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EARLY HISTORY OF THE GENERALIZED CONTINUUM HYPOTHESIS: 1878–1938

GREGORY H. MOORE

Abstract. This paper explores how the Generalized Continuum Hypothesis (GCH) arose from Cantor's Continuum Hypothesis in the work of Peirce, Jourdain, Hausdorff, Tarski, and how GCH was used up to Gödel's relative consistency result.

The early history of the Continuum Hypothesis has been studied by various historians of mathematics, in particular by Dauben [1979] and Moore [1989]. However, the origins and early history of the Generalized Continuum Hypothesis hahaveve not. We do so in the present article. This necessitates some discussion of what was known during the same period about the Continuum Hypothesis, cardinal exponentiation, and cardinal products of infinitely many factors.

§1. The origins of the Continuum Hypothesis. Cantor published the first version of the Continuum Hypothesis in [1878], and for reasons that will become clear, we call this the Weak Continuum Hypothesis (WCH): Any infinite subset of the set $\mathbb{R}$ of all real numbers can be put in one-one correspondence with $\mathbb{R}$ or with the set $\mathbb{N}$ of all natural numbers. A few years later, after developing his theory of transfinite ordinal numbers, he gave a second form to the Continuum Hypothesis: The set $\mathbb{R}$ has the cardinality of the set of all countable ordinal numbers [1883, p. 574]. He first communicated his belief that he could prove this second form in a letter of 25 October 1882 to Gösta Mittag-Leffler [Cantor 1991, p. 91]. Finally, in the 1890s, after he developed the notion of exponentiation between cardinal numbers and also his symbolism for alephs (i.e., the cardinal numbers of infinite well-ordered sets), he gave a third and final form to the Continuum Hypothesis,[1] which

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[1]This is equivalent to the second form, whereas the equivalence of the third form to WCH requires some use of the Axiom of Choice, or the equivalent principle that every set can be well-ordered. For this reason, and because the first form can be true in a model of set theory in which the real numbers cannot be well-ordered, we call the first form the "Weak Continuum Hypothesis". The Axiom of Choice was formulated in 1904 by Zermelo. In the 1960s, the Axiom of Determinacy was shown to imply WCH but to make CH false.

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we abbreviate in the now standard way as CH:

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

In a letter of 12 November 1882 to Mittag-Leffler, Cantor gave what was in effect a slight generalization of CH when he asserted that the set of all real functions has the cardinality of the third number-class, i.e., $\aleph_2$ in his 1895 notation.[2] In that letter he stated this assertion as a theorem, not as a conjecture. He published the contents of these letters in ***Mathematische Annalen*** [1883, p. 590]. However, Cantor never generalized CH any further. As we will see below, the creation of GCH occurred more than once, and involved C. S. Peirce, Philip Jourdain, Felix Hausdorff, and Alfred Tarski.

Between those two 1882 letters to Mittag-Leffler, Cantor wrote to Richard Dedekind (who had been his chief mathematical correspondent during the 1870s but in 1882 was ceasing to be so) a long letter on 5 November on the same subject and informed him of many of the results found in the October letter to Mittag-Leffler. In regard to the Continuum Hypothesis Cantor made an additional comment. For he referred to the Weak Continuum Hypothesis as the "Two-Class Theorem" ("Zweiclassensatz")—no doubt because in effect WCH uses cardinality to partition the infinite subsets of $\mathbb{R}$ into just two classes, those of the cardinality of $\mathbb{N}$ and those of the cardinality of $\mathbb{R}$. But this name never appeared in his publications. Nor he did use the term "Continuum Hypothesis", then or ever, in any published or unpublished writing.

For a brief period late in 1884, Cantor was convinced that CH was false. The reason was as follows. Previously he had found his proof that any two dense orders of power $\aleph_0$ without endpoints are order-isomorphic. Now he thought that he could prove the isomorphism of any two dense orders of power $\aleph_1$ without endpoints. But within two days he reversed himself and became convinced that this proposition about dense orders of power $\aleph_1$ was false [Moore 1989, pp. 91–93].

Cantor's final published work on set theory appeared as a two-part article, the "Beiträge" [1895; 1897], in ***Mathematische Annalen***. The connection between CH and this article is evident from a letter that Cantor wrote to Valerian von Derwies on 17 October 1895 concerning the Continuum Hypothesis:

> You are right to stress the importance of a rigorous proof of the theorem
>
> $$2^{\aleph_0} = \aleph_1,$$
>
> which I expressed in another form in 1877 .... I hope in this respect to satisfy you in a continuation of the work whose first installment [1895] you have at hand.[3]

[2]Cantor 1991, p. 99.

[3]This letter is not found in Cantor's published letters [Cantor 1991] but only in his unpublished letterbooks. However, in a letter of July 1894 to Alexander Vassiliev, Cantor

Thus Cantor hoped to establish CH in the "Beiträge". But when its second installment appeared [1897], it made no mention of the Continuum Hypothesis.

In the first installment [1895] he had stated the equation $\mathfrak{o} = 2^{\aleph_0}$, where $\mathfrak{o}$ was now his symbol for the cardinal of the real numbers and where the notation $2^{\aleph_0}$ used his new concept of cardinal exponentiation to represent the real numbers as binary sequences.[4] But he did not state CH in print, then or ever, in the form $2^{\aleph_0} = \aleph_1$ found in his letter to von Derwies.

Nevertheless, that form could have suggested a straightforward generalization if zero were replaced by an arbitrary ordinal α:

$$2^{\aleph_\alpha} = \aleph_{\alpha+1},$$

This is what is now called the Generalized Continuum Hypothesis (GCH). At that time, no one published this generalization, and at present there exists no evidence that anyone even thought of it.[5]

§2. The partial generalization of the Continuum Hypothesis by C. S. Peirce. The first to generalize the Continuum Hypothesis past Cantor's claim that the set of all real functions has cardinality $\aleph_2$ was the American logician Charles S. Peirce. Writing to Philip Jourdain on 5 December 1908, Peirce recalled that he "cannot well have made the least acquaintance with Cantor's work until the winter of 1883–4 or later" [Peirce 1976, p. 882]. In his unpublished book ***Grand Logic*** of 1893, which was only printed in 1933, Peirce did not find Cantor's definition of a continuum satisfactory, but was intrigued by his notion of cardinal number. With his penchant for neologisms and his own idiosyncratic terminology, Peirce renamed Cantor's term "cardinal number" as "multitude".

Peirce returned to the theme of transfinite cardinals in an unpublished article of about 1895, "On Quantity, with Special Reference to Collectional and Mathematical Infinity" (only printed in 1976). There he was particularly concerned to prove the Trichotomy of Cardinals. In effect, however, his

mentioned that he had recently received a letter from a St. Petersburg mathematician by the name of Valerian Derwies [Cantor 1991, p. 352].

[4]The use of German letters for cardinal numbers goes back to that article [Cantor 1895, p. 484] and continued in general use, e.g., in [Levy 1979, p. 92]. The more recent avoidance of German letters, with κ and λ used instead for cardinals, can be found in [Kunen 1980, p. 28].

[5]Grattan-Guinness [2000, p. 116] argued that Cantor "may have perceived it [GCH] in some intuitive form." While it is impossible to refute what Cantor may have perceived, there is no evidence known which is even suggestive that he did perceive it in some form. Grattan-Guinness's argument was that, since Cantor was aware of the aleph hierarchy and the beth hierarchy, it "would surely have been natural to him" to suppose their identity. But Cantor never stated the beth hierarchy, even at all finite levels, in any form. Cantor does not even seem to have been stimulated in the direction of GCH by the appearance of the beth hierarchy in the 1900 letter from Peirce.

argument assumed what it meant to prove [1976, p. 50]. In that same article, on the other hand, Peirce introduced a hierarchy, which he later named the beths, of transfinite cardinals. In the beth notation of his unpublished letter of 23 December 1900 to Cantor, this hierarchy is as follows:

$$\beth_0 = \aleph_0,\ \beth_1 = 2^{\aleph_0},\ \beth_2 = 2^{2^{\aleph_0}},\ \text{etc.}$$

Although Peirce did not introduce this notation until 1900, he defined this sequence of cardinals in detail in the 1895 article, calling $\beth_1$ "the first abnumeral", $\beth_2$ "the second abnumeral", and so on [1976, p. 52]. If he had used a recursive definition, which he did not, it would have contained the clause that $\beth_{n+1} = 2^{\beth_n}$ for all finite n. But he believed that there is no cardinal $\beth_\omega$.

This definition of beth numbers, named after the second Hebrew letter and indexed by all ordinals, is standard now. Apparently it was reinvented in the 1960s in the context of model theory, perhaps by Morley [1965, p. 265], who observed that "the generalized continuum hypothesis may be stated as $\beth_\alpha = \aleph_\alpha$ for all α." But the way in which he introduced a more general notation 2^κ_α, from which he defined the beth hierarchy as a special case, suggests that this hierarchy may have originated with someone else, perhaps as part of the folklore of the subject. Scott [1965, p. 335] also explicitly used $\beth_{\omega_1}$ but in a way which suggests that he may have gotten the beth notation from Morley. In any case, the hierarchy spread first in the context of Hanf numbers of logics (e.g., [Lopez-Escobar 1966], [Malitz 1968], [Shelah 1970]) and, a few years later, into set theory textbooks (e.g., [Drake 1974], [Levy 1979], [Kunen 1980], [Moschovakis 1994]).[6] Already in [1925, p. 9] Tarski had introduced a different notation for the (later) beth hierarchy, namely $\aleph_{\pi(\alpha)}$, but his notation did not become generally accepted; nevertheless Tarski's notation was the first time that the beth hierarchy was indexed by all ordinals rather than, as with Peirce, just finite ones.

In his 1895 article Peirce immediately went on to make two assertions: "It remains to be shown that there can be no multitude [cardinal] intermediate between these multitudes, and none greater than them all." The first assertion included the Weak Continuum Hypothesis as a special case and generalized it below $\beth_\omega$:

(1) For every finite n, there is no cardinal M such that $\beth_n < M < \beth_{n+1}$.

This was the first version of the Generalized Continuum Hypothesis. He gave a fallacious argument for (1).[7]

Peirce's second assertion was, in effect, that there is no cardinal greater than $\beth_n$ for every finite n. In modern terminology, he claimed that $\beth_\omega$ does

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[6] An anonymous referee pointed out the occurrence in Morley, the relevance of model theory, and some uses re Hanf numbers. I have not succeeded in finding uses of the beth hierarchy notation, other than by Peirce, prior to [Morley 1965].

[7] See [Moore 1989, pp. 101–102]. Please note that at the end of the last line on page 102 it should read $2^{\beth_n}$, not $2^{\aleph_n}$.

not exist. His fallacious argument for this, which was repeated in his 1900 letter to Cantor, is discussed in [Moore 1989, pp. 103–104].

Nevertheless, in an unpublished article of about 1909 [1909/1933, p. 572], Peirce asserted that his argument was correct and that (1) was true, so that the only infinite cardinals are the beths of finite index. Consequently, he concluded, there are many more ordinals than cardinals—contradicting Cantor's 1883 claim that there are just as many alephs as ordinals.

It is unlikely that any mathematician was influenced by Peirce's version of the Generalized Continuum Hypothesis during his lifetime.

§3. The Continuum Hypothesis around 1900. When Arthur Schoenflies published the chapter on set theory in the multi-volume German encyclopedia of mathematics, the ***Encyklopädie der mathematischen Wissenschaften***, he referred to WCH without giving any name to it, calling it a "conjecture" ("Vermutung"), and made no mention of CH in the form $2^{\aleph_0} = \aleph_1$ [1899, p. 186]. A year later, when he published his first book-length report on set theory, he remained skeptical that the real numbers can be well-ordered. Referring to c as the cardinal of the set of real numbers, he mentioned "Cantor's conviction ... [that] $c = \aleph_1$", but concluded: "Nevertheless, the question of the power of the continuum remains unsolved today, just as when Cantor first recognized [in 1874] that the continuum is uncountable" [1900, pp. 49–50].

The Continuum Problem (which was the "question" in the quotation from Schoenflies but which had not yet been named) was thrust into the limelight when David Hilbert discussed it in his celebrated Paris lecture of 1900 at the International Congress of Mathematicians. Indeed, he posed the Continuum Problem as the first of 23 problems central to the development of twentieth-century mathematics. He treated the Continuum Problem as consisting of two parts: (a) the Weak Continuum Hypothesis and (b) the existence of a well-ordering for the real numbers [1900, pp. 263–264]. Hilbert maintained an active interest in the Continuum Problem during the decades that followed, and he made a serious attempt to prove it during the period 1925–1928.

In part, Hilbert was doubtless influenced to include the Continuum Problem in his Paris lecture of 1900 by Cantor's letter to him of 26 September 1897, where Cantor reaffirmed his belief that "the power c of the linear continuum is equal to a definite aleph ($c = \aleph_1$, as I hope to show)." Although Hilbert did not accept the letter's alleged proof that every set can be well-ordered, he was probably inclined to accept CH as true. (Certainly he never gave any indication, then or later, that he doubted CH.) He agreed with Cantor that these two problems were central to the further development of set theory. Moreover, his Paris lecture presented the Continuum Problem as part of analysis and as continuing the nineteenth-century arithmetization of analysis carried out by Cantor and others.

