects (expressions, terms, formulas, proofs) as sequences (or sequences of sequences, etc.) of numbers and then representing such sequences $(m_1, \ldots, m_k)$ as single numbers n using the prime power representation $n = p_1^{m_1} \ldots p_k^{m_k}$ (this process is called *Gödel numbering*). Gödel then used this numbering to formally represent the syntax of the system PM within itself. More generally, he showed that if S is any extension of PM with a primitive recursive set of axioms (under its Gödel numbering) and S is (what he called) *ω-consistent*, then S is *incomplete*; that is, there are statements A such that neither A nor $\neg A$ is provable in S. Here ω-consistency is a mild technical extension of the usual concept of consistency. The hypothesis of ω-consistency was later replaced by that of simple consistency by an argument of Rosser,[40] who further strengthened Gödel's theorem so as to apply to any recursive system S extending PM. Gödel pointed out in 1931 that much weaker systems than PM served the same purpose. In fact, other later improvements gave the result that any recursive consistent extension S of PA is incomplete.[41]

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[38] In the simplest case, of a function of one argument, inductive definition takes the form $F(0) = c, F(n+1) = H(n, F(n))$, where H is a previously defined function; more generally, one defines functions $F(n, m_1, \ldots, m_k)$ by $F(0, m_1, \ldots, m_k) = G(m_1, \ldots, m_k), F(n+1, m_1, \ldots m_k) = H(n, m_1, \ldots, m_k, F(n, m_1, \ldots, m_k))$ where G and H are previously defined functions. Nowadays, one uses "recursive" for "inductive" in such definitions of functions.

[39] See Davis (1965).

[40] See his 1937 paper reproduced in Davis (1965).

[41] See Tarski, Mostowski, and Robinson (1953) for the history and still more general

One can say more about the kinds of statements A which are not decided by such S; they can be chosen in the form $\forall n(F(n) = 0)$ where F is a primitive recursive function. For primitive recursive S, Gödel arrives at this by showing that the relation $Proof_S(m, n)$ which expresses that n is the number of a proof in S of the statement with number m, is primitive recursive; that is, it has a primitive recursive characteristic function. He then constructs by a diagonal procedure a statement A such that

(1) $$A \leftrightarrow \forall n \neg Proof_S(\ulcorner A \urcorner, n)$$

is provable in S, where $\ulcorner A \urcorner$ is the number of A; informally speaking, A expresses of itself that it is not provable in S. By the above we obtain a primitive recursive function F such that $F(n) = 0 \leftrightarrow \neg Proof_S(\ulcorner A \urcorner, n)$, so

(2) $$A \leftrightarrow \forall n(F(n) = 0).$$

For this choice of A, Gödel shows that

(3) (i) if S is simply consistent then A is not provable in S, and
(ii) if S is ω-consistent then $\neg A$ is not provable in S.

This is Gödel's first incompleteness theorem. (The construction of A must be modified for Rosser's improvement to simple consistency in (ii)).

The consistency of S can be expressed as the statement that no proof in S ends in a contradiction; this may also be expressed by a statement $Cons$ of the form $\forall n(G(n) = 0)$, with G primitive recursive. By ordinary logic, consistency is equivalent to the statement that *some* formula is not provable in S; hence the statement $\forall n \neg Proof_S(\ulcorner A \urcorner, n)$ implies Con_S. Now (3)(i) expresses informally that the following formal statement is true:

(4) $$Con_S \rightarrow \forall n \neg Proof_S(\ulcorner A \urcorner, n).$$

Gödel succeeded in showing that not only is (4) true, but it is also provable in S—this, by formalizing the argument for (3)(i). Combining all of these facts it follows that

(5) if S is simply consistent then Con_S is not provable in S

(Gödel's second incompleteness theorem).

These results were both stunning and disappointing. Consider the latter aspect first: since, under the given hypothesis, the A constructed in (1) is not provable in S, then A is obviously true, for that is just what A expresses of itself. No statement of prior mathematical interest (such as the twin prime conjecture or the Riemann Hypothesis) has been shown

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results. The reduction to PA essentially makes use of Gödel's result from his 1931 paper that all primitive recursive functions are arithmetically definable, that is, definable in the first-order language having $0, 1, +, \cdot, =$ as basic symbols.

to be independent of S. Rather, the statement produced has simply been cooked up for the occasion, again by a diagonal trick. Even more, since A is obviously true, there is no question which of $A, \neg A$ we would add to S as an axiom if we were to try to overcome *this* instance of incompleteness; if S is to reflect truth in $\mathbf{N}$ then A should be added. Of course then $S' = S + \{A\}$ is again subject to the incompleteness theorem, but that is another matter.

What was stunning about the incompleteness theorems was how they put Hilbert's entire conception of mathematical existence and his consistency program into question. First of all, if one gives up categoricity as the requirement on an axiom system S for it to uniquely specify the concept of a mathematical structure (such as that of the natural numbers), still one might hope that all truths in the supposed structure would be derivable in S, in other words, that S would be complete. Indeed, Hilbert had said as much in the statement of Problem 2 in his (1900) list of problems (quoted above), and he later specifically conjectured that a formal system for arithmetic is complete. This was shown false by Gödel's first incompleteness theorem.

Second, Hilbert had divided the propositions of a formal system into those which are *real* and those which are *ideal*, the latter being used simply as an aid to derive the former. Among the "real" propositions would be all statements of the form $\forall n(F(n) = 0)$ where F is a finitarily defined function. Since Hilbert granted that all primitive recursive functions are finitarily defined, Gödel's results from (2) and (3) above showed that no one consistent formal system S could even serve to derive all true "real" propositions. Thus, even the fallback position from completeness for all statements to completeness only for the class of "real" statements gives way.

Third, Hilbert's call for proofs of consistency of axioms systems to secure the concepts they were supposed to express fails to secure the correctness of some of the most elementary theorems in those systems. For if A is a true statement of the form $\forall n(F(n) = 0)$ with F primitive recursive which is not provable in S, then $S + \{\neg A\}$ is consistent and proves the false statement $\exists n(F(n) \neq 0)$.[42] Still, it is at least the case that consistency of S (of the sort to which the incompleteness theorems apply) ensures that if S proves a "real" statement B of the form $\forall n(G(n) = 0)$, with G primitive recursive, then B must be true; for, otherwise, there would exist a specific k such that $G(k) \neq 0$, hence also $\exists n(G(n) \neq 0)$ would be provable in S, contradicting $\forall n(G(n) = 0)$.[43]

Finally, there are the unsettling consequences that Gödel's second incompleteness theorems ((5) above) have for Hilbert's program of finitary

[42]This was pointed out by Gödel in his first public announcement (in 1930) of the incompleteness theorems; see Gödel (1986), pp. 196–204.

[43]This point has been stressed particularly by G. Kreisel. See his article (1976) on Hilbert's Second Problem, in Browder (1976).

consistency proofs. Since all mathematics is supposed to be formalizable, this holds in particular for finitary mathematics. Now if all finitary methods can be represented in a single formal system S, then either S is not consistent or its consistency cannot be proved by finitary methods. A possible alternative is that no one consistent formal system can serve to formalize *all* of finitary mathematics, though any given portion can be formalized in one system or another. However, it is difficult to conceive how single global systems like PM or ZF could fail to formalize all finitary arguments. In fact, all the finitary proofs of consistency of various systems that had been carried out by workers in the Hilbert school were already formalizable in PA, and this explains why their best efforts failed with PA itself. Nevertheless, it remained conceivable that the use of finitary methods involving novel and/or more difficult arguments could serve to handle PA and eventually go on to succeed with systems like PM and ZF. Gödel himself was cautious on this point at the conclusion of his 1931 paper, but eventually he came to the view that Hilbert's conception of finitism *is* limited by PA.[44]

A basic problem here is that the general concept of finitary proof is not sufficiently clear that one can draw definitive conclusions about its possible limitations. Every actual finitary proof which had been carried out could be recognized as such, and most proofs of current mathematics are evidently nonfinitary, but this experience has not been sufficient to determine the concept in any sharp way. Beginning with Gerhard Gentzen in 1936,[45] workers in proof theory extended the evidently finitary methods employed for various consistency proofs to include certain *transfinite* elements, such as principles of induction on various effectively presented systems of ordinal notations, while hewing to Hilbert's requirement to use only "real" statements $\forall n(F(n) = 0)$, with F primitive recursive. [Cf. chapter 10 below.]

Postscript on Second-Order Logic

Gödel's 1930 completeness theorem showed that an effectively given system PC_1 of axioms and rules of inference for the first-order predicate calculus serves to prove just those statements which are valid in all models. His 1931 results could be used to show that there is no corresponding completeness theorem for second-order predicate calculus PC_2, if by validity is meant validity in the standard sense, that is, that in each interpretation M the set variables required are to range over $\mathcal{P}(M)$. Suppose to the contrary that PC_2 has an effectively given system of axioms and rules which proves just those statements that are valid in this sense. Then Peano's axioms with induction in its set-theoretical form are categorical when we use with them the logic of PC_2; call this system PA_2. Now since PA_2 is true in the structure of natural numbers, every theorem A of PA_2 is a truth about $\mathbf{N}$. Conversely, every truth A about $\mathbf{N}$ is equally a truth about any model

[44] See the introduction to Gödel (1958); this paper is reproduced in translation, together with a later extended, revised version (1972) in Gödel (1990).

[45] See Gentzen (1969).

isomorphic to **N**, hence any model of PA_2; so, finally, A is a theorem of PA_2. Thus PA_2 is formally complete: for every statement A in its language, either PA_2 proves A or it proves $\neg A$. Since PA_2 contains PA, and is supposed to be effectively given, the conclusion violates Gödel's first incompleteness theorem. This fulfills the promise made in the previous subsection on the Löwenheim-Skolem theorem, that second-order logic cannot be axiomatized effectively.

Elimination of the Law of Excluded Middle

For Hilbert, the completed infinite already made its appearance, albeit implicitly, in the use of instances of the Law of Excluded Middle (LEM) of the form $\forall n(F(n) = 0) \lor \neg\forall n(F(n) = 0)$; these in turn lead to $\forall n(F(n) = 0) \lor \exists n(F(n) \neq 0)$ and the method of proof by contradiction for simple existential results. This is what made PA problematic for Hilbert and brought him to call for a proof of its consistency. In *1930*, Arend Heyting set up a formal system of logic without LEM which was acceptable to the Brouwerians and has thus come to be called *intuitionistic logic*. One can associate with any axiomatic system S based on classical logic a corresponding system S^i based on intuitionistic logic while otherwise retaining the same mathematical axioms; obviously S^i is contained in S. The particular system PA^i is called *Heyting Arithmetic* and is denoted HA. By a straightforward argument in a paper of 1933 on the relationship between classical and intuitionistic arithmetic, Gödel showed that PA can be translated into HA in such a way as to preserve statements of the form $\forall n(F(n) = 0)$ with F primitive recursive. To be more precise, with each statement A of the language of PA (which is the same as that of HA) is associated a statement A' such that if PA proves A then HA proves A'. Moreover, for A of the form $\forall n(F(n) = 0), A' = A$.[46] Gödel's translation was subsequently extended to a wide variety of systems besides PA.[47]

Thus once more Gödel undermined Hilbert, who had stressed establishing the consistency of the *tertium non datur* (excluded third, or LEM) in the promotion of his program. Since the intuitionists said that they too reject the completed infinite, and since HA is intuitionistically acceptable, and since, finally, PA is reducible to HA by Gödel's result, what more would a finitary consistency proof of PA accomplish in the way of eliminating the "actual" infinite? Hilbert himself gave no answer to that question.

