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The Ways of Hilbert's Axiomatics: Structural and Formal

Wilfried Sieg

It is a remarkable fact that Hilbert's programmatic papers from the 1920s still shape, almost exclusively, the standard contemporary perspective of his views concerning (the foundations of) mathematics; even his own, quite different work on the foundations of geometry and arithmetic[1] from the late 1890s is often understood from that vantage point. My essay pursues one main goal, namely, to contrast Hilbert's *formal axiomatic method* from the early 1920s with his *structural axiomatic approach* from the 1890s. Such a contrast illuminates the circuitous beginnings of the finitist consistency program and connects the complex emergence of structural axiomatics with transformations in mathematics and philosophy during the 19th century; the sheer complexity and methodological difficulties of the latter development are partially reflected in the well known, but not well understood correspondence between Frege and Hilbert. Taking seriously the goal of formalizing mathematics in an *effective* logical framework leads also to contemporary tasks, not just historical and systematic insights; those are briefly described as "one direction" for fascinating work.

1. Context

Hilbert gave lectures on the foundations of mathematics throughout his career. Notes for many of them have been preserved and are treasures of information; they allow us to reconstruct the path from Hilbert's logicist position, deeply influenced by Dedekind and presented in lectures starting around 1890, to the program of finitist proof theory in the early 1920s. The development toward *proof theory* begins, in some sense, in 1917, when Hilbert gave his talk "Axiomatisches Denken" in Zürich. This talk is rooted in the past and points to the future. As to the future, Hilbert suggested:

[1] Arithmetic is understood in this early work not as dealing with natural but rather with real numbers.

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> We must—that is my conviction—take the concept of the specifically mathematical proof as an object of investigation, just as the astronomer has to consider the movement of his position, the physicist must study the theory of his apparatus, and the philosopher criticizes reason itself. (Hilbert 1918, 1115)

Hilbert recognized in the next sentence that "the execution of this program is at present, to be sure, still an unsolved problem." If one takes formalization of mathematical proofs as an important part of this program, then initial tentative steps were taken at the 1904 International Congress of Mathematicians in Heidelberg. Hilbert presented there an equational fragment of elementary number theory and used its formal structure as the basis for a syntactic consistency proof (by induction on derivations).

Four years earlier, Hilbert had articulated the need of a consistency proof for arithmetic in the Second Problem of his famous talk at the International Congress of Mathematicians in Paris; he wrote:

> I wish to designate the following as the most important among the numerous questions that can be asked with regard to the axioms: to prove that they are not contradictory, that is, that a finite number of logical steps based upon them can never lead to contradictory results. (Hilbert 1900b, 1104)

The axioms really concern analysis, i.e., the theory of complete ordered fields, and Hilbert points for their formulation to his paper *Über den Zahlbegriff*, which had been delivered at the meeting of the German Association of Mathematicians in September 1899. Its title indicates a part of the intellectual context, as Kronecker had published 12 years earlier a well-known paper with the same title (Kronecker 1887). In that paper, Kronecker sketched a way of introducing irrational numbers, without accepting the general notion. It is precisely to the *general concept* that Hilbert wanted to give a proper foundation—using the axiomatic method. The axiom system Hilbert formulated for the real numbers is not presented in the contemporary formal-logical style. Rather, it is given in an algebraic way and assumes that a system exists whose elements satisfy the axiomatic conditions; consistency proofs were to discharge that assumption. Because of this existence assumption, Hilbert and Bernays called this methodological approach *existential axiomatics* in the 1920s; I want to call it structural axiomatics and contrast it with formal axiomatics.

Section 2 of this chapter discusses structural axiomatics, whereas section 4 is devoted to the emergence and significance of formal axiomatics. The recognition of the dramatic difference between the two and the very character of the former is crucial for elucidating the different perspectives Frege and Hilbert expressed in their correspondence concerning Hilbert's *Grundlagen der Geometrie*; that topic is treated in the short interlude between sections 2 and 4. It is ironic that Frege saw a way of formulating Hilbert's view and the characteristic abstract element of modern mathematics, but insisted on a narrow *mis*understanding. What then is the methodological approach of structural axiomatics around 1900? How and for what purpose did Hilbert move, almost 20 years later, from it to formal axiomatics, using Frege's work as mediated by Whitehead and Russell's *Principia Mathematica* (1910–13)?

2. Structural Axiomatics

To begin with, Hilbert points out in *Über den Zahlbegriff* that the *axiomatic* way of proceeding is quite different from the *genetic method* used in arithmetic; it rather parallels the ways of geometry.

> Here [in geometry] one begins customarily by assuming the existence of all the elements, i.e., one postulates at the outset three systems of things (namely, the points, lines, and planes), and then—essentially after the model of Euclid—brings these elements into relationship with one another by means of certain axioms of linking, order, congruence, and continuity. [Hilbert should have included the axiom of parallels.] (Hilbert 1900a, 1092)

The geometric ways are taken over for the arithmetic of real numbers or rather, one might argue, are reintroduced into arithmetic by Hilbert; after all, they do have their origin in Dedekind's work on arithmetic and algebra. Hilbert frames and formulates the axioms for the real numbers in his (1900a) as follows: "We think a system of things, and we call them numbers and denote them by a, b, c. . . . We think these numbers to be in certain mutual relations, whose precise and complete description is obtained through the following axioms." Then the axioms for an ordered field are formulated and rounded out by the requirement of continuity via the Archimedean principle and the axiom of completeness.

This formulation is not only in the spirit of the geometric ways, but mimics Hilbert's contemporaneous and axiomatic presentation of *Grundlagen der Geometrie*, which is viewed even today as paradigmatically modern.

