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HILBERT'S PROGRAMS: 1917–1922

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Abstract. Hilbert's finitist program was not created at the beginning of the twenties solely to counteract Brouwer's intuitionism, but rather emerged out of broad philosophical reflections on the foundations of mathematics and out of detailed logical work; that is evident from notes of lecture courses that were given by Hilbert and prepared in collaboration with Bernays during the period from 1917 to 1922. These notes reveal a dialectic progression from a critical logicism through a radical constructivism toward finitism; the progression has to be seen against the background of the stunning presentation of mathematical logic in the lectures given during the winter term 1917/18. In this paper, I sketch the connection of Hilbert's considerations to issues in the foundations of mathematics during the second half of the 19th century, describe the work that laid the basis of modern mathematical logic, and analyze the first steps in the new subject of proof theory. A revision of the standard view of Hilbert's and Bernays's contributions to the foundational discussion in our century has long been overdue. It is almost scandalous that their carefully worked out notes have not been used yet to understand more accurately the evolution of modern logic in general and of Hilbert's Program in particular. One conclusion will be obvious: the dogmatic formalist Hilbert is a figment of historical (de)construction! Indeed, the study and analysis of these lectures reveal a depth of mathematical-logical achievement and of philosophical reflection that is remarkable. In the course of my presentation many questions are raised and many more can be explored; thus, I hope this paper will stimulate interest for new historical and systematic work.

Introduction. At the very end of a sequence of lectures he gave in 1919 under the title *Natur und mathematisches Erkennen*, Hilbert emphasized that some physical paradoxes had directed his discussion away from the methods

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of physics to the general philosophical problem, "whether and how it is possible to understand our thinking by thinking itself and to free it from any paradoxes".[1] Hilbert saw this problem also at the basis of his work in mathematical logic. One might ask polemically, whether there is more to Hilbert's contribution to that problem than the narrow and technical consistency program pursued in Göttingen during the twenties. A critical reader of the relevant historical and philosophical literature, and even of some of Hilbert's own writings, almost certainly would be inclined to give a negative answer.

During the last ten or fifteen years a more positive and more accurate perspective on the work of the Hilbert School has been emerging, for example, in papers by Feferman, Hallett, Sieg, and Stein. This has been achieved mainly by bringing out the rich context in which the published work is embedded: important connections have been established, on the one hand, to foundational work of the 19th century (that had been viewed as largely irrelevant) and, on the other hand, to a general reductive program (that evolved out of Hilbert's Program and underlies implicitly most modern proof theoretic investigations).[2] However, it is crucial to gain a better understanding of the development of Hilbert's thought on the foundations of arithmetic, where arithmetic is understood in a broad sense that includes elementary number theory and reaches all the way to set theory. Admittedly, this is just one aspect of Hilbert's work on the foundations of mathematics, as it disregards the complex interactions with his work on the foundations of geometry and of the natural sciences. It is, nevertheless, a most significant aspect, as it reveals a surprising internal dialectic progression (in an attempt to address broad philosophical issues) and throws a distinctive new light on the development of modern mathematical logic.

Standard wisdom partitions Hilbert's work on the foundations of arithmetic with some justification into two periods. The first period is taken to extend from 1900 to 1905, the second from 1922 to 1931. The periods are marked by dates of outstanding publications. Hilbert published in 1900 and 1905 respectively *Über den Zahlbegriff* and *Über die Grundlagen der Logik und Arithmetik*. According to the standard view, the considerations of the latter paper were taken up around 1921, were quickly expanded into the proof theoretic program, and were exposed first in 1922 through Hilbert's *Neubegründung der Mathematik* and Bernays's *Über Hilberts Gedanken zur Grundlegung der Arithmetik*. This "continuity" is pointed out also by Hilbert

[1] This quotation is found on page 117 of [61].

[2] [1] and [99] contain much valuable information concerning earlier roots of Hilbert's foundational work around the turn of the century, in Toepell's case for geometry. Peckhaus gives in his [81] a detailed account of subsequent developments in Göttingen up to 1917; this includes a discussion of some of Hilbert's lectures (e.g., those of 1905), but also of Hilbert's "Personalpolitik" concerning Zermelo and Nelson.

and Bernays without emphasizing their early mathematical logical work or the exploration of alternative foundational perspectives. Finally, it is argued that the pursuit of the program was halted in 1931 by Gödel's paper *Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme I.*

This partition of Hilbert's work does not include, or accommodate easily, the programmatic paper *Axiomatisches Denken* published in 1918. The paper had been presented already in September 1917 to the Swiss Mathematical Society in Zürich and advocates a logicist reduction of mathematics. In sharp contrast, the 1922 papers by Hilbert and Bernays seem to set out the philosophical and mathematical-logical goals of *the* Hilbert Program. This remarkable progression is not at all elucidated by publications, but it can be analyzed by reference to notes for courses Hilbert gave during that period in Göttingen. The lectures were prepared with the assistance of Bernays who wrote all the notes, except that Schönfinkel helped prepare the notes for the summer term 1920. I will discuss this development, after sketching in Part A connections to foundational investigations of the 19th century; Part B describes the strikingly novel treatment of general logical and meta-mathematical issues, whereas Part C is devoted to the emergence of specifically proof theoretic investigations. Thus, here is a first attempt to bridge the gap in the published record between Hilbert's Zürich Lecture and the proof theoretic papers from 1922; the study and analysis of these lectures reveal a depth of mathematical-logical achievement and of philosophical reflection that is remarkable.[3] In the course of my presentation many questions are raised and many more can be explored; thus, I hope this paper will stimulate interest for new historical and systematic work.

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[3]There is some work that covers this period of Hilbert's foundational investigations. Abrusci, in his [2], lists 25 lectures concerned with foundational matters that were given by Hilbert between 1898 and 1933. On pp. 335–8 he attempts to give a rough impression of the richness of these lectures by highlighting the contents of some. He emphasizes that the lectures "testify the remarkable Hilbert's interest [sic] in the foundations of mathematics during the years 1905–1917" and that the 1917-8 notes are the beginnning of the golden period of Hilbert's logical and foundational investigations. This paper is a very brief, tentative description; it promises, but does not provide, a sustained analysis.—Peckhaus describes in his [82] Hilbert's development from "Axiomatik" to "Beweistheorie", but disregards all the lectures between 1917 and 1922 and mentions, for the step to finitist proof theory, only Hilbert's publications starting with [65].—Moore discusses in Section 8, pp. 113–6, of his [79] the 1917-8 lectures as part of a general account of the "emergence of first-order logic"; however, the really novel aspects of these lectures, emphasized below in Part B, are not brought out. As far as the emergence of proof theory is concerned, Moore's brief discussion starts with the first publication of Hilbert's investigations in 1922. The account of Hilbert's (and Bernays') contribution to the emergence of mathematical logic is deepened in [80]. Moore focuses also here more broadly on "standard" metalogical issues, whereas I concentrate on (finitist) consistency and the emergence of proof theory. There is a significant difference in our overall analyses of the developments between 1917 and 1922; cf. note 45.

Part A. Before 1917: axiomatic method and consistency. Hilbert viewed the *axiomatic method* as holding the key to a systematic organization of any sufficiently developed subject; he also saw it as providing the basis for metamathematical investigations of independence and completeness issues and for philosophical reflections. However, *consistency* was Hilbert's central concern ever since he turned his attention to the foundations of analysis in the late nineties of the last century. For analysis, Dedekind and Kronecker had put forward two radically different kinds of arithmetizations in response to Dirichlet's demand that *any* theorem of algebra and higher analysis be formulated as a theorem about natural numbers.

A1. Arithmetization strict and logical. Kronecker admitted as objects of analysis only natural numbers and constructed from them, in now well-known ways, integers, rationals, and even algebraic reals. The general notion of irrational number was rejected, however, because of two restrictive methodological requirements: concepts must be decidable, and existence proofs must be carried out in such a way that they present objects of the appropriate kind. For Kronecker there could be no infinite mathematical objects, and geometry was banned from analysis even as a motivating factor. (Hilbert's critical, but also appreciative discussion in his lectures during the summer term 1920 emphasizes these broad methodological points.) Clearly, this procedure is strictly arithmetic, and Kronecker believed that analysis could be re-obtained by following it. It is difficult for me to judge to what extent Kronecker pursued a program of developing parts of analysis in an elementary, constructive way. Such a program is not chimerical, as mathematical work during the last two decades has established that a good deal of analysis and algebra can be done in conservative extensions of primitive recursive arithmetic.

In contrast to Kronecker, Dedekind defined a general notion of real number, motivated cuts explicitly in geometric terms, and used infinite sets of natural numbers as respectable mathematical objects. The principles underlying the definition of cuts were for Dedekind logical ones which allowed the "creation" of new numbers, such that their system has "the same completeness or ... the same continuity as the straight line". Dedekind emphasized in a letter to Lipschitz that this *continuous completeness* is essential for a scientific foundation of the arithmetic of real numbers, as it relieves us in analysis of the necessity to assume existences without sufficient proof. Indeed, it provides the answer to Dedekind's rhetorical question:

> How shall we recognize the admissible existence assumptions and distinguish them from the countless inadmissible ones ... ? Is this to depend only on the success, on the accidental discovery of an internal contradiction?[4]

[4] Letter to Lipschitz of July 27, 1876; in [33], p. 477.

Dedekind is considering here assumptions about the existence of individual real numbers. Such assumptions are not needed when a complete system is investigated: the question concerning the existence of particular reals is shifted to the question concerning the existence of their *complete system*.

If we interpret the essay *Stetigkeit und Irrationale Zahlen* in light of Dedekind's considerations in *Was sind und was sollen die Zahlen*? and his letter to Keferstein, we can describe his procedure in an extremely schematic and yet accurate way: the essays present informal analyses that lead with compelling directness to the axioms for a *complete ordered field*, respectively to those for a *simply infinite system*. Then models for these axioms are given in logical terms; thus, the consistency of the axiomatically characterized notions seemed to be secured on logical grounds.[5] With respect to simply infinite systems Dedekind wrote to Keferstein on February 27, 1890:

> After the essential nature of the simply infinite system, whose abstract type is the number sequence N, had been recognized in my analysis ..., the question arose: does such a system *exist* at all in the realm of our ideas? Without a logical proof of existence it would always remain doubtful whether the notion of such a system might not perhaps contain internal contradictions. Hence the need for such a proof (articles 66 and 72 of my essay).[6]

Dedekind viewed these considerations not as specific for the foundational context of his essays, but rather as paradigmatic for a mathematical procedure to introduce axiomatically characterized notions.[7]

A2. Consistency of sets and theories[8]. The origins of Hilbert's Program can be traced back to these foundational problems in general and to Dedekind's proposed solution in particular. Hilbert turned his attention to them, as he recognized that some observations of Cantor had an absolutely devastating effect on Dedekind's essays.[9] Cantor had remarked in letters, dated September 26 and October 2, 1897, that he had been led "many years ago" to the necessity of distinguishing two kinds of totalities (multiplicities, systems), namely *absolutely infinite* and *completed* ones. In his letter to Dedekind of July 28, 1899, totalities of the first kind are called *inconsistent* and those of the second kind *consistent*. This distinction avoids, in a trivial way, the

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[5]That such a proof is intended also in Stetigkeit und irrationale Zahlen is most strongly supported by the discussion in [27], p. 338.

[6]In [101], p. 101. The essay Dedekind refers to is [27].

[7]Cf. the discussion of ideals in [26], where he draws direct parallels to the steps taken here. [26], pp. 268–269, in particular the long footnote on p. 269.

[8]Note that Hilbert talked in his [51] about consistent sets; if he had followed strictly Cantor's terminology, he would have mentioned only sets—which are defined as consistent multiplicities.

[9]In particular in section 66 of *Was sind und was sollen die Zahlen*. That is clear from Cantor's response of November 15, 1899 to a letter of Hilbert's (presumably not preserved).

contradiction that arose from assuming, as Dedekind had done, that the totality of all things is consistent.

In 1899 Hilbert wrote *Über den Zahlbegriff*, his first paper addressing foundational issues of analysis. He intended—never too modest about aims—to rescue the set theoretic arithmetization of analysis from the Cantorian difficulties. To that end he gave a categorical axiomatization of the real numbers based on Dedekind's work, claimed that its consistency can be proved by a "suitable modification of familiar methods"[10], and remarked that such a proof constitutes "the proof for the existence of the totality of real numbers or—in the terminology of G. Cantor—the proof of the fact that the system of real numbers is a consistent (completed) set". In his subsequent Paris address Hilbert went even further and claimed that the existence of Cantor's higher number classes and of the alephs can be proved in an analogous way.[11] For the real numbers he suggested more specifically that the familiar inference methods of the theory of irrational numbers have to be modified with the aim of obtaining a "direct" consistency proof; such a direct proof would show that one cannot obtain from the axioms, by means of a finite number of logical inferences, results that contradict each other.[12] Hilbert realized soon that the consistency problem, even for the theory of real numbers, could not be solved as easily as he had thought. Bernays commented later that "the considerable difficulties of this task emerged" when Hilbert actually tried to prove these consistency claims.