The Elusiveness of Cantor's Continuum Problem

With the general crumbling of Hilbert's program, his specific aim to use it in order to solve the continuum problem came, in the end, to nought.

[46]Gödel's result had a precursor in a similar one by Kolmogorov for the predicate calculus. The result for arithmetic was found independently by Gentzen and Bernays in papers withdrawn from publication when Gödel's work appeared; see Gödel (1986), p. 284.

[47]See the introductory note to his paper *ibidem*, especially pp. 284–285.

The alternatives then seemed to be either to follow Brouwer and Weyl and grant no definite meaning and no interest to the Continuum Hypothesis, or to take seriously the claims for the reality and cogency of set theory based on the platonistic vision of the set-theoretical universe. Apparently Gödel himself took this latter position early on, though he did not elaborate his view until much later, in the 1940s. One can find only a few scattered, brief indications thereof in his writings up to 1940.[48] One early sign of the direction in which his views pointed is in footnote 48a in Gödel's 1931 paper, evidently added as an afterthought:

> the true reason for the incompleteness inherent in all formal systems of mathematics is that the formation of ever higher types can be continued into the transfinite . . ., while in any formal system at most denumerably many of them are available. For it can be shown that the undecidable propositions constructed here become decidable whenever appropriate higher types are added. . . . An analogous situation prevails for the axiom system of set theory.[49]

The full extent of Gödel's platonism only began to emerge in his paper "Russell's mathematical logic" (1944). His attitude toward the Continuum Hypothesis was then spelled out more specifically in the article "What is Cantor's continuum problem?" (1947, and in revised and expanded form, 1964). There he stated in no uncertain terms his views that Cantor's *notion of cardinal number is definite and unique and that the Continuum* Hypothesis CH has a determinate truth value. His own conjecture was that CH is false, because of various "implausible" consequences it has. In any case, it was Gödel's conviction that it made sense to try to settle CH. At the same time, he acknowledged the failure thus far to come even remotely close to a solution. And finally, it was metamathematics that served to explain why it was proving so difficult to arrive at an answer. Indeed, once again, Gödel himself (had already) provided the first definitive results of that character, as follows.

Consistency of the Axiom of Choice and the Continuum Hypothesis

In a series of brief descriptions of results, 1938–1939, and finally at length in his 1940 monograph, Gödel proved the following result:

(1) if ZF is consistent then it remains consistent when we add to it the Axiom of Choice AC and the Generalized Continuum Hypothesis GCH, $\forall\alpha(2^{\aleph_\alpha} = \aleph_{\alpha+1})$, as new axioms.

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[48]See the discussion of Gödel's philosophy of mathematics in Feferman (1986), pp. 28–32 [reproduced in chapter 6 in this volume].

[49]Gödel (1986), p. 181.

Gödel's method of proof was to introduce a notion of "constructible set" in the language of set theory and to show that when the universe of sets is restricted to the constructible sets then all the axioms of ZF together with AC and GCH become validated. Another way Gödel had of putting this (in his system of sets and classes of his 1940 monograph), using V to denote the class of all sets and L to denote the class of all constructible sets, is that

(2) (i) if ZF is consistent then ZF + (V = L) is consistent, and

(ii) ZF + (V = L) proves AC and GCH.

Here the equation V = L expresses the assumption that all sets are constructible, which is shown to be true in the universe of constructible sets (a seemingly obvious proposition yet one whose precise statement requires some technical work to establish, because the relativization of the notion of constructibility to L must be shown to be *absolute*, that is, unchanged thereby).

As we have seen, almost all the work with cardinal arithmetic requires the assumption of AC. Once its consistency with ZF is established via (2)(i), (ii), it makes sense to speak of the scale of alephs $\aleph_\alpha$ and then to consider the truth value of $2^{\aleph_\alpha} = \aleph_{\alpha+1}$, for any and all α. But one still needs the stronger hypothesis V = L to prove GCH itself.

What Gödel's consistency result showed was that one could not hope to *disprove* AC, and that if AC is assumed, one could not hope to *disprove* any instance of GCH using Zermelo-Fraenkel Set Theory. It was still conceivable that ZF could *prove* AC, or that ZFC (= ZF + AC) could *prove* some instance of GCH, in particular, CH itself. Gödel himself made efforts to establish these results, with only partial success, and only concerning the independence of AC. In fact, no progress on this problem was made in over twenty years, despite the general expectation that both AC and CH would be independent, respectively, of ZF and ZFC.[50]

Independence of the Axiom of Choice and the Continuum Hypothesis

In 1963, Paul Cohen obtained the following results:

(3)(i) if ZF is consistent then AC is independent of ZF; that is, it cannot be derived from ZF;

(ii) if ZF is consistent then CH is independent of ZFC;

(iii) if ZF is consistent then V = L is independent of ZFC + GCH.

Cohen's method of proof[51] involved a novel technique for building models of set theory, called the method of *forcing and generic sets.* Where

[50] According to Gödel's views in his article of 1947 and 1964, AC is true and CH is false, so he would certainly expect independence of CH from ZFC.

[51] See Cohen (1966) for an exposition.

Gödel had restricted a presumed model of set theory to obtain that of the constructible sets, Cohen extended such a model by adjunction of (a variety of) "generic" sets. For example, by adjoining sufficiently many generic subsets of **N**, he was able to construct a model of ZFC in which $2^{\aleph_0} = \aleph_2$, thus contradicting CH.

Subsequently, building on Cohen's work, it was shown by Easton that for each regular $\aleph_\alpha$, the power $2^{\aleph_\alpha}$ "can be anything it ought to be"; that is, one can arrange the simultaneous equations $2^{\aleph_\alpha} = \aleph_{F(\alpha)}$ in a suitable model of ZF, for any function $F(\alpha) = \beta$, from ordinals to ordinals satisfying a few simple restrictions.

Consistency and Independence of Definable Well-Orderings

Gödel's consistency result for (V = L) and proof that $2^{\aleph_0} = \aleph_1$ holds under that assumption showed

(4) it is consistent to assume with ZFC that there is a definable well-ordering of the continuum **R**; in fact, this is provable in ZF + (V = L).

Behind (4) lies the definition of L as $\bigcup_\alpha L_\alpha$, where at each stage, L_α consists of the sets explicitly definable (in a predicative way) from the sequence of L_β for $\beta < \alpha$. (Gödel pointed out that this was an extension of Russell's predicative ramified hierarchy through all the transfinite ordinals.) Each constructible set is definable, and their definitions can be laid out in a transfinite sequence $\varphi_0, \ldots, \varphi_\alpha, \ldots$ without repetitions. Taking A_α to be the set in L defined by φ_α, this determines a definable well-ordering of all of L by $A_\beta < A_\alpha \leftrightarrow \beta < \alpha$. In particular, the restriction of this well-ordering to **R** (or to $\mathcal{P}(\mathbf{N})$) gives a definable well-ordering of **R** (resp. $\mathcal{P}(\mathbf{N})$) provably in ZF + (V = L).

Using Cohen's generic model of ZFC + GCH + (V $\neq$ L), I was able to show the following:[52]

(5) it is consistent with ZFC + GCH that there is no definable well-ordering of **R** (or $\mathcal{P}(\mathbf{N})$).

In other words, even if one assumes with ZF both the Axiom of Choice and the Generalized Continuum Hypothesis (and hence in particular $2^{\aleph_0} = \aleph_1$), one will not be able (provably) to arrive at any explicit definition of a well-ordering for the real numbers. It is in this sense that Hilbert's expressed hope in Problem 1 of his 1900 address cannot be realized.[53]

Of course Hilbert's hope could be satisfied if one granted the truth of V = L (or perhaps some similar axiom). By Cohen's work (3)(iii) the truth of V = L is not automatic if one accepts ZFC + GCH. In fact, most

[52] Feferman (1965).

[53] See footnote 6 above.

everyone who holds a platonist view of set theory denies the truth of V = L, for it is an axiom which says that every set is definable and, moreover, in a special way. But the restriction to definable objects is just opposite to the platonist position according to which the objects of set theory exist independent of any means of definitions. So, for the avowed platonist, there is nothing disturbing in (5).

New Axioms?

Gödel already projected in his 1947 paper that CH would be independent of ZFC, and that new axioms might be required to decide it; he developed these ideas further in the 1964 revision of that paper.

> For if the meanings of the primitive terms of set-theory . . . are accepted as sound, it follows that the set-theoretical concepts and theorems describe some well-determined reality, in which Cantor's conjecture must be either true or false. Hence its undecidability from the axioms being assumed today can only mean that these axioms do not contain a complete description of that reality. . . . [T]he axioms of set theory by no means form a system closed in itself but, quite the contrary, the very concept of set on which they are based suggests their extensions by new axioms which assert the existence of still further iterations of the operation "set of."[54]

The main kinds of new axioms thus suggested are called *axioms of infinity* or *large cardinal axioms.* The first of them asserts the existence of an *inaccessible cardinal* m. This is supposed to satisfy the properties (i) $\aleph_0 < m$, (ii) if $n < m$ then $2^n < m$, and (iii) if $card(A) < m$ and F is a function from A to cardinals less than m then $\sum_{x \in A} F(x) < m$. In other words, such m is a transfinite cardinal closed under exponentiation and summation over any smaller number of cardinals. It is not hard to show (in ZFC) that the assumption $(\exists m)$ "m is inaccessible" implies the existence of a model for ZFC, and hence the consistency of ZFC. Thus by Gödel's second incompleteness theorem, ZFC cannot prove $(\exists m)$ "m is inaccessible," if it is consistent.

