~~Gödel~~ FEFERMAN et al.

Introductory note to *1947* and *1964*

1. Introduction

Cantor's continuum problem served as one of the principal and periodic foci for Gödel's research from 1935 until his death more than four decades later. His article *1947* (substantially revised and expanded to become *1964*) originated from a request, made in 1945 by the editor of the *American mathematical monthly*, for a paper on the continuum problem. The result was an expository article written in the style for which the *Monthly* is well known, but having a flavor that reflected Gödel's distinctive blend of mathematical and philosophical interests. Although *1947* contains no new technical results, it gives considerable insight into his philosophical views on set theory and on what would and would not, in his opinion, constitute a solution to the continuum problem. In one sense, *1947* can be regarded as a continuation, and as a variation in a different key, of his reflections in *1944* on Russell and mathematical logic. Like *1944*, the article *1947* originated from a request for a contribution by Gödel, and included both technical hints for possible future research in mathematics and cogent philosophical arguments in favor of Platonism. But *1947*, unlike *1944*, was expository (indeed, the only expository article that Gödel ever published) and concerned a specific mathematical problem rather than a philosopher's contribution to logic.

This introductory note has seven sections, which serve different purposes. Section 2 places *1947* in a historical context by tracing the continuum problem from its origins to Gödel's attempts (circa 1938–1942) to establish the independence of the continuum hypothesis. Section 3 recounts the circumstances which led Gödel to write *1947*. The content of *1947* is analyzed in Section 4, while Section 5 indicates how Gödel's perspective changed in the revised version *1964* (and in his 1966 plans for a third version of the paper). Section 6 discusses the effect of recent mathematical developments on Gödel's claims in *1947* and *1964*. Finally, Section 7 concerns his two unpublished articles on the continuum hypothesis, both written about 1970.

2. Historical background to the continuum problem, including Gödel's work before *1947*

The continuum problem, which Cantor first posed in *1878*, grew out of research that he began in 1873. At that time, in a letter to Dedekind,

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Cantor posed the question whether the set $\mathbb{R}$ of real numbers can be put in one-to-one correspondence with the set $\mathbb{N}$ of natural numbers. Although Dedekind at first doubted the importance of this question, he was pleased when Cantor discovered a proof that such a correspondence cannot exist. In January 1874 Cantor posed a further question to Dedekind: Can a line segment be put in one-to-one correspondence with a square and its interior? Three years passed before Cantor succeeded in showing that there exists such a correspondence between a line segment and n-dimensional space for any n.[a] At the end of the article (*1878*) detailing this proof, Cantor stated that every uncountable set of real numbers can be put in one-to-one correspondence with the set of all real numbers, i.e., that there is no cardinal number strictly between that of $\mathbb{N}$ and that of $\mathbb{R}$. This proposition was the original form of the continuum hypothesis. Since there is no standard terminology for this form, we shall call it the *weak continuum hypothesis*.

When in *1883* Cantor developed the notion of well-ordering and asserted that every set can be well-ordered, he gave a second and more elegant form to this hypothesis: $\mathbb{R}$ has the same power as the set of countable ordinals. In his aleph notation of *1895* this can be stated as $2^{\aleph_0} = \aleph_1$, the form in which the continuum hypothesis (*CH*) is now known. (It is easily seen that *CH* is equivalent to the conjunction of the weak continuum hypothesis and the proposition that $\mathbb{R}$ can be well-ordered.) Cantor himself never used the term "continuum hypothesis"; instead, in his 1882 correspondence with Dedekind, he referred to the weak continuum hypothesis as the "two-class theorem".

In *1883* Cantor began to generalize *CH*, asserting that the set of all real functions has the third infinite power; in his later notation, this stated that $2^{\aleph_1} = \aleph_2$. He never discussed any more general form of *CH*, perhaps because he saw no use for such a generalization. The generalized continuum hypothesis (*GCH*), which states that $2^{\aleph_\alpha} = \aleph_{\alpha+1}$ for all ordinals α, was first formulated by Hausdorff (*1908*, pages 487, 494) and was given this name by Tarski (*1925*).

Despite very intense research, especially during 1884, Cantor never succeeded in demonstrating *CH*. However, he obtained a special case of the weak continuum hypothesis: Every uncountable *closed* subset of $\mathbb{R}$ has the power of $\mathbb{R}$ (*1884*). For a while that year, during August and again during October, he believed that he had proved *CH*, and then, for a brief period in November, that he had refuted *CH* (*Moore 1982*, pages 43–44).

In August 1904 a Hungarian mathematician, J. König, also claimed to have disproved *CH*. This occurred in a lecture he gave at the Inter-

[a]See *Noether and Cavaillès 1937*, pp. 12–13, 20–21, 25.

national Congress of Mathematicians at Heidelberg. However, the next day E. Zermelo found the gap in König's argument. When revised for publication (*König 1905*), König's result was that the power of ℝ cannot equal $\aleph_{\alpha+\omega}$ for any ordinal α. In the light of Hausdorff's 1906–1908 researches on cofinality, the result was extended to the following: $2^{\aleph_0}$ cannot equal $\aleph_\beta$ for any β of cofinality ω. In *1947* Gödel observed that nothing beyond this was known about the cardinality of ℝ.

As F. Bernstein noted (*1901*, page 14), one line of research on the continuum problem consisted in trying to extend, to larger and larger classes of subsets of ℝ, Cantor's result that the weak continuum hypothesis holds for the closed subsets of ℝ. The hierarchy soon used for this purpose was that of the Borel sets, introduced by E. Borel (*1898*) and first extended to transfinite levels by H. Lebesgue (*1905*). In *1903* W. H. Young strengthened Cantor's result by showing that every uncountable G_δ subset of ℝ has the power of ℝ. A decade later Hausdorff succeeded in extending the result further, first to the $G_{\delta\sigma\delta}$ sets (*1914a*) and then to the entire Borel hierarchy (*1916*).

For the next two decades, almost all progress on *CH* had a close connection with N. Luzin and his students (such as P. S. Aleksandrov and M. Suslin), who together made up the Moscow school of function theorists. The school's first result occurred when Aleksandrov (*1916*) obtained the above-mentioned theorem on the Borel hierarchy at the same time that Hausdorff did. In 1917 Luzin and Suslin extended the Borel hierarchy by introducing the analytic sets, the first level of what later became the projective hierarchy. Suslin established that the weak continuum hypothesis holds for the analytic sets, now called the $\mathbf{\Sigma}^1_1$ sets, since every uncountable analytic set has a perfect subset.[b] Yet, as Gödel observed in *1947* (page 517), progress stopped there; for it had not been shown that the weak continuum hypothesis holds for every $\mathbf{\Pi}^1_1$ set but only that an uncountable $\mathbf{\Pi}^1_1$ set has either the cardinality $\aleph_1$ or that of ℝ—a result due to K. Kuratowski (*1933*, page 246).

