Historia Mathematica **29** (2002), 40–64
doi:10.1006/hmat.2001.2332, available online at http://www.idealibrary.com on IDEAL®

Hilbert on the Infinite: The Role of Set Theory in the Evolution of Hilbert's Thought

Gregory H. Moore

Department of Mathematics, McMaster University, Hamilton, Ontario, Canada L8S 4K1

Although Hilbert created no new set-theoretic theorems, he had a profound effect on the development of set theory by his advocacy of its importance. This article explores Hilbert's interactions with set theory, as expressed in his published work and especially in his unpublished lecture courses. © 2002 Elsevier Science (USA)

Bien qu'il n'ait pas énoncé de nouveaux théorèmes de la théorie des ensembles, Hilbert a profondément influen é le développement de cette dernière par ses plaidoyers visant à en montrer l'importance en mathématiques. Le présent article examine les interactions de Hilbert avec la théorie des ensembles telles qu'elles apparaissent dans ses oeuvres publiées, mais surtout dans ses notes de cours inédites. © 2002 Elsevier Science (USA)

Obwohl Hilbert keine neuen mengentheoretischen Sätze beigetragen hat, war sein Einflus auf die Entwicklung dieses Gegenstandes äusserst bedeutsam, besonders wegen der Betonung der Wichtigkeit. Wir untersuchungen dieses Einfluss in seinen Veröffentlichungen, und darüber hinaus und besonders in seinen unveröffentlichen Vorlesungsskripten. © 2002 Elsevier Science (USA)

MSC 1991 subject classifications 01A55, 01A60, 01A70, 03E30, 04A25, 04A30.
Key Words: David Hilbert; set theory; axiom of choice; continuum hypothesis.

1. INTRODUCTION

"No one shall expel us from the paradise which Cantor has created for us." In this famous sentence of 1926, Hilbert echoes (and negates) the expulsion of Adam from the Garden of Eden [1926, 170]. It reflect Hilbert's vigorous advocacy of set theory, an advocacy which he pursued for more than three decades. That advocacy is familiar to both set theorists [Jech 1978, vii] and historians [Mehrtens 1990, 164].

Yet no one has studied in its entirety the role that set theory played for Hilbert—in his personal interactions (with Bernstein, Cantor, Hausdorff, Minkowski, and Zermelo, among others), in his published work, and in his unpublished lectures. The present article attempts to do so, placing particular emphasis on unpublished materials. The emphasis here is on Hilbert's influenc in making set-theoretic ideas respectable and widespread—thereby shedding light both on set theory itself, as a mathematical discipline, and on the social interactions that led to its increasing influenc in the early 20th century.

To understand the role that set theory played in Hilbert's work, it is vital to conceive of set theory in a *broad* sense. Set theory, in this context, is the mathematical study of infinit classes. With this definition the importance of set theory in the emergence of modern mathematics becomes clear. Set theory is not just Cantor's infinit cardinal and ordinal

0315-0860/02 $35.00

numbers, but also includes the set-theoretic techniques, coming largely from Dedekind, that revolutionized algebra. Dedekind's use of infinite classes (as opposed to merely working, as Gauss had done, with representatives of equivalence classes) deepened algebraic number theory, culminating in Hilbert's justly famous *Zahlbericht*, and, in time, led to the creation of modern algebra through techniques which simultaneously relied on Cantor's ordinals and Dedekind's set-theoretic constructions (e.g. Steinitz's work on fields). It is to be regretted that so little is known about the interactions between Dedekind and Hilbert.[1]

Hilbert's first systematic use of infinite sets occurred in 1894 when he gave a new proof of Dedekind's fundamental theorem that, in an algebraic number field, every ideal can be decomposed uniquely into a product of prime ideals [Hilbert 1894]. But it is likely that Hilbert had been introduced to Dedekind's approach to number theory in his student days, when he took a course in number theory from Dedekind's friend and algebraic collaborator, Heinrich Weber [Blumenthal 1935, 389]. (Indeed, Weber was the first to use set theory in an algebra textbook.) When he turned from the invariant theory of his *Doktorvater* Lindemann to algebraic number theory, Hilbert was greatly influenced by Dedekind's formulation of ideal theory, which depended in an essential way on the use of infinite sets. (Hilbert, like Dedekind before him, was not using infinite sets in general but just those infinite sets that are *finitely* generated.) In his 1897 *Zahlbericht* on algebraic number theory, Hilbert wrote that "in general the modern development of pure mathematics takes place *under the sign of number*: Dedekind's and Weierstrass's definitions of the fundamental arithmetic notions and Cantor's formation of [real] numbers lead to an *arithmetization of function theory*" [1897, 66]. Here Hilbert saw real analysis as reducible to the rational numbers; the essential role which set theory played in that reduction was not yet altogether clear. In fact, the question of the precise relationship between arithmetic and set theory would be an major theme of Hilbert's later work.

Hilbert's favorable attitude toward Cantor and Dedekind must be contrasted with that of his eminent colleague at Göttingen, Felix Klein. In his lengthy 1928 book on the development of mathematics in the 19th century, Klein did not mention Cantor's work even once—despite having edited several of Cantor's articles for *Mathematische Annalen*. By contrast, Klein briefly mentioned Dedekind's work on algebraic number theory, but remarked that Dedekind's terminology was "unpleasant" and "altogether unintuitive" [1928, 304]. Dedekind, Klein added, had taken an "abstract turn," i.e., had worked with infinite sets of numbers rather than with individual numbers. He regarded Hilbert as too "attached to Dedekind's way of thinking" [1928, 310].

2. HILBERT, MINKOWSKI, AND CANTOR: 1895

It is uncertain when Hilbert first learned of Cantor's set theory. Perhaps he did so from Hurwitz, who, coming to Königsberg in 1884, was soon in intimate scientific contact with Hilbert and had just made one of the earliest uses, in complex analysis, of Cantor's results on infinite cardinals [1883, 204]—a use which Hilbert himself remarked in his later obituary for Hurwitz. Certainly Cantor was already mentioned in a letter that Hilbert's close friend Minkowski sent to him, in June 1889, about meeting Cantor while at Kronecker's home

[1] The author is far from unique in arguing for this broader understanding of set theory. A similar view was taken in 1965 by Medvedev [1965], and more recently by Ferreiros [1993] and [1999].

[Minkowski 1973, 35]. When Minkowski wrote to Hilbert in 1895, he referred to Cantor in the course of discussing a lecture that he had recently given on the infinite:

> The "actual infinite" is an expression that I took from a paper by Cantor, and for the most part I included in my lecture theorems of Cantor which have a general interest. Only a few did not want to believe in them. The actual infinite in nature, about which I mainly spoke... was the position of points in space.... On this occasion I perceived anew that Cantor is one of the most ingenious living mathematicians. His purely abstract definition of the power [cardinal number] of points on a line segment with the help of the so-called transfinite numbers is really wonderful. [1973, 68]

Hilbert returned to this theme in his 1910 obituary for Minkowski, showing his strong support for set theory, which he called "that mathematical discipline which today plays a prominent role and exercises a powerful influence on all fields of mathematics" [1910b, 466]. Hilbert contrasted that influence in 1910 with the position of set theory some decades earlier, when Kronecker had attacked it: "Minkowski was the first mathematician of our generation—and I supported him vigorously in this—who recognized the great significance of Cantor's theory and sought to see it respected" [1910b, 466]. In his unpublished 1920 lectures on logic, Hilbert again affirmed his and Minkowski's role in support of set theory:

> For a time the influence of Kronecker's direction was very strong, and in my student years it was clearly disreputable to concern oneself with set theory. Through the mystical embellishment, which in the beginning Cantor gave to his theory, the antipathy to it was intensified. The first in the younger generation who seriously espoused Cantor's cause were Minkowski and myself.... [1920, 21]

Blumenthal, in his authorized biography of Hilbert, pointed out that the only publication from Hilbert's first period on a topic other than invariant theory was on "a set-theoretic theme" [1935, 389]. What Blumenthal had in mind was Hilbert's brief article on a Peano curve [1891]. Although Hilbert did not cite Cantor directly in that paper, he did cite Peano [1890], who in turn had referred explicitly to Cantor's work.

3. HILBERT ENCOUNTERS CANTOR: 1897–1900

The earliest known encounter between Cantor and Hilbert took place on 25 September 1897. For the next day, Cantor wrote to Hilbert:

> Unfortunately, because of an early lunch yesterday at the Braunschweig Polytechnic, I had to break off our conversation about set theory at the point where you had just raised the question whether all transfinite cardinal numbers... are included in the alephs, in other words whether each definite **a** or **b** is also a definite aleph. [1991, 388]

The question which Hilbert raised, i.e., whether every definite well-defined *completed* [*fertige*] set can be well-ordered, was to be the subject of quite a few letters between them.[2] And in 1900 Hilbert made it part of the first of his famous problems for the 20th century. It would lead to Zermelo's 1904 letter to Hilbert, proving by means of his Axiom of Choice that the answer to the question is yes (See [Moore 1982].)

But in the letter of September 1897 Cantor's concern was twofold. First, he was in the process of preparing a third installment to his article "Beiträge zur Begründung der

[2] Only Cantor's side of the correspondence survives, within Hilbert's *Nachlass*. Some of these letters are discussed by Purkert [1986], who rightly recognizes that Cantor was not bothered by the paradoxes because he had already made the relevant distinction in 1883 between those collections small enough to be sets and those that were too big to be sets (e.g., the collection of all sets).

transfiniten Mengenlehre," which had appeared in *Mathematische Annalen* in 1895 and 1897. Klein had seen those two installments through press, but for some reason Cantor now preferred to discuss the third installment (which, in fact, would never be finished and is not extant) with another, and newer, member of the *Annalen* editorial board: Hilbert. Second, Cantor was concerned to prove that the answer to Hilbert's question was yes, in the sense that every "definite well-defined *completed* [*fertige*] set" can be well-ordered. Cantor did so by pointing out that the collection of all alephs is not such a set M, since if it were, then M would have an aleph as its cardinal number, and hence this aleph would have another aleph following it but also belonging to M—a contradiction [1991, 388]. (In effect, this was a form of what was later called Burali–Forti's paradox; on the tangled history of this paradox, see [Moore and Garciadiego 1981] in this journal.)