Besides drawing the attention of the mathematical world at large to the Continuum Problem and to the problem of well-ordering the real numbers, Hilbert influenced several mathematicians to investigate those problems. He encouraged the work of Felix Bernstein and Felix Hausdorff on the Continuum Problem. Furthermore, Ernst Zermelo's proof of 1904 that every set can be well-ordered was written, and published, as a letter to Hilbert.

Bernstein's doctoral dissertation [1901], written under Hilbert, stated that two problems presently stood at the center of set-theoretic interest. The first of these was what he named Cantor's Continuum Problem: How many distinct cardinals $\overline{\overline{A}}$ exist, where A is an infinite subset of $\mathbb{R}$? This was the first time that the term "Cantor's Continuum Problem" ("das Cantorsche Continuumproblem") appeared in print. Hilbert in his Paris lecture had entitled his first problem "Cantor's Problem of the Power of the Continuum", and presumably Bernstein obtained the name "Continuum Problem" by shortening Hilbert's title.

In December 1900 Bernstein sent Hilbert a letter containing some of his results on set theory to be included in his dissertation. One point in the letter directly concerned the Continuum Hypothesis:

> Perhaps there has come to your attention a work by Beppo Levi [1900], in which he claims to have solved Cantor's [Continuum] Problem and intends to communicate a proof of this later. That work gave me a reason to put forward the following theorem:
>
> 4. The totality of all linear closed sets has the power of the continuum.
>
> Therefore one can, for example, characterize each perfect set by a definite real number. It follows easily—from Theorem 4 and from Cantor's theorem that the totality of all subsets of the continuum has a power, 2^c, which is greater than c—that B. Levi's main theorem is false.
>
> The interest of Theorem 4 lies in the fact that it shows how small is the domain of subsets [of $\mathbb{R}$] for which, up to the present, the Two-Class Theorem [WCH] has been proved.
>
> I have communicated all these theorems to Professor Cantor, and he has confirmed them.

This letter shows that in 1900 both Bernstein and Hilbert were familiar with Cantor's unpublished term "Two-Class Theorem" for WCH.

The article [Levi 1900] which Bernstein cited here was devoted to the general notion of real function and, in particular, to finding properties possessed by all real functions. His main theorem stated that every subset A of $\mathbb{R}$ can be represented as a union over the natural numbers n of sets $F_n - B_n$, where the F_n are closed sets and the B_n are of first category. He restated his theorem in the form that every subset A of $\mathbb{R}$ can be decomposed into a closed set augmented by a set of first category and diminished by another set of first category; stated in later terminology, his assertion was that every subset A of $\mathbb{R}$ has the Baire property.

Levi added that it was "an immediate consequence" of this theorem that WCH holds. He distinguished sharply between WCH and CH—unlike mathematicians before him and most of those after him. Yet he gave no argument as to why WCH holds if every subset of $\mathbb{R}$ has the Baire property. He promised that a forthcoming article, to be entitled "Recherches sur le continu et sur sa puissance" ("Investigations on the Continuum and Its Power"), would contain the proofs omitted from his 1900 paper, and this proof in particular. However, this promised article never appeared.

Bernstein, in the last section of his [1901] dissertation, constructed what he called an "ultracontinuum". This was an ordered set M whose power was $2^{\aleph_0}$ but which had an order-type different from that of the real numbers in their usual order. The set M consisted of all the functions from ω to ω_1, ordered lexicographically: A function f was less than g in this ordering if, for the least n at which f and g disagree, $f(n) < g(n)$. It was to deal with the cardinality of this set that he introduced the proposition (2) below, which would cause so much trouble at the Heidelberg Congress of Mathematicians in 1904.

At the end of his dissertation Bernstein conjectured that $2^{\aleph_0} < 2^{\aleph_1}$ and remarked that, from his other results and this conjecture, it would follow that the cardinality of more subsets of $\mathbb{R}$ would be known than were known at present [1901, p. 54]. Bernstein's conjecture (though without recognition of his formulating it) would eventually be used, especially in topology, as a weaker assumption than CH.

One of the few mathematicians to be directly influenced by Bernstein's dissertation was Hausdorff, whose earlier research had been largely in mathematical astronomy and optics. The year 1901 saw Hausdorff's mathematical interests turn strongly toward set theory. In that year he gave his first course on set theory, and also published his first article on set theory. It is not known why this change in his mathematical interests occurred, except that it was part of his general shift from applied mathematics to pure mathematics. The change does not seem to have caused any difficulties for him at his university. His thesis supervisor, although an astronomer, had exchanged letters about set theory with Cantor in 1892. One such letter concerned the Continuum Hypothesis, a subject which Hausdorff himself would soon pursue [Moore 1989, p. 97].

In December 1901 there appeared Hausdorff's first research contribution to set theory, "On a Certain Kind of Ordered Set", stimulated primarily by Bernstein's dissertation. In his article Hausdorff rightly remarked that, despite Cantor's pioneering work, almost nothing was known about order-types in general; only for those special order-types that were ordinal numbers was the possible behavior known. Nevertheless, Hausdorff added, the classification of order-types belonged to the inner circle of set-theoretic problems, since a theorem by Cantor and Bernstein (a theorem that Hausdorff himself had found independently, a fact which he did not mention in his article) shows that the class W of all denumerable order-types has the power $\aleph$ of $\mathbb{R}$, and so connects $\aleph$ with $\aleph_1$, the power of the class C of all countable ordinals. That is, $\aleph \geq \aleph_1$.[8] From this it followed, Hausdorff observed, that

> for the comparison between $\aleph$ and $\aleph_1$ it may be of importance to establish intermediate classes [between C and W] .... Just so, Cantor's hypothesis ["die Cantor'sche Vermuthung"] that $\aleph = \aleph_1$ promises to make its proof easier by the interpolation of intermediate classes. Conversely, in case it turns out that $\aleph > \aleph_1$, this way may be more roundabout. [1901, p. 460]

Here too is evidence that the name "Continuum Hypothesis" had not yet been generally adopted.

The intermediate class that Hausdorff chose to study was what he called the layered (*gestuft*) order-types. An ordered set was said to be 'layered' if no two of its initial segments were order-isomorphic. Thus, in particular, every well-ordered set was layered, and every ordinal was a layered order-type, but there were layered order-types that were not ordinals. He went on to show that the class of denumerable layered order-types has the same power as the set of all denumerable order-types, i.e., the power of the continuum.

As it turned out, the layered order-types did not hold the key to Cantor's Continuum Hypothesis. But Hausdorff would keep trying to find that key.

§4. The "refutation" of the Continuum Hypothesis and its aftermath. One cardinality result in Bernstein's dissertation did not attract particular interest when it appeared, but became of fundamental importance three years later. This was his purported theorem [1901, p. 49] that, for every ordinal α and μ,

$$\aleph_\mu^{\aleph_\alpha} = 2^{\aleph_\alpha} \cdot \aleph_\mu. \tag{2}$$

For at the International Congress of Mathematicians held at Heidelberg in August 1904, the Hungarian mathematician Julius König offered a "proof" that CH is false since the power of the continuum is not an aleph. Consequently, he added, Cantor's claim that every set can be well-ordered is false since $\mathbb{R}$ cannot be well-ordered. König's argument depended on the assumption that the power of the continuum is an aleph, and so did not affect the Weak Continuum Hypothesis one way or another.

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[8] Hausdorff appears to have originated the convention of taking the cardinal of $\mathbb{R}$ to be an aleph without subscript, and justified this by stating that "G. Cantor thinks of publishing shortly a proof that every transfinite cardinal number must occur in the 'sequence of alephs' " [1901, p. 460].

What Cantor, who was present at the Congress, undoubtedly found devastating about König's argument was that it was based on Cantorian tools. It was not simply another argument put forward by the opponents of set theory. The crux of König's argument was the proposition that the sum of a countable increasing sequence of infinite cardinals is less than the same sum raised to the power $\aleph_0$. This proposition was correct, and the error in his argument lay elsewhere, namely in Bernstein's proposition (2) when $\alpha = 0$ and $\mu = \omega$:

$$\aleph_\omega^{\aleph_0} = 2^{\aleph_0} \cdot \aleph_\omega.$$

Gerhard Kowalewski claimed, decades later [1950, p. 202], that Zermelo had found the error the next day, but this claim is not accurate. What happened was much more complex. H. Fehr published a summary of the talk in his report on the Congress in the journal ***L'enseignement mathématique***:

> Monsieur König (Budapest) made a communication which was one of the most important. Its aim was *to show that the continuum cannot be conceived as a well-ordered set.* Because of its great interest, it attracted a large audience.
>
> The subject was a claim made by Monsieur G. Cantor and mentioned by Monsieur Hilbert in his "Mathematical Problems" at the Paris Congress in 1900. Monsieur König showed that this claim, according to which the continuum can be put in the form of a well-ordered set, cannot be accepted. He gave an indirect proof, which assumed that the continuum is equivalent to a well-ordered set and gave this hypothesis in the form of an equation. Using properties of set theory, notably a theorem established by Monsieur Bernstein, Monsieur König deduced a contradiction. His communication was followed by a discussion in which Messieurs G. Cantor, Hilbert, and Schoenflies took part. Only simple remarks were made, and no objection was raised to the argument. However, Monsieur Cantor reserved the right to a deeper examination of the problem. [1904, p. 385]

In the aftermath of the Heidelberg Congress, Hausdorff met Cantor, Hilbert, and Schoenflies at Wengen in the Swiss Alps, where they discussed König's purported proof and Bernstein's ill-fated proposition (2) [Schoenflies 1922, pp. 100–101]. On 29 September 1904, shortly after Hausdorff returned home to Leipzig, he wrote to Hilbert: "After the Continuum Problem had plagued me at Wengen almost like a monomania, naturally I looked first here at Bernstein's dissertation." The error in the proof of (2) lay where it was suspected—in Bernstein's claim that if $\alpha < \beta$ then every subset A of power $\aleph_\alpha$ taken from a set B of power $\aleph_\beta$ lies in an initial segment of B. This was false, Hausdorff observed, when A was of power $\aleph_0$ and B was of power $\aleph_\omega$. (The notion waiting to be discovered here was that of *cofinality*, which Hausdorff would formulate and make central to his researches on order-types two years later [1906].) Hausdorff was inclined, in the letter to Hilbert,

> to regard König's proof as false and König's proposition as the height of improbability. On the other hand, you will scarcely have received the impression that Cantor has found, during the last weeks, what he has sought in vain for 30 years. And so your Problem No. 1 appears, after the Heidelberg Congress, to stand precisely where you left it at the Paris Congress.
>
> But perhaps, as I write this, one of the parties to the dispute is already in possession of the truth. I am very anxious to see the printed proceedings of the [Heidelberg] Congress.

Some three weeks before Hausdorff sent this letter to Hilbert, König had himself written to Hilbert. From König's letter of 7 September, it is clear that König had promised to publish an article based on his lecture at the Heidelberg Congress in Hilbert's journal ***Mathematische Annalen***:

> After a series of unhappy days, I must finally report to you that I *cannot* send you the article promised for the ***Annalen***. The proof of Bernstein's theorem is false. The inductive inference fails for $\aleph_\omega$. And I find it incomprehensible how I could not see this earlier!
>
> Consequently the assertion that Bernstein's theorem is false is identical with the assertion that the continuum can be put in one-one correspondence with a well-ordered set.
>
> How I regret what happened, and how I suffer from it, I will not describe to you any further.

König was mistaken in thinking that the negation of Bernstein's theorem is equivalent to the real numbers being well-orderable. And König did not see right away that he had found an important theorem hidden in his false proof.

A few days later on 19 September, Hilbert wrote from Göttingen to his colleague Felix Klein (they ran mathematics at Göttingen University between them) about what had happened during the recent vacation:

> Our trip gave us much relaxation and enjoyment. We encountered many mathematicians: Cantor, Schoenflies, Hensel, Hausdorff, ... etc. [at Wengen.] The most interesting, however, is that Bernstein's theorem on which König based his sensational lecture was abandoned as false. This is a great triumph for Cantor, who spoke in Wengen of nothing else and to whom I first communicated this [abandonment] from here [Göttingen]. [Hilbert in Frei 1985, p. 132]

Zermelo also described what happened in a post card to Max Dehn on 27 October 1904:

> And you still know nothing about the fate of König's lecture? What an innocent soul! Nothing else was discussed during the entire

> vacation. Solemnly, to both Hilbert and me, König retracted his Heidelberg proof, and so did Bernstein for his proposition about exponentiation [of cardinals]. König can be happy that the library at Heidelberg had closed so early [for the vacation]. Otherwise he could have embarrassed himself on the spot. So I had to wait until my return home to Göttingen to check, and then it was immediately obvious.[9]

The error was definitely in the proof of Bernstein's theorem for the case used by König. Zermelo had not yet understood at this stage, any more than König himself, that there was an important result about cardinality hidden in König's argument, a result obscured at first by Bernstein's theorem.