[10][50], p. 261. The German original is: "Um die Widerspruchsfreiheit der aufgestellten Axiome zu beweisen, bedarf es nur einer geeigneten Modifikation bekannter Schlußmethoden." [12] reports on pp. 198–199 in very similar words, but with a mysterious addition: "Zur Durchführung des Nachweises gedachte Hilbert mit einer geeigneten Modifikation der in der Theorie der reellen Zahlen angewandten Methoden auszukommen."

[11]Cantor, by contrast, insists in his letter to Dedekind of August 28, 1899 that even finite multiplicities cannot be proved to be consistent. The fact of their consistency is a simple, unprovable truth—"the axiom of arithmetic"; the fact of the consistency of multiplicities that have an aleph as their cardinal number is in exactly the same way an axiom, the "axiom of the extended transfinite arithmetic". [110], pp. 447–8.

[12]This is part of Hilbert's formulation of the second problem; more fully we find: "Vor allem aber möchte ich unter den zahlreichen Fragen, welche hinsichtlich der Axiome gestellt werden können, dies als das wichtigste Problem bezeichnen, *zu beweisen, daß man auf Grund derselben mittels einer endlichen Anzahl von logischen Schlüssen niemals zu Resultaten gelangen kann, die mit einander in Widerspruch stehen*." Howard Stein pointed out to me that a syntactic view of the consistency problem is entertained here by Hilbert, though there is no indication of the logical matters that have to be faced: that is done vaguely and programmatically in 1904, but concretely and systematically only in the winter term 1917/18. Indeed, already in section 9 (pp. 19/20) of [49] a syntactic formulation of consistency is given; however, the immediately following argument for consistency is a thoroughly semantic one: "Um dies [die Widerspruchslosigkeit, WS] einzusehen, genügt es, eine Geometrie anzugeben, in der sämtliche Axiome . . . erfüllt sind."—The general problematic, indicated here only indirectly, was most carefully analyzed by Bernays in his [13].

In his address to the International Congress of Mathematicians, Heidelberg 1904, Hilbert examined more systematically various attempts at providing foundations for analysis, including Cantor's. The critical attitude towards Cantor, that was implicit in *Über den Zahlbegriff*, was made explicit here. Hilbert accused Cantor of not giving a rigorous criterion for distinguishing consistent from inconsistent totalities, as Cantor's conception "leaves latitude for subjective judgment and therefore affords no objective certainty".[13] He suggested again that consistency proofs for suitable axiomatizations provide an appropriate remedy and described in greater detail how he envisioned such a proof: develop logic together with analysis in a common frame, so that proofs can be viewed as *finite mathematical* objects; then show that such formal proofs cannot lead to a contradiction. Here we have seemingly in very rough outline Hilbert's Program; but it should be noticed that the point of consistency proofs is still to guarantee the existence of sets, that the logical frame is only vaguely conceived, and that a reflection on the mathematical means admissible in consistency proofs is completely lacking. Indeed, as we will see, the path to the program is still rather circuitous.

One reason for the circuitous route is, so it seems, the critique of the enterprise by Poincaré; the latter agrees with Hilbert on the fundamental point that mathematical existence can mean only freedom from contradiction: "If therefore we have a system of postulates, and if we can demonstrate that these postulates imply no contradiction, we shall have the right to consider them as representing the definition of one of the notions entering therein."[14] But any such proof (for systems that involve an infinite number of consequences) requires the principle of complete induction; this point is re-emphasized over and over in Poincaré's remarks on Hilbert's 1905 paper. At one point he summarizes matters as follows:

> So, Hilbert's reasoning not only assumes the principle of induction, but it supposes that this principle is given us not as a simple definition, but as a synthetic judgment *a priori*.
>
> To sum up:
>
> A demonstration [of consistency] is necessary.
>
> The only demonstration possible is the proof by recurrence.
>
> This is legitimate only if we admit the principle of induction and if we regard it not as definition but as a synthetic judgment.[15]

[13][52] in [101], p. 131.

[14][83], p. 1026. Having discussed Mill's view of (mathematical) existence and characterizing the latter's opinion as "inadmissible", Poincaré writes in the immediately preceding paragraph: "Mathematics is independent of the existence of material objects; in mathematics the word exist can have only one meaning, it means free from contradiction. . . . in defining a thing, we affirm that the definition implies no contradiction."

[15][85], p. 1059. Howard Stein raised in discussion the question whether Poincaré's criticism had the effect of postponing the development of proof theory; it seems to me that indeed it did.

Only after exploring alternative foundational approaches did Hilbert "return" to proof theory and address explicitly Poincaré's objection; I will resume that discussion in C1 below.

A3. Developments from 1905 to 1917. In contrast to an almost universally held opinion, Hilbert continued to be concerned with the foundations of mathematics. There is no record of publications supporting this claim, but Hilbert gave a number of lecture courses on the topic between 1905 and 1917, and extensive notes of his lectures are available. The lectures on *Logische Prinzipien des mathematischen Denkens* from the summer term 1905 are preserved in two different sets of notes, one of which was prepared by Max Born; Hilbert lectured on *Zahlbegriff und Prinzipienfragen der Mathematik* (Summer 1908), on *Elemente und Prinzipienfragen der Mathematik* (Summer 1910), on *Grundlagen der Mathematik und Physik* (Summer 1913), *Prinzipien der Mathematik* (Summer 1913), *Probleme und Prinzipien der Mathematik* (Winter 1914/15), and on *Mengenlehre* (Summer 1917). Let me describe paradigmatically some crucial features of the 1910-lectures; the notes were written by Richard Courant.

The lectures start out with a *plan* dividing the course into three parts. Part I is to deal with "The Quadrature of the Circle and Related Problems" and is clearly based on lectures Hilbert had given repeatedly under that title, e.g., in 1904; Part II is called "Problems of Analysis and Mechanics"; the content of Part III is indicated by "Critique of Basic Notions. Axiomatic Method. Logic and Mathematical Thought". The final result, shaped no doubt by the exigencies of ordinary academic life, is quite different and develops only a fraction of what was announced for Part III. Quantitatively, one finds six handwritten pages of a total of 162 pages under the heading "Chapter 5: *On Logical Paradoxes and Logical Calculus*". Yet, what there is—is of genuine interest. Hilbert discusses first Richard's paradox and dismisses it as easily solvable:

> One just has to look at this whole argument without prejudice to recognize that it is completely inadmissible. The ambiguous, subjective character of language does not allow us to assert the exact claim that certain words must always refer to one and the same concept; this remark is already sufficient to recognize the fallacy.[16]

Concerning the Russell-Zermelo paradox, Hilbert claims that it was removed from set theory by Zermelo, but that "it has not yet been resolved in a satisfactory way as a logical antinomy" (p. 159). With this enigmatic remark

[16]Man braucht die ganze Argumentation nur vorurteilslos anzusehen, um zu erkennen, dass sie völlig unzulässig ist. Schon der Einwand, dass der vieldeutige, subjektive Charakter der Sprache es nicht gestattet, die exakte Behauptung aufzustellen, dass bestimmte Worte stets einen und denselben Begriff bezeichnen müssen, reicht hin, um den Trugschluss zu erkennen. (p. 158)

he moves on to the last point of the lectures, a sketch of basic ideas for a logical calculus that will be taken up again later. "We assume that we have the capacity to name things by signs, that we can recognize them again. With these signs we can then carry out operations that are analogous to those of arithmetic and that obey analogous laws."[17] This remark is followed by a brief algebraic description of sentential logic and the programmatic formulation of the task of a logical calculus, "to draw logical inferences by means of purely formal operations with letters". Some examples of such inferences are then presented.

These lectures do not break new ground, but they do provide clarifications, broader perspectives, and a sharpening of central problems; the main issues are closely related to those I discussed above, but in none of the lectures, except those from the summer term of 1905, does Hilbert take up the proof theoretic approach of his Heidelberg paper. All of this can be seen from the lectures on set theory given in the summer term 1917, most poignantly when comparing them to the lectures given just a few months later in the winter term 1917/18. Chapter I of the set theory notes treats rational, algebraic, and transcendental numbers; under the heading "The Numbers and their Axioms", Chapter II presents a version of the axiom system for the reals formulated in *Über den Zahlbegriff* and supplements it by investigations of independence questions familiar from *Grundlagen der Geometrie*; Chapter III focuses on the concept of set, in particular, on that of an ordered and well-ordered set. Finally, in Chapter IV, Hilbert intends to deal with "Application of Set Theory to Mathematical Logic". I am not sure how to understand the last heading, as there is no discussion of mathematical logic in that chapter! But there *is* again a discussion of Richard's paradox and of the Russell-Zermelo antinomy. This time the fundamental problem is seen as related to what Hilbert calls "genetische Definitionen". The remarks warrant discussion: they point to the past as represented by Kronecker and by his own 1905 lectures, and to the future, i.e., to a fully developed finitist standpoint.

A4. Genetic definitions. These definitions include all impredicative ones. One example is given by the set theoretic definition of inductively generated classes as the smallest sets satisfying certain closure conditions; another example is extracted in Hilbert's analysis of Dedekind's proof of the existence of an infinite system. Dedekind's proof involves the "system of all things that can be the object of my thought" and thus a system whose definition employs universal quantification. Hilbert does not emphasize in either example that the range of the quantifier must include the set that is being defined, and that is of course the characteristic feature of impredicative definitions. Instead,

[17]Wir gehen von der Annahme aus, dass wir die Fähigkeit haben, Dinge durch Zeichen zu benennen, dass wir sie wiederzuerkennen vermögen. Mit diesen Zeichen werden wir dann gewisse Operationen ausführen können, die denen der Arithmetik analog sind und analogen Gesetzen folgen. (p. 159)

Hilbert simplifies matters in a quite radical way by taking a "new and unusual" standpoint that disapproves of the use of words like "all", "every", or "and so on". Hilbert views the use of these words as characteristic of genetic definitions and as pervasive in mathematics.

> There is no need to consider irrational numbers; the geometric series $1 + 1/2 + 1/4 + 1/8 +$ "and so on" is already an example. Not even formulas in which finite, but only indeterminate whole numbers n occur are immune to our critique. To be able to apply them one sets $n = 1, 2, 3, 4, 5,$ "and so on". Kronecker who intended to reduce all of mathematics to the whole numbers was consequently not radical enough, for 'n' does occur in his formula. He should have restricted himself to the specific numbers 7, 15, 24. Thus, one sees what kind of difficulties have to be faced when calculating with letters. Already the simple formula $a + b = b + a$ can be attacked.[18]

Finally, closing the circle to the earlier considerations, Hilbert views sets that can be given *only* through genetic definitions as *inconsistent*.

The natural numbers are given in Dedekind's set theoretic way as well as in the informal Kroneckerian way by genetic definitions; thus, Hilbert rejects the natural numbers as the fundamental system for mathematics. I take it that these reflections constitute the reasoned rejection of the "genetic method" as described in [50]; the discussion of the genetic and axiomatic method is concluded there as follows: "Despite the high pedagogic and heuristic value of the genetic method, for the final presentation and the complete logical grounding of our knowledge the axiomatic method deserves to be preferred." In the present lecture notes he follows Peano in giving an axiom system for natural numbers and remarks, against Poincaré, that this is but a first step in the foundational investigation:

> ... if we set up the axioms of arithmetic, but forego their further reduction and take over uncritically the usual laws of logic, then we have to realize that we have not overcome the difficulties for

[18]Wir brauchen nicht einmal Irrationalzahlen zu betrachten, schon die geometrische Reihe $1+1/2+1/4+1/8+$'und so weiter' ist ein Beispiel dafür. Ja nicht einmal Formeln, in denen endliche, aber nur unbestimmte ganze Zahlen n vorkommen, halten unserer Kritik stand. Denn um sie anwenden zu können, setzt man $n = 1, 2, 3, 4, 5,$ 'und so weiter'. Kronecker, der die ganze Mathematik auf die ganzen Zahlen zurückführen wollte, war also noch nicht radikal genug; denn in seiner Formel kommt das 'n' vor. Er hätte sich vielmehr auf spezielle Zahlen 7, 15, 24 beschränken müssen. Man sieht also, was für Schwierigkeiten der Rechnung mit Buchstaben entgegenstehen. Schon die einfache Formel $a + b = b + a$ ist anfechtbar. (p. 137)—Indeed, Hallett in his [48] makes it clear that this is really an "old" standpoint of Hilbert's going back to 1904/5; cf. also note 49 below.

> a first philosophical-epistemological foundation; rather, we have just cut them off in this way.[19]

Hilbert answers the question "To what can we further reduce the axioms?" by "To the laws of logic!" He claims that if we try to achieve such a reduction to logic,

> . . . we are facing one of the most difficult problems of mathematics. Poincaré has even the view that this is not at all possible. But with that view one could rest content only if it had been proved that the further reduction of the axioms for arithmetic is impossible; but that is not the case. Next term, I hope to be able to examine more closely a foundation for logic.[20]

One has again the sense that the exigencies of academic life and the complexity of the issues diverted Hilbert's attention to his own great dissatisfaction. That is, I assume, what motivated Hilbert's action in the spring (or fall) of 1917: he invited Paul Bernays to assist him in efforts to examine the foundations of mathematics.[21] Bernays returned to Göttingen, where he had been a student, and started to work with Hilbert on lectures that were offered in the winter term 1917/18 under the title *Prinzipien der Mathematik*.

