A Tribute to Kurt Gödel

The Gödel Villa in Brno

Childhood portrait of Rudolf (left) and Kurt Gödel

Kurt and Adele Gödel (wedding portrait)

Gödel as a young scholar at the University of Vienna

Gödel and his mother Marianne with Oskar Morgenstern, taken by Dorothy Morgenstern

Honorary degree recipients at Rockefeller University (Gödel far right, Hao Wang third from right)

Einstein and Gödel, 13 May 1947, taken by Oskar Morgenstern

Gödel in his living room, Princeton

One of Gödel's high school report cards

Gödel at the blackboard

Oskar Morgenstern and Gödel, taken by Dorothy Morgenstern

Gödel in his office, taken by Finnish philosopher Veli Valpola

Photographs courtesy of John Dawson, the Institute for Advanced Study, and Mrs. Dorothy Morgenstern Thomas.

The Incompleteness Theorem

Martin Davis

In September 1930 in Königsberg, on the third day of a symposium devoted to the foundations of mathematics, the young Kurt Gödel launched his bombshell announcing his incompleteness theorem. At that time, there were three recognized "schools" on the foundations of mathematics: the logicism based on the work of Frege, Russell, and Whitehead that saw mathematics as simply part of logic, Brouwer's radical intuitionism, and Hilbert's proof theory (also called "formalism"). In fact two days earlier, lectures representing these schools had been delivered by Carnap, Heyting, and von Neumann respectively. Von Neumann may have been the only person in the room to have grasped the significance of what Gödel had done. He saw that the goals of Hilbert's proof theory had been shown to be simply unattainable. Logicism had also been dealt a death blow, but Carnap, who had known about Gödel's incompleteness theorem for over a week when he gave his address, seemed not to realize its significance.

Formalization of Mathematics

It was Gottlob Frege in his *Begriffsschrift* of 1879 who had shown how the logical reasoning used in mathematical proofs can be reduced to the combinatorial manipulation of symbols. By the 1920s foundational work had made it clear that the full expanse of classical mathematics could be encapsulated in such formal combinatorial systems. In these systems, a proposition of mathematics was represented by a string of symbols, and a proof, by a finite sequence of such strings. Since these systems were simple combinatorial objects, it seemed quite possible to apply mathematical methods to study their properties. Hilbert's program aimed to prove, by utterly unimpeachable methods, that these systems were consistent and complete: that they were safe from the catastrophic inconsistency, due to Russell's paradox, that had struck Frege's ambitious attempt to bridge the gap between the elements of formal logic and mathematics proper, and that with respect to some specified class of statements, each statement of the class could be either proved or refuted within the system. Gödel's incompleteness theorem did away with the second of these goals, and shortly thereafter Gödel was able to show that the first was likewise unachievable. Gödel's theorem had made it clear that no single formal system could be devised that would enable all mathematical truths, even those expressible in terms of basic operations on the natural numbers, to be provided with a formal proof.

Gödel's Proof

Gödel proceeded to define a code by means of which each expression of a formal system would have its own natural number, what has come to be called its *Gödel number*, associated with it. Thus, expressions of the system that represent propositions about the natural numbers might be seen by someone privy to the code as also making assertions, incidentally as it were, about the system itself. Working with a particular formal system loosely based on that of Whitehead and Russell and exploiting this idea, Gödel showed how to construct a remarkable expression of the system we

Martin Davis is professor emeritus of mathematics and computer science, New York University, and is a Visiting Scholar in mathematics at the University of California, Berkeley. His email address is `martin@eipye.com`.

may designate as U. To someone who didn't know the code, U would be seen as expressing a complicated and peculiar statement about the natural numbers. But to someone who could decipher it, U would be seen as also asserting that some statement expressible in the system is unprovable. Looking more closely, it would be found that the statement asserted to be unprovable is U itself. Thus we may say:

U asserts that it is unprovable.

Thus, if U were false, it would be provable, and hence, presumably, true. This contradiction shows that U is true, and hence, given what it asserts, unprovable. *There are true statements unprovable in the given system.*

Of course, this heuristic outline would have hardly been convincing. But Gödel carefully worked out the details leaving no doubt about the correctness of his conclusions.[1] Nevertheless, a whiff of paradox hung over the matter; it seemed hard to believe that a trick so close to puzzles usually offered for amusement could really be used to demonstrate something profound about mathematics.

Computability Theory Makes a Contribution

We write $N = \{0, 1, 2, \dots\}$ for the set of natural numbers. A function $f : N \to N$ is called *computable* if there is an algorithm that given an $x \in N$ will compute $f(x)$. Here the notion of algorithm is assumed to involve no restriction as to the amount of time or space required to complete a computation.[2] Finally a set $S \subseteq N$ is called *computable* if its characteristic function

$$C_S(x) = \begin{cases} 1 & \text{if } x \in S \\ 0 & \text{otherwise} \end{cases}$$

is computable.

The following is fundamental:

Theorem. *There is a computable function f whose range*

$$K = \{f(0), f(1), f(2), \dots\}$$

is not computable.[3]

[1] *Detailed proofs can be found in a number of textbooks, for example [3]. In addition Gödel's clear and meticulous original exposition [8] still repays study.*

[2] *Computability theory has provided a number of precise characterizations to replace this heuristic explanation, and they have all been proved equivalent to one another.*

[3] *See for example [1]. Computability theory is also known as recursion theory and used to also be called recursive function theory. Computable functions are also called recursive. Sets that are the range of a computable function as well as the empty set are called recursively enumerable, or more recently, computably enumerable, or listable.*

Computability theory provides a perspective from which it can be seen that incompleteness is a pervasive fundamental property not dependent on a trifling trick. From this point of view the formal systems studied by logicians are simply computable functions that spew out theorems (more precisely, Gödel numbers of theorems). Such systems are usually given in terms of a set of axioms and rules of inference. One can then imagine an algorithm that begins with the axioms and proceeds by iteratively applying the rules of inference.

To obtain a form of the incompleteness theorem let us begin with the set K whose existence is given by the theorem above, and consider propositions of the form $n \notin K$ where n is a fixed natural number. We can suppose that, in a particular formal system these propositions are each represented by a corresponding string of symbols we may write as P_n. We need only assume that there is an algorithm for obtaining P_n given n.[4] Let us use the symbol $\mathcal{F}$ for some formal system, and write $\vdash_{\mathcal{F}} P_n$ to mean that P_n is provable in $\mathcal{F}$. We will say that $\mathcal{F}$ is *sound* if

Whenever $\vdash_{\mathcal{F}} P_n$ for a particular n,
it will also be the case that $n \notin K$.

Since P_n is intended to stand for the proposition $n \notin K$, soundness simply means that the provable statements are true.

Incompleteness Theorem. *Let $\mathcal{F}$ be a sound formal system. Then there is a number n_0 such that $n_0 \notin K$, but it is not the case that $\vdash_{\mathcal{F}} P_{n_0}$.*

Again, we have a true sentence that is not provable. Note that we only succeed in changing the value of the particular number n_0 as we attempt to create stronger and stronger formal systems that can prove more and more.

Proof of the Incompleteness Theorem. Suppose that there is no such n_0. Then we would have:

$$\vdash_{\mathcal{F}} P_n \text{ for a particular } n, \text{ if and only if } n \notin K.$$

Recall that K is the range of the computable function f. Then the following would be an algorithm for computing $C_K(n)$ for a given value of n, contradicting the fact that K is not computable: Begin generating the theorems of $\mathcal{F}$ and at the same time begin computing the successive values $f(0), f(1), f(2), \dots$. If $n \in K$, then n will eventually show up in the list of values of f so $C_K(n) = 1$. Otherwise, P_n will eventually show up in the theorem list of $\mathcal{F}$ so that $C_K(n) = 0$. □

[4] *In a traditional formal system, for a given number n, P_n will be obtained by replacing, in a certain specific formula, a symbol for a variable by a "numeral" representing the number n.*

A Diophantine Perspective

The following result, known variously as MRDP and as Matiyasevich's Theorem, enables it to be seen that the truths unprovable in specified formal systems can have a straightforward mathematical form.

Theorem. *If S is the range of a computable function, then there is a polynomial $p(a, x_1, \ldots, x_m)$ with integer coefficients such that the equation $p(a, x_1, \ldots, x_m) = 0$ has a solution in natural numbers $x_1, \ldots, x_m$ for a given value of a if and only if $a \in S$ (see [9, 2]).*

Applying this result to the case $S = K$, let us call the corresponding polynomial p_0. Now we can think of the expressions P_n as standing for the proposition that $p_0(n, x_1, \ldots, x_m) = 0$ has no solutions in natural numbers, and say that $\mathcal{F}$ is *Diophantine-sound* if $\vdash_{\mathcal{F}} P_n$ implies that the equation $p_0(n, x_1, \ldots, x_m) = 0$ does indeed fail to have solutions. Then, the incompleteness theorem of the previous section takes the form:

Diophantine Incompleteness Theorem. *Let $\mathcal{F}$ be Diophantine-sound. Then there is a number n_0 such that the equation $p_0(n_0, x_1, \ldots, x_m) = 0$ has no solutions in natural numbers although it is not the case that $\vdash_{\mathcal{F}} P_{n_0}$.*

It is worth remarking that the proof of MRDP is entirely constructive so the polynomial p_0 could be produced quite explicitly.

Two Formal Systems: PA and ZFC

What Frege showed is that the ordinary reasoning in proofs of mathematical theorems amounts to formal manipulations of the propositional connectives $\neg \ \rightarrow \ \vee \ \wedge$ together with the quantifiers $\forall \ \exists$. Manipulations of the propositional connectives amounts to carrying out the operations of Boolean algebra. The quantifiers get in the way of this, and careful rules are needed to justify removing and reinstating them. Once these rules are specified (which can be done in a number of equivalent ways), the way is open to set up formal systems encapsulating greater or lesser portions of mathematics. This involves supplying a vocabulary of symbols representing various constants, functions, and relations appropriate to the part of mathematics being formalized. Finally a list of axioms must be given: these are written using this vocabulary together with the symbols listed above corresponding to the operations of logical inference. A symbol for equality should also be available.[5]

For the system PA (for "Peano Arithmetic"), the vocabulary consists of symbols for the number 0, and for the successor, sum, and product functions on the natural numbers. The axioms are the familiar Peano postulates together with equations serving to implicitly define sum and product. The induction postulate, whose informal statement is that a set of natural numbers containing 0 and closed under successor must consist of all natural numbers, appears in a restricted form: it is stated only for sets *definable* in terms of the vocabulary.[6]

PA formalizes the elementary number theory of the textbooks as well as (via clever coding) substantial parts of elementary analysis. In contrast ZFC formalizes the full scope of modern set-theoretic mathematics including such things as general topology and transfinite arithmetic. The vocabulary can be extremely parsimonious consisting only of the symbol $\in$ for set membership. For our purposes it will be useful to be slightly less frugal, allowing as well symbols $\varnothing$ (the empty set), $\{\ldots\}$ (the set consisting of a single element), and $\cup$ (binary union). The axioms are those of Zermelo-Fraenkel together with the axiom of choice, and the resulting system is powerful enough to encapsulate the full scope of classical mathematics, and indeed, much more (see for example [4]).

We write $\vdash_{\text{PA}}$ and $\vdash_{\text{ZFC}}$ for provability in PA and ZFC, respectively. We will use $\mathcal{F}$ as a subscript to refer ambiguously to either of these systems. Also we write $\nvdash_{\mathcal{F}}$ to express non-provability in the corresponding systems.

In each of PA and ZFC, a simple notation is available for representing the natural numbers by sequences of strings we call *numerals*. We will write $\overline{n}$ for the numeral representing the natural number n. In PA this may be defined as follows using the letter s for successor and letting 0 be represented by its usual symbol:

$$\overline{0} = 0\,; \ \overline{n+1} = s\overline{n}$$

Following von Neumann, the numerals in ZFC can be defined as follows:

$$\overline{0} = \varnothing; \ \overline{n+1} = \overline{n} \cup \{\overline{n}\}$$

Now, for $\mathcal{F}$ standing for either PA or ZFC, associated with the polynomial p_0 of the previous section, there is a formula $\pi_{\mathcal{F}}(x_0, x_1, \ldots, x_m)$ such that for arbitrary natural numbers $a, a_1, \ldots, a_m$:

$$\text{if } p_0(a, a_1, \ldots, a_m) = 0 \text{ then } \vdash_{\mathcal{F}} \pi(\overline{a}, \overline{a_1}, \ldots, \overline{a_m})$$
$$\text{if } p_0(a, a_1, \ldots, a_m) \neq 0 \text{ then } \vdash_{\mathcal{F}} \neg\pi(\overline{a}, \overline{a_1}, \ldots, \overline{a_m})$$

For PA, this is almost a triviality because symbols for addition and multiplication are part of its vocabulary, and the axioms justify ordinary calculations. For ZFC, some circumlocution is needed, but

[5] *Equality can be thought of as the most "advanced" part of the underlying logic, or as the most fundamental mathematical relation.*

[6] *A fuller account of PA will be found in Feferman's article [5] in this issue of the* Notices. *Full details will be found in textbooks such as [3].*

the ordinary facts about addition and multiplication of natural numbers can still be replicated.

Incompleteness Theorem for PA and ZFC. *If $\mathcal{F}$ is consistent, then there is a natural number n_0 such that*

$$\nvdash_{\mathcal{F}} (\forall x_1)\ldots(\forall x_m)\neg\pi(\overline{n_0}, x_1, \ldots, x_m),$$

and

$$\nvdash_{\mathcal{F}} \neg(\forall x_1)\ldots(\forall x_m)\neg\pi(\overline{n_0}, x_1, \ldots, x_m).$$

Thus the *sentence* $(\forall x_1)\ldots(\forall x_m)\neg\pi(\overline{n_0}, x_1, \ldots, x_m)$ is *undecidable* in $\mathcal{F}$: neither it nor its negation is provable. However, what that sentence asserts, namely that the equation $p_0(n_0, x_1, \ldots, x_m) = 0$ has no solutions in natural numbers, is true. Moreover that truth is a consequence of its undecidability. For if $p_0(n_0, a_1, \ldots, a_m) = 0$ we would have $\vdash_{\mathcal{F}} \pi(\overline{n_0}, \overline{a_1}, \ldots, \overline{a_m})$ and using elementary logic we would obtain

$$\vdash_{\mathcal{F}} (\exists x_1)\ldots(\exists x_m)\pi(\overline{n_0}, x_1, \ldots, x_m)$$

from which we readily obtain

$$\vdash_{\mathcal{F}} \neg(\forall x_1)\ldots(\forall x_m)\neg\pi(\overline{n_0}, x_1, \ldots, x_m)$$

contradicting the claimed undecidability.

There has been much confusion about this situation. How is it that *we* can see that the proposition is true although a system as powerful as ZFC cannot? The answer is that ZFC can indeed see what we can, namely that *if* ZFC is consistent then the proposition is true but undecidable by its means. In fact, it was precisely by analyzing this situation that Gödel could conclude that systems like PA and ZFC cannot prove their own consistency, thereby shattering Hilbert's hopes.

The fact that ZFC is stronger than PA (in actual fact very much stronger) is exemplified by the following result:

Theorem. *If PA is consistent, then there is a natural number n_0 such that*

$$\nvdash_{\mathrm{PA}} (\forall x_1)\ldots(\forall x_m)\neg\pi(\overline{n_0}, x_1, \ldots, x_m),$$

and

$$\nvdash_{\mathrm{PA}} \neg(\forall x_1)\ldots(\forall x_m)\neg\pi(\overline{n_0}, x_1, \ldots, x_m),$$

but

$$\vdash_{\mathrm{ZFC}} (\forall x_1)\ldots(\forall x_m)\neg\pi(\overline{n_0}, x_1, \ldots, x_m).$$

So the undecidability in PA is decided in ZFC! But then ZFC has its own undecidability and with the very same formula π. Only the number n_0 changes. The values of n_0 for either system will be enormous since all the complexity of the algorithms for generating theorems these systems provide must be contained in those numbers.

We will refer to statements to the effect that some polynomial equation has no solutions in natural numbers as $\forall$-statements.[7] They all have the property just exhibited that their undecidability in a reasonable formal system implies their truth. It follows from the MRDP theorem that statements asserting that some computable property holds for all natural numbers are provably equivalent (for example in PA) to a $\forall$-statement. Many famous problems are thus seen to belong to this class, in particular, Fermat's last theorem, the Goldbach conjecture, the four-color theorem, and the Riemann Hypothesis (see [2]).

Beyond ZFC

At the same time that the ZFC axioms provide a foundation for mathematics, they also can be regarded as defining a class of mathematical structures. From this point of view they can be seen as providing "closure" under such operations as forming the set of all subsets of a given set or the union of all of its elements. In a normal situation of this kind it would be natural to find the least set closed under all of the operations called for by the axioms. Remarkably, as natural as such an object appears, its existence cannot be proved in ZFC. This is because if such existence could be proved, it would provide a model for the axioms and hence lead to a proof in ZFC of its own consistency. And this, Gödel had proved to be impossible. So systems like ZFC lead in a natural way to extensions, and in each such extension new $\forall$-propositions become provable. In fact there will be new values of n_0 for which the fact that the equation $p_0(n_0, x_1, \ldots, x_m) = 0$ has no solutions in natural numbers becomes provable. What remains unclear is whether some really mathematically significant $\forall$-propositions, perhaps like some of those mentioned at the end of the previous section, require means beyond ZFC for their proof. We conclude this article with a recent example announced by Harvey Friedman (see [7]) of a $\forall$-proposition that is unprovable in ZFC, but becomes provable with the aid of a so-called "large cardinal" axiom, an assumption of the existence of an infinite set of a size larger than any whose existence can be proved in ZFC.[8]

Friedman's example concerns finite directed graphs (no multiple edges allowed) whose vertices are finite sequences of integers. What is striking is that what looks like a harmless additional conclusion in a theorem provable in ZFC (and even in much weaker systems) results in a proposition that is unprovable in ZFC but becomes provable on the addition of a large cardinal axiom, an assumption of the existence of a set too large for that existence to be provable in ZFC.[9] Some preliminary definitions are needed. For a natural number n we write

[7] *Logicians call these Π^0_1 statements. As this notation suggests, they find their place in a hierarchy.*

[8] *The article by Juliet Floyd and Akihiro Kanamori [6] in this issue of the* Notices *contains some discussion of large cardinal axioms.*

[9] *Specifically, an axiom of the Mahlo type.*

$\hat{n}$ for the set $\{1, 2, \ldots, n\}$. So $\hat{n}^k$ is the set of all sequences of these numbers of length k. If $x, y \in \hat{n}^k$, we write $x * y$ for the element of $\hat{n}^{2k}$ obtained by concatenating x and y. We will work with directed graphs G whose vertex set $V(G)$ consists of elements of $\hat{n}^k$ for certain fixed n, k. For $x, y \in V(G)$ we write (x, y) for a possible edge proceeding from x to y. G is called an *upgraph* if for every edge (x, y) of G, we have $\max(x) < \max(y)$. We say that $u, v \in \hat{n}^\ell$ are *order equivalent* if for all $1 \le i, j \le \ell$, we have $u_i < u_j$ if and only if $v_i < v_j$. An upgraph G is called *order invarant* if whenever $x * y$ is order equivalent to $z * w$, we have (x, y) is an edge of G if and only if (z, w) is an edge of G. For $A \subseteq V(G)$ we write $GA = \{y \mid \exists x \in A \text{ say that } (x, y)$ is an edge of $G\}$. A is called *independent* if no two elements of A are connected by an edge of G. Sets $B, C \subseteq V(G)$ are *G-isomorphic* if there is a bijection h from B to C such that for all $x, y \in B$, (x, y) is an edge in G if and only if (hx, hy) is also an edge in G. Finally, we call $x \in V(G)$ *two-powered* if each x_i is a member of the set $\{1, 2, 4, 8, \ldots\}$ of powers of 2. Now, we have:

Theorem. *For all $n, k, r \ge 1$ every order-invariant upgraph G on $\hat{n}^k$ has an independent set A such that if $B \subseteq V(G) - A$ and $|B| \le r$, then B is G-isomorphic to a set $C \subseteq GA$ such that B and C have the same two-powered elements.*

This is provable not only in ZFC but also in PA and even in still weaker systems. However consider the following variant:

Proposition. *For all $n, k, r \ge 1$ every order-invariant upgraph G on $\hat{n}^k$ has an independent set A such that if $B \subseteq V(G) - A$ and $|B| \le r$, then B is G-isomorphic to a set $C \subseteq GA$ such that B and C have the same two-powered elements, and furthermore, the particular number $2^{(4kr)^2} - 1$ doesn't occur in any element of C.*

Harvey Friedman has announced that this $\forall$-statement is not provable in ZFC but becomes provable on the addition of a large cardinal axiom.