A second approach to the continuum problem was begun by Luzin (*1914*) and pursued vigorously in Poland by his collaborator W. Sierpiński. In this approach, various propositions were shown to be consequences of *CH*. By assuming *CH* as a hypothesis, set theorists gained knowledge about its strength and were able to settle various open problems. Sierpiński, beginning in *1919*, was especially concerned to find interesting propositions equivalent to *CH*. He summarized his results in a book, *Hypothèse du continu* (*1934*), the source for the "paradoxical" consequences of *CH* that Gödel cited in *1947*.

[b] *Luzin 1917*. For discussion of the projective hierarchy, as well as the definition of $\mathbf{\Sigma}^1_n$ and $\mathbf{\Pi}^1_n$ sets, see p. 13 above of the introductory note to *1938*.

In 1923 D. Hilbert claimed that his recently developed proof theory could not only provide a foundation for mathematics but could even settle classical unsolved problems of set theory such as the continuum problem (*1923*, page 151). Three years later he published his attempt to sketch a proof, based on definability considerations, of what he called the "continuum theorem" (*1926*). This attempted proof of *CH* met with widespread skepticism, in particular from Fraenkel (*1928*) and from Luzin (*1929*). In 1935 Luzin returned to this question, arguing that there was not in fact one continuum hypothesis but rather several continuum hypotheses; he dubbed as the "second continuum hypothesis" the following proposition contradicting *CH*:

$$2^{\aleph_0} = 2^{\aleph_1}.$$

Finally, he argued that the second continuum hypothesis accorded with a proposition (contradicting *CH*) of whose truth he felt certain: Every subset of ℝ having power $\aleph_1$ is a $\mathbf{\Pi}^1_1$ set (*1935*, pages 129–131). Gödel referred in passing to these matters (*1947*, page 523) while mentioning that Luzin, like Gödel himself, believed *CH* to be false.

In the absence of a proof or refutation of *CH*, mathematicians could try to establish its undecidability on the basis of the accepted axioms of set theory. As early as 1923, T. Skolem conjectured that *CH* cannot be settled by Zermelo's 1908 axiom system (*Skolem 1923a*, page 229). But, when Skolem wrote, the understanding of models of set theory was still very rudimentary. Luzin hoped that Hilbert's proof theory would supply a consistency proof for the "second" continuum hypothesis as well as for *CH* (*Luzin 1935*, pages 129–131).

During the 1920s it was also uncertain whether models of set theory should be studied within second-order logic, as did Fraenkel (*1922a*) and Zermelo (*1929*, *1930*), or within first-order logic, as Skolem proposed (*1923a*, *1930*). In *1930* Zermelo showed that all second-order models of Zermelo–Fraenkel set theory (ZF) consist of the αth stage of the cumulative type hierarchy, where α is a strongly inaccessible ordinal. In an unpublished report of about 1930 to the Emergency Society of German Science, Zermelo pointed out that *CH* is either true in all of these models or false in all of them, so that in either case *CH* is decided in second-order ZF.[c] This result contrasts with the later discoveries of

[c] This report is printed in *Moore 1980*, pp. 130–134, and the observation on *CH* can be found on p. 134. Kreisel (*1967a*, pp. 99–100) also emphasized this point, though unaware that Zermelo had formulated it almost four decades earlier; however, L. Kalmár (*1967*, p. 104) and A. Mostowski (*1967a*, p. 107) reacted negatively to Kreisel's observation, and the second-order version of *CH* has been little studied.

Gödel and P. J. Cohen that *CH* is undecided in the first-order version of ZF.

About 1935 Gödel realized that if Zermelo's cumulative hierarchy were restricted at each level to the sets first-order definable from those obtained at previous levels, then one would have a class model of first-order ZF in which various important propositions held. Originally, in 1935, he proved only that the axiom of choice is such a proposition, but by 1937 he had shown that *GCH* holds in the model as well. In *1938* he was inclined to accept the axiom of constructibility as true, referring to it as "a natural completion of the axioms of set theory" (page 557), and hence to believe that the generalized continuum hypothesis is also true. Yet Gödel refrained, for more than a year, from publishing an announcement of these relative consistency results. A clue to his silence can be found in his letter, written in December 1937 to Karl Menger, which reveals Gödel's hopes for an even stronger result about *CH*:

> I continued my work on the continuum problem last summer, and I finally succeeded in proving the consistency of the continuum hypothesis (even in the generalized form $2^{\aleph_\alpha} = \aleph_{\alpha+1}$) with respect to general set theory. But I ask you, for the time being, please not to tell anyone about this. So far, except for you, I have communicated this result only to von Neumann .... Right now I am also trying to prove the independence of the continuum hypothesis, but do not yet know whether I will succeed with it ....

Unfortunately, Gödel did not succeed in proving the independence of *CH*, despite repeated attempts.

On the other hand, Gödel's efforts to show the independence of the axiom of choice, and consequently of the axiom of constructibility as well, were more fruitful. When Cohen received the Fields Medal for establishing the independence of *CH*, A. Church pointed out, in his speech awarding the medal (*1968*, page 17), that

> Gödel ... in 1942 found a proof of the independence of the axiom of constructibility in ⟦finite⟧ type theory. According to his own statement (in a private communication), he believed that this could be extended to an independence proof of the axiom of choice; but due to a shifting of his interests toward philosophy, he soon afterward ceased to work in this area, without having settled its main problems. The partial result mentioned was never worked out in full detail or put into form for publication.

Gödel also commented on his independence results in a letter of 1967 to W. Rautenberg, who had written to Gödel inquiring about Mostowski's claim that Gödel, about 1940, had obtained most of Cohen's independence results. In his reply (written in German and translated here), Gödel confirmed what Church had stated:

> In reply to your inquiry I would like to refer to the presentation of the facts that Professor Alonzo Church gave in his lecture at the last International Congress of Mathematicians.
>
> Mostowski's assertion is incorrect insofar as I was merely in possession of certain partial results, namely, of proofs for the independence of the axiom of constructibility and of the axiom of choice in type theory. Because of my highly incomplete records from that time (i.e., 1942) I can only reconstruct the first of these two proofs without difficulty. My method had a very close connection with that recently developed by Dana Scott ⟦Boolean-valued models⟧ and had less connection with Cohen's method.
>
> I never obtained a proof for the independence of the continuum hypothesis from the axiom of choice, and I found it very doubtful that the method that I used would lead to such a result.