B. From 1917 to 1920: logic and metamathematics. As background for the 1917/18 lectures one should keep in mind that Hilbert saw himself as pursuing one of the most difficult problems of mathematics, i.e., its reduction to logic. In *Axiomatisches Denken* he had formulated matters as follows:

> The examination of consistency is an unavoidable task; thus, it seems to be necessary to axiomatize logic itself and to show that number theory as well as set theory are just parts of logic. This avenue, prepared for a long time, not least by the deep investigations of Frege, has finally been taken most successfully by the penetrating mathematician and logician Russell. The completion

[19]. . . wenn wir die Axiome der Arithmetik aufstellen, aber auf eine weitere Zurückführung derselben verzichten und die gewöhnlichen Gesetze der Logik ungeprüft übernehmen, so müssen wir uns bewusst sein, dass wir dadurch die Schwierigkeiten einer ersten philosophisch-erkenntnistheoretischen Begründung nicht überwunden, sondern nur kurz abgeschnitten haben. (p. 146)

[20]. . . so stehen wir vor einem der schwierigsten Probleme der Mathematik überhaupt. Poincaré vertritt sogar den Standpunkt, dass dies garnicht möglich ist, aber damit könnte man sich erst zufrieden geben, wenn der Unmöglichkeitsbeweis für die weitere Zurückführung der Axiome der Arithmetik geführt wäre, was nicht der Fall ist. Auf eine Begründung der Logik hoffe ich im nächsten Semester näher eingehen zu können. (pp. 145–6)

[21]According to Constance Reid, pp. 150–1, Hilbert invited Bernays in the spring of 1917; Bernays, however, writes in his biographical note: "Im Herbst 1917 wurde ich von Hilbert anlässlich seines in Zürich gehaltenen Vortrages *Axiomatisches Denken* aufgefordert, an seinen wieder aufgenommenen Untersuchungen über die Grundlagen der Arithmetik als sein Assistent mitzuwirken."

> of this broad Russellian enterprise of axiomatizing logic might be viewed quite simply as the crowning achievement of the work of axiomatization.[22]

The detailed pursuit of that goal required the presentation of a formal language (for capturing the logical form of informal statements), the use of a formal calculus (for representing the structure of logical arguments), and the formulation of "logical" principles (for defining mathematical objects). This is carried through with remarkable focus, elegance, and directness. From the very beginning, the logical and mathematical questions are mixed with, or rather driven by, philosophical reflections on the foundations of mathematics, and we find penetrating discussions of the axiom of reducibility that become increasingly critical and lead ultimately to the rejection of the logicist enterprise (in 1920; cf. B3).

B1. Future and past. The collaboration of Hilbert and Bernays led to a remarkable sequence of lectures, where we can witness the creation of modern mathematical logic and the emergence of proof theory. The relevant lectures are: *Prinzipien der Mathematik* (Winter 1917/18); *Logik-Kalkül* (Winter 1920); *Probleme der mathematischen Logik* (Summer 1920); *Grundlagen der Mathematik* (Winter 1921/22); *Logische Grundlagen* (Winter 1922/23). In the winter term 1919 Hilbert gave related lectures entitled *Natur und mathematisches Erkennen*. In presenting the lectures from 1917/18 and the further development in those from 1920, I will highlight three groups of issues, namely, logical, mathematical, and general metamathematical ones. Proof theoretic issues began to emerge only in 1920 and are the topic of Part C. But I want to make first a few remarks about the immediate historical context of these lectures.

A polished presentation of the material developed in this sequence of lectures (leaving out the specifically proof theoretic considerations) is found in Hilbert and Ackermann's book, *Grundzüge der theoretischen Logik*, published in 1928. Indeed, the basic structure of the book is the same as that of the 1917/18 notes, large parts of the texts are identical, and there are hardly any new metamathematical results (except for important results that had been obtained in the meantime, like special cases of the decision problem, the Löwenheim Skolem theorem). In the preface to the book Hilbert wrote:

> In preparing the above lectures [WS 17/18, WS 20, WS 21/22] I received support and advice in essential ways from my colleague

[22] Da aber die Prüfung der Widerspruchslosigkeit eine unabweisbare Aufgabe ist, so scheint es nötig, die Logik selbst zu axiomatisieren und nachzuweisen, daß Zahlentheorie sowie Mengenlehre nur Teile der Logik sind. Dieser Weg, seit langem vorbereitet—nicht zum mindesten durch die tiefgehenden Untersuchungen von Frege—ist schließlich am erfolgreichsten durch den scharfsinnigen Mathematiker und Logiker Russell eingeschlagen worden. In der Vollendung dieses großzügigen Russellschen Unternehmens der *Axiomatisierung der Logik* könnte man die Krönung des Werkes der Axiomatisierung überhaupt erblicken. (p. 153)

> P. Bernays; the latter also wrote the notes for these lectures most carefully.—Using and supplementing the material that had been accumulated in this way, W. Ackermann ... provided the present organization and gave the definitive presentation of the total material.[23]

The fact that the supplements by Ackermann are minimal is historically important, as the book has been taken falsely, for example by Goldfarb [46], as the endproduct of a cumulative development. This one misjudgment informs others; for example, it is claimed that quantifiers were properly understood only in the book of 1928, and as evidence Goldfarb adduces that "... in his [Hilbert's] early presentations of axiom systems [in [65] and [67]] we first meet some quantifier-free number-theoretic and analytic axioms; the so-called transfinite axioms which introduce quantification then follow. The direction, in short, is the reverse of that which would highlight the underlying nature of quantificational logic ...." (p. 359) That expresses a deep misunderstanding of the early published work on proof theory and its systematic background. As we will see shortly, the 1917/18 notes contain first order logic in a fully developed form. Restricted calculi were introduced for programmatic reasons and not, as has been suggested, because of a "finitist prejudice" or because fuller calculi had yet to be developed.

Hilbert and Bernays's achievements during this period are overshadowed by Gödel's subsequent work—that is inspired by it and that builds on it. The book with Ackermann, though recognized as a landmark, has been severely criticized (e.g., by Goldfarb, Dreben, and van Heijenoort); the most substantial critical remarks are taken up in B3 below. Seeing the 1928 book as the product of a sustained development in Göttingen makes it extremely difficult to appreciate the novelty and originality of the very early published work. It makes it even more difficult, on the one hand, to understand how Hilbert and Bernays's work was influenced by contemporaneous work in logic (e.g., that of Russell and Whitehead or that in the algebraic tradition of Schröder) and, on the other hand, to appreciate in what respects it was strikingly different.

As to the influence of contemporaneous logical work, I learned through a personal communication from Alasdair Urquhart that Hilbert and Russell exchanged some postcards between 1916 and 1919; for details see Appendix B. The most relevant information for the discussion here is Hilbert's claim made on his postcard to Russell dated April 12, 1916, "that we have been discussing in the Math. Society your theory of knowledge already for a long

[23] Bei der Vorbereitung der genannten Vorlesungen [WS 17/18, WS 20, WS 21/22] bin ich von meinem Kollegen P. Bernays wesentlich unterstützt und beraten worden; derselbe hat diese Vorlesungen auch aufs sorgfältigste ausgearbeitet.—Unter Benutzung und Ergänzung des so entstandenen Materials hat W. Ackermann ... die vorliegende Gliederung und definitive Darstellung des Gesamtstoffes durchgeführt. ["WS" stands for "Winter Semester".]

time, and that we had intended, just before the outbreak of the war, to invite you to Göttingen, so that you could give a sequence of lectures on your solution to the problem of the paradoxes." The notes for lectures Hilbert gave before the winter term 1917/18, even for those of the immediately preceding summer term 1917, do not contain any reference to ***Principia Mathematica*** nor any hint of a Russellian influence. There is only one exception I discovered; in his lectures *Probleme und Prinzipien der Mathematik* given in the winter term 1914/15, Hilbert mentions Russell and remarks briefly that type theory contains something true, but that it has to be deepened significantly. Here is a real gap in our historical understanding; we also do not have a sense of Bernays's possibly pivotal role or of other influences, like Weyl's through his book *Das Kontinuum*. This gap is puzzling and very much worth closing. (The detailed analysis of Behmann's dissertation, described in Mancosu's 1998 manuscript, will undoubtedly throw light on the Russellian influence; cf. note 67.)

B2. Languages and calculi. The lectures given during the summer term 1917 do not contain a proper logical system: what indication of logical matters one finds there is of a very restricted algebraic sort. An algebraic motivation is still present in the lectures of the following winter term, but only in a broad methodological sense. We read on page 63 for example: "The logical calculus consists in the application of the formal methods of algebra to logic." However, the general and explicit goal is to develop a symbolic language and a suitable logical calculus that allow a thoroughgoing formalization of mathematics, in particular of analysis.

The 1917/18 notes consist of 246 type-written pages and are divided into two parts. Part A, *Axiomatische Methode*, gives on sixty-two pages Hilbert's standard account of the axiomatic method, in particular, as it applies to geometry. Part B, *Mathematische Logik*, is a beautifully organized, almost definitive presentation of the very core of modern mathematical logic. The material is organized under the chapter headings:

1. The sentential calculus;
2. The predicate calculus and class calculus [the former is just monadic logic];
3. Transition to the function calculus [i.e., first order logic];
4. Systematic presentation of the function calculus;
5. The extended function calculus.

Chapters 1 through 4 lead, in part, to a systematic formulation of first order logic; every step taken in expanding the logical framework is semantically motivated and carefully argued for. This material was novel at the time; by now it is all too familiar and will not be discussed, except to note and emphasize one important difference: the languages contain *sentential* and *function* (i.e., relation) *variables*. Weyl presented in his almost contemporaneous book *Das Kontinuum* the language of first order logic in a very similar way.[24] He did not introduce a logical calculus, but discussed very informatively the main task of logic, namely, to describe the syntactic, formal structures that would allow one to establish all the semantic, logical consequences of given assumptions; cf. the brief discussion in Remark 3 at the end of B3 below. This main task of logic is partially resolved for first order logic in the 1917/18 notes, where, at the very end of Chapter 4, the suitability of the calculus for the formal-axiomatic presentation of theories is re-examined:

> The calculus is well suited for this purpose mainly for two reasons: one, because its application prevents that—without being noticed—assumptions are used that have not been introduced as axioms, and, furthermore, because the logical dependencies so crucial in axiomatic investigations are represented by the symbolism of the calculus in a particularly perspicuous way.[25]

Chapter 5 takes a noteworthy turn. After all, if only a formalization of logical reasoning were aimed for, no additional work beyond that of chapter 1 through 4 would be needed. The logical calculus is to play, however, an important role for the investigation of mathematical theories and their relation to logic.

> Not only do we want to develop individual theories from their principles in a purely formal way, but we also want to investigate the foundations of the mathematical theories and examine what their relation to logic is and how far they can be built up from purely logical operations and concepts; and for this purpose the logical calculus is to serve as an auxiliary tool.[26]

If one wants to use the calculus for that logicist purpose, one is led to extend the rules of formally operating within the calculus in "a certain direction". Up to now, statements and functions had been sharply separated from objects; correspondingly, indeterminate statement- and function-signs (i.e., sentential and function variables) had been strictly separated from variables that can be taken as arguments, but this is being changed:

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[24]The basic formulation goes back to [103], where the language is also built up using disjunction, negation, and existential quantification.

[25]Für diesen Zweck ist der Kalkül vor allem aus zwei Gründen sehr geeignet, einmal weil bei seiner Anwendung verhütet wird, dass man unbemerkt Voraussetzungen benutzt, die nicht als Axiome eingeführt sind, und weil ferner durch die Symbolik des Kalküls die logischen Abhängigkeits-Verhältnisse, auf die es ja bei der axiomatischen Untersuchung ankommt, in besonders prägnanter Weise zur Darstellung gelangen. (p. 187)

[26]Wir wollen nicht nur imstande sein, einzelne Theorien für sich von ihren Prinzipien aus rein formal zu entwickeln, sondern wollen die Grundlagen der mathematischen Theorien selbst auch zum Gegenstand der Untersuchung machen und sie daraufhin prüfen, in welcher Beziehung sie zu der Logik stehen und inwieweit sie aus rein logischen Operationen und Begriffsbildungen gewonnen werden können; und hierzu soll uns der logische Kalkül als Hilfsmittel dienen. (p. 188)

> —we will allow now that statements and functions can be taken as values of logical variables in the same way as proper objects and that indeterminate statement signs and function signs can appear as arguments of symbolic expressions.[27]

A free Fregean expansion of the function calculus leads, however, to contradictions. Reflecting on the principles on which this expansion is based, a "logical circle" is discovered. The domain, associated with the original (first order) function calculus and providing the logical meaning of quantifiers, was expanded by new kinds of objects, namely statements, predicates, and relations. Then new symbolic expressions were admitted, whose "logical meaning [as they involve quantifiers] requires a reference to the totality of statements, respectively of functions".