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How Gödel Transformed Set Theory

Juliet Floyd and Akihiro Kanamori

Kurt Gödel (1906–1978), with his work on the constructible universe L, established the relative consistency of the Axiom of Choice and the Continuum Hypothesis. More broadly, he secured the cumulative hierarchy view of the universe of sets and ensured the ascendancy of first-order logic as the framework for set theory. Gödel thereby transformed set theory and launched it with structured subject matter and specific methods of proof as a distinctive field of mathematics. What follows is a survey of prior developments in set theory and logic intended to set the stage, an account of how Gödel marshaled the ideas and constructions to formulate L and establish his results, and a description of subsequent developments in set theory that resonated with his speculations. The survey trots out in quick succession the groundbreaking work at the beginning of a young subject.

Numbers, Types, and Well-Ordering

Set theory was born on that day in December 1873 when Georg Cantor (1845–1918) established that *the continuum is not countable*: There is no bijection between the natural numbers $\mathbf{N} = \{0, 1, 2, 3, \dots\}$ and the real numbers $\mathbf{R}$, since for any (countable) sequence of reals one can specify nested intervals so that any real in the intersection will not be in the sequence. Cantor soon investigated ways to define bijections between sets of reals and the like. He stipulated that two sets have the same *power* if there is a bijection between them, and, implicitly at first, that one set has a *higher* power than another if there is an injection of the latter into the first but no bijection. In an 1878 publication he showed that $\mathbf{R}$, the plane $\mathbf{R} \times \mathbf{R}$, and generally $\mathbf{R}^n$ are all of the same power, but there were still only the two infinite powers as set out by his 1873 proof. At the end of the publication Cantor asserted a dichotomy:

> Every infinite set of real numbers either is countable or has the power of the continuum.

This was the *Continuum Hypothesis* (CH) in its nascent context, and the *continuum problem*, to resolve this hypothesis, would become a major motivation for Cantor's large-scale investigations of infinite numbers and sets.

In his *Grundlagen* of 1883, Cantor developed the *transfinite numbers* and the key concept of *well-ordering*. The progression of transfinite numbers could be depicted, in his later notation, in terms of natural extensions of arithmetical operations:

$$0, 1, 2, \dots \omega, \omega+1, \omega+2, \dots \omega+\omega (=\omega\cdot 2),$$
$$\dots \omega\cdot 3, \dots \omega\cdot\omega (=\omega^2), \dots \omega^3, \dots \omega^\omega, \dots$$

A relation $\prec$ is a *well-ordering* of a set if and only if it is a strict linear ordering of the set such that every nonempty subset has a $\prec$-least element. Well-orderings carry the sense of sequential counting, and the transfinite numbers serve as standards for gauging well-orderings. Cantor called the set of natural numbers $\mathbf{N}$ the first number class (I) and

Juliet Floyd is professor of philosophy at Boston University. Her email address is `jfloyd@bu.edu`.

Akihiro Kanamori is professor of mathematics at Boston University. His email address is `aki@math.bu.edu`.

the set of numbers whose predecessors are in bijective correspondence with (I) the second number class (II). The infinite numbers in the above display are all in (II). Cantor conceived of (II) as bounded above and showed that (II) itself is not countable. Proceeding upward, Cantor called the set of numbers whose predecessors are in bijective correspondence with (II) the third number class (III), and so on. Cantor then propounded a basic principle in the *Grundlagen*:

> "It is always possible to bring any *well-defined* set into the form of a *well-ordered* set."

Sets are to be well-ordered and thus to be gauged by his numbers and number classes. With this framework Cantor had transformed CH into the positive assertion that (II) *and* **R** *have the same power*. However, an emerging problem for Cantor was that he could not even *define* a well-ordering of **R**; the continuum, at the heart of mathematics, could not be easily brought into the fold of the transfinite numbers.

Almost two decades after his initial 1873 proof, Cantor in 1891 came to his celebrated *diagonal* argument. In various guises the argument would become fundamental in mathematical logic. Cantor himself proceeded in terms of functions, ushering collections of arbitrary functions into mathematics, but we cast his result as is done nowadays in terms of the power set $\mathcal{P}(x) = \{y \mid y \subseteq x\}$ of a set x. *For any set* x, $\mathcal{P}(x)$ *has a higher power than* x.

First, the function associating each $a \in x$ with $\{a\}$ is an injection: $x \to \mathcal{P}(x)$. Suppose now that F is any function: $x \to \mathcal{P}(x)$. Consider the "diagonal" set $d = \{a \in x \mid a \notin F(a)\}$. If d itself were a value of F, say $d = F(b)$, then we would have the contradiction: $b \in d$ if and only if $b \notin d$. Hence, F cannot be surjective.

Cantor had been shifting his notion of set to a level of abstraction beyond sets of real numbers and the like; the diagonal argument can be drawn out of the earlier argument, and the new result generalized the old since $\mathcal{P}(\mathbf{N})$ and **R** have the same power. The new result showed for the first time that there is a set of a higher power than **R**, e.g., $\mathcal{P}(\mathcal{P}(\mathbf{N}))$.

Cantor's *Beiträge* of 1895 and 1897 presented his mature theory of the transfinite. Cantor reconstrued power as *cardinal number*, now an autonomous concept beyond *une façon de parler* about bijective correspondence. He defined the addition, multiplication, and exponentiation of cardinal numbers primordially in terms of set-theoretic operations and functions. As befits the introduction of new numbers Cantor then introduced a new notation, one using the Hebrew letter aleph, $\aleph$. $\aleph_0$ is to be the cardinal number of **N** and the successive alephs

$$\aleph_0, \aleph_1, \aleph_2, \ldots, \aleph_\alpha, \ldots$$

are now to be the cardinal numbers of the successive number classes from the *Grundlagen* and thus to exhaust all the infinite cardinal numbers. Cantor pointed out that $2^{\aleph_0}$ is the cardinal number of **R**, but frustrated in his efforts to establish CH he did not even mention the hypothesis, which could now have been stated as $2^{\aleph_0} = \aleph_1$. Every well-ordered set has an aleph as its cardinal number, but where is $2^{\aleph_0}$ in the aleph sequence?

CH was thus embedded in the very interstices of the beginnings of set theory. The structures that Cantor built, while now of great intrinsic interest, emerged largely out of efforts to articulate and establish it. The continuum problem was made the very first in David Hilbert's famous list of problems at the 1900 International Congress of Mathematicians; Hilbert drew out Cantor's difficulty by suggesting the desirability of "actually giving" a well-ordering of **R**.

Bertrand Russell (1872–1970), a main architect of the analytic tradition in philosophy, focused in 1900 on Cantor's work. Russell was pivoting from idealism toward a realism about propositions and with it logicism, the thesis that mathematics can be founded in logic. Taking a universalist approach to logic with all-encompassing categories, Russell took the class of all classes to have the largest cardinal number but saw that Cantor's 1891 result leading to higher cardinal numbers presented a problem. Analyzing that argument, by the spring of 1901 he came to the famous *Russell's Paradox*, a surprisingly simple counterexample to *full comprehension*, the assertion that for every property $A(x)$ the collection of objects having that property, the *class* $\{x \mid A(x)\}$, is also an object. Consider Russell's $\{x \mid x \notin x\}$. If this were an object r, then we would have the contradiction $r \in r$ if and only if $r \notin r$. Gottlob Frege (1848-1925) was the first to systematize quantificational logic in a formalized language, and he aimed to establish a purely logical foundation for arithmetic. Russell famously communicated his paradox to Frege in 1902, who immediately saw that it revealed a contradiction within his mature logical system.

Russell's own reaction was to build a complex logical structure, one used later to develop mathematics in Whitehead and Russell's 1910-3 *Principia Mathematica*. Russell's *ramified theory of types* is a scheme of logical definitions based on *orders* and *types* indexed by the natural numbers. Russell proceeded "intensionally"; he conceived this scheme as a classification of propositions based on the notion of *propositional function*, a notion not reducible to membership (extensionality). Proceeding in modern fashion, we may say that the universe of the *Principia* consists of *objects* stratified into disjoint types T_n, where T_0 consists of the *individuals*, $T_{n+1} \subseteq \{Y \mid Y \subseteq T_n\}$, and the types T_n for $n > 0$ are further ramified into orders O_n^i with

$T_n = \bigcup_i O^i_n$. An object in O^i_n is to be defined either in terms of individuals or of objects in some fixed O^j_m for some $j < i$ and $m \le n$, the definitions allowing for quantification only over O^j_m. This precludes Russell's Paradox and other "vicious circles", as objects consist only of previous objects and are built up through definitions referring only to previous stages. However, in this system it is impossible to quantify over all objects in a type T_n, and this makes the formulation of numerous mathematical propositions at best cumbersome and at worst impossible. Russell was led to introduce his *Axiom of Reducibility*, which asserts that *for each object there is a predicative object consisting of exactly the same objects*, where an object is *predicative* if its order is the least greater than that of its constituents. This axiom reduced consideration to individuals, predicative objects consisting of individuals, predicative objects consisting of predicative objects consisting of individuals, and so on—the *simple theory of types*. In traumatic reaction to his paradox Russell had built a complex system of orders and types only to collapse it with his Axiom of Reducibility, a fearful symmetry imposed by an artful dodger.

Ernst Zermelo (1871-1953) made his major advances in set theory in the first decade of the new century. Zermelo's first substantial result was his independent discovery of the argument for Russell's Paradox. He then established in 1904 the Well-Ordering Theorem, that *every set can be well-ordered*, assuming what he soon called the Axiom of Choice (AC). Zermelo thereby shifted the notion of set away from Cantor's principle that every well-defined set is well-orderable and replaced that principle by an explicit axiom.

In retrospect Zermelo's argument for his Well-Ordering Theorem proved to be pivotal for the development of set theory. To summarize, suppose that x is a set to be well-ordered, and through Zermelo's AC hypothesis assume that the power set $\mathcal{P}(x) = \{y \mid y \subseteq x\}$ has a choice function, i.e., a function γ such that for every nonempty member y of $\mathcal{P}(x)$, $\gamma(y) \in y$. Call a subset y of x a *γ-set* if there is a well-ordering R of y such that for each $a \in y$, $\gamma(\{z \mid z\ R\ a \text{ fails}\}) = a$. That is, each member of y is what γ "chooses" from what does not R-precede it. The main observation is that γ-sets cohere in the following sense: If y is a γ-set with well-ordering R and z is a γ-set with well-ordering S, then $y \subseteq z$ and S is a prolongation of R, or vice versa. With this, let w be the union of all the γ-sets. Then w too is a γ-set, and by its maximality it must be all of x, and hence x is well-ordered.

Cantor's work had served to exacerbate a growing discord among mathematicians with respect to two related issues: whether infinite collections can be mathematically investigated at all, and how far the function concept is to be extended. The positive use of an arbitrary function operating on arbitrary subsets of a set having been made explicit, there was open controversy after the appearance of Zermelo's proof. This can be viewed as a turning point for mathematics, with the subsequent tilting toward the acceptance of AC symptomatic of a conceptual shift.

Axiomatization

In response to his critics Zermelo published a second proof of the Well-Ordering Theorem in 1908, and with axiomatization assuming a general methodological role in mathematics he also published in 1908 the first full-fledged axiomatization of set theory. But as with Cantor's work, this was no idle structure building, but a response to pressure for a new mathematical context. In this case it was not for the formulation and solution of a *problem* but rather to clarify a *proof*. Zermelo's motive in large part for axiomatizing set theory was to buttress his Well-Ordering Theorem by making explicit its underlying set existence assumptions.

To summarize Zermelo's axioms much as they would be presented today, there is an initial axiom asserting that two sets are the same if they contain the same members (Extensionality, i.e., membership determines equality), and an axiom asserting that there is an initial set $\emptyset$ having no members (Empty Set). Then there are the generative axioms, specific instances of comprehension: For any sets x, y, $\{x, y\} = \{z \mid z = x \text{ or } z = y\}$ is a set (Pairs), $\bigcup x = \{z \mid \exists y(y \in x \text{ and } z \in y)\}$ is a set (Union), and $\mathcal{P}(x) = \{y \mid y \subseteq x\}$ is a set (Power Set). There is an axiom asserting the existence of a particular recursively specified infinite set (Infinity). Zermelo aptly formulated AC in terms of sets as follows: *For any set x consisting of nonempty, pairwise disjoint sets, there is a set y such that each member of x intersects with y in exactly one element.* Finally, there is the axiom (schema) of Separation: *For any set x and "definite" property $A(y)$, $\{y \in x \mid A(y)\}$ is a set.* That is, the intersection of a set x and a class $\{y \mid A(y)\}$ is again a set. Zermelo saw that Separation suffices for a development of set theory that still allows for the "logical" formation of sets according to property; Russell's Paradox is precluded since only "logical" *sub*sets are to be allowed. But what exactly is a "definite" property? This was a central vagary that would be addressed in the subsequent formalization of Zermelo's set theory.

With his axioms Zermelo ushered in a new, abstract view of sets as structured solely by membership and built up iteratively according to governing axioms, a view that would soon come to dominate. Zermelo's work also pioneered the reduction of mathematical concepts and arguments to set-theoretic concepts and arguments from axioms, based on sets doing the work of mathematical objects.

Unlike the development of classical mathematics from marketplace arithmetic and Greek geometry, sets were neither laden with nor bolstered by well-worked antecedents. Zermelo axiomatization, unlike Russell's cumbersome theory of types, provided a simple system for the development of mathematics. Set theory would provide an underpinning of mathematics, and Zermelo's axioms would resonate with mathematical practice.

In the 1920s fresh initiatives structured the loose Zermelian framework with new features and corresponding developments in axiomatics, the most consequential moves made by John von Neumann (1903–1957) in his dissertation, with anticipations by Dimitry Mirimanoff (1861–1945). The transfinite numbers had been central for Cantor but peripheral to Zermelo, and in Zermelo's system not even $2^{\aleph_0} = \aleph_1$ could be stated directly. Von Neumann reconstrued the transfinite numbers as *bona fide* sets, the ordinals, and established their efficacy by formalizing transfinite recursion.

Ordinals manifest the idea, natural once iterative set formation is assimilated, of taking the relation of precedence in a well-ordering simply to be membership. A set (or class) x is *transitive* if and only if whenever $a \in b$ for $b \in x$, $a \in x$. A set x is a (von Neumann) *ordinal* if and only if x is transitive, and the membership relation restricted to $x = \{y \mid y \in x\}$ is a well-ordering of x. The first several ordinals are $\emptyset, \{\emptyset\}, \{\emptyset, \{\emptyset\}\}, \{\emptyset, \{\emptyset\}, \{\emptyset, \{\emptyset\}\}\}, \ldots,$ to be taken as the natural numbers $0,1,2,3,\ldots$. The union of these finite ordinals is an ordinal, to be taken as ω; $\omega \cup \{\omega\}$ is an ordinal, to be taken as $\omega + 1$; and so forth. It has become customary to use the Greek letters $\alpha, \beta, \gamma, \ldots$ to denote ordinals; the class of all ordinals is itself well-ordered by membership, and $\alpha < \beta$ is written for $\alpha \in \beta$; and an ordinal without an immediate predecessor is a *limit* ordinal. Von Neumann established, as had Mirimanoff before him, the key instrumental property of Cantor's ordinal numbers for ordinals: *Every well-ordered set is order-isomorphic to exactly one ordinal with membership.* The proof was the first to make full use of the Axiom of Replacement and thus drew that axiom into set theory.

For a set x and property $A(v, w)$, the property is said to be *functional on* x if for any $a \in x$, there is exactly one b such that $A(a, b)$. The Axiom (schema) of Replacement asserts: *For any set* x *and property* $A(v, w)$ *functional on* x, $\{b \mid \exists a(a \in x \text{ and } A(a, b))\}$ *is a set.* This axiom posits sets that result when members of a set are "replaced" according to a property; a simple argument shows that Replacement subsumes Separation.

Von Neumann generally ascribed to the ordinals the role of Cantor's ordinal numbers, and already to incorporate transfinite arithmetic into set theory he saw the need to establish the Transfinite Recursion Theorem, the theorem that validates recursive definition along well-orderings. The proof had an antecedent in the Zermelo 1904 proof, but Replacement was necessary even for the very formulation, let alone the proof, of the theorem. With the ordinals in place von Neumann completed the incorporation of the Cantorian transfinite by defining the *cardinals* as the *initial ordinals*, those ordinals not in bijective correspondence with any of their predecessors.

Replacement has been latterly regarded as somehow less necessary or crucial than the other axioms, the purported effect of the axiom being only on large-cardinality sets. Initially, Abraham Fraenkel (1891–1965) and Thoralf Skolem (1887–1963) had independently proposed adjoining Replacement to ensure that $E(a) = \{a, \mathcal{P}(a), \mathcal{P}(\mathcal{P}(a)), \ldots\}$ would be a set for a, the infinite set given by Zermelo's Axiom of Infinity, since, as they pointed out, Zermelo's axioms cannot establish this. However, even $E(\emptyset)$ cannot be proved to be a set from Zermelo's axioms, and if his Axiom of Infinity were reformulated to accommodate $E(\emptyset)$, there would still be many finite sets a such that $E(a)$ cannot be proved to be a set. Replacement serves to rectify the situation by admitting new infinite sets defined by "replacing" members of the one infinite set given by the Axiom of Infinity. In any case, the full exercise of Replacement is part and parcel of transfinite recursion, which is now used everywhere in modern set theory, and it was von Neumann's formal incorporation of this method into set theory, as necessitated by his proofs, that brought in Replacement.

Von Neumann (and others) also investigated the salutary effects of restricting the universe of sets to the well-founded sets. The *well-founded* sets are the sets that belong to some "rank" V_α, these definable through transfinite recursion:

$$V_0 = \emptyset;\ V_{\alpha+1} = \mathcal{P}(V_\alpha);\ \text{and } V_\delta = \bigcup\{V_\alpha \mid \alpha < \delta\} \text{ for limit ordinals } \delta.$$

$V_{\omega+1}$ contains every set consisting of natural numbers (finite ordinals), and so already at early levels there are set counterparts to many objects in mathematics. That the universe V of all sets is the *cumulative hierarchy*

$$V = \bigcup\{V_\alpha \mid \alpha \text{ is an ordinal}\}$$

is thus the assertion that every set is well-founded. Von Neumann essentially showed that this assertion is equivalent to a simple assertion about sets, the Axiom of Foundation: *Any nonempty set* x *has a member* y *such that* $x \cap y$ *is empty.* Thus, nonempty well-founded sets have $\in$-minimal members. If a set x satisfies $x \in x$, then $\{x\}$ is not well-founded; similarly, if there are $x_1 \in x_2 \in x_1$, then $\{x_1, x_2\}$ is not well-founded. Ordinals and sets consisting of ordinals are well-founded, and well-foundedness can be viewed as a generalization of the notion of being an ordinal that loosens the connection with transitivity. The Axiom of Foundation eliminates pathologies like $x \in x$ and through the cumulative hierarchy rendition allows inductive arguments to establish results about the entire universe.

In a remarkable 1930 publication Zermelo provided his final axiomatization of set theory, one that recast his 1908 axiomatization and incorporated both Replacement and Foundation. He herewith completed his transmutation of the notion of set, his abstract, prescriptive view stabilized by further axioms that structured the universe of sets. Replacement provided the means for transfinite recursion and induction, and Foundation made possible the application of those means to get results about *all* sets. Zermelo proceeded to offer a striking, synthetic view of a procession of natural models for his axioms that would have a modern resonance and applied Replacement and Foundation to establish isomorphism and embedding results.