> We think three different systems of things: we call the things of the first system points and denote them by A, B, C, . . . ; we call the things of the second system lines and denote them by a, b, c, . . . ; we call the things of the third system planes and denote them by α, β, γ, . . . ; . . . We think the points, lines, planes in certain mutual relations . . . ; the precise and complete description of these relations is obtained by the axioms of geometry. (Hilbert 1899, 437)

Five groups of geometric axioms follow and, in the original Festschrift, the fifth group consists of just the Archimedean principle. In the French edition of 1900 and the second German edition of 1903, the completeness axiom is included. The latter axiom requires in both the geometric and the arithmetic case that the assumed structure is maximal, i.e., any extension satisfying the remaining axioms must already be contained in it. Hilbert's completeness formulations are frequently criticized as being metamathematical and, to boot, of a peculiar sort. However, they are just ordinary mathematical ones, if the abstract algebraic character of the axiom systems is kept in mind; they provide *structural definitions* of Euclidean space and the continuum, respectively. In the case of arithmetic we can proceed as follows: call a system *A continuous* when it satisfies the axioms of an ordered field and the Archimedean axiom, and call it *fully continuous* if and only if A is continuous and for any system B, if $A \subseteq B$ and B is continuous, then $B \subseteq A$. So Hilbert's axioms characterize fully continuous systems in analogy to the way in which Dedekind's conditions characterize simply infinite ones in (Dedekind 1888), or in which the axioms of group theory characterize groups.

Hilbert thought about axiom systems in this structural way already in his first lectures on the foundations of geometry. He had planned to give them in the summer term of 1893, but their presentation was shifted to the following summer term. Using the notions *System* and *Ding* so prominent in (Dedekind 1888), he formulated the central question as follows:

> What are the necessary and sufficient and mutually independent conditions a system of things has to satisfy, so that to each property of these things a geometric fact corresponds and conversely, thereby making it possible to completely describe and order all geometric facts by means of the above system of things? (Hilbert *1894, 72–73)

At a later point, Hilbert inserted the remark that this system of things provides a "complete and simple image of geometric reality." In the introduction to the notes for the 1898–99 lectures *Elemente der Euklidischen Geometrie*, this question is connected with Hertz's *Prinzipien der Mechanik*:

> Using an expression of Hertz (in the introduction to the *Prinzipien der Mechanik*) we can formulate our main question as follows: What are the necessary and sufficient and mutually independent conditions a system of things has to be subjected to, so that to each property of these things a geometric fact corresponds, and conversely, thereby having these things provide a complete "image" of geometric reality. (Hilbert *1898–99, 303)

One can see here the shape of a certain logical or set-theoretic structuralism in the foundations of mathematics and physics.[2] But what are the things whose system is implicitly postulated? As late as 1922 Hilbert articulated the *axiomatische Begründungsmethode* for analysis as follows:

> The continuum of real numbers is a system of things that are connected to each other by certain relations, so-called axioms.[3] In particular the definition of the real numbers by Dedekind cuts is replaced by two continuity axioms, namely, the Archimedean axiom and the so-called completeness axiom. In fact, Dedekind cuts can then serve to determine the individual real numbers, but they do not serve to define [the concept of] real number. On the contrary, conceptually a real number is just a thing of our system. . . . The standpoint just described is altogether logically completely impeccable, and it only remains thereby undecided, whether a system of the required kind can be thought, i.e., whether the axioms do not lead to a contradiction. (Hilbert 1922, 1118)

The remark "conceptually a real number is just a thing of our system" does not answer any question concerning the (nature of the) things making up the system, but it expresses a crucial element of structural axiomatics and is fully in line with Dedekind's views. In addition, the issue of consistency had been an explicit part of Dedekind's logicist program, and the further discussion of that issue will reveal details of Hilbert's position.

In the 19th century, logicians viewed the *consistency of a notion* from a semantic perspective as requiring a model. That is the way *we* put matters, whereas those earlier logicians, including Frege, saw themselves as facing the task of exhibiting a system that falls under the notion. Dedekind addressed the consistency problem for the notion of a simply infinite system exactly from such a traditional view. The methodological need for doing that is implicit in his (1872), but

[2] At this point one might also ask: What is the mathematical connection, in particular, between arithmetic and geometric structures? The informal comparison of the geometric line with the system of cuts of rational numbers in Dedekind's (1872) contains almost all the ingredients to establish these structures to be isomorphic; missing is the concept of mapping. That concept was available to Dedekind by 1879 and, with it, these considerations can be extended to show that arbitrary, fully continuous systems are isomorphic. The methodological remarks in (Dedekind 1888) about the arithmetic of natural numbers can now be extended to that of the real numbers.

[3] This is a peculiar formulation, even in the original German. As it happens, Hilbert formulated matters more precisely in his letter of September 22, 1900, addressed to Frege: "I am of the opinion that a concept can be logically determined only through its relations to other concepts. These relations, formulated in particular statements, I call axioms and thus I arrive at the view that the axioms . . . are the definitions of the concepts."

Here is the German text: "Meine Meinung ist eben die, dass ein Begriff nur durch seine Beziehungen zu anderen Begriffen logisch festgelegt werden kann. Diese Beziehungen, in bestimmten Aussagen formulirt [*sic*!], nenne ich Axiome und komme so dazu, dass Axiome . . . die Definitionen der Begriffe sind."

it is formulated most clearly in a letter to Keferstein dated February 27, 1890, a little more than a year after the publication of *Was sind und was sollen die Zahlen?*

> After the essential nature of the simply infinite system, whose abstract type is the number sequence N, had been recognized in my analysis (71, 73) the question arose: Does such a system exist at all in the realm of our thoughts? 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).

In article 66, Dedekind attempted to prove the existence of an infinite system within logic and, on the basis of that "proof," he provided in article 72 an example of a simply infinite system that was to guard against internal contradictions of the very notion.