Thus there can be no doubt that Gödel believed that he had obtained some significant independence results, but not for *CH*.

By the time that Gödel composed *1947* he had become convinced, contrary to the views he expressed in *1938*, that *CH* (and hence the axiom of constructibility as well) was false.

3. The origins of *Gödel 1947*

Gödel undertook to write the article *1947* at the request of Lester R. Ford, the editor of the *American mathematical monthly*. "For some time we have been running a series of papers ...", Ford wrote Gödel on 30 November 1945,

> which we call the "What Is?" series. In these papers the authors have presented some small aspect of higher mathematics in as simple, elementary and popular a way as they possibly can. We have had papers by both Birkhoffs, Morse, Kline, Wilder and several others.
>
> I am writing this to ask if you would like to prepare such a paper. The subject would be of your own choosing, but I had thought of "What is the problem of ⟦the⟧ continuum?"

When Gödel did not respond, Ford wrote again on 31 January 1946. On 14 February, Gödel, who had not received the earlier letter, expressed his willingness to consider the matter, adding that "in any case I could not write the paper immediately, because I am unfortunately very busy with other things at present." A week later, Ford replied: "Let me know as promptly as you can whether you can write this paper. I ought to have it by the month of July. It will not be a long paper and its writing ought not to take a great deal of time."

Ford did not realize that, when composing an article, Gödel was an extreme perfectionist. Another year passed before Gödel completed the paper that, in March 1946, he agreed to write. On 13 August 1946 Ford inquired about the paper, since he wished to print it before his editorship ended in December. Gödel answered on 31 August: "The paper about the continuum problem ... was finished and typewritten a few weeks ago, but on rereading it, I found some insertions desirable, which I have now about completed." Once again, this was not to be.

Finally, on 29 May 1947, Gödel sent the paper to the new editor, C. V. Newsom. In his covering letter, Gödel mentioned that he had "inserted a great number of footnotes whose order does not completely agree with the order in which they occur in the text." He suggested that the new footnotes be printed after the text of the article. Unfortunately, as Gödel learned when he saw the article in print, the footnotes had been renumbered in page proof without changing the internal references to them.[d] He had received no page proofs, having returned his galley proofs at the last moment. Newsom apologized for the errors, which occurred when the compositor tried to make sense of the footnotes, and added by way of compensation: "Your paper has brought many compliments; it is by far the best article in volume 54."

4. How Gödel viewed the continuum problem in *1947*

Gödel's essay *1947* consists of four sections: (1) a discussion of the notion of cardinal number, (2) a survey of the known results about the power $2^{\aleph_0}$ of the continuum $\mathbb{R}$, (3) a philosophical analysis of set theory, and (4) a proposal for solving the continuum problem.

In Section 1, Gödel stressed that Cantor's notion of cardinal number is unique, provided one accepts the minimal requirement that if two sets have the same cardinal number, then there exists a one-to-one correspondence between them. Here Gödel did not discuss how the notion of cardinal number might be defined, contenting himself with the definition

[d]These errors, which Gödel noted in volume 55 of the *Monthly*, are corrected in the text of *1947* printed in the present volume.

of equality between cardinal numbers. In this context he introduced the continuum problem as the question of how many points there are on a Euclidean straight line (or equivalently, how many sets of integers exist). This problem would lack meaning, he observed, if there were not a "natural" representation for the infinite cardinal numbers. But since the alephs $\aleph_\alpha$ provide such a representation and since, by the axiom of choice, the cardinal number of every set is an aleph, it follows that the continuum problem is meaningful. In footnote 2 he defended such uses of the axiom of choice by arguing, on the one hand, that this axiom is consistent relative to the usual axioms for set theory (as shown in his *1940*); on the other hand, he asserted that the axiom of choice is quite as self-evident as the usual axioms for the notion of arbitrary set and is even provable for "sets in the sense of extensions of definable properties" (that is, for the constructible sets, as well as for the ordinal-definable sets of his *1946*).

In Section 2, Gödel reformulated the continuum problem as the question:

Which $\aleph_\alpha$ is the cardinal number of $\mathbb{R}$?

He noted that Cantor had conjectured *CH* as an answer. But he did not mention that Cantor not only conjectured the truth of *CH* but also, on numerous occasions, claimed in print to have proved *CH*. (In fact, many mathematicians took *CH* as true during the 1880s and 1890s.) Nor did Gödel distinguish between *CH* and the weak continuum hypothesis, regarding them as equivalent since he assumed the axiom of choice. Later researchers, however, would find it necessary to distinguish carefully between *CH* and the weak continuum hypothesis when they attempted to solve the continuum problem (especially when the axiom of determinacy was involved; cf. Section 6 below).

Gödel stressed how little was known about the power $2^{\aleph_0}$ of $\mathbb{R}$, despite the many years that had passed since Cantor formulated *CH*. Indeed, Gödel remarked that only two facts were known: (a) $2^{\aleph_0}$ does not have cofinality ω and (b) the weak continuum hypothesis holds for the Σ^1_1 sets (the analytic sets), which, however, are only a tiny fraction of all the subsets of $\mathbb{R}$. In particular, he added, it was not known whether:

(i) There is some given aleph that is an upper bound for $2^{\aleph_0}$,
(ii) $2^{\aleph_0}$ is accessible or is weakly inaccessible,
(iii) $2^{\aleph_0}$ is singular or regular,

or

(iv) $2^{\aleph_0}$ has any restrictions on its cofinality other than König's result that its cofinality is uncountable.

What was known, he continued, was merely a large number of propositions that follow from *CH* as well as several propositions that are equivalent to it.[e]

Gödel observed that our ignorance about the power of the continuum was part of a greater ignorance about infinite cardinal products. In particular, the power of the continuum, $2^{\aleph_0}$, is the simplest non-trivial cardinal product, namely, the product of $\aleph_0$ copies of 2. He added that it was not even known whether

(v) there is some given cardinal that is an upper bound for some infinite product of cardinals greater than 1.