Hilbert, however, was not convinced of the correctness of Cantor's proof and replied the very next day: "The collection of alephs can be conceived as a definite well-defined set, since, after all, if any object is given, it must always be determinate whether this object is an aleph or not; more than this is not required of a well-defined set."[3] Here Hilbert was merely following the criterion for being a set that Cantor himself had introduced some years earlier [1882, 114].

A few days later, on 2 October 1897, Cantor tried to meet Hilbert's objection. Cantor emphasized that in his September letter he had required not just a definite well-defined set but a *completed* set, and stated explicitly as a theorem that "the totality of all alephs cannot be conceived as a definite, well-defined and at the same time completed set" [1991, 390]. He attempted to make his views precise:

> I venture to designate this *theorem, which is completely secure and is proved from the definition of the "totality of all alephs,"* as the most important and noble theorem of set theory. But one must understand the expression "completed" correctly. I say of a set that it can be thought of as *completed,* and I name such a set "*transfinite*" ... when it is infinite, if it is possible without contradiction (as is the case for finite sets) to think of all its elements as *existing together* and hence the set itself as *joined together as a thing by itself*; or also, in other words, if it is *possible to think* of the set with the totality of its elements as *presently existing*. [1991, 390]

But even after this explanation, Hilbert was not completely satisfied with Cantor's conception of "completed" set and regarded it, quite correctly, as lacking in precision. (Cantor was mistaken in thinking that the class of all alephs was self-contradictory, but not until von Neumann's work in the 1920s [1925] was it clear that the problem was in allowing such a proper class to be a member of a set; allowing this was what led to a contradiction, not the mere existence of such large classes, which are today used routinely in set theory.) Meanwhile, on 11 October 1897, Cantor wrote again, pointing out that the set of all continuous real functions has the same cardinal as the set of all real numbers [1991, 391].

Despite his reservations about "completed" sets, Hilbert was already impressed with Cantorian set theory. In Hilbert's *Nachlass* is a bound notebook with notes from two of his short courses for school teachers. The second of them reads: "Feriencursus: über den

[3] This is one of the few passages that survive from Hilbert's side of the correspondence, in this case because Cantor quoted it in his letter of 2 October.

Begriff des Unendlichen. Ostern 1898." This was a short course which he gave, at Easter 1898, on the infinite in geometry and in arithmetic.[4] It is the earliest known reference in his writings to Cantor's work (e.g., [1895, 1897]).

In the course, Hilbert wished to show the connection between school mathematics and the "most modern" research in mathematics by describing this connection in regard to "one of the most important concepts in mathematics: the concept of the infinite" [1898, 1]. He pointed out the errors in earlier treatments of the infinite, where "one tended to transfer to infinite systems [i.e., sets] properties which were obvious for finite systems" [1898, 3]. He contrasted this with the "modern treatment of the concept of the infinite," namely Cantor's transfinite cardinals [1898, 7]. In particular, he gave Cantor's proof that the set of rational numbers is countable and showed, as Cantor had done in his October letter, that the set of all continuous functions has no greater power than the set of real numbers.

Hilbert's 1898 course at Göttingen was one of the first in Germany to speak in detail about set theory. (A year earlier, in France, Borel had given a course on function theory that included a good deal of set theory [1898, vii].) The first lecture courses devoted entirely to set theory were given soon afterward: by Zermelo at Göttingen (1900), then Hausdorff at Leipzig (1901), and Landau at Berlin (1902).[5]

On 16 September 1898, Hilbert wrote again to Cantor, proposing that they should meet in Dusseldorf. Cantor replied on 6 October, explaining that he had been on vacation with his family in Oberhof and that Hilbert's letter had not been forwarded to him. After expressing regret that he had been unable to meet Hilbert in Dusseldorf, Cantor was "happy for the interest that you show in set theory. How often during the past year my thoughts have involuntarily turned to you with the question whether your participation opposite me in research would really be granted" [1991, 393]. The rest of the letter was devoted to explaining what he meant by "completed set."

Cantor wrote again four days later, and on that occasion came close to formulating an axiomatization for set theory. It was not an axiomatization because, in trying to answer Hilbert's critique, Cantor proposed to define the notion of set rather than to axiomatize it. Cantor's definition was the following: "By a *completed set* is to be understood any collection all of whose elements *exist together without a contradiction* and hence can be conceived as a *thing by itself*" [1991, 396]. He then argued that many theorems followed from this definition, and the following four in particular:

I. If M is a completed set, then every subset of M is also a completed set.

II. If completed sets are substituted for the elements of a completed set M, then the resulting collection is a completed set.

[4] This course, particularly as it concerns geometry, is discussed in [Toepell 1986, 115–142]. There Toepell points out that, surprisingly and mistakenly, Hilbert believed in 1898 that the Archimedean Axiom is equivalent to continuity in the usual sense [1986, 127].

[5] Hausdorff gave his course to three students. See the report on his call to Leipzig in 1901 by H. Bruns in [Beckert and Purkert 1987, 234]. Shields [1989, 8] thought that Hausdorff's course, delivered in the summer semester of 1901, was "the first lecture course on set theory anywhere in Germany." The previous winter semester of 1900–1901, however, Zermelo had also given such a course. Landau's course of one hour per week was given in the winter semester of 1902–1903 to 84 students, and he offered it again two years later to 119 students [Biermann 1973, 138].

III. If two collections are equivalent [equipotent] and one of them is a completed set, then so is the other.

IV. The collection of *all subsets* of a completed set M is a completed set. [1991, 396]

Cantor added that it seemed to him an "axiomatically certain" proposition that denumerable collections are completed sets, whereas it could then be proved, from III and IV, that the linear continuum is a completed set. Finally, he believed that it could be proved that the alephs $\aleph_1$, $\aleph_2$, ... are completed sets.

These four "theorems" bear a strong resemblance to the later axioms of set theory put forward by Zermelo and others. In particular, Theorems I and IV are close to Zermelo's Axiom of Separation and his Axiom of Power Set (1908), while II and III are forms of the Axiom of Replacement due to Fraenkel (1921) and Mirimanoff (1917) respectively. Two years later, Cantor presented essentially the same definition of completed set, and its consequences, to Dedekind [1991, 407].

On 9 May 1899, Cantor wrote again to Hilbert, in reply to the latter's postcard, noting that he now used the term "consistent" where he had formerly used "completed" and had changed his usage so that what he previously called "completed sets" were now called "sets" or "consistent collections [consistente Vielheiten]". He then remarked that

> it is our common conviction that the "arithmetic continuum" is a "set" in this sense; the question is whether this truth is provable or whether it is an axiom. I now incline more to the latter alternative, although I would gladly be convinced by you of the former. [1991, 399]

A few letters later, Cantor thanked Hilbert on 20 June for his kindness when Cantor, who had come to Göttingen for the unveiling of the Gauss–Weber monument, was a guest in his home. Cantor added, in regard to his third article continuing in the *Annalen* those of 1895 and 1897, that it "has been for years, as you know, *completed in regard to its content*; I hope soon to come to putting it down on paper."[6] On 28 June, Cantor sent some notes on the four possible relations of cardinality between two sets, with the intention that these notes would be passed on to Arthur Schoenflies [1991, 401–403]. In the letter accompanying those notes, Cantor commented on Hilbert's axioms for geometry, and then pointed out a set-theoretic error in Weber's algebra textbook. Weber had claimed [Vol. 2, 2nd ed., 823] that the order-type ω could be characterized by the condition that it have a first element, that every element have an immediate successor, and that every element except the first have an immediate predecessor. Cantor observed that, on the contrary, the order-types $\omega + {}^*\omega + \omega$ and $\omega + {}^*\omega + \omega + {}^*\omega + \omega$ also satisfy that condition. He ended the letter with a conjecture: All denumerable order-types can be generated from the finite and denumerable ordinals, the inverse operation ${}^*\alpha$, and the order-type η of the rationals.

In August 1899, Cantor corresponded with Dedekind about the collection of all alephs and about the distinction between consistent and inconsistent collections (a distinction that had already been the subject of many letters between Cantor and Hilbert). Writing to Hilbert on 15 November, Cantor mentioned that he would already have sent Hilbert that third installment for the *Annalen* if he had received a reply from Dedekind to his letters on the correct foundation for set theory. Cantor saw Dedekind's set-theoretic work, particularly

[6] This letter, like that of 28 June below, is previously unpublished and is found in Hilbert's *Nachlass,* Cod. Ms. D. Hilbert 54.

Was sind und was sollen die Zahlen?, as threatened—in contrast to Cantor's own work which, since 1883, had distinguished between consistent and inconsistent collections: "My foundation stands *diametrically opposed* to the core of his investigations, which consist in the naive assumption that *every well-defined collection or system* is always also a '*consistent system*'" [1991, 414]. He added that he had noticed Dedekind's error a decade before, but had only had the occasion this year to bring it to Dedekind's attention. Continuing on this theme in his letter of 27 January 1900, Cantor explained that his third installment was delayed by the need to answer the conflicting claims of "two great authorities, Gauss and Dedekind," i.e., Gauss's view that all infinite sets are inconsistent and Dedekind's view that inconsistent sets are consistent [1991, 427].[7]

Beginning in May 1899, Hilbert also received letters from Schoenflies about the *Bericht* on set theory which he was then preparing. (Their extant correspondence began with this letter, presumably since Schoenflies left Göttingen in 1899 to take up a position as Ordinarius in Königsberg.) On 28 June, Cantor sent Hilbert various remarks on cardinality which were intended for Schoenflies [1991, 403]. Hilbert apparently informed Schoenflies about Zermelo's discovery of Russell's paradox, since Schoenflies wrote to Hilbert on 17 October 1899: "May I ask you once more to communicate how you have constructed the set such that every set is contained in it as a subset?"[8] And on 28 December 1900, Schoenflies asked Hilbert if he would have Zermelo send any gaps or errors that Zermelo found in the *Bericht* on set theory: "He is perhaps the only person who will really read the *Bericht*." [9]

During 1899–1900 Felix Bernstein, who had previously studied with Cantor at Halle, took courses from Hilbert, Klein, and Zermelo at Göttingen. Bernstein chose Hilbert as the supervisor of his dissertation, which contained a variety of topics in set theory, especially the Cantor–Bernstein Theorem. In its "Lebenslauf" he thanked "Professor Cantor, who introduced me to the field of set theory, for the scientific stimulation received from him and above all for the steady interest and benevolence that he has always shown toward me, as well as Professor Hilbert for the assistance that was my share of him" [1901, 55]. Bernstein was the first student to write a dissertation under Hilbert on set theory, and was the last to do so until Kurt Grelling in 1910.