Hilbert turned his attention to natural numbers around 1904 and used Dedekind's conditions for simply infinite systems, not as part of a structural definition, but as *formal* axioms. Until then he had taken for granted their proper foundation and focused on the notion of real numbers. Hilbert's retrospective remarks in (*1904) make this quite clear: the general concept of irrational number had created the "greatest difficulties," and Kronecker represented this point of view most sharply.[4] Those difficulties, Hilbert now claims, are overcome when the concept of natural number is secured, as the further steps toward real numbers can be taken without a problem. (It remains a puzzle why that was not as clear to Hilbert in 1899 as it had been to Dedekind in 1888; but see the discussion below.) This dramatic change of view raises the question, what did

[4] These issues are discussed in (Hilbert *1904, 164–167). The remark concerning Kronecker is found on pp. 165–166: "Die Untersuchungen in dieser Richtung [foundations for the real numbers] nahmen lange Zeit den breitesten Raum ein. Man kann den Standpunkt, von dem dieselben ausgingen, folgendermaßen charakterisieren: Die Gesetze der ganzen Zahlen, der Anzahlen, nimmt man vorweg, begründet sie nicht mehr; die Hauptschwiergkeit wird in jenen Erweiterungen des Zahlbegriffs (irrationale und weiterhin komplexe Zahlen) gesehen. Am schärfsten wurde dieser Standpunkt von Kronecker vertreten. Dieser stellte geradezu die Forderung auf: Wir müssen in der Mathematik jede Tatsache, so verwickelt sie auch sein möge, auf Beziehungen zwischen ganzen rationalen Zahlen zurückführen; die Gesetze dieser Zahlen andrerseits müssen wir ohne weiteres hinnehmen. Kronecker sah in den Definitionen der irrationalen Zahlen Schwierigkeiten und ging so weit, dieselben gar nicht anzuerkennen."

Here is the English translation: The investigations in this direction [concerning the foundations for real numbers] took the largest space for a long time. The standpoint from which they started can be characterized as follows: the laws for integers, the cardinal numbers, are taken for granted without any further justification; the main difficulty is seen in the extensions of the number concept (irrational and furthermore complex numbers). This standpoint was most strongly represented by Kronecker. He in fact required outright: in mathematics, we have to reduce every fact, however complicated it may be, to relations between whole rational numbers; the laws for these numbers, on the other hand, we have to accept without much ado. Kronecker saw difficulties in the definitions of irrational numbers and went so far as not to recognize them at all.

Hilbert see then as the "greatest difficulties" for the general concept of irrational numbers?

A somewhat vague, but nevertheless informative answer emerges from Hilbert's earlier discussion of a consistency proof for arithmetic; such a proof, Hilbert writes in *Über den Zahlbegriff*, should use "a suitable modification of familiar methods of reasoning." In the Paris lecture he suggested finding a *direct* proof and made "familiar methods of reasoning" more explicit:

> I am convinced that it must be possible to find a direct proof for the consistency of the arithmetical axioms [as proposed in *Über den Zahlbegriff* for the real numbers], by means of a careful study and suitable modification of the known methods of reasoning in the theory of irrational numbers. (Hilbert 1900b, 1104)

Hilbert believed at this point, it seems, that the genetic buildup of the real numbers could *somehow* be exploited to yield the blueprint for a semantic consistency proof in Dedekind's style. There are, however, difficulties with the genetic method that prevent it from easily providing a proper foundation for the general concept of irrational numbers. Hilbert's concerns are formulated most clearly in (Hilbert *1905, 10–11):

> It [the genetic method] defines things by generative processes, not by properties—what must really appear to be desirable. Even if there is no objection to defining fractions as systems of two integers, the definition of irrational numbers as a system of infinitely many numbers must appear to be dubious. Must this number sequence be subject to a law, and what is to be understood by a law? Is an irrational number being defined, if one determines a number sequence by throwing dice? These are the kinds of questions with which the genetic perspective has to be confronted.

Precisely this issue was to be overcome (or to be sidestepped) by the axiomatic method. In *Über den Zahlbegriff* Hilbert writes:

> Under the conception described above, the doubts that have been raised against the existence of the totality of real numbers (and against the existence of infinite sets generally) lose all justification; for by the set of real numbers we do not have to imagine, say, the totality of all possible laws according to which the elements of a fundamental sequence can proceed, but rather—as just described—a system of things whose mutual relations are given by the finite and closed system of axioms I–IV. (Hilbert 1900a, 1095)

In his Paris lecture he articulated that point and re-emphasized that "the continuum . . . is not the totality of all possible series in decimal fractions, or of all possible laws according to which the elements of a fundamental sequence may proceed." Rather, it is *any* system of things whose mutual relations are governed by the axioms; the completeness axiom, in particular, guarantees the continuity of the system without depending on any method of generating real numbers. The consistency proof is "the proof of the existence of the totality of real numbers." Hilbert expanded the second point by saying,

> In the case before us, where we are concerned with the axioms for real numbers in arithmetic, the proof of the consistency of the axioms is at the same time the proof of the mathematical existence of the totality [*Inbegriff*] of real numbers or of the continuum. Indeed, when the proof for the consistency of the axioms shall be fully accomplished, the doubts, which have been expressed occasionally as to the existence of the totality of real numbers, will become totally groundless. (Hilbert 1900b, 1105)

Could Hilbert think of addressing the consistency problem "by a careful study and suitable modification of the known methods of reasoning in the theory of irrational numbers," if he did not have in mind, ever so vaguely, the construction of a particular (Dedekindian) logical model?

Hilbert had known since 1897, through his correspondence with Cantor, about the difficulties in set theory and their impact on Dedekind's foundational work. Nevertheless, he did not move away from his programmatic position and the associated strategy for proving consistency until 1903 or 1904 at the latest. In the summer term of 1904, Hilbert lectured on *Zahlbegriff und Quadratur des Kreises*, and the notes written by Max Born reveal a significant change: Hilbert examines the paradoxes for the first time and sketches various foundational approaches. These discussions are taken up in his talk at the Heidelberg Congress in August of that year, where he presents a syntactic approach to the consistency problem. The goal is still to guarantee the existence of a suitable system, but the method of proof is inspired by one important aspect of the earlier investigations; he, in contrast to Dedekind, had formulated a quasi-syntactic notion of consistency already in his (1899) and (1900a); namely, no finite number of logical steps leads from the axioms to a contradiction. This notion is *quasi*-syntactic, as no deductive principles are explicitly provided.