All that was known were certain lower bounds on infinite products, such as Cantor's theorem that the product of $\aleph_0$ copies of 2 is greater than $\aleph_0$ and the Zermelo–König theorem that if $\mathfrak{m}_\alpha < \mathfrak{n}_\alpha$ for all α in some given set I, then

$$\sum_{\alpha\epsilon I}\mathfrak{m}_\alpha < \prod_{\alpha\epsilon I}\mathfrak{n}_\alpha.$$

Thus it was not even known whether the product of $\aleph_0$ copies of 2 is less than the product of $\aleph_1$ copies of 2, that is, whether

$$2^{\aleph_0} < 2^{\aleph_1}.$$

In Section 3 Gödel argued that this lack of knowledge was not due entirely to a failure to find the appropriate proofs, but stemmed from the fact that the concept of set required "a more profound ⟦conceptual⟧ analysis ... than mathematics is accustomed to give" (page 518). He began his philosophical analysis of this concept by rejecting intuitionism, because it is destructive of set theory, and by laying aside the semi-intuitionistic viewpoints of Poincaré and Weyl for the same reason. Instead, he insisted that axiomatic set theory provides the proper foundation for Cantorian set theory. Protecting himself against the objection that the paradoxes threaten set theory, he asserted that no paradox has ever emerged for the iterated notion of "set of" (the cumulative type hierarchy V_α).[f] Here Gödel permitted a set of urelements (the integers, for example) as the basis from which the cumulative hierarchy is built up; incidentally, this corroborates the view that he adopted the cumulative hierarchy from *Zermelo 1930*. Finally, Gödel insisted that the continuum problem—if formulated in a combinatorial way as the question whether *CH* can be deduced from the axioms of set theory—retains

[e]What is now known about (i)–(iv) is discussed in Section 6 below.

[f]The cumulative hierarchy V_α is also called $R(\alpha)$. On this hierarchy, see p. 4 above of the introductory note to *1938*.

a meaning, independently of one's philosophical standpoint, even for the most extreme intuitionist.

If the usual axioms of set theory are consistent, Gödel remarked, then *CH* is either provable, disprovable, or undecidable. After noting that his *1940* ruled out the second possibility, he asserted that the third one is probably correct. To attempt to establish that *CH* is undecidable, he insisted, was the most promising way of attacking the problem.[g]

What is especially important, however, is this: Although Gödel argued that *CH* is almost certainly independent from ZF (as formulated in first-order logic), he insisted strongly that a proof of its independence would *not* solve the continuum problem. Indeed, he emphasized, as Zermelo (*1930*) had done, that "the axioms of set theory by no means form a system closed in itself, but, quite on the contrary, the very concept of set on which they are based ⟦the cumulative hierarchy⟧ suggests their extension by new axioms which assert the existence of still further iterations of the operation 'set of'" (*1947*, page 520). Consequently, he urged mathematicians to search for new large cardinal axioms which would, he hoped, decide *CH*. He added, with his incompleteness theorems in mind, that such axioms would settle questions about Diophantine equations undecidable by the usual axioms.

Here Gödel's strongly held Platonism was visible, as it had been in *1944* and as it would be even more strongly in *1964*. If the undecidability of Cantor's conjecture *CH* were established, he stressed, this would not settle the continuum problem—for essentially philosophical reasons. In fact, he wrote (*1947*, page 520),

> only someone who (like the intuitionist) 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, 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 that these axioms do not contain a complete description of this reality.

After granting that all large cardinal axioms known at the time failed to settle *CH*, since all of them were consistent with the axiom of constructibility, Gödel made an eloquent plea for new axioms (*1947*, page 521):

[g]On the other hand, Gödel did not mention that in *1923a* Skolem had also argued for the independence of *CH*, nor that he himself had worked intensively at establishing its independence during 1942 (as his *Arbeitshefte* attest).

> Even disregarding the intrinsic necessity of some new axiom, and even in case it had no intrinsic necessity at all, a decision about its truth is possible also ... inductively by studying ... its fruitfulness in consequences and in particular in ... consequences demonstrable without the new axiom, whose proofs by means of the new axiom, however, are considerably simpler and easier to discover, and make it possible to condense into one proof many different proofs .... There might exist axioms so abundant in their verifiable consequences, shedding so much light upon a whole discipline, and furnishing such powerful methods for solving given problems (and even solving them, as far as that is possible, in a constructivistic way) that quite irrespective of their intrinsic necessity they would have to be assumed at least in the same sense as any well-established physical theory.

This allusion to physics illustrates his view (already stated in *1944*, page 137) that the assumption of an underlying reality is as "necessary to obtain a satisfactory theory of mathematics" as the assumption of the reality of physical objects is "necessary for a satisfactory theory of our sense perceptions".

In Section 4, Gödel returned to his conjecture that *CH* is not decided by the usual axioms for set theory, arguing that there were at least two reasons for expecting such undecidability. The first was that there exist two quite different classes satisfying the usual axioms: the class of constructible sets and the class of "sets in the sense of arbitrary multitudes" (page 521). Thus he believed that one could not expect *CH* to be settled if one did not specify axiomatically which of these two classes was being considered. (He did not mention here, perhaps for philosophical reasons, a third such class, namely the class of ordinal-definable sets, to which he alluded in footnote 26.[h]) Half of his conjecture about undecidability had already been verified, namely the relative consistency of *CH* with the usual axioms, since *CH* is true in the class of constructible sets.

Gödel then made the important suggestion that "from an axiom in some sense directly opposite to this ⟦axiom of constructibility⟧ the negation of Cantor's conjecture ⟦*CH*⟧ could perhaps be derived" (page 522). The difficulty, of course, with Gödel's suggestion resides in the phrase "directly opposite", since he himself rightly believed that the mere nega-

[h]At first glance it might appear that in footnote 20 he conflated the class of ordinal-definable sets, introduced in *1946*, with the class of constructible sets. However, by comparing footnote 20 with footnote 26, one sees that in the earlier footnote he had in mind the constructible sets and, in the latter, the ordinal-definable sets. Likewise, in footnote 21 of *1964* he meant the constructible sets rather than the ordinal-definable ones.

tion of the axiom of constructibility would not suffice for this purpose (see Section 5). Yet insofar as the axiom of constructibility is a minimality axiom (expressing that the power set of a set, and hence the universe, is as small as possible), he may have had in mind here some kind of maximality axiom, as he certainly did in *1964* (see pages 167–168 below).