Meanwhile Zermelo, stimulated by the Heidelberg controversy, discovered his proof (based on his Axiom of Choice) that every set can be well-ordered, and submitted it to ***Mathematische Annalen*** in the form of a letter to Hilbert, dated 24 September [1904]. An intense controversy resulted, which resulted in quite a few articles in the ***Annalen*** in 1905.[10]

One of those involved in that controversy in the ***Annalen*** was Emile Borel. On 29 November 1904 Otto Blumenthal, acting in the name of the ***Annalen***'s editorial board and of Hilbert in particular, asked Borel for a note about his remarks to Blumenthal criticizing Zermelo's proof. Borel wrote the note very quickly, apparently accepting König's original "proof" as correct,[11] and Blumenthal replied in turn on 3 December:

> But permit me a remark relative to your note's final lines, which have already been falsified by history.
>
> It appears that you do not yet know the strange fate of the renowned lecture of Monsieur König, which, in fact, was the original reason for Zermelo's proof. Shortly after the Congress several mathematicians realized at the same time that Monsieur König's proof was false. First of all, Monsieur König himself, trying to edit for ***Mathematische Annalen*** his proof of Monsieur Bernstein's theorem, saw with horror that it was not valid. He wrote about it to Monsieur Hilbert, using the term "the catastrophe of the Congress". Then, independently of each other, Messieurs Cantor, Bernstein, and Zermelo.[12]

Blumenthal was apparently unaware of Hausdorff's letter to Hilbert which showed that Hausdorff's name needed to be added to the list of König, Cantor, Bernstein, and Zermelo.

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[9] Zermelo in [Ebbinghaus 2007, p. 431].

[10] On Zermelo's proof and the resulting controversy, see [Moore 1982].

[11] That both Borel, who had been at Heidelberg, and his correspondent Henri Lebesgue at first regarded König's proof as correct is clear from Lebesgue's letter of 17 August 1904 to Borel, which contains the phrase "the correct proof of König" [Lebesgue 1991, p. 65].

[12] Blumenthal in [Dugac 1989, p. 74].

Meanwhile, near the end of 1904, Hausdorff published in the in the "Sprechsaal" (i.e., letters to the editor) of the ***Jahresbericht*** of the German Mathematical Union the following comment on Bernstein's purported theorem:

> If $\aleph_\mu$ is an aleph which has an immediate predecessor $\aleph_{\mu-1}$, then the recursive formula
>
> $$\aleph_\mu^{\aleph_\alpha} = \aleph_\mu \cdot \aleph_{\mu-1}^{\aleph_\alpha}$$
>
> is valid .... This recursion principle ... *fails, however, for those* $\aleph_\mu$ *which do not have an immediate predecessor* (μ a limit ordinal) .... So, for example, $\aleph_\omega^{\aleph_0}$ cannot, at least in this way, be expressed through $\aleph_\nu^{\aleph_0}$ (ν finite) since the denumerable subsets of $\aleph_\omega$ can be ordinal numbers from every number-class of finite order. The unrestricted recursion formula
>
> $$\aleph_\mu^{\aleph_\alpha} = \aleph_\mu \cdot 2^{\aleph_\alpha}$$
>
> of Herr F. Bernstein [1901, p. 50] is therefore, for the time being, to be regarded as unproved. Its validity appears so much the more problematical since from it, as Herr J. König has shown, *would follow the paradoxical result that the power of the continuum is not an aleph and that there are cardinal numbers greater than every aleph.* [1904, pp. 570–571]

Replying to Hausdorff three issues later in the same "Sprechsaal", Bernstein wrote about his "theorem" (2): "After Herr König contributed his interesting research, as yet unpublished, it was noticed quite soon by various people (in particular by Herr König himself) that the proof of equation (2) is incomplete ... " [Bernstein 1905, p. 199]. It should be stressed that Bernstein wrote then only of the proof as being incomplete, not of the theorem as wrong. He added that the theorem was correct for alephs of finite index, which were all that he needed and used in his dissertation. When he republished his dissertation in ***Mathematische Annalen*** later in 1905, he restricted his proof for (2) to alephs of finite index, and finally accepted that it was not true in general: "On the contrary, Theorem 1 is not, as I had originally thought, provable for every aleph" [1905a, p. 151].

König's original argument as given at Heidelberg no longer exists. What survives is the version that he published in 1905, after he no longer accepted the general form (2) of Bernstein's theorem. But the basic structure of the published version of König's argument must have agreed with what he put forward at Heidelberg, and is as follows. Given an infinite sequence M_1, M_2, M_3, ... of disjoint sets with corresponding cardinals $\mathfrak{m}_1, \mathfrak{m}_2, \mathfrak{m}_3, \dots$, he considered the cardinal $\mathfrak{s}$ of the union of the M_n as well as the cardinal $\mathfrak{p}$ of the "product" of the M_n (i.e., the set of infinite sequences $(a_1, a_2, a_3, \dots)$ where a_n belongs to M_n for all n). He then required that all the M_n be infinite and concluded that

$$\mathfrak{s} \le \mathfrak{p} \le \mathfrak{s}^{\aleph_0}$$

From this result he deduced that $\mathfrak{p}^{\aleph_0} = \mathfrak{s}^{\aleph_0}$. If for all n he had $\mathfrak{m}_n < \mathfrak{m}_{n+1}$, then $\mathfrak{p} > \mathfrak{s}$ and also $\mathfrak{p}^{\aleph_0} > \mathfrak{s}$; so, for any ordinal μ, letting $\mathfrak{m}_n = \aleph_{m+n}$ would give $\mathfrak{s}^{\aleph_0} > \mathfrak{s}$. If $2^{\aleph_0}$ equals $\aleph_{\mu+\omega}$ for some ordinal μ, then letting $\mathfrak{m}_n = \aleph_{\mu+n}$ would contradict the fact that $(2^{\aleph_0})^{\aleph_0} = 2^{\aleph_0}$. He concluded that $2^{\aleph_0}$ could not equal $\aleph_{\mu+\omega}$ for any ordinal μ.

If Bernstein's theorem (2) was true, he observed, then the stronger conclusion would follow that the real numbers cannot be well-ordered. "Unfortunately", he added, "the proof [of (2)] has an essential gap since for $\aleph_\omega$ and each of the 'singular' well-ordered sets considered above, the assumption that each countable subset lies in an *initial segment* of the whole set is no longer permissible" [1905, p. 180]. His final comment was that

(3) $2^{\aleph_0}$ occurs in the countable sequence $\aleph_1, \aleph_2, \aleph_3, \ldots$ or not according to whether ${\aleph_\omega}^{\aleph_0}$ is greater than, or is equal to, $2^{\aleph_0}$.

König's reference to "singular" well-ordered sets would, no doubt, influence Hausdorff's choice of terminology when he isolated the concept of cofinality in 1906 and then of singular and regular cardinal in 1907 (see §6 below).

Surprisingly, although Hausdorff was already on the verge of the concept of cofinality in [1904], had clearly thought intensely about König's 1904 argument, and introduced cofinality in [1906], Hausdorff apparently never noticed—or at least never stated in print—that König's proof could be strengthened to show that

(4) $2^{\aleph_0}$ cannot equal $\aleph_\alpha$ for any ordinal α of cofinality ω.

This more general form of König's theorem was not published by anyone at the time. It appears to have been stated first by Adolf Lindenbaum and Alfred Tarski in [1926, p. 196] when they cited Arthur Schoenflies' and Hans Hahn's 1913 report on set theory, a report where, however, (4) was not mentioned. Rather, Schoenflies and Hahn [1913, p. 138] had used a (mistaken) generalization of König's theorem to conclude that

(5) For every limit ordinal β, $\aleph_\beta^{\aleph_0} > \aleph_\beta$.

Unfortunately, (5) is false whenever $\aleph_\beta$ is a strong limit cardinal. In particular, if $\aleph_\beta = \beth_{\omega_1}$, we get $\aleph_\beta^{\aleph_0} = \aleph_\beta$ according to Tarski [1925, p. 9], a fact which contradicts (5).

§5. Jourdain: The second appearance (and first published appearance) of the Generalized Continuum Hypothesis. In 1902 Philip Jourdain graduated in mathematics from Trinity College, Cambridge, and later published articles in analysis, set theory, and the history of mathematics. Already the previous year he had begun to correspond with Georg Cantor [Grattan-Guinness

1971]. Jourdain was one of those who attended Bertrand Russell's Cambridge lectures on mathematical logic during 1901–1902, and soon afterward there began a correspondence between them [Grattan-Guinness 1977].

On 7 November 1903 Jourdain wrote to Cantor that he had proved several theorems about transfinite cardinals but that "the equality $2^{\aleph_0} = \aleph_1$, however, appears to me hopeless to prove."[13]

What happened in August 1904 at the Heidelberg Congress would become a topic of interest in the correspondence between Jourdain and Russell. Writing to Russell on 5 September from Geneva, Louis Couturat brought striking news:

> I learn from Monsieur Fehr that at the congress of mathematicians at Heidelberg a Monsieur König proved (against G. Cantor) that the continuum is not a well-ordered set. Amazing!! No one found an error in the proof, and G. Cantor himself asked for time for reflection. [Schmid 2001, p. 437]

Russell informed Jourdain of this on 12 October. And on 18 December, Couturat commented further:

> I am glad to be able to give you important news (which I owe to my friend Borel): Monsieur König was mistaken in his refutation of G. Cantor's [well-ordering] theorem, and recognized it himself. On the other hand Monsieur Zermelo is publishing in ***Mathematische Annalen*** a proof of this same theorem. But since the proof is based on the calculus of "alephs", some mathematicians do not credit it with more value than Monsieur König's refutation. [Schmid 2001, p. 455]

Couturat was curious to know what Russell thought of Zermelo's proof. On 5 February 1905 Russell wrote to Couturat that he had not yet seen the proof but that Whitehead, who had seen it, said that it postulated the existence (i.e., non-emptiness) of a class of relations that was empty in the doubtful cases [Schmid 2001, p. 471]. Russell was soon skeptical of Zermelo's Axiom of Choice. Some months before the Heidelberg Congress, Jourdain had published an argument purporting to prove that every set can be well-ordered [1904].

On 15 June 1905 Russell wrote to Jourdain that König "*does* prove $2^{\aleph_0} \neq \aleph_1$, so that Cantor's expectation was in any case mistaken" [Grattan-Guinness 1977, p. 53]. It is not at all clear why Russell made this mistake.

Meanwhile, in an article published in January 1905, Jourdain gave his first statement of the Generalized Continuum Hypothesis when he raised "the question of the equality

$$2^{\aleph_\alpha} = \aleph_{\alpha+1}$$

[13] Jourdain in [Grattan-Guinness 1977], p. 117.

where α is any ordinal number" [1905, p. 42]. This was the first time that GCH was stated with all ordinals as indices of alephs, in contrast to what Peirce had done for finite ordinals. And since what Peirce did was unpublished, this was also the first published version of GCH.

In this article Jourdain was concerned with cardinal numbers of the form $\mathfrak{a}^{\mathfrak{b}}$ where $\mathfrak{b}$ is infinite and $\mathfrak{a} > 1$. He wished to determine necessary and sufficient conditions such that $\mathfrak{a}^{\mathfrak{b}} = \mathfrak{a}$. Unfortunately, what he did was less useful because it depended on Bernstein's dubious "theorem" (2) that

$$\aleph_\alpha^{\aleph_\beta} = 2^{\aleph_\beta} \cdot \aleph_\alpha.$$

If $\alpha \leq \beta$, he noted, then (2) easily followed from $\aleph_\alpha^{\aleph_\beta} = 2^{\aleph_\beta}$. But he gave a fallacious argument for (2) when $\beta > \alpha$ since he was not yet aware that (2) was false in general [1905, p. 49]. By using (2), Jourdain was led to believe (apropos of finding conditions for $\mathfrak{a}^{\mathfrak{b}} = \mathfrak{a}$ to hold) the mistaken proposition that $\aleph_\alpha^{\aleph_\beta} = \aleph_\alpha$ if and only if $2^{\aleph_\beta} \leq \aleph_\alpha$. Then he argued [1905, p. 50] that to have $\aleph_\alpha^{\aleph_\beta} = \aleph_\alpha$ when $\aleph_\beta < \aleph_\alpha$, it is necessary and sufficient that

$$2^{\aleph_\beta} = \aleph_{\beta+1}.$$

(But this is wrong for $\alpha = \omega$ and $\beta = 0$ since, by König's result, $\aleph_\omega^{\aleph_0} \neq \aleph_\omega$ regardless of whether $2^{\aleph_0} = \aleph_1$.) Jourdain's article, whether because of its mistaken argument or, more likely, because German mathematicians did not generally read the ***Philosophical Magazine*** where it appeared, seems never to have been cited.

Influenced by earlier articles in the same journal by Jourdain, including his [1904], A. E. Harward published there in October 1905 an article on cardinal arithmetic. Harward, an amateur British mathematician serving in the Indian Civil Service in Calcutta, had sent his article to Jourdain before submitting it for publication and benefited from comments by Jourdain. Although Harward's article is of interest for various statements in it that amount to most of Zermelo's later axioms, together with a version of the Axiom of Replacement,[14] it is relevant in the present context because it also gives a statement which is in essence the Generalized Continuum Hypothesis. At the end of his article Harward wrote:

> I think it probable that
>
> $$\begin{aligned} \aleph_\beta^{\aleph_\gamma} &= \aleph_\beta & (\gamma < \beta) \\ \text{and} \quad &= \aleph_{\gamma+1} & (\gamma \geq \beta), \end{aligned}$$
>
> but I cannot prove it at present. [1905, p. 455]

His hypothesis—that, for any ordinals β and γ, $\aleph_\beta^{\aleph_\gamma} = \aleph_\beta$ if $\gamma < \beta$—is false since it contradicts König's result that $\aleph_\omega^{\aleph_0} \neq \aleph_\omega$. But Harward's hypothesis

[14] See [Moore 1976].

that

$$\aleph_\beta^{\aleph_\gamma} = \aleph_{\gamma+1} \text{ if } \gamma \geq \beta$$

is in effect GCH.