> This way of proceeding is indeed suspicious, insofar as those expressions that gain their meaning only through reference to the totality of statements, respectively functions are counted then among the statements and functions; on the other hand, in order to be able to refer to the totality of statements and functions, we have to view the statements, respectively functions as being determined from the very beginning.[28]

This suspicious way of proceeding involves the logical circle, and there is reason to assume that "this circle is the cause for the presence of the paradoxes". The goal of avoiding any reference to dubious totalities of statements and functions leads "in the most natural way" to ramified type theory.

The formal framework of ramified type theory is seen, however, as too narrow for mathematics, because it does not, for example, allow the proper formalization of Cantor's proof of the existence of uncountable sets; cf. pp. 229–30. To achieve greater flexibility for the calculus Russell's axiom of reducibility is adopted; this broader framework is then used for the development of the beginnings of analysis, in particular, the least upper bound principle is established. The notes end with the remark:

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[27]... wir [werden] nunmehr zulassen, dass Aussagen und Funktionen in gleicher Weise wie eigentliche Gegenstände als Werte von logischen Variablen genommen werden und dass unbestimmte Aussagezeichen und Funktionszeichen als Argumente von symbolischen Ausdrücken auftreten. (p. 188)

[28]Dies Vorgehen ist nun in der Tat bedenklich, insofern nämlich dabei jene Ausdrücke, welche erst durch die Bezugnahme auf die Gesamtheit der Aussagen bezw. der Funktionen ihren Inhalt gewinnen, ihrerseits wieder zu den Aussagen und Funktionen hinzugerechnet werden, während wir doch andererseits, um uns auf die Gesamtheit der Aussagen und Funktionen beziehen zu können, die Aussagen bezw. die Funktionen als von vornherein bestimmt ansehen müssen. (p. 219)

> Thus it is clear that the introduction of the axiom of reducibility is the appropriate means to turn the ramified calculus into a system out of which the foundations for higher mathematics can be developed.[29]

Is the outline I gave consistent with a formalist perspective on Hilbert, never mind the metamathematical novelties and logicist tendencies these developments exhibit?—Prima facie the answer may be "yes", but such a perspective is completely inadequate. Why that is so will be clear, I hope, from the further issues I will present.

B3. Semantic interpretation. The formal frame I have been discussing is not only contentually motivated, but the semantics is properly specified and the central semantic notions are carefully formulated. Sometimes one finds that syntactic notions are interwoven with semantic concepts—amusing to a modern reader who is expecting a "formalist" presentation. But before giving an example, I have to discuss a *very* important, fundamental point that was hinted at already in B2. First order theories are always viewed together with suitable non-empty domains, *Bereiche*, indicating the range of the individual variables of the theory, and interpretations of the non-logical vocabulary (except, of course, the sentential and function variables). In modern terms, the theories are always presented together with a *structure*. Hilbert and Bernays call this the "existential aspect" of the axiomatic method. A significant philosophical motivation is revealed, when Hilbert reemphasizes the important role of domains as ranges for individual variables and notes: "This remark resolves the difficulties, discussed by Russell, in interpreting general judgments."[30] Weyl also emphasizes this broader point in *Das Kontinuum*, when he says that existential judgments presuppose that "the particulars of the categorial being under consideration should form a closed system of determinate, independently existing objects".[31]

Finally, I can mention an example concerning the mixing of semantic and syntactic considerations or rather, how semantic considerations lead to restrictions on syntactic constructions. In the lectures from the winter term 1917/18 (pp. 112/3 and 129 ff), but also in later ones, e.g., from the winter term 1920 (p. 24), a many-sorted logic is introduced. The argument places, *Leerstellen*, of particular functions are taken to be related to particular domains: if an argument place is filled by the name of an object

[29] So zeigt sich, dass die Einführung des Axioms der Reduzierbarkeit das geeignete Mittel ist, um den Stufen-Kalkül zu einem System zu gestalten, aus welchem die Grundlagen der höheren Mathematik entwickelt werden können. (p. 246)

[30] Auf Grund dieser Bemerkung erledigen sich die von Russell erörterten Schwierigkeiten in der Interpretation des allgemeinen Urteils. [62], pp. 25–6.—Hilbert may refer here to the difficulties discussed by Russell already in sections II and III of his 1908 paper *Mathematical logic as based on the theory of types*. Cf. also the beginning of Part C1.

[31] l.c., p. 4.—The full German sentence is: "In diesem Sinne verstehen wir die Voraussetzung, daß *die Besonderungen des kategorialen Wesens, um das es sich handelt, ein geschlossenes System bestimmter, an sich existierender Gegenstände ausmachen sollen*."

from an inappropriate domain, then the resulting formula is considered as meaningless (*sinnlos*). This is done similarly for quantification (p. 132); if the same quantified variable is used in two argument places that are related to different domains, the resulting formula is meaningless. Clearly, this can be reflected in a purely syntactic way, as it is done later on; the interesting point here is the direct semantic motivation for restrictive conditions.

How are expressions of the formal language to be understood, given the associated domain? After the discussion of the axiom system for the function calculus, including the specification of the syntax (pp. 129–135), there is the following remark clarifying where a semantic understanding is needed and where pure formality is essential:

> This system of axioms provides us with a procedure to carry out logical proofs strictly formally, i.e., in such a way that we need not be concerned at all with the meaning of the judgments that are represented by formulas, rather we just have to attend to the prescriptions contained in the rules. However, we have to interpret the signs of our calculus when representing symbolically the premises from which we start and when understanding the results obtained by formal operations.
>
> The logical signs are interpreted as before according to the prescribed linguistic reading; and the occurrence of indeterminate statement-signs and function-signs in a formula is to be understood as follows: for arbitrary replacements by determinate statements and functions ... the claim that results from the formula is correct.[32]

This remark points to an answer to the question I raised; it is followed (pp. 136 and 137) by a careful explanation of why the application of the function calculus, given the semantic interpretation, "inhaltliche Auslegung" or "Deutung", leads always to correct results. The underlying concept of

[32]Dieses System von Axiomen liefert uns ein Verfahren, um logische Beweisführungen streng formal zu vollziehen, d.h. so, dass wir uns um den Sinn der durch die Formeln dargestellten Urteile gar nicht zu kümmern brauchen, sondern lediglich die in den Regeln enthaltenen Vorschriften zu beachten haben. Allerdings müssen wir bei der symbolischen Darstellung der Prämissen, von denen wir ausgehen, sowie bei der Interpretation der durch die formalen Operationen erhaltenen Ergebnisse den Zeichen unseres Kalküls eine Deutung beilegen.

Diese Deutung geschieht bei den logischen Zeichen in der bisherigen Weise, entsprechend der vorgeschriebenen sprachlichen Lesart; und das Auftreten von unbestimmten Aussage-Zeichen und Funktions-Zeichen in einer Formel ist so zu verstehen, dass bei jeder beliebigen Einsetzung von bestimmten Aussagen und Funktionen ... die aus der Formel entstehende Behauptung richtig ist. (pp. 135–6)

This remark, almost verbatim, is found in Hilbert and Ackermann's book on page 54. Furthermore, in the notes from the winter term 1920, p. 31: "... ferner soll unter einer 'richtigen Formel' ein solcher Ausdruck verstanden werden, der bei beliebiger inhaltlicher Festlegung der vorkommenden unbestimmten Zeichen eine richtige Aussage darstellt."

correctness, *Richtigkeit*, with respect to a domain is to be understood as follows: (1) statements involving no sentential or function variables are "correct" if they are true in the domain, and that is understood informally in exactly the same way as in the model theoretic arguments for independence and relative consistency in Hilbert's *Grundlagen der Geometrie* or, for that matter, in Gödel's dissertation ([34] and [35]); (2) if a statement does contain such variables, then the clause "for arbitrary replacements by determinate statements and functions the claim that results from the formula is correct" is invoked to define "correctness" for this broader class of statements.[33] This clarification will be used below.

In this semantic context I want to return to the discussion of the ramified theory of types. The standpoint that motivated ramified type theory was this: one takes for granted a domain of individuals with basic properties and basic relations between them. From this basis, all further predicates and relations are obtained, constructively, by the logical operations. Already in the lecture notes from the winter term 1917/18 it is acknowledged that the axiom of reducibility is in conflict with this constructive standpoint. It has to be assumed that "certain predicates and relations have to be viewed as having an independent existence, so that their manifold depends neither on actually given definitions nor on our possibilities of giving definitions".[34] This argument is concisely rehearsed in the notes from the summer term 1920; in the notes for the winter term 1921/22 it leads to an explicit rejection of the logicist route. After all, it is argued, if one chooses the basis in an arbitrary way, the axiom of reducibility is certainly not satisfied. Thus, one would have to expand the "system of basic properties and relations" in such a way that the demand of the axiom is met. The question, whether such an expansion can be achieved by a logical-constructive procedure, is answered negatively.

> Thus, there remains only the possibility to assume that the system of predicates and relations of first order is an independently existing totality satisfying the axiom of reducibility.—In this way we return to the axiomatic standpoint and give up the goal of a logical foundation of arithmetic and analysis. Because now a reduction to logic is given only nominally.

I have only sketched this discussion; it is subtle and deserves a detailed analysis and careful comparison with its modified version, influenced by [87], in *Hilbert and Ackermann*, but also with Weyl's considerations in *Das Kontinuum*. This is important, not least as it sounds explicitly and most

[33] Bill Howard pointed out quite correctly that this notion is used in a context sensitive way; most often it is used in the way I just described it, namely as "true formula", but sometimes also in the sense of "provable formula". This foreshadows a certain ambiguity in *Hilbert and Ackermann*; cf. Remark 1 on completeness below.

[34] [59], p. 232.

clearly themes that will be found in the literature with equally good sense and balance only in Gödel's paper on *Russell's Mathematical Logic* and, less systematically, in [38]. Let me return briefly to the discussion of metamathematical issues that are faced and formulated with a completely new rigor.

For the purpose of the logical calculus in the systematic investigation it is crucial that it allows one to recapture formally the ordinary forms of argumentation. This is clearly expressed in the 1917/18 lecture notes:

> As for any other axiomatic system, one can raise also for this system the questions concerning consistency, logical dependencies, and completeness. The most important question is here that concerning completeness. After all, the goal of symbolic logic is to develop ordinary logic from the formalized assumptions. Thus, it is essential to show that our axiom system suffices for the development of ordinary logic.[35]

These notes contain prominently only one mathematically precise concept of completeness for logical calculi, namely "Post-completeness": "We will call the presented axiom system complete, if the addition of a formula, hitherto unprovable, to the system of basic formulas always leads to an inconsistent system."[36] That is quickly established for sentential logic, and the semantic completeness is mentioned and proved in a footnote (on p. 153). The latter notion is brought to the fore, unequivocally and beautifully, in Bernays's Habilitationsschrift[37] of 1918, where the completeness theorem receives its first "classical" formulation: "Every provable formula is a valid formula and vice versa."[38] For first order logic the question of its Post-completeness is raised in the lecture notes (p. 156), and it is conjectured that the answer is negative. The proof of this fact, explicitly attributed to Ackermann, is then given in *Hilbert and Ackermann* (p. 66) where the considerations for sentential logic can also be found. The presentation follows that of the notes

[35]Wie bei jeder Axiomatik lassen sich auch für dieses System die Fragen nach der Widerspruchslosigkeit, nach den logischen Abhängigkeiten und nach der Vollständigkeit aufwerfen. Am wichtigsten ist hier die Frage der Vollständigkeit. Denn das Ziel der symbolischen Logik besteht ja darin, aus den formalisierten Voraussetzungen die übliche Logik zu entwickeln. Es kommt also wesentlich darauf an, zu zeigen, dass unser Axiomensystem zum Aufbau der gewöhnlichen Logik ausreicht. (p. 67)

[36]Wir wollen das vorgelegte Axiomen-System vollständig nennen, falls durch die Hinzufügung einer bisher nicht ableitbaren Formel zu dem System der Grundformeln stets ein widerspruchsvolles System entsteht. (p. 152)—This completeness concept is clearly related to that formulated in the axiomatization of the real numbers and of geometry; this connection should be explored carefully.

[37]Only a much abbreviated version of this was published in 1926 as [9]; the publication focuses on the independence results.

[38]Jede beweisbare Formel ist eine allgemeingültige Formel und umgekehr (p. 6).

closely, but elevates the semantic completeness proof from the footnote into the main text (p. 33).