Gödel's second reason for expecting the independence of *CH* was that *CH* has certain "paradoxical" consequences which he found unlikely to be true—in particular, the existence of certain very thin subsets of $\mathbb{R}$ that have the power $2^{\aleph_0}$. The first effect of *CH* was to ensure that some kinds of thin subsets of $\mathbb{R}$, proved in ZFC to have instances that are uncountable, can actually have the power $2^{\aleph_0}$. Examples of such sets are

(1) sets of first category on every perfect subset of $\mathbb{R}$,

and

(2) sets carried into a set of measure zero by every continuous one-to-one mapping of $\mathbb{R}$ onto itself.

The second effect of *CH* was to imply that certain kinds of thin subsets of $\mathbb{R}$ can have the power $2^{\aleph_0}$ even though, in ZFC, no instances of these kinds are known that are uncountable. Here he gave as an example the sets of absolute measure zero (by definition, such a set is coverable by a given sequence of intervals of arbitrarily small positive lengths). He then gave several other examples, such as a subset of $\mathbb{R}$ including no uncountable set of measure zero.[i]

Gödel attempted to protect himself against the rejoinder that many kinds of point-sets obtained without *CH* (such as a Peano curve) are highly counterintuitive. In these cases, he argued, the implausibility of the point-sets was due to "a lack of agreement between our intuitive geometrical concepts and the set-theoretical ones occurring in the theorems" (page 524).

Nevertheless, there appears to be little evidence that analysts and set theorists now regard as "paradoxical" the kinds of thin sets cited by Gödel. For example, P. J. Cohen, when asked his opinion of these thin sets of power $2^{\aleph_0}$, was not troubled by them.[j] Likewise, in an article surveying recent work on *CH*, D. A. Martin responded negatively to Gödel's claim: "While Gödel's intuitions should never be taken lightly, it is very hard to see that the situation ⟦with *CH*⟧ *is* different from that of Peano curves, and it is even hard for some of us to see why the examples Gödel cites are implausible at all" (*1976*, page 87).

In the conclusion to his article, Gödel insisted that "it is very suspi-

[i]This particular example, however, was dropped in his *1964* version of the article.

[j]Personal communication from P. J. Cohen, April 1984.

cious 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" (*1947*, page 524). What are these "numerous plausible propositions"? We cannot be certain, since Gödel did not mention even one of them explicitly. Perhaps he simply intended such propositions to be the negations of those that he had called "paradoxical". In any case, here he was uncharacteristically incautious in his assertion. In 1970 he himself would find a proposition, which he then regarded as quite plausible, that implies *CH* (see Section 7).

5. Gödel's altered perspective in *1964*

The article *1964* resulted from a request, made to Gödel by P. Benacerraf and H. Putnam, for permission to reprint both of the essays *1944* and *1947* in their forthcoming source book *Philosophy of mathematics: Selected readings*. At first, Gödel hesitated to grant permission, fearing that the introduction to their book would subject his article to positivistic attacks. He asked Benacerraf, in conversation, for what amounted to editorial control of the editor's introduction to the source book. As an alternative, since such control could not be granted, Benacerraf assured Gödel that he would be shown the introduction and, furthermore, that the editors did not intend it to make a major philosophical statement but rather to outline the issues. Thus placated, Gödel gave permission to reprint his two essays, and began extensively revising *1947*. Benacerraf met with Gödel a number of times to go over the revisions, since Gödel felt that he did not know English "well enough". Yet Benacerraf knew no one with a more subtle grasp of the various ways in which an English text could be interpreted. While considering the proposed changes, Gödel repeatedly pointed out to Benacerraf various of their unwanted consequences.[k]

Whereas Gödel made no substantive modifications in reprinting *1944*, merely adding an initial footnote, he introduced more than one hundred separate alterations in *1947* in the course of preparing *1964*. Most of these changes were stylistic and reflected his increasing acquaintance with the nuances of the English language. In particular, a number of long and rather Germanic sentences were divided into shorter and more idiomatic ones.

[k]Personal communications from P. Benacerraf, July 1982 and March 1986.

Nevertheless, a substantial number of his changes were more than stylistic. A minor example is his reference in *1947* to a "natural" representation of the infinite cardinal numbers (the alephs), replaced in *1964* with a reference to a "systematic" representation. Far more surprising is his omission in *1964* of all reference to the ordinal-definable sets, which in *1947* he had discussed on page 522 and in footnote 26. It is uncertain what prompted him to omit this notion of set that he had introduced in his *1946*.

In Gödel's *Nachlass* there exist two drafts of his *1964*, each an offprint of *1947* with alterations written on it. The second of these contains a revision, not incorporated into *1964*, that credits Zermelo (*1930*) with "substantially the same solution of the paradoxes" as is embodied in the cumulative type hierarchy, which Gödel designates by his notion "set of". Again, it is unknown why he intended to credit Zermelo and then decided not to do so.

One particularly important addition occurred in footnote 20 of *1964*, where large cardinal axioms were discussed. Here he remarked that D. Scott (*1961*) had proved that the existence of a measurable cardinal contradicts the axiom of constructibility—in contrast to earlier large cardinal axioms, such as those of Mahlo (*1911, 1913*), which are consistent with that axiom. Consequently, he continued, the relative consistency proof for *CH* by means of the class of constructible sets fails if one assumes that there is a measurable cardinal. (In *1971a*, however, J. Silver established that *GCH* holds in the class of sets constructible from a countably additive measure on the least measurable cardinal. In *1967*, Levy and Solovay had already shown, by means of forcing, that *CH* is relatively consistent with a measurable cardinal; see footnote p below.) Gödel then added that it was not yet certain whether "the general concept of set" implies the existence of a measurable cardinal in the same way as it implies Mahlo's axioms. By contrast with this uncertainty, in Gödel's unpublished revision of September 1966 he argued for the existence of a measurable cardinal since this follows "from the existence of generalizations of Stone's representation theorem to Boolean algebras with operations on infinitely many elements" (page 261 below).[l]

Another noteworthy addition occurred in footnote 23 of *1964*. Whereas in the *1947* version of this footnote, Gödel had argued that *CH* might be decided by means of some axiom diametrically opposite to the axiom of constructibility, in *1964* he spelled out what he meant:

[l]See also Gödel's oral comments about measurable cardinals to Solovay on p. 19 above.

> I am thinking of an axiom which (similar to Hilbert's completeness axiom in geometry) would state some maximum property of the system of all sets, whereas axiom A ⟦the axiom of constructibility⟧ states a minimum property. Note that only a maximum property would seem to harmonize with the concept of set explained in footnote 14 ⟦arbitrary sets of the cumulative type hierarchy⟧.