Hilbert viewed the geometric axioms not only as characterizing a system of things that presents a "complete and simple image of geometric reality," but viewed them also in a very traditional way: the axioms must allow us to purely logically establish all geometric facts. Dedekind held such a view quite explicitly with respect to his "axioms" for natural numbers, i.e., the characteristic conditions for simply infinite systems; see his (1888, #73). Hilbert described this pivotal deductive role of axioms in the introduction to the Festschrift in a methodologically refined way:

> The present investigation is a new attempt at formulating for geometry a *simple* and *complete* system of mutually independent axioms; it is also an attempt at deriving from them the most important geometric propositions in such a way that the significance of the different groups of axioms and the import of the consequences of the individual axioms is brought to light as clearly as possible. (Hilbert 1899, 436)

The same perspective is expressed in the Paris lecture, where Hilbert states, first of all, that the totality of real numbers is "a system of things whose mutual relations are governed by the axioms set up and for which all propositions, and only those, are true that can be derived from the axioms by a finite number of logical inferences." Then, two fundamental problems have to be confronted for both geometry and arithmetic:

> The necessary task then arises of showing the consistency and the completeness of these axioms; i.e., it must be proved that the application of the given axioms can never lead to contradictions, and, further, that the system of axioms suffices to prove all geometric [and arithmetic] propositions. (Hilbert 1900a, 1092–1093)

It is not clear whether completeness of the axioms requires the proof of *all* true geometric (arithmetic) propositions or just of those that are part of the established corpora.

Independent of this issue is the question, which logical inferences are admitted in proofs? Frege criticized Dedekind on that point in the preface to his *Grundgesetze der Arithmetik*, claiming that the brevity of Dedekind's development of arithmetic in (Dedekind 1888) is only possible "because much of it is not really proved at all." He continues:

> Nowhere is there a statement of the logical or other laws on which he builds, and, even if there were, we could not possibly find out whether really no others were used—for to make that possible the proof must be not merely indicated but completely carried out.

Apart from demanding that the logical principles be made explicit, Frege hints at an additional aspect of such a systematic presentation that applies to Hilbert's *Grundlagen der Geometrie* as well. That aspect will be discussed in section 4, whereas the next section attempts to clarify, with the broader understanding of structural axiomatics we have gained, the main issue in the correspondence between Frege and Hilbert.

3. Interlude

My discussion is concerned exclusively with the six letters that were exchanged between Frege and Hilbert in the period from December 1899 to September 1900; they are all concerned with Hilbert's *Grundlagen der Geometrie* (and are found in Frege 1980). Frege wrote the opening letter to Hilbert on December 27, 1899; in it he seeks clarification on some important methodological questions pertaining to the *Grundlagen*. Frege reports that he had discussed parts of the work with his Jena colleagues Thomae and Gutzmer, and that they were not always clear about Hilbert's "real view" (*eigentliche Meinung*). As a start, Frege asks about Hilbert's use of "Erklärung" and "Definition"; they seem to be used for similar purposes, but by using both Hilbert presumably wants to indicate a difference—which is not clear to them. What makes matters even more difficult to understand, Frege points out, is the fact that *axioms* are taken to *define* relations under the heading *Erklärung*. Thus, it appears to Frege, Hilbert does not respect the sharp boundaries between axioms and definitions. Definitions are, after all, *Festsetzungen* ("determinations," "stipulations," or "agreements"), whereas axioms are true statements that are not be proved, as our knowledge of them arises from a source that is different from logic. That leads Frege to the observation that the truth of axioms guarantees that they do not contradict each other, and that no separate proof of consistency is required. Although that is of course a perspective different from Hilbert's, there seems to be some common ground when Frege remarks, in the context of independence proofs for the axioms, "You had to take a higher standpoint, from which Euclidian geometry appears as a special case of a more general [case]." (Frege 1980, 11)

In his response of December 29, 1899, Hilbert points out that, for example, the *Erklärung* for the concept "between" is indeed a proper definition, as its characteristic conditions (*Merkmale*) are given by the group of axioms II 1–II 5 that involve "between." If one wants to take "definition" in the exact traditional sense, he writes, then one would have to say:

> "Between" is a relation for the points of a line that satisfies the following characteristic conditions: II 1 . . . II 5. (Frege 1980, 11)

Later on, he emphasizes that he has absolutely no objection, if Frege wanted to simply call his axioms characteristic conditions (cf. footnote 3). Having discussed the striking and much-emphasized difference of their views concerning consistency and truth, Hilbert comes back to what he very strongly views as *the main issue* (*Hauptsache*) and asserts:

> The renaming of "axioms" as "characteristic conditions" is a pure formality and, in addition, a matter of taste—in any event, it is easily accomplished. (Frege 1980, 12).

This assertion holds sensibly for relations like "between," but not—as Hilbert then also claims—for the basic objects, e.g., points. The latter claim is in conflict with Hilbert's own view he describes next (Frege 1980, 13), namely that "any theory is only a framework [*Fachwerk*] or a schema of concepts together with the necessary relations between them." The basic elements (*Grundelemente*), Hilbert says, "can be thought in arbitrary ways."

Neither Hilbert nor Frege remembered that Dedekind presented in his (1888) under the heading *Erklärung* the definition of a simply infinite system: a system N is *simply infinite* if and only if there is an element 1 and a mapping Φ, such that the characteristic conditions (α)–(γ) hold for them.[5] This structural definition can be seen as providing a second-level concept in the sense in which Frege discusses it in his next letter of January 6, 1900 (Frege 1980, 17); Hilbert could have easily reformulated his *Erklärung* as a Dedekindian one: a triple of systems P, L, and E is a *Euclidian space* if and only if there are relations . . . , such that the characteristic conditions I–V (i.e., the geometric axioms in groups I through V) hold for them. Given such a common perspective, there would have been no reason for the fundamental disagreement Frege saw; indeed, there would have been a precise logical articulation of the abstract character of the emerging modern mathematics.[6]

4. Formal Axiomatics

Hilbert insisted that theorems in geometry or arithmetic must be established by a finite sequence of logical steps from the axioms; for the arithmetic of natural numbers Dedekind made exactly the same demand, considering as starting points of proofs the characteristic conditions for simply infinite systems. Since "axiom" can be taken for Hilbert as synonymous with "characteristic condition," Dedekind and Hilbert share this perspective on proof. Frege, starting with his

[5] These characteristic conditions "correspond" to the so-called Peano axioms and express the following: (α) – ϕ is a mapping from N to N; (β) – N is the chain of the system {1}; (γ) – 1 is not in the ϕ image of N; (δ) – ϕ is a similar (injective) mapping.