Zermelo's 1930 publication was in part a response to Skolem's advocacy, already in 1922, of the idea of framing Zermelo's 1908 axioms in first-order logic. *First-order logic* is the logic of formal languages consisting of formulas built up from specified function and relation symbols using logical connectives and first-order quantifiers $\forall$ and $\exists$, quantifiers to be interpreted as ranging over the *elements* of a domain of discourse. (*Second-order logic* has quantifiers to be interpreted as ranging over arbitrary subsets of a domain.) Skolem had proposed formalizing Zermelo's axioms in the first-order language with $\in$ and $=$ as binary relation symbols. Zermelo's *definite* properties would then be those expressible in this first-order language in terms of given sets, and Separation would become a schema of axioms, one for each first-order formula. Analogous remarks apply to the formalization of Replacement in first-order logic. As set theory was to develop, the formalization of Zermelo's 1930 axiomatization in first-order logic would become the standard axiomatization, *Zermelo-Fraenkel with Choice* (ZFC). The "Fraenkel" acknowledges Fraenkel's early suggestion of incorporating Replacement. *Zermelo-Fraenkel* (ZF) is ZFC without AC.

Significantly, before this standardization both Skolem and Zermelo raised issues about the limitations of set theory as cast in first-order logic. Skolem had established a fundamental result for first-order logic with the Löwenheim-Skolem Theorem: *If a countable collection of first-order sentences has a model, then it has a countable model.* Having proposed framing set theory in first-order terms, Skolem pointed out as a palliative for taking set theory as a foundation for mathematics what has come to be called the *Skolem Paradox*: Zermelo's 1908 axioms when cast in first-order logic become a countable collection of sentences, and so if they have a model at all, they have a countable model. We thus have the "paradoxical" existence of countable models for Zermelo's axioms although they entail the existence of uncountable sets. Zermelo found this antithetical and repugnant, and proceeded in avowedly second-order terms in his 1930 work. However, stronger currents were at work leading to the ascendancy of first-order logic.

Constructible Universe

Enter Gödel. Gödel virtually completed the mathematization of logic by submerging "metamathematical" methods into mathematics. The Completeness Theorem from his 1930 dissertation established that logical consequence could be captured by formal proof for first-order logic and secured its key instrumental property of compactness for building models. The main advance was of course the direct coding, "the arithmetization of syntax", which together with a refined version of Cantor's diagonal argument led to the celebrated 1931 Incompleteness Theorem. This theorem established a fundamental distinction between what is *true* about the natural numbers and what is *provable* and transformed a program advanced by Hilbert in the 1920s to establish the consistency of mathematics by finitary means. Gödel's work showed in particular that for a (schematically definable) collection of axioms A, its *consistency*, that from A one cannot prove a contradiction, has a formal counterpart in an arithmetical formula $\mathrm{Con}(A)$ about natural numbers. Gödel's "second" theorem asserts that if A is consistent and subsumes the elementary arithmetic of the natural numbers, then $\mathrm{Con}(A)$ cannot be proved from A.

Gödel's advances in set theory can be seen as part of a steady intellectual development from his fundamental work on incompleteness. His 1931 paper had a prescient footnote 48a:

> As will be shown in Part II of this paper, 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 (cf. D. Hilbert, "Über das Unendliche", Math. Ann. **95**, p. 184), while in any formal system at most countably many of them are available. For it can be shown that the undecidable propositions constructed here become decidable whenever appropriate higher types are added (for example, the type ω to the system P [the simple theory of types superposed on the natural numbers as individuals satisfying the Peano

axioms]). An analogous situation prevails for the axiom system of set theory.

Gödel's letters and lectures clarify that the addition of an infinite type ω to Russell's theory of types would provide a "definition for 'truth' " for the theory and hence establish hitherto unprovable propositions like those provided by his Incompleteness Theorem. Inherent in Russell's theory was the indexing of types by the natural numbers, and Gödel's citation in the footnote of Hilbert's 1926 paper in connection with the possibility of adjoining transfinite types would bridge the past and the future. Hilbert there had attempted a proof of CH using transfinite indexing in his formalism, and Gödel would achieve what success is possible in this direction. Gödel never published the announced Part II, which was to have been on truth, but his engagement with truth and its distinction from provability could be viewed as his entrée into full blown set theory. In a 1933 lecture Gödel expounded on axiomatic set theory as a natural generalization of the simple theory of types "if certain superfluous restrictions" are removed: One could *cumulate* the types starting with individuals D_0 and taking $D_{n+1} = D_n \cup \mathcal{P}(D_n)$, and one could *extend* the process into the transfinite, mindful that for any type-theoretic system S a new proposition (e.g., Con(S)) becomes provable if to S is adjoined the "the next higher type and the axioms concerning it". Thus Gödel came to the cumulative hierarchy as a transfinite extension of the theory of types that incorporates higher and higher levels of truth.

Alfred Tarski (1902–1983) completed the mathematization of logic in the early 1930s by providing a systematic "definition of truth", exercising philosophers ever since. Tarski simply schematized truth by taking it to be a correspondence between formulas of a formal language and set-theoretic assertions about an interpretation of the language and by providing a recursive definition of the *satisfaction* relation, that which obtains when a formula holds in an interpretation. The eventual effect of Tarski's mathematical formulation of semantics would be not only to make mathematics out of the informal notion of satisfiability, but also to enrich ongoing mathematics with a systematic method for forming mathematical analogues of several intuitive semantic notions. For coming purposes, the following specifies notation and concepts:

For a first-order language, suppose that M is an interpretation of the language (i.e., a specification of a domain as well as interpretations of the function and relation symbols), $\varphi(v_1, v_2, \ldots, v_n)$ is a formula of the language with the (unquantified) variables as displayed, and $a_1, a_2, \ldots, a_n$ are the domain of M. Then

$$M \models \varphi[a_1, a_2, \ldots, a_n]$$

asserts that the formula φ is satisfied in M according to Tarski's recursive definition when v_i is interpreted as a_i. A subset y of the domain of M is *first-order definable over M* if and only if there is a formula $\psi(v_0, v_1, v_2, \ldots, v_n)$ and $a_1, a_2, \ldots, a_n$ in the domain of M such that

$$y = \{z \mid M \models \psi[z, a_1, \ldots, a_n]\}.$$

Set theory was launched on an independent course as a distinctive field of mathematics by Gödel's formulation of the class L of *constructible* sets through which he established the relative consistency of AC and CH. He thus attended to the fundamental issues raised at the beginning of set theory by Cantor and Zermelo. In his first 1938 announcement Gödel described L as a hierarchy "which can be obtained by Russell's ramified hierarchy of types, if extended to include transfinite orders." Indeed, with L Gödel had refined the cumulative hierarchy of sets to a cumulative hierarchy of *definable* sets which is analogous to the orders of Russell's *ramified* theory. Gödel's further innovation was to continue the indexing of the hierarchy through *all* the ordinals. Von Neumann's canonical well-orderings would be the spine for a thin hierarchy of sets, and this would be the key to both the AC and CH results. In a 1939 note Gödel informally presented L essentially as is done today: For any set x let def(x) denote the collection of subsets of x first-order definable over x according to the previous definition. Then define:

$$L_0 = \emptyset;\ L_{\alpha+1} = \mathrm{def}(L_\alpha),\ L_\delta = \bigcup\{L_\alpha \mid \alpha < \delta\}$$
$$\text{for limit ordinals } \delta;$$

and the *constructible universe*

$$L = \bigcup\{L_\alpha \mid \alpha \text{ is an ordinal}\}.$$

Gödel pointed out that L "can be defined and its theory developed in the formal systems of set theory themselves." This follows by transfinite recursion from the formalizability of def(x) in set theory, the "definability of definability", which was later reaffirmed by Tarski's systematic definition of the satisfaction relation in set-theoretic terms. In modern parlance, an *inner model* is a transitive class containing all the ordinals such that, with membership and quantification restricted to it, the class satisfies each axiom of ZF. Gödel in effect argued in ZF to show that L is an inner model and moreover that L satisfies AC and CH. He thus established the relative consistency Con(ZF) implies Con(ZFC + CH).

In his 1940 monograph, based on 1938 lectures, Gödel formulated L via a transfinite recursion that generated L set by set. His incompleteness proof had featured "Gödel numbering", the encoding of formulas by natural numbers, and his L recursion was a veritable Gödel numbering with ordinals, one that relies on their extent as given beforehand to generate a universe of sets. This approach may have obfuscated the satisfaction aspects of the construction, but on the other hand it did make more evident other aspects: Since there is a direct, definable well-ordering of L, choice functions abound in L, and AC holds there. Also, L was seen to have the important property of *absoluteness* through the simple operations involved in Gödel's recursion, one consequence of which is that for any inner model M, the construction of L in the sense of M again leads to the same class L. Decades later many inner models based on first-order definability would be investigated for which absoluteness considerations would be pivotal, and Gödel had formulated the canonical inner model, rather analogous to the algebraic numbers for fields of characteristic zero.

In a 1939 lecture about L Gödel described what amounts to the Russell orders for the simple situation when the members of a countable collection of real numbers are taken as the individuals and new real numbers are successively defined via quantification over previously defined real numbers, and he emphasized that the process can be continued into the transfinite. He then observed that this procedure can be applied to sets of real numbers and the like, as individuals, and moreover, that one can "intermix" the procedure for the real numbers with the procedure for sets of real numbers "by using in the definition of a real number quantifiers that refer to sets of real numbers, and similarly in still more complicated ways." Gödel called a constructible set "the most general [object] that can at all be obtained in this way, where the quantifiers may refer not only to sets of real numbers, but also to sets of sets of real numbers and so on, *ad transfinitum*, and where the indices of iteration ...can also be arbitrary transfinite ordinal numbers." Gödel considered that although this definition of constructible set might seem at first to be "unbearably complicated", "the *greatest generality yields*, as it so often does, at the same time the *greatest simplicity*." Gödel was picturing Russell's ramified theory of types by first disassociating the types from the orders, with the orders here given through definability and the types represented by real numbers, sets of real numbers, and so forth. Gödel's intermixing then amounted to a recapturing of the complexity of Russell's ramification, the extension of the hierarchy into the transfinite allowing for a new simplicity.

Gödel went on to describe the universe of set theory, "the objects of which set theory speaks", as falling into "a transfinite sequence of Russellian [simple] types", the cumulative hierarchy of sets. He then formulated the constructible sets as an analogous hierarchy, the hierarchy of his 1939 note. In a comment bringing out the intermixing of types and orders, Gödel pointed out that "there are sets *of lower type* that *can* only *be defined* with the help of *quantifiers for sets of higher type*." For example, constructible members of $V_{\omega+1}$ in the cumulative hierarchy will first appear quite high in the constructible L_α hierarchy; resonant with Gödel's earlier remarks about truth, members of $V_{\omega+1}$, in particular sets of natural numbers, will encode truth propositions about higher L_α's. Gödel had given priority to the ordinals and recursively formulated a hierarchy of orders based on definability, and the hierarchy of types was spread out across the orders. The jumble of the *Principia Mathematica* had been transfigured into the constructible universe L.

Gödel's argument for CH holding in L rests, as he himself wrote in a brief 1939 summary, on "a generalization of Skolem's method for constructing enumerable models", now embodied in the well-known Skolem Hull argument and Condensation Lemma for L. It is the first significant application of the Löwenheim-Skolem Theorem since Skolem's own to get his paradox. Ironically, though Skolem sought through his paradox to discredit set theory based on first-order logic as a foundation for mathematics, Gödel turned paradox into method, one promoting first-order logic. Gödel showed that in L every subset of L_α belongs to some L_β for some β of the same power as α (so that in L every real belongs to some L_β for a countable β, and CH holds). In the 1939 lecture he asserted that "this fundamental theorem constitutes the corrected core of the so-called Russellian axiom of reducibility." Thus, Gödel established another connection between L and Russell's ramified theory of types. But while Russell had to *postulate* his Axiom of Reducibility for his finite orders, Gödel was able to *derive* an analogous form for his transfinite hierarchy, one that asserts that the types are delimited in the hierarchy of orders.

Gödel brought into set theory a method of construction and argument and thereby affirmed several features of its axiomatic presentation. First, Gödel showed how first-order definability can be formalized and used in a transfinite recursive construction to establish striking new mathematical results. This significantly contributed to a lasting ascendancy for first-order logic which beyond its *sufficiency* as a logical framework for mathematics was seen to have considerable *operational efficacy*. Gödel's construction moreover buttressed the incorporation of Replacement and Foundation into set theory. Replacement was immanent in the arbitrary extent of the ordinals for the indexing of L and in its formal definition via transfinite recursion. As for Foundation, underlying the construction was the well-foundedness of sets. Gödel in a footnote to his 1939 note wrote: "In order to give A [the axiom $V = L$, that the universe is L] an intuitive meaning, one has to understand by 'sets' all objects obtained by building up the simplified hierarchy of types on an empty set of individuals (including types of arbitrary transfinite orders)." Some have been baffled about how the cumulative hierarchy picture came to dominate in set-theoretic practice; although there was Mirimanoff, von Neumann, and especially Zermelo, the picture came in with Gödel's method, the reasons being both thematic and historical: Gödel's work with L with its incisive analysis of first-order definability was readily recognized as a signal advance, while Zermelo's 1930 paper with its second-order vagaries remained somewhat obscure. As the construction of L was gradually digested, the sense that it promoted of a cumulative hierarchy reverberated to become the basic picture of the universe of sets.

New Axioms

How Gödel transformed set theory can be broadly cast as follows: On the larger stage, from the time of Cantor, sets began making their way into topology, algebra, and analysis so that by the time of Gödel, they were fairly entrenched in the structure and language of mathematics. But how were sets viewed among set *theorists*, those investigating sets as such? Before Gödel, the main concerns were what sets *are* and how sets and their axioms can serve as a reductive basis for mathematics. Even today, those preoccupied with ontology, questions of mathematical existence, focus mostly upon the set theory of the early period. After Gödel, the main concerns became what sets *do* and how set theory is to advance as an autonomous field of mathematics. The cumulative hierarchy picture was in place as subject matter, and the metamathematical methods of first-order logic mediated the subject. There was a decided shift toward epistemological questions, e.g., what can be proved about sets and on what basis.

As a pivotal figure, what was Gödel's own stance? What he *said* would align him more with his predecessors, but what he *did* would lead to the development of methods and models. In a 1944 article on Russell's mathematical logic, in a 1947 article on Cantor's continuum problem (and in a 1964 revision), and in subsequent lectures and correspondence, Gödel articulated his philosophy of "conceptual realism" about mathematics. He espoused a staunchly objective "concept of set" according to which the axioms of set theory are true and are descriptive of an objective reality schematized by the cumulative hierarchy. Be that as it may, his actual mathematical work laid the groundwork for the development of a range of models and axioms for set theory. Already in the early 1940s Gödel worked out for himself a possible model for the negation of AC, and in a 1946 address he described a new inner model, the class of ordinal definable sets.

In his 1947 article on the continuum problem Gödel pointed out the desirability of establishing the independence of CH, i.e., in addition to his relative consistency result, that also Con(ZF) implies Con(ZFC + the negation of CH). However, Gödel stressed that this would not *solve* the problem. The axioms of set theory do not "form a system closed in itself", and so the "very concept of set on which they are based" suggests their extension by new axioms, axioms that may decide issues like CH. New axioms could even be entertained on the extrinsic basis of the "fruitfulness of their consequences". Gödel concluded by advancing the remarkable opinion that CH "will turn out to be wrong" since it has as paradoxical consequences the existence of thin, in various senses he described, sets of reals of the power of the continuum.

Later touted as his "program", Gödel's advocacy of the search for new axioms mainly had to do with *large cardinal* axioms. These postulate structure in the higher reaches of the cumulative hierarchy, often by positing cardinals whose properties entail their inaccessibility from below in strong senses. Speculations about large cardinal possibilities had occurred as far back as the time of Zermelo's first axiomatization of set theory. Gödel advocated their investigation, and they can be viewed as a further manifestation of his footnote 48a idea of capturing more truth, this time by positing strong closure points for the cumulative hierarchy. In the early 1960s large cardinals were vitalized by the infusion of model-theoretic methods, which established their central involvement in *embeddings* of models of set theory. The subject was then to become a mainstream of set theory after the dramatic introduction of a new way of getting *extensions* of models of set theory.

Paul Cohen (1934–) in 1963 established the independence of AC from ZF and the independence of CH from ZFC. That is, Cohen established that Con(ZF) implies Con(ZF + the negation of AC) and that Con(ZF) implies Con(ZFC + the negation of CH). These results delimited ZF and ZFC in terms of the two fundamental issues raised at the beginning of set theory. But beyond that, Cohen's proofs were soon to flow into method, becoming the inaugural examples of *forcing*, a remarkably general and flexible method for extending models of set theory by adding "generic" sets. Forcing has strong intuitive underpinnings and reinforces the notion of set as given by the first-order ZF axioms with conspicuous uses of Replacement and Foundation. With L analogous to the field of algebraic numbers, forcing is analogous to making transcendental field extensions. If Gödel's construction of L had launched set theory as a distinctive field of mathematics, then Cohen's method of forcing began its transformation into a modern, sophisticated one. Set theorists rushed in and were soon establishing a cornucopia of relative consistency results, truths in a wider sense, some illuminating problems of classical mathematics. In this sea change the extent and breadth of the expansion of set theory dwarfed what came before, both in terms of the numbers of people involved and the results established.

Already in the 1960s and into the 1970s large cardinal postulations were charted out and elaborated, investigated because of the "fruitfulness of their consequences" since they provided quick proofs of various strong propositions and because they provided the consistency strength to establish new relative consistency results. A subtle connection quickly emerged between large cardinals and combinatorial propositions low in the cumulative hierarchy: Forcing showed just how relative the Cantorian notion of cardinality is, since bijections could be adjoined easily, often with little disturbance to the universe. In particular, large cardinals, highly inaccessible from below, were found to satisfy substantial propositions even after they were "collapsed" by forcing to $\aleph_1$ or $\aleph_2$, i.e., bijections were adjoined to make the cardinal the first or second uncountable cardinal. Conversely, such propositions were found to entail large cardinal hypotheses in the clarity of an L-like inner model, sometimes the very same initial large cardinal hypothesis. In a subtle synthesis, hypotheses of length concerning the extent of the transfinite were correlated with hypotheses of width concerning the fullness of power sets low in the cumulative hierarchy, sometimes the arguments providing *equi-consistencies*. Thus, large cardinals found not only extrinsic but intrinsic justifications. Although their emergence was historically contingent, large cardinals were seen to form a *linear* hierarchy, and there was the growing conviction that this hierarchy provides *the* hierarchy of exhaustive principles against which all possible consistency strengths can be gauged, a kind of hierarchical completion of ZFC.

In the 1970s and 1980s possibilities for new complementarity were explored with the development of *inner model theory* for large cardinals, the investigation of minimal L-like inner models having large cardinals, models that exhibited the kind of fine structure that Gödel had first explored for L. Also, *determinacy* hypotheses about sets of reals were explored because of their fruitful consequences in descriptive set theory, the definability theory of the continuum. Then in a grand synthesis, certain large cardinals were found to provide just the consistency strength to establish the consistency of $\mathrm{AD}^{L(\mathbf{R})}$, the Axiom of Determinacy holding in the minimal inner model $L(\mathbf{R})$ containing all the reals. In a different direction, Harvey Friedman has recently provided a variety of propositions of finite combinatorics that are equi-consistent with the existence of large cardinals; this incisive work serves to affirm the "necessary use" of large cardinal axioms even in finite mathematics. In set theory itself, Hugh Woodin has developed a scheme based on a new logic in an environment of large cardinals that argues against CH itself, and with an additional axiom, that $2^{\aleph_0} = \aleph_2$. These results serve as remarkable vindications for Gödel's original hopes for large cardinals.

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Pictures at an Exhibition

Karl Sigmund

In 1965 Kurt Gödel wrote to his mother Marianne: "I am happy not to have to take part in the Viennese festivities, as I hate these things." So we must concede: the Viennese festivities planned for April 2006, on the occasion of his centenary, would probably have made him wince.