The existence of inaccessible cardinals can be iterated further into the transfinite; that is, one may assume as a new and stronger axiom the statement that there is a strictly increasing sequence of inaccessible cardinals m_α, indexed by arbitrary ordinals α. Then one could go on to postulate the existence of inaccessible cardinals p such that whenever $\alpha < p$ then $m_\alpha < p$. The existence of such p cannot be proved under the assumption of the existence of the sequence of $m_\alpha s$. Now p is an inaccessible fixed point of the function $F(\alpha) = m_\alpha$, that is, $F(p) = p$, and it may be assumed that there is a function of ordinals, $F'(\alpha) = p_\alpha$, which enumerates

[54] Gödel (1964), p. 264 [or Gödel (1990), p. 260].

all such fixed points in increasing order. One is led in this way to notions of higher and higher levels of inaccessibility, which were first formulated in a systematic way by P. Mahlo in 1911; the cardinals of these types are thus called Mahlo cardinals. One is led correspondingly to stronger and stronger axioms of infinity; according to Gödel these "show clearly, not only that the axiomatic system of set theory as used today is incomplete, but also that it can be supplemented without arbitrariness by new axioms which only unfold the content of the concept of set."[55]

The study of large cardinal axioms has been carried on intensively since the 1960s, and by the introduction of new ideas vastly stronger axioms than those for the existence of Mahlo cardinals have been proposed: existence of "measurable" cardinals, "compact" cardinals, "supercompact" cardinals, etc. These involve extremely technical notions which can be understood only with somewhat advanced training in axiomatic set theory, so no attempt will be made to explain them here.[56]

There are two questions to be asked concerning the various existence statements for infinite cardinals that have been indicated here. First, what would lead one to accept them as axioms to be added to ZFC? Second, do they help decide the continuum problem? Concerning the first question, Gödel thought that these or other types of proposed new axioms need not "force themselves upon us" as being true in the same way as the axioms of ZFC, but that "a more profound understanding of the concepts underlying logic and mathematics would enable us to recognize [them] as implied by these concepts."[57] Here the final arbiter would be that of "mathematical intuition [which] need not be conceived of as a faculty giving an *immediate* knowledge of the objects concerned."[58] Gödel goes on to compare mathematical intuition with physical experience as a source of our ideas about underlying physical objects, but he is not specific as to how this intuition serves to decide between opposing theories of underlying mathematical objects in general, and between the existence or nonexistence of some huge cardinal in particular. While Gödel's remarks may be heartening to platonist set-theoreticians, they are too vague to be decisive in any particular case. Rather, their tendency seems to be that if one's mathematical intuition leads one to judge that a statement about sets is true, then it *is* true.

In a technical survey piece on large cardinal axioms, Kanamori and Magidor suggest the acceptance of such axioms on "theological" grounds, but have alternative arguments to engage those who are not already "true believers." For the latter, what is offered is an investigation of such statements on a purely formal level whose interest lies in the fascinating and

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[55] *Loc. cit.*

[56] For a substantial introduction to the subject see Drake (1974). [Cf. also the more recent comprehensive work, Kanamori (1994).]

[57] Gödel (1964), p. 265.

[58] *Ibidem*, p. 271.

"aesthetic" intricacy of the net of consequences and interrelationships between them.[59]

Others have attempted to provide an overall rationale which would make large cardinal axioms the consequence of some very general principles. Main attention has been given to forms of the set-theoretical *reflection principle*, according to which any property of the universe of all sets must already be true of a level C_α in the cumulative hierarchy of sets (where $C_0 = \emptyset, C_{\alpha+1} = C_\alpha \bigcup \mathcal{P}(C_\alpha), C_\lambda = \bigcup_{\alpha<\lambda} C_\alpha$ for limit λ, and the universe is the union of all $C_\alpha s$). Initially formulated by Azriel Levy for first-order set-theoretical properties, this principle was extended to include second-order properties in an elegant way by Paul Bernays, who showed that its assumption leads to the existence of the Mahlo cardinals. This line was continued in Reinhardt's "Remarks on reflection principles, large cardinals and elementary embeddings," which provides a useful introduction to and motivation for the approach. Reinhardt considers this work "a first step in recognizing axioms . . . which will make [them] seem worth considering as axioms rather than merely as conjectures or speculations."[60] On the other hand, in his first footnote to this paper he says that Gödel finds Reinhardt's own proposed justification "rather unsatisfactory" and that preferably an alternative one should be given in terms of some concept of structural properties of sets.

It is apparent that those who accept large cardinal axioms have some rationale or other for doing so (perhaps rationalization would be the better word), but are well aware that the whole enterprise may be put in question. One of the most forceful statements in opposition is that of Paul Cohen (1971) in his statement of reasons for rejecting the platonist realist position and accepting a formalist position in the set-theoretical foundations of mathematics.

> A weakness which I believe any realist would have to accept is his inability to explain the source of the never-ending sequence of higher axioms, such as the higher axioms of infinity. Certainly even the staunchest realist must flinch when contemplating cardinals of a sufficiently inaccessible type. Also there are axioms such as that of the Measurable Cardinal which are more powerful than the most general Axiom of Infinity yet considered, but for which there seems absolutely no intuitively convincing evidence for either rejection or acceptance.[61]

He goes on to accuse "zealous set-theoreticians" of a certain "opportunism" in the acceptance and/or pursuit of these axioms. In opposition to that view we have, in turn, the comments of Kreisel (1971), who agrees

[59] Kanamori and Magidor (1978), pp. 103–105. [Cf. also Kanamori (1994).]

[60] Reinhardt (1974), p. 205. [Cf. also Feferman (1996) for a discussion of Gödel's program for new axioms, as well as Feferman (1998).]

[61] Cohen (1971), pp. 11–12.

that those who work on large cardinals may have a "vested interest" in the subject, but only such people are "competent to judge axioms about them."[62]

What of Gödel's original hope, that new axioms and, specifically, large cardinal axioms could help decide the Continuum Hypothesis? Here the situation was surveyed in the report by Donald A. Martin on Hilbert's First Problem,[63] with a conclusion that is easily summarized: CH is consistent with and independent from every large cardinal axiom A that has been proposed as at all plausible.[64]

Besides large cardinal axioms, little has been offered as a new axiom which would be plausible and decide CH. In unpublished work, Gödel proposed axioms about "scales of functions" which he stated imply $2^{\aleph_0} = \aleph_2$, but his proof turned out to be wrong, and he withdrew the paper containing it. Still later, he proposed other axioms about scales which imply $2^{\aleph_0} = \aleph_1$ (also unpublished[65]). Informed opinion about both of these efforts is that they are very inconclusive. Indeed, the whole of Gödel's thought about the definiteness of the continuum problem and the program to find new axioms to decide CH has so far come to nought, somewhat to the chagrin of his followers. In the words of Martin,

> I have been assuming a naive and uncritical attitude toward CH. While this *is* in fact my attitude, I by no means wish to dismiss the opposite viewpoint. Those who argue that the concept of set is not sufficiently clear to fix the truth-value of CH have a position which is at present difficult to assail. As long as no new axiom is found which decides CH, their case will continue to grow stronger, and our assertion that the meaning of CH is clear will sound more and more empty.[66]

Metamathematics, of which Gödel was the first real master and clear-eyed practitioner, has thus brought the platonist position in set theory, of which Gödel was the foremost exponent, to a very embarrassing position. The continuum problem—to locate $2^{\aleph_0}$ in the scale of cardinals $\aleph_\alpha$ and, more specifically, to decide whether or not it is $\aleph_1$—is the very first chal-

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[62] Kreisel (1971). p. 194.

[63] Martin (1976).

[64] To be more precise, for each such A it has been shown that if ZFC + A is consistent then it remains consistent when we add either CH or its negation. The independence results make use of strong extensions of Cohen's forcing methods but even the consistency results require novel ideas, since Dana Scott (1961) showed that the existence of measurable cardinals implies $V \neq L$. There are large cardinal axioms that give some partial information about GCH. For example, Robert Solovay has shown that the existence of a compact cardinal implies $2^{\aleph_\alpha} = \aleph_{\alpha+1}$ for all sufficiently large cardinals of a special kind (cf. Martin 1976, p. 86).

[65] Cf. Gödel (1986), pp. 26–27. [Gödel's unpublished work on scales of functions is now to be found in Gödel (1995) as **1970a*, *b*, and *c*; cf. also the introduction to those items by Solovay, *ibid.*, p. 405ff.]

[66] Martin (1976), pp. 90–91.

lenging problem of Cantorian set theory. The fact that it has not been settled by any remotely plausible assumption leads me, for one, to agree with Weyl that it is an inherently indefinite problem which will never be "solved."[67] Of course, this conclusion will be difficult to accept for anyone who regards "ordinary" set theory (for example, ZFC) as perfectly reasonable and coherent and so tends to think of it as being about a fixed and definite world. I believe a quite different account can be given for the reasonableness and coherence of ZFC on the basis of a conception of an ideal world of sets in the cumulative hierarchy, much like the original conception of geometry as being about a world of ideal points (pure positions), ideal lines (perfectly straight and without thickness), etc. This conception of sets can be visualized well enough, just as for the conception of geometrical objects. Such an account would give grounds for the plausibility of the consistency of ZFC without assumption of its truth in some supposed real world of sets. One might even go farther to say that the picture of sets in the cumulative hierarchy is sufficiently clear that the portion C_ω of the cumulative hierarchy consisting of the hereditarily finite sets is well determined. Since the natural numbers are extracted from C_ω in usual developments of set theory, according to this picture every number-theoretical statement provable in ZFC is true. What is cloudy about the picture of the cumulative hierarchy is both the effect of the power set operation in general and the use of "arbitrary" ordinals. But in gross the picture is clear enough to justify confidence in the use of ZFC (and like theories) for deriving number-theoretical results.

[67] I am here excluding V = L from the "remotely plausible assumptions," since, as explained above, it is rejected by set-theoretical platonists as being contrary to the conception of a universe of arbitrary sets existing independently of any means of definition. One should also mention recent work of Freiling (1986), where certain simple "axioms of symmetry" are added to ZFC and used to disprove CH. These new assumptions are supposed to be consequences of a thought experiment involving throwing two (or more) random darts at the real line. They are plausible-looking, but I have not found any support for these assumptions among leading experts in set theory.

12

Infinity in Mathematics: Is Cantor Necessary? (Conclusion)

Is the Cantorian Transfinite Necessary for Finitary Mathematics? Gödel's Doctrine

By Gödel's doctrine I mean the view first enunciated in footnote 48a of Gödel (1931) that the "true reason" for the incompleteness phenomena is that "the formation of ever higher types can be continued into the transfinite," both in systems explicitly using types and in systems of set theory such as ZF for which the (cumulative) type structure is implicit in the axioms. For, as Gödel says, the "undecidable propositions constructed here become decidable whenever appropriate higher types are added." Since the undecidable propositions are of finitary character, Gödel's doctrine says in effect that the unlimited transfinite iteration of the power-set operation is necessary to account for finitary mathematics.