Remarks. (1) The semantic completeness for first order logic is formulated as an open problem in *Hilbert and Ackermann*: "Whether the axiom system is complete at least in the sense that really all logical formulas that are correct for all domains of individuals can be derived from it is an unsolved question. We can only say purely empirically that this axiom system has always sufficed for any application."[39] Some recent commentators have viewed this formulation as *oddly obscure* (Goldfarb) or even *circular* (Dreben and van Heijenoort). Those views rest on a very particular reading of "logical formulas" that is narrowly correct, as Hilbert and Ackermann (following verbatim the 1917/18 lecture notes) define them on page 54 as those formulas that (i) do not contain "individuelle Zeichen" (i.e., symbols for determinate individuals and functions), and (ii) can be proved by appealing only to the logical axioms. Under this reading the formulation is indeed close to non-sensical. However, if one takes into account that "logische Formel" and "logischer Ausdruck" are used repeatedly[40] as indicating just those formulas satisfying (i), then their formulation together with the explication of correctness I reviewed earlier is exactly right. Indeed, the formulation of the completeness problem involves then *precisely* the definition of "allgemeingültig" given in [35], notes 3 and 4. Gödel emphasizes in the first of these footnotes that "This paper's terminology and symbolism follows closely *Hilbert and Ackermann* 1928."[41] An equally correct formulation of completeness is given in Hilbert's talk to the International Congress of Mathematicians in Bologna, 3 September 1928 [70]; validity is defined as non-refutability by an arithmetic model. It is also of interest to note that Hilbert contemplates there the incompleteness of axiomatic systems for "higher areas" (*höhere Gebiete*). (Obviously, both these observations implicitly use the Löwenheim Skolem theorem.)

(2) Weyl defined on pages 9 and 10 of *Das Kontinuum* a semantic notion of logical truth and consequence: "Some pertinent judgments we recognize as true purely on the basis of their logical structure—without regard either to the characteristics of the category of objects involved or to the extension of the basic underlying properties and relations or to the objects used in the operation of 'filling in' .... Such judgments which are true purely on account of their formal (logical) structure ... we wish to call (*logically*)

[39] l.c., p. 68. Ob das Axiomensystem wenigstens in dem Sinne vollständig ist, daß wirklich alle logischen Formeln, die für jeden Individuenbereich richtig sind, daraus abgeleitet werden können, ist eine noch ungelöste Frage. Es läßt sich nur rein empirisch sagen, daß bei allen Anwendungen dieses Axiomensystem immer ausgereicht hat.

[40] For example, on pages 72, 73, and 80 in the discussion of the *Entscheidungsproblem*. Gödel uses in his [34] and also in [35] "logischer Ausdruck" in exactly this sense.

[41] In Terminologie und Symbolik schließt sich die folgende Arbeit an *Hilbert und Ackermann 1928* an.

self-evident. A judgment whose negation is self-evident is called *absurd*. If $U\&\neg V$ is absurd, then the judgment V is a *logical consequence* of U; if U is true, then we can be certain that V is also true."[42] (I have used the standard sentential connectives here.)—Recall that the very isolation of the language of first order logic goes back to [103].

(3) The word 'Entscheidungsproblem' is used, as far as I can see, for the first time in these lectures in the winter term of 1922/23 on page 25, cf. also Kneser's "Mitschrift" on p. 12. Clearly, the general problem of mechanically deciding mathematical questions had been mentioned already earlier by Hilbert, for example, in *Axiomatisches Denken* and even in his Paris lecture of 1900.

Exploiting the standard arithmetic interpretation of the logical connectives, Hilbert addresses the consistency problem for logic in the lectures from the winter term 1917/18. He shows, by induction on derivations in sentential and first order logic, that provable formulas are always true; consistency of the logical calculi is a direct consequence.[43] However, in a note the reader is warned not to overestimate the significance of this result, because "[i]t does not give us a guarantee that the system of provable formulas remains free of contradictions after the symbolic introduction of contentually correct assumptions".[44] That much more difficult problem has to be

[42]The English translation is from [104]; the German text is this: "Unter den einschlägigen Urteilen gibt es solche, die wir als wahr erkennen auf Grund ihrer logischen Struktur—ganz unabhängig davon, um was für eine Gegenstandskategorie es sich handelt, was die zugrunde liegenden Ur-Eigenschaften bedeuten und welche Gegenstände ... zur 'Ausfüllung' benutzt werden. Solche rein ihres formalen (logischen) Baus wegen wahren Urteile ... wollen wir (*logisch*) *selbstverständlich* nennen. Ein Urteil, dessen Negation selbstverständlich ist, heiße *sinnwidrig*. Ist $U\&\neg V$ sinnwidrig, so ist das Urteil V eine '*logische Folge*' von U; ist U wahr, so können wir sicher sein, daß dann auch V wahr ist."

[43]This is done on pages 70 ff and 150 ff; the analogous considerations are contained in *Hilbert and Ackermann* on pages 30 ff and 65 ff.

[44]Man darf dieses Ergebnis in seiner Bedeutung nicht überschätzen. Wir haben ja damit noch keine Gewähr, dass bei der symbolischen Einführung von inhaltlich einwandfreien Voraussetzungen das System der beweisbaren Formeln widerspruchslos bleibt. (p. 156)—In *Hilbert and Ackermann* there is a significant expansion of this remark: Man darf das Ergebnis dieses Beweises für die Widerspruchsfreiheit unserer Axiome übrigens in seiner Bedeutung nicht überschätzen. Der angegebene Beweis der Widerspruchsfreiheit kommt nämlich darauf hinaus, daß man annimmt, der zugrunde gelegte Individuenbereich bestehe nur aus einem einzigen Element, sei also endlich. Wir haben damit durchaus keine Gewähr, daß bei der symbolischen Einführung von inhaltlich einwandfreien Voraussetzungen das System der beweisbaren Formeln widerspruchsfrei bleibt. Z.B. bleibt die Frage unbeantwortet, ob nicht bei der Hinzufügung der mathematischen Axiome in unserem Kalkül jede beliebige Formel beweisbar wird. Dieses Problem, dessen Lösung eine zentrale Bedeutung für die Mathematik besitzt, läßt sich in bezug auf Schwierigkeit mit der von uns behandelten Frage garnicht vergleichen. Die mathematischen Axiome setzen gerade einen unendlichen Individuenbereich voraus, und mit dem Begriff des Unendlichen sind die Schwierigkeiten und Paradoxien verknüpft, die bei der Diskussion über die Grundlagen der Mathematik eine Rolle spielen. (pp. 65–6)

attacked in special ways—by a logicist reduction, perhaps, or by quite new ways of proceeding; we come to these new ways now.

C. From 1920 to 1922: consistency and proof theory. A rigid and dogmatic formalist view is popularly attributed to Hilbert and his collaborators. This attribution is untempered by accessible works, for example, the two monumental volumes of *Grundlagen der Mathematik* published in 1934 and 1939, or Bernays's philosophical investigations starting with essays from 1922. The content of the early lecture notes should help to put Hilbert's views in proper perspective. Notice that, up to now, no specifically proof-theoretic considerations concerning the consistency problem have been mentioned in these lectures. Indeed, the development towards the Hilbert Program as we think of it was completed only in the lectures given in the winter term 1921/22. Hilbert arrived at its formulation after abandoning the logicist route through two quite distinct steps, and only the second takes up the earlier suggestion of a theory of (formal) proofs.

C1. Constructive number theory. The first step is taken in the winter term 1920.[45] Hilbert reviews the logical development of his 1917/18 lectures in a polished form, frequently referring back to them for additional details. The last third of the notes is devoted, however, to a completely different topic. Hilbert argues that the set theoretic or logical developments of Dedekind and Frege did not succeed in establishing the consistency of ordinary number theory and concludes:

> To solve these problems I don't see any other possibility, but to rebuild number theory from the beginning and to shape concepts and inferences in such a way that paradoxes are excluded from the outset and that proof procedures become completely surveyable.
>
> Now I will show how I think of the beginning of such a foundation for number theory.[46]

[45]See the discussion in Appendix A concerning the sequencing of the lectures from winter term 1920 and the summer term 1920. Moore [80] asserts, incorrectly, that the lectures of the winter term 1920 were given after those of the summer term of that year. This mistake leads not only to misunderstandings of the very lectures and their broader historical context (involving Brouwer and Weyl), but it is also partially responsible for a quite different overall assessment which is summarized in the abstract of the paper as follows: "By 1917, strongly influenced by PM, Hilbert accepted the theory of types and logicism—a surprising shift. But by 1922 he abandoned the axiom of reducibility and then drew back from logicism, returning to his 1905 approach to prove the consistency of number theory syntactically." Clearly, as documented here, logicism had been given up as a viable option in the summer of 1920 explicitly; implicitly, that recognition is already in the background for the lectures in the winter of 1920 discussed in this section. The special constructivist stance taken by Hilbert here and its connection to earlier reflections of Hilbert's are not recognized by Moore, thus also not the expanding step toward finitist mathematics. The latter is discussed in Section C2.

[46]Zur Lösung dieser Probleme sehe ich keine andere Möglichkeit, als dass man den Aufbau der Zahlentheorie von Anfang an durchgeht und die Begriffsbildungen und Schlüsse in eine

The considerations are put back into the broader context of the earlier investigations, re-emphasizing the semantic underpinnings for axiom systems:

> We have analyzed the language (of the logical calculus proper) in its function as a universal instrument of human reasoning and revealed the mechanism of logical argumentation.
>
> However, the kind of viewpoint we have taken is incomplete in so far as the application of the logical calculus to a particular domain of knowledge requires an axiom system as its basis. I.e., a system (or several systems) of objects must be given and between them certain relations with particular assumed basic properties are considered.[47]

This method is perfectly appropriate, Hilbert continues, when we are trying to obtain new results or present a particular science systematically. However, mathematical logic pursues also the goal of securing the foundations of mathematics.

> For this purpose it seems appropriate to connect the mathematical constructions to what can be concretely exhibited and to interpret the mathematical inference methods in such a way that one stays always within the domain of what can be checked. And in fact one is going to start with arithmetic, as one finds here the simplest mathematical concepts.
>
> In addition, it has been the endeavor in mathematics for a long time to reduce all conceptual systems (geometry, analysis) to the integers.[48]

This remark is followed by the development of what might be called *strict finitist number theory*. The considerations are delicate (and their detailed

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solche Fassung bringt, bei der von vornherein Paradoxien ausgeschlossen sind und das Verfahren der Beweisführung vollständig überblickbar wird.

Ich will nun im Folgenden zeigen, wie ich mir den Ansatz zu einer solchen Begründung der Zahlentheorie denke. (p. 48)

[47] . . . Wir haben die Sprache (des eigentlichen Logikkalküls) in ihrer Funktion als universales Instrument des menschlichen Denkens zergliedert und den Mechanismus der logischen Beweisführung blossgelegt.

Jedoch ist die Art der Betrachtungsweise, die wir angewandt haben, insofern unvollständig, als die Anwendung des Logikkalküls auf bestimmte Wissensgebiete ein Axiomensystem als Grundlage erfordert. D.h. es muss ein System (bzw. mehrere Systeme) von Gegenständen gegeben sein, zwischen denen gewisse Beziehungen mit bestimmten vorausgesetzten Grundeigenschaften betrachtet werden. (pp. 46–7)

[48] Zu diesem Zwecke erscheint es als der geeignete Weg, dass man die mathematischen Konstruktionen an das konkret Aufweisbare anknüpft und die mathematischen Schlussmethoden so interpretiert, dass man immer im Bereiche des Kontrollierbaren bleibt. Und zwar wird man hiermit bei der Zahlentheorie den Anfang machen, da hier die einfachsten mathematischen Begriffsbildungen vorliegen.

Auch ist es ja seit langem das Bestreben in der Mathematik, alle Begriffssysteme (Geometrie, Analysis) auf die ganzen Zahlen zurückzuführen. (pp. 47–8) presentation has to wait for another occasion), but one thing is perfectly clear: here is a version of constructive arithmetic stricter than what will appear a little later as finitist mathematics. The basic and directly meaningful part consists only of *closed* numerical equations. This is in line with Hilbert's remark, quoted in Section A4 on genetic definitions, about Kronecker's not being sufficiently radical. Bernays pointed to the *evolution towards* finitist mathematics at a number of places; for example, in his [14] he wrote: "Originally, Hilbert also intended to take the narrower standpoint that does not assume the intuitive general concept of numeral. That can be seen, for example, from his Heidelberg lecture (1904). It was already a kind of compromise that he adopted the finitist standpoint as presented in his publications."[49]

In the lectures from the winter term 1920 this "intuitive general concept of numeral" is not yet assumed; instead, general statements like $x + y = y + x$ are given a constructive and extremely rule-based interpretation:

> Such an equation … is not viewed as a claim for all numbers, rather it is interpreted in such a way that its full meaning is given by a proof procedure: each step of the procedure is an action that can be completely exhibited and that follows fixed rules.[50]

This view entails that the equation $2 + 3 = 3 + 2$ is not a special case of the general equation $x + y = y + x$; on the contrary, having proved the latter, the former still has to be established, as the proof of the general equation yields only a guide to the proof of its instance. Hilbert points out, as a second consequence of this view, that the usual logical relations between general and existential statements do not obtain. After all, the truth of a general statement is usually equivalent to the non-existence of a counterexample. Under the given constructive interpretation the alternative between a general statement and the existence of a counterexample would be evident only with the additional assumption "Every equation without a counterexample is provable from the assumed arithmetic principles", as the meaning of the general statement depends on the underlying system of

[49] [14], p. 12. The German text: Ursprünglich wollte auch Hilbert den engeren Standpunkt einnehmen, der nicht den anschaulichen Allgemeinbegriff der Ziffer voraussetzt. Das ist unter anderem aus seinem Heidelberger Vortrag (1904) zu ersehen. Es war schon eine Art Kompromiss, dass er sich zu dem in seinen Publikationen eingenommenen finiten Standpunkt entschloss.—In [48], pp. 169 and 173, there is further evidence of this early "strict finitist" view.