4. HILBERT'S PARIS LECTURE: 1900

In his paper "Über den Zahlbegriff," dated October 1899 but published in 1900, Hilbert discussed Cantor's work in print for the first time. There Hilbert gave his axioms for the real numbers and suggested (wrongly, as it turned out) that their consistency could be shown by "appropriate modifications of known methods of proof" [1900a, 184]. At the end of the paper he turned to Cantor's work: "If we wished to prove the existence of the collection of all powers (or of all Cantor's alephs), the attempt would fail. In fact, the collection of all powers does not exist, or—in Cantor's manner of expression—the system of all powers is an

[7] Waterhouse [1979] has noted in this journal, correctly, that Gauss's remarks were not directed against set theory, which in any case did not exist at that time. But Cantor saw Gauss's remarks as aimed against the actual infinite, and therefore against set theory. Cantor did so in 1883, when he learned of Gauss's remarks, and continued to do so in his 1900 letter [Cantor 1991, 148].

[8] Cod. Ms. D. Hilbert 355, Letter 5.

[9] Cod. Ms. D. Hilbert 355, Letter 9. By October 1899, Schoenflies knew Hilbert well enough to address him in the letter as "Dear friend."

inconsistent (incomplete) set" [1900a, 184]. Here Hilbert was referring to matters from his unpublished correspondence with Cantor; in January 1900, Cantor had written to Hilbert with what he called the "Axiom of Transfinite Number Theory," which asserted that every collection whose power is an aleph is consistent [1991, 427].

Within that same letter Cantor analyzed what he saw as three different kinds of axioms. First, there were the "logical axioms," which all sciences had in common and which "have been recently treated systematically in the logical calculus" [1991, 426]. Second, there were the "physical axioms of mathematics," i.e., the axioms of geometry and of mechanics. Third, there were the "metaphysical axioms of mathematics," which included above all the axioms of finite and transfinite arithmetic. Cantor's concept of axiom is clearly distinct from Hilbert's, but it is nevertheless significant that they exchanged letters concerning axioms.

In his earlier work on algebraic number theory [1897, 69, 73] and in that on the foundations of geometry (1899), Hilbert had used Dedekind's term "systems" ("Systeme") for "sets" rather than Cantor's term "Mengen." Beginning with the paper "Über den Zahlbegriff," Hilbert began to speak occasionally of "Mengen," although even in his Paris lecture he referred more frequently to "Systeme."

In his Paris lecture of 1900 on his 23 problems, Hilbert praised Cantor. Hilbert argued that in the foundations of analysis and geometry the most important results obtained during the 19th century were "the arithmetic conception of the notion of the continuum in the works of Cauchy, Bolzano, and Cantor" and the discovery of non-Euclidean geometry [1900b, 298]. Hilbert's problems referred explicitly to set theory. The first asked for a proof of Cantor's Continuum Hypothesis that every uncountable set of real numbers has the same cardinal number as the set of all real numbers, as well as for a proof that the real numbers can be well-ordered.

Hilbert's second problem sought a direct consistency proof for the axioms of the real number system (as opposed to a relative consistency proof such as the one that he had given in 1899 for geometry). He was convinced that there was such a direct proof for the real numbers, and equally convinced that this was so in the case of Cantor's infinite ordinals and alephs. By contrast, Hilbert believed that the collection of all of Cantor's alephs was inconsistent [1900b, 301]. Although in 1900 Hilbert was aware of various paradoxes of set theory, he did not show any concern that set theory, or logic, was threatened. The matter would look very different to him in 1904 when he returned to the subject.

5. HILBERT AT HEIDELBERG AND ITS REPERCUSSIONS: 1904–1905

When Hilbert spoke on the foundations of arithmetic to the International Congress of Mathematicians at Heidelberg in August 1904, he criticized earlier researchers in the area, such as Dedekind, Frege, and Kronecker. In discussing Frege, Hilbert mentioned for the first time in his writings the set-theoretic paradoxes (especially Russell's paradox which had just appeared in Frege's *Grundgesetze* (1903)) and the importance of overcoming them. The last to be criticized was Cantor, whose earlier letters Hilbert had in mind:

> *Cantor* was aware of the contradiction just mentioned [of the set of all sets] and gave substance to this awareness by distinguishing between "consistent" and "inconsistent" sets. Since in my view, however, he provides no precise criterion for this distinction, I must describe his conception of this matter as one that still leaves room for *subjective* opinion and hence furnishes no objective certainty. [1905a, 176]

Hilbert then presented his axiomatic method as an objective way out of the paradoxes.

During the summer semester of 1905, while giving a course called "Logical Principles of Mathematical Thought," Hilbert touched on these matters at greater length.[10] "It is the merit of *set theory*," he argued, "to have brought these contradictions [of both set theory and logic] to light" [1905b, 5].

Hilbert compared the development of set theory to that of the infinitesimal calculus and of geometry. Both geometry and the calculus were built, he observed, in a way just opposite to that of a house. The living room was built first, and only later when the security of the structure was in danger were the foundations built. Such was the proper order in mathematics, with a naive period followed by a critical period, in which the foundations must be investigated and strengthened [1905b, 191–194].

> Here it concerns *set theory,* which about 30 years ago began to find its way through hard struggle. It is now in a state very similar to that of the infinitesimal calculus before the critical period got underway; only here it is a matter of questions which tend to be essentially deeper as regards theoretical philosophy than those which arose earlier. [1905b, 194–195]

He compared the use of Aristotelian logic in set theory to the unrestrained use, in infinite series, of the commutative law, which then led to contradictions. "Exactly in this way, by using purely logical operations that elsewhere in mathematics and logic are used without scruple (here it is particularly a matter of collecting many concepts into a general concept), one arrives in set theory at unsolvable contradictions, which may not be clarified with the tools available up to now" [1905b, 195].

By way of example, Hilbert discussed countability, and described as "one of the most beautiful proofs in set theory" Cantor's demonstration that the set of real numbers is uncountable, whereas the rationals are countable [1905b, 196]. But Hilbert's perplexity with the fundamental issues was clear from his handwritten comment in the margin: "Why is the totality of all sets not permissible? Why is the set of all real numbers a permissible collection?" [1905b, 215].

After mentioning Richard's paradox and the liar paradox, Hilbert turned to Russell's paradox (which he credited to Zermelo) and to his own paradox. Hilbert described the Zermelo–Russell paradox as "purely logical," whereas he himself had found a paradox (or contradiction) that he described as "purely mathematical" [1905b, 204, 210]. Regarding his own paradox, Hilbert said: "At first, when I discovered it, I believed that insurmountable difficulties lay in the way of set theory and that on those difficulties it must founder" [1905b, 204]. But now he was sure that, "as always up to now in science," a revision in the foundations would suffice to preserve what was essential. Although he had not published his paradox, it was, he insisted, known to set theorists and to Cantor in particular.

Hilbert's paradox began with the set of natural numbers, and closed it under two operations. The first of these was the operation of taking the union of a family of sets; the second was the "covering" operation on a set M that gives the set of all functions from M to M. This closure was a totality T of sets, and the union of T was called U. He then defined the set F of all functions from U to U. The contradiction resulted from asserting that F is a member of U, by using Cantor's diagonalization process to find a member of F that is

[10] For more details on this course, see [Peckhaus 1990, 58–72; 1994; 1994a].

not in U [1905b, 207].[11] Why Hilbert regarded this as a contradiction is not clear, since he could equally well have regarded it as showing that U is not a member of itself. But it was certainly constructed by using Cantorian tools.

The cause of these various contradictions, in Hilbert's eyes, was taking the "totality of all things" as a consistent set. He added that Dedekind himself no longer regarded this totality as a satisfactory foundation for number, and, as a result, did not permit his *Was sind und was sollen die Zahlen?* to be reprinted. Thus Hilbert seems to have adopted Cantor's view that Dedekind's work was particularly threatened by the paradoxes. Nevertheless, Hilbert credited Dedekind with being the first to recognize, in contrast to Weierstrass and Kronecker, that the natural numbers needed to be "reduced to logic" [1905b, 212].

Hilbert concluded those lectures by describing them as "only a first preliminary attempt" to solve the foundational questions:

> Finally I want to point out once again the great merit that *G. Cantor* and *Dedekind* have earned in regard to all of these questions. One could candidly say that in this context both men are what Newton and Leibniz were for the infinitesimal calculus. Just as Newton and Leibniz began a great development, which first took a decisive step forward with Cauchy and finally arrived at a complete resolution of all difficulties with modern "Weierstrassian" rigor, so also, in set theory and the questions now surrounding it, a new and primarily clarifying development appears to be beginning. [1905b, 272]

Although Hilbert saw the axiomatic method as the heart of this new development, he did not propose an axiomatization of set theory. But such an axiomatization was the fruit of a mathematician heavily influenced by Hilbert: Ernst Zermelo.

In a report written sometime between 1930 and 1933 to the Notgemeinschaft der Deutschen Wissenschaften, Zermelo observed: "Already 30 years ago, when I was a Privatdozent at Göttingen, I began under the influence of D. Hilbert, whom I have the most to thank for my scientific development, to concern myself with the foundations of mathematics, and particularly with the fundamental problems of Cantorian *set theory,* of which I first became fully aware through the mutual cooperation, at that time so fruitful, of the Göttingen mathematicians" [Zermelo in [Moore 1980, 130]].