This was the second time that GCH was published. Since Jourdain had maintained a strict neutrality toward the truth of falsity of GCH when he introduced it, Harward's article was the first time that GCH was conjectured as probably true. Like Jourdain's article [1905], Harward's was apparently never cited by other mathematicians, and his statement of GCH was never noticed.

In a 1908 article on infinite sums and products of cardinals, Jourdain proved a generalization of a form of König's theorem. König had shown that if $\mathfrak{m}_n < \mathfrak{m}_{n+1}$ for every finite n and for given cardinals $\mathfrak{m}_n$, then $\prod_n \mathfrak{m}_n > \sum_n \mathfrak{m}_n$. Jourdain extended this theorem by letting the subscripts vary over any given infinite initial ordinal γ and assuming only that for each $\alpha < \gamma$ there is some $\beta < \alpha$ with $\mathfrak{m}_\alpha < \mathfrak{m}_\beta$. From this assumption he concluded that

$$\prod_{\alpha<\gamma} \mathfrak{m}_\alpha > \sum_{\alpha<\gamma} \mathfrak{m}_\alpha .$$

It then followed in particular that if γ is a limit ordinal such that $\omega_\beta \leq \gamma < \omega_{\beta+1}$, then $\aleph_{\alpha+\gamma}^{\aleph_\beta} > \aleph_{\alpha+\gamma}$. Consequently, he concluded [1908, p. 383],

$$2^{\aleph_\beta} \neq \aleph_{\alpha+\gamma}$$

for any α and any limit ordinal γ such that $\omega_\beta \leq \gamma < \omega_{\beta+1}$. This was a significant generalization of König's 1905 result that $2^{\aleph_0}$ cannot equal $\aleph_{\alpha+\omega}$ for any ordinal α.

Zermelo [1908, p. 277] had independently given a similar generalization of König's result, namely: If $\mathfrak{m}_x < \mathfrak{n}_x$ for every x of some set A, then

$$\sum_{x\in A} \mathfrak{m}_x < \prod_{x\in A} \mathfrak{n}_x .$$

However, Zermelo did not draw any conclusions about cardinal exponentiation, such as Jourdain had done. Neither of them made any use of Hausdorff's new notion of cofinality.

§6. Hausdorff and the third published appearance of the Generalized Continuum Hypothesis. When Hausdorff published his 1904 article on set theory, only the final section was concerned with cardinals. The remainder was devoted to extending the concept of exponentiation from ordinals to arbitrary order-types. Cantor had defined the exponentiation of an ordinal by an ordinal, and Hausdorff succeeded in extending this to the exponentiation of an order-type by an ordinal or by the inverse of an ordinal. To do this, Hausdorff took over from Bernstein the idea of ordering by what he called first differences [1906, p. 109], now called lexicographical ordering.

In 1906 Hausdorff continued his work of 1904 by publishing the first of a series of lengthy articles deepening what was known about order-types in general. The basic idea was to use ordinals to gain insight into order-types. For this purpose he considered an arbitrary set M ordered by some relation $<$ and an arbitrary subset A. He then introduced a concept that turned out to be fundamental for later work on transfinite cardinals: A was said to be *cofinal* with M if no x in M follows all members of A. Dually, he defined the notion that A is *coinitial* with M. An element x of M was said to be a ω_n-limit of an increasing sequence of length ω_n (for n finite) if x is the least upper bound of the sequence; dually an element x of M was defined to be an ω_n^*-limit of a decreasing sequence. Putting these two ideas together for finite m and n gave the notion that an element x is an $\omega_m\omega_n^*$-limit. If M does not have an $\omega_m\omega_n^*$-limit, it is said to have an $\omega_m\omega_n^*$-gap [1906, p. 124, p. 148]. He next extended concepts that Cantor had introduced for real numbers: The ordered set M was said to be *closed* if every monotonic sequence has a limit, to be *dense-in-itself* if every element of M is a limit of some monotonic sequence, and to be *perfect* if both closed and dense-in-itself [1906, p. 125].

Then Hausdorff turned to classifying the simplest order-types which Cantor had not treated, i.e., those of power $\aleph_1$. He began by considering what he called homogeneous order-types, i.e., those all of whose non-empty open intervals were order-isomorphic. By using his generalized exponentiation of order-types, he constructed $\aleph_1$ different order-types, all of which were very much like the order-type of the closed interval $[0, 1]$ of real numbers; all of these order-types had the power $2^{\aleph_0}$, were perfect, coinitial with ω^*, cofinal with ω, complete in Dedekind's sense, equal to their inverse order-type, and (if the endpoints were omitted) homogeneous [1906, p. 143].

Hausdorff classified the homogeneous order-types of power ω_1 into 50 species of order-types, according to their gaps, limits, cofinality, and coinitiality. He asked which of those 50 species could actually be shown to exist:

> A definitive answer to this question cannot be given so long as the question of the power of the continuum has not been cleared up. (Otherwise there would originate right here a new way to settle the Continuum Problem ["Kontinuumfrage"] after so many other ways have already been tried in vain). For simple inspection shows that ... *each dense order-type without $\omega\omega^*$-gaps includes the usual continuum as a subset.* Hence, so long as no one has succeeded in constructing a dense order-type of the second power without $\omega\omega^*$-gaps (and thereby verifying the Cantorian hypothesis $2^{\aleph_0} = \aleph_1$), the question of the existence of homogeneous order-types of the second power must be restricted to just the 32 species with $\omega\omega^*$-gaps. [1906, p. 156]

In 1907 Hausdorff continued this series of papers by posing the following problem: Which of the fifty species of homogeneous order-types discussed above were found among sets of the power of the continuum $2^{\aleph_0}$? Here too the Continuum Problem was intimately involved. Five of these species could only have instances of the power of the continuum if the Continuum Hypothesis was false. This was due to the following theorem of Hausdorff [1907, p. 85]:

> A dense order-type with no $\omega\omega^*$-limits or $\omega\omega^*$-gaps has ω_1-sequences and ω_1^*-sequences; if it has no $\omega_1\omega_1^*$-gaps, then its power is greater than $\aleph_1$.

For the remaining forty-five species, he was able to construct examples of power $2^{\aleph_0}$ by using his general notion of exponentiation of order-types.

Among Hausdorff's major discoveries in his 1907 paper were the η_α-types, which in turn led him to formulate the Generalized Continuum Hypothesis. The η_α-types generalized the order-type η of the rational numbers to higher cardinalities. But the roots of Hausdorff's interest in these order-types came from a 19th-century rival and opponent of Cantor, the German analyst Paul du Bois-Reymond, whose work was on the border between set theory and traditional analysis and concerned an unusual ordering of real functions. (Already in his 1904 paper Hausdorff had referred to du Bois-Reymond's contribution.)

In 1871 du Bois-Reymond introduced his *infinitary calculus* as a way of comparing the growth of two real functions $f(x)$ and $g(x)$, where the functions both approached 0 or ∞ as $x \to \infty$. He defined a relation $<$ on such functions as follows: $f > g$ if $\lim f(x)/g(x) = \infty$. He described this situation by saying that f is "infinitely greater" than g. If $\lim f(x)/g(x) = 0$, then he said that $f < g$, i.e., f is "infinitely smaller" than g. And if $\lim f(x)/g(x)$ was finite but non-zero, then he said that $f(x)$ is "infinitarily equal" to $g(x)$, i.e., $f = g$.

Next he introduced what he called a "sequence" of functions ordered by his relation $<$. It was not completely unambiguous which functions were in the sequence and which were not, but he certainly included those functions built up from powers, roots, exponentials, and natural logarithms; he definitely excluded functions with infinitely many extrema or inflection points, and he wished the functions to be differentiable in general [1871, p. 345]. Any function in this sequence was infinitely greater than any preceding function, and in the representation below there were, in fact, infinitely many functions of the sequence in each place indicated, since the constant a could take any positive value:

$$\ldots, e^{-ae^x},\ e^{-ax},\ x^{-a},\ (\ln x)^{-a},\ \text{constants},\ (\ln x)^a,\ x^a,\ e^{ax},\ e^{e^{ax}}, \ldots$$

Du Bois-Reymond saw this sequence as analogous to the linear ordering of real numbers, and noted in particular that both were dense in their respective orderings. Later he would realize that the similarity was not as strong as he thought.

In his 1882 book on the theory of functions, du Bois-Reymond developed his infinitary calculus for the set of all unbounded increasing real functions f, a set which he called the "infinitary pantachie" (he no longer required the functions to be differentiable). He argued that this set is ordered by his relation $<$, provided that we identify all functions that are infinitarily equal, and is "a completely definite collection" [1882, p. 282]. In contrast to his view in 1871, when he emphasized the similarity between the set of all real numbers and the 1871 version of the infinitary pantachie, he now stressed how they were different. He believed it would be difficult to compare the cardinality of these two sets. One important difference between them, he noted, was the following. The set of real numbers is such that any bounded increasing sequence has a least upper bound. But this was not true of the infinitary pantachie, where an increasing sequence of functions always has an upper bound but never has a least upper bound.

There was, however, a serious difficulty with du Bois-Reymond's infinitary pantachie which he did not recognize. It was not well defined. There were functions f and g in it that were not comparable, i.e., such that neither $f < g$, nor $g < f$, nor $f = g$. Earlier, he himself had given an example of two such incomparable functions [1871, p. 344]. But in his book he seemed to forget this fact when defining the infinitary pantachie. During the 1890s, the usual way around this difficulty was to consider a somewhat vague class of increasing functions, any two of which were assumed to be comparable; this was done, for example, by Borel [1898] and Schoenflies [1899]. Cantor utterly rejected the infinitary pantachie because of this lack of comparability [1895, p. 107].

The unsatisfactory situation surrounding du Bois-Reymond's infinitary pantachie was squarely faced by Hausdorff in 1907. Three years earlier, Hausdorff used his exponentiation of order-types to give an example of an order-type for which, as in the infinitary pantachie, increasing sequences did not have least upper bounds [1904, p. 570]. By contrast, in 1907 he stressed that the infinitary pantachie did not exist. The reason was that there were increasing functions which were not comparable, and he suspected that this fact had not escaped du Bois-Reymond. The latter's work, Hausdorff observed, generally considered only restricted sets of pairwise comparable functions and most of the time only discussed sets that were countable or only contained countable sequences of functions, such as those generated by finite combinations of x^a, e^{ax}, and $\log x$.

Hausdorff, while not wishing to underestimate the interest of such problems for analysis, stressed that his focus was on set theory. From a set-theoretic standpoint, such special classes of functions were of secondary importance. What was essential was to investigate a set of pairwise compatible functions that was as inclusive as possible and to link that set with the Cantorian theory of order-types. Since there existed no infinitary pantachie, he took over du Bois-Reymond's term and gave it a different meaning: For Hausdorff, a "pantachie" was defined to be a maximal set of increasing real functions ordered by $<$, and a "pantachie-type" was the order-type of a pantachie [1907, pp. 109–110].

The first question that Hausdorff examined was the cardinality of any pantachie-type. He established that, contrary to the expectations of du Bois-Reymond, it was the same as the cardinality of the real numbers. (This was a consequence of Hausdorff's proof that there are only $2^{\aleph_0}$ non-decreasing real functions.)

Next, Hausdorff considered sequences of real numbers instead of functions, and defined $<$ and $=$ in the analogous way for such sequences. If a set of functions was a pantachie, so was the corresponding set of sequences.

Finally, he made a major change in the infinitary ordering of functions, which used $f(x)/g(x)$ or, equivalently, $\log f(x) - \log g(x)$. Hausdorff opted to consider, rather than $\lim f(x)/g(x)$, the eventual sign of $f(x) - g(x)$. In this way Hausdorff introduced the relation $<$ of "final rank-ordering". That is, $f < g$, $f = g$, or $f > g$ respectively in the final rank-ordering if, there is some y such that for all x greater than y, $f(x) < g(x)$, $f(x) = g(x)$, or $f(x) > g(x)$ respectively. This definition was then restricted to apply to sequences rather than real functions. He now used "pantachie" in analogy to what he had done before, viz. as a maximal set of sequences (of real numbers) ordered by $<$; but henceforth $<$ was the relation of final rank-ordering [1907, pp. 114–117].

By relying on tools developed in his earlier work on order-types, Hausdorff determined common properties of all pantachie-types of sequences. Along the way he rephrased the results on pantachies—of du Bois-Reymond, Hadamard, and Borel—in terms of order-types. Thus du Bois-Reymond's Theorem (any increasing sequence of functions ordered by his infinitary ordering $<$ has an upper bound) became, for Hausdorff, the statement that a pantachie-type is not cofinal with ω. Hausdorff defined a new variety of order-type, the H-types, which have what he regarded as the essential properties of pantachies. An order-type was said to be an "H-type" if it is dense, has no first or last element, is not cofinal with ω or coinitial with ω^*, has no ω-limit or ω^*-limit, and has no $\omega\omega^*$-gaps. In particular, any pantachie-type is an H-type [1907, pp. 120–121].

Then Hausdorff explored the H-types from a different direction. He defined a more general class, the η-types, to be those dense order-types without first or last element. The η-types were named after the simplest such type, the order-type η of the rational numbers. Every H-type was an η-type, and he used this fact to give a different description of the H-types. If for sets A and B of elements in an order-type, we say that $A < B$ if every element of A is less than every element of B, then η-types could be characterized as follows:

(6) An order-type α is an η-type if whenever $A < B$ for finite subsets of α, there are elements x, y, z in α such that $x < A$, $A < y < B$, and $B < z$.