[50] Eine solche Gleichung … wird nicht aufgefasst als eine Aussage über alle Zahlen, vielmehr wird sie so gedeutet, dass ihr Sinn sich in einem Beweisverfahren erschöpft, bei welchem jeder Schritt eine vollständig aufweisbare Handlung ist, die nach festgesetzten Regeln vollzogen wird. (p. 60)

inference rules.[51] The lecture notes conclude with this (judicious) statement in which Brouwer's name appears for the very first time:

> This consideration helps us to gain an understanding for the sense of the paradoxical claim, made recently by Brouwer, that for infinite systems the law of the excluded middle (the "tertium non datur") loses its validity.[52]

It must have been a discouraging conclusion for Hilbert to see that this new approach could not secure the foundations of classical mathematics either. However, he overcame the setback by taking a second strategic step in the lectures for the summer term 1920 that joined the considerations concerning a thoroughly constructive foundation of number theory with the detailed formal logical work. Recall, that already in his Heidelberg talk of 1904 and again in his Zürich lecture of 1917, Hilbert had argued for a "Beweistheorie", but had not pursued his suggestion systematically. Here, in Section 7 of the notes from the summer term 1920, we do find initial steps, namely a consistency proof for an extremely restricted, quantifier-free part of elementary number theory that involves negations only as applied to equations.

These considerations, slightly modified, can be found in the first part of Hilbert's paper *Neubegründung der Mathematik*, a paper that is based on talks given in Copenhagen and Hamburg during the spring and summer of 1921; cf. Appendix A. The second part of the paper expands the basic set-up in new ways. Bernays pointed repeatedly to this "break" in the paper and describes its first part, for example in his [12], as "a remnant from that

[51] Hilbert mentions that this assumption would amount to the claim that all number theoretic questions are decidable; cf. p. 61. The relevant German text is: Ein allgemeines Urteil im eigentlichen Sinne ist dann und nur dann richtig, wenn es kein Gegenbeispiel gibt. Bei einer symbolischen Gleichung [i.e., an equation with free variables] wissen wir freilich in dem Falle, wo uns ein Gegenbeispiel bekannt ist, dass sie nicht richtig sein kann. Wir können aber nicht sagen, dass eine symbolische Gleichung stets entweder richtig oder durch ein Gegenbeispiel widerlegbar sein muss. Denn die Bedeutung der richtigen Formeln hängt ja von dem System der Beweisregeln ab, und jenes Entweder-Oder wäre nur unter der Voraussetzung selbstverständlich, dass mit Hülfe der Beweisregeln jede nicht widerlegbare symbolische Gleichung bewiesen werden kann. (p. 61)

The obvious historical question here is, what did Hilbert know about Brouwer's views. In [12] the following papers are listed, when the impact of Brouwer and Weyl in 1920 is discussed: [18], [19], [20], and [21] by Brouwer and [104] and [105] by Weyl. One should recall in this context also that in 1919 Brouwer had been offered a professorship in Göttingen. More precisely, according to a private communication of Dirk van Dalen, "... the decision of the Göttingen faculty to put Brouwer no. 1 on the list for the chair was made on 30.10.1919". Cf. also note 56.

[52] Wir gewinnen durch diese Überlegung ein Verständnis für den Sinn der neuerdings durch Brouwer aufgestellten paradoxen Behauptung, dass bei unendlichen Systemen der Satz vom ausgeschlossenen Dritten (das "tertium non datur") seine Gültigkeit verliere. (pp. 61–2)

stage, at which this separation [between the formalism and metamathematical considerations] had not been made yet".[53] The new ways are pursued further in the lectures given during the winter term 1921/22; how direct the connections are can be appreciated from Bernays's outline, *Disposition*, for the lectures that is contained in Appendix A.

C2. Finitist proof theory. The 1921/22 lectures contain for the first time the terms *finite Mathematik*, *transfinite Schlussweisen*, *Hilbertsche Beweistheorie*, and their third part is entitled: The founding of the consistency of arithmetic by the new Hilbertian proof theory (or in German, Die Begründung der Widerspruchsfreiheit der Arithmetik durch die neue Hilbertsche Beweistheorie). The clear separation of mathematical and metamathematical considerations allows Hilbert to address, finally, Poincaré's critique by distinguishing between contentual, metamathematical and formal, mathematical induction; this point is emphasized in early publications, namely, [8], [65], but most strongly in [69]. Hilbert claims in the last paper, presented as a talk in Hamburg, that Poincaré arrived at "his mistaken conviction by not distinguishing these two methods of induction, which are of entirely different kinds" and feels that "[u]nder these circumstances Poincaré had to reject my theory, which, incidentally, existed at that time only in its completely inadequate early stages".[54] Weyl, responding to Hilbert's talk, turns the argument around and justly claims that "... Hilbert's proof theory shows Poincaré to have been exactly right on this point". After all, Hilbert has to be concerned not just with particular numerals, but "with an *arbitrary concretely given* numeral", and the contentual arguments of proof theory must "be carried out *in hypothetical generality*, on *any* proof, on *any* numeral". Weyl recognizes clearly the significance of the distinction and its importance for the fully articulated proof theoretic enterprise; he sees it as facing the two complementary tasks of formalizing classical mathematics without reducing its "inventory" and of proving the consistency within the limits of "contentual thought".

That there are limits to contentual thought (inhaltliches Denken) was established, according to [106], by Brouwer. An obviously related fundamental insight was obtained, as we saw, in Hilbert's notes for the winter term 1920, i.e., at the very beginning of 1920. It is of greatest interest to know in what ways Hilbert may have been influenced by Brouwer (or Weyl, as will be discussed below and in footnote 56); that there was some influence can be taken for granted; after all, Brouwer is mentioned at the end of the notes. Hilbert's insight was based on an interpretation of quantifiers that is bound

[53][12], p. 203.

[54][69], p. 473. How important this critique was can be seen from Weyl's remarks below, but also from the writings of others, for example, Skolem; see his papers [96] and [97]. In the introduction to [106] in *From Frege to Gödel*, pp. 480–1, one finds a very thoughtful discussion of the underlying issues.

up with a particular formal calculus. The understanding of quantifiers is explored anew in the context of an informal presentation of finitist number theory on pages 52 to 69 of the 1921/22 lectures and deepened in the long introduction to their third part. That part expands, as I described earlier, the second part of Hilbert's 1922 paper.

The interpretation is here no longer tied to a formal calculus that allows us to establish free-variable statements, but rather it assumes the "intuitive general concept of numeral" as part of the finitist standpoint.

> In intuitive number theory, the general sentences have a purely hypothetical sense. A sentence like
>
> $$a + b = b + a$$
>
> only means: given two numerals a, b, the additive composition of a with b yields the same numeral as the additive composition of b with a. There is no mention of the totality of all numbers. Furthermore, the existential sentences have in intuitive number theory only the meaning of partial-judgments, i.e., they are substatements of more precisely determined statements, whose precise content, however, is inessential for many applications.
>
> ... thus, in general, a more detailed sentence complements in intuitive number theory an existential judgment; the sentence determines more precisely the content of that judgment. The existential claim here has sense only as a pointer to a search procedure which one possesses, but that ordinarily need not be elaborated, because it suffices generally to know that one has it.[55]

This is exactly the understanding that is formulated in 1925 in *Über das Unendliche* (pp. 172–3) and, most extensively, in 1934 in the first volume of *Grundlagen der Mathematik*; it is also strikingly similar to Weyl's viewpoint

[55]In der anschaulichen Zahlentheorie haben die allgemeinen Sätze rein hypothetischen Sinn. Ein Satz wie

$$a + b = b + a$$

besagt nur: Wenn zwei Zahlzeichen a, b gegeben sind, so liefert die additive Zusammensetzung von a mit b dasselbe Zahlzeichen wie die additive Zusammensetzung von b mit a. Von der Gesamtheit aller Zahlen ist dabei nicht die Rede. Ferner, die existenzialen Sätze haben in der anschaulichen Zahlentheorie nur die Bedeutung von Partial-Urteilen, d.h. sie sind Teilaussagen von näher bestimmten Aussagen, deren genauer Inhalt jedoch für viele Anwendungen unwesentlich ist.

... so gehört allgemein in der anschaulichen Zahlentheorie zu einem existenzialen Urteil ein genauerer Satz, welcher den Inhalt jenes Urteils näher bestimmt. Die Existenzbehauptung hat hier überhaupt nur einen Sinn als ein Hinweis auf ein Verfahren der Auffindung, welches man besitzt, das man aber für gewöhnlich nicht näher anzugeben braucht, weil es im allgemeinen genügt, zu wissen, dass man es besitzt. (pp. 67–8) in [105].[56] With this understanding of quantifiers the conclusion concerning the non-validity of the law of the excluded middle is again obtained. Hilbert points out:

> Thus we see that, for a strict foundation of mathematics, the usual inference methods of analysis must not be taken as logically obvious. Rather, it is exactly the task for the foundational investigation to recognize why it is that the application of transfinite inference methods as used in analysis and (axiomatic) set theory leads always to correct results.[57]

As that recognition has to be obtained on the basis of finitist logic, Hilbert argues, we have to extend our considerations in a different direction to go beyond elementary number theory:

> We have to extend the domain of objects to be considered; i.e., we have to apply our intuitive considerations also to figures that are not number signs. Thus we have good reason to distance ourselves from the earlier dominant principle according to which each theorem of pure mathematics is in the end a statement concerning integers. *This principle was viewed as expressing a fundamental methodological insight, but it has to be given up as a prejudice*.[58]

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[56]Weyl's paper must have been known to Hilbert in 1921: in Hilbert's *Neubegründung der Mathematik* one finds the remark (on p. 160), "Wenn man von einer Krise spricht, so darf man jedenfalls nicht, wie es Weyl tut, von einer neuen Krise sprechen." This is obviously an allusion to the title of [105]. According to [100], p. 145, a draft of Weyl's paper was completed by May 1920, and a copy sent to Brouwer.—What is puzzling here is the circumstance that Weyl's views are, in some important respects (the understanding of quantifiers is one such point) close to the finitist standpoint; Weyl presents them as being different from Brouwer's, and Brouwer in turn recognizes immediately that Weyl is "in the restriction of the object of mathematics" even more radical than he himself; cf. [100], p. 148 and p. 167. Why did it take the people in the Hilbert school such a long time to recognize that finitism was more restrictive than intuitionism? In a letter to Hilbert dated 25. X. 1925, Bernays mentions "a certain difference between the finitist standpoint and that of Brouwer"; but there is no elaboration of what this difference might be, and I don't know of any place where it is discussed by members of the Hilbert school before 1933. Indeed, in [10], the mathematical methods of finitism and intuitionism are viewed as co-extensional; it is only in the context of the Gödel-Gentzen reduction of classical to intuitionistic arithmetic that both Gödel and Gentzen point out that finitism is more restrictive than intuitionism; cf. [38], p. 294. This fact is then discussed in [11], p. 77; the significance of the result is described in [15].

[57]Wir sehen also, dass für den Zweck einer strengen Begründung der Mathematik die üblichen Schlussweisen der Analysis in der Tat nicht als logisch selbstverständlich übernommen werden dürfen. Vielmehr ist es gerade erst die Aufgabe für die Begründung, zu erkennen, warum die Anwendung der transfiniten Schlussweisen, sowie sie in der Analysis und in der (axiomatisch begründeten) Mengenlehre geschieht, stets richtige Resultate liefert. (p. 4a)

[58](The emphasis is mine; WS.) Wir müssen den Bereich der betrachteten Gegenstände erweitern, d.h. wir müssen unsere anschaulichen Überlegungen auch auf andere Figuren als auf Zahlzeichen anwenden. Wir sehen uns somit veranlasst, von dem früher herrschenden

This is a strong statement against a tradition that started with Dirichlet and includes such distinguished mathematicians as Weierstrass and Dedekind; it is also a surprising statement in the sense that such an extension was obviously implicit in Hilbert's earlier formulations of "Beweistheorie", for example in his [60]. But what is the new extended domain of objects, and what has to be preserved from the "fundamental methodological insight"? As to the domain of objects, it is clear what has to be included, namely the formulas and proofs from formal theories. By contrast, geometric figures are definitely excluded; the reason for holding that geometric figures are "not suitable objects" for Hilbert's considerations is articulated as follows:

> ... the figures we take as objects must be completely surveyable and only discrete determinations are to be considered for them. It is only under these conditions that our claims and considerations have the same reliability and evidence as in intuitive number theory.[59]

From this *new standpoint*, as he calls it, Hilbert exploits the formalizability of a fragment of number theory in full first order logic to formulate and prove its consistency. So, here we finally close the gap to the published record—with a fully developed programmatic perspective. I intend to give a proper mathematical exposition of this early work, including the elementary consistency proofs from [52], winter term 1920, summer term 1920, and the winter term 1921/22. The exposition will emphasize the inductive generation of syntactic structures and, based thereon, proofs by induction and definition by recursion; that is only natural, as soon as one has taken the methodological step Hilbert suggested. It was most strongly emphasized by von Neumann in his [102].