The earliest fruit of Zermelo's efforts was twofold. First, as mentioned above, during 1900–1901 he gave a course on set theory. Unfortunately, his notes for that course which survive in his *Nachlass* are written in an obscure shorthand, and so it is unlikely that we shall ever know what they say.[12] Second, in 1901 he published his first paper on set theory, giving a new proof of the Cantor–Bernstein Theorem by using infinite series of infinite cardinals [1901].

Zermelo's first major contribution to set theory was his proof of September 1904 that every set can be well-ordered. This proof, based on his Axiom of Choice, was written as a letter to Hilbert for publication in *Mathematische Annalen* [1904]. Zermelo's proof evoked an extremely vigorous reaction on the part of mathematicians from England, France, Germany, Hungary, and Italy (see [Moore 1982, 85–150]).

The role of Hilbert in this set-theoretic controversy is clear from his undated letter of late 1904 to his friend Adolf Hurwitz:

[11] Hilbert's paradox was first discussed in print in [Peckhaus 1990, 52–54].

[12] According to Hans Gebhardt, an expert on German shorthand, Zermelo used the system of shorthand invented by Leopold Arends, a system that is quite difficult and has many exception rules (personal communication from Volker Peckhaus).

> I have spoken my mind to Schoenflies about Zermelo's proof, which recently appeared in the *Annalen,* of the possibility of well-ordering all sets. Schoenflies raised objections against Zermelo which, however, I do not find any more correct than those that Bernstein (Halle) maintains. The whole polemic will appear in one of the next issues of the *Annalen*. The foundations of arithmetic will be considered from the most different perspectives. [Hilbert in [Dugac 1976, 271]]

Thus Hilbert helped stimulate the controversy surrounding Zermelo's proof and the Axiom of Choice by making the *Annalen* available as a sounding board. In particular, Emile Borel wrote to Hilbert on 30 October 1904, expressing reservations about that proof.[13] Borel's reservations were then published in the *Annalen* [1905], as were those of Bernstein [1905], Jourdain [1905], and Schoenflies [1905]. In the 1920s, as we shall see below, Zermelo's Axiom of Choice proved to be very important in Hilbert's *Beweistheorie*.

The next sentence in Hilbert's letter to Hurwitz shows that this controversy over Zermelo's proof was connected in his mind to larger questions about the foundations of set theory and mathematics: "I have long held the view that it is precisely the most important and interesting questions of Dedekind and Cantor which have not yet been settled (and especially not by Weierstrass and Kronecker)" [Hilbert in [Dugac 1976, 271]]. In order, as Hilbert put it, "to have the necessity of reflecting on them coherently," he had announced the course for the summer of 1905 that was discussed above.

During 1905, Zermelo was away from Göttingen for reasons of health, and carried on a correspondence with Hilbert that was concerned, in good part, with set theory. On 7 May, Zermelo wrote that he was resuming his research on set theory, and on 27 June, that he had found that the axiom of choice was needed to prove the equivalence of Dedekind's definition of finiteness with the usual definition using natural numbers. The following day, Zermelo wrote again, giving a new proof of the Equivalence Theorem that used Dedekind's theory of chains. (Three decades later, Emmy Noether would publish the fragment from Dedekind's *Nachlass* that showed he had found it himself in 1887, but, though he had sent it to Cantor in 1897, had never published it.)

6. INTERLUDE: 1906–1916

Hilbert's 1905 attempt in his lecture course to solve the set-theoretic paradoxes and to deal with the foundations of mathematics was not successful, as he realized himself at the time. Nevertheless, he came back to the subject in a lecture course, "Prinzipien der Mathematik," which he gave during the summer semester of 1908, and a second course, "Elemente und Prinzipienfragen der Mathematik" [1910a]. In his 1905 course geometry had played a subsidiary role, serving as an example of an important axiom system. But in the 1908 course, geometry predominated entirely. Despite its title, the course was devoted to the theory of ruler and compass constructions, culminating in Lindemann's proof of the transcendence of π. Only the introduction was devoted to larger questions of foundations, mentioning briefly the work of Cantor, Dedekind, and Weierstrass on the foundations of the real numbers [1908, 3]. Logical and set-theoretic questions were more or less absent, although the axiomatic method was emphasized.

The 1910 course was very similar to the 1908 course, sharing about half the material. Only in the last seven pages of the 1910 course did Hilbert discuss logic and the set-theoretic paradoxes [1910a, 157–163]. The material he gave there was merely a more elementary

[13] Borel's letter is found in Hilbert's *Nachlass*, Cod. Ms. D. Hilbert 39.

version of material from his 1905 course. In 1910 he used the same version of the logical calculus that he had in the 1905 course, viz. Schröder's version of Boolean algebra.[14]

One of the striking features of Hilbert's lectures of 1908 and 1910 was that they did not mention Zermelo's axiomatization of set theory at all. Zermelo had given a lecture on those axioms to the Göttingen Mathematical Society in June 1906 [Zermelo 1906], and in 1907 he submitted an article on them to *Mathematische Annalen,* where it appeared the following year [1908]. Thus Hilbert was well aware of Zermelo's axiomatization. Nevertheless, Hilbert did not mention it in his letter of January 1910 evaluating Zermelo for a professorship at Zurich, although he emphasized the importance of Zermelo's Well-Ordering Theorem, as he had done in 1907 when recommending Zermelo for an extraordinary professorship at Göttingen.[15] It is surprising that Hilbert's earliest reference to Zermelo's axiomatization appears in his Zurich lecture of September 1917, "Axiomatisches Denkern."

7. HILBERT IN 1917: SET THEORY

During the summer semester of 1917 Hilbert gave his f rst course devoted entirely to set theory.[16] The basic view of set theory that he espoused there seems all but a truism now: "Today set theory is that mathematical discipline upon which all others are built" [1917, 124]. Yet at the time it was not a truism but a distinctly controversial statement. Very few mathematicians had made such a statement by 1917, and only a few more had accepted it.

No doubt the earliest to do so was Cantor, who wrote on the subject in his article of 1885, submitted for publication in *Acta Mathematica* but then withdrawn, on the theory of order types:

> By all indications the general *theory of* [*order*] *types* seems to me to promise great usefulness.
> It forms a large and important part of *pure set theory,* therefore also of *pure mathematics,* the latter being in my conception nothing other than *pure set theory*.
> Then it stands in a close relation to the remaining parts of *pure,* but also to *applied, set theory,* as for example the *theory of point sets*, the *theory of functions*, and *mathematical physics*. [1885/1970, 84]

When Cantor rewrote that article for publication in a philosophical journal [1887; 1888], he omitted the passage quoted. Cantor never stated in print that all of mathematics can be built up from set theory. Nor did Cantor give a clear statement of this claim in what survives of his extensive correspondence, perhaps the closest being in his letter of 20 September 1912 to Hilbert [Cantor 1991, 459].

Among those who saw eye to eye with Hilbert on the role of set theory was Hausdorff, who had written in his 1914 book: "Set theory is the foundation of all of mathematics; differential and integral calculus, analysis and geometry continually work in reality, if also perhaps in a veiled manner, with inf nite sets" [1914, 1]. Hausdorff shared with Hilbert the view that set theory was the foundation for all of mathematics, both analysis and geometry.[17] Zermelo, on the other hand, appeared in 1908 to view set theory in a somewhat more restricted fashion:

[14] On Hilbert's logical calculus of 1905, see [Peckhaus 1990, 61–67].

[15] See Hilbert's *Nachlass*, Cod. Ms. D. Hilbert 492, items 3 and 4.

[16] The only other course that Hilbert gave on set theory was one from 1929 called "Mengenlehre," according to the list of his lecture courses in his *Nachlass* (Cod. Ms. D. Hilbert 520). But no version of the 1929 course is known to be extant.

[17] It will come as no surprise that Hausdorff's book was one of only three sources which Hilbert cited in the bibliography for his course.

"Set theory is that branch of mathematics whose task it is to investigate mathematically the fundamental notions of number, order, and function in their original simplicity and thereby to develop the logical foundations of all of arithmetic and analysis" [1908, 261]. Zermelo, unlike Hausdorff, made no mention that set theory was the foundation of geometry. If one accepted that geometry was founded on analysis, then Zermelo's perspective would agree with those of Hausdorff and Hilbert. But it is not certain that Zermelo did so.

Around 1917 there were many mathematicians who did not regard set theory as the proper foundation for all of mathematics. Some of them were suspicious of set theory, such as many French mathematicians, including some who had used parts of it in their research (e.g., René Baire and Henri Lebesgue). Some rejected set theory as a foundation and rejected mathematical logic at the same time, as did L. E. J. Brouwer and Henri Poincaré. Some rejected set theory as a foundation because the proper foundation of mathematics was mathematical logic; here the most striking representative was Bertrand Russell. And many, perhaps most, mathematicians at the time showed no concern with foundational questions at all.

In his lecture course Hilbert regarded set theory as "the creation of a single mathematician, George Cantor" [1917, 1], not mentioning Dedekind's contributions. He spoke of set theory in mythological fashion, echoing Ariadne's help for Theseus against the minotaur, as "the red thread for orientation in the mathematical labyrinth, whose pursuit leads to the source of mathematical, and even general philosophical, knowledge" [1917, 1].

Holding such a high opinion of set theory, Hilbert does surprisingly little with it in his course. It would seem natural, for example, that he would show how within set theory the natural numbers can be built up as sets, then the rational numbers and even the real numbers. But he did not do so. Instead, after showing, e.g., the transcendence of e and π, he presented the real numbers axiomatically [1917, 42–62], as he had done in 1900.[18]

Hilbert then turned to the general notion of set. What is surprising here is that, like Hausdorff in 1914, he did not use Zermelo's axiomatization of set theory but gave a naive definition of set: "A *set* is a system of well-defined things, called elements" [1917, 63]. This was Cantor's definition of set, to which Hilbert had himself objected 20 years earlier.

Next Hilbert considered finite and denumerable sets. In the course of doing so, he introduced the well-known Hilbert's Hotel. Ordinary hotels have only finitely many rooms. When such a hotel is full and another guest comes, that guest cannot be accommodated. But Hilbert's Hotel has as many rooms as there are positive integers. So when Hilbert's Hotel is full and another guest comes, that guest can be accommodated. The occupant of room 1 must move to room 2, the occupant of room 2 to room 3, and so on. Then the new guest can move into room 1 [1917, 66].