If this condition was strengthened to require that A and B be of power less than $\aleph_1$, then the order-type was said to be an η_1-type. He then established that an order-type α is an η_1-type if and only if it is an H-type [1907, p. 127].

Moreover, he showed that the η_1-types generalize the order-type η of the rational numbers in two ways: He had found that the type of the rational numbers is "universal" for order-types of power $\aleph_0$. That is, any denumerable order-type can be embedded in the type of the rational numbers. Now he succeeded in proving that any η_1-type is universal for order-types of power $\aleph_1$. On the other hand, the η_1-types yielded a proposition equivalent to the Continuum Hypothesis:

(7) There exists an η_1-type of power $\aleph_1$.

Moreover, CH implied that if there is an η_1-type of power $\aleph_1$, then it is unique.

Hausdorff saw how to generalize his η_1-types from an index 1 to any finite index m by a simple change in his definition of η_1-types; namely, A and B in (6) were required to each be of power less than $\aleph_m$. In this way, he obtained the η_m-types. He then obtained the following theorems: Each order-type of power $\aleph_m$ is isomorphic to some subset of any given η_m-type; if there is an η_m-type of power $\aleph_m$, there is only one; any η_m-type has at least the power $2^{\aleph_{m-1}}$; and any η_m-type without $\omega_m\omega_m^*$-gaps has at least the power $2^{\aleph_m}$ [1907, p. 133]. These results led him to generalize the Continuum Hypothesis: "If Cantor's hypothesis $2^{\aleph_0} = \aleph_1$ holds in an extended form, so that for each finite v we have $2^{\aleph_v} = \aleph_{v+1}$, then there is one and only one η_v-type of power $\aleph_v$, namely η_v itself" [1907, p. 133]. That is, Hausdorff hypothesized that

(8) For every finite m, $2^{\aleph_m} = \aleph_{m+1}$.[15]

But, in contrast to Cantor, Hausdorff did not assert that the Continuum Hypothesis is true, much less his generalization of that hypothesis to all finite levels.

Thus Hausdorff's first generalization of the Continuum Hypothesis grew out of his work on order-types, on pantachie-types, and especially on η_m-types where m is finite. Consequently, his first generalization did not reach as far as $\aleph_\omega$. But that would change the following year.

Meanwhile, in a second 1907 article, he introduced for infinite cardinals the notion of singular cardinal (i.e., one cofinal with a smaller cardinal) and

[15]Neither here nor at any other time was Hausdorff aware that Jourdain in 1905 had already formulated the Generalized Continuum Hypothesis.

of regular cardinal (i.e., one not singular) [1907a, p. 542]. Any ordered set without a last element was then cofinal with a unique regular ordinal.

In 1908 Hausdorff published a lengthy article containing the deepest and most general results yet achieved on ordered sets. This led to the notion of η_σ-type for an arbitrary regular ordinal σ and to the following hypothesis which generalized (7) above:

(9) There exists an η_σ-type of power $\aleph_\sigma$.

If (9) is true for a given regular σ, he wrote, then

(10) the sum of all those powers $\aleph_\sigma$, where each is raised to a distinct cardinal exponent less than $\aleph_\sigma$, is $\aleph_\sigma$.

About (10), he stated: "We wish to name this *Cantor's Aleph Hypothesis* for $\aleph_\sigma$ since in the earliest doubtful case $\sigma = 1$ it turns into the Continuum Hypothesis $\aleph_1 = \aleph_1^{\aleph_0}$". He then added that if σ has an immediate predecessor $\sigma - 1$, then "the Aleph Hypothesis states that

$$\aleph_\sigma = \aleph_\sigma^{\aleph_{\sigma-1}} = 2^{\aleph_{\sigma-1}}.$$

It must at no time be forgotten that we are speaking here exclusively of *regular* $\aleph_\sigma$" He then pointed out that, by a generalization of König's [1905] formula, (10) is false if $\aleph_\sigma$ is singular [1908, p. 494].

In one sense, Hausdorff did not state the Generalized Continuum Hypothesis in 1908, for he was interested in (10), which he called Cantor's Aleph Hypothesis, only for regular $\aleph_\sigma$. In particular, he wanted to know for which σ this hypothesis is true. Yet in another sense, he did state the Generalized Continuum Hypothesis, for if in the quoted passage σ was replaced by $\sigma + 1$, which did have an immediate predecessor, then it states $\aleph_{\sigma+1} = 2^{\aleph_\sigma}$. For, as he had previously discovered, $\aleph_{\sigma+1}$ is necessarily regular by the Axiom of Choice.

When Hausdorff published this article in 1908, set theory had just been axiomatized by Zermelo. It would only become clear much later that Hausdorff had inadvertently bumped against the glass ceiling of independence results. In 1908 he had also defined the weakly inaccessible cardinals, the first of what were later called "large cardinals", although he mistakenly thought that these inaccessible cardinals would have no relevance to ordinary mathematics.

In sum, Hausdorff's results on order-types were a major advance, but without much effect on other researchers at the time. In several cases, Hausdorff's results were rediscovered decades later by others. Nevertheless, he had done what he set out to do: develop a genuine theory of order-types. The Generalized Continuum Hypothesis was only taken up in the 1920s by Tarski.

By 1912, Hausdorff himself was beginning to shift direction. He was about to formulate his concept of topological space, with the enormous resonance that this would have over the ensuing decades. Not until the 1930s, just as the Nazis' rise to power would force him out of university life, did Hausdorff return to order-types, since at that time he came to see quite clearly the intimate connections between order-types and topological spaces.

Meanwhile, in his famous 1914 book ***Grundzüge der Mengenlehre*** (*Fundamental Aspects of Set Theory*), Hausdorff posed as the Continuum Problem the question whether $2^{\aleph_0} = \aleph_1$ or $2^{\aleph_0} > \aleph_1$. After observing that there are infinitely many alephs $\aleph_\beta$ for which $\aleph_\beta^{\aleph_0} = \aleph_\beta$, he stated the Continuum Problem in an equivalent way as the question whether $\aleph_1^{\aleph_0} = \aleph_1$ or $\aleph_1^{\aleph_0} > \aleph_1$ [1914, pp. 127–128]. This time he stated GCH in the form $2^{\aleph_\sigma} = \aleph_{\sigma+1}$ for all ordinals σ, and again pointed out its equivalence to the existence of an $\eta_{\sigma+1}$-type of power $\aleph_{\sigma+1}$. However, he called the equation $2^{\aleph_\sigma} = \aleph_{\sigma+1}$ "thoroughly problematical" [1914, p. 182], stressing our ignorance about it even in the simplest case of $\sigma = 0$.

Instead Hausdorff largely restricted himself to the program begun by Cantor in 1884, showing that every closed subset of $\mathbb{R}$ which contains a condensation point has the power of the continuum, i.e., $2^{\aleph_0}$. (Cantor had not phrased it in this way, but rather in terms of two theorems: Any perfect subset of $\mathbb{R}$ has the power of the continuum; any uncountable closed set can be partitioned into a perfect set and a countable set, i.e., the Cantor–Bendixson Theorem.) But Hausdorff phrased matters, not in terms of $\mathbb{R}$, but of complete metric spaces. And so he mentioned William Young's result as stating that in a complete metric space any G_δ-set M (i.e., the intersection of countably many open sets) has at least the cardinality of the power of the continuum [1914, pp. 318–319]. Young, like Cantor, had exclusively concerned himself with $\mathbb{R}$, not metric spaces, and proved that such a set M includes a perfect subset and hence is exactly of the power of the continuum [1903]. Hausdorff in his book managed to extend Young's result a bit higher to $G_{\delta\sigma\delta\sigma}$-sets [1914, pp. 465–466], but the real breakthrough came two years later when Hausdorff [1916] (and, independently, Pavel Aleksandrov [1916] in Moscow) proved WCH for all Borel sets by showing that every uncountable Borel set includes a perfect subset. (There were bound to be limitations to how far this approach could be extended, since Bernstein in [1908] had already shown, in effect using the Axiom of Choice, that there is an uncountable set of real numbers without a perfect subset.) Hausdorff wrote about his proof of WCH for all Borel sets:

> For a very inclusive class of sets the [possible] cardinality is thus clarified. Of course, this can scarcely be conceived as a step toward the solution of the Continuum Problem since the Borel sets are very special and form only a vanishingly small subsystem (i.e., of the power of the continuum) of the system of all subsets [of $\mathbb{R}$].[16]

[16][Hausdorff 1916, p. 437]. Only in the 1960s was it realized that, in the absence of the Axiom of Choice, *every* subset of $\mathbb{R}$ can be a Borel set.

Aleksandrov's result was a sign that, as discussed in the next two sections, the center of gravity of set theory was shifting east from Germany and coming to reside in Poland and Russia.

§7. Luzin and Sierpiński: Research on the Continuum Hypothesis. Prior to 1914, mathematicians thought about CH, when they thought about it at all, from the standpoint of trying to prove it true or false. But in that year the Russian mathematician Nikolai Luzin, publishing in the ***Comptes Rendus*** of the Paris Academy of Sciences, took an entirely new approach. He treated CH *as a hypothesis* and asked what could be proved from it. And he did so in a very traditional part of mathematics: classical analysis.

In his doctoral dissertation of 1899 the French analyst René Baire had defined his hierarchy of real functions and shown that it classified real functions into ω_1 different classes. His class 0 consists of the continuous functions, and class 1 of the functions that are pointwise limits of sequences of continuous functions. More generally, class α consists of the functions that are pointwise limits of sequences of functions occurring in previous classes. (By cardinality considerations, if the Axiom of Choice is assumed, almost all real functions do not occur in his classification.) Baire had given a necessary criterion for a real function to occur in his classification and had asked if the criterion was also sufficient. His criterion was for the function to be pointwise discontinuous on every perfect set when one neglects sets that are of first category relative to that perfect set. What Luzin did in 1914 was to show that if CH is true, then this criterion is not sufficient.

More specifically, Luzin used CH to show that there exists what is now called a *Luzin set*, i.e., a set M of real numbers of power $2^{\aleph_0}$ such that the intersection of M with any perfect nowhere dense set is countable [1914]. The existence of a Luzin set then enabled him to prove his theorem about Baire's classification. Only several years later did Luzin succeed in proving, without assuming CH, that Baire's criterion was not sufficient [1921].

Meanwhile the First World War had taken place. The Polish mathematician Wacław Sierpiński, who had been a professor at the University of Lwow since 1910, was interned by the Russian authorities (Lwow then being a part of Russia) in Moscow. There he was welcomed cordially by Luzin and Egorov, who ensured that he had good conditions for doing research [Kuratowski 1974, p. 10]. Luzin and Sierpiński shared a common research interest in set theory, and it may have been partly Luzin's influence that encouraged Sierpiński to do research on CH from the same hypothetical point of view which Luzin took in his 1914 article. In any case, they published in [1917] in the Paris ***Comptes Rendus*** a joint article, part of which was a new proof of König's theorem that $2^{\aleph_0}$ cannot equal $\aleph_\omega$, but one which explicitly avoided any use of the Axiom of Choice. Late in 1918 Sierpiński became a professor at the new University of Warsaw, now no longer part of Russia but of the newly recreated country of Poland.

Early in 1919 Sierpiński published the first of what, over several decades, were to be many articles devoted to CH. In particular, he established a geometric form of CH, namely that CH is equivalent to the existence of a set M in the plane such that the intersection of M with any line parallel to the x-axis is countable and the intersection of the complement of M with any line parallel to the y-axis is also countable [1919]. Three years later he proved another theorem about CH. Call a set S of subsets a *chain* if, for any A and B in S, either $A \subseteq B$ or $B \subseteq A$. He showed that CH is equivalent to the following proposition:

(11) A set of power $2^{\aleph_0}$ is the union of some chain of its countable subsets [1922].

In 1924 Sierpiński published in ***Fundamenta Mathematicae*** an article summing up what was then known about CH:

> We still do not know if CH is true or not, and there even exist people who do not rule out the possibility that the problem could hardly be resolved without admitting a new axiom. In the present state of knowledge, it may nevertheless be useful to examine the theorems which are equivalent to CH, as well as the consequences which result if CH is true and those which result if it is false. [1924, p. 527]

He observed that "we know very few propositions which are equivalent to the Continuum Hypothesis" [1924, p. 528], mentioning the two propositions that he had proved equivalent to it in 1919 and 1922. Also he remarked that CH is equivalent to the inequality

$$\aleph_2^{\aleph_0} > \aleph_1^{\aleph_0}. \tag{12}$$

More generally, he showed, in regard to a solution of the Continuum Problem, that $2^{\aleph_0} = \aleph_\alpha$ if and only if α is the smallest ordinal such that

$$\aleph_\alpha^{\aleph_0} < \aleph_{\alpha+1}^{\aleph_0} \tag{13}$$

or equivalently, the smallest ordinal α such that

$$\aleph_\alpha^{\aleph_0} = \aleph_\alpha. \tag{14}$$

He mentioned in passing that Hausdorff had shown how CH implies the proposition that there exists an ordered set M of power $\aleph_1$ whose ordered subsets have every possible order-type of power $\aleph_1$ [1924, p. 532].[17] Likewise Sierpiński noted that CH implies $2^{\aleph_0} < 2^{\aleph_1}$ but that it was not known if that proposition is equivalent to CH. He devoted much more space, however, to the existence of a Luzin set and its consequences. Among the consequences of CH within classical analysis Sierpiński mentioned the following:

(15) Any subset of $\mathbb{R}$ with power less than that of $\mathbb{R}$ is of first category.