In April 2006 a large scientific congress will be held at the University of Vienna, organized by the International Kurt Gödel Society and generously sponsored by the Templeton Foundation (see `http://www.logic.at/goedel2006/`). It will be attended by the few scientists who had close personal exchanges with Gödel, such as Georg Kreisel, Dana Scott, and Gaisi Takeuti. There will also be an exhibition on the life and times of Gödel, and this provides me, who rashly volunteered for the job, with a new set of experiences.

The University of Vienna is well-advised, of course, to celebrate Kurt Gödel as much as it can. The game theorist Oskar Morgenstern, one of Gödel's few friends in later years, was surely right when he wrote: "Among all those who taught at the University of Vienna, there is probably nobody whose name outshines that of Gödel." Gödel did most of his best work in Vienna. The university did not exactly pamper him, however. He remained a lowly *Privatdozent* (meaning he had the right to lecture, but no salary worth speaking of), and he eventually had to escape, under hair-raising circumstances, to the safety offered by Princeton. No place other than the Institute for Advanced Study (IAS) could have provided him with more peace and ease, or better intellectual companionship. Yet within a dozen years his productivity trickled off, although he certainly did not relax in either ambition or hard work.

The city of Vienna cannot boast about the way it treated Gödel, but his centenary is a good occasion for making amends. The same applies to Mozart. And as luck will have it, Mozart's 250th anniversary also takes place in 2006. And Freud's 150th. Some competition!

Exhibitions are costly. Aspiring to just one or two percent of the sum lavished by Berlin, for example, on its splendid Albert Einstein Exhibition 2005 is not a trivial matter. Fortunately, among officials in some ministries and magistrates I found several dedicated enthusiasts, true crypto-Gödelians who were ready to help. I also encountered some who had never heard of him: but as soon as I mentioned that *Time* magazine had listed Gödel among the hundred most important persons of the twentieth century, conversation flowed more easily. Whoever drew up that list deserves an accolade!

Exhibitions also need space. Eventually, the list of possible locations boiled down to three candidates. One was the main building of the University of Vienna. During the Congress, in the week of Gödel's birthday (April 28), hundreds of experts from set theory and mathematical logic will stroll its neo-Renaissance arcades, in addition to the daily stream of students; but many other Viennese will be reticent to cross the university's entrance stairs. The second location is Palais Pallfy, on the

Karl Sigmund is professor of mathematics at the University of Vienna and a research scholar at the Institute for Applied Systems Analysis in Laxenburg, Austria. His email address is `karl.sigmund@univie.ac.at`.

Josefsplatz, the most beautiful piazza in town, just opposite the Hofburg of the Habsburgs. Since this will be the venue of many high-level conferences in the first half of 2006 (when Austria presides over the council of the European Union), one hopes that some visitors will take a look at Gödel. But people around the Josefsplatz are usually busy. Finally, there is the beautiful MuseumsQuarter, whose baroque halls are available during the summer months, when flocks of tourists throng between the Museum for the History of Art, the Leopold Museum with its collections of Klimt and Schiele, and a lively scene of bars and restaurants. No drawback here, except that Gödel's birthday does not fall in summer. It finally was decided that the exhibition will travel and visit all three spots in different incarnations, from the end of April to mid-August 2006.

What can one show in an exhibition on Kurt Gödel? His work was abstract and his life withdrawn. There is nothing equivalent to, say, the couch of Sigmund Freud, or the fiddle of Albert Einstein. Gödel's famous spectacles have not been preserved, so it seems, but his optometric prescription from 1925 has survived. For Gödel, who grew up as an avid stamp collector, was not one to throw things lightly away. He kept the bill for his wedding meal, for instance, as well as the testy reminder, signed by Helmut Hasse, that he had not paid his membership dues for the German Mathematical Society. Gödel also kept the receipt for the purchase of *Principia Mathematica*, acquired during his student days. The Gödel *Nachlass*, which belongs to the IAS and is kept at Princeton University's Firestone Library, is a gold mine of tidbits like these, but also of more serious information, and Gödel's biographer John Dawson, who knows that *Nachlass* like no one else, has kindly agreed to co-curate the exhibition. He writes, in this issue of the *Notices*, about his experiences cataloguing the boxes of *Nachlass* material (see also Dawson 1997).

The only other place with a sizeable amount of information on Kurt Gödel is Vienna (Köhler at al, 2002). There are the archives of the university, containing much on his brilliant Ph.D. thesis, on his epochal *Habilitation*, and on the sinister correspondence between academic officials and high-level Nazis after 1938. Gödel managed twice, after Hitler annexed Austria, to go on leave to visit the U.S., first at the height of the Munich crisis and again in the tense months of the "phony war". More than a year after Gödel had settled down for good in the U.S., the German ministry sent him (c/o University of Vienna) a lavishly emblazoned diploma with his promotion to "*Dozent Neuer Ordnung*" and the pompous guarantee of the Führer's special protection. The document was never collected, though, and the receipt still waits to be signed.

Next to the university, the richest source on Gödel is in City Hall: the municipality bought hundreds of letters which he wrote to his mother on every other Sunday evening, for the first twenty years after the war. They were bought, thanks to Werner Schimanovich and Peter Weibel (producers of a documentary on Gödel), from the heirs of Gödel's brother Rudolf. Unfortunately, there is no trace of the letters Gödel must have sent from Princeton during his visits there in the 1930s, and after Gödel's death his wife Adele destroyed all letters by her mother-in-law.

STAMP OF APPROVAL. "I replied very gently to the magistrate, saying that I owed my education to the University," wrote Gödel to his mother. He had experienced superb teachers but obnoxious administration. During his time as *Privatdozent*, he mostly was on leave, either in Princeton or in sanatoria, and lectured only for three semesters altogether.

The gist of Gödel's major discoveries can be explained fairly easily to a large public: (a) incompleteness, (b) the consistency of the continuum hypothesis, and (c) time travel in rotating universes. But I do not believe that an exhibition can explain the finer points of "Gödel's proof" at the level of detail which several trade books have achieved. Visitors stroll from one spot to another. Exhibitions are meant for meandering around. This implies some superficiality. Anyone wanting greater depth should sit down to read or listen to a lecture. In contrast, the format of an exhibition seems well suited to giving an idea of Gödel's intellectual surroundings. That is the right topic for an easy stroll.

So let us take a stroll through Gödel's Vienna, even if that means walking in a Circle. Gödel was an unusually quiet and withdrawn person, but by no means a hermit in his Viennese years. He was a member of the Vienna Circle. This shaped him profoundly, but in an indirect, almost contrary way. In a questionnaire that Gödel filled out much later (but which he never sent off—Dawson excavated it from the *Nachlass*), he states that the most decisive influences on his intellectual development were the lectures by Heinrich Gomperz in philosophy and Philipp Furtwängler in mathematics. One would have expected the names of Moritz Schlick and Hans Hahn instead, the professors of philosophy and mathematics who had founded the Vienna

RIPE IDEALS. Philipp Furtwängler, a cousin of the famous conductor, was a first-rate number theorist. He was partially paralyzed from the neck down and had to be carried into the lecture hall in his chair. His greatest achievement came in 1929 (the year of Gödel's completeness theorem), when he proved another conjecture of Hilbert, the *Hauptidealsatz* (principal ideal theorem) for class fields, at the respectable age of sixty.

Circle (and were certainly no mean lecturers). But no: Gomperz and Furtwängler, who held the introductory lectures for Gödel's cohort, imprinted him for life. There is little reason to doubt that Gödel was a Platonist by the age of nineteen and never wavered in this conviction (Feferman 1984).

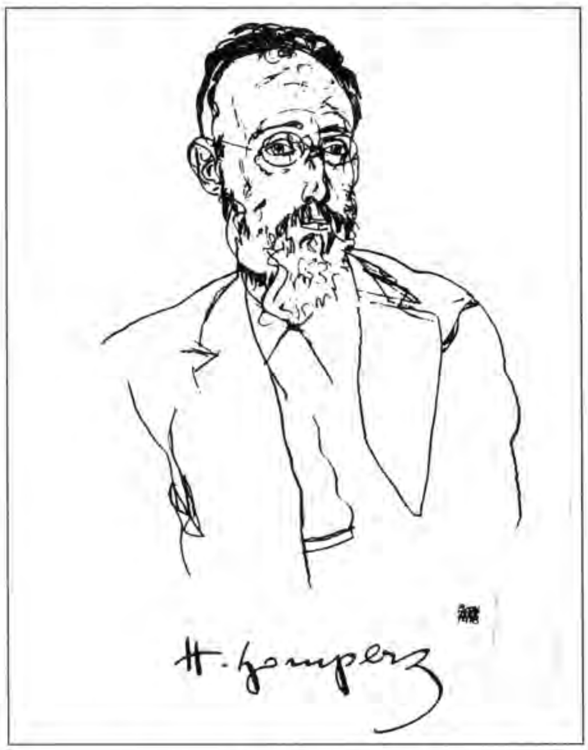
Karl Menger, the professor of geometry who was just four years older than Kurt Gödel and who became his mentor for many years, described how Gödel usually refrained from speaking out, but when he disagreed with something he heard, showed his disagreement by a slight movement of his head (Menger 1994). The sessions of the Vienna Circle gave Gödel many opportunities to do so, during discussions on Wittgenstein or Russell. Most of the younger members of the Circle seem to have displayed a healthy but discreet skepticism towards their more outspoken seniors. Gödel's correspondence indicates that his closest friends in those days were Marcel Natkin and Herbert Feigl, two students of philosophy and mathematics who both did their Ph.D. with Schlick. Both venerated their professor but were not above poking gentle fun at him. "For consolation, I'll send you Schlick's essay, an example that one can talk sensibly only about nonsense. Did you hear from Feigl," wrote Marcel Natkin to Kurt Gödel in the summer break of 1928, "how Wittgenstein and Schlick enjoyed speaking for hours about the unspeakable?"

Hahn became Gödel's thesis adviser but did not have to do much. A careful analysis of the Ph.D. thesis (as found in the university archives, reprinted in the *Collected Works*) and of the version published in the *Monatshefte*, suggests that Gödel slightly adapted the latter to better fit the "party line" of Hahn (Feferman 1984). It was only twelve years later, after having solved two and a half of Hilbert's problems, that Gödel started to express his Platonism publicly. He then could argue that his success was due to the firmness of his conviction on the reality of abstract concepts.

In unison with Menger, Gödel drifted away from the Vienna Circle and became a member of another circle, this time of younger mathematicians, including Georg Nöbeling, Franz Alt, and Abraham Wald. It was to this group that Gödel lectured first on incompleteness. "That's very interesting," said a voice in the awestruck silence ending Gödel's lecture. "You should publish that." (Alt, 1998) "I am consumed with unjustifiable pride," wrote Natkin from Paris. "So you have proved that Hilbert's system of axioms contains unsolvable problems—why, this is not a trifling matter."

The young prodigy was soon pointed out by Karl Menger and John von Neumann to Oswald Veblen, who toured Europe as talent scout for the IAS. The newly founded institute invited Gödel to be among the first group of visiting scholars. Feigl, who had been the first member of the Vienna Circle to emigrate to the U.S., wrote from Iowa: "So you too, my son, like Einstein and all other celebrities, could not help it and had to cross the great water. Well then, probably a permanent position will come out of it in the end, and the Germans and Austrians will again have lost a scientist (racially pure, this time)."

The words of Feigl were prophetic. The Circle of Vienna disbanded rapidly. Menger was one of those who left for the U.S. In 1937 he wrote to Alt, who was still in Vienna: "I believe you should get

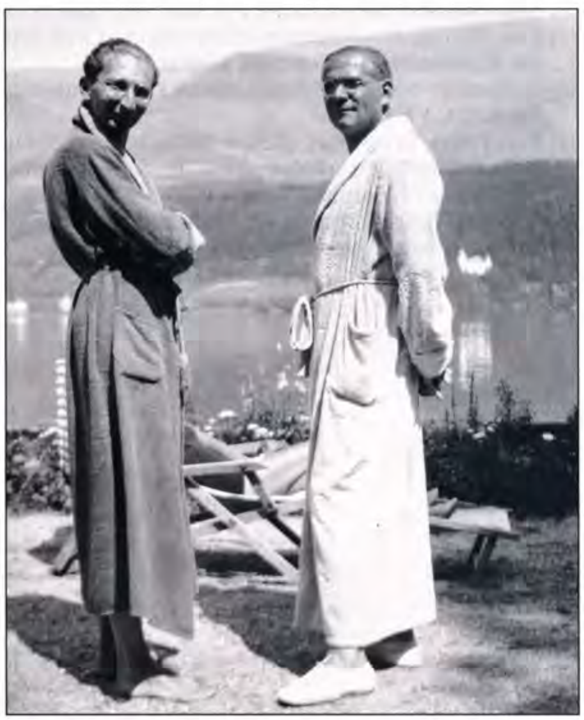


CLASSICAL TASTE. Heinrich Gomperz, philosopher, in a drawing by Egon Schiele. An intriguing literary vignette of Gomperz can be found in the autobiography of Elias Canetti, who studied chemistry in the same building and at the same time as Gödel and who later won a Nobel Prize in literature.

together from time to time, and especially see that Gödel takes part in the *Kolloquium*. It would not only be of greatest benefit for all other participants, but also for himself, though he might not realize it. Heaven knows into what he could entangle himself if he does not talk to you and the other friends in Vienna from time to time. If necessary, be pushy, on my say-so." But by the time that letter reached him, Alt had to work urgently on his own escape.

Menger would later ruminate that Gödel needed to move within a sympathetic group, which would stimulate him to lecture frequently and gently remind him to write things down, and even push him a little to do so, if necessary (Menger 1994). This is what Vienna had been able to provide, for a few blessed years. Hahn and Schlick had managed to create a critical mass (as one says nowadays) of young people studying both philosophy and mathematics. But in those Viennese days the two topics were in the air. The writers Hermann Broch and Robert Musil, and the philosophers Ludwig Wittgenstein and Karl Popper had the same twin interests. Their opinions were widely divergent (as were opinions within the Circle); but that heterogeneity was also probably an advantage for Kurt Gödel. He could have found splendid teachers in Göttingen or Cambridge, but probably nowhere else a similar variety of views. For someone who was perfectly aware that his opinions were unfashionable to the extreme, and who wished to explore a completely new approach, this must have been encouraging.

SKEPTICAL ATTITUDE. Marcel Natkin, a young philosopher and student friend of Gödel, eventually became one of the most eminent photographers in Paris. When Natkin, Feigl, and Gödel met in New York thirty years later, Gödel wrote to his mother: "The two have hardly changed. I do not know whether the same thing can be said of me."



PHILOSOPHICAL CONFIDENCE. Herbert Feigl (with bare feet) and Moritz Schlick (without) on the shores of Lake Millstaedter. Feigl, a student friend of Gödel, later became professor of philosophy at the universities of Iowa and Minnesota, and president of the American Philosophical Association.

The main contribution of the Vienna Circle, then, might have been to give everyone some clear statements to clearly disagree with. Karl Popper is a case in point. For the last sixty years of his life he kept repeating that he did not mind at all never having been invited to a meeting of the Vienna Circle. But his huge first book, *Die Logik der Forschung*, appeared in the series edited by Frank and Schlick, and Hahn had gently said "as kind words as I could have wished". (Popper 1995) Nevertheless, Popper took an almost oedipal pleasure in claiming, later, that the Vienna Circle was dead. "Who killed it? …I am afraid I did it." (Stadler 1997)

Popper was as vehemently anti-Plato as Gödel was Platonist. The two met but never were close. "I recently met a Mr. Popper (philosopher)," wrote Gödel to Menger in 1934. "He has just finished a huge book in which, so he claims, all phil. problems are solved. —Do you think he is any good?" (Gödel, *Collected Works*, 1986–2002)

The novelist Hermann Broch also turned away from logical empiricism, but for entirely different reasons. In the 1920s, Broch was a minor celebrity in the Viennese coffee-house scene, the well-off heir to a textile firm and a womanizer of renown. He decided, at the age of forty, to study mathematics and philosophy and sat through many of the same

STAIRS TO PERDITION. Schlick's murder on the staircase of the university became a *cause célèbre* in the Vienna of 1936. The murderer was released from prison in the fall of 1938.

lectures as Kurt Gödel, his junior by twenty years. Thirty of Broch's notebooks covering these lectures are kept at Duke University. Broch was disappointed by the anti-metaphysical bent of the philosophers of the Vienna Circle, but acknowledged that they had one saving grace: *Krankheitseinsicht* (meaning that they knew how sick they were). In the end, Broch became a major figure in German literature. His second novel, *The Unknown Quantity*, is about a young mathematician who dreams of finding "a logic without axioms". Musil's "Man without Qualities" is also a research mathematician, but Musil's attempt at a "crystal-clear mysticism" is literally worlds apart from the positivism of his friend Richard von Mises, another Viennese who combined mathematics with philosophy.

Hans Nelböck, the man who shot Schlick on the stairs of the university in 1936, and who thus really murdered the Vienna Circle, had studied mathematics and philosophy at the same time as Gödel and Broch and had written his Ph.D. thesis with Schlick, just as Natkin and Feigl had done. In the 1930s, both Nelböck and Gödel were confined for several periods in psychiatric institutions. Both had been living in the same street, the Lange Gasse. At that time, in 1929, Gödel's attention was taken up by another neighbor, who had just moved in, a young masseuse named Adele Nimbursky. He would marry her ten years later. It is not certain that Gödel ever met Schlick's murderer. But novelists with a sense for the bizarre will recognize the leitmotiv of the *Doppelgänger* (the double). Vienna's golden autumn, so full of radiance, was also filled with violence and hate.

Gödel had no wish to return to Vienna after the war. "I am so happy to have escaped from beautiful Europe," he would write to his mother. But if you happen, between May and August, to stopover in beautiful Europe, do come and visit the Gödel exhibition. It is free!

Acknowledgments

The author wishes to acknowledge gratefully the use of material from the Gödel *Nachlass* kept at the Firestone Library for Rare Manuscripts at Princeton University, the archives of the IAS and the University of Vienna, the *Handschriftensammlung* of the Wiener Stadt- und Landesbibliothek and the Vienna Circle Foundation in Amsterdam, in particular Monika Cliburn (-Schlick) and G. M. H. van de Velde (-Schlick). Friedrich Stadler and John Dawson helped generously.

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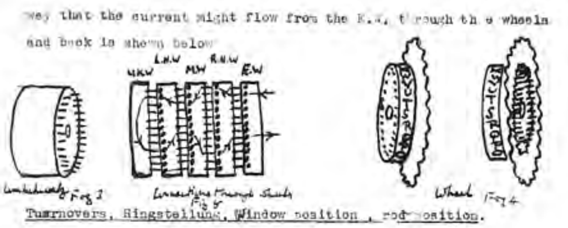
About the Cover

Mathematical theory of the Enigma machine

This month's cover shows the first few pages of Alan Turing's treatise on the Enigma machine, from the master copy typed by Turing himself in 1940, and also containing Turing's sketches and annotations.



It is now held as document HW25/3 in the National Archives of the U. K. (http://www.nationalarchives.gov.uk). A copy made from microfilm, originating in American archives, can also be found in the Turing Digital Archive at http://www.turingarchive.org/browse.php/C/30.

This year's theme for Mathematics Awareness Month is "Mathematics and Internet Security". (The official website is http://www.mathaware.org/index.html.) Mathematics pervades security measures on the Internet, as Susan Landau showed in her article on hash functions in the March issue of the *Notices*, but it's a tough topic to illustrate. The theme is closely connected to the more general one of mathematics in cryptography, however, and in this regard little can compare in dramatic interest to the British work on reading German codes and ciphers at Bletchley Park during World War II. Polish mathematicians began the process early in the 1930s and British mathematicians, most prominently Alan Turing, were major contributors during the war. Turing also made extremely important contributions to the theory of computability, not far removed from the interests of Kurt Gödel, so it seems particularly appropriate that his work appear in this issue, which contains several articles on Gödel.