After discussing cardinal arithmetic on sets of the power of the continuum and showing, for example, that the set of all continuous real functions has the power of the continuum, Hilbert turned to what he called "two important problems" [1917, 85]. The first was the Trichotomy of Cardinals, i.e., whether the cardinal numbers of any two sets A and B are equal or one is greater than the other. The second was Cantor's Continuum Problem, which he called "very difficult and fundamental" [1917, 118]. In order to discuss these problems, he gave an introduction to ordered sets and especially to well-ordered sets. Here he expressed his attitude toward Cantor's ordinals and also toward philosophy:

[18] He did mention Dedekind cuts in passing [1917, 39].

> It cannot be emphasized enough that the transfinite [ordinal] numbers are something quite real, and something just as definite as the finite ordinal numbers.
> It is an altogether essential superiority of mathematics, in contrast to philosophy, that in its speculations it never wraps itself in fantastic and mystical darkness.[19] [1917, 102]

Using the axiom of choice, he then gave a proof for Zermelo's well-ordering theorem and showed that it implies the trichotomy of cardinals. The well-ordering theorem, he asserted, was "the second great advance which set theory has made since Cantor."[20] He showed his familiarity with the recent literature by mentioning Hartogs' proof (1915) that, axiomatically, the trichotomy of cardinals implies the well-ordering theorem. Finally, he concluded this chapter by discussing in detail how "the well-ordering theorem, strange to say, plays a role in the parallelogram of forces" and implies, as Hamel had shown in 1905, the existence of discontinuous real functions such that $f(x + y) = f(x) + f(y)$.[21]

The final chapter of Hilbert's lecture course was entitled "Applications of Set Theory to Mathematical Logic." Here he argued that set theory is the foundation of mathematics, and so unclarities and contradictions in other branches of mathematics must be corrected within set theory. He acknowledged that, because of the paradoxes, this was not yet the case. Here he regarded Russell's Paradox as "purely logical" [1917, 132], and contrasted it with a paradox that he had found himself and that he described as "purely set-theoretic and mathematical" [1917, 134]. In effect, his unpublished paradox, given in his 1905 course and discussed above, created a model of Zermelo's set theory *as a set*. Hilbert emphasized how it used only mathematical concepts and operations (as opposed to those of pure logic), and added: "I felt so much consternation when I found it that I said to myself: Now the entire method of inference in all of mathematics must be definitively reformed" [1917, 134].

What did Hilbert see as the source of his paradox? He laid the blame on the "genetic definition" of his set M. Although he did not make precise what he meant by "genetic definition," he did remark that such a definition used the word "all" in an illegitimate way [1917, 136]. Among his examples of such an illegitimate genetic definition was "the totality of all objects of my thought," which Dedekind had used in his ill-fated attempt to prove the existence of an infinite set [Hilbert 1917, 142].

He was much happier with Peano's approach to this matter since Peano had explicitly stated axioms for the natural numbers. All other axiom systems, he believed, were reducible to that for the natural numbers. Thinking of Dedekind's and Frege's treatment of arithmetic as a part of logic, he observed: "If we want to found the axioms for the [natural numbers] by reducing them to the laws of logic itself, then we stand before one of the most difficult problems anywhere in mathematics" [Hilbert 1917, 145–146].

Next, in the context of Peano's axioms, Hilbert made a statement that is very surprising in view of his later work: "Since a mathematical existence proof is always understood only to be a proof that the statement in question does not lead to a contradiction, then the existence of the natural numbers cannot be proved in this sense. If, however, one assumes this consistency without proof, then" the Peano postulates determine them up to isomorphism [1917, 148].

19 [Hilbert 1917, 119]. For his more positive views on philosophers, see [Peckhaus 1990, 224].

20 Although Hilbert did not state explicitly what the first advance was, it may well have been the Cantor–Bernstein Theorem, which he referred to as the "Equivalence Theorem of Bernstein ... this fundamental theorem" [1917, 81].

21 [Hilbert 1917, 119]. For a discussion of this surprising connection, see [Moore 1995, 12–14].

What is surprising here is that, in his view, the consistency of the natural numbers cannot be proved, since in his later work he would be particularly concerned to find such a consistency proof for them.

Hilbert concluded his lectures by various examples, such as a Peano curve, to illustrate how set theory was quite useful in geometry and real analysis [1917, 166].

8. HILBERT IN 1917–1918: LOGIC

Those lectures on set theory, delivered in the summer of 1917, were the first sign of Hilbert's serious return to foundational questions. But the lectures remained unpublished. This return first resulted in a publication when, in September 1917, he gave a lecture at Zurich on "Axiomatic Thinking" and printed it in *Mathematische Annalen* the following year. In it he argued strongly for the axiomatic method in both mathematics and physics. He saw axiomatization as appropriate for each branch of mathematics and physics once it had reached a certain maturity.

It was in the Zurich lecture that Hilbert finally, after a decade, mentioned Zermelo's axiomatization of set theory [1918a, 152]. Hilbert saw this axiomatization as limiting the arbitrariness of definitions in set theory, but in such a way that the scope and applicability of set theory were not reduced.

Hilbert's concern with consistency remained strong, and he pointed out that relative consistency proofs could be used in all but two cases: the axioms for the natural numbers and those for set theory. In these two cases a relative consistency proof could only come from logic, and "thus it appears to be necessary to axiomatize logic itself and to show that number theory and set theory are only parts of logic" [1918a, 153]. This statement shows that, contrary to the usual interpretation, Hilbert was then a logicist—in the sense that he believed mathematics can be reduced to logic.

Hilbert praised Russell's axiomatization of logic (the theory of types), but emphasized that this axiomatization needed much further investigation: "the question of the consistency of the natural numbers and of set theory is not an isolated one, but belongs to a broad domain of the most difficult epistemological questions of specifically mathematical coloration" [1918a, 153]. These questions concerned decidability and also the relation between content and formalization in mathematics and logic.

During the winter semester of 1917–1918 Hilbert gave what is certainly his most important lecture course on the foundations of mathematics. The course consisted of two parts, the first on the axiomatic method as illustrated primarily by geometry and the second on mathematical logic [1918b]. Although first part (60 pages) largely duplicated what he had done before, the second part (170 pages) was strikingly new. In contrast to all his earlier courses on logic, this one showed a surprising maturity. Whereas his 1905 course, for example, used a limited Schröderian logic that was clearly inadequate to the needs of mathematics, the 1917–1918 course was based on Russell's theory of types. But Hilbert went far beyond Russell by separating out first-order logic as a subsystem. Over the next four decades, first-order logic would become the overwhelmingly dominant form of mathematical logic.

The importance of this second part of the lectures is shown by the fact that they were the basis for the 1928 book by Hilbert and his student Wilhelm Ackermann, *Grundzüge der theoretischen Logik*. The book had four chapters, entitled "Der Aussagenkalkül", "Der Prädikaten-und Klassenkalkül", "Der engere Funktionenkalkül"(first-order logic), and

"Der erweiterte Funktionenkalkül" (higher-order logic), and these were borrowed word for word from the titles of the chapters in the 1917–1918 course.[22] Yet the book was by no means simply lifted from the lectures. Sometimes lengthy passages were rewritten and then made part of the book, while at other times passages were taken over directly, though in a different order.[23] The third and fourth chapters of the book were very heavily indebted to the lectures, the second was somewhat indebted, and the first was hardly indebted at all. As it happens, the first chapter owed much to Hilbert's lecture course on logic given in [1921].

Set theory played only a secondary role in the 1917–1918 course, as it did in the later book. But one aspect of that role is important in view of more recent developments. Hilbert treated set theory within higher-order logic, rather than within first-order logic (as was done by Skolem in 1923 and as is done now). Hilbert emphasized that higher-order logic was essential for set theory and that first-order logic was not adequate for it.[24]

Hilbert was the first to treat first-order logic as a separate subsystem of logic and to discuss what parts of mathematics could be treated in first-order logic. Among the examples that he gave for which this could be done were elementary number-theory and elementary geometry [1918b, 181–186]. But he was convinced that first-order logic was not adequate for all of mathematics.

His first examples of a mathematical proposition that went beyond first-order logic was from the Peano postulates for the natural numbers, namely the principle of complete induction. He expressed this principle in the form "if a predicate is true for the number 1 and if, in case it is true for a number, it is also true for the next number, then the predicate is true for every number" [1918b, 189]. This principle went beyond first-order logic because, he observed, to formalize it one needs to put in front of it the universal quantifier "for every P," where P ranges over predicates.

Next he turned to the concept of natural number as an illustration of the need to go beyond first-order logic by considering propositional functions whose arguments are themselves propositional functions. Here he treated natural numbers as properties of predicates, more or less as Frege had done in 1884. Then Hilbert added: "From the standpoint of logic, conceiving numbers as properties of predicates is the most obvious thing to do. To the mathematician it is more familiar to consider numbers as *properties of sets*. That objectively these two ways of considering the matter come down to the same thing becomes obvious if we examine the relations between sets and predicates" [1918b, 193–194]. Hilbert considered each set to be defined by a predicate.

What was the relationship between logic and number theory on the one hand and between logic and set theory on the other? Hilbert had considered this matter in his Heidelberg lecture of 1904. There he wrote that "arithmetic is often considered to be a part of logic," no doubt thinking of Frege and perhaps also of Russell. "Yet if we observe attentively," Hilbert added, "we realize that in the traditional treatment of the laws of logic certain fundamental notions from arithmetic are already used, such as the notion of set and, to some extent, that of number

[22] The lectures also contained a fifth chapter, "Ueberleitung zum Funktionen-Kalkül."

[23] By way of example, consider the following passages. In the book, the passage on pp. 68–71. ("Wir wollen nun...folgendermasen ableiten") is taken almost verbatim from pp. 179–186 of the lectures. Likewise, in the book the passage on pp. 109–110 ("Nach Dedekind... *y* die Eigenschaft *P*.") is taken almost word for word from pp. 237–238 of the lectures. Such examples could be multiplied indefinitely.