In order to discuss Gödel's doctrine in detail, we shall have to introduce various classifications of statements and formulas. In modern logical symbolism, statements of the form $\forall n(F(n) = 0)$ with F primitive recursive are in the class Π^0_1 and their negations $\exists n(F(n) \neq 0)$ in the class Σ^0_1. More generally $\Pi^0_k(\Sigma^0_k)$ is used for the class of statements having a primitive recursive matrix preceded by k alternating quantifiers of which the first is a universal (existential) quantifier, and in which all variables range over the natural numbers. The same classification is applied to formulas with free variables and to the relations which such formulas define in $\mathbf{N}$. The negation of a formula in $\Pi^0_k(\Sigma^0_k)$ is equivalent to one in $\Sigma^0_k(\Pi^0_k)$. A relation is said to be in class Δ^0_k if it is in both Π^0_k and Σ^0_k. The class Σ^0_1 is identical

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with that of the recursively enumerable relations, and thus, by a theorem of Post, Δ^0_1 consists exactly of the recursive relations. Π^0_∞ is used for the union of the classes $\Pi^0_k (k < \omega)$; every arithmetical formula is equivalent to one in Π^0_∞.

When second-order quantifiers with variables $X, Y, \ldots$ ranging over sets of natural numbers are introduced, a formula is said to be in $\Pi^1_k(\Sigma^1_k)$ form if it has an arithmetical matrix preceded by k alternating second-order quantifiers, beginning with a universal (existential) quantifier. The negation of a formula in $\Pi^1_k(\Sigma^1_k)$ is equivalent to one in $\Sigma^1_k(\Pi^1_k)$. Again, Δ^1_k is used for $\Pi^1_k \cap \Sigma^1_k$. In some cases it is preferable to use variables ranging over functions of natural numbers rather than over sets. The resulting classifications are essentially the same, using the equivalence of sets with their characteristic functions and of functions with their graphs (sets of ordered pairs coded as numbers using a primitive recursive pairing function $\langle n, m \rangle$).

If $\mathcal{F}$ is a class of formulas, the *comprehension axiom schema* (CA) for formulas in $\mathcal{F}$ is

$$(\mathcal{F}\text{-CA}) \qquad \exists X \forall n [n \in X \leftrightarrow P(n)],$$

where $P \in \mathcal{F}$ and P may have free variables (first or second order) besides "n." To express comprehension for properties definable by formulas both in $\mathcal{F}$ and in complementary form, we use

$$(\Delta_{\mathcal{F}}\text{-CA}) \qquad \forall n [P(n) \leftrightarrow Q(n)] \rightarrow \exists X \forall n [n \in X \leftrightarrow P(n)]$$

where $P \in \mathcal{F}$ and $(\sim Q) \in \mathcal{F}$ (up to logical equivalence). In particular, $(\Delta^1_1\text{-CA})$ is used for $(\Delta_{\Pi^1_1}\text{-CA})$. In should be noted that $(\Pi^0_\infty\text{-CA})$ follows from $(\Delta^1_1\text{-CA})$.

Gödel's doctrine is illustrated in the case of the extension of the first-order system PA of Peano arithmetic to a second-order language and axioms. Basically, one introduces axioms which are sufficiently strong to define the formal notion of truth for the first-order language and to establish the formal statement expressing that every statement provable in PA is true; this theory thus proves the consistency statement Con_{PA} which, as we have seen, is in Π^0_1 form.

Such an argument will be carried out in an extension of PA_2, that is, second-order PA, by suitable comprehension axioms. Here PA_2 has the same axioms as PA but with the induction scheme extended to apply to all second-order formulas $P(n)$. It is usual to take $(\mathcal{F}\text{-CA})$ as an abbreviation for the system $\text{PA}_2 + (\mathcal{F}\text{-CA})$. Incidentally, we shall deal later on with systems with restricted induction: $(\text{PA}_2){\restriction}$ has the induction schema replaced by the second-order axiom

$$\forall X [0 \in X \wedge \forall n (n \in X \rightarrow (n+1) \in X) \rightarrow \forall n (n \in X)],$$

and in general if S is any system with the full induction schema, $\text{S}{\restriction}$ is the corresponding system with the schema replaced by this axiom. $(\mathcal{F}\text{-CA}){\restriction}$

thus abbreviates $(\mathrm{PA}_2)\restriction + (\mathcal{F}\text{-}CA)$; this system includes PA as long as $\mathcal{F}$ includes Π^0_∞. For any system S, $\mathrm{S} \vdash A$ means that A is provable in S.

For the argument indicated above, the obvious step is to define the set of (Gödel numbers of) true statements as the smallest set satisfying certain arithmetical closure conditions; this requires (Π^1_1-CA). However, one can make do with less, namely, (Δ^1_1-CA). For one first defines a formula $Tr_1(X,k)$ which expresses that X satisfies the conditions to be the truth-set for the set St_k of statements of logical complexity $\leq k$. Then it is easily proved using (Π^0_∞-CA) that

$$(1) \qquad \forall X \forall k[Tr_1(X,k) \to \exists Y(Tr_1(Y,k+1) \wedge Y \cap St_k = X)].$$

Then $(\Pi^0_\infty - \mathrm{CA}) \vdash \forall k \exists! X Tr_1(X,k)$ by induction on k. Finally, one can define $Tr_1(a)$ in the two equivalent forms

$$St(a) \wedge \exists k \exists X[c(a) \leq k \wedge Tr_1(X,k) \wedge a \in X], \text{ and}$$
$$St(a) \wedge \forall k \forall X[c(a) \leq k \wedge Tr_1(X,k) \to a \in X]$$

where $c(a)$ = complexity of a. Thus

$$(2) \qquad (\Delta^1_1 - \mathrm{CA}) \vdash \exists X \forall a[a \in X \leftrightarrow Tr_1(a)].$$

With the existence of the truth-set thus established, one can go on to prove by induction in (Δ^1_1-CA) that every numerical instance of every formula provable in PA is true. Hence

$$(3) \qquad (\Delta^1_1\text{-}\mathrm{CA}) \vdash \forall a[St(a) \wedge Prov_{\mathrm{PA}}(a) \to Tr_1(a)].$$

Since $\neg Tr_1(\ulcorner 0 = 1 \urcorner)$, it follows that

$$(4) \qquad (\Delta^1_1\text{-}\mathrm{CA}) \vdash Con_{\mathrm{PA}}.$$

As we have seen, Con_{PA} is a true Π^0_1 statement not provable in PA assuming PA is consistent, so in this case adjunction of higher types leads to new results with finitary content.

This argument is paradigmatic: for any system S whose axioms are intuitively true, we can define a formal extension S^* of S using quantification at a next higher level and axioms analogous to full induction plus Δ^1_1-comprehension, for which we can establish

$$(5) \qquad \mathrm{S}^* \vdash Con_{\mathrm{S}}.$$

In the case of systems of set theory, we proceed in a variant manner. Most reasonable theories of sets S have natural models in the cumulative hierarchy; that is, for suitable α satisfying a special property $K(\alpha)$, the structure $(V_\alpha, \in \cap V_\alpha)$ is a model of S. For example, for S = ZFC and $K(\alpha) = Inacc(\alpha)$ (the property that α is an inaccessible cardinal), we have that V_α is a model of S whenever $K(\alpha)$ holds. For such S in general let $\mathrm{S}^* = \mathrm{S}$

$+ \exists\alpha K(\alpha)$. Then in $S^\star$ one can define the notion of truth in the structure $(V_\alpha, \in \cap V_\alpha)$ and prove that this is a model of S whenever $K(\alpha)$; hence (5) also holds in this situation. Thus Gödel's doctrine is verified in these cases by adjoining a "new type" V_α in the form of an assumption $\exists\alpha K(\alpha)$. (Here the implicit adjunction of new types is far more wasteful than the direct extension of a system by the adjunction of one higher type, since from the axioms of ZFC, once we have an ordinal α we have many ordinals β larger than α and thence the corresponding new type levels V_β.)

In this formal sense, then, Gödel's doctrine can be verified: by adding higher types to a system S either explicitly or implicitly we are able to prove new Π^0_1 statements, and by iterating this process transfinitely, we are led to ever stronger such statements. Of course this assumes that at each stage along the way, the system S is consistent. That will be the case if we start out with a system true for the natural numbers, and we accept that the iteration of the power-set operation leading to higher types has a well-determined meaning so that at each stage, the full comprehension axiom is validated (using the language available at that stage). In particular, both full induction and the analogue of the Δ^1_1-comprehension axiom schema will be true under that assumption and so the extension from S to $S^\star$ as above will preserve correctness and hence consistency. An analogous style of reasoning can be applied to the set-theoretical case.

However, Gödel's doctrine can be challenged when it is read as asserting that the platonistic view of the determinateness of the power-set operation and its iteration through all the ordinals is *necessary* for the derivation of *previously* undecidable but true Π^0_1 statements. Consider the situation at the outset: assuming one grants the correctness in an informal sense of PA under its intended interpretation in the structure of natural numbers, would not one immediately accept a predicate $Tr_1(x)$ with the appropriate closure axioms for truth in the first-order language, without regarding it as necessary to *define* this predicate in second-order terms? If so, extension of PA by these axioms (with induction extended to the new formulas) suffices to prove Con_{PA}. Call this extension $Tr_1(\mathrm{PA})$; it is a first-order theory which serves to decide the undecidable proposition constructed for PA. So why is the extension to a higher type necessary? To be sure, $Tr_1(\mathrm{PA})$ is again subject to the incompleteness results. But the same line of reasoning, which begins with an informal judgment of the correctness of PA under its intended interpretation and a recognition of the meaningfulness of the predicate $Tr_1(x)$ for the language of PA, can be repeated: one recognizes the correctness of $Tr_1(\mathrm{PA})$ and the meaningfulness of a truth predicate $Tr_2(x)$ for the language of $Tr_1(\mathrm{PA})$, and then the correctness of new axioms involving Tr_2 by means of which we can prove $Con_{Tr_1(\mathrm{PA})}$, and so on.

According to this line of thinking, it is equally reasonable to replace Gödel's doctrine by a variant doctrine, according to which the "true reason" for the incompleteness phenomena is that the truth definition for the language of a formal system is not expressible in that language (Tarski's theorem) and that by adjunction of the notion of truth with suitable axioms, the undecidable statements produced by Gödel become undecidable. In fact, even less will do: one need merely adjoin to S the formal *reflection scheme*

$$(6) \qquad Prov_S(\ulcorner A \urcorner) \rightarrow A, \text{ for each sentence } A,$$

as a means of expressing faith in the correctness of S without any new predicates at all. For, if one takes the specific instance of (6) with $A = (0 = 1)$, the consistency statement Con_S is a consequence of S. In other words, for $S^\star = S + \{Prov_S(\ulcorner A \urcorner) \rightarrow A : A$ any sentence of S$\}$, we have $S^\star \vdash Con_S$. So still another variant of Gödel's doctrine is that the "true reason" for the incompleteness phenomena is that though a formal system S may be informally recognized to be correct, we must adjoin formal expression of that recognition by means of a reflection principle in order to decide Gödel's undecidable statements. Either way, one has a reasonable alternative to Gödel's doctrine—the arguments for which are certainly no less persuasive than for his—and which does *not* claim the necessity to accept higher type notions *à la* Cantorian set theory.