—Bill Casselman, Graphics Editor
(notices-covers@ams.org)

The Impact of the Incompleteness Theorems on Mathematics

Solomon Feferman

In addition to this being the centenary of Kurt Gödel's birth, January marked 75 years since the publication (1931) of his stunning incompleteness theorems. Though widely known in one form or another by practicing mathematicians, and generally thought to say something fundamental about the limits and potentialities of mathematical knowledge, the actual importance of these results for mathematics is little understood. Nor is this an isolated example among famous results. For example, not long ago, Philip Davis wrote me about what he calls The Paradox of Irrelevance: "There are many math problems that have achieved the cachet of tremendous significance, e.g., Fermat, four-color, Kepler's packing, Gödel, etc. Of Fermat, I have read: 'the most famous math problem of all time'. Of Gödel, I have read: 'the most mathematically significant achievement of the 20th century'. ... Yet, these problems have engaged the attention of relatively few research mathematicians—even in pure math." What accounts for this disconnect between fame and relevance? Before going into the question for Gödel's theorems, it should be distinguished in one respect from the other examples mentioned, which in any case form quite a mixed bag. Namely, each of the Fermat, four-color, and Kepler's packing problems posed a stand-out challenge following extended efforts to settle them; meeting the challenge in each case required new ideas or approaches and intense work, obviously of different degrees. By contrast, Gödel's theorems were simply unexpected, and their proofs, though requiring novel techniques, were not difficult on the scale of things. Setting that aside, my view of Gödel's incompleteness theorems is that their relevance to mathematical logic (and its offspring in the theory of computation) is paramount; further, their philosophical relevance is significant, but in just what way is far from settled; and finally, their mathematical relevance outside of logic is very much unsubstantiated but is the object of ongoing, tantalizing efforts. My main purpose here is to elaborate this last assessment.

Solomon Feferman is professor of mathematics and philosophy, emeritus, at Stanford University. His email address is sf@csli.stanford.edu.

Informal and Formal Axiom Systems

One big reason for the expressed disconnect is that Gödel's theorems are about formal axiom systems of a kind that play no role in daily mathematical work. Informal axiom systems for various kinds of structures are of course ubiquitous in practice, viz. axioms for groups, rings, fields, vector spaces, topological spaces, Hilbert spaces, etc., etc.; these axioms and their basic consequences are so familiar it is rarely necessary to appeal to them explicitly, but they serve to define one's subject matter. They are to be contrasted with foundational axiom systems for the "mother" structures—the natural numbers (Peano) and the real numbers (Dedekind)—on the one hand, and for the general concepts of set and function (Zermelo-Fraenkel) used throughout mathematics, on the other. Mathematicians may make explicit appeal to the principle of induction for the natural numbers or the least upper bound principle for the real numbers or the axiom of choice for sets, but reference to foundational axiom systems in practice hardly goes beyond that.

One informal statement of the basic Peano axioms for the natural numbers is that they concern a structure $(N, 0, s)$ where 0 is in N, the successor function s is a unary one-one map from N into N

which does not have 0 in its range, and the Induction Principle is satisfied in the following form:

(IP) for any property $P(x)$, if $P(0)$ holds and if for all x in N, $P(x)$ implies $P(s(x))$ then for all x in $N, P(x)$ holds.

But this is too indefinite to become the subject of precise logical studies, and for that purpose one needs to say exactly *which* properties P are admissible in (IP), and to do *that* one needs to specify a formal language L within which we can single out a class of well-formed formulas (wffs) A which express the admitted properties. And to do *that* we have to prescribe a list of basic symbols and we have to say which finite sequences of basic symbols constitute well-formed terms and which constitute wffs. Finally, we have to specify which wffs are axioms (both logical and non-logical), and which relations between wffs are instances of rules of inference. The wffs without free variables are those that constitute definite statements and are called the closed formulas or sentences of L. All of this is what goes into specifying a formal axiom system S.

In the case of a formal version of the Peano axioms, once its basic symbols are specified, and the logical symbols are taken to be $\neg$ ("not"), $\wedge$ ("and"), $\vee$ ("or"), $\Rightarrow$ ("implies"), $\forall$ ("for all"), and $\exists$ ("there exists"), one puts in place of the Induction Principle an Induction Axiom Scheme:

(IA) $A(0) \wedge \forall x(A(x) \implies A(s(x))) \implies \forall x A(x)$, where A is an arbitrary wff of the language L and $A(t)$ indicates the result of substituting the term t for all free occurrences of the variable x in A.

N.B. (IA) is not a single axiom but an infinite collection of axioms, each instance of which is given by some wff A of our language.

But what about the basic vocabulary of L? Besides zero and successor, nothing of number-theoretical interest can be derived without expanding it to include at least addition and multiplication. As shown by Dedekind, the existence of those operations as given by their recursive defining conditions can be established using (IP) applied to predicates P involving quantification over functions. But for a formal axiom system PA ("Peano Arithmetic") for elementary number theory, in which one quantifies only over numbers, one needs to posit those operations at the outset. The basic vocabulary of PA is thus taken to consist of the constant symbol 0 and the operation symbols $s, +$, and $\times$ together with the relation symbol $=$. Then the axioms indicated above for zero and successor are supplemented by axioms giving the recursive characterizations of addition and multiplication, namely: $x + 0 = x, x + s(y) = s(x + y), x \times 0 = 0$, and $x \times s(y) = (x \times y) + x$.

Consistency, Completeness, and Incompleteness

All such formal details are irrelevant to the working mathematician's use of arguments by induction on the natural numbers, but for the logician, the way a formal system S is specified can make the difference between night and day. This is the case, in particular, concerning the questions whether S is *consistent*, i.e., no contradiction is provable from S, and whether S is *complete*, i.e., every sentence A is *decided by* S in the sense that either S proves A or S proves $\neg A$. If neither A nor $\neg A$ is provable in S then A is said to be *undecidable by* S, and S is said to be *incomplete*.

As an example of how matters of consistency and completeness can change dramatically depending on the formalization taken, consider the subsystem of PA obtained by restricting throughout to terms and formulas that do not contain the multiplication symbol $\times$. That system, sometimes called Presburger Arithmetic, was shown to be complete by Moses Presburger in 1928, and his proof of completeness also gives a finite combinatorial proof of its consistency.[1] Gödel's discovery in 1931 was that we have a radical change when we move to the full axiom system PA. What Gödel showed was that PA is not complete and that, unlike Presburger Arithmetic, its consistency cannot be established by finite combinatorial means, at least not those that can be formalized in PA. Before going into the mathematical significance of these results, let us take a closer look at how Gödel formulated and established them not only for PA, but also for a very wide class of its extensions S.[2] To do this he showed that the language of PA is much more expressively complete than appears on the surface. A primitive recursive (p.r.) function on N (in any number of arguments) is a function generated from zero and successor both by explicit definition and definition by recursion along N. A p.r. relation (which may be unary, i.e., a set) is a relation whose characteristic function is p.r. Gödel showed that every p.r. function is definable in the language of PA, and its defining equations can be proved there. For example, the operations of exponentiation x^y, the factorial $x!$, and the sequence of prime

[1] *Presburger's work was carried out as an "exercise" in a seminar at the University of Warsaw run by Alfred Tarski. His proof applies the method of elimination of quantifiers to show that every formula is equivalent to a propositional combination of congruences. At its core it makes use of the Chinese Remainder Theorem giving conditions for the existence of solutions of simultaneous congruences.*

[2] *Gödel's initial statement of his results was for extensions of a variant P of the system of* Principia Mathematica, *but a year later he announced his results more generally for a system like PA in place of P; no new methods of proof were required. Nowadays it is known that much weaker systems than PA suffice for his results.*

numbers p_x, each of which is p.r., can all be represented in this way in PA, facts that are not at all obvious.[3] Each instance of a p.r. relation is decidable by PA; for example if R is a binary p.r. relation then for each $n, m \in N$, either PA proves $R(n, m)$ or it proves $\neg R(n, m)$.

Gödel's Incompleteness Theorems

To apply these notions to the language and deductive structure of PA, Gödel assigned natural numbers to the basic symbols. Then any finite sequence σ of symbols gets coded by a number $\#\sigma$, say, using prime power representation; $\#\sigma$ is nowadays called the Gödel number (g.n.) of σ. A relation R between syntactic objects (terms, formulas, etc.) is said to be p.r. if the corresponding relation between g.n.'s is p.r. For example, with a basic finite vocabulary, the sets of terms and wffs are both p.r. Finally a formal system S for such a language is said to be p.r. if its set of axioms and its rules of inference are both p.r. The formal system PA and its subsystems defined above are all p.r.

Throughout the following, S is assumed to be any p.r. formal system that extends PA. Denote by $\mathrm{Proof}_S(x, y)$ the relation which holds just in case y is the g.n. of a proof in S of a wff with g.n. x. Then $\mathrm{Proof}_S(x, y)$ is p.r. Using its definition in PA, the formula $\exists y\ \mathrm{Proof}_S(x, y)$ expresses that x is the g.n. of a provable formula; this is denoted $\mathrm{Prov}_S(x)$. Finally, for each wff A, $\mathrm{Prov}_S(\#A)$ expresses in the language of PA that A is provable. By a diagonal argument, Gödel constructed a closed wff D_S which is provably equivalent in PA to $\neg\mathrm{Prov}_S(\#D_S)$; more loosely, "$D_S$ says of itself that it is not provable in S." And, indeed,

(1) if S is consistent, D_S is not provable in S.

Hence, in ordinary informal terms, D_S is true, so S cannot establish all arithmetical truths. This is one way that Gödel's first incompleteness theorem is often stated, but actually (1) is only the first part of the way that *he* stated it. For that we need a few more slightly technical notions. A sentence A of the language of PA is said to be in $\exists$-form if, up to equivalence, it is of the form $\exists y R(y)$ where R defines a p.r. set, and A is said to be in $\forall$-form if, up to equivalence, $\neg A$ is in $\exists$-form, or what comes to the same, if A can be expressed in the form $\forall y R_1(y)$ with R_1 p.r.[4] Thus D_S is in $\forall$-form and its negation is in $\exists$-form. S is said to be *1-consistent* if every $\exists$-sentence provable in S is true. Automatically, every 1-consistent system is consistent, but the converse is not true: by (1), if S is consistent it remains consistent when we add $\neg D_S$ to it as a new axiom, and the resulting system is not 1-consistent. The following is Gödel's first incompleteness theorem:[5]

(2) if S is 1-consistent then D_S is undecidable by S; hence S is not complete.

It turns out that only the first part, (1), is needed for his second incompleteness theorem. Let C be the sentence $\neg(0 = 0)$, so S is consistent if and only if C is not provable in S; this is expressed by the $\forall$-sentence $\neg\mathrm{Prov}_S(\#C)$, which is denoted Con_S. By formalizing the proof of (1) it may be shown that the formal implication $\mathrm{Con}_S \Rightarrow \mathrm{Prov}_S(\#D_S)$ is provable in PA. But since $\neg\mathrm{Prov}_S(\#D_S) \Rightarrow D_S$ in PA by the diagonal construction, we have $\mathrm{Con}_S \Rightarrow D_S$ too. Hence:

(3) if S is consistent then S does not prove Con_S.

That is Gödel's second incompleteness theorem. Its impact on Hilbert's consistency program has been much discussed by logicians and historians and philosophers of mathematics and will not be gone into here, except to say that it is generally agreed that the program as originally conceived cannot be carried out for PA or any of its extensions. However, various modified forms of the program have been and continue to be vigorously pursued within the area of logic called proof theory, inaugurated by Hilbert as the tool to carry out his program. I recommend Zach (2003) (readily accessible online) for an excellent overview and introduction to the literature on Hilbert's program.

Gödel's Theorems and Unsettled Mathematical Problems

With this background in place we can now return to the question of the impact of the incompleteness theorems on mathematics; in that respect it is mainly the first incompleteness theorem that is of concern, and indeed only the first part of it, namely (1). A common complaint about this result is that it just uses the diagonal method to "cook up" an example of an undecidable statement. What one would really like to show undecidable by PA or some other formal system is a natural number-theoretical or combinatorial statement of prior interest. The situation is analogous to Cantor's use of the diagonal method to infer the existence of transcendental numbers from the denumerability of the set of algebraic numbers; however, that did not provide any natural example. The existence of transcendentals had previously been established by an explicit but artificial example by Liouville.

[3] *The way that is done might interest number theorists; see Franzén (2004), Ch. 4, for an exposition.*

[4] *Standard logical terminology for $\exists$-form and $\forall$-form is Σ^0_1-form and Π^0_1-form, respectively. It should be noted that the formulas in $\exists$-form are closed under existential quantification and those in $\forall$-form under universal quantification. That is, like quantifiers can be collapsed to a single one of that type.*

[5] *Gödel used a stronger, purely syntactic, assumption in place of 1-consistency, that he called ω-consistency.*

Neither argument helped to show that e and π, among other reals, are transcendental, but they did at least show that questions of transcendence are non-vacuous. Similarly, Gödel's first incompleteness theorem shows that the question of undecidability of sentences by PA or any one of its consistent extensions is non-vacuous. That suggests looking for natural arithmetical statements which have resisted attack so far to try to see whether that is because they are not decided by systems that formalize a significant part of mathematical practice, and in particular to look for such statements in $\forall$-form. An obvious candidate for a long time was the Fermat conjecture; now that we know it is true, it would be interesting to see just what principles are needed to establish it from a logical point of view. Some logicians have speculated that it has an elementary proof that can be formalized in PA, but we don't have any evidence so far to settle this one way or the other. Another obvious candidate is the Goldbach conjecture; indeed, Gödel often referred to his independent statements as being "of Goldbach type", by which he simply meant that they are both expressible in $\forall$-form. A far less obvious candidate is the Riemann Hypothesis; Georg Kreisel showed that this is equivalent to a $\forall$-statement (see Davis, Matijasevič, and Robinson (1976), p. 335, for an explicit presentation of such a sentence). No example like these has been shown to be independent of PA or any of its presumably consistent extensions.

Unsolvable Diophantine Problems

Consider any system S containing PA that is known or assumed to be consistent and suppose that A is a $\forall$-sentence conjectured to be undecidable by S. It turns out that proving its undecidability would automatically show A to be true, since $\neg A$ is equivalent to a $\exists$-sentence $\exists y R(y)$; thus if $\neg A$ were true there would be an n such that $R(n)$ is true, hence provable in PA and thence in S. So, finally, $\neg A$ would be provable in S, contradicting the supposed undecidability of A by S. The odd thing about this is that if we want to show a $\forall$-sentence undecidable by a given S, we better expect it to be true. And if we can show it to be true by one means or another, who cares (other than the logician who is interested in exactly what depends on what) whether it can or can't be proved in S? Still, the first incompleteness theorem is tantalizing for its prospects in this direction. The closest that one has come is due to the work of Martin Davis, Hilary Putnam, Julia Robinson, and Yuri Matiyasevich resulting, finally, in the latter's negative resolution of Hilbert's 10th Problem on the algorithmic solvability of diophantine equations (cf. Matiyasevich 1993). It follows from their work that every $\exists$-sentence is equivalent in PA to the existence of natural numbers $x_1, \ldots, x_n$ such that $p(x_1, \ldots, x_n) = 0$, where p is a suitable polynomial with integer coefficients. So each $\forall$-sentence A is equivalent to the non-solvability of a suitable diophantine equation, in particular, sentences known to be undecidable in particular systems such as the Gödel sentences D_S. The trouble with this result compared to open questions in the literature about the solvability of specific diophantine equations in two or three variables or of low degree is that the best value known for the above representation in terms of number of variables is $n = 9$, and in terms of degree d with a much larger number of variables is $d = 4$ (cf. Jones 1982).

Combinatorial Independence Results

Things look more promising if we consider $\forall\exists$-sentences, i.e., those that can be brought to the form $\forall x \exists y R(x, y)$ with R p.r.[6] The statement that there are infinitely many y's satisfying a p.r. condition $P(y)$ is an example of a $\forall\exists$-sentence, since it is expressed by $\forall x \exists y (y > x \wedge P(y))$. In particular, the twin prime conjecture has this form. Again, no problems of prior mathematical interest that are in $\forall\exists$-form have been shown to be undecidable in PA or one of its extensions. However, in 1977, Jeff Paris and Leo Harrington published a proof of the independence from PA of a modified form of the Finite Ramsey Theorem. The latter says that for each n, r and k there is an m such that for every r-colored partition π of the n-element subsets of $M = \{l, m\}$ there is a subset H of M of cardinality at least k such that H is homogeneous for π, i.e., all n-element subsets of H are assigned the same color by π. The Paris-Harrington modification consists in adding the condition that $\mathrm{card}(H) \geq \min(H)$. It may be seen that this statement, call it PH, is in $\forall\exists$-form. Their result is that PH is not provable in PA. But they also showed that PH is true, since it is a consequence of the Infinite Ramsey Theorem. The way that PH was shown to be independent of PA was to prove that it implies Con_{PA}; in fact, it implies the formal statement of the 1-consistency of PA. That work gave rise to a number of similar independence results for stronger systems S, in each case yielding a $\forall\exists$-sentence A_S which is a variant of a combinatorial result already in the literature such that A_S is true but unprovable from S on the assumption that S is 1-consistent. The proof consists in showing that A_S implies (and is in some cases equivalent to) the formal statement of its 1-consistency. However, no example is known of an unprovable $\forall\exists$-sentence whose truth has been a matter of prior conjecture.

[6] *Standard logical terminology for these is Π^0_2 sentences.*

Set Theory and Incompleteness

Gödel signaled a move into more speculative territory in footnote 48a (evidently an afterthought) of his 1931 paper:

> As will be shown in Part II of this paper, 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... 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.[7]

The reason for this, roughly speaking, is that for each system S the notion of truth for the language of S can be developed in an axiomatic system S' for the subsets of the domain of interpretation of S; then in S' one can prove by induction that the statements provable in S are all true, and hence that S is consistent. Implicit in the quote is that S' ought to be accepted if S is accepted. Later, in his famous article on Cantor's Continuum Problem (1947), Gödel pointed to the need for new set-theoretic axioms to settle specifically set-theoretic problems, such as the Continuum Hypothesis (CH).

At that point, it was only known as a result of his earlier work that AC (the Axiom of Choice) and CH are consistent relative to Zermelo-Fraenkel axiomatic set theory ZF.[8] In Gödel's 1947 article he argued that CH is a definite mathematical problem, and, in fact, he conjectured that it is false while all the axioms of ZFC (= ZF + AC) are true. Hence CH must be independent of ZFC; he thus concluded that one will need new axioms to determine the cardinal number of the continuum. In particular, he suggested for that purpose the possible use of axioms of infinity not provable in ZFC:

> The simplest of these ... assert the existence of inaccessible numbers... The latter axiom, roughly speaking, means nothing else but that the totality of sets obtainable by exclusive use of the processes of formation of sets expressed in the other axioms forms again a set (and, therefore, a new basis for a further application of these processes). Other axioms of infinity have been formulated by P. Mahlo. ... These axioms show clearly, not only that the axiomatic system of set theory as known today is incomplete, but also that it can be supplemented without arbitrariness by new axioms which are only the natural continuation of those set up so far. (Gödel 1947, as reprinted in 1990, p. 182).

Whether or not one agreed with Gödel about the ontological underpinnings of set theory and in particular about the truth or falsity of CH, in the following years it was widely believed to be independent of ZFC; that was finally demonstrated in 1963 by Paul Cohen using a new method of building models of set theory. And, contrary to Gödel's expectations, it has subsequently been shown by an expansion of Cohen's method that CH is undecidable in every plausible extension of ZFC that has been considered so far, at least along the lines of Gödel's proposal (cf. Martin 1976 and Kanamori 2003). For the most recent work on CH, see the conclusion of Floyd and Kanamori (this issue).