[24] [Hilbert 1918b, 200]. On the emergence of first-order logic, see [Moore 1988].

as well" [1905, 176]. Thus in 1904 he considered the notion of set to be "arithmetic," i.e., part of arithmetic or number theory in the broad sense. This was in keeping with the general approach whereby Cantor and Dedekind used set-theoretic notions in their reduction of the real numbers to the rationals.

In the 1917–1918 lectures Hilbert's perspective was somewhat different. "The relation of logic to number theory," he now wrote, "is a special case of a more general relation between logic and set theory" [1918b, 195]. He wanted to consider the relationship between logic and set theory more closely, not only because he regarded set theory as important in the foundation of mathematics but also because this relationship played a role in extending first-order logic to higher-order logic.

He then turned to examples of the relationship between sets and predicates, such as the power set of a set and the union (or intersection) of a set of sets. In all of these cases, he reduced the sets to predicates. He had no way of expressing sets directly in his logical symbolism, but he did have a way of expressing predicates. It was for this reason that he regarded it as essential to use higher-order logic so as to be able to formulate set theory within it [1918b, 200].

By examining the paradoxes of logic and set theory (such as Russell's paradox and the liar paradox), Hilbert concluded that it was necessary to use a theory of types. In fact, what he used at first was a version of the simple theory of types [1918b, 218–223]. However, he then showed that Cantor's proof for the existence of an uncountable set (now rephrased in terms of predicates) could not be carried though in this theory. As a way out of this difficulty, Hilbert then adopted Russell's axiom of reducibility. Although Hilbert considered this axiom to be odd, he believed it to be necessary in order to develop set theory and analysis within his logical calculus. Without that axiom, he insisted, every propositional function had to be explicitly given, and so all such functions would only form a countable totality; hence they would not suffice to represent uncountable sets, which are essential in mathematics. With that axiom, he showed that Cantor's proof for the existence of an uncountable set could be carried through [1918b, 229–235]. He ended his lectures by demonstrating how the axiom of reducibility enabled Dedekind's construction of the real numbers to be carried out in Hilbert's system, and concluded that the ramified theory of types is the proper tool through which to develop the foundations of higher mathematics [1918b, 246].

9. HILBERT AND ZERMELO'S AXIOMATIZATION: 1920

When Hilbert next discussed foundational questions, in a course given during the summer semester of 1920, he treated several matters differently. The first of these concerned Kronecker and Poincaré. In 1910 Hilbert had referred to Kronecker's prejudices against set theory, and in 1917 had again mentioned in passing Kronecker's views [1918a, 132]. But in 1920 Hilbert was much more negative toward Kronecker, referring to him as "the most thoroughgoing and radical dictator" in restricting set theory [1920, 17]. Likewise, Hilbert underlined Poincaré's rejection of the actual infinite, set theory, and Zermelo's proof of the well-ordering theorem [1920, 20]. It is very likely that Hilbert took such a hard line toward Kronecker and Poincaré, both of them dead, because of the very living critique of Brouwer and Weyl against set theory and analysis. Indeed, Brouwer and Weyl were mentioned on the very first page of Hilbert's 1920 lectures.

The second important shift in those lectures concerned Zermelo's axiomatization. Although Hilbert had mentioned this axiomatization in passing in his Zurich lecture, these 1920 lectures were the f rst time that he discussed it in detail and treated it very positively: "The person who in recent years has newly founded [set] theory and who has done so, in my view, in the most precise way which is at the same time appropriate to the spirit of the theory, that person is Zermelo" [1920, 21–22]. Hilbert contrasted Kronecker's prohibitions about set theory with the views of Zermelo, who prohibited only as much as was needed to remove the contradictions. Moreover, Hilbert saw Zermelo as making precise Cantor's notion of "consistent set." Since Zermelo's axiomatic set theory included all special mathematical theories (such as number theory and geometry), then by establishing the consistency of Zermelo's theory, one would also establish the consistency of all those special theories [1920, 23].

Hilbert presented in detail a version of Zermelo's axioms within axiomatic logic. All formulas were built up with a symbolic version of the usual connectives such as "or" and "not", together with "for all" and "there exists." Such formulas were built up from two relation symbols, Id(x, y) or identity, and E(x, y) or membership. But the only assumptions made about Id(x, y) and E(x, y) were that they satisf ed the axioms. (For Id(x, y) these axioms made it ref exive, symmetric, and transitive.) He made precise Zermelo's notion of "def nite" property by assuming that "only such expressions are introduced as predicates and relations which are built up from Id(x, y) and E(x, y) by means of the logical connectives and by the substitution of proper names" [1920, 23]. In this framework he presented Zermelo's axioms as Zermelo had given them in 1908. The only exception was in reformulating Zermelo's axiom of elementary sets as what he called "Axiom für das endliche Operieren." This reformulation assumed the existence of a set $M \cup \{A\}$ when M and A were given [1920, 25]. He used this reformulation to give Zermelo's axiom of inf nity in the form that is standard now, rather than the form originally used. In fact, this "Axiom für das endliche Operieren" was one which Zermelo himself had introduced and then abandoned, in a preliminary version of his axiomatization [Moore 1982, 155].

Hilbert then turned to the question of whether, and to what degree, this axiom system for set theory can be deduced from pure logic, and added: "The old problem of reducing all of mathematics to logic gains a strong stimulus through Zermelo's axiom system ..." [1920, 27]. As he had done in the 1917–1918 course, Hilbert underlined the parallelism between sets and predicates. Now he emphasized this parallelism from the perspective of reducing mathematics to logic. The theory of predicates reduced Zermelo's system to pure logic, but, Hilbert emphasized, there was a diff culty surrounding the expression "there is a predicate P." This diff culty, which had been recognized by Russell and Weyl, affected the operation of taking the union of a collection of sets. In general, one could not take such a union. It was necessary not to use "there is a predicate P" in the logical construction of predicates, or else the paradoxes would come back. He concluded that "today the goal of reducing set theory to logic (and thereby also reducing the customary methods of analysis to logic) has not been achieved, and perhaps cannot be achieved in general" [1920, 33]. Thus he was no longer a logicist. But, he insisted, the importance of the axiomatic methods does not depend on such a reduction.

Hilbert's overall assessment of Zermelo's system was that it was "the most brilliant example of a perfected elaboration of the axiomatic method" [1920, 33]. He then turned to criticizing Brouwer and Weyl for crippling this method. From their standpoint, he added, the

Continuum Problem disappears, although this is "one of the most stimulating and ambitious problems in mathematics" [1920, 34].

Hilbert devoted the rest of his course to the consistency of number theory. The weak version of number theory that he considered did not include much more than his 1904 version, and so his consistency proof for it was not really an advance. Yet he was now turning definitively to his emerging *Beweistheorie,* which was to be the subject of several more lecture courses over the next few years.

10. HILBERT AND THE AXIOM OF CHOICE: 1922–1923

The first publication in which Hilbert developed his proof theory in detail beyond what he had done in 1904 (and first spoke explicitly about "Beweistheorie" and "Metamathematik") was his Hamburg lecture [1922]. There he said very little about Zermelo's axiomatization [1922, 162], but Zermelo's axiom of choice played a role: "In mathematical matters there must in principle be no doubt .... It must be possible to formulate Zermelo's set-theoretic axiom of choice in such a way that it is just as reliably valid, and in the same sense, as the arithmetic assertion that $2 + 2 = 4$" [1922, 157]. At this point the axiom of choice was for him an important test case. Soon it played an even more fundamental role.

In a lecture given to the Deutsche Naturforscher-Gesellschaft in September 1922 and published the following year, Hilbert made the axiom of choice the cornerstone of his proof theory. Dismissing the critics of this axiom, he wrote grandly:

> The essential idea on which the axiom of choice is based constitutes a general logical principle, which, even for the first elements of mathematical inference, is necessary and indispensable. When we secure these first elements, we obtain at the same time the foundation for the axiom of choice; both are done by means of my proof theory. [1923, 152]

What Hilbert had in mind was a new postulate for logic. This postulate embodied a version of the axiom of choice, and enabled him to avoid introducing the quantifiers "for all" and "there exists" directly. Instead they could be defined from this new simple postulate. It relied on a new logical function τ which was a kind of "anti-choice" function acting on predicate. His new postulate, which he called "the transfinite axiom," was the following:

$$A(\tau A) \to A(a).$$

In words, this stated that if a predicate $A(x)$ is true for the particular object that the function τ assigns to $A(x)$, then $A(x)$ is true for any object whatsoever [1923, 156].

Hilbert called his axiom "transfinite" because it went beyond what he considered finitary axioms, such as those in propositional logic. He conceived of it as an "ideal element," in the sense that geometry appended points at infinity. He pointed out that his axiom was provable in the case of number theory (since the natural numbers are well-ordered). By way of example, he used his axiom to deduce the axiom of choice for any family of sets of real numbers [1923, 159–165].

The transfinite axiom would soon undergo some changes. In some handwritten notes appended to the *Ausarbeitung* of his lecture course "Logische Grundlagen der Mathematik" from the winter semester of 1922–1923, he made a note to replace τA with εA. Despite this change of notation, the axiom remained the same. Only in his 1926 essay on the infinite did he revise his axiom to state

$$A(a) \to A(\varepsilon A),$$

where εA was now a choice function picking any object b such that $A(b)$ was true. Thus this new axiom, the ε-axiom, stated that if $A(x)$ was true when x was the object a, then $A(x)$ was true when X was the object εA. Hilbert then referred to his function εA as a "transfinite logical choice function."[25]

11. HILBERT AND THE CONTINUUM HYPOTHESIS: 1924–1928

As early as 1900, in his Paris problems, Hilbert had emphasized the importance of Cantor's continuum problem: To determine for which ordinal α the cardinal of the set of all real numbers is $\aleph_\alpha$. The continuum hypothesis (CH) is true if $\alpha = 1$. Cantor himself had shown that α is not 0. In his 1917 lecture course on set theory, Hilbert was aware that Julius König [1905] had shown that α is not ω. Moreover, in this course Hilbert asserted that "the continuum problem is the principal problem in all of set theory" [1917, 154]. He thought it extremely probable that CH is true. In particular, he was convinced that it was unlikely that α was infinite, and even less likely that it was finite but greater than one.