These variant forms of Gödel's doctrine, which lead to the consideration of certain uniform extension procedures $S \mapsto S^\star$, may also be iterated into the transfinite. But here, if one is not to accept the transfinite in the Cantorian sense, ordinals must be understood and treated in a more constructive way. That has been carried out in the subject of *transfinite recursive progressions of formal systems*, initiated by Turing (1939) and continued in Feferman (1962) and, for predicative extension procedures, in Feferman (1964). The conclusion is that the incompleteness phenomena do not, by themselves, force one to accept the "formation of ever higher types . . . into the transfinite," that is, the transfinite iteration of the power-set operation. Application of Occam's razor would cut the discussion at that.

However, the platonist will still insist on a rejoinder: notwithstanding this counterargument to Gödel's doctrine, the systems of analysis (= full second-order CA) and of type theory and, further on, of ZFC and ZFC $+\forall\alpha\exists\beta(Inacc(\beta) \wedge \beta > \alpha)$ etc., are in fact *all true*, and for each of these S the consistency statement will be unprovable in S. Moreover, these statements Con_S go, in strength, far beyond those of the piddling extensions produced above by iterating formal reflection principles or truth definitions. So, according to this line, very strong set-theoretical principles are needed to decide statements of finitary character.

But isn't this argument begging the question? Sure, whatever leads one to accept "correctness" of such S will, with one little additional step, lead one to accept the consistency statement for S. But, as Brouwer repeatedly (and Gödel himself) emphasized, consistency does not by itself ensure correctness. While consistency of such systems may be plausible, the plausibility of such can (as I indicated at the end of chapter 2) rest on quite different grounds than the assumption of an underlying set-theoretical reality in its platonistic sense. In other words, unless one is *already* a set-theoretical platonist, Gödel's doctrine does not by itself provide compelling reasons to embrace the Cantorian transfinite.[1]

There remains the question: What, if any, relevance do the incompleteness phenomena have to finite combinatorial mathematics in the everyday sense of the word? There are a number of specific results in recent years which it is claimed establish that relevance. We turn next to a review of those results.

Undecidable Diophantine Problems

The results here, and the question of their relevance to everyday mathematics just raised, are comparatively easy to discuss. Diophantine equations are those of the form $p(x_1, \ldots, x_n) = q(x_1, \ldots, x_n)$ where p, q are polynomials in one or more variables with coefficients in $\mathbf{Z}$ (the integers). A Diophantine problem concerns whether there exists an integer solution of such an equation, that is, whether $(\exists x_1, \ldots, x_n \in \mathbf{Z})p(x_1, \ldots, x_n) = q(x_1, \ldots, x_n)$. Hilbert's Tenth Problem in his 1900 list called for an effective method to determine whether or not a Diophantine equation is solvable, that is, has any integer solutions [see chapter 1 in this volume for an introduction to Hilbert's Tenth Problem]. There is a closely related group of problems for solutions in $\mathbf{N}$, and for solutions in $\mathbf{Q}$. Specific such questions were first considered by the Greek mathematician Diophantus (third century A.D.); after a long lapse, the subject was revived by Pierre de Fermat in the seventeenth century and has been a staple of number theory ever since. Many specific Diophantine problems have so far resisted attack.* [Also it follows from the work described below that the (still unproved) Goldbach Conjecture, according to which every even integer greater than two is a sum of two primes, reduces to a Diophantine problem. It happens that Gödel frequently referred to his undecidable propositions as being of "Goldbach type."]

The first step toward a negative solution of Hilbert's Tenth Problem was made by Gödel in his 1931 incompleteness paper. He showed there that every primitive recursive function $F(x)$ is arithmetically definable, and hence can be put in the form

$$(1) \qquad F(x) = y \leftrightarrow Q_1 z_1 \ldots Q_n z_n R(x, y, z_1, \ldots, z_n),$$

[1]The viewpoint here concerning independence results for arithmetical statements should be compared with that of Isaacson (1987). He argues that the truths expressible in the first-order language of arithmetic which are not derivable in PA "contain essentially higher-order, or infinitary concepts." This is opposite to the position taken here, that one may be led to accept such results without assuming higher-type notions in the platonist sense. However, as regards the finite combinatorial independence results to be discussed in the next subsection, Isaacson and I agree that they are obtained by a transmutation of essentially metamathematical statements coded in the language of arithmetic.

*[At the time I wrote the article from which this chapter was drawn, the example given of the most famous such problem was Fermat's Last Theorem. This was subsequently settled by A. Wiles by very advanced methods; see chapter 1 in this volume.]

where each Q_i is the universal ($\forall$) or existential quantifier ($\exists$), the variables z_i range over $\mathbf{N}$, and R is built up by $\wedge, \vee$, and $\neg$ from polynomial equations. It follows that every Π_1^0 statement $\forall x F(x) = 0$ is likewise definable in that form. Gödel concluded from (1) that for each (ω-)consistent formal system S containing a sufficient amount of number theory, there are arithmetical propositions A which cannot be decided by S, that is, such that neither $\mathrm{S} \vdash A$ nor $\mathrm{S} \vdash \neg A$.

In the later development of the theory of effective computability at the hands of Church, Kleene, Turing, Post, and others, attention shifted from individual problems A undecidable relative to given formal systems S, to effectively undecidable problems for subsets C of $\mathbf{N}$, that is, for which there is no effective method to determine, given an arbitrary x in $\mathbf{N}$, whether or not $x \in C$. By Church's thesis, the effectively decidable sets C are exactly those which have a general recursive characteristic function, and then these are exactly the same as the Δ_1^0 sets. Church gave examples of recursively enumerable sets which are not recursive, in other words, which are in the class Σ_1^0 but not in Π_1^0.

Beginning in the 1950s, considerable progress was made on Hilbert's Tenth Problem through the work of M. Davis, H. Putnam, and J. Robinson. They succeeded in showing that if *variable* exponents are permitted, every Σ_1^0 set C is definable in the form

$$(2) \qquad x \in C \leftrightarrow \exists z_1, \dots, z_n[p(x, z_1, \dots, z_n) = q(x, z_1, \dots, z_n)],$$

where p, q are *exponential* integer polynomials. Finally, in 1970, J. Matiyasevich succeeded in establishing a result of the same form for *ordinary* integer polynomials; this finally showed the answer to Hilbert's Tenth Problem to be negative. Subsequently, Matiyasevich and Robinson succeeded in showing that it suffices to take $n \leq 13$ to represent as in (2) every recursively enumerable set with ordinary p, q. This and a number of other results concerning Hilbert's Tenth Problem are reviewed in Davis, Matiyasevich, and Robinson (1976). A few years later, Matiyasevich managed to reduce the number of variables in the representation (2) of recursively enumerable sets from 13 to 9 for ordinary polynomials, and even further to 3 variables for exponential polynomials.[2]

Now, returning to undecidable propositions, it follows by Gödel's results that for suitable consistent systems S of arithmetic, there are true Π_1^0 statements A of the form

$$(3) \qquad A = \forall x, z_1, \dots, x_n[p(x, z_1, \dots, z_n) \neq q(x, z_1, \dots, z_n)]$$

which are not provable in S; by formalizing the work described above, this can be reduced to $n \leq 9$ for ordinary integer polynomials p, q.

[2]For references to this further work see *Mathematical Reviews*, 81f: 03055 [and Matiyasevich (1993)].

Impressive as these results are, the problems they concern are still very distant from the bread-and-butter Diophantine problems of everyday number theory ("everyday" over the last three hundred years), because the number of variables is so much larger than is considered in such problems, and the complexities of the polynomials involved are so great (according to various measures of complexity). There is no evidence that these undecidability and incompleteness results have any relevance to the classic unsettled problems that have challenged generations of number theorists. [Besides the example of the Goldbach Conjecture mentioned above, it has been shown by Davis, Matiyasevich, and Robinson (1976) that the Riemann Hypothesis—which is one of the most important outstanding problems in number theory—reduces to a Diophantine problem. However, the numbers of variables and degrees of the polynomials required for this might be rather large.] Though the chain from the original undecidable propositions to the undecidable propositions of the form (3) is a long and technically complicated one (so that the point of departure recedes into the background), it remains the case that the problems thus shown to be undecidable were not of any prior mathematical interest—they have simply been derived in a step-by-step process from problems "cooked up" to demonstrate the incompleteness of formal systems. As a further reinforcement to the view here that the results on undecidable Diophantine problems are irrelevant to everyday mathematical concerns is that the character of these problems is insensitive to the formal system considered. If one takes A in (3) to be equivalent to Con_{PA}, there is no way to tell it apart from an A taken equivalent to Con_{ZFC} or from one which is equivalent to the consistency (with ZFC) of the existence of measurable cardinals. This suggests that the mathematical content of the resulting individual Diophantine problems simply has nothing to do with the internal mathematical content of the formal systems from which their independence is established.

How much different it would be if one showed that some outstanding number-theoretic Π_1^0 statement A [such as the Goldbach Conjecture or the Riemann , Hypothesis] is not provable in PA, or even more strikingly, in ZFC—and thus demonstrated why it's so difficult to prove it (if true)! There is nothing in the work on undecidable diophantine problems to suggest that one is anywhere near obtaining such a result—or anything comparable—nor that further efforts in chopping n down in (2) or (3) is going to get one any closer to achieving such a result. The situation at present is thus analogous to that concerning transcendental numbers with which we began this story in chapter 2; Liouville and Cantor showed how to construct (explicitly or implicitly) transcendental numbers, but mathematicians wanted to know whether the numbers π and e are transcendental. For those results the work of Liouville and Cantor was useless. All they did was demonstrate that the effort to show specific numbers to be transcendental numbers was not a waste of time, since such numbers do after all exist. Working number theorists want to know about the *truth* of specific number-theoretic problems which can be put in diophantine form. It is highly unlikely that one will simultaneously demonstrate the unprovability of, say, the Riemann Hypothesis RH from certain S *and* the truth of RH, as was the case with the nonprovable propositions produced by Gödel. But mathematicians would no doubt sit up and take notice if *merely* unprovability were established. All that the chain of work from Gödel to Matiyasevich permits one to say currently is that efforts to obtain such independence results for propositions of prior mathematical interest are not necessarily a waste of time.

Independence Results for Statements of Finite Combinatorial Character

Starting in the mid 1970s, a number of interesting independence results have been obtained with respect to a wide range of formal systems for statements which are prima facie relevant to finite combinatorial mathematics in its everyday sense. The expository article Simpson (1987a) provides an excellent introduction to this area of work, and has been quite useful to me in the following. Many of the results are "finitizations" P_{fin} of strong infinitary propositions P, where P implies P_{fin}. In some cases we even have P equivalent to P_{fin}. The classic example of this sort is *König's Infinity Lemma*, according to which if T is a finitely branching infinite tree then T has an infinite branch and (as is obvious) conversely. For T represented as a collection of finite sequences in N, the statement P that T has an infinite branch is Σ_1^1, while for T finitely branching the equivalent statement P_{fin} that it contains infinitely many nodes is Π_2^0, and even Π_1^0 for branching with fixed bounds.