(16) There is a real function discontinuous on every uncountable set.

[17] Strange to say, Sierpiński stated that it was not known whether CH was equivalent to Hausdorff's proposition, although Hausdorff had mentioned that equivalence.

(17) Any subset of $\mathbb{R}$ with that is not Lebesgue measurable has power $2^{\aleph_0}$.

(18) There is an uncountable subset M of $\mathbb{R}$ such that every uncountable subset of M is not Lebesgue measurable. [1924, p. 533]

Finally, he observed that "very few propositions are known which seem probable but which we do not know how to prove except by granting that the Continuum Hypothesis is false" [1924, p. 535]. One such consequence of the negation of CH was the following:

(19) If a subset M of $\mathbb{R}$ has power $2^{\aleph_0}$ and is the complement of an analytic set, then M includes a perfect subset. [1924, p. 536]

§8. Tarski and the fourth published appearance of the Generalized Continuum Hypothesis. Although by 1925 Sierpiński had investigated CH more than any other mathematician, he had not even mentioned the Generalized Continuum Hypothesis in print. By contrast, Alfred Tarski, who was one of Sierpiński's former students and who had first published on set theory the previous year, would in 1925 begin a deep study of GCH within the context of cardinal arithmetic. His article of that year made the first real progress on cardinal exponentiation since Hausdorff [1908], Jourdain [1908], and Zermelo [1908].

Tarski began by proving a generalization of Hausdorff's theorem on cardinal exponentiation. Using Cantor's notation $\bar{\delta}$ for the cardinal of an ordinal δ, Tarski's formula was

$$\text{(20)} \qquad \text{If } \bar{\delta} \leq \aleph_\beta, \text{ then } \aleph_{\alpha+\beta}^{\aleph_\beta} = \aleph_\alpha^{\aleph_\beta} \cdot \aleph_{\alpha+\delta}^{\bar{\delta}}.$$

Hausdorff's formula

$$\aleph_{\alpha+1}^{\aleph_\beta} = \aleph_\alpha^{\aleph_\beta} \cdot \aleph_{\alpha+1}$$

was the special case of (20) where $\delta = 1$. Likewise Tarski used (20) to prove

$$\text{(21)} \qquad \text{If } \bar{\alpha} \leq \aleph_\beta, \text{ then } \aleph_\alpha^{\aleph_\beta} = 2^{\aleph_\beta} \cdot \aleph_\alpha^{\bar{\alpha}}.$$

If α was finite and if β could be either finite or infinite, then (21) became $\aleph_\alpha^{\aleph_\beta} = 2^{\aleph_\beta} \cdot \aleph_\alpha$, which was a more general form of Bernstein's theorem (2) with the dubious cases omitted [1925, pp. 2–5]. Tarski then defined what he called "Bernstein's condition" for an infinite cardinal $\mathfrak{m}$:

$$\text{(22)} \qquad \text{for every infinite cardinal } \mathfrak{n},\ \mathfrak{m}^{\mathfrak{n}} = 2^{\mathfrak{n}} \cdot \mathfrak{m}.$$

Tarski regarded König [1905] as having shown, by using the Axiom of Choice, that Bernstein's condition is not true for all infinite cardinals. (König, however, had not been aware that in effect he was using this axiom.) Later in the article Tarski gave an example, without using the Axiom of Choice, of a cardinal for which Bernstein's condition failed [1925, p. 10].

A high point of the article was the following two theorems:

(23) If α is a limit ordinal and $\omega_\beta < \mathrm{cf}(\omega_\alpha)$, then $\aleph_\alpha^{\aleph_\beta} = \sum_{\delta<\alpha} \aleph_\delta^{\aleph_\beta}$.

and

(24) If α is a limit ordinal and $\omega_\beta \geq \mathrm{cf}(\omega_\alpha)$, then $\aleph_\alpha^{\aleph_\beta} = \prod_{\delta<\mathrm{cf}(\alpha)} \aleph_\delta^{\aleph_\beta}$.

Next he introduced what amounted to the beth hierarchy, but since he defined it with a less convenient notation $\pi(\alpha)$, we paraphrase his definition by using beths:

$$\beth_0 = \aleph_0,\ \beth_{\alpha+1} = 2^{\beth_\alpha}, \text{ and if } \alpha \text{ is a limit ordinal, } \beth_\alpha = \sum_{\beta<\alpha} \beth_\beta.$$

This was the first time that the beth hierarchy (though not under that name) was defined with an index varying over all ordinals. At this point he commented on GCH [1925, p. 10]:

> I want to draw attention to the following fact. The following hypothesis:
>
> $$\text{for every ordinal } \alpha,\ \aleph_{\alpha+1} = 2^{\aleph_\alpha},$$
>
> a hypothesis which can be called the *Generalized Continuum Hypothesis*, and about which at present we do not know if it is true or false or even independent of the axioms of set theory, permits us to prove by an easy induction that
>
> $$\text{for every ordinal } \alpha,\ \aleph_{\pi(\alpha)} = \aleph_\alpha \text{ [i.e., } \beth_\alpha = \aleph_\alpha\text{]}.$$

Tarski cited Hausdorff's 1908 article naming "Cantor's Aleph Hypothesis", i.e., formula (10) above. Thus, although Tarski did not originate GCH, he did give it the simple form (much simpler than Hausdorff's (10)) found already by Jourdain [1905]. But Tarski did not appear to know this work of Jourdain.

Tarski concluded his article with a formula which seemed probable to him but which he did not know how to prove [1925, p. 14]:

(25) If α is a limit ordinal and $\mathrm{cf}(\omega_\alpha) \leq \omega_\beta$, then $\aleph_\alpha^{\aleph_\beta} = 2^{\aleph_\beta} \cdot \aleph_\alpha^{\aleph_{\mathrm{cf}(\omega_\alpha)}}$.

It followed immediately from theorems in Tarski's article that GCH completely determines the result of cardinal exponentiation in terms of alephs, although he did not state this fact explicitly [1925, p. 9]:

(26) $\aleph_{\alpha+1}^{\aleph_\beta} = \aleph_{\alpha+1}$ if $\beta < \alpha + 1$,

$\aleph_{\alpha+1}^{\aleph_\beta} = \aleph_{\beta+1}$ if $\beta \geq \alpha + 1$,

$\aleph_\alpha^{\aleph_\beta} = \aleph_\alpha$ if α is a limit ordinal and $\omega_\beta < \mathrm{cf}(\omega_\alpha)$,

$$\aleph_\alpha^{\aleph_\beta} = \aleph_{\alpha+1} \text{ if } \alpha \text{ is a limit ordinal and } \mathrm{cf}(\omega_\alpha) \leq \omega_\beta \leq \omega_\alpha,$$

$$\aleph_\alpha^{\aleph_\beta} = \aleph_{\beta+1} \text{ if } \alpha \text{ is a limit ordinal and } \omega_\alpha \leq \omega_\beta.$$

In the first article devoted entirely to GCH, the Canadian mathematician K. W. Folley [1928, p. 150, p. 156] was influenced by the results of Sierpiński [1924] and Tarski [1925] to give some propositions, each equivalent to GCH:

(27) for every α, a set of power $2^{\aleph_\alpha}$ is the union of some chain of its subsets of power $\aleph_\alpha$,

(28) for every α, $\aleph_{\alpha+1}^{\aleph_\alpha} < \aleph_{\alpha+2}^{\aleph_\alpha}$.

Perhaps more interestingly, he proved each of the following propositions to be equivalent, for a given ordinal α, to the equality $2^{\aleph_\alpha} = \aleph_\beta$. In (29), we think of $A \times A$ as a Cartesian plane with x-axis and y-axis, except that the set A has power $2^{\aleph_\alpha}$:

(29) $A \times A$ can be partitioned into sets B and C such that the intersection of B with any line parallel to the x-axis has power $< \aleph_\beta$, while the intersection of C with any line parallel to the y-axis likewise has power $< \aleph_\beta$.

(30) β is the smallest ordinal such that $\aleph_\beta^{\aleph_\alpha} = \aleph_\beta$.

Among the consequences of $2^{\aleph_\alpha} = \aleph_\beta$, for a given α and β, he proved the following:

(31) If β is cofinal with ω_δ, then $2^{\aleph_\alpha} \neq 2^{\aleph_\delta}$.

(32) If β is cofinal with $\omega_{\alpha+1}$, then $2^{\aleph_\alpha} < 2^{\aleph_{\alpha+1}}$.

Thus, in particular, it followed that although $2^{\aleph_0} < 2^{\aleph_1}$ is a consequence of CH, it is not equivalent to CH; for, by (32), $2^{\aleph_0} < 2^{\aleph_1}$ is also a consequence of $2^{\aleph_0} = \aleph_{\omega_1}$ [1928, pp. 154–164].

Folley's work remained unfamiliar at the time to those in Europe working on set theory.

A second mathematician to be influenced by Tarski's 1925 article, and in particular by its introduction of GCH, was Ladislaus Patai, a Hungarian. In 1930 he was stimulated by Tarski's conjecture (25), which asserted that if α is a limit ordinal and $\mathrm{cf}(\omega_\alpha) \leq \omega_\beta$, then $\aleph_\alpha^{\aleph_\beta} = 2^{\aleph_\beta} \cdot \aleph_\alpha^{\aleph_{\mathrm{cf}(\alpha)}}$, and was able to prove it in special cases. But he confessed that he was unable to prove, using methods then familiar, the following proposition:

(33) If $\mathrm{cf}(\omega_\alpha) > \omega_\beta$, then $\aleph_\alpha^{\aleph_\beta} = 2^{\aleph_\beta} \cdot \aleph_\alpha$.

Patai considered these two unproved propositions (25) and (33) "as *fundamental problems* of set theory" [1930, p. 137]. He concluded his article with a discussion of a more general proposition of which GCH is a particular instance:

(34) There is some ordinal β such that for every α, $2^{\aleph_\alpha} = \aleph_{\alpha+\beta}$.

This was the first proposition that anyone had suggested which gave an alternative to GCH. Patai succeeded in disproving (34) in almost all cases [1930, p. 141]:

(35) If, for every α, $2^{\aleph_\alpha} = \aleph_{\alpha+\beta}$, then β is finite.

He was then able to prove a slight generalization of (35):

(36) If, for every α, $2^{\aleph_\alpha} = \aleph_{\alpha\delta+\beta}$, then β is finite and δ is not a limit ordinal.

In [1991] Foreman and Woodin, assuming a large cardinal hypothesis (a supercompact cardinal with infinitely many inaccessibles above it) showed that GCH can fail everywhere and that it can happen, in particular, that $2^{\aleph_\alpha} = \aleph_{\alpha+2}$ for all α.

Meanwhile, in 1926, Adolf Lindenbaum and Tarski published a lengthy joint article, "Communication of Research in Set Theory", in which they stated more than 100 theorems which they had proved. They reserved their proofs to later publications. (For some of these theorems they never published proofs, and it was left to others to do so much later.) As was usual at the time, they used $\mathfrak{m}$ and $\mathfrak{n}$ for arbitrary cardinal numbers which were not necessarily alephs.

One result due to both of them was a generalization of König's 1905 result that $2^{\aleph_0}$ is not equal to $\aleph_\omega$. Their generalization, which did not use the Axiom of Choice, was

(37) If α is cofinal with ω, then there is no cardinal $\mathfrak{m}$ such that $2^{\mathfrak{m}} = \aleph_\alpha$.

Although they did not mention the fact, this was the first time that König's theorem was expressed in the way that is usual now, using cofinality [1926, p. 310]. A second related result was due to Tarski alone:

(38) If $2^{\mathfrak{m}} = \aleph_\alpha$ where α is cofinal with ω_1, then $\mathfrak{m} = \aleph_0$.

Since it followed easily from Cantor's theorem $\aleph_\alpha < 2^{\aleph_\alpha}$ and the Axiom of Choice that $\aleph_{\alpha+1} \leq 2^{\aleph_\alpha}$, they did not mention this result. But Tarski had succeeded in proving without the Axiom of Choice that $\aleph_{\alpha+1} < 2^{2^{\aleph_\alpha}}$, a result relative to the possibilities of cardinal arithmetic in the absence of the Generalized Continuum Hypothesis [1926, p. 311]. They then added:

> Monsieur Lindenbaum has posed the problem of investigating the logical relations between the Axiom of Choice and the following propositions which express in different ways the hypothesis called the "Generalized Continuum Hypothesis" (or "Cantor's Aleph Hypothesis"):
>
> D_1. For any transfinite cardinal $\mathfrak{m}$ there is no cardinal $\mathfrak{n}$ such that $\mathfrak{m} < \mathfrak{n} < 2^{\mathfrak{m}}$.
>
> D_2. For any ordinal α there is no cardinal $\mathfrak{n}$ such that $\aleph_\alpha < \mathfrak{n} < 2^{\aleph_\alpha}$.

> D_3. For every ordinal α we have $\aleph_{\alpha+1} = 2^{\aleph_\alpha}$.
>
> Messieurs Lindenbaum and Tarski solved this problem together in 1925 by using various general theorems on cardinal numbers .... [1926, p. 313]

To express those general theorems, none of which used the Axiom of Choice, it will be helpful to introduce an abbreviation. Notice that their proposition D_1 is the generalization of the Weak Continuum Hypothesis to an arbitrary infinite cardinal $\mathfrak{m}$ and so we write Weak GCH($\mathfrak{m}$) to express that D_1 holds for the particular cardinal $\mathfrak{m}$. Among the more interesting of those theorems are the following:

(39) If Weak GCH($\mathfrak{m}$) holds and $\mathfrak{m} < \mathfrak{n}$, then $2^{\mathfrak{m}} < 2^{\mathfrak{n}}$.