Hilbert's famous essay "Über das Unendliche" of 1926 contained his serious, but fundamentally mistaken, attempt to prove CH. Before turning to that essay, however, we must consider the lecture course bearing the same title which he gave during the winter semester of 1924–1925. Hilbert used the *Ausarbeitung* of this lecture course to prepare the text of the lecture that he delivered on the infinite at Münster in June 1925 and published in [1926]. Among the passages taken over verbatim from the lecture course to the published lecture was the following: "From time immemorial the infinite has stirred the *mind and heart* of man like no other question; the infinite has had a more stimulating and fruitful effect on human understanding than scarcely any other idea; yet the infinite needs to be *clarified* like no other *concept*" [1925, 1; 1926, 163].

The lecture course, which was devoted to the various guises of the infinite (in number theory, geometry, and analysis; in atomic theory, probability theory, and biology; and finally in set theory and logic), ended with a discussion of his proof theory. The final paragraph was devoted to CH. After his claim to have shown the consistency of the proposition that every mathematical problem can be solved, he added: "As far as I see at present, the most important step toward solving the Continuum Problem is taken in this way, and in the affirmative sense" [1925, 136]. Thus he believed himself to have proved CH.

In his Münster lecture he sketched his proof in some detail, something totally lacking in the course. Again he claimed that his proof theory could establish CH, and observed: "The continuum problem is distinguished by its originality and its inner beauty; it has the advantage over other famous problems of combining in itself two qualities: Its solution requires new methods, since the old ones fail to settle it, and, moreover, its solution in and of itself has the greatest interest on account of the result that is to be established" [1926, 180]. Instead of using the real numbers, he employed the set of number-theoretic functions,

[25] [Hilbert 1926, 178]. Now $A(\varepsilon A)$ was taken to be the definition of the existential quantifier "for some x, $A(x)$" rather than the universal quantifier "for every x, $A(x)$."

and concerned himself with the possible definitions of such functions and of transfinite ordinals. For such definitions of functions he introduced what he called "Variablentypen."

Hilbert came back to CH in his Hamburg lecture delivered in July 1927 and published in [1928]. Although he pointed out that his sketch of a proof for CH had been criticized, he made no mention of who had done so [1928, 81].

In an essay on the foundations of arithmetic, Bernays later remarked [1935, 205] that Hilbert, when reprinting his 1926 and 1928 articles on proof theory, omitted those parts devoted to his attempt to prove CH. At the same time Blumenthal referred to Hilbert's 1926 article as "a daring series of inferences ... leading to the solution of the continuum problem" [1935, 424]. Since both Bernays' and Blumenthal's essays appeared in 1935 within the third volume of Hilbert's *Gesammelte Abhandlungen,* it is difficult to know which view Hilbert favored—Bernays', suggesting that Hilbert's "proof" of CH was unsuccessful, or Blumenthal's, asserting the opposite.

In any case, there was no lack of criticism of Hilbert's purported proof, beginning in 1928. Abraham Fraenkel, a ring theorist who became a set theorist, argued that Hilbert had not offered a proof of CH but a possible proof for the consistency of CH. Nevertheless, Fraenkel added, this sketch of a proof could not be regarded as conclusive so long as it was not carried out in detail—especially for some of Hilbert's lemmas, which seemed to be as difficult as CH itself [Fraenkel 1928, 375].

Also in 1928, the Russian analyst Nikolai Luzin raised doubts about Hilbert's proof at the International Congress of Mathematicians in Bologna, where Hilbert himself spoke on foundational questions. Luzin asked whether that proof would be accepted by all mathematicians, both "idealists and realists" as he called them, following Lebesgue's philosophical terminology. Luzin, who sided with realists such as Lebesgue, considered Hilbert's argument to border on Richard's paradox, and hence believed that this argument would be rejected even by idealist mathematicians [1929, 297].

A third mathematician to express doubts about the proof was the Norwegian logician Thoralf Skolem, a thoroughgoing opponent of uncountable sets who espoused a kind of set-theoretic relativism in which all models of set theory are countable. In 1929, at a Scandinavian Mathematical Congress, he mentioned that Hilbert's proof was very problematical: "I have spoken about it with many mathematicians, and I have the impression that very few of them believe his assertions to be correct" [1929, 18]. Skolem referred, for perhaps the first time in print, to Hilbert's famous sentence which begins the present paper: "He [Hilbert] also said that he wants not be driven out of Cantor's paradise. It is very curious to compare his remark with that of Poincaré, mentioned earlier, that set theory is a disease."[26]

Within Hilbert's proof, Skolem objected particularly to the lemma asserting that no "Variablentypen" of rank higher than the second number-class needed to be used. In the end, this objection turned out to be ironic, since Hilbert's lemma, rephrased in the context of Gödel's constructible sets of 1939, turned out to be exactly what was needed to prove the relative consistency of CH.

12. CONCLUSION

Hilbert had spoken to the International Congress at Bologna in 1928, and the resulting article was published in its proceedings and in *Mathematische Annalen* [1930]. There he

[26] [Hilbert 1929, 19]. On this famous "remark" by Poincaré, see [Gray 1991].

again praised Zermelo's axiomatization of set theory and embraced the axiom of choice, in the form of the ε-axiom, as the foundation of his proof theory [1930, 2–3]. He had no further new ideas vis-a-vis set theory, and his mathematical career was almost over. However, his collaborator Bernays did have an important new idea within a Hilbertian framework. In a course at Göttingen on mathematical logic which Bernays gave in the winter semester of 1929–1930, he put forward his axioms for set theory, later known at the Bernays–Gödel system of set theory. Some seven years would pass before Bernays published those axioms [1937].

What did Hilbert contribute to set theory during the three decades in which he was actively involved with it? He did not publish any new set-theoretic results. His attempt to prove CH was abortive, although it probably had some influence on Gödel's relative consistency proof. He did use set-theoretic ideas, especially the axiom of choice, in mathematical logic. Above all, he helped to legitimize set theory as an important part of mathematics, though not as a foundation for mathematics. During the same period he encouraged his students and colleagues to investigate and apply set theory. In this vein, he supervised doctoral dissertations on set theory by Bernstein (1901), Grelling (1910),[27] and the Rumanian mathematician Gabriel Sudan (1925). He exchanged many letters with Cantor, and in both published and unpublished work he repeatedly praised, and expounded, the set-theoretic contributions of Cantor and Dedekind. Finally, he repeatedly thrust the axiom of choice and the continuum hypothesis into the limelight.

The value that Hilbert placed on Cantor's work is particularly clear in the draft of a letter found in his *Nachlass*. Hilbert's undated letter, addressed to an unnamed colleague, was written after Cantor's death in 1918:

> It gives me great joy to contribute to the remembrance of Georg Cantor, who is one of the first in the successive masters of our science. For originality and boldness of thought, there is no mathematician in history—from Euclid to Einstein—who surpassed him. He created something completely new: set theory. Its conceptual methods and applications have by now become the common property of all mathematicians, although I believe that it is only in recent decades that the deepest thoughts of his theory have had their greatest effects. [Purkert 1986, 326]

Thanks in no small measure to Hilbert's influence, during the period 1900–1930 set theory moved from the periphery of mathematics to a position much closer to the center. Hilbert had successfully combated the Kroneckerian influences which, he felt, endangered the proper growth of mathematics.

ACKNOWLEDGMENT

We thank the Niedersächsische Staats- und Universitätsbibliothek Göttingen for permission to quote from various unpublished documents in its possession.

REFERENCES

Beckert, H., & Purkert, W. 1987. *Leipziger mathematische Antrittsvorlesungen,* Teubner-Archiv zur Mathematik, Vol. 8. Leipzig: Teubner.

Bernays, P. 1935. Hilberts Untersuchungen über die Grundlagen der Arithmetik. In [Hilbert 1935], 196–216.

[27] Zermelo played an important role in this dissertation, as Peckhaus [1990, 145] has pointed out. In particular, Grelling's topic came from Zermelo, who also wrote the official referee's report on the dissertation.

———1937. A system of axiomatic set theory, I. *Journal of Symbolic Logic* **2**, 65–77.

Bernstein, F. 1901. *Untersuchungen aus der Mengenlehre*. Halle a. S.: Waisenhaus. [Printed doctoral dissertation, University of Göttingen]

———1905. Über die Reihe der transf niten Ordnungszahlen. *Mathematische Annalen* **60**, 187–193.

Biermann, K.-R. 1973. *Die Mathematik und ihre Dozenten an der Berliner Universität. Stationen auf dem Wege eines mathematischen Zentrums von Weltgeltung*. Berlin: Akademie-Verlag.

Blumenthal, O. 1935. Lebensgeschichte. In [Hilbert 1935], 388–429.

Borel, E. 1898. *Leçons sur la théorie des fonctions*. Paris: Gauthier–Villars.

———1905. Quelques remarques sur les principes de la theorie des ensembles. *Mathematische Annalen* **60**, 194–195.

Cantor, G. 1882. Über unendliche lineare Punktmannichfaltigkeiten, 3. *Mathematische Annalen* **20**, 113–121.

———1885/1970. Principien einer Theorie der Ordnungstypen. Erste Mitteilung. *Acta Mathematica* **124**, 65–107. [First publication of an 1885 paper, edited by I. Grattan-Guinness]

———1887. Mitteilungen zur Lehre vom Transf niten. *Zeitschrift für Philosophie und philosophische Kritik* **91**, 81–125, 252–270.

———1888. Mitteilungen zur Lehre vom Transf niten. *Zeitschrift für Philosophie und philosophische Kritik* **92**, 240–265.

———1895. Beiträge zur Begründung der transf niten Mengenlehre, I. *Mathematische Annalen* **46**, 481–512.

———1897. Beiträge zur Begrüdung der transf niten Mengenlehre, II. *Mathematische Annalen* **49**, 207–246.

———1991. *Briefe*. H. Meschkowski & W. Nilson, Eds. Berlin: Springer-Verlag.