[6] There are important connections to 19th-century theories of concept formation, in particular to those formulated by H. Lotze in his *Logik* of 1843 as well as in the expanded editions of 1874 and 1880. There are good reasons to think that Dedekind was influenced by them already very early on in

1879 *Begriffsschrift*, precisely described the logical steps that can be taken in order to obtain "gapless" proofs and asserted later that in his logical system "inference is conducted like a calculation," but observed:

> I do not mean this in a narrow sense, as if it were subject to an algorithm the same as . . . ordinary addition or multiplication, but only in the sense that there is an algorithm at all, i.e., a totality of rules which governs the transition from one sentence or from two sentences to a new one in such a way that nothing happens except in conformity with these rules. (Frege 1984, 237)

In his 1902 review of Hilbert's *Grundlagen der Geometrie*, Poincaré radicalized the formal character of the axiomatic conditions and the algorithmic nature of logical rules, in a different context and for a different purpose; he writes:

> M. Hilbert has tried, so to speak, putting the axioms in such a form that they could be applied by someone who doesn't understand their meaning, because he has not ever seen either a point, or a line, or a plane. It must be possible, according to him [Hilbert], to reduce reasoning to purely mechanical rules.

Poincaré brings out this essential formal, mechanical aspect in a dramatic way and reinterprets the idea of strict formalization as machine executability.[7] Indeed, he suggests giving the axioms to a reasoning machine, like Jevons's logical piano, and observing whether all of geometry could be obtained. Such a mechanical formalization might seem "artificial and childish," Poincaré remarks, if it were not for the important question of completeness:

> Is the list of axioms complete, or have some of them escaped us, namely those we use unconsciously? . . . One has to find out whether geometry is a logical consequence of the explicitly stated axioms or, in other words, whether the axioms, when given to the reasoning machine, will make it possible to obtain the sequence of all theorems as output [of the machine].

his career; Dedekind's stay in Göttingen as a student and then Privatdozent (from 1850 to 1858) fell completely into the period Lotze was professor of philosophy there (from 1844 to 1880). The parallelism of Dedekind's general reflections on concepts in his (1854) and the expanding remarks on their significance in the preface to (Dedekind 1888) is rather striking, as are their view that arithmetic is a part of logic. However, a very distinctive notion of "abstraction" is centrally used by Lotze already in the 1843 *Logik* and allows a cohesive understanding of Dedekind's way of introducing "abstract" concepts. That has been worked out in a paper I wrote with Rebecca Morris. The paper was accepted for publication in 2015 and published as (Sieg and Morris 2018). (2018)

[7] How these considerations are woven into a broader philosophical and mathematical web is discussed in my paper *On Computability* (2009a), in particular on pp. 535–561.

The completeness problem is not formulated as a "mechanical" one in Hilbert's Festschrift, but the issue of what logical steps can be used in proofs is coming to the fore in Hilbert's lectures through references to logical calculi.

The syntactic approach to consistency proofs Hilbert suggested in his 1904 Heidelberg talk uses formal axioms and a logical calculus that is extremely restricted—it is purely equational! In the summer term 1905, Hilbert gave lectures under the title *Logische Prinzipien des mathematischen Denkens*; they are as special as those from 1904, but for a different reason: one finds in them a critical examination of logical principles and a realization that a broader logical calculus is needed that captures, in particular, universal statements and inferences.[8] In his subsequent lectures on the foundations of mathematics, Hilbert does not really progress beyond the reflections presented in his (*1905) until 1917: in the Zürich talk *Axiomatisches Denken* a new perspective emerges. In that essay, Hilbert remarks that the consistency of the axioms for the real numbers can be reduced, by employing set theoretic concepts, to that of integers. Hilbert continues:

> In only two cases is this method of reduction to another more special domain of knowledge clearly not available, namely, when it is a matter of the axioms for the *integers* themselves, and when it is a matter of the foundation of *set theory*; for here there is no other discipline besides logic to which it were possible to appeal.
>
> But since the examination of consistency is a task that cannot be avoided, it appears necessary to axiomatize logic itself and to prove that number theory and set theory are only parts of logic. (Hilbert 1918, 1113)

Hilbert remarks that Russell and Frege provided the basis for this approach.

The detailed study of *Principia Mathematica* began, however, already in 1913 and resulted in the remarkable 1917–18 lectures *Prinzipien der Mathematik*, the very first lectures on modern mathematical logic. All the tools for formally developing mathematics (number theory, but also analysis) were made available in these lectures and are in the background of the work of the Hilbert group during the 1920s. The material was published only 10 years later in (Hilbert and Ackermann 1928). As to the formalization issue, one finds this remark at the

[8] How important those lectures were can be seen from a letter Hilbert sent to his friend Hurwitz in late 1904 or early 1905, definitely after the Heidelberg talk: "It seems that various parties started again to investigate the foundations of arithmetic. It has been my view for a long time that exactly the most important and most interesting questions have not been settled by Dedekind and Cantor (and a fortiori not by Weierstrass and Kronecker). In order to be forced into the position to reflect on these matters systematically, I announced a seminar on the 'logical foundations of mathematical thought' for next semester." In Dugac (1976, 271).

very end of the lecture notes, after the beginnings of analysis had been developed and, in particular, the least upper bound principle had been established: "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" (Hilbert *1917–18, 246).