Hilbert's axiom of completeness (*1902*), which belongs to second-order logic, had characterized Euclidean geometry (and, analogously, the real numbers) as the maximal structure satisfying his other axioms. What Gödel proposed for set theory was vague but suggestive; in particular, the various large cardinal axioms can be regarded as steps in the direction of maximality. His meaning is made more definite by a letter he wrote to S. Ulam (quoted in *Ulam 1958*, page 13) apropos of von Neumann's axiom (*1925*) that a class S is a proper class if and only if S is equipotent with the class V of all sets:

> The great interest which this axiom has lies in the fact that it is a maximum principle, somewhat similar to Hilbert's axiom of completeness in geometry. For, roughly speaking, it says that any set which does not, in a certain well-defined way, imply an inconsistency exists. Its being a maximum principle also explains the fact that this axiom implies the axiom of choice. I believe that the basic problems of abstract set theory, such as Cantor's continuum problem, will be solved satisfactorily only with the help of stronger axioms of *this* kind, which in a sense are opposite or complementary to the constructivistic interpretation of mathematics.

More recent attempts to formulate such a maximum principle have not been completely successful. J. Friedman (*1971*) proposed one such proposition, called the generalized maximization principle, and showed it to be equivalent to *GCH*; thus far it has attracted little attention. Recently, S. Shelah's strong version of his proper forcing axiom, PFA+ (by which, in *1982*, he generalized Martin's axiom in the direction of maximality), and the principle dubbed "Martin's maximum" by M. Foreman, M. Magidor and Shelah have each been shown (by them in *198?*, and independently by S. Todorcevic) to imply that $2^{\aleph_0} = \aleph_2$; more recently, Todorcevic has announced a proof that $2^{\aleph_0} = \aleph_2$ already follows from PFA. At present, there is no consensus among set theorists as to the truth of these hypotheses. Nor does the author wish to conjecture what Gödel would have thought of them.

By far the most substantial alteration in *Gödel 1964* was the addition of a long supplement, together with a brief postscript noting that Cohen (*1963, 1964*) had just established the independence of *CH* and thereby

had verified Gödel's *1947* claim that *CH* would not be settled by the usual axioms for set theory. The supplement consists of a discussion of new results that Gödel considered important, along with an extended philosophical defense of his Platonist position on *CH*.

Ostensibly, this defense was stimulated by A. Errera's article *1952*, claiming that if *CH* is not decided by the usual axioms for set theory, then the question whether *CH* is true will lose its meaning, just as happened to the parallel postulate when non-Euclidean geometry was proved consistent. Gödel insisted that, on the contrary, "the situation in set theory is very different from that in geometry, both from the mathematical and from the epistemological point of view" (*1964*, page 270). Here he stressed the asymmetry between assuming that there is, and assuming that there is not, a strongly inaccessible cardinal. The former assumption was fruitful in the sense of having consequences for number theory, while the latter was not. Likewise, he continued, *CH* "can be shown to be sterile for number theory . . . , whereas for some other assumption about the power of the continuum this perhaps is not so" (page 271). This "sterility", for first-order number theory, was due to the fact that $\mathbb{N}$ is absolute for L, the class of all constructible sets. (In his revisions of 1966–1967, discussed below, he here replaced *CH* by *GCH*, and "power of the continuum" by "power of $2^{\aleph_\alpha}$".)

By using later results, we can say more. In *1969* R. A. Platek established that if a sentence of second-order number theory is provable from *CH*, then it is already provable from the usual axioms of set theory along with the axiom of choice; moreover, he showed that the same holds for any Π^2_1 sentence of third-order number theory.[m] (No further extension was possible, since *CH* itself is a Σ^2_1 sentence.) By 1965 Solovay had independently found Platek's result on *CH*, and in addition had discovered a corresponding result for not-*CH*: If a Π^1_3 sentence of second-order number theory is provable from not-*CH*, then it is already provable from ZF and the axiom of choice.[n] In this sense, then, both *CH* and not-*CH* are sterile for number theory.

The Platonist views put forward by Gödel in *1947* were strengthened in *1964*, not only in the supplement but in the text as well, where he described himself as "someone who considers mathematical objects to exist independently of our constructions" (page 262). Nevertheless, his Platonism was most visible in the supplement, where on page 271 he pursued at some length the analogy between mathematics and physical theories that he had already broached in *1947*:

[m] S. Kripke and J. Silver had each independently arrived at the same result (*Platek 1969*, p. 219).

[n] Personal communication from R. M. Solovay, 27 October 1984.

Despite their remoteness from sense experience, we do have something like a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true. I don't see any reason why we should have less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception, which induces us to build up physical theories and to expect that future sense perceptions will agree with them, and, moreover, to believe that a question not decidable now has meaning and may be decided in the future.

In September 1966, Gödel wrote an addendum called "Changes to be made in 3rd edition", anticipating that *1964* would be reprinted.[o] Already in the postscript to *1964*, which was added when *1964* was almost in press, Gödel had mentioned Cohen's 1963 proof of the independence of *CH*. But in the 1966 addendum Gödel expressed himself more strongly: "Cohen's work ... is the greatest advance in the foundations of set theory since its axiomatization". He added that Cohen's forcing "has been used to settle several other important independence questions"; yet he mentioned only one result, namely, that all known large cardinal axioms "are not sufficient to answer the question of the truth or falsehood of Cantor's continuum hypothesis" (page 270 below). Although he did not give a reference, he was almost certainly referring to the result of Levy and Solovay that, for all known large cardinals κ (and in particular for measurable cardinals), if there is a model of set theory containing κ, then there is a model containing κ in which *CH* is true and another model containing κ in which *CH* is false.[p]

6. Later research affecting *1947* and *1964*

There were two major developments that affected Gödel's program, as proposed in *1947* and *1964*, for settling *CH*. The first of these was research on large cardinals, and the second consisted of new independence results obtained by Cohen's method of forcing. In fact, there has been an extremely fruitful interaction, which still continues, between these two lines of development.

[o]These changes have been incorporated into the text of *1964* in the present volume, where they are printed in square brackets. Gödel made additional changes in a manuscript of October 1967. The textual notes record the exact changes to *1964* made in 1966 and 1967. On the other hand, the reprinting of *1964* in *Benacerraf and Putnam 1983* does not include these alterations and additions.

[p]This result, announced in *Levy 1964* and independently in *Solovay 1965a*, was proved in detail in *Levy and Solovay 1967*.