Ferreirós, J. 1993. On the Relations between Georg Cantor and Richard Dedekind. *Historia Mathematica* **20**, 343–363.

———1999. *Labyrinth of Thought: A History of Set Theory and Its Role in Modern Mathematics*. Basel: Birkhäuser.

Fraenkel, A. A. 1928. *Einleitung in die Mengenlehre,* 3rd ed. Berlin: Springer-Verlag.

Gray, J. 1991. Did Poincaré Say "Set Theory Is a Disease"? *Mathematical Intelligencer* **13**, No. 1, 19–22.

Hausdorff, F. 1914. *Grundzüge der Mengenlehre*. Leipzig: Veit.

Hilbert, D. 1891. Über die stetige Abbildung einer Linie auf ein Flächenstück. *Mathematische Annalen* **38**, 459–460.

———1894. Über die Zerlegung der Ideale eines Zahlkörpers in Primideale. *Mathematische Annalen* **44**, 1–8.

———1897. Die Theorie der algebraischen Zahlkörper. *Jahresbericht der Deutschen Mathematiker-Vereinigung* **4**, 175–546. [Pagination follows the reprinting in [1935].]

———1898. *Ueber den Begriff des Unendlichen*. Lecture course delivered at Göttingen. SUB Göttingen, Cod. Ms. D. Hilbert 597.

———1899. *Grundlagen der Geometrie*. Leipzig: Teubner. [This f rst edition was part of the *Festschrift zur Feier der Enthüllung des Gauß-Weber-Denkmals in Göttingen,* pp. 1–92, while later editions were printed separately.]

———1900a. Über den Zahlbegriff. *Jahresbericht der Deutschen Mathematiker-Vereinigung* **8**, 180–184.

———1900b. Mathematische Probleme. Vortrag, gehalten auf dem internationalen Mathematiker-Kongreß zu Paris 1900. *Nachrichten von der Königlich Gesellschaft der Wissenschaften zu Göttingen, Mathematisch-physicalische Klasse,* 253–297. [Pagination follows the reprinting in [1935].]

———1905a. Über die Grundlagen der Logik und der Arithmetik. In *Verhandlungen des dritten internationalen Mathematiker-Kongresses in Heidelberg vom 8. bis 13. August 1904,* pp. 174–185. Leipzig: Teubner.

———1905b. *Logische Principien des mathematischen Denkens*. Lecture course delivered at Göttingen (summer semester 1905) and written up by E. Hellinger (library of the Mathematical Institute of Gšttingen University).

———1908. *Prinzipien der Mathematik*. Lecture course delivered at Göttingen, summer semester 1908 (library of the Mathematical Institute of Gšttingen University).

———1910a. *Elemente und Prinzipienfragen der Mathematik*. Lecture course delivered at Göttingen in the summer semester of 1910 and written up by R. Courant (library of the Mathematical Institute of Gšttingen University).

———1910b. Hermann Minkowski. *Mathematische Annalen* **68**, 445–471.

———1917. *Mengenlehre*. Lecture course delivered at Göttingen, summer semester 1917 (library of the Mathematical Institute of Göttingen University).

———1918a. Axiomatisches Denken. *Mathematische Annalen* **78**, 405–415. [Pagination follows the reprinting in [1935].]

———1918b. *Prinzipien der Mathematik*. Lecture course delivered at Göttingen in the winter semester of 1917–1918 and written up by P. Bernays (library of the Mathematical Institute of Göttingen University).

———1920. *Probleme der mathematischen Logik*. Lecture course delivered at Göttingen in the summer semester of 1920 and written up by M. Schönf nkel and P. Bernays (library of the Mathematical Institute of Gšttingen University).

———1921. *Logik-Kalkül*. Lecture course delivered at Göttingen in the winter semester of 1920–1921 and written up by Paul Bernays (library of the Mathematical Institute of Göttingen University).

———1922. Neubegründung der Mathematik. Erste Mitteilung. *Abhandlungen aus dem Mathematischen Seminar der Hamburgischen Universität* **1**, 157–177. [Pagination follows the reprinting in [1935].]

———1923. Die logischen Grundlagen der Mathematik. *Mathematische Annalen* **88**, 151–165.

———1925. *Über das Unendliche*. Lecture course delivered at Göttingen in the winter semester of 1924–1925 and written up by Lothar Nordheim (library of the Mathematical Institute of Göttingen University).

———1926. Über das Unendliche. *Mathematische Annalen* **95**, 161–190.

———1928. Die Grundlagen der Mathematik. *Abhandlungen aus dem Mathematischen Seminar der Hamburgischen Universität* **6**, 65–85.

———1930. Probleme der Grundlegung der Mathematik. *Mathematische Annalen* **102**, 1–9.

———1932. *Gesammelte Abhandlungen,* Vol. 1, *Zahlentheorie*. Berlin: Springer-Verlag.

———1935. *Gesammelte Abhandlungen,* Vol. 3: *Analysis, Grundlagen der Mathematik, Physik, Verschiedenes, Lebensgeschichte*. Berlin: Springer-Verlag.

Hilbert, D., & Ackermann, W. 1928. *Grundzüge der theoretischen Logik*. Berlin: Springer-Verlag.

Hurwitz, A. 1883. Beweis des Satzes, dass eine einwerthige Function beliebig vieler Variablen, welche überall als Quotient zweier Potenzreihen dargestellt werden kann, eine rationale Function ihrer Argumente ist. *Journal für die reine und angewandte Mathematik* **95**, 201–206.

Jech, T. J. 1978. *Set Theory.* New York: Academic Press.

Jourdain, P. E. B. 1905. On a proof that every aggregate can be well-ordered. *Mathematische Annalen* **60**, 464–470.

Klein, F. 1928. *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert* (Berlin: Springer), Teil I. [Pagination follows the 1979 translation, *Development of Mathematics in the 19th Century*. Brookline, MA: Math-Sci Press.]

Luzin, N. N. 1929. Sur les voies de la théorie des ensembles. In *Atti del Congresso internationale dei matematici, 3–10 settembre, Bologna,* Vol. 1, pp. 295–299. Bologna: Zanichelli.

Medvedev, F.A. 1965. *Razvitiye teorii mnozhestv b XIX beke* (*The Development of Set Theory in the 19th Century*). Moscow: Nauka.

Mehrtens, H. 1990. *Moderne—Sprache—Mathematik: Eine Geschichte des Streits um die Grundlagen der Disziplin und des Subjekts formaler Systeme*. Frankfurt: Suhrkamp.

Minkowski, H. 1973. *Briefe an David Hilbert*. L. Rüdenberg & H. Zassenhaus, Eds. Berlin: Springer-Verlag.

Moore, G. H. 1980. Beyond f rst-order logic: The historical interplay between mathematical logic and axiomatic set theory. *History and Philosophy of Logic* **1**, 95–137.

———1982. *Zermelo's Axiom of Choice: Its Origins, Development and Influence*. New York: Springer-Verlag.

———1988. The emergence of f rst-order logic. In *History and Philosophy of Modern Mathematics,* W. Aspray & P. Kitcher, Eds., pp. 95–135. Minneapolis: University of Minnesota Press.

———1995. The axiomatization of linear algebra: 1875–1940. *Historia Mathematica* **22**, 262–303.

Moore, G. H., & Garciadiego, A. 1981. Burali-Forti's paradox: A reappraisal of its origins. *Historia Mathematica* **8**, 319–350.

Peano, G. 1890. Sur une courbe, qui remplit toute une aire plane, *Mathematische Annalen* **36**, 157–160.

Peckhaus, V. 1990. *Hilbertprogramm und kritische Philosophie. Das Göttinger Modell interdisziplinärer Zusammenarbeit zwischen Mathematik und Philosophie*. Göttingen: Vandenhoeck & Ruprecht.

———1994. Logic in transition: The logical calculi of Hilbert (1905) and Zermelo (1908). In *Logic and Philosophy of Science in Uppsala: Papers from the 9th International Congress of Logic, Methodology and Philosophy of Science,* D. Prawitz & D. Westerstahl, Eds., pp. 311–323. Dordrecht: Kluwer.

———1994a. Hilbert's axiomatic programme and philosophy. In *The History of Modern Mathematics,* E. Knobloch & D. E. Rowe, Eds., Vol. III, pp. 91–112. Boston: Academic Press.

Purkert, W. 1986. Georg Cantor und die Antinomien der Mengenlehre. *Bulletin de la Société Mathématique de Belgique,* Series A, **38**, 313–327.

Schoenf ies, A. 1905. Über wohlgeordnete Mengen. *Mathematische Annalen* **60**, 181–186.

Shields, A. 1989. Felix Hausdorff: Grundzüge der Mengenlehre. *Mathematical Intelligencer* **11**, No. 1, 6–10.

Skolem, T. 1929. Über die Grundlagendiskussionen in der Mathematik. In *Proceedings of the 7th Scandanavian Mathematical Congress,* pp. 3–21. Oslo: Brøggers.

Toepell, M.-M. 1986 *Über die Entstehung von Hilberts Grundlagen der Geometrie*. Göttingen: Vandenhoeck & Ruprecht.

von Neumann, J. 1925. Eine Axiomatisierung der Mengenlehre, *Journal für die reine und angewandte Mathematik* **154**, 219–240.

Waterhouse, W. C. 1979. Gauss on Inf nity. *Historia Mathematica* **6**, 430–436.

Zermelo, E. 1901. Über die Addition transf niter Kardinalzahlen, *Nachrichten von der Königlich Gesellschaft der Wissenschaften zu Göttingen, Mathematisch-physikalische Klasse,* 34–38.

———1904. Beweis, daß jede Menge wohlgeordnet werden kann (Aus einem an Herrn Hilbert gerichteten Briefe). *Mathematische Annalen* **59**, 514–516.

———1906. [Abstract of lecture at Göttingen on 19 June 1906]. *Jahresbericht der Deutschen Mathematiker-Vereinigung* **15**, 406–407.

———1908. Untersuchungen über die Grundlagen der Mengenlehre, I. *Mathematische Annalen* **65**, 261–281.