The 1917–18 lectures gave a full and rigorous mathematical presentation of first- and higher-order logic, including a careful distinction between syntax and semantics.[9] There was, however, no immediate return to a syntactic approach to the consistency problem. Poincaré's incisive analysis of the "proof theoretic" approach in Hilbert (1905), but also Hilbert's own insight into its shortcomings, shifted his attention from the stand he had advocated in Heidelberg. Hilbert came back to it only in the summer semester of 1920. The notes from that term contain a consistency proof for the same fragment of arithmetic that had been investigated in 1904. Its formulation is informed by the investigations of the 1917–18 term: the language is more properly described; the combinatorial argument is sharper (albeit a bit different from that given in 1904), and it is further simplified in (Hilbert 1922). The details are important for (the development of) proof theory, but I emphasize here only the overarching strategic point of the modified argument; namely, Hilbert insists that Poincaré has been refuted.

> Poincaré's objection, claiming that the principle of complete induction can only be proved by complete induction, has been refuted by my theory. (Hilbert 1922, 167)

In the second part of this paper, the formal theory is expanded beyond the purely equational calculus. This expansion has one peculiarity, namely, that negation is applied only to identities. Hilbert gives as the reason for this severe restriction that the formal system is to be kept constructive. Thus, we can conclude that in (Hilbert 1922) the proper metamathematical direction of Hilbert's finitist program had not yet been taken.

The paper was based on talks Hilbert had given in the spring and summer of 1921 in Copenhagen and Hamburg. The first of three Copenhagen talks was devoted to the role of mathematics in physics and has been preserved as a manuscript in Hilbert's own hand. It is worth quoting its last paragraph in order to re-emphasize Hilbert's broad vision for mathematics.

[9] In Hilbert's lecture, a proof of the semantic completeness of the logical calculus for sentential logic is indicated; it is formulated and proved in Bernays' Göttingen *Habilitationsschrift* (Bernays 1918).

> We went rapidly through those chapters of theoretical physics that are currently most important. If we ask, what kind of mathematics do physicists consider, then we see that it is *analysis* that serves physicists in its complete *content* and *extension*. Indeed, it does so in two different ways: first it serves to *clarify* and *formulate* their ideas, and second—as an instrument of calculation—it serves to obtain quickly and reliably numerical results, which help to check the correctness of their ideas. Apart from this face seen by physicists, there is a completely different face that is directed toward philosophy; the features of that face deserve no less our interest. That topic will be discussed in my subsequent talks. (Hilbert *1921, 28–29)

In his "subsequent talks" Hilbert expounded his philosophical perspective, but argued also against the constructive stand of Brouwer and Weyl. In the 1922 essay he contrasts their constructivism with his own, claiming that Weyl has "failed to see the path to the fulfillment of these [constructive] tendencies" and that "only the path taken here in pursuit of axiomatics will do full justice to the constructive tendencies":

> The goal of securing a firm foundation for mathematics is also my goal. I should like to regain for mathematics the old reputation for incontestable truth, which it appears to have lost as the result of the paradoxes of set theory; but I believe that this can be done while fully preserving its accomplishments. The method I follow in pursuit of this goal is none other than the axiomatic method; its essence is as follows. (Hilbert 1922, 1119)

Having described the essential nature of the axiomatic method, he points to the task of recognizing the consistency of the arithmetical axioms including, at this point, axioms for number theory, analysis, and set theory. This task leads now to the investigation of formalisms, in which parts of mathematics can be carried out. The concepts of proof and provability are thus "relativized" to the underlying formal axiom system, but Hilbert emphasizes:

> This relativism is natural and necessary; it causes no harm, since the axiom system is constantly being extended, and the formal structure, in keeping with our constructive tendency, is becoming more and more complete. (Hilbert 1922, 1127)

Hilbert's version of constructivism comes in not only through the construction of ever more complete formalisms for the development of mathematics, but most importantly through their effective character; after all, it is the effectiveness of the basic concepts, in particular of the concept of (formal) proof, that makes it possible to investigate the formalisms from a restricted mathematical, "finitist" point of view.

The term *finite Mathematik* (finitist mathematics) appears for the first time in the 1921–22 notes.[10] Hilbert and Bernays give no philosophical explication; they rather develop finitist number theory, which they do not view as encompassing all of finitist mathematics. On the contrary, they envision a dramatic expansion in order to recognize why and to what extent "the application of transfinite inferences [in analysis and set theory] always leads to correct results." We have to expand, so they demand, the domain of objects that are being considered:

> I.e., we have to apply our intuitive considerations also to figures that are not number signs. Thus we have good reasons to distance ourselves from the earlier dominant principle according to which each theorem of pure mathematics is ultimately a statement concerning integers. This principle was viewed as expressing a fundamental methodological insight, but it has to be given up as a prejudice.
>
> We have to adhere firmly to one demand, namely, that the figures we take as objects must be completely surveyable and that 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. (Hilbert *1921–22, Part III, 4a–5a)

Hilbert and Bernays had thus arrived at a new standpoint that was to serve as the basis for consistency proofs, and formulated the goal of establishing the correctness of formally provable finitist statements.[11] The new approach involves induction and recursion principles for the broader class of "figures," that is, for effectively generated syntactic objects, like terms or formulas or

[10] What is the status of "finit" in "finite Mathematik" in historical regard? Was it introduced from a special philosophical perspective that emerged in the early 1920s? The way in which the concept is actually introduced in (*1921–22), very matter-of-factly, almost leads one to suspect that Hilbert and Bernays employ a familiar one. That suspicion is hardened by *aspects of the past* and an *attitude* that is pervasive until 1932: as to the attitude, finitism and intuitionism were considered as coextensional until Gödel and Gentzen proved in 1932 the consistency of classical arithmetic relative to its intuitionist version; as to aspects of the past, Hilbert himself remarked that Kronecker's conception of mathematics "essentially coincides with our finitist mode of thought."

The concrete background of the term "finitism" should be a topic of thorough historical analysis and definitely include Bernstein's paper (1918). I just state as a fact that in the lecture notes from the 1920s no detailed discussion of "finite Mathematik" is found. The most penetrating analysis is given in (Bernays 1930), still emphasizing the coextensionality of finitism and intuitionism. Indeed, Bernays interprets Brouwer's mathematical work as showing that considerable parts of analysis and set theory can be "given a finitist foundation." For a contemporary and informed discussion, see (Tait 1981) and (Tait 2002).