(They did not raise any question, however, of the relative strength, compared to GCH, of the proposition that for all transfinite $\mathfrak{m}$, if $\mathfrak{m} < \mathfrak{n}$, then $2^{\mathfrak{m}} < 2^{\mathfrak{n}}$.)

(40) If Weak GCH($\mathfrak{m}$) holds when $\mathfrak{m}$ is $\mathfrak{n}$, $2^{\mathfrak{n}}$, and $2^{2^{\mathfrak{n}}}$, then $\mathfrak{n}$, $2^{\mathfrak{n}}$, and $2^{2^{\mathfrak{n}}}$ are all alephs.

(41) If Weak GCH($2^{\aleph_\alpha}$) holds, then $\aleph_{\alpha+1} \leq 2^{\aleph_\alpha}$.

(42) If Weak GCH($\aleph_\alpha$) and Weak GCH($2^{\aleph_\alpha}$) hold, then $\aleph_{\alpha+1} = 2^{\aleph_\alpha}$.

(43) If, for some $\beta > \alpha$, Weak GCH($\aleph_\alpha$) and Weak GCH($\aleph_\beta$) hold, then $\aleph_{\alpha+1} = 2^{\aleph_\alpha}$.

From these theorems they could deduce without the Axiom of Choice (or the Axiom of Foundation) that D_2 and D_3 are equivalent and that D_1 is equivalent to the conjunction of the Axiom of Choice, D_2, and D_3. Thus, while Weak GCH($\aleph_0$), which was Cantor's original form and which we have called the Weak Continuum Hypothesis, does not imply $2^{\aleph_0} = \aleph_1$, the assumption of Weak GCH($\aleph_\alpha$) for all α does imply $2^{\aleph_\alpha} = \aleph_{\alpha+1}$.

Unfortunately, neither Lindenbaum nor Tarski ever published proofs of these theorems. In [1947] Sierpiński independently published a proof that D_1 (i.e., Weak GCH($\mathfrak{m}$) for all infinite $\mathfrak{m}$) implies the Axiom of Choice. Along the way he was able to prove, from the hypothesis of (40), that $\mathfrak{n}$ is an aleph. While the latter result was weaker than (40), in [1954] Ernst Specker was able to prove a result stronger than (40), namely that if Weak GCH($\mathfrak{m}$) holds when $\mathfrak{m}$ is $\mathfrak{n}$ and $2^{\mathfrak{n}}$, then the latter is an aleph.

Sierpiński also influenced the next work that Tarski published relating to GCH. In [1928] Sierpiński introduced the notion of two sets being "almost disjoint". He defined two infinite sets to be almost disjoint if their intersection has a power less than that of each of the sets. He then proved a result that he called "somewhat paradoxical" [1928, p. 719]: Any infinite set of power $\mathfrak{m}$ can be decomposed into a set of power greater than $\mathfrak{m}$ of almost disjoint sets.

For Tarski to state his related results, he needed a new concept. The "degree of disjunction" of a family K of sets, in symbols $\delta(K)$, was the smallest cardinal $\mathfrak{p}$ such that $X \cap Y$ has cardinal less than $\mathfrak{p}$ for every distinct X and Y in K. One of his results gave an equivalent, in terms of almost disjoint sets, to $2^{\aleph_\alpha} < 2^{\aleph_{\alpha+1}}$:

(44) In order that every set M of power $2^{\aleph_\alpha}$ can be decomposed into a family K of power greater than $2^{\aleph_\alpha}$ of almost disjoint sets having power $\aleph_{\alpha+1}$, it is necessary and sufficient that $2^{\aleph_\alpha} < 2^{\aleph_{\alpha+1}}$. [1928, p. 198]

Another theorem gave an equivalent to a given case of the negation of GCH:

(45) In order that every set M of power $\aleph_{\alpha+1}$ can be decomposed into a family K of power greater than $\aleph_{\alpha+1}$ of infinite sets that are almost disjoint and such that $\delta(K) \leq \aleph_\alpha$, it is necessary and sufficient that $\aleph_{\alpha+1} = 2^{\aleph_\alpha}$ is false.[18]

Tarski mentioned that (45) had been communicated to him by Sierpiński in the special case $\alpha = 0$ with the remark that it gave a condition equivalent to the negation of the Continuum Hypothesis [1928, p. 201].

Summing up his 1928 results, Tarski remarked that, as he had shown in his [1925] article, the Generalized Continuum Hypothesis has strong consequences for cardinal exponentiation; the importance of GCH was equally clear for families of almost disjoint sets. In particular, it followed from GCH that both (46) and (47) are provable:

(46) If $\omega_\beta < \mathrm{cf}(\omega_\alpha)$, then no set M of power $\aleph_\alpha$ can be decomposed into a family K of power greater than $\aleph_\alpha$ of almost disjoint sets such that $\delta(K) \leq \aleph_\beta$.

(47) If $\omega_\beta \geq \mathrm{cf}(\omega_\alpha)$, then any set M of power $\aleph_\alpha$ can be decomposed into a family K of power greater than $\aleph_\alpha$ of almost disjoint sets such that $\delta(K) \leq \aleph_\beta$.[19] [1928, pp. 201–202]

In the course of proving (46) and (47) Tarski first stated

$$\aleph_\alpha^{\aleph_\beta} = \aleph_\alpha \text{ if } \omega_\beta < \mathrm{cf}(\omega_\alpha) \text{ and} \tag{48}$$

$$\aleph_\alpha^{\aleph_\beta} = \aleph_{\alpha+1} \text{ if } \mathrm{cf}(\omega_\alpha) \leq \omega_\beta \leq \omega_\alpha \text{ and} \tag{49}$$

which are most of what we noted in (26) above as consequences of GCH.

The following year Tarski came back to the subject of almost disjoint sets and dealt with the problems which he had not known how to solve the year before, even in the simplest case of $\beta = 0$. His principal new results all presupposed GCH:

[18][1928, p. 200]. The reader of Tarski's theorem in his original text (Corollary 24) should note that $\aleph_{\alpha+1} \neq 2^{\aleph_\alpha}$ has been misprinted as $\aleph_{\alpha+1} = 2^{\aleph_\alpha}$, a typographical error apparent when one reads his proof.

[19]Much later Baumgartner [1976, p. 414] proved, from a much weaker hypothesis than GCH, that there are $2^{\aleph_1}$ almost disjoint subsets of $\aleph_1$, namely from $2^{\aleph_0} < \aleph_{\omega_1}$.

(50) If $\mathrm{cf}(\omega_\alpha) \neq \mathrm{cf}(\omega_\beta)$, then no set of power $\aleph_\alpha$ can be decomposed into a family K of power greater than $\aleph_\alpha$ of almost disjoint sets, each of power greater than or equal to $\aleph_\beta$, such that $\delta(K) \leq \aleph_\beta$.

(51) If $\mathrm{cf}(\omega_\alpha) = \mathrm{cf}(\omega_\beta)$ and $\beta \leq \alpha$, then any set M of power $\aleph_\alpha$ can be decomposed into a family K of power greater than $\aleph_\alpha$ (in fact of power equal to $2^{\aleph_\alpha}$) of almost disjoint sets, each of power $\aleph_\beta$, such that $\delta(K) \leq \aleph_\beta$. [1929, pp. 211–212]

Closely related to (50) was the following consequence of GCH:

(52) No set of power $\aleph_\alpha$ can be decomposed into a family K of power greater than $\aleph_\alpha$ of almost disjoint sets, each of power greater than $\aleph_\beta$, such that $\delta(K) \leq \aleph_\beta$.

However, he was able to deduce (52), for the case when $\beta = 0$, from an assumption which he called H_0 and which he believed to be weaker than GCH. He gave the following three equivalent forms of H_0:

(53) For each α, either $\aleph_\alpha^{\aleph_0} = \aleph_\alpha$ or $\aleph_\alpha^{\aleph_0} = \aleph_{\alpha+1}$, i.e., there is no cardinal $\mathfrak{m}$ such that $\aleph_\alpha < \mathfrak{m} < \aleph_\alpha^{\aleph_0}$.

(54) $\aleph_\alpha^{\aleph_0} = \aleph_\alpha$ if $\mathrm{cf}(\aleph_\alpha)$ is uncountable.

(55) $\aleph_\alpha^{\aleph_0} = \aleph_{\alpha+1}$ if $\mathrm{cf}(\aleph_\alpha)$ is countable. [1929, p. 213]

But he was able to prove the following weakened forms of (50) and (51) without assuming GCH:

(56) If α is a limit ordinal and $\mathrm{cf}(\omega_\alpha) \neq \mathrm{cf}(\omega_\beta)$, then no set of power $\beth_\alpha$ can be decomposed into a family K of power greater than $\beth_\alpha$ of almost disjoint sets, each of power greater than or equal to $\aleph_\beta$, such that $\delta(K) \leq \aleph_\beta$.

(57) If $\mathrm{cf}(\omega_\alpha) = \mathrm{cf}(\omega_\beta)$ and $\beta \leq \alpha$, then any set M of power $\beth_\alpha$ can be decomposed into a family K of power greater than $\beth_\alpha$ (in fact of power equal to $\beth_{\alpha+1}$) of almost disjoint sets, each of power greater than or equal to $\aleph_\beta$, such that $\delta(K) \leq \aleph_\beta$. [1929, p. 214]

Summing up his results of 1928 and 1929, in which he acknowledged the substantial role of GCH, he concluded with a question for future research: "It remains to be settled to what degree the Generalized Continuum Hypothesis must appear in the proofs of the theorems of this field" [1929, p. 215].

Also in 1929, Tarski published a more general article on the "historical development and present state" of cardinal arithmetic. In this context he was particularly interested in describing "those parts of cardinal arithmetic which, with the means available in today's mathematics, cannot be sufficiently mastered, namely the *theory of exponentiation and of infinite products of cardinals*" [1929a, p. 52]. Here he mentioned his own work of 1925 and his joint article with Lindenbaum of 1926. Among what he considered the most difficult problems "the *Continuum Hypothesis* and its generalization *Cantor's Aleph Hypothesis* take first place" [1929a, p. 53]. He added:

> The main result in this direction can be characterized in the following way: Cantor's Aleph Hypothesis has the same significance for the theory of [cardinal] exponentiation as the Axiom of Choice has for other parts of the field under discussion [i.e., cardinal arithmetic]—*the assumption of this hypothesis would bring with it a decision for all interesting problems of* [cardinal] *exponentiation and thereby make this part of the theory trivial.* [1929a, p. 53]

Except for the connection to large cardinals (see §10 below), Tarski did not devote any further article to GCH. The one exception was in 1938 in an article about the deductive strength of various definitions of finite set, when such a definition was assumed equivalent to the usual definition of finiteness (i.e., able to be put in one-one correspondence with an ordinal less than ω). He gave the following definition of finite set and observed that its equivalence with the usual definition implies GCH:

(58) A set M is finite if and only if either M has at most one element or there exists a set with a larger power than M and a smaller power than the set of all subsets of M. [1938, p. 163]

Meanwhile in [1932] Miss S. Braun[20] and Sierpiński published a joint article devoted to establishing the equivalence of CH with various propositions. They then generalized their results by showing that for each cardinal $\mathfrak{m} \geq \aleph_0$, the following five propositions are equivalent. Note that the first of them is what we called Weak GCH($\mathfrak{m}$):

(59) There is no cardinal $\mathfrak{n}$ such that $\mathfrak{m} < \mathfrak{n} < 2^{\mathfrak{m}}$.

(60) Suppose that M has power $\mathfrak{m}$ and N has power $2^{\mathfrak{m}}$. Then there exists a family F of power $2^{\mathfrak{m}}$ of functions $f : M \to N$ such that, given an arbitrary function $\phi : M \to N$, the set of all f in F such that, for all x in M, $f(x) \neq \phi(x)$ has power $\leq \mathfrak{m}$.

(61) Suppose that M has power $\mathfrak{m}$ and N has power $2^{\mathfrak{m}}$. Then there exists a family S of sets A_n^m for all m in M and n in N such that
   (i) for each fixed m in M, we have $N = \bigcup_{n \in N} A_n^m$;
   (ii) for each m in M and each n and p in N, A_n^m and A_p^m are disjoint if $n \neq p$;
   (iii) for each $f : M \to N$, the set $N - \bigcup_{m \in M} A_{f(m)}^m$ has power $\leq \mathfrak{m}$.

(62) Suppose that N has power $2^{\mathfrak{m}}$. Then there exists a family S of power $\mathfrak{m}$ whose members are functions $f : N \to N$ such that for each subset P of N of power $> \mathfrak{m}$ there is at least one f in S such that $f''P = N$.

(63) Every set of power $2^{\mathfrak{m}}$ is the union of a chain of sets each of power $\mathfrak{m}$.

Braun and Sierpiński remarked [1932, p. 6] that it was Lindenbaum who had noticed that (59) implies (60) which in turn implies (61). Although

[20] Efforts to determine her first name have been unsuccessful. She was killed under unknown circumstances during the Nazi occupation of Poland.

the joint authors did not mention the fact, (63) is the same as Folley's (27), which in turn had generalized Sierpiński's (11).