But what about arithmetical problems? For a number of years, Harvey Friedman has been working to produce mathematically perspicuous finite combinatorial $\forall\exists$-statements A whose proof requires the use of many Mahlo cardinals and even stronger axioms of infinity (such as the so-called subtle cardinals) and has come up with a variety of candidates; for a fairly recent report, including work in progress, see Friedman (2000).[9] The strategy for establishing that such a statement A needs a system S incorporating the strong axioms in question is like that above: one shows that A is equivalent to (or in certain cases is slightly stronger than) the 1-consistency of S. In my discussion of this in Feferman (2000), p. 407, I wrote: "In my view, it is begging the question to claim this shows we need axioms of large cardinals in order to demonstrate the truth of such A, since this only shows that we 'need' their 1-consistency. However plausible we might find the latter for one reason or another, it doesn't follow that we should accept *those axioms*

[7] *Part II of Gödel (1931) never appeared. Also promised for it was a full proof of the second incompleteness theorem, the idea for which was only indicated in Part I. He later explained that since the second incompleteness theorem had been readily accepted there was no need to publish a complete proof. Actually, the impact of Gödel's work was not so rapid as this suggests; the only one who immediately grasped the first incompleteness theorem was John von Neumann, who then went on to see for himself that the second incompleteness theorem must hold. Others were much slower to absorb the significance of Gödel's results (cf. Dawson 1997, pp. 72–75.) The first detailed proof of the second incompleteness theorem for a system Z equivalent to PA appeared in Hilbert and Bernays (1939).*

[8] *See Floyd and Kanamori in this issue of the* Notices.

[9] *More recently, Friedman has announced the need for such large cardinal axioms in order to prove a certain combinatorial statement A that can be expressed in $\forall$-form; see the final section of Davis (this issue). Here, A implies Con_S and is itself provable in S' for S and S' embodying suitable large cardinal axioms.*

themselves as first-class mathematical principles." (Cf. also op. cit. p. 412).

Prospects and Practice

As things stand today, these explorations of the set-theoretical stratosphere are clearly irrelevant to the concerns of most working mathematicians. A reason for this was also given by Gödel near the outset of his 1951 Gibbs lecture (posthumously published in 1995), where he said that the "phenomenon of the inexhaustibility of mathematics" follows from the fact that "the very formulation of the axioms [of set theory over the natural numbers] up to a certain stage gives rise to the next axiom. It is true that in the mathematics of today the higher levels of this hierarchy are practically never used. It is safe to say that 99.9% of present-day mathematics is contained in the first three levels of this hierarchy." In fact, modern logical studies have shown that the system corresponding to the second level of this hierarchy, called second-order arithmetic or analysis and dealing with the theory of sets of natural numbers, already accounts for the bulk of present-day mathematics. Indeed, much weaker systems suffice, as is demonstrated in Simpson (1999). Even more, I have conjectured and verified to a considerable extent that all of current scientifically applicable mathematics can be formalized in a system that is proof-theoretically no stronger than PA (cf. Feferman 1998, Ch. 14).

Whether or not the kind of inexhaustibility of mathematics discovered by Gödel is relevant to present-day pure and applied mathematics, there is a different kind of inexhaustibility which is clearly significant for practice: no matter which axiomatic system S is taken to underlie one's work at any given stage in the development of our subject, there is a potential infinity of propositions that can be demonstrated in S, and at any moment, only a finite number of them have been established. Experience shows that significant progress at each such point depends to an enormous extent on creative ingenuity in the exploitation of accepted principles rather than essentially new principles. But Gödel's theorems will always call us to try to find out what lies beyond them.

Note to the reader: Gödel's incompleteness paper (1931) is a classic of its kind; elegantly organized and clearly presented, it progresses steadily and efficiently from start to finish, with no wasted energy. The reader can find it in the German original along with a convenient facing English translation in Vol. I of his *Collected Works* (1986). I recommend it highly to all who are interested in this landmark in the history of our subject.

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The Popular Impact of Gödel's Incompleteness Theorem

Torkel Franzén

Among Gödel's celebrated results in logic, there are two that can be formulated in terms that are intelligible in a general way even to those unfamiliar with the technicalities involved. The first is his completeness theorem for first order logic. This theorem, which is not widely known outside the world of logic, can be formulated as saying that every statement that follows logically from a set of axioms in a formalized language, such as that used in Zermelo-Fraenkel set theory with the axiom of choice (ZFC) or first order Peano Arithmetic (PA), can be proved using those axioms and the rules of logic. A general audience of nonmathematicians would probably find this statement of the completeness theorem intelligible but unexciting. After all, isn't that what "follows logically" means? It is no easy task to explain the distinction between the semantic concept of logical consequence and the purely formal notion of derivability so as to bring out the importance of this result for an audience unacquainted with logic or mathematics. By the time the expositor is done explaining that the proof of completeness depends essentially on the language being first order, few interested listeners or readers are likely to remain. The distinction between first order and higher order languages, although logically highly significant, is not one that holds any immediate appeal to the imagination.

The second result, Gödel's incompleteness theorem, is a very different matter. "Every sufficiently strong axiomatic theory is either incomplete or inconsistent." Many nonmathematicians at once find this fascinating and are ready to apply what they take to be the incompleteness theorem in many different contexts. The task of the expositor becomes, rather, to dampen their spirits by explaining that the theorem doesn't really apply in these contexts. But as experience shows, even the most determined wet blanket cannot prevent people from appealing to the incompleteness theorem in contexts where its relevance is at best a matter of analogy or metaphor. This is true not only of the first incompleteness theorem (as formulated above), but also of the second incompleteness theorem, about the unprovability in a consistent axiomatic theory T of a statement formalizing "T is consistent."

Supposed applications of the first incompleteness theorem in nonmathematical contexts usually disregard the fact that the theorem is a statement about formal systems and is stated in terms of mathematically defined concepts of consistency and completeness. This mathematically essential aspect is easily set aside, since "complete", "consistent", and "system" are words that are used in many different ways outside formal logic. Thus the incompleteness theorem has been invoked in justification of claims that quantum mechanics, the Bible, the philosophy of Ayn Rand, evolutionary biology, the legal system, and so on, must be incomplete or inconsistent. To dismiss such invocations of the incompleteness theorem is not to say that it doesn't make good sense to speak of these various "systems" as complete or consistent, incomplete or inconsistent. When people say that

Torkel Franzén is Universitetslektor at Luleå University of Technology in Sweden. His email address is `torkel@sm.luth.se`.

the Bible is inconsistent, they are arguing that it contains apparently irreconcilable statements, and those who regard the Bible as a complete guide to life presumably mean that they can find answers in the Bible to all questions that confront them about how to live their lives. Einstein, in regarding quantum mechanics as incomplete (although consistent), believed that it is possible to find a more fundamental physical theory. The system of laws of any country is incomplete or inconsistent or both in the sense that there are always situations in which conflicting legal arguments can be brought to bear, or in which no statute seems applicable. But the incompleteness theorem adds nothing to such claims or observations, for two reasons. The first is that these "systems" are not at all formal systems in the logical sense. There is no formally specified language, and there are no formal rules of inference in the logical sense associated with quantum mechanics, the Bible, a system of laws, and so on. What follows or does not follow from a religious or philosophical text, a scientific theory, or a system of laws is not determined by any formal rules of inference, such as might be implemented on a computer, but is very much a matter of interpretation, argument, and opinion, where the relevant reasoning is limited only by the vast boundaries of human thought in scientific, religious, political, or philosophical contexts.

The second reason for the irrelevance of Gödel's theorem in such discussions is that the incompleteness of any sufficiently strong consistent axiomatic theory established by that theorem concerns only what may be called the *arithmetical component* of the theory. A formal system has such a component if it is possible to interpret some of its statements as statements about the natural numbers, in such a way that the system proves some basic principles of arithmetic. Given this, we can produce (using Rosser's strengthening of Gödel's theorem in conjunction with the proof of the Matiyasevich-Davis-Robinson-Putnam theorem about the representability of recursively enumerable sets by Diophantine equations) a particular statement of the form "The Diophantine equation $p(x_1, \ldots, x_n) = 0$ has no solution" which is undecidable in the theory, provided it is consistent. While it is mathematically a very striking fact that any sufficiently strong consistent formal system is incomplete with respect to this class of statements, it is unlikely to be thought interesting in a nonmathematical context where completeness or consistency (in some informal sense) is at issue. Nobody expects the Bible, the laws of the land, or the philosophy of Ayn Rand to settle every question in arithmetic.

There is also a different kind of appeal to the first incompleteness theorem outside mathematics, one that recognizes that the theorem applies only to formal mathematical theories. This is a line of thought that tends to appeal to postmodernists and theologians. From this point of view, the incompleteness theorem shows that even in mathematics, that supreme bastion of reason, truth is either beyond us or a matter of more or less arbitrary consensus rather than objective fact. Given our most powerful mathematical theory, we know that, assuming its consistency, we can produce an arithmetical statement such that we can add either that statement or its negation to the theory, obtaining incompatible theories that are still consistent. So either reason is powerless in this context (as in the wider context of the universe as a whole, with truth ultimately resting only in God), or there is no other truth than that which we more or less arbitrarily agree upon (just as in the physical sciences, according to this line of thought). Either way, after Gödel's theorem, mathematics flounders in a sea of undecidability.

When we look at mathematical practice, however, we find that mathematicians, although generally aware of the phenomenon of incompleteness, and therewith of the theoretical possibility that a problem they are working on may be unsolvable in the current axiomatic framework of mathematics, are by no means floundering in a sea of undecidability.

In the year 1900 David Hilbert made a famous affirmation in his presentation of twenty-three problems facing mathematics in the twentieth century [Browder 1976]. At first glance, it might be thought that the incompleteness theorem scuttles the confidence expressed in this affirmation:

> Take any definite unsolved problem, such as the question as to the irrationality of the Euler-Mascheroni constant C, or the existence of an infinite number of prime numbers of the form $2^n + 1$. However unapproachable these problems may seem to us, and however helpless we stand before them, we have, nevertheless, the firm conviction that their solution must follow by a finite number of purely logical processes. ...This conviction of the solvability of every mathematical problem is a powerful incentive to the worker. We hear within us the perpetual call: There is the problem. Seek its solution. You can find it by pure reason, for in mathematics there is no *ignorabimus*.

Although Hilbert did not specify just what he meant by a "definite" problem, it is no doubt significant that his two examples are statements that can be formulated in arithmetical terms. Today, mathematicians have accepted that some apparently straightforward questions in set theory, such as the very first problem on Hilbert's list, that of the truth or falsity of Cantor's continuum hypothesis, cannot in fact be settled by a mathematical proof as proof is ordinarily understood in mathematics today. Instead, we must either rest content with proving hypothetical statements such as "Assuming CH, there is a group G with the properties..." or extend set theory by new axioms. Also, mathematicians who work in set theory or areas closely connected with set theory have learned to recognize the kind of problem or conjecture that may well be affected by the incompleteness of set theory. (It should be emphasized that this category of incompleteness, although established on the basis of the pioneering work in set theory by Gödel, and later Paul Cohen, is not a consequence of the incompleteness theorem.)

The situation is different with Hilbert's examples of "definite unsolved problems." It would be startling indeed if it turned out that ZFC does not settle whether or not there are infinitely many Fermat primes. In such a case, very few mathematicians would be content to note that we can consistently take the number of Fermat primes to be either finite or infinite, and leave it at that. Rather, mathematical instinct, if nothing else, tells us that whether or not there are infinitely many Fermat primes is not a question that can be meaningfully settled by stipulation, but if it can be settled at all calls for an argument that we perceive as mathematically compelling. Given such an incompleteness result, the search for new axioms in mathematics would take on a new urgency.

However, no famous arithmetical conjecture has been shown to be undecidable in ZFC. We do know that certain natural statements formalizable in arithmetic are undecidable in ZFC (given the consistency, or more accurately what is called in logic the 1-consistency, of ZFC), typically consistency statements, such as a statement formalizing "ZFC is consistent." Here again mathematical instinct tells us that whether or not ZFC is consistent cannot be meaningfully settled by stipulation, but statements of this kind are not at all what mathematicians normally seek to prove. Mathematicians tend to be content with accepting that the consistency of the most powerful formal theory to which they ordinarily refer in foundational contexts cannot be proved in ordinary mathematics, without thereby concluding that their own mathematical efforts are likely to run up against the barrier of undecidability. For, while we have no basis for a general claim that every arithmetical problem that arises naturally in mathematics is decidable in ZFC, we don't have a single example of an arithmetical problem—about primes or Diophantine equations or other such things—that has occurred to mathematicians in a natural mathematical context being shown to be unsolvable in ZFC. From a logician's point of view, it would be immensely interesting if some famous conjecture in arithmetic turned out to be undecidable in ZFC, but it seems too much to hope for. In short, while Hilbert's affirmation does not have any theoretical support from logic, logic does not refute that affirmation, as naturally understood from the point of view of the working mathematician.

It is thus understandable that the first incompleteness theorem has not had much of a "popular impact" among mathematicians, who are unlikely to seek to apply a mathematical theorem to the Bible and so on, and who are, for the reasons indicated, not overly concerned about the possibility of an arithmetical problem that they are working on being unsolvable in current mathematics. Mathematics may be "floundering" as far as solving a particular problem is concerned, but this neither leads to any inclination to regard problems such as those mentioned by Hilbert as in any way solvable by fiat or consensus, nor instills any sense that the problem may be unsolvable. This natural attitude is, furthermore, supported both by experience and by logical and philosophical argument, as briefly touched on above.

The second incompleteness theorem, although not as often referred to in nonmathematical contexts, has also prompted theologians and postmodernists to reflect that since mathematics cannot prove its own consistency, reason is powerless to justify itself, so that either there is no justification to be had, or reason can be supported only by faith. Without going into detail about such ideas, it is a relevant observation that a somewhat similar line of thought seems to have had considerable "popular impact" even among mathematicians, in their more philosophical moments. What I have in mind here is the following. Mathematicians often tend to regard proofs of consistency, not only of ZFC but of such very much weaker theories as PA, as somehow more unattainable or problematic than proofs of ordinary arithmetical statements. Indeed, it is not uncommon for mathematicians to say that arithmetic cannot be proved consistent. Thus Ian Stewart, in summing up the second incompleteness theorem for popular consumption, remarks that "Goedel showed that...if anyone finds a proof that arithmetic is consistent, then it isn't!" ([Stewart 1996], p. 262)

What is odd about such remarks is that we can easily, indeed trivially, prove PA consistent using reasoning of a kind that mathematicians otherwise use without qualms in proving theorems of arithmetic. Basically, this easy consistency proof observes that all theorems of PA are derived by valid logical reasoning from basic principles true of the natural numbers, so no contradiction is derivable in PA. It appears that many mathematicians have come to absorb the view that a consistency proof for PA is not really a consistency proof unless it convinces somebody who does not accept the axioms of PA as expressing valid principles of mathematics that PA is nevertheless consistent. The second incompleteness theorem and general experience do indeed indicate that no such proof is to be expected. If we were to make similar demands on proofs of arithmetical statements in general, we would be forced to the conclusion that it is equally impossible to prove the prime number theorem, Dirichlet's theorem, and so on. The insight underlying the idea that it is impossible to prove, in this sense, the consistency of arithmetic is a perfectly valid one, but it has nothing to do with Gödel's theorem. Instead it is the insight, familiar since antiquity, that we cannot prove everything. We need to start from some basic principles in our mathematical reasoning, principles that we can justify only in informal terms. The principles formalized in PA are the infinity of the natural number series, the basic properties of addition and multiplication, and the principle of mathematical induction. As long as we accept these principles as mathematically valid—as a large majority of mathematicians do in practice—there is no reason why we should not accept a proof of the kind described as proving the consistency of PA, just as we accept other mathematical proofs that depend on the validity of these principles. Those who do have genuine doubts about the consistency of PA will of course not accept this proof of consistency, but then there is no reason why they should accept standard proofs of the prime number theorem, Dirichlet's theorem, and so on, either.

It should be noted that in the logical literature, there are various nontrivial consistency proofs for PA, but the question of their interest and content is a subtle one, and I think it can be safely said that they will not convince anybody who has genuine doubts about the consistency of PA.

Of course, the above comments do not apply to every question of consistency. For example, the consistency of PA extended with the axiom "PA is inconsistent" is established only through the proof of the second incompleteness theorem itself, and proving the consistency of PA extended with Goldbach's conjecture as a new axiom is equivalent to proving Goldbach's conjecture. In these cases, the theory whose consistency is at issue is not one that formalizes basic principles of mathematical reasoning.

A point that deserves to be made in this connection is that the significance of consistency proofs as a means of justifying our mathematical reasoning is easily overstated. For a mathematician, it may at times seem convenient to refer to consistency in response to philosophical prodding about the truth or validity of mathematical axioms and methods of reasoning: only consistency matters, not the existence of the objects studied in mathematics or the philosophical justifiability of mathematics, and as far as we know, mathematics as it stands today is consistent. But such a view is at odds with how we actually think about arithmetical problems in mathematics. For example, there is no logical basis for claiming that there are infinitely many twin primes if all we know is that PA is consistent and proves the twin prime conjecture. Consistent theories of arithmetic may prove false theorems (when we are not talking about theorems having the logical form of Goldbach's conjecture), and if we conclude that there are infinitely many twin primes on the basis of a proof in some particular mathematical framework, the mere consistency of that framework is insufficient to justify our conclusion.

There is of course much more that could be said about the impact of the incompleteness theorem outside the field of logic proper. For one thing, there is the whole subject of Lucas-Penrose arguments in the philosophy of mind, which seek to establish that the human mind does not work on mechanical principles in mathematics by appealing to the incompleteness theorem. A more extensive discussion can be found in [Franzén 2005].

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In Quest of Kurt Gödel: Reflections of a Biographer

John W. Dawson Jr.

Since the days of E. T. Bell the writing of mathematical biographies has gradually matured. Nevertheless, chronicling the life of a mathematician remains a difficult undertaking. The question of audience is paramount, and the response to it determines both the style of presentation and the level of detail. Balancing the demands of lucidity and mathematical accuracy against the constraints of popular understanding presents a particularly vexing challenge.

The observations that follow are based upon my own experiences in writing a biography of Kurt Gödel [Dawson 1997]. I hope that others who consider becoming involved in biographical endeavors may find them of value.

Who Should Write Mathematical Biographies?

Interest in the lives of mathematicians and in the history of mathematics in general has increased markedly in recent years. More mathematical biographies are now being written, they are receiving greater attention from reviewers and readers, and the standards and sophistication of the genre have improved substantially. Writing a mathematical biography has accordingly become both a more rewarding and a more demanding task—the more so the broader the audience to whom the biography is addressed.

Most mathematical biographies, unsurprisingly, are written by and for mathematicians. Some, aimed at a more general readership, have been written by journalists, among whom Constance Reid is the preeminent example; and there are a few, such as [Lützen 1990], whose authors are trained historians of mathematics. The latter, however, are a rare breed. In general, historians of science have displayed an astonishing lack of interest in mathematics, in part, perhaps, because the private nature of mathematical research is less amenable to the sociological kind of analysis that is presently so fashionable in historical studies of the natural sciences.

Because of the highly technical nature of modern mathematics one might presume that only a mathematician can adequately understand and explain the work of another. But a biography is not a textbook. It is a portrayal of a life. And recounting the life of a mathematician requires a sensitivity to human values as well as an understanding of the details of theorems and proofs.

All too often, biographical memoirs written by mathematicians are anecdotal in nature and focus on mathematical results rather than on the personality or habits of life and work of the individual in question. The authors are frequently former colleagues, now past the most productive years of their own careers (understandably enough, since few who are actively involved in mathematical research or concerned about professional advancement can afford the time to engage in serious biographical scholarship). In many cases they are or were close friends of the subject and so may be unable to evaluate the person's character and contributions objectively. They may also be too close to the mathematical subject matter, so that, despite having a thorough understanding of the technical details, they may lack historical perspective concerning the development of the underlying mathematical ideas. In particular, many mathematicians cleave to the Whiggish view that the development of mathematics has been a story of continuing and inevitable progress.

To Whom Should Mathematical Biographies be Addressed?

For the mathematical biographer there is a strong temptation to preach to the converted. To be sure, writing for an audience of mathematicians demands great precision in the description of mathematical details. But much less effort need be devoted to explaining the concepts involved, or to

John W. Dawson Jr. is professor of mathematics at the York campus of Pennsylvania State University. His email address is `jwd7@psu.edu`.

developing interest in what the person in question did. And outside the mathematical community sales of any mathematical biography are likely to be few. That, however, is disturbing and suggests that there are larger purposes that mathematical biography might serve.