[11] The claim that consistency implies (mathematical) existence is no longer maintained; see in particular Bernays's later reflections in a note from between 1925 and 1928 that was published in Sieg (2002).

proofs. That is clearly articulated in the second half of the 1921–22 lectures and carried out with strikingly novel, genuinely proof-theoretic techniques. Hilbert and Bernays proved in these lectures the consistency of a quantifier-free fragment of formal elementary number theory, roughly what is now called primitive recursive arithmetic (PRA); the argument is sketched and the methodological approach is described in (Hilbert 1923)—a talk Hilbert gave in September 1922.

In the notes for other lectures from the early 1920s, one finds innovative meta-mathematical work, in particular, the introduction of the epsilon calculus and the associated substitution method, which tries to overcome in leaps and bounds the obstinate difficulties of giving finitist consistency proofs for strong formal theories, but in the end that work is unsuccessful. The reason for this failure was revealed already in 1931 for the theories that were of central interest, analysis and set theory: Gödel's second incompleteness theorem states for them that their consistency cannot be proved by means that are formalizable in those very theories. For the general formulation of the incompleteness theorems (as pertaining to *all* formal theories containing a modicum of number or set theory) Gödel needed an adequate notion of computability characterizing the "formality" of formal theories. In the 1964 postscriptum to his 1934 Princeton lectures, he argued that Turing's work provides such a notion of *mechanical procedure*, and that it is actually "required by the concept of formal system, whose essence it is that reasoning is completely replaced by mechanical operations on formulas" (Gödel 1964, 370). The second incompleteness theorem is usually taken in the way I formulated it earlier: finitist consistency proofs cannot be obtained for theories that are sufficiently strong; in other words, Hilbert's *finitist* program has been refuted for theories like analysis or set theory. The first incompleteness theorem is frequently taken to refute Hilbert's view that there is no *ignorabimus* in mathematics. However, that is not Gödel's view at all. In the 1964 postscriptum he explicitly states that the incompleteness theorems "do not establish any bounds for the power of human reason, but rather for the potentialities of pure formalism in mathematics" (370). For him, Hilbert's no-ignorabimus view is not connected to "pure formalism," as I'll point out in the next section.

5. One Direction

Gödel begins his (193?) by recalling Hilbert's famous words, "For any precisely formulated mathematical question a unique answer can be found." He takes these words to mean that for any mathematical proposition A there is a proof of either A or not-A, "where by 'proof' is meant something which starts from evident axioms and proceeds by evident inferences." He argues that the incompleteness theorems show that something is lost when one takes the step from this notion of proof to a formalized one: "It is not possible to formalize mathematical evidence even in the domain of number theory, but the conviction about which Hilbert speaks remains entirely untouched. Another way of putting the result is this: it is not possible to mechanize mathematical reasoning."

It is important to recognize early and deep roots of Hilbert's foundational thinking. His work in geometry and arithmetic around 1900 gave or indicated systematic developments, within the framework of structural axiomatics. A more formal presentation was sought already in Hilbert (*1905), but was viewed as extremely difficult. It is equally important to see that the study of *Principia Mathematica* raised the prospect of formalizing mathematics on the broad basis of type or set theory. In order to more closely reflect mathematical practice, Hilbert and Bernays even developed in (*1921–22) a new kind of logical calculus with axioms for all the logical connectives; these axioms were later basic for the introduction and elimination rules of Gentzen's natural deduction calculi.[12] But the more urgent proof theoretic issues surrounding the consistency problem shifted attention away from the formal representation of mathematical practice. With the advance of computer technology and the myriad problems that can be addressed mathematically, it is important, however, to construct formal frameworks in which mathematics can be formally developed not only "in principle," but actually and intelligibly.

To achieve that goal, it has been argued for a long time, computers have to take over routine parts of argumentation, so that human users can focus on the broader conceptual and strategic aspects of proof construction. In spite of much exciting contemporary work in (interactive) theorem proving, there is still no somewhat general theory of mathematical proof (as Hilbert had called for in 1917). I have taken the lack of a general theory as one central reason to formulate and implement strategies for *automated* proof search; the work I have been doing in this direction is described in (Sieg 2010). This is a first step not toward a general theory, but rather toward the more modest goal of finding intelligible proofs that reflect (and are inspired by) logical and mathematical understanding. Even this step already forces us, on the one hand, to make explicit the conceptual ingenuity underlying successful human proof construction; it asks us, on the other hand, to integrate it with proof-theoretic features of derivations (subformula properties of normal forms) for the sake of efficiency.

Coming back to the beginning of this essay, we clearly have to analyze concepts and articulate characteristic conditions for them, but we must also

[12] This connection is sketched in (Sieg 2010, 197–198).

consider mathematical arguments as they present themselves "in experience," so to speak; that is how Dedekind in (Dedekind 1890) described his attitude toward the notion of natural numbers. That requires enriching suitable formal frames by *leading ideas* for particular parts of mathematics, thus, an effective conceptual organization that can be expressed through appropriate heuristics.[13] Saunders Mac Lane, one of the last logic students in Hilbert's Göttingen and a friend of Gentzen, wrote his thesis (Mac Lane 1934) with this general goal. He published an English summary (1935) that emphasizes the crucial programmatic features. In particular, it is pointed out that proofs are not "mere collections of atomic processes, but are rather complex combinations with a highly rational structure." When reflecting in 1979 on his early work, Mac Lane ended with the remark: "There remains the real question of the actual structure of mathematical proofs and their strategy. It is a topic long given up by mathematical logicians, but one which still—properly handled—might give us some real insight" (Mac Lane 1979, 66). It seems to me that we have the computational and logical tools to successfully tackle Mac Lane's "real question."