As Simpson points out, König in his 1927 paper "Über eine Schlussweise aus dem Endlichen ins Unendliche" thought of his lemma as a method of obtaining infinitary results from finitary ones. Indeed, one of the most striking early applications of KL (König's Lemma) was Gödel's use of it in his proof of the completeness theorem for first order predicate logic: if a sentence A is consistent in that logic then it has a model.[3] The hypothesis of consistency is Π_1^0, but this is used to build a binary branching infinite tree whose nodes correspond to sequences of statements or their negations consistent with S. In this case we obtain a Σ_1^1 conclusion from a Π_1^0 hypothesis. The argument requires classical logic and can be carried out formally within PA, since the model constructed is arithmetically definable. (This situation is in a way a vindication of Hilbert's view discussed in chapter 2 that the completed infinite is already implicit in the use of the Law of Excluded Middle when combined with quantifier logic.)

Now, to continue with Simpson's point, the newer results discussed here proceed in the opposite direction. Starting with P which is independent

[3] Gödel did not explicitly acknowledge the use of König's Lemma; cf. the discussion in Gödel (1986), pp. 53–54, of his 1929–1930 work on completeness.

of a formal system S, the effort is to obtain a finitary consequence P_{fin} of P which is still independent of S. The first striking example of such was provided by the theorem of Paris and Harrington (1977), which is a modified form of the famous combinatorial theorem due to F. P. Ramsey in 1930. Ramsey's theorem concern partitions of the set of all k-element subsets of a set X, that is,

(1) $$[X]^k = C_1 \cup \ldots \cup C_\ell$$

where $[X]^k = \{Y : Y \subseteq X \text{ and } card(Y) = k\}$ and $C_1, \ldots, C_\ell$ are pairwise disjoint. We deal here only with denumerable X and may assume $X \subseteq \mathbf{N}$. The *Infinite Ramsey Theorem* P is the statement that if X is infinite then for any k and partition of $[X]^k$ as in (1), there exists an $i \leq \ell$ and an infinite subset of Y of X such that

(2) $$[Y]^k \subseteq C_i.$$

In the case $k = \ell = 2$ this is interpreted as saying that any infinite graph whose edges (that is, members of $[X]^2$) are colored by one of two colors (C_1 or C_2), there exists an infinite subgraph all of whose edges have the same color. Ramsey proved a finitary form P_{fin} of P which is as follows:

(3) for any k, ℓ, and m there exists n such that for any X with $card(X) = n$ and partition $[X]^k = C_1 \cup \ldots \cup C_\ell$, there exists $i \leq \ell$ and $Y \subseteq X$ such that $card(Y) \geq m$ and $[Y]^k \subseteq C_i$.

This statement can be seen to be a consequence of P by use of KL; if (3) is assumed false, then a finitely branching tree T can be constructed such that an infinite branch X through it violates the conclusion of the Infinite Ramsey Theorem. It turns out that this *Finite Ramsey Theorem* (3) can be proved in Peano Arithmetic PA. The surprising result found by Paris and Harrington is that a slightly modified form $P^{\star}_{fin}$ of (3) is independent of PA. This *Modified Finite Ramsey Theorem* differs from (3) only by addition, to the conclusion, of the requirement that $card(Y) \geq min(Y)$, where $min(Y)$ is the least element of Y. The argument that P implies $P^{\star}_{fin}$ can again be carried out by contradiction and use of KL (though, as pointed out by Simpson, this use of KL is inessential).

Since the work of Paris and Harrington, a number of other results of finite combinatory character have been shown to be independent of PA. One of the simplest is a theorem due to Goodstein concerning the effect of shifting bases in the representations of natural numbers to various bases b, which has been shown by Kirby and Paris to be independent of PA.[4]

It should be emphasized at this point that the statements P_{fin} thus shown independent of PA are recognized to be true by infinitary methods

[4]See Simpson (1987a) for details.

which go beyond PA. They can in fact be proved in the second-order system $(\Pi^0_\infty\text{-}CA)$ based on the arithmetical comprehension axiom with full induction. Moreover, P_{fin} is equivalent to the 1-consistency of PA (a notion intermediate between ω-consistency and consistency). Incidentally (as is discussed in the next and final section), the system $(\Pi^0_\infty\text{-CA})\upharpoonright$ with restricted induction proves the same arithmetical statements as PA.[5]

The system $(\Pi^0_\infty\text{-CA})$ is *predicatively justified*, since the second-order variables can be interpreted as ranging over the arithmetically definable subsets of $\mathbf{N}$. Schütte and I both analyzed in formal terms the transfinite iteration of the formal process of introducing sets by definitions referring only to previously determined collections of sets, where the iteration extends only to those ordinals corresponding to previously recognized well-orderings. The limit of these is a certain countable ordinal Γ_0.[6] This characterization of *predicativity* yields a sequence of *ramified systems of analysis* R_α each of which is predicatively justified for $\alpha < \Gamma_0$, but not for $\alpha = \Gamma_0$. In my 1964 paper and in several subsequent papers I produced a variety of single unramified systems S which are proof-theoretically of the same strength as $\bigcup R_\alpha[\alpha < \Gamma_\alpha]$ and hence are *locally predicative*.[7] Another such system of strength Γ_0 has been introduced and utilized by Harvey Friedman, and denoted ATR_0 by him; in our notation this is denoted $\mathrm{ATR}\upharpoonright$, since it uses restricted induction on $\mathbf{N}$. The locally predicative system $(\mathrm{ATR}\upharpoonright)$ is itself a relatively weak subsystem of the patently *impredicative* system $(\Pi^1_1\text{-CA})\upharpoonright$.

The point of all this here is that Friedman found a finite combinatorial version P_{fin} of an existing infinite combinatorial theorem P known as *Kruskal's Theorem*, such that P_{fin} is independent of $ATR\upharpoonright$ and hence cannot be proved by predicative methods. In this case Kruskal's proposition P concerns a certain (relatively simple) embeddability relation $\leq$ between finite trees; it says that the collection of such trees is "well-quasi-ordered" under $\leq$, that is,

(4) for any infinite sequence $T_1, T_2, \ldots, T_n, \ldots$ of finite trees there exist i, j with $i < j$ and $T_i \leq T_j$.

(In other words, there is no infinite descending sequence in this collection under the relation $<^\star$ defined by $T <^\star T' \leftrightarrow T' \not\leq T$). Note that (4) is equivalent to a Π^1_1-statement. Now the following is Friedman's finite form P_{fin} of Kruskal's theorem:

(5) For any k there exists n such that for any finite sequence $T_1, T_2, \ldots, T_n$ of trees with $card(T_i) \leq k \cdot i$ for all $i \leq n$, there exist i, j with $i < j$ and T_i embeddable in T_j.

[5] See Simpson (1987a) for details.

[6] Cf. Feferman (1964).

[7] See Feferman (1968a) for references.

This P_{fin} is a consequence of Kruskal's P; its proof goes by assuming (5) false and (once again) applying KL in a suitable way. Friedman showed that the statement (5) implies the 1-consistency of $\mathrm{ATR}\restriction$ and hence is not predicatively provable; it is, however, provable in the system $(\Pi^1_1\text{-CA})\restriction$. Friedman also found an extended finite form of Kruskal's theorem which is independent of $(\Pi^1_1\text{-CA})\restriction$ though provable in $(\Pi^1_1\text{-CA}) + \mathrm{BI}$, where BI is a principle described below. This makes use of labeled finite trees and a stronger embeddability relation. Details of the proofs of these independence results are in Simpson (1985).

At the time of writing the strongest subsystem of full second-order analysis $(\Pi^1_\infty\text{-CA})$ for which a finite combinatory independence result has been found is the system $(\Pi^1_1\text{-CA}) + \mathrm{BI}$, where BI is the scheme expressing that transfinite induction can be carried out (with respect to any second-order formula) along any well-founded ordering relation in $\mathbf{N}$. This result is due to Buchholz (1987); the statement shown independent concerns "hydra" games which involve specified alternations of amputation and regeneration of limbs in finite trees.

With reference to our original question as to the relevance of such independence results to finite combinatorial mathematics, I would say the following. None of the statements P_{fin} thus shown to be independent were previously considered per se in ordinary combinatorial work. It is a matter of pure speculation whether they would naturally have arisen for consideration in the normal course of such work, had they not come out of the efforts by logicians to establish the mathematical relevance of finitary results. At any rate, all these statements are at first sight close enough to the kinds of results which combinatorists *have* been dealing with and, conceivably, *could* have dealt with, so that there is at least a prima facie case for their relevance. Taking that for granted for the moment, what conclusions are to be drawn as to the significance of this work?

The most favorable interpretation to be placed on these results is that they tend to support Gödel's doctrine. The argument goes somewhat as follows. The system $(\Pi^1_1\text{-CA})\restriction$ is based on the impredicative comprehension axiom:

$$(6) \qquad \exists X \forall n[n \in X \leftrightarrow (\forall Y)A(n,Y)],$$

where A is arithmetical. Now (the argument continues), this system is justified only if one assumes that the power set $\mathcal{P}(\mathbf{N})$ exists as a fixed definite totality. Hence, according to this line of argument, it is necessary to make platonistic assumptions concerning the existence of uncountable totalities in order to derive finite combinatorial truths P_{fin} such as those, indicated above, independent of $(\Pi^1_1\text{-CA})\restriction$. In other words, according to this line of thought, at least the first stage of the Cantorian transfinite is necessary for everyday combinatorial mathematics.

Now there is an immediate counterargument to this particular effort to support Gödel's doctrine. Namely, although the infinitary statements P from which the finitary P_{fin} are derived as consequences clearly make use of impredicative principles, it turns out that the statements P_{fin} only require the (1-)consistency of such principles. And, as we have stressed above, one cannot argue from consistency (or even 1-consistency) to existence of the concepts in question, at least in their supposed "standard" intended sense.