§9. Baer and applications of GCH in algebra. The German algebraist Reinhold Baer, who during the latter part of the 1920s was in close contact with Zermelo at Freiburg, was particularly interested in the relations between set theory and algebra. In 1929 Baer published an axiomatization of cardinal arithmetic, explicitly taking GCH as an axiom, for he aimed to show the categoricity of his axioms [1929, p. 382]. (Somewhat surprisingly, Fraenkel's earlier axiomatization of cardinal arithmetic [1922] had made no mention of CH or GCH.)

The following year Baer made the first application of GCH to an area outside of set theory, namely to algebra and in particular to field theory. He showed that the following theorem is a consequence of GCH: If F is any field and if K is an algebraic extension of F, then any two chains of fields which produce K from F have equal cardinality [1930, p. 132]. (A chain of fields which produces K from F is a β-sequence of fields K_α, for some ordinal β, such that $K_0 = F$ and $K_{\alpha+1}$ is obtained from K by closing under the field operations after adjoining a single element of K not in K_α, and K_α is the union of all earlier K_δ when α is a limit ordinal, and finally $K_\beta = K$.)

§10. GCH and early large cardinals. The first "large cardinals", as they are called now, to be considered were the weakly inaccessible cardinals, which Hausdorff introduced in 1908. In [1906] he had introduced the notion of the cofinality of an ordered set and had used it to explore the possible ordered sets of cardinality $\aleph_1$. The following year he applied cofinality to define singular ordinals and regular ordinals [1907a, p. 542].

In 1908 he proved, using the Axiom of Choice, that ω_α is regular for every successor ordinal α, and then asked:

> The question whether the converse of this theorem is true or whether there are regular initial ordinals whose index is a limit ordinal must remain undecided here, but we make the following remark about it. A regular initial ordinal with limit index ... can only be found among those initial ordinals which are equal to their own index ($\omega_\zeta = \zeta$) and which for the time being we call, as did Hessenberg, ζ-numbers [1908, p. 443].

Hausdorff was aware, as Hessenberg had been in 1906, that there is a ζ-number greater than any given ordinal. Only many years later did those ordinals ω_α whose index α is a regular limit ordinal come to be called weakly inaccessible. Hausdorff concluded that the existence of such a weakly inaccessible ordinal was "problematical, but must be considered as a possibility in everything that follows" [1908, p. 444].

In his 1914 book Hausdorff came back to this question. In the interim he had developed the theory of normal functions (i.e., monotone continuous functions on the class of all ordinals). He observed that ω_α, treated as a function of α, is a normal function and hence has a fixed point, i.e., some β such that $\omega_\beta = \beta$. After constructing the first such fixed point β and noting that it is singular and has cofinality ω, he commented on the possible existence of regular fixed points, i.e., weakly inaccessible cardinals:

> If there are regular initial ordinals with a limit ordinal as index (and so far it has not been possible to derive a contradiction from this assumption), then the smallest of them is of so exorbitant a magnitude that it can scarcely come into consideration for the usual purposes of set theory. [1914, p. 131]

Hausdorff turned out to be mistaken about the effect of these weakly inaccessible cardinals on ordinary set theory; indeed, it would eventually become clear that the existence of some of them had strong consequences for possible sets of real numbers.

Researchers on set theory soon after Hausdorff generally assumed that there is no weakly inaccessible cardinal. Such was the case with Fraenkel [1922] when axiomatizing cardinal arithmetic with an "Axiom of Restriction" (whose effect was to exclude weak inaccessibles), with von Neumann who, when giving an extension of his axiom system for set theory, assumed that there is no regular ω_α such that α is a limit ordinal [1925, §6]. Baer, in his axiomatization of cardinal arithmetic, made the same assumption [1929, p. 382]. Baer also credited Zermelo for the unpublished result that if set theory with a weak inaccessible is consistent, then so is set theory without any weak inaccessible [1929, p. 383].

Likewise Tarski wrote in [1925, p. 10] that it was "very doubtful" that weakly inaccessible cardinals exist. (He cited [Schoenflies and Hahn 1913, p. 241] in this regard.) By contrast, in 1926 he seriously considered their existence when axiomatizing the arithmetic of infinite ordinals [Lindenbaum and Tarski 1926, p. 325].

Tarski in 1929 discussed large cardinals in his survey article "Historical Development and Present State of the Theory of Equipotence and Cardinal Arithmetic":

> Mention must be made of a final group of still unsolved problems of the theory of equipotence: those that concern *the existence of sufficiently large infinite cardinals.* In contrast to everything discussed above, these problems are in large measure dependent on the specific properties of that system of set theory taken as a foundation: while in one system, such as ***Principia Mathematica***, even the existence of a single infinite cardinal cannot be either asserted or denied, on the basis of another system, such as that of Zermelo–Fraenkel, the first doubtful matter is the existence of those "exorbitant quantities" of

> Hausdorff, i.e., the regular alephs with limit index. The treatment of these questions, so far little pursued, has brought to light almost no points of contact with other considerations in the field of cardinal arithmetic; rather, they are closely connected to investigations in the foundations of (part or all of) set theory. [1929, pp. 53–54]

Although in 1929 Tarski found the existence of such weakly inaccessible cardinals unlikely to be provable in Zermelo–Fraenkel set theory, his attitude of 1925 had changed and now he was intrigued by them. In a joint article with Sierpiński [1930], he introduced the notion of strongly inaccessible cardinal, and indicated that the name "inaccessible" came from Kazimierz Kuratowski:

> The term *inaccessible numbers* has been proposed by Monsieur Kuratowski to designate regular alephs whose indices α are of the second species. The purpose of this note is to establish a characteristic property of inaccessible numbers. Nevertheless, the definition of these numbers adopted here is different from that of Monsieur Kuratowski, and it is not known how to prove the equivalence of these definitions without using Cantor's aleph hypothesis: $2^{\aleph_\alpha} = \aleph_{\alpha+1}$ for every ordinal α [1930, p. 292].

(Kuratowski's definition of inaccessible, i.e., a regular limit cardinal, is now called weakly inaccessible.[21]) Sierpiński and Tarski defined an infinite cardinal $\mathfrak{m}$ to be inaccessible—now often called strongly inaccessible—if $\mathfrak{m}$ is not the product of a smaller number of cardinals each smaller than $\mathfrak{m}$. With this definition, they noted, $\aleph_0$ is strongly inaccessible, and so the question was this: Do there exist any other strongly inaccessible cardinals? They showed that every strongly inaccessible cardinal is weakly inaccessible and that GCH implies that every weakly inaccessible cardinal is strongly inaccessible [1930, p. 293].

Meanwhile another Polish mathematician, Stefan Banach, had come upon a further relationship between analysis, inaccessible cardinals, and GCH. Banach remarked that in 1904 Lebesgue had posed the "Measure Problem": Does there exist a function m, defined on all subsets A of the real interval $E = [0, 1]$ and with $m(A) \geq 0$, such that

1. $m(A) = m(B)$ if A and B are congruent,
2. m is countably additive, and
3. $m(E) = 1$?

Using the Axiom of Choice, Giuseppe Vitali in 1905 had shown that the answer was no. And in 1914 Hausdorff proved that the answer was also no,

[21] Kuratowski [1924] remarked that with the Zermelo–Fraenkel axioms it was not known how to prove the existence of a regular ω_α where α is a limit ordinal. But he did not use the term "inaccessible" in print. So it appears that this term spread orally from Kuratowski to Sierpiński and Tarski.

in three dimensions or more, even if condition (2) was weakened to finite additivity. Banach himself, in 1923, established that with finite additivity the answer was yes for one or two dimensions.

Now, in 1929, Banach was interested in the question, which he called the Generalized Measure Problem, whether countable additivity could be reinstated if condition (1) was replaced by the condition that $m(\{p\}) = 0$ for each point p, and if (3) was replaced by requiring that $m(A)$ is not zero for some set A. Banach found that he could solve this problem negatively if he could prove a certain combinatorial lemma on countable unions. Banach and Kuratowski independently found a proof of the lemma by using the Continuum Hypothesis, and so they published their results in a joint paper [1929]. They noted:

> As is seen here, the fact that the Measure Problem has no solution is not *geometric* in nature (as you might believe on the basis of condition (1)) but is a fact of *set theory.* As for the set E, clearly we can assume in what follows that it is an arbitrary set of the power of the continuum and not necessarily an interval. [1929, p. 128]

Banach pursued the question further along the lines of the previous sentence by replacing the set E in the Generalized Measure Problem, where it had the power of the continuum, with a set E of arbitrary cardinality. Then this problem, called the Generalized Measure Problem for Abstract Sets, became a question of the existence of such a cardinal. By assuming the Generalized Continuum Hypothesis, Banach was able to show that the smallest such E must be weakly inaccessible [1930].

Stanislaw Ulam, then a 21-year-old student, was encouraged by Kuratowski to improve Banach's result. Ulam [1930] succeeded in proving Banach's 1930 result (which would be expressed nowadays by saying that a real-valued measurable cardinal must be weakly inaccessible) without using CH or GCH at all. And he strengthened the Banach–Kuratowski result by showing, without assuming CH, that the Generalized Measure Problem has no solution if there exists no weakly inaccessible cardinal less than or equal to $2^{\aleph_0}$.

Thus ended the early interactions between CH, GCH, and large cardinals. Only in the 1960s did these interactions begin anew.

§11. Conclusion: Gödel and GCH. Prior to 1963 and Cohen's work, the most striking result about GCH was Kurt Gödel's proof [1938] of its consistency relative to the accepted axioms for set theory. By 1938, when Gödel first published his proof, relatively little was known about GCH and its consequences. Sierpiński's 1934 book on CH, which gave eleven propositions equivalent to CH and 82 other consequences of CH, had little to say about GCH. What it did say did not go beyond what Tarski had done in the period

1925–1929 and what Sierpiński himself had done with Braun in 1932 (see §8 above). Gödel's results on constructible sets, which depended in an essential way on first-order logic, stood as an isolated landmark until the 1960s, when forcing and large cardinals opened up the set-theoretic landscape. (A second-order version of Gödel's constructible sets, i.e., using second-order definability, would give the hereditarily ordinal definable sets rather than L; see [Myhill and Scott 1971, pp. 277–278].)

It was during the night of 14–15 June 1937 that Gödel discovered a crucial step in proving the relative consistency of GCH [Gödel 1986, p. 40]. On 13 July von Neumann wrote to Gödel, expressing an interest in publishing "your paper on the Axiom of Choice" in the ***Annals of Mathematics***, of which von Neumann was an editor [Gödel 2003, p. 353]. So by that date he presumably knew of Gödel's relative consistency proof for that axiom. On 3 July Gödel had written to his friend Karl Menger, referring to "the partial result on the Continuum Hypothesis which I mentioned to you" [Menger 1981, p. 14]. And on 15 December Gödel gave a more precise statement:

> I continued my work on the Continuum Problem last summer, and I finally succeeded in proving the consistency of the Continuum Hypothesis (even the generalized form, $2^{\aleph_\alpha} = \aleph_{\alpha+1}$) with respect to general set theory. But I ask you for the time being to please not tell anyone about this. So far, I have communicated this, besides to yourself, only to von Neumann, for whom I sketched the proof during his latest stay in Vienna. Right now I am trying to also prove the independence of the Continuum Hypothesis, but do not yet know whether I will succeed with it . . . . [Menger 1981, p. 15]

Gödel's attempts to establish the independence of CH, as well as that of the Axiom of Choice, did not become public knowledge until after Cohen had discovered forcing. We know from a variety of sources that Gödel believed he had succeeded in establishing the independence of $V = L$ and of the Axiom of Choice. Kreisel, at the Reading meeting in 1958, stated to Gandy that Gödel had told Kreisel the independence proofs. Kreisel felt that he had understood Gödel's proofs well enough to reconstruct them, if necessary. The proofs made use of a modified logic.[22]

The first public acknowledgement of Gödel's independence results occurred when Cohen received the Fields Medal at the International Congress of Mathematicians held at Moscow in 1966. In his speech awarding the medal, Alonzo Church remarked:

> Gödel . . . in 1942 found a proof of the independence of the Axiom of Constructibility in type theory. According to his own statement (in a private communication), he believed that this could be

[22] Interview of Robin O. Gandy by G. H. Moore at Palo Alto, CA, 30 March 1982, and at Hull, U.K., 17 July 1986.

> 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. [Church 1968, p. 17]

There is also some direct evidence about Gödel's independence results. In 1967 Wolfgang Rautenberg wrote to Gödel, stating that Mostowski had claimed that about 1940 Gödel was in possession of most of Cohen's independence results. Gödel's reply reveals that he considered his method to be related to that of Boolean-valued models:

> Dear Professor Rautenberg:
>
> In reply to your inquiry I would like to refer you 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 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. [Moore 1988, p. 151]

But about 1930, prior to Gödel's work on constructible sets, Zermelo had pointed out that CH was decided in the same way in *all* second-order models of Zermelo–Fraenkel set theory [Moore 1980, p. 134]. This was a consequence of his result that a second-order model without urelements was determined by the first inaccessible ordinal not belonging to the model, a result that he had published in [1930]. In fact, it followed from his result that $2^{\aleph_\alpha} = \aleph_{\alpha+1}$ held for a given ordinal α in one such model if and only if it held in all such models containing the ordinal α. Nevertheless, Zermelo's approach in terms of second-order models was to be a path not followed by later set-theorists.

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DEPARTMENT OF MATHEMATICS
MCMASTER UNIVERSITY
HAMILTON, ONTARIO L8S 4K1, CANADA
E-mail: ghmoore@mcmaster.ca