In that regard the biographical writings of E. T. Bell are instructive. They have been much dispraised by professional historians, both because of Bell's tendency to romanticize his subjects and because of factual errors in some of his accounts. In his *Mathematics, Queen and Servant of Science*, for example, Bell asserts that Gödel received a degree in engineering from the University of Brno—a statement that has no basis in fact. Such sloppy scholarship cannot be defended, of course. Yet the positive impact that Bell's writings have had cannot be denied either. Indeed, several mathematicians of stature have attributed the awakening of their interest in the discipline to their reading of *Men of Mathematics*. Julia Robinson, for one, recalled that she hardly knew what mathematics was until she read Bell's book. "I cannot," she declared, "overemphasize the importance of such books...in the intellectual life of a student like myself [who was] completely out of contact with research mathematicians." ([Reid 1996], p. 25)

Not all readers would share Robinson's attraction to mathematics as a career. But surely our field would benefit from a wider appreciation of what it is that mathematicians do. We are not accountants, as so many seem to think, and to combat that widespread misimpression it is important to communicate to laymen how genuinely exciting the exploration of mathematical ideas can be.

In the end, it is up to each individual author to decide what audience to address. In doing so, he or she must consider the particulars of the life to be chronicled, whether the story has been told before, and how accessible the mathematical ideas are that must be discussed. In addition, the would-be author must decide whether or not he or she is intellectually suited to the task at hand. I, for example, am too much a child of my own time to consider writing about a figure from an earlier century.

Becoming a Biographer: My Own Experience

When I began my studies of Gödel, a few years after his death, very little about him had appeared in print. I had no idea what sources might be available, no reason to expect that details of his life could be reconstructed to any considerable extent, and no intention of becoming his biographer.

Shortly before, I had achieved tenure at a two-year branch campus of Penn State. My department was supportive, I had good access to library resources, and my teaching load was not unduly heavy. But I found myself out of touch with colleagues with whom I could discuss research questions in logic, and the joys of teaching calculus term after term had begun to pall. I felt that I was losing contact with my discipline and was no longer able to contribute productively to mathematical research.

To avoid intellectual stagnation I resolved to go back and study the works of the masters. The obvious place for a logician to start was Gödel's writings; but to my surprise, I found that no list of them had ever been compiled. Preparing an annotated bibliography of his published works thus appeared to be a worthwhile endeavor—one I felt I was capable of undertaking and that was a necessary first step toward the larger goal of compiling a comprehensive edition of those works.

I made a firm decision to take on the bibliographic task when, following a tip from my friend Fred Rickey, I discovered three short papers on geometry that Gödel had published in the 1930s, which had not been cited by any previous commentators. That so aroused my curiosity that I began a detailed search of the literature. The result was [Dawson 1983], whose appearance brought my efforts to the attention of others in the logical community and led straightaway to my complete immersion in Gödel studies. On the strength of that compilation I was invited, at almost the same time, both to become one of the editors of Gödel's *Collected Works* and to undertake the cataloguing of his *Nachlass* (literary remains) at the Institute for Advanced Study (IAS).

While compiling my list of Gödel's publications I began to wonder about his unpublished manuscripts as well. I had no idea how extensive they might be or what condition they might be in. I did know that other scholars had tried without success to gain access to them, and I had heard that many of the papers were written in some sort of shorthand; so the prospect of my making much headway with them seemed remote.

How, within a few months, I was offered the opportunity to catalog those papers is a story too long to relate in detail here. Suffice it to say that I was persistent in making inquiries to the IAS, and that, unknown to me, the mathematicians there were faced with the problem of deciding what to do with donated materials for which the IAS had no proper storage facilities. I was due for a sabbatical the coming year, so I happened to make my inquiries at just the right time.

It took me two full years to complete the cataloging. The problem of reading Gödel's Gabelsberger shorthand was overcome with the help of my wife, who volunteered to learn that now-obsolete system. Through our combined efforts I became more familiar than anyone else with the contents of Gödel's *Nachlass*, and my work on the *Collected Works* edition brought me into contact with such scholars as Solomon Feferman and Jean van Heijenoort, whose experience and expertise immeasurably deepened my own understanding of Gödel's accomplishments.[1]

I soon realized that there was no dearth of documentary sources to work with. Quite the contrary! Gödel's *Nachlass* contained a great deal of scientific interest, including correspondence, manuscripts, and research notebooks. Gödel saved much that others would have discarded, some of which, such as the library slips for books he checked out over the years, constituted important biographical resources. But there was a great deal of chaff as well (letters from cranks, luggage tags, miscellaneous memoranda slips, etc.), and there were also some notable gaps. There were, for example, no financial records after his emigration to America, and no letters to or from his mother or brother. Nor did Gödel ever keep a diary. He did, however, keep a daily record of his body temperature and milk of magnesia consumption!

In the midst of my cataloging efforts a major international logic conference took place in Salzburg, and that event gave me the opportunity both to organize a commemorative session for Gödel in his former homeland and to travel to Vienna and Brno (Gödel's birthplace), where I was able to meet his brother, to photograph sites associated with Gödel's childhood, and to become acquainted with Austrian scholars who were aware of sources unknown to me. In particular, through the efforts and generosity of Werner DePauli-Schimanovich and Eckehart Köhler I was able to obtain photocopies of Gödel's surviving letters to his mother (now preserved in the Wiener Stadt- und Landesbibliothek), of documents from the archives of the University of Vienna, of some early photographs of Gödel, and of a memoir about him by Karl Menger (eventually published posthumously, in a revised English version, as a chapter in [Menger 1994]).

In Princeton and elsewhere I also tried to interview as many as I could of those who had known Gödel personally,[2] knowing full well that time was of the essence if I were to do so before my informants' health and memories failed. Among those with whom I spoke was Abraham Pais, whose biography of Einstein [Pais 1982] had been published not long before. When I asked him if he had any advice for a would-be biographer, he replied simply, "Patience".

[1] *For a retrospective account of the Gödel* Collected Works *project see [Feferman 2005].*

[2] *I myself never met Gödel, nor did I ever have any correspondence with him.*

The Task of Writing

By the time I returned home from Princeton I had done most of the necessary data gathering. I brought back with me many folders of photocopied material, and as a result of my cataloguing experience I had formed an overall view both of the structure of Gödel's life and of the sources available to draw upon. I knew that a great deal of reading, note-taking, and reflection lay ahead before I could commence writing. The problem was to find time for all that in the midst of my teaching responsibilities.

Those and other commitments forced me to heed Pais's advice. I spent the next seven years—the time until my next sabbatical—studying the various documentary materials, fitting pieces of the puzzle together, and, above all, developing a view of what sort of person Gödel had been.

Establishing such a viewpoint is crucial to the success of any biographical endeavor. For every biography, rightly so called, must portray a life from a definite perspective. The point of view chosen will vary from one biographer to another—that is why there can be more than one biography of the same person—and it will necessarily reflect the author's own background and biases. Accordingly, no biography can ever be the final word on its subject. But without some perspective around which to organize the narrative, the account will be an undirected chronicle of events. As with all history, biographical writing demands interpretation of the data.

By the fall of 1991, when my second sabbatical commenced, I was ready to begin writing. By then I had extracted a great many details about Gödel's life from the sources I had studied, and I felt that I must begin to assemble them into a narrative while my memory of them was still fresh. Although there were still some loose ends, I had formed a definite idea of what made Gödel tick; I could envision the chapter structure of the book quite clearly; and I knew from Gödel's own example that if I waited too long my book might never be written at all. The experience of cataloguing his *Nachlass* had also taught me an important lesson about tackling big projects: One dare not look too far ahead, lest the work remaining to be done appear too daunting. I had never written a book before, but I knew that in doing so I would have to keep my head down and concentrate on one chapter at a time.

During my sabbatical year I wrote seven of the fourteen chapters, at the rate of about one a month. I was pleased with the progress I was able to make, but only too well aware that once my teaching duties resumed my pace would slow abruptly.

In fact, it took four more years to complete the manuscript. Yet, as things turned out, had I begun earlier I would have had much rewriting to do; for, quite by chance, in March of 1992 I discovered a new source of information that significantly enriched my knowledge of Gödel's later years: the diaries of the economist Oskar Morgenstern.

About six months earlier I had seen an announcement in the newsletter of the History of Science Society that Morgenstern's papers had been donated to the archives at Duke University. I knew that Morgenstern had had a long-standing friendship with Gödel, so I was mildly interested. But since his widow Dorothy was one of those I had interviewed during my stay in Princeton, and since she had been quite willing to share her recollections of Gödel with me and to talk about her husband's relationship with him, I didn't expect to find much in Morgenstern's papers that I didn't already know. I was not aware that he had kept any diaries, but when I later learned of their existence, I thought it unlikely that they would contain very much of relevance to my interests. It did not occur to me that his widow might not read German and so might herself have been unaware of their contents.

As luck would have it, that spring the Association for Symbolic Logic met at Duke. I took that opportunity to look at the Morgenstern papers, and quickly found a slim folder labeled "Gödel". There were only a few pages of interest in it, but the librarian suggested that references to Gödel might also be found in Morgenstern's diary entries. I requested to see them, not realizing until she said "Which volumes?" that his diaries covered a period of nearly sixty years. A cursory glance at a couple of the volumes was enough to reveal what a wealth of information about Gödel they contained. I later spent a full week examining the diaries in detail. What I found filled many gaps in the record of Gödel's life and gave me an insider's view of his final years unobtainable elsewhere.

Similarities and Contrasts Between Historical and Mathematical Research

Proving theorems is an analytical endeavor, whereas historical research is synthetic in character. Nevertheless, the standards of logical rigor to which mathematicians are accustomed have much in common with the standards of historical evidence. Nor is there any less satisfaction or excitement in settling historical questions or discovering new historical facts than there is in discovering new mathematical results. Historical research is constrained by the available data and guided by historical acuity just as mathematical research is constrained by axioms or the behavior of the real world and guided by mathematical insight. Both activities can be equally frustrating or intellectually rewarding, and if done properly, both demand equal standards of scholarship and should be accorded equal scholarly respect.

In all those regards, research in the history of mathematics should be congenial to those trained to do traditional mathematical research. What is different is the lack of finality that historical conclusions possess: the most carefully supported interpretations of historical events may be upset by the discovery of new artifacts or data, whereas the truth of a mathematical statement, once proven, is seldom called into question. Nevertheless, standards of mathematical proof do slowly change, and the recognition of that fact is perhaps what most distinguishes the viewpoint of the mathematical historian from that of most mathematical practitioners.

It is comforting to think, after proving a theorem, that one has settled a question once and for all. That feeling of security is one of the attractions of doing mathematical research, and it can be hard to turn away from that and accept the vulnerability inherent in doing historical work. Those reluctant to do so should probably leave historical studies to others. But a degree of risk can also be exciting, and to the extent that mathematical research is "safe" it is also abstractly removed from the affairs of the world.

Speaking personally, I believe that biographical or historical work can be a valuable adjunct to traditional mathematical pursuits, especially for those who are remote from centers of mathematical research and do not have regular contact with advanced students or colleagues in their discipline. I recommend it to readers who may be dissatisfied with their present situation and are seeking an alternative way to remain productive and intellectually alive.

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Book Review

Incompleteness: The Proof and Paradox of Kurt Gödel

Reviewed by Juliette Kennedy

Incompleteness: The Proof and Paradox of Kurt Gödel
Rebecca Goldstein
W. W. Norton & Company
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$22.95, 296 pages, ISBN 0393051692

Popular books on mathematics play an important role in the lay public's education. But as is known to anyone who has given a popular mathematics lecture or written about a famous theorem for an audience of nonmathematicians, doing justice to the mathematics in question is almost impossible in those circumstances. Rebecca Goldstein, the MacArthur Foundation fellow and author of *The Mind-Body Problem* (a novel which seems to be quite popular among mathematicians) attempts an even more difficult task in her short new book *Incompleteness: The Proof and Paradox of Kurt Gödel*, namely, to place a significant piece of mathematics—Gödel's Incompleteness Theorems—in the context of the wider intellectual currents of the twentieth century, both within the mathematical logic and the philosophy of mathematics communities, as well as within the intellectual culture at large.

The theorems are presented in the context of a somewhat detailed personal and intellectual biography of Gödel as well as in that of the various schools in foundations of mathematics in existence at the time. A vast amount of material is covered: everything from the history of the Vienna Circle, to Gödel's philosophical differences with Wittgenstein, to the Hilbert Program, to Gödel's views on appointments at the Institute for Advanced Study, to many aspects of his personal biography, in addition to the account of the two Incompleteness Theorems. This means that some of these areas are covered more comprehensively than others.

In places the author succeeds creditably; for example, her portrayals of behind-the-scenes academic life will likely be of interest to readers who enjoy such material—indeed, such portrayals seem to be her forte.

As is often the case with books about mathematics written by nonmathematicians though, shortfalls of precision occurring here and there will leave mathematicians unsatisfied; and the misstatement of the Fixed Point Theorem on page 180, the heart of the matter technically, makes it, unfortunately, quite impossible for anyone to reconstruct the proof of the First Incompleteness Theorem from Goldstein's account.

In this review I will comment on the three main aspects of Goldstein's book, apart from her technical account of the theorems: first, her portrayal of the life and personality of Gödel and the social surroundings in which he worked; second, her main claim, which is that Gödel's work has been misunderstood and misused by postmodernists and other intellectuals; and third, her rather substantial discussion of foundational issues, which unfortunately is the weakest of these three aspects of the book.

The author's novelistic skills are at their most conspicuous in the section of the book devoted to her colorful portrait of Gödel. From p. 59:

> I think it is fair to say...that like so many of us Gödel fell in love while an undergraduate. He underwent love's ecstatic transfiguration, its radical reordering of priorities, giving life new focus and meaning. One is not quite the same person as before.

Juliette Kennedy is university lecturer in the Department of Mathematics and Statistics at the University of Helsinki. Her email address is `juliette.kennedy@helsinki.fi`.

> Kurt Gödel fell in love with Platonism, and he was not quite the same person as he was before.

Another example of this theme occurs on p. 110, where Gödel is described as "a man whose soul had been blasted by the Platonic vision of truth."

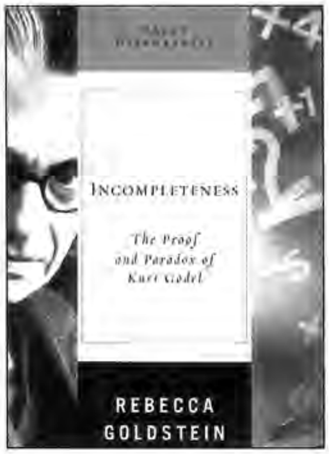
Goldstein includes all of the standard anecdotes about Gödel, as reported by the occasional, usually confounded, eyewitness. Some of these are quite amusing, for example, the story of Gödel's citizenship hearings (pp. 232–233), in which a logical inconsistency Gödel discovered in the American Constitution threatened to upset the proceedings.

The psychological analysis the author sprinkles here and there into the biographical material is nothing if not enterprising. For example, Goldstein has a theory about the source of Gödel's psychological difficulties (pp. 48–49):

> as I hope will become ever clearer in the chapters to come, the internal paradoxes in Gödel's personality were at least partially provoked by the world's paradoxical responses to his famous work.

See also p. 57:

> ...the precocious Gödel grasped the limits of parental omniscience at about the age of five. It would be comforting, in the presence of such a shattering conclusion, especially when it's reinforced by serious illness a few years later, to derive the following additional conclusion...the grownups around me may be a sorry lot, but luckily I don't need to depend on them. I can figure out everything for myself. The world is thoroughly logical and so is my mind—a perfect fit.
>
> Quite possibly the young Gödel had some such thoughts to quell the terror of discovering at too young an age that he was far more intelligent than his parents. It would explain much about the man he would become.[1]



Those familiar with Gödel's life may find all this somewhat reductive; although it is clear from the author's portrayal of him that she very much sympathizes with Gödel. Some occasional wrong notes include the discussion on pp. 226–227, in which some key background facts are omitted, as well as the discussion on p. 223, in which the author ventures to describe Gödel's marriage to his wife Adele, a lively and witty woman who seems to have been somewhat out of place in Princeton, as "weird", "according to just about everyone". One wonders about this characterization of their marriage, when many of the Gödels' friends seem to give a different impression in their reports of it. In fact, on the whole the portrait of Adele is a bit ungenerous, with the author making very heavy weather, for example, about such things as Mrs. Gödel's appalling—*to Goldstein*—taste in home decoration.

Does the real person come through in this account? Gödel was an extremely private person who at the same time suffered, as many creative people do, from disabling episodes of anxiety and depression. The episodes became worse with age. It is not a very pretty story; but his productivity, given the circumstances, makes it a very moving one—albeit one that in the end, at least in this book, may remain to be told.

That said, interviews the author has conducted with the principals, for example Armand Borel, have yielded valuable new information about Gödel's relationship with his colleagues at the Institute for Advanced Study, answering the question why Gödel was so isolated from his colleagues during his later years there. Goldstein also draws upon her experience as a graduate student in philosophy at Princeton, which has put her in the position of being able to speak firsthand about the Princeton academic culture of the time—even if her perspective is very much that of an awestruck student.

The book is centered around the claim that, in an ironic twist of events, the "intellectual community", as Goldstein refers to it, used Gödel's own incompleteness theorems to discredit his philosophical Platonism; that the Incompleteness Theorems became "grist for the postmodern mill", if not the main weapon in the contemporary "revolt against objectivity"; that consequently Gödel, to whom the notion of mathematical truth was an absolute and objective one, had to battle to the end

[1] *Some of the important memoirs about Gödel include those by his brother Rudolf Gödel, his classmate Olga Taussky-Todd [18], the obituary of Gödel for the Royal Society by Georg Kreisel [11], as well as other writings of Kreisel on Gödel, Hao Wang's three books based on his conversations with Gödel [14], [15], [16], as well as Stephen Kleene's memoir [10], to name just a few. The reader is also referred to the biography of Gödel by John Dawson entitled* Logical Dilemmas *[4], as well as to Palle Yourgrau's portrayal of Gödel in* A World without Time *[17], which focuses mostly on Gödel's friendship with Einstein and the scientific work which grew out of it.*

of his days against postmodern misconceptions and misconstruals of his theorems, which were misinterpreted to show that there is no such thing as truth.

Platonism in the context of foundations of mathematics is essentially the view that mathematics is a descriptive science, although, unlike the empirical sciences, the domain described is thought to consist of abstract objects. Another tenet of Platonism is that the concept of mathematical truth is a meaningful one. Gödel held this view from about 1925 onwards (though he wavered a bit between then and about 1940). Gödel also thought infinitistic methods were fully acceptable, something which, in his case, had to do with his Platonism.

The author's description of Gödel's mature Platonism is essentially correct. But her interpretation of the "intellectual community's" reaction to the Incompleteness Theorems, although making for a dramatic story, and probably correct in some particulars, is an oversimplification of the facts. One problem is that there seem to be at least two communities described as misconstruing Gödel's theorems: his colleagues in the logic and foundations of mathematics communities, and secondly the culture at large together with the postmodern philosophy community—quite a different audience for those theorems. Although it is the second of these that is referred to more often, the claim is a global one. Unfortunately this leads to some confusion, especially since it is often unclear to which community the author is referring in particular instances; and the abundance of vague references to, for example, "eminent thinkers", (p. 198) or "intellectual gurus", (p. 40), or, simply, "they", (pp. 135-6) is no help:

> The irony of course is that while his theorems were accepted as of paramount importance, others did not always hear what he was attempting to say in them. They heard—and continue to hear—the voice of the Vienna Circle or of existentialism or postmodernism or of any other of the various fashionable outlooks of the twentieth century. They heard everything except what Gödel was trying to say.

The text is full of such assertions, without their ever being pursued. The reader may find this vexing: Who are "they"?