There is a second line of response to the above argument, which makes use of a body of proof-theoretical work on reductions of the subsystems of analysis in question to constructive theories, in particular to various intuitionistic systems ID^i_α of iterated inductive definitions. ("ID" abbreviates "inductive definition," "i" is for "intuitionistic logic," and the ordinal subscript "α" measures the extent of the iteration.) This work is reported in Buchholz, Feferman, Pohlers, and Sieg (1981), which brings together a series of earlier contributions by the individual authors. In particular, it is demonstrated there that the system $(\Pi^1_1\text{-CA})\restriction$ is finitarily reducible to $\mathrm{ID}^i_{<\omega} = \bigcup_{n<\omega} \mathrm{ID}^i_n$, and that $(\Pi^1_1\text{-CA}) + \mathrm{BI}$ is finitarily reducible to ID^i_ω (in both cases preserving Π^0_2 sentences). Now the justification for the systems ID^i_α (for low α) is quite opposite to that for the classical subsystems of analysis which have been shown reducible to them. The systems ID^i_α concern specific countable sets (the recursive number classes) which are inductively generated "from below"; moreover, since only intuitionistic logic is used, it may be claimed there is no implicit appeal to the completed infinite in them. (This last is particularly clear for ID^i_1, where it turns out that Friedman's first finite form of Kruskal's theorem can be proved, but calls for argument in the case of ID^i_α when $\alpha \geq 2$.) At any rate, what the above reductions demonstrate is that the classical systems in question have an *alternative constructive justification* which does not require anything like belief in a preexisting totality of subsets of $\mathbf{N}$. Moreover, the same kind of alternative constructive justification has been given for far stronger theories than $(\Pi^1_1\text{-CA}) + \mathrm{BI}$, namely, for the system $(\Delta^1_2\text{-CA})$ in the work of Buchholz, Feferman, Pohlers, and Sieg cited above, and for $(\Delta^1_2\text{-CA}) + \mathrm{BI}$ in further work of Jäger and Pohlers.[8] At present, none of the independence results for finite combinatorial systems of the kind described above come close to exhausting the strength of systems for which we thus have a completely opposite (nonplatonist) constructive justification.

In the preceding discussion I have, for the sake of argument, taken for granted that the independent statements produced are indeed relevant to everyday combinatorial mathematics, even though they do not solve problems of prior mathematical interest. But even that may be challenged: it seems to me that as one passes from statements of the sort provided by Paris-Harrington and Kirby-Paris for PA, through those provided by Friedman for $\mathrm{ATR}\restriction$ and $(\Pi^1_1\text{-CA})\restriction$, on to those provided by Buchholz for $(\Pi^1_1\text{-CA}) + \mathrm{BI}$, the connection to practice becomes more and more tenuous. Viewed as an observer from the sidelines, there is a nagging feeling that the statements still have a "cooked up" look. This already appears with the use

[8]See Buchholz et al. (1981) for a general review of this work and further references.

in the Paris-Harrington statement of the condition $card(Y) \geq min(Y)$, and in Friedman's finite form of Kruskal's theorem of the condition $card(T_i) \leq k \cdot i$. Examination of the details of the argument in Simpson (1985) for the independence of the latter from ATR↾ makes clear how the imposition of the linear condition on growth allows one to pass from that statement (with the aid of König's Lemma) to the well-foundedness with respect to a wide-enough class of (possible) descending sequences to carry through a proof-theoretical demonstration of the 1-consistency of ATR↾. Note that the Paris-Harrington statement, which is most clearly relevant to results of established mathematical interest (finitary forms of Ramsey's theorem), does nothing to support Gödel's doctrine, since it can be proved in a mild, predicatively justified, extension of PA. But, in my view, the further one moves to support Gödel's doctrine by independence results for increasingly stronger systems, the less convincing is the case for the relevance of such.[9]

This section would not be complete without some discussion of the leap that has been taken in Friedman (1986). The effort there is to produce finite combinatorial statements P_{fin} independent of relatively strong systems of set theory, and in particular of ZFC + $\{$"(there exists an n-Mahlo cardinal)"$\}_{n<\omega}$. While the statement thus produced[10] does make use only of concepts from ordinary finitary mathematics, it is extremely remote from any ordinary mathematical reading. (Indeed, so much so that it would be unrewarding to reproduce the statement here—the reader must see for himself in Friedman's paper.) Here, I am evidently in complete disagreement with Friedman that his statement is "readily understandable." But Friedman himself goes on to say that this statement "is still not quite as simple or as natural as we would like [and] we are continuing to strive for an improved form."* What Friedman believes his result in this instance accomplishes is to "open up, for the first time, the realistic possibility, if not probability, that strong abstract set theory will prove to play an essential role in a variety of more standard finite mathematical contexts." If that case can be made then, according to Friedman, "this would open up a foundational crisis of nearly unprecedented magnitude since we seem to have no way of convincing ourselves of the correctness or consistency of such set theoretic principles short of faith in our very uneasy intuition about them."[11] In other words, Friedman is hesitatingly plumping for some form of Gödel's doctrine.

[9]In personal communications to me, both Friedman and Simpson have taken issue with the views expressed in this paragraph, particularly with respect to the comparison of the finite form of Kruskal's theorem given in the text and the Paris-Harrington statement (PH). I agree with them that the former statement is more readily understandable (or visualizable) than the latter, and also that cardinality restrictions on the sizes of finite trees are more natural than the ad hoc cardinality condition appearing in PH. However, this does not affect my overall conclusion at the end of the paragraph, which also concerns much stronger statements.

[10]Friedman (1986), proposition VIII.

*[His most recent efforts in that direction are presented in Friedman (1998).]

[11]The quotations are from Friedman (1986), pp. 92–93.

My general arguments against that doctrine in the last section, except one, apply equally to this specific case. Once more there is a *petitio principii*: would we believe Friedman's independent finitary statement to be true if we did not believe in the correctness of ZFC + $\forall n$ ("there exists an n-Mahlo cardinal") in some independent set-theoretical reality? So how does it justify the latter to demonstrate independence of the former from a slightly weaker system? Since the statement is clearly fabricated to do a job, and the evidence for its truth begs the question, the necessary use of such strong systems for everyday finitary mathematics has yet to be demonstrated. As I read Friedman's statements above, he agrees that the case has yet to be clinched; clearly, we differ on how much farther one will have to go in order to do that decisively, if at all. But my overall conclusion (announced in the introduction to chapter 2) about the independence results discussed in this section is even stronger: the case remains to be established that any use of the Cantorian transfinite beyond $\aleph_0$ is necessary for the mathematics of the finite in the everyday sense of the word.*

Countable Foundations for Applicable Mathematics

In this final section,** I shall explain the basis for the second claim announced in the introduction to chapter 2, that higher set theory is dispensable in scientifically applicable mathematics. The argument for this follows the trail blazed by Weyl in his 1918 monograph *Das Kontinuum*, but goes much farther by making use of modern logical tools. First of all, the work sketched by Weyl can be formalized in $(\Pi^0_\infty\text{-CA})\restriction$, which is a *conservative extension* of PA; that is, for any first order A, if $(\Pi^0_\infty\text{-CA})\restriction \vdash A$ then PA $\vdash A$. This conservation result can be proved very simply by use of Gödel's completeness theorem, as follows. If PA $\nvdash A$ then there is a model M of PA + $\{\neg A\}$; one then expands M to a model M' of $(\Pi^0_\infty\text{-CA})\restriction + \{\neg A\}$ by taking the range of the set variables to be the collection of all first-order definable subsets of M. There are also standard proof-theoretical techniques which may be used to prove this conservation result by strictly finitary means. Thus, the portion of classical mathematical analysis that can be formalized in $(\Pi^0_\infty\text{-CA})\restriction$, following Weyl's plan, rests on first-order Peano Arithmetic as a foundation. Since the general notion of real number is defined in the wider system, the conservation result shows that such uses of the uncountable in classical analysis can be eliminated.

Though Weyl did not himself carry his plan very far, the direction he set for it was clear enough to see how to formalize substantially all of the classical analysis of piecewise continuous functions of a real variable developed through the nineteenth century (and relatedly for complex analysis).

*[For a more recent discussion of the issues in this and the preceding sections, see Feferman (1998).]

**[The material of this section overlaps that of chapters 13 and 14 in this volume, whose writing it preceded.]

Since this part of analysis already includes a considerable portion of scientifically applicable mathematics, the execution of Weyl's program would go far toward substantiating the claim made above. However, one would also have to be able to provide a countable foundation for significant portions of twentieth-century analysis and accessory parts of mathematics in algebra and topology in order to establish the claim in full. Among other things one would want to be able to deal with the *Lebesgue integral* and more general theories of integration, as well as with a variety of *function spaces* and more general abstract spaces (*Banach spaces, Hilbert spaces,* etc.), and the operators on them, which form the subject matter of *functional analysis.* One way in which this might be done is to deal only with countably presented mathematical objects (functions, sets, structures, spaces), each of which is determined by a countable amount of information and hence can be represented by a subset of **N**. While this approach is possible in principle, it is not very natural. It is preferable to find a system S for which one can proceed in a direct way from a body of mathematical notions and theorems to the formalization of such in the system, and such that S can be given a countable foundation (as $(\Pi^0_\infty\text{-CA})\restriction$ is justified by PA). However, a system of this type would have to incorporate higher type notions. For, as soon as we wish to speak about *functions* of real numbers in a direct way in general, we must move to *third-order* concepts; then direct discussion of *functionals* (such as various linear operators on functions) would require dealing with (prima facie) *fourth-order* concepts, and so on.

Thus the aim would be to find a *finite type theory* S which can be given a countable foundation, say by proof-theoretical reduction to PA, and in which scientifically applicable twentieth-century mathematics can be directly formalized. The first part of this aim was realized in two ways by systems presented in Feferman (1977) and Takeuti (1978). But these systems are still not as convenient as one would like for the second part of the aim. The reason is that types are *fixed* syntactic objects in these systems, so there is no simple direct means to handle *subtypes* given by separating with respect to properties having variable parameters. A second reason for wanting *variable* types is that, following modern abstract mathematics, one wants to speak of arbitrary structures of any given algebraic, topological, or analytic kind and there is no natural sense in which these should be restricted to range over a fixed type. For example, one would like to speak of *all* groups (or all linear spaces, or all Banach spaces, etc.) and not just of all groups (etc.) within a given type. What is thus required is a formal system in which the types themselves may be *variable* and in which subtypes can be freely separated by suitable definitions. Such a system would approach set theory in appearance. Indeed, Friedman (1980) provided a theory ALPO which is contained in ZFC and is conceptually rich, yet is a conservative extension of PA. However, Friedman's system is semi-intuitionistic (in the sense that classical logic is applied in it only to number-theoretic equations).

Since the use of classical logic pervades modern mathematics, in my view it is preferable to have a formal system with variable types using classical logic in full and having a countable foundation, say by reduction to PA. Such a theory has been described in Feferman (1985a)[12]; it was there denoted Res-VT + (μ) but is denoted $\mathrm{VT}_\mu\upharpoonright$ in the following ("VT" abbreviates "Variable Types" and "μ" is for the unbounded minimum operator described below). While the formalism employs set variables, it differs from set theory in that the notion of function is treated as a primitive and not reduced to the notion of set. This conceptual separation is characteristic of certain approaches to constructive mathematics. At the same time it accords with the highly asymmetric use that working mathematicians make of functions and sets (in general, in practice, functions are not nearly as complicated as sets).