The question of the relationship of *CH* to large cardinal axioms, and to new axioms such as the axiom of determinacy (*AD*), has turned out to be unexpectedly complicated. Large cardinal axioms are now known to affect the class of sets for which the weak continuum hypothesis is true. In particular, Solovay showed (*1969*) that if there exists a measurable cardinal, then the weak continuum hypothesis is true for Σ^1_2 sets. Moreover, *AD*, which may be regarded as a kind of large cardinal axiom, implies that the weak continuum hypothesis holds for every subset of $\mathbb{R}$. Unfortunately, *AD* contradicts *CH*, since it implies that the real numbers cannot be well-ordered (*Mycielski 1964*, page 209), and so was surely unacceptable to Gödel as a solution to the continuum problem. On the other hand, the axiom of projective determinacy (that is, *AD* restricted to the projective sets) is also a kind of large cardinal axiom and has recently been shown to be consistent with the axiom of choice, provided a sufficiently large cardinal exists. Indeed, D.A. Martin and J.R. Steel (*198?*) have recently established, among other things, that if there is a supercompact cardinal (or, what is weaker, infinitely many Woodin cardinals), then projective determinacy is true and hence the weak continuum hypothesis is true for all projective sets.[q]

The second line of development, independence proofs, profoundly affected Gödel's program. In 1963 Cohen established not only that *CH* is independent but also that $2^{\aleph_0}$ can be arbitrarily large among the alephs. Feferman then showed that it is consistent with ZF to have $2^{\aleph_0} = 2^{\aleph_1}$, Luzin's second continuum hypothesis (*Cohen 1964*, page 110). From Cohen's work it followed, in regard to (i)–(iv) on page 161 above, that $2^{\aleph_0}$ is not bounded above by any given aleph and can be either accessible or weakly inaccessible, singular or regular; moreover, there are no restrictions on the cofinality of $2^{\aleph_0}$ other than König's theorem. Solovay independently determined the α for which $2^{\aleph_0} = \aleph_\alpha$ is consistent, namely all $\aleph_\alpha$ of uncountable cofinality (*1965*). Thus it was shown that our ignorance regarding (i)–(iv) is inevitable if we assume only the usual first-order axioms of set theory. (In *1964*, Gödel was inclined to believe that $2^{\aleph_0}$ is rather large, and favored the proposition that $2^{\aleph_0}$ is the first weakly inaccessible cardinal (*1964*, page 270).)

Shortly after Cohen announced his results in 1963, research on the continuum problem turned to establishing what are the possibilities for the continuum function $F(\aleph_\alpha) = 2^{\aleph_\alpha}$, defined on all ordinals. The first major breakthrough was Easton's theorem (*1964*, *1970*) that the continuum function F can, on regular cardinals, be any nondecreasing function

[q]By combining this result with earlier work of Woodin, one obtains from a supercompact cardinal the existence of a transitive class model of ZF + *AD* + *DC* containing all real numbers and all ordinals.

for which the cofinality of $F(\aleph_\alpha)$ is greater than $\aleph_\alpha$. For a decade there was a consensus among set theorists that something analogous to Easton's result would also be shown for singular cardinals. Both Easton and Solovay, among others, attempted to solve what came to be called the singular cardinals problem.

Consequently, set theorists were quite surprised in 1974 when Silver established that if *GCH* holds below a singular cardinal κ of uncountable cofinality, then it holds at κ as well (*Silver 1975*). Even this result, however, by no means settled the singular cardinals problem—provided that this problem is taken as asking for all the laws about cardinal exponentiation relative to singular cardinals. A first step occurred when Bukovský (*1965*) proved that cardinal exponentiation is determined by the so-called gimel function $\aleph_\alpha^{\mathrm{cf}(\aleph_\alpha)}$—a result that Gödel had stated but not proved in *1947* (page 517).

One important spinoff of Silver's result was Jensen's covering theorem (*Devlin and Jensen 1975*), which states that if the large cardinal axiom asserting the existence of $0^\#$ is false, then the singular cardinals hypothesis is true.[r] This hypothesis asserts that the continuum function $F(\aleph_\alpha) = 2^{\aleph_\alpha}$ is determined by its behavior at regular $\aleph_\alpha$. Thus, although known large cardinal axioms did not settle *CH*, the negation of a large cardinal axiom settled the behavior of the continuum function F at singular cardinals.

Silver's result was extended by Galvin and Hajnal (*1975*) for the case where κ is a singular strong limit cardinal of uncountable cofinality. For such a κ, they found an upper bound on 2^κ in terms of the behavior of 2^λ for a stationary set of $\lambda < \kappa$. Somewhat earlier, in *1974*, Solovay proved that if κ is strongly compact, then there is a proper class of cardinals for which *GCH* holds, namely, the class of singular strong limit cardinals greater than κ.

Magidor (*1977*) established that Silver's assumption of uncountable cofinality is necessary. In particular, Magidor showed, using a very large cardinal, that if *GCH* holds below $\aleph_\omega$, then it may happen that $2^{\aleph_\omega} = \aleph_{\omega+2}$. Shelah (*1982*) obtained a bound on $2^{\aleph_\omega}$ under the assumption that *GCH* holds below $\aleph_\omega$. Furthermore, Shelah discovered an analogue of the Galvin–Hajnal result for singular cardinals of countable cofinality. Finally, using a large cardinal assumption, Foreman and Woodin found a model of ZFC in which *GCH* fails everywhere; Woodin later improved this to $2^{\aleph_\alpha} = \aleph_{\alpha+2}$ for all α. (It is known, thanks to an earlier result of L. Patai, that if, for all α and for a fixed β, $2^{\aleph_\alpha} = \aleph_{\alpha+\beta}$, then β is finite; see *Jech 1978*, pages 48 and 580.)

[r]Concerning $0^\#$, see p. 21 above of the introductory note to *1938*.

Recently Foreman (*1986*) has proposed the axiom of resemblance, which he regards as a generalization of large cardinal axioms, and has announced that it implies both *GCH* and the axiom of projective determinacy. (He has shown in *1986* that, from *CH* and the axiom of resemblance, *GCH* follows.) For Gödel, however, the fact that the axiom of resemblance implies *GCH* would probably have disqualified it as settling the continuum problem.[s]

7. Gödel's unpublished papers on *CH*

After his proposal for using large cardinal axioms to decide *CH* did not succeed, Gödel introduced other axioms that he hoped would decide it. In January 1964, before he knew that such axioms, and in particular the existence of a measurable cardinal, did not settle *CH*, he wrote to Cohen about a related question:

> Once the continuum hypothesis is dropped, the key problem concerning the structure of the continuum, in my opinion, is the question of whether there exists a set of sequences of integers of power $\aleph_1$ which, for any given sequence of integers, contains one majorizing it from a certain point on .... I always suspected that, in contrast to the continuum hypothesis, this proposition is correct and perhaps even demonstrable from the axioms of set theory.