At least within the foundational community, a critical attitude toward semantic notions, such as the concept of mathematical truth, together with a bias against infinitistic methods, had been entrenched for decades before the Incompleteness Theorems. It was the driving force behind most of the foundational schools of the first half of the twentieth century and was simply the dominant view of Gödel's time. For example, the Hilbert Program aimed to show that all that was required to formalize mathematics were finitary axioms stated in a precise syntax together with finitary rules of proof (proving consistency was the second desideratum of it). In showing that mathematics can be construed as a "formal game of symbols", a slogan which came into use at the time, mathematics would be put on a firm foundation by eliminating reference to infinite objects, as well as the use of unstated assumptions or proof procedures that might lead to paradoxes. The part of mathematics which remains, and to which it is possible to reduce the infinitary part, was called the *inhaltlich*, or contentual, part. Formalism was Brouwer's term for the school associated with the Hilbert Program.

Indeed the influence of the Hilbert Program on Gödel can be seen from the fact that Gödel stated and proved the Incompleteness Theorems "syntactically", i.e., so as to avoid any reference to the notion of truth in the standard interpretation—that is, truth in the domain of the natural numbers. He himself viewed the informal argument, which involves the concept of truth in the standard interpretation, as sufficient.

As for whether the Incompleteness Theorems served as a further stimulus for the anti-semantical or anti-truth view within the mathematical logic and foundations community, to some extent the influence flowed in the opposite direction, as the author notes, upsetting the entrenched view. This is because, with respect to carrying out the envisaged formal reconstruction of mathematics, eliminating semantical notions, such as meaning and truth, depends on showing that formal provability is all that is needed.

The First Incompleteness Theorem refutes this by showing that the concept of mathematical truth (if one accepts the concept at all) *properly contains*, in a sense, the concept of formal provability.[2] Therefore the latter cannot bear the burden of the formalization.

Gödel demonstrated, then, if not the indispensability of semantic notions altogether, at least some grounds for their necessity. Many accepted the view that semantics were ineliminable, at least those, unlike strict finitists, who were not predisposed against the idea. And on the other hand, many did not: the fact that the Incompleteness Theorems can be formulated and proved completely syntactically led some to conclude that the theorems say nothing at all about the validity—or lack thereof—of semantic methods. As for vindicating the use of infinitistic methods, Gödel had already achieved this, to some extent, with his

[2] *Gödel's way of putting it was to say that the activity of the mathematician cannot be mechanized. See p. 164, [8].*

completeness theorem, the main result of his doctoral thesis (submitted October 1929).

In sum, differing views as to what constitutes contentual mathematics led to the establishment of various foundational schools long before the Incompleteness Theorems were published. But the complicated story of how those foundational schools absorbed the impact of those theorems is touched upon so briefly here, and is blended in so indistinguishably with references to the second community's absorption of those theorems, that it is difficult to support the central claim about the misuse and misinterpretation of Gödel's works.

The response of experts to the theorem aside, the author's claim that postmodern philosophers or thinkers have used Gödel's results to show, for example, that truth is relative, or nonexistent, seems plausible.[3] How Gödel's theorems were ultimately stirred into the mix, though, seems to be a very large topic, deserving, possibly, a book of its own.[4]

On the side of scholarship, it is to be regretted that a book which has and will continue to gather such a wide and enthusiastic readership (at least judging from the satisfied customer reviews on Amazon) should contain at the same time such a truly alarming preponderance of factual and conceptual errors regarding other matters.[5] Names of major figures, such as Georg Kreisel's, are misspelt. Many crucial dates are incorrect, including, twice in the book, the date of Gödel's death (off by two years). Hilbert's famous list of problems numbered 23 and not 10, as Goldstein has it, and Tarski's original surname was Tajtelbaum (usually written Teitelbaum), not Tannenbaum.[6] The diagram on page 125 is incorrect. The existence of nonstandard models of arithmetic follows already from Gödel's Completeness Theorem—the First Incompleteness Theorem is not needed. The continuum hypothesis, namely the question whether there are any infinite cardinals strictly between $\aleph_0$, the cardinality of the natural numbers, and $2^{\aleph_0}$, that of the reals, is misstated—the author confuses ordinals with cardinals. The First Incompleteness Theorem itself is misstated on p. 191 (although the author gets it right elsewhere).

Also on the continuum hypothesis, Goldstein claims that Gödel's landmark paper "What is Cantor's Continuum Hypothesis?", "...explains how Cantor's continuum hypothesis has been shown to be independent of the axioms of set theory...." But as the paper was written in 1947, well before the independence had been shown, it cannot possibly explain how the independence of the continuum hypothesis has been shown.[7]

The author's lengthy discussion of the Hilbert Program, which she characterizes as being dedicated to "eliminating intuitions", is not exactly erroneous, but does violence to the spirit of that program, in the opinion of this reviewer, and will justifiably perplex mathematicians.

From p. 129 and p. 133:

> The drive for limiting our intuitions went even further. The aim became to eliminate intuitions altogether.
>
> If it could be shown that logically consistent formal systems are adequate for proving all the truths of mathematics, then we would have successfully banished intuitions from mathematics.

She also remarks (p. 131) that

> We don't have to appeal to our intuitions about numbers or sets or space in laying down the givens of a formal system.

Giving a finitary formal reconstruction of mathematics of the kind the Hilbert school envisaged in no way eliminates intuitions from mathematics. Formal systems are to be set up exactly on the *basis* of our intuitions. Of course one can arbitrarily set up a great variety of formal systems; but only those which are conceived on the basis of our

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[3] *The author seems to use "postmodernism" to refer to the views associated with a much wider group of philosophers, writers, sociologists, and so on, than those associated with the French school around, for example, Lacan and Derrida. This may confuse European readers.*

[4] *There is a growing literature in the area of postmodern commentaries of Gödel's theorems. For example, Régis Debray has used Gödel's theorems to demonstrate the logical inconsistency of self-government. For a critical view of this and related developments, see Bricmont and Sokal's* Fashionable Nonsense *[13]. For a more positive view see Michael Harris's review of the latter, "I know what you mean!" [9]. See also the recently published [6] by Torkel Franzén as well as Franzén's "The popular impact of Gödel's incompleteness theorem", this issue of the* Notices.

[5] *Apart from the error which has been noted earlier, in the treatment of the First Incompleteness Theorem itself.*

[6] *Though Stanley* Tennenbaum *was an important logician and Princeton figure.*

[7] *The revised 1964 version of Gödel's paper contains a very brief appendix commenting on P. J. Cohen's proof of this result, published while Gödel's revised paper was in proofs; perhaps this explains Goldstein's error. What is striking about Gödel's paper is that in it he predicts that the independence of the continuum hypothesis* will *be shown. That is, he predicts the 1963 result due to P. J. Cohen, that the negation of the continuum hypothesis is consistent with the Zermelo-Fraenkel axioms for set theory. This in spite of the fact that Gödel himself proved in 1937 that the continuum hypothesis is itself consistent with the axioms of set theory, a result that is in some sense opposite to what Cohen proved.*

intuitions about number, set, and so on, can possibly be of interest. Again, what the Hilbert school wanted was to capture mathematical content in the kind of formal system in which reference to infinite entities does not occur. It is only in that precise sense one can say those entities have been "eliminated". For this to be possible one needs to define what it is that is to be eliminated, for a start, and infinite entities such as $\aleph_1$ *are* sufficiently clearly defined—whereas the concept of intuition is not.

On the philosophical side of things, connections between the notion of pure intuition and the Hilbert Program are deep and important. Getting the concept of intuition right, whether it be the Kantian notion or otherwise, was a project of considerable importance to the adherents of the Hilbert Program, throughout the 1920s.[8] Simply put, the Kantian notion of pure intuition was seen by them to be the very *basis* of what Hilbert called "the finite mode of thought". As Bernays put it in 1928:

> The "finitistic attitude" required by Hilbert as a methodological basis must be characterized epistemologically as a form of pure intuition. ([12], p. 170)

And indeed Gödel's so-called Dialectica paper of 1958 (see [8]) contains an extensive discussion of the topic.

For pellucid discussions of these matters by the principals, mathematicians are referred to *Philosophy of Mathematics* [1], a collection of landmark papers by Poincaré, von Neumann, Gödel, and others involved in these developments. For the Incompleteness Theorems themselves, mathematicians are referred to Gödel's original 1931 paper, "Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme, I,"[9] a powerful piece of mathematics written in majestic prose.[10]

As an aside, on the topic of different communities' response to the Incompleteness Theorems, the readers of this review may see themselves as comprising a third community—that of working mathematicians. What was the impact of the Incompleteness Theorems on this community? This is an interesting question.

One might think of the average mathematician as taking the view that mathematics is complete—in some sense of the term. There are different ways to interpret this, but for the sake of the present discussion let us take the assertion that mathematics is complete to mean that any mathematical statement, suitably precisely stated, is going to be decided one way or the other by mathematical means —as following from some *suitable* set of axioms for example (at least theoretically, that is, leaving aside questions such as those involving resources). In fact this very question, namely whether mathematics is complete in a more general sense than the technical one with respect to which he answered the question negatively, occupied Gödel himself to a very great degree—and long after he proved the Incompleteness Theorems. For example, in the late 1930s he pondered the existence of absolutely undecidable sentences, by which he meant precisely stated mathematical assertions "undecidable, not just within some particular axiomatic system, but by *any* mathematical proof the human mind can conceive." ([8], p. 310) Interestingly, after a short period during which he entertained the possibility that there might be absolutely undecidable mathematical sentences, Gödel came to the conclusion around the early 1940s—and held to it for the remainder of his life—that this would turn out not to be the case. There would be no absolutely undecidable sentences in mathematics. What he called "Hilbert's original rationalistic conception"—that "for any precisely formulated mathematical question a unique answer can be found" ([8], p. 163)—was, in his view, the correct one.

How is this possible? Didn't Gödel prove that mathematics is incomplete (if consistent)?

The answer, of course, is no. What Gödel proved is that certain formal systems, including canonical ones like Peano Arithmetic or Zermelo-Fraenkel set theory, are incomplete. He did not prove that mathematics is incomplete in the sense we have defined completeness above; and therefore in some sense the question is still an open one.

An issue which bears on the question being open is the fact that, as it turns out, there are different classes of undecidable sentences. The so-called Gödel sentences published by Gödel in 1931, do not seem to have much to do with mathematical practice as such. The "I am unprovable" sentences arising from the First Incompleteness theorem are somewhat ad hoc. (Indeed Gödel himself referred to the theorems occasionally, in conversations with Kreisel, as the result of a "parlor trick", see [11].) In any case they, as well as the other Gödel sentences, namely those involving consistency, can be decided simply by passing in a natural way to systems of so-called "higher type".[11]

[8] *Consequently they wrote about this extensively. See for example Hilbert's "Foundations of Elementary Number Theory", reprinted in [12].*

[9] *Reprinted with a facing English translation in [7]. The excellent introductory note to it by Kleene is also strongly recommended.*

[10] *There are many modern accounts of the theorem. See, for example "The Incompleteness Theorem", by Martin Davis, this issue of the* Notices.

[11] *See [3], as well as "The impact of the incompleteness theorem on mathematics", by Solomon Feferman, this issue.*

In Conversation with Rebecca Goldstein

Rebecca Goldstein's 1983 novel *The Mind-Body Problem* has been widely admired among mathematicians for its authentic depiction of academic life, as well as for its exploration of how philosophical issues impinge on everyday life. Her new book, *Incompleteness: The Proof and Paradox of Kurt Gödel*, is a volume in the "Great Discoveries" series published by W. W. Norton. The series aims to take a fresh look at great stories in science and mathematics. Another mathematical book in this series is *Infinity and More* by novelist David Foster Wallace (reviewed in the June/July 2004 issue of the *Notices*).

In March 2005 the Mathematical Sciences Research Institute (MSRI) in Berkeley held a public event in which its special projects director, Robert Osserman, talked with Goldstein about her work. The conversation, which took place before an audience of about fifty people at the Commonwealth Club in San Francisco, was taped and later broadcast on radio.

A member of the audience posed a question that has been on the minds of many of Goldstein's readers: Is *The Mind-Body Problem* based on her own life? She did indeed study philosophy at Princeton, finishing her Ph.D. in 1976 with a thesis titled "Reduction, Realism, and the Mind". She said that while there are correlations between her life and the novel, the book is not autobiographical. Many have speculated that the model for one of the main characters, Noam Himmel, is Princeton philosopher Saul Kripke. Not so, said Goldstein: All of the characters are fictional composites, except for the protagonist's father, who is based on Goldstein's own father.

One of the attractions of writing about Gödel's work for the general public, she said, is that his main theorems seem to say something about the nature of mathematics itself and even to reach beyond boundaries of the field. "To have a result that has the rigor of mathematics and the reach of philosophy is beautiful," Goldstein remarked.

She also talked about the relationship between Gödel and his colleague at the Institute for Advanced Study, Albert Einstein. The two were very different: As Goldstein put it, "Einstein was a real mensch, and Gödel was very neurotic." Nevertheless, a friendship sprang up between the two. It was based in part, Goldstein speculated, on their both being exiles—exiles from Europe and intellectual exiles. Gödel's work was sometimes taken to mean that even mathematical truth is uncertain, she noted, while Einstein's theories of relativity were seen as implying the sweeping view that "everything is relative." These misinterpretations irked both men, said Goldstein. "Einstein and Gödel were realists and did not like it when their work was put to the opposite purpose."

—*Allyn Jackson*

In that spirit some (see [5]) refer to this type of incompleteness as "residual incompleteness", a phrase meant to capture the idea that this kind of incompleteness arises only as an artifact of formalization—after all, such sentences simply do not arise in ordinary unformalized mathematics—and is an exception to completeness of an entirely inessential nature, more or less on a par with including zero as a counterexample to the axiom that every element of a field has a multiplicative inverse. The basic question whether mathematics is complete in the sense we defined it is therefore not affected by Gödel's examples. As Gödel put it in referring to the undecidable sentences of 1931: "As to problems with the answer Yes or No, the conviction that they are always decidable remains untouched by these results."[12]

The situation regarding statements emerging from set theory, however, such as the continuum hypothesis, is much more complicated, much more interesting—and much more threatening to the Platonist viewpoint.

As was noted above, the continuum hypothesis is independent of the (highly canonical) Zermelo-Fraenkel axioms. But the continuum hypothesis is an elementary statement from the multiplication table of cardinal numbers, as Gödel put it in his 1947 "What is Cantor's Continuum Problem?" (reprinted in [1]); it has a clear and unambiguous meaning. Therefore it ought to have a definite truth value. As Gödel wrote in that paper:

> Only someone who...denies that the concepts and axioms of classical set theory have any meaning (or any well-defined meaning) could be satisfied with such a solution,[13] not someone who believes them to describe some well-determined reality. For in this reality Cantor's conjecture must be either true or false, and its undecidability from the axioms as known today can only mean

[12] *See [8], pp. 174–5. There is a point of view that emphasizes more the importance of what we have called "residual incompleteness". But this would not have been Gödel's view.*

[13] *That there is no way to settle the continuum problem definitively. (Footnote the author's.)*

> that these axioms do not contain a complete description of this reality...

But there is a problem with deciding whether the continuum hypothesis is true or false in this sense—that is, with deciding the continuum hypothesis by finding a natural extension of the Zermelo-Fraenkel axioms that decides it (not to mention the problem with deciding other set-theoretical statements such as that asserting the existence of measurable cardinals). The reason is that there seem to be a number of natural extensions of the Zermelo-Fraenkel axioms that decide the continuum hypothesis in different ways—natural, at least, at first glance. But then, just as in the case of the parallel axiom in geometry, the question of the truth value of the continuum hypothesis takes on a different meaning. Is the parallel axiom true or not? To most mathematicians this is a meaningless question. The answer depends upon which geometry one is referring to. Similarly with the continuum hypothesis, some argue that its truth value depends upon which model of set theory one is in, so to speak, among a spectrum of natural models to choose from. This is a kind of formalism to which, for example, Cohen has subscribed (see his *Set Theory and the Continuum Hypothesis* [2]).

Of course many argue against the notion that the continuum hypothesis is analogous to the parallel axiom, and indeed Gödel was among the first (if not the very first) to argue against the analogy in his 1947 paper mentioned above. Among other arguments given there, Gödel observes that, "as against the numerous plausible propositions which imply the negation of the continuum hypothesis, not one plausible proposition is known which would imply the continuum hypothesis." (See [8], p. 264.)

This counts in favor of the continuum hypothesis being false.

The arguments he gave in that paper have grown into the so-called "large cardinal program", which is the program of finding a natural extension of the Zermelo-Fraenkel axioms that decides the mathematical statements one is interested in, meaning the "natural" ones, or more generally, those not arising from residual incompleteness.

As of today, the technical developments have not settled this issue in a definitive way. On the one hand the large cardinal program of Gödel is alive and well. Accordingly, for an important faction of set theorists, the continuum hypothesis is simply a problem to be solved—granted a very difficult one—just like any other mathematical problem (see [5]). This means, interestingly enough, that it is still possible for the discoverer of incompleteness to be vindicated in his view that mathematics is, for all practical purposes, complete. On the other hand it is easy enough to find set theorists who disagree with Gödel; in fact many, perhaps even a majority, of set theorists see themselves as standing in—or near—the formalist camp.[14]

Returning to the book under review, of its three facets—Gödel's incompleteness theorems, Gödel's theorems set against the background of the intellectual currents of his time, and finally Gödel the man as well as the behind-the-scenes look at the academic life of his contemporaries: as noted earlier, the author's account of the incompleteness theorems is not sufficient to reconstruct them, while some defects in the treatment of the second aspect of the book have also been indicated. As for the third aspect of the book, as mentioned, a rather colorful portrait of Gödel is to be found in it.

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[14] *There is of course much more to be said about which independent statements need to be taken seriously, at least by Platonists. The present discussion has omitted the natural statements independent of Peano arithmetic of the Paris-Harrington type, as well as those arising from the work of Harvey Friedman. For discussion of these and related matters see the above cited "The impact of the incompleteness theorem on mathematics", by Solomon Feferman, this issue of the* Notices. *For a discussion of Gödel's results in set theory together with their impact, see "How Gödel transformed set theory", by Juliet Floyd and Aki Kanamori, also this issue.*

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2006 Events Celebrating the Gödel Centenary

Several events are taking place around the world to mark the one hundredth anniversary of the birth of Kurt Gödel. Below is a partial list.

March 25–26, University of Edinburgh: Conference on "Truth and Proof: Kurt Gödel and the Foundations of Mathematics", organized by Jeffrey Ketland.

April 25–29, Technical University of Brno: Centennial conference on Gödel's life and work, organized by Masaryk University, Brno, and the Institute of Philosophy, Czech Academy of Sciences, Prague.

April 27–29, University of Vienna: "Horizons of Truth: Logics, Foundations of Mathematics, and the Quest for Understanding the Nature of Knowledge". This conference celebrating the Gödel centenary is organized by the Kurt Gödel Society with the support of the John Templeton Foundation. Website: `http://www.logic.at/goedel2006/`.

May 17–21, Montreal: The 2006 annual meeting of the Association for Symbolic Logic will include several sessions devoted to Gödel, organized by a subcommittee chaired by Charles Parsons.

May 18–21, University of Lille: Workshop on "Kurt Gödel: The writings", organized by Pierre Cassou-Nogués and Mark van Atten.

June 30–July 5, University of Wales, Swansea: The conference "Computability in Europe 2006: Logical approaches to computational barriers", organized by Arnold Beckman and John Tucker, will include a special session "Gödel Centenary: His Legacy for Computability", organized by John Dawson and Matthias Baaz, and lectures on Gödel by Martin Davis and John Dawson. Website: `http://www.cs.swan.ac.uk/cie06/give-page.php?1`.

August 12–14, Seattle: The IEEE Symposium on Logic in Computer Science, part of the 2006 Federated Logic Conference, will include a special session on Gödel, organized by Moshe Vardi. Website: `http://www.informatik.hu-berlin.de/lics/lics06/`.

In addition, a major indoor and outdoor exhibition on Gödel will open in Vienna in April (curated by Karl Sigmund and John Dawson; see the article by Karl Sigmund in this issue of the *Notices*).

A panel session on "Gödel's Contributions to the Foundations of Mathematics", has also been proposed (by Linda Bercerra and Ron Barnes of the University of Houston Downtown) for MathFest 2006, August 10–12, in Knoxville.