If we take this expansion of the domain of objects seriously, we are dealing not just with numerals, but more generally with elements of inductively generated classes. (The generation has to be elementary and deterministic, in modern terminology.) A related point was made by Poincaré, when he emphasized after discussing the principle of induction for natural numbers:

> I did not mean to say, as has been supposed, that all mathematical reasonings can be reduced to an application of this principle. Examining these reasonings closely, we should see applied there many other analogous principles, presenting the same essential

Grundsatz abzugehen, wonach jeder Satz der reinen Mathematik letzten Endes in einer Aussage über ganze Zahlen bestehen sollte. Dieses Prinzip, in welchem man eine grundlegende methodische Erkenntnis erblickt hat, müssen wir jetzt als ein Vorurteil preisgeben. (p. 4a)

[59] An einer Forderung aber müssen wir festhalten, dass nämlich die Figuren, welche wir als Gegenstände nehmen, vollkommen überblickbar sind, und dass an ihnen nur diskrete Bestimmungen in Betracht kommen. Denn nur unter diesen Bedingungen können unsere Behauptungen und Überlegungen die gleiche Sicherheit und Handgreiflichkeit haben wie in der anschaulichen Zahlentheorie. (p. 5a)

> characteristics. In this category of principles, that of complete induction is only the simplest of all and this is why I have chosen it as a type. (p. 1025)

The difficult issue is to recognize from Hilbert's standpoint induction and recursion principles. When discussing in his [37] the "unobjectionable methods" by means of which consistency proofs are to be carried out, Gödel formulates a first characteristic of the strictest form of constructive mathematics as follows:

> The application of the notion of "all" or "any" is to be restricted to those infinite totalities for which we can give a finite procedure for generating all their elements (as we can see, e.g., for the totality of integers by the process of forming the next greater integer and as we cannot, e.g., for the totality of all properties of integers).

According to the second characteristic, existential statements are viewed as abbreviations indicating that an example has been found; and thus there is essentially only one way of establishing general propositions, namely, "complete induction applied to the generating process of our elements". Only decidable properties and calculable functions are to be introduced. As the latter, according to Gödel, can always be defined by complete induction, the system for this form of constructive mathematics (and Gödel assumes that this is really finitist mathematics) is "exclusively based on the method of complete induction in its definitions as well as in its proofs". Gödel believes, with Poincaré and Hilbert, that "the method of complete induction" has a "particularly high degree of evidence". But what is the nature of this evidence? In spite of much important work that has been done for elementary number theory, this is still a significant question and should be addressed. The suggestion that the work for number theory covers all the bases, because of a simple effective Gödel numbering, misses the opportunity of articulating in greater generality the evidential features of inductively generated objects, constructed in elementary and less elementary ways.[60]

Finally, there is ample room to improve our understanding of Hilbert's and Bernays's views on the matter. I take it, for example, that Gödel's attempt to characterize the finitist standpoint in his 1958 paper is in conflict with their views and with his own informal description of the central features of finitist mathematics sketched above. At issue is whether the insights needed to carry out proofs concerning finitist objects spring purely from the combinatorial (spatiotemporal) properties of the sign combinations that represent them, or whether an element of "reflection" is needed, reflection that takes into account the uniform generation of the objects. The latter is explicitly affirmed in [10] and implicit, by my lights, in Hilbert's description

[60] That is, as a matter of fact, the starting point of my systematic considerations concerning "accessible domains" in [93] and [95].

of the "extra-logical concrete objects" that are needed to secure meaningful logical reasoning: such objects must not only be surveyable, but the fact that they *follow each other*, in particular, is immediately given intuitively together with the objects and cannot be further reduced.[61]

C3. A concise review. The dialectic of the developments that emerges from the lectures (given between 1917 and 1922) is described in Bernays's paper of 1922 and is also formulated very carefully on pp. 29-33 of the 1922/23 lectures. Here is Bernays's description that brings out the "Ansatzcharakter" of the proposed solution: in order to provide a rigorous foundation for arithmetic (that includes analysis and set theory) one proceeds axiomatically and starts out with the assumption of a system of objects satisfying certain structural conditions. However, in the assumption of such a system "lies something so-to-speak transcendental for mathematics, and the question arises, which principled position is to be taken [towards that assumption]". Bernays considers two "natural positions", positions that had been thoroughly explored as we saw. The first position, attributed to Frege and Russell, attempts to provide a foundation for mathematics by purely logical means; this attempt is judged to be a failure.

The second position is seen in counterpoint to the logical foundations of arithmetic: "As one does not succeed in establishing the logical necessity of the mathematical transcendental assumptions, one asks oneself, is it not possible simply to do without them." Thus one attempts a constructive foundation replacing existential assumptions by construction postulates; that is the second position and is associated with Kronecker, Poincaré, Brouwer, and Weyl. The methodological restrictions to which this position leads are viewed as unsatisfactory, as one is forced "to give up the most successful, most elegant, and most proven methods only because one does not have a foundation for them from a particular standpoint".

Hilbert takes from these foundational positions, Bernays continues in his analysis, what is "positively fruitful": from the first the strict formalization of mathematical reasoning; from the second the emphasis on constructions. Hilbert does not want to give up the constructive tendency, but emphasizes it on the contrary in the strongest possible terms. Finitist mathematics is viewed as part of an "Ansatz" to finding a principled position towards the transcendental assumptions:

> Under this perspective[62] we are going to try, whether it is not possible to give a foundation to these transcendental assumptions in such a way that only primitive intuitive knowledge is used.[63]

[61]That description is found in [65] on pp. 162/3, but also later in [68], p. 171, and [69], p. 65.

[62]Of taking into account the tendency of the exact sciences to use as far as possible only the most primitive "Erkenntnismittel". That does not mean, as Bernays emphasizes, to deny any other, stronger form of intuitive evidence.

The program is taken as a tool for an alternative constructive foundation of all of classical mathematics. The great advantage of Hilbert's method is judged to be this: "the problems and difficulties that present themselves in the foundations of mathematics can be transferred from the epistemological-philosophical to the properly mathematical domain." So Bernays, without great fanfare, gives an illuminating summary of about four years of quite intense work!

Concluding remarks. I find absolutely remarkable the free and open way in which Hilbert and Bernays joined, in the end, a number of different tendencies into a sharply focused program with a special mathematical and philosophical perspective. The metamathematical core of the program amounts to this: classical mathematics is represented in a formal theory **P**, expressing "the whole thought content of mathematics in a uniform way"; based on this representation, it is *programatically* taken as a *formula game*. But the latter aspect should not be over-emphasized, as there are other important considerations, namely that intended mathematical structures are projected through their (assumed complete) formalizations into the properly mathematical domain, i.e., finitist mathematics.[64] In any event, the consistency of **P** has to be established within finitist mathematics **F**. **P**'s consistency is in **F** equivalent to the reflection principle, expressing formally the soundness of **P**:

$$(\forall x)(\mathrm{Prf}(x', s') \Longrightarrow s).$$

Prf is the finitist proof predicate for **P**, s a finitist statement, and 's' the corresponding formula in the language of **P**. A consistency proof in **F** would show, because of this equivalence, that the formal, technical apparatus **P** can serve reliably as an instrument for the proof of finitist statements.

At first it seemed as if Hilbert's approach would yield proof theoretic results rather quickly and decisively: Ackermann's "proof" of the consistency of analysis was published in 1925, but had been submitted on March 30, 1924! However, difficulties emerged and culminated in the real obstacles presented by Gödel's Incompleteness Theorems. The program has been transformed, quite in accord with the broad strategy underlying Hilbert's proposal, to a *general reductive* one; here one tries to give consistency proofs for strong classical theories relative to "appropriate constructive" theories. Even Gödel found the mathematical reductive program with its attendant philosophical one attractive in the thirties; his illuminating reflections, partly in an examination of Gentzen's first consistency proof for arithmetic, are presented in previously unpublished papers[65] that are now available in the third

[63] [8], p. 11.
[64] Cf. Section 2.1 of [93].
[65] I am thinking in particular of [37], [39], and [40].

volume of his *Collected Works*. Foundationally inspired work in proof theory is being continued, weaving strong set theoretic and recursion theoretic strands into the metamathematical work.

This expanding development of proof theory is but *one* effect of Hilbert's broad view on foundational problems and his sharply articulated questions. Another effect is plainly visible in the rich and varied contributions that were given to us by Hilbert, Bernays, and other members of the Hilbert School (Ackermann, von Neumann, Gentzen, Schütte); finally, we have to consider also the stimulus his approach and questions provided to contemporaries outside the school (Herbrand, Gödel, Church, Turing and, much earlier already, Zermelo). Indeed, there is no foundational enterprise with a more profound and far-reaching effect on the emergence and development of modern mathematical logic; it could, if we just cared to be open, have a similar effect on philosophical reflections concerned with mathematical experience: it can help us to gain a perspective that includes traditional philosophical concerns, but that, most importantly, allows us to ask questions transcending traditional boundaries.

Appendix A: Lectures and early papers. Here I am providing some information on (i) when the lectures of the winter term 1920 were most likely given (or the notes written) and (ii) the connection of the lectures during the early twenties with the first published accounts of Hilbert's proof theory, including their chronology as far as I can determine it presently. Quite a few specific issues remain that could be resolved with some additional archival work (and a little luck in finding appropriate documents).

As to (i), it is perfectly clear from the content of these lectures that they preceded those of the summer term 1920; the (small) puzzle is that all other winter term lectures have the indication of their year in the form 19xx/xx+1. This is the general rule, but for the years 1919 and 1920 there was an exception (as Ralf Haubrich found out in Göttingen in response to an inquiry of mine). Because of the end of World War I and soldiers having returned to the university, an extra semester was pressed into those two years: there was a "Zwischensemester" in 1919 (from September 22 to December 20); that was followed by the winter term 1920 and, then, by the regular summer term 1920 beginning on April 26.

At the moment I only know an upper bound for the completion of the notes for the 1917/18 lectures, as Bernays's Habilitationsschrift, submitted in 1918, mentions the "Ausarbeitung" in note 1 on page IV with correct page references to the relevant sections on sentential logic.—Now let me proceed systematically with (ii).

Hilbert's [65] (*Neubegründung der Mathematik*) was based on lectures given in Kopenhagen (Spring 1921) and Hamburg (Summer 1921); the paper contains on pages 168–174 material that overlaps with material presented on pages 33–46 of *Probleme der mathematischen Logik* (summer term 1920) and on pages 174–177 material from the very beginning of Part III of *Grundlagen der Mathematik* (winter term 1921/22). The two different parts of the paper were distinguished by the editors of Hilbert's *Gesammelte Abhandlungen* in note 2 on page 168: "Die hier folgenden Betrachtungen greifen auf ein früheres Stadium der Beweistheorie zurück, in welchem die Untersuchung sich zunächst auf einen ganz engen Formalismus beschränkte, der dann schrittweise verschiedene Erweiterungen erfuhr. Dieser Gedankengang wird im folgenden dargestellt und hernach—auf S. 174 ff—der Übergang von jenem provisorischen Ansatz zu dem in der vorliegenden Abhandlung intendierten Formalismus vollzogen." The intended formalism is further investigated in Part III of *Grundlagen der Mathematik*. This reflects, in a very understated way, the dramatic methodological shift that is analyzed in C1 and C2 above.

It seems, but we don't have any notes for this, that Hilbert gave also a course on foundational matters in the winter term 1920/21. There are a few written communications between Hilbert and Bernays; one of them is a postcard written on October 22, 1920 and sent to Bernays from Switzerland. Hilbert announces that he will be back in Göttingen on Monday night and asks Bernays to stop by on Tuesday morning (at 11 a.m.). The point of the meeting is described as follows:

> Wir müssen vor Allem das Donnerstags-Colleg vorbereiten. Ich möchte gern als Einleitung etwas Allgemeines über reine Anschauung und reines logisches Denken sagen, die beide in der Math. eine so grosse Rolle spielen und übrigens auch in meiner gegenwärtigen Beweistheorie—die beständig ganz gute Fortschritte macht—gleichzeitig in merkwürdiger Verknüpfung stehen. Es wäre vielleicht angebracht, wenn ich auf solche allgemeinen Fragen am Schlusse des Collegs zu sprechen käme. Könnten Sie vielleicht sich für Donnerstag etwas Einleitendes überlegen?