It is not possible here to go into details of the system $\mathrm{VT}_\mu\upharpoonright$, but the following sketches its main features. Besides class variables (or "type variables") $X, Y, Z \ldots$ there are class terms $U, V, W, \ldots$ built up from class constants and variables by the following operations: (i) if U, V are class terms then so also are $U \times V$ and $(U \to V)$; and (ii) if U is a class term and A is a bounded formula (defined below), then $\{x \in U : A\}$ is a class term. Individual terms are all introduced in context: for any class term U (which may contain variables) there is a list of individual variables $x^U, y^U, z^U, \ldots$ of type U; further terms are built from these and individual constants by pairing and projections (corresponding to $U \times V$), and by function application and abstraction (corresponding to $U \to V$). Thus we have the full means of a typed λ-calculus but with variable types. Unlike ordinary type theory, the formalism permits equations $s = t$ between terms of arbitrary type. Formulas are built from atomic formulas $(s = t)$ by $\neg, \wedge, \vee, \to$, and universal and existential quantification with respect to both class variables and (relative to any class term U), individual variables $\exists x^U(\ldots)$ and $\forall x^U(\ldots)$.[13] Bounded formulas are those containing no quantified class variables. (The preceding description of the syntax requires a simultaneous definition of class terms, individual terms and formulas). One defines $(t \in U)$ as $\exists x^U(t = x)$ and $\{x \in U : A\} = \{x^U : A\}, (\exists x \in U)A = \exists x^U A$, and $(\forall x \in U)A = \forall x^U A$.

The basic axioms of $\mathrm{VT}_\mu\upharpoonright$ are the usual ones for abstraction and application, pairing and projections, and separation. The further axioms concern $\mathbf{N}$ (a constant symbol for the set of natural numbers). This comes equipped with constants for 0 and the successor function $sc(x) = x'$, and a primitive recursor $r_\mathbf{N}$ which gives for each $a \in \mathbf{N}$ and $g \in \mathbf{N} \times \mathbf{N} \to \mathbf{N}$ a function $f = r_\mathbf{N}(a, g) \in (\mathbf{N} \to \mathbf{N})$ with $f(0) = a \wedge \forall n \in \mathbf{N}[f(n') = g(n, f(n))]$. From this we can derive primitive recursion with parameters drawn from

[12] This paper was presented to a meeting in Bogotá, Colombia, in 1981. The exceptional delay in publication of the proceedings of the meeting accounts for its date. [An improved system W for this purpose is described in chapters 13 and 14.]

[13] The system in Feferman (1985a) did not employ quantified class variables, using such only for pure statements of generality. The present extension is conservative over that system.

any classes. The axiom of induction for $\mathbf{N}$ is given in a form restricted to classes defined by characteristic functions:

(1) $f(0) = 0 \wedge \forall x \in \mathbf{N}(f(x) = 0 \rightarrow f(x') = 0) \rightarrow \forall x \in \mathbf{N}(f(x) = 0).$

Finally, there is an axiom for a function $\mu \in (\mathbf{N} \rightarrow \mathbf{N}) \rightarrow \mathbf{N}$ with

(2) $\mu(f) = \underset{x \in \mathbf{N}}{min}[f(x) = 0]$ if $(\exists x \in \mathbf{N})[f(x) = 0]$, otherwise 0;

μ is called the *unbounded minimum operator.* Using it we can define an operator $E_{\mathbf{N}}$ with $E_{\mathbf{N}}(f) = 0 \leftrightarrow (\exists x \in \mathbf{N})(f(x) = 0)$.

The main mathematical result obtained for this system is that

(3) $\mathrm{VT}_{\mu}\restriction$ is a conservative extension of PA.

The proof of (3) described in Feferman (1985a) proceeds by a series of reduction steps—first from variable types to constant types, then by elimination of subtypes, and finally by reduction to the system Res-$\hat{\mathrm{Z}}^{\omega} + (\mu)$ of Feferman (1977); it was there shown how to reduce the latter system to PA by proof-theoretical means. All of this argument is finitary; furthermore, conservation with respect to arithmetical statements is preserved at each step.*

While (3) shows that $\mathrm{VT}_{\mu}\restriction$ has a countable foundation, many sets which are ordinarily thought of as being uncountable can be defined to exist in the system. To prepare the way, $\mathbf{Z}$ is defined as usual in terms of $\mathbf{N} \times \mathbf{N}$, and $\mathbf{Q}$ in terms of $\mathbf{Z} \times (\mathbf{Z} - \{0\})$. Then the class $\mathbf{R}$ of real numbers is defined to be the class of all Cauchy sequences of rationals, that is, all functions from $\mathbf{N}$ to $\mathbf{Q}$ satisfying the Cauchy convergence criterion; an equality relation $=_{\mathbf{R}}$ is introduced to tell when these represent the same real number. The class $Fun_{\mathbf{R}}$ of all real functions is then defined to be the subclass of $(\mathbf{R} \rightarrow \mathbf{R})$ consisting of all f which preserve $=_{\mathbf{R}}$. We can then proceed to define the class of all functionals on these and so on. Of course the complex numbers $\mathbf{C}$ are defined as usual from $\mathbf{R}$ via $\mathbf{R} \times \mathbf{R}$. Since n-tupling can be explained in terms of pairing, one has all finite-dimensional spaces $\mathbf{R}^n$ and $\mathbf{C}^n$; one also has Cantor space and Baire space via $\mathbf{N} \rightarrow \{0,1\}$ and $\mathbf{N} \rightarrow \mathbf{N}$, respectively. All of these spaces may be shown to be *locally sequentially compact*; that is, every bounded sequence contains a convergent subsequence. (The proof uses König's Lemma, which in turn is proved by a combination of primitive recursion and the $E_{\mathbf{N}}$ operator.) The *Cauchy completeness* of these spaces follows. More generally one deals with arbitrary *complete separable metric spaces*, for which fundamental results like the Baire Category Theorem and the Contraction Mapping Theorem (existence of a fixed point) can be proved.

The space $\mathbf{R}$ as dealt with in $\mathrm{VT}_{\mu}\restriction$ cannot be proved complete in the sense that the l.u.b. (or g.l.b.) axioms holds for arbitrary *sets* of reals; one

*[The reduction to PA of the improved system W referred to in the postscript to ftn. 12 above is carried out in Feferman and Jäger (1993) and (1996).]

only has that for arbitrary *sequences* of reals. Since the l.u.b. (or g.l.b.) axiom for sets of reals is applied ubiquitously in analysis, one must in each case see whether such an application can be replaced by use of the corresponding principle for sequences. Often that is very easy but in some cases it requires more care; finally, there are certain cases in actual analysis where this replacement simply cannot be made. One such is when Lebesgue measure $meas(X)$ of a set $X \subseteq R$ is defined in terms of its outer measure $meas^{\star}(X)$, which is the g.l.b. of $\{meas(Y) : X \subseteq Y \text{ and } Y \text{ is open}\}$ (where for open Y, $meas(Y)$ is the sum of the lengths of the components of Y). Failing this, we cannot deal with nonmeasurable sets. But for applications it is only necessary to deal with *measurable sets* X, and for these a direct definition of measure is possible in terms of sequential open covers of X and of the complement of X. Similarly *measurable functions* can be dealt with directly in terms of limits (a.e.) of sequences of step functions. By this means we can represent the positive theory of Lebesgue measurable sets and functions (and similarly for more general theories of measure and integration).

Finally, one can develop substantial portions of functional analysis in $\mathrm{VT}_\mu \restriction$ for linear operators on separable Banach and Hilbert spaces, including the principal results for the spectral theory of compact self-adjoint operators. I have verified that usable forms of the Riesz Representation Theorem, Hahn-Banach Theorem, Uniform Boundedness Theorem, and the Open Mapping Theorem can all be proved in $\mathrm{VT}_\mu \restriction$. It seems, then, that all of *applicable classical and modern analysis* can be developed in this conservative extension of PA.

There is further detailed evidence which can be brought from another corner to support this claim. Namely, in his famous 1967 work, Errett Bishop gave a new constructive redevelopment of classical and modern analysis which he carried out in great detail for a number of fundamental results (many of which could be considered test cases). Bishop said that each of his theorems provides a constructive substitute $A^{\star}$ for a classical theorem A. The relationship is that A implies $A^{\star}$ and that $A^{\star}$ implies A by use of classical logic. In fact, according to Bishop the use of classical logic can be reduced in each case to that of what he called the Limited Principle of Omniscience (LPO), namely, that

$$(4) \qquad \forall n \in \mathbf{N}(f(n) = 0) \vee \exists n \in \mathbf{N}(f(n) \neq 0)$$

holds for each $f \in (\mathbf{N} \rightarrow \mathbf{N})$.[14] Now Friedman had found in 1977 a subsystem of intuitionistic ZF in which all of Bishop's constructive analysis (with a later form of his theory of measure) can be formalized, and which is conservative over HA. In my paper "Constructive theories of functions and classes"[15] I provided an alternative constructive theory of functions

[14] This explains Friedman's use, in his 1980 paper, of "ALPO" as an abbreviation for "Analysis with the Limited Principle of Omniscience."

[15] Feferman (1979).

and classes T_0, containing a subtheory $EM_0\restriction$ in which all of this work of Bishop can be formalized. I showed that $EM_0\restriction$ with classical logic is conservative over PA and Beeson showed its conservation over HA in intuitionistic logic.[16] The crucial point for the present purposes is that when classical logic is added, one regains from Bishop's work all the classical theorems A for which he found constructive substitutes $A^\star$, in a theory conservative over PA. (The system $EM_0\restriction$ is related in certain ways to $VT\restriction$, without the μ operator; addition of the latter builds in a strong form of LPO.) Actually as far as the claim here is concerned, the detour via Bishop's work takes one through unnecessary technical complications, since those are necessitated only by his insistence on constructivity. But at least the foregoing provides another way of giving massive detailed support for the main claim of this section.

Still further evidence for that has been gathered in the program of "Reverse Mathematics" initiated by Friedman and pursued by Simpson and others, which shows that even much weaker theories, conservative over PRA (Primitive Recursive Arithmetic), suffice for the development of significant portions of analysis and algebra.[17]

Nothing here is meant to suggest that a system S which makes prima facie use of uncountable sets of various kinds *must* be reduced to PA in order to show that higher set theory is eliminable in the applications of S. Any predicatively reducible system is a candidate for that and even, from a certain point of view (suggested above), are certain (prima facie) impredicative theories of iterated inductive definitions. Moreover, there is no question that in the current practice of mathematics there are a number of results in which the use of transfinite set theory is essential. While these results are far from the applications of mathematics, further efforts should be made to see whether any of them might lead to a crucial test case of the claim made here.

Independently of such detailed work which puts into question the necessity of higher set theory for everyday mathematics,* I am convinced that the platonism which underlies Cantorian set theory is utterly unsatisfactory as a philosophy of our subject, despite the apparent coherence of current set-theoretical conceptions and methods. To echo Weyl, *platonism is the medieval metaphysics of mathematics*; surely we can do better.

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[16] Feferman (1979), pp. 218–219.

[17] See Simpson (1985a), for an introduction and references [and, now, Simpson (1998)].

*[In this respect, cf. also the following chapters 13 and 14.]