Six years later, Gödel postulated the existence of such a set of sequences as one of his axioms, now called Gödel's square axioms, which were intended to resolve the continuum problem.

The square axioms are an axiom schema stating that, for each natural number n, there exists a scale, of type ω_{n+1}, of functions from ω_n to ω_n.[t] Perhaps Gödel was led to formulate the square axioms by reading *Borel 1898*, which he cites. On page 116, Borel claimed that there exists a scale for the case $n = 0$ for all "effectively defined" functions, though he did not give a proof of his claim.

Gödel introduced these axioms in his final contribution to solving the continuum problem, a short paper written in 1970 and entitled "Some considerations leading to the probable conclusion that the true power of the continuum is $\aleph_2$", which he intended to publish in the *Proceedings*

[s]A recent argument that $2^{\aleph_0} \geq \aleph_\omega$ can be found in *Freiling 1986*.

[t]In other words, let F be the set of functions from ω_n to ω_n; then F has a subset S of power $\aleph_{n+1}$ such that for any function f in F there is some function g in S such that for some α and for all $\beta > \alpha$, $f(\beta) < g(\beta)$.

of the National Academy of Sciences.[u] In this paper he proposed four axioms (or axiom schemas), of which the square axioms were the first. The second axiom asserted that there are exactly $\aleph_n$ initial segments of the scale given by the square axioms. The third axiom was that there exists a maximal scale of functions from $\mathbb{N}$ to $\mathbb{R}$ such that "every ascending or descending sequence has cofinality ω". The fourth and final axiom consisted of the Hausdorff continuity axiom for the scale given by Gödel's third axiom. (Axiom 4 implies that $2^{\aleph_0} = 2^{\aleph_1}$.)

Gödel mailed his paper to Tarski, who then asked Solovay to examine its correctness. D. A. Martin, to whom Solovay had sent a copy of the paper, found that a result in it contradicted a theorem of Solovay's. In particular, Martin observed, since Solovay had shown that the square axioms do not put an upper bound on the size of $2^{\aleph_0}$, Gödel had to be mistaken in his claim that these axioms yield the result that $2^{\aleph_0}$ is bounded by $\aleph_2$.[v] On 19 May 1970 Tarski returned the paper to Gödel, adding in his covering letter that "you will certainly hear still in this matter either from me or from somebody else in Berkeley."

The whole matter was tinged with irony. For by 1965, having become convinced of the proposition that the square axioms do put an upper bound on $2^{\aleph_0}$, Gödel discussed this proposition with Solovay at the Institute for Advanced Study. At Gödel's request Solovay looked into the matter and found that there are models of set theory satisfying the square axioms but having $2^{\aleph_0}$ arbitrarily large. Gödel remained unconvinced, despite K. Prikry's assurances that Solovay was correct.[w] Solovay's result had to be rediscovered independently by E. Ellentuck (about 1973) before Gödel came to accept it.[x]

In 1970, not long after receiving Tarski's letter, Gödel drafted a second version of his paper on *CH*, entitled "A proof of Cantor's continuum hypothesis from a highly plausible axiom about orders of growth". His title represented a sudden and unexpected shift in his longstanding rejection of *CH*. This change in attitude appears to have been due to his belief that the square "axioms for $\aleph_n$ (or even any regular ordinal) are highly plausible, much more so than the continuum hypothesis." Indeed, he claimed that *CH* follows from the square axiom for $\aleph_1$ (that is, for $n = 1$). In conclusion, he wrote:

> It seems to me this argument gives *much* more likelihood to the truth of Cantor's continuum hypothesis than any counterargument set up to now gave to its falsehood, and it has at any rate the virtue of deriving the power of the set of *all* functions $\omega \to \omega$ from that of certain *very* special sets of these functions. Of course the argument can be applied to higher cases of the generalized continuum hypothesis (in particular to all $\aleph_n$). It is, however, questionable whether the whole generalized continuum hypothesis follows.

At the top of this second version Gödel had written "nur für mich geschrieben" ("written only for myself"). It is unclear who, if anyone, saw this version before Gödel's death.

A third version of the paper (so Gödel described it) was a draft of a letter to Tarski, apparently never sent, that survives in Gödel's *Nachlass.* This letter is much closer in spirit to the first version of the paper than to the second. In the letter Gödel stated that he had written the first version hurriedly right after an illness for which he had been taking medication. What he had proved, he now believed, "is a nice equivalence result for the generalized continuum hypothesis ... ⟦showing that it⟧ follows from certain very special and weak cases of it." Gödel concluded the letter with some speculations:

> My conviction that $2^{\aleph_0} = \aleph_2$ of course has been somewhat shaken. But it still seems plausible to me. One of my reasons is that I don't believe in any kind of irrationality such as, e.g., random sequences in any absolute sense. Perhaps $2^{\aleph_0} = \aleph_2$ does follow from my axioms 1–4, but unfortunately Axiom 4 is rather doubtful, while axioms 1–3 seem *extremely likely* to me.

Yet he conceded that Axioms 1–3 do not imply $2^{\aleph_0} \leq \aleph_2$.[y]

Thus ended Gödel's last attempt to settle the continuum problem, which he had analyzed so brilliantly in *1947* and *1964*.[z]

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[u] The various versions of this paper are being considered for inclusion in Volume III of these *Collected works.*

[v] Personal communication from D. A. Martin and R. M. Solovay, 13 February 1984.

[w] Personal communication from R. M. Solovay, 4 April 1984.

[x] Ellentuck only learned of Solovay's priority for this result after finding it himself; see Ellentuck's note, dated February 1973, in Gödel's *Nachlass.*

[y] Before 1973, Gödel's square axioms were studied by G. Takeuti, who established that the existence of a scale from ω_1 to ω_0 implies *CH.* These axioms are also investigated in *Ellentuck 1975*, *Takeuti 1978* and *P. E. Cohen 1979.*

[z] I would like to thank S. Feferman for many substantive suggestions, and J. Dawson for many stylistic ones, to an earlier draft of this introductory note; Dawson has also been of considerable assistance on archival matters. I am especially grateful to R. M. Solovay for his many useful suggestions regarding Section 6.