At the very beginning of the postcard Hilbert had already mentioned that he quite agreed with the "Disposition" for the "Colleg" that had been drafted by Bernays.

It is of real interest to consider the "Disposition" for the 1921/22 lectures that was proposed by Bernays before returning to Göttingen for that term in his letter to Hilbert of 17. X. 1921 (with annotations in Hilbert's handwriting which are not reproduced here).

> I. Bisherige Methoden der Beweise für Widerspruchslosigkeit oder Unabhängigkeit.
>   A. Methode der Aufweisung.
>   Beispiel des Aussagenkalküls in der mathem[atischen] Logik.
>   B. Methode der Zurückführung.

Beispiele:
1) Widerspruchslosigkeit der Euklidischen Geometrie
2) Unabhängigkeit des Parallenaxioms
3) Widerspruchslosigkeit des Rechnens mit komplexen Zahlen

II. Versuche der Behandlung des Problems der Widerspruchslosigkeit der Arithmetik.
   A. Die Zurückführung auf die Logik bietet keinen Vorteil, weil der Standpunkt der Arithmetik schon der formal allgemeinste ist. (Frege; Russell)
   B. Die konstruktive Arithmetik: Definition der Zahl als Zeichen von bestimmter Art.

III. Die weitere Fassung des konstruktiven Gedankens: Konstruktion der Beweise, wodurch die Formalisierung der höheren Schlußweisen gelingt und das Problem der Widerspruchslosigkeit in allgemeiner Weise angreifbar wird.
Hier würde sich dann die Ausführung der Beweistheorie anschließen.

This reflects much more clearly than the remark (from the editors in Hilbert's "Gesammelte Abhandlungen" I quoted above) the significance of the methodological step that had been taken, when "the earlier dominant principle according to which each theorem of pure mathematics is in the end a statement concerning integers" was viewed as a prejudice. Interestingly, in the case of these lectures, we not only have the above "Disposition" and the official lecture notes written by Bernays, but also the *Mitschrift* of Kneser; Kneser's notes show the lectures in real-time progress beginning with the first meeting on 31. X. 1921. The *Mitschrift* shows, first of all, that the lectures proceeded according to Bernays' "Disposition" and, secondly, that towards the end of the lectures (on February 27, 1922) the logical τ-function was introduced. That function was to play a prominent role in Hilbert's 1923-paper (submitted for publication on September 29, 1922); it is replaced by the ε-symbol already in the lectures of the following winter term 1922/23. A detailed comparison of Bernays's Notes and Kneser's *Mitschrift* might be of genuine interest, in particular if one considers also the Notes and *Mitschrift* for the winter term 1922/23.

Bernays's [8] (*Über Hilberts Gedanken zur Grundlegung der Mathematik*) was presented at the September meeting of the German Mathematical Association (DMV) in Jena and was received for publication by the ***Jahresberichte der DMV*** on October 13, 1921. In the letter to Hilbert dated 17. X. 1921, in which he proposed the "Disposition", Bernays also wrote: "Wie Sie wohl wissen, habe ich an der Tagung in Jena teilgenommen und dort über Ihre neue Theorie vorgetragen. Mit dem Interesse, welches mein Vortrag fand, konnte ich sehr zufrieden sein; und ich habe ihn übrigens zur Veröffentlichung in den ***Jahresberichten d[er] Mathematiker V[ereinigun]g*** (auf Veranlassung von Prof. Bieberbach) ausgearbeitet.—Man fragte mich des öfteren, wie es mit der Publikation Ihrer Hamburger Vorträge stehe. Ich wußte in dieser Hinsicht über Ihre Absichten nicht recht Bescheid. Jedenfalls würde Hecke diese Vorträge gern in der neuen Hamburger Zeitschrift drucken." Note that Bernays talks of Hilbert's "new" theory!

Bernays's [7] (*Die Bedeutung Hilberts für die Philosophie der Mathematik*), appeared in ***Naturwissenschaften***, Heft 4, with 27. I. 1922 as its publication date; it must have been prepared during the late summer/fall of 1921, as Bernays refers explicitly to Hilbert's Hamburg lectures. Finally, a paper I did not discuss extensively, Hilbert's 1923 (*Die logischen Grundlagen der Mathematik*), was based on a lecture given at the Leipzig meeting of the Deutsche Naturforscher-Gesellschaft in September 1922; the paper was received for publication by the Mathematische Annalen on September 29, 1922. It contains a summary of the consistency proof given in the lectures of the winter term 1921/22 and the first step toward a treatment of quantifiers.

Appendix B: Correspondence with Russell. Alasdair Urquhart informed me about the Russell-Hilbert connection. First of all, Urquhart mentioned that in the *Selected Letters of Bertrand Russell*, Volume 1, Nicholas Griffin (ed.), there is a letter from Russell to Lady Ottoline Morrell of 18 January 1914 that contains the following passage: "Littlewood tells me that Hilbert (the chief mathematical professor there) has grown interested in Whitehead's and my work, and that they think of asking me to lecture there next year. I hope they will." (p. 487) In this letter Russell says of Göttingen: "It makes one's mouth water to hear how many good students they have, and what advanced lectures are attended in large numbers."

Secondly, Urquhart also pointed out a brief passage in Constance Reid's Hilbert biography; one finds on p. 144 the following remarks: "The lack of contact with foreign mathematicians was extremely frustrating to Hilbert. Just before the war Bertrand Russell, with A. N. Whitehead, had published his ***Principia Mathematica***. Hilbert was convinced that the combination of mathematics, philosophy and logic represented by Russell should play a greater role in science. Since he could not now bring Russell himself to Göttingen, he set about improving the position of his philosopher friend Leonard Nelson."

Thirdly (and most importantly), he pointed me to the exchange of postcards between Russell and Hilbert; some are preserved in the Russell Archives at McMaster University, Hamilton. Here are their transcriptions:

Postcard from Hilbert to Russell, dated April 12, 1916:

> Hochgeehrter Herr Kollege.
>
> Ich gestatte mir Ihnen mitzuteilen, dass wir uns in der math. Gesellschaft schon seit langem mit Ihrer Erkenntnistheorie beschäftigen und dass wir gerade vor Ausbruch des Krieges die Absicht hatten,

Sie—von der Wohlfskehlstiftung[66] aus—nach Göttingen einzuladen, damit Sie uns persönlich über Ihre Lösung des Paradoxienproblem [sic] einen Zyklus von Vorträgen halten könnten. Ich hoffe, dass die Ausführung dieses Planes durch den Krieg nicht aufgehoben, sondern nur aufgeschoben worden ist.

Mit ausgezeichneter Hochachtung, Hilbert

Postcard from Hilbert to Russell, dated May 24, 1919:

Hochgeehrter Herr Professor:

Vielen Dank für Ihre Karte, die ich vor drei Jahren via Hecke—Basel erhielt. Hoffentlich bereuen Sie nicht die Zusage, die Sie damals hinsichtlich der Abhaltung von Vorträgen über Logik und Erkenntnistheorie in Göttingen gemacht haben. Vorläufig freilich sind die Zustände in Deutschland noch zu unerfreulich. Wir haben indess in unserem mathematisch-philosophischen Göttinger Kreise das Studium Ihrer Werke eifrig betrieben und versprechen uns viel von Ihrer persönlichen Anwesenheit in Göttingen.

In Erwartung besserer Zeiten und der Wiederherstellung der internationalen Gelehrten-Gemeinschaft bin ich mit bestem Gruss Ihr ergebenster Hilbert

Postcard from Russell to Hilbert, dated June 4, 1919:

Dear Professor Hilbert

My best thanks for your postcard. I in no way repent of what I wrote before. I am very glad of what you say, and I hope correctly that better times for all will return sooner than now seems probable. When it is possible, I should like nothing better than to carry out your interrupted project, and to contribute what one man can to the restoration of international scientific cooperation.

Yours very truly,
Bertrand Russell.

The crucial issue is: which of Russell's writings had actually been read in Göttingen?[67]—Finally, there is a (draft of a) letter of Russell's wife, written on May 20, 1924 and responding to an inquiry of Hilbert, whether Ackermann could study with Russell in England. Mrs. Russell relates: "My husband, Bertrand Russell, is away in America, but will be back before very long. He asks me to say that he would be very glad indeed to have Dr. Ackermann study with him in England."

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[66]Paul Wolfskehl (1856–1906) gave 100,000 German marks to the University of Göttingen to be awarded to the first person to give a (correct) proof of Fermat's Last Theorem. The interest from that fund was used in 1911 and 1912 to invite Poincaré, Lorentz, and Sommerfeld for lecture series in Göttingen.

[67]Mancosu's manuscript [77] contains additional, important information that should be explored properly (but this came to my attention only in June of 1998, too late for this paper).

Appendix C: Lectures from winter term 1922/23. They contain a very informative discussion of the (new) finitist standpoint and the formal presentation of mathematics that allow Hilbert to address the consistency problem in a novel way. Let me quote extensively from pp. 29–33:

> In der Verfolgung dieses Zieles, das Gesamtgebäude der Mathematik zu sichern, wurden wir auf zwei Gesichtspunkte geführt:
>
> Der eine betraf die *finite Einstellung*, welche im Bereiche der elementaren Zahlenlehre auch ausreichend ist, während für die Analysis die transfiniten Schlussweisen wesentlich und unentbehrlich sind.
>
> Der andere Gesichtspunkt bestand in der Präzisierung der Sprache, soweit sie zur Darstellung der mathematischen Tatsachen und logischen Zusammenhänge in Betracht kommt. Die Präzisierung geschieht durch den Formalismus des logischen Kalküls, in welchem sich alle logischen Schlüsse, auch die transfiniten Schlussweisen, formal darstellen lassen.
>
> Wenn wir nun diese beiden Gesichtspunkte neben einander halten, so kann uns dies darauf bringen, sie in einer neuen Weise zu verknüpfen.
>
> Nämlich die Zeichen und Formeln des logischen Kalküls sind ja durchweg finite Objekte, wenngleich durch sie auch die transfiniten Schlüsse zur Darstellung kommen. Wir haben also die Möglichkeit, diese Formeln selbst zum Gegenstande inhaltlicher finiter Überlegungen zu machen, ganz entsprechend wie es in der elementaren Zahlenlehre mit den Zahlzeichen geschieht.
>
> Natürlich ist der Formalismus, mit dem wir es dann zu tun haben, viel mannigfaltiger und komplizierter als derjenige der Zahlzeichen: umfasst er doch alle (in Formeln ausgedrückten) mathematischen Beziehungen.
>
> Dafür trägt er aber auch weiter, und wir können erwarten, mit Hilfe dieser weitergehenden Formalisierung die Gesamtmathematik in den Bereich der finiten Betrachtung zu ziehen.

After a further discussion of the logical calculus the following question is raised: "Was nützt uns nun diese Methode der Formalisierung und die Einsicht, dass die formalisierten Beweise finite Objekte sind, für unser Problem der Begründung der Analysis?" It is answered on p. 33:

> Ebenso können wir nun bei der Begründung der Analysis von dem Wahrheitsgehalt der Axiome und Sätze absehen, wenn wir uns nur die Gewähr verschaffen können, dass alle Ergebnisse, zu denen die Prinzipien und die Schlussmethoden der Analysis führen, im Einklang mit einander stehen, sodass wir nicht, wie bisher immer, nur auf guten Glauben die Widerspruchsfreiheit annehmen und der Möglichkeit ausgesetzt sind, eines Tages durch ein Paradoxon

überrascht zu werden, wie es z.B. Frege in so dramatischer Weise geschah.

Wenn wir uns nun auf diesen Standpunkt stellen und also die Aufgabe der Begründung ausschliesslich darin sehen, zu zeigen, dass die üblichen transfiniten Schlüsse und Prinzipien der Analysis (und Mengenlehre) nicht auf Widersprüche führen können, so wird für einen solchen Nachweis in der Tat durch unsere vorigen Gedanken eine grundsätzliche Möglichkeit eröffnet.

Denn das Problem der Widerspruchsfreiheit gewinnt nunmehr eine ganz bestimmte, greifbare Form: es handelt sich nicht mehr darum, ein System von unendlich vielen Dingen mit gegebenen Verknüpfungs-Eigenschaften als logisch möglich zu erweisen, sondern es kommt nur darauf an, einzusehen, dass es unmöglich ist, aus den in Formeln vorliegenden Axiomen nach den Regeln des logischen Kalküls ein Paar von Formeln wie A und $\neg A$ abzuleiten.

Hier kommt es zur Geltung, dass die Beweise, wenn sie auch inhaltlich sich im Transfiniten bewegen, doch, als Gegenstände genommen und formalisiert, von finiter Struktur sind. Aus diesem Grunde ist die Behauptung, dass aus bestimmten Axiomen nicht zwei Formeln A, $\neg A$ bewiesen werden können, methodisch gleichzustellen mit inhaltlichen Behauptungen der anschaulichen Zahlentheorie, wie z.B. der, dass man nicht zwei Zahlzeichen a, b finden kann, für welche $a^2 = 2b^2$ [gilt].

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