Appendix

The text that follows is a (small) part of the lectures Hilbert gave, with the assistance of Bernays, in the winter semester of 1917–18. As an example of the systematic presentation and penetrating analysis the axiomatic method affords, Hilbert discussed at first the axiom system for Euclidean geometry and then gave proofs of consistency and independence. It is the beginning of that section of the lectures that is presented here. The noteworthy fact is the emphasis on the *assumption* of a system of objects, etc., the core feature of structural axiomatics. There is no hint of a finitist proof-theoretic approach to the consistency problem, neither here nor later in these lectures when the system of arithmetic (for real numbers) is being discussed; at the very end, one rather finds the suggestion that the theory of types (with the axiom of reducibility) provides the appropriate means for developing the foundations of higher mathematics. This is an echo of the logicist leanings Hilbert had expressed in his Zürich lecture *Axiomatisches Denken* (Hilbert 1918).

[13] Three particular examples are discussed in my (2010): Gödel's incompleteness theorems, the Cantor-Bernstein theorem, and the Pythagorean theorem.

German Text (Hilbert 1917–18, 19–20)

Zu dem geometrischen Axiomensystem, dessen Aufstellung ich das letzte Mal beendet hatte, sei zunächst bemerkt, dass die Anordnung der Axiome im Einzelnen zwar eine gewisse Willkür aufweist, im grossen aber doch mit Notwendigkeit bestimmt ist. Bei Untersuchungen über mögliche Vereinfachungen dieses Axiomensystems hat man darauf zu achten, dass Kürzungen durch eine Reduktion der Annahmen nicht immer von Vorteil sind, insofern dadurch die Uebersicht leiden kann.

Wenden wir uns nun zur genaueren Diskussion des vorgelegten Systems der geometrischen Axiome, so ist zuerst die Frage der *Widerspruchslosigkeit* zu behandeln. Diese Frage ist darum die wichtigste, weil durch einen Widerspruch, zu dem die Konsequenzen aus den Axiomen führen würden, dem ganzen System seine Bedeutung genommen wäre. Das Axiomensystem ist ja so aufzufassen, dass über dem Ganzen die Annahme steht, es gebe drei Arten von Dingen, die wir als Punkte, Geraden und Ebenen bezeichnen und zwischen denen gewisse Beziehungen bestehen, welche durch die Sätze, die wir Axiome nennen, beschrieben werden. Diese Annahme wäre offenbar gegenstandslos, wenn man von den Axiomen durch richtige Schlussfolgerungen zu einem Satz und auch zu seinem Gegenteil gelangen könnte. Die Unmöglichkeit eines solchen Falles nennen wir die Widerspruchslosigkeit des Axiomensystems.

Den Beweis der Widerspruchslosigkeit für die Axiome der Geometrie werde ich führen durch Aufweisung eines Systems von Gegenständen, die miteinander in solcher Weise verknüpft sind, dass sich eine Zuordnung dieser Gegenstände und Verknüpfungen zu den in den geometrischen Axiomen vorkommenden Gegenständen und Beziehungen herstellen lässt, bei welcher sämtliche Axiome erfüllt sind. Die Gegenstände, auf die ich mich hierbei als auf etwas Gegebenes berufe, sind der Arithmetik entnommen, und das Beweisverfahren kommt also darauf hinaus, dass die Widerspruchslosigkeit der Geometrie auf die Widerspruchslosigkeit der Arithmetik zurückgeführt wird, indem gezeigt wird, dass ein Widerspruch, der sich bei den Folgerungen aus den geometrischen Axiomen ergäbe, auch innerhalb der Arithmetik einen Widerspruch zur Folge haben müsste.

Translation

As to the geometric axiom system whose exposition I completed last time, I would like to remark, first of all, that the particular ordering of the axioms shows in the small a certain arbitrariness, but in the large it is determined with necessity. When investigating possible simplifications of this axiom system one has to observe carefully that shortenings by reducing the [number of] assumptions is not always advantageous, as such a reduction may diminish the overall perspicuity.

When turning attention now to the more precise discussion of this system of geometric axioms, the question of consistency has to be addressed first. This question is the most important one, because the whole system would lose its significance if a contradiction could be inferred from the axioms. After all, the axiom system is to be understood as being completely covered by the assumption that there are three kinds of things we refer to as points, lines, and planes, and that certain relations obtain between them that are described by the statements we call axioms. This assumption obviously would be groundless if it were possible to obtain a statement and its negation from the axioms by correct inferences. The impossibility of such a case is called the consistency of the axiom system.

I will carry out the consistency proof for the axioms of geometry by exhibiting a system of objects that are connected to each other in a particular way; these objects and connections can be associated with the objects and relations that occur in the geometric axioms in such a way that all the axioms are satisfied. The objects to which I appeal as something given are taken from arithmetic, and the method of proof amounts to reducing the consistency of geometry to the consistency of arithmetic. We do this by showing that a contradiction that could be inferred from the geometric axioms must lead to a contradiction within arithmetic.

Acknowledgments

This chapter directly builds on my *Hilbert's Proof Theory* (2009b), but focuses on the dramatically different perspectives on the axiomatic method during the 1890s, culminating in the Paris address of 1900, and the early 1920s, when the finitist consistency program developed in a methodologically coherent way; that program was presented for the first time in Hilbert's talk in Leipzig in the fall of 1922. The lectures of Hilbert and Bernays from around 1920 were described in (Sieg 1999) and, finally, have been published in (Ewald and Sieg 2013). The chapter was originally published in *Perspectives on Science* 22 (2014), 133–157. It is being republished here with the permission of MIT Press; I made only minor modifications.

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Translations in this chapter are my own, except when I am quoting directly an English source (sometimes with modifications). Unpublished lecture notes of Hilbert's are located in Göttingen in two different places, namely, the Staats- und

Universitätsbibliothek and the Mathematisches Institut. The reference year of these notes below is preceded by a "*"; their location is indicated by SUB xyz and MI, respectively. Many of them have been published or are being prepared for publication in *David Hilbert's Lectures on the Foundations of Mathematics and Physics, 1891–1933* (Heidelberg: Springer).

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