THE PURSUIT OF RIGOR:
David Hilbert's Early Philosophy of Mathematics

by

YOSHINORI OGAWA

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Abstract

The present study attempts to provide a new approach to Hilbert's philosophy of mathematics by going back to the origin of his foundational investigation and clearly describing the *Problematik* within which it was framed and developed. In so doing, its main objective is to identify and highlight the general intellectual tendencies invariably and continuously motivating Hilbert's research program throughout his long career.

The study consists of two parts. In the first half, special emphasis is laid upon Hilbert's axiomatic method and his accompanying view of axioms and definitions. It is argued there that Hilbert's goal with his axiomatization program is to demonstrate the objectivity of mathematical judgment and inference and to systematize and thereby to increase our understanding of mathematics. The present study attempts to support this claim by embedding Hilbert's project in the context of the late nineteenth century movement of the "rigorization" of mathematics and by understanding it as a development of the methodological standpoint represented by Dedekind.

On the interpretation presented here, then, Hilbert's foundational investigation was not, as is often claimed, motivated by the philosophical concerns for the absolute certainty and a prioricity of our mathematical knowledge and, indeed, it combated against the intrusion of such concerns by relegating framework-independent elements through the "new"

methodological turn in the conception of axiomatics. In the second half of the study, this non-standard reading is extended to Hilbert's consistency program, and his first attempt of a direct consistency proof and Poincaré's criticism of it are considered in this light. Hilbert's answer to Poincaré came with the remarkable idea of proof-theory and the formulation of finitary mathematics as the framework for proof-theoretic considerations. But, seen from a philosophical viewpoint, this methodological move meant the re-introduction of the notions of truth and existence taken in the absolute sense and, as a result, a motivation for adopting (mathematical) instrumentalism as the philosophy of Hilbert's program arose. But even after this "epistemological" turn, the earlier view continued to be operative in Hilbert's thought, and this, I shall argue, explains the "tension" found in the philosophy behind Hilbert's program.

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Dedication

This study is dedicated to the memory of my wife, Motoko Ogawa.

Introduction

One of the greatest mathematicians of the twentieth century, David Hilbert, is regularly considered to be the founder of the formalist school, which, together with Bertrand Russell *et al*'s logicist school and L.E.J. Brouwer *et al*'s intuitionist school, forms the "three principal present-day philosophies of mathematics" [Eves 1990, p.266]. Correspondingly, many of us today, when describing Hilbert's project in the foundations of mathematics, tend to characterize it with two key terms: "formalism" and "consistency-proof." By these terms, the two central theses of the Hilbert school are supposed to be captured: the one that mathematics is nothing more than a combination of meaningless symbols or "a game of formulae, ruled by certain conventions, which is very well comparable to the game of chess" [Weyl 1925, p.136]; and the other that once a proof is given (in metamathematics) that no contradiction arises in the formal system of mathematics, there are no more questions about its legitimacy.

But Hilbert's project so considered seems hardly capable of offering a satisfactory philosophy of mathematics. Would it make any sense at all to talk of the freedom from contradiction if mathematics consisted entirely of strings of *meaningless* symbols and formulae? Or, perhaps more importantly, granting that there exists some way in which the consistency of mathematics can be considered through an investigation into such a game of formulae, how could a consistency-proof be sufficient for establishing the *legitimacy* of mathematics? To be sure, consistency, i.e., the possibility of being true, is a necessary but, in itself, not a sufficient condition for truth or certainty. It is quite obvious then that some important pieces are missing in such a portrayal of Hilbert's theory. But what are they? And how do they complete the picture?

At this point, we turn our attention to the interpretation of Hilbert's philosophy of mathematics that has been accepted by the vast majority of philosophers of mathematics as that which is most able to provide an answer to these questions. The standard account agrees with the "naive" view that Hilbert's main objective with his program was, from the outset, to resolve epistemological worries about our mathematical knowledge and establish its truth and certainty.[1] Hilbert's following remark is often quoted as textual evidence:

> The goal of my theory is to establish once and for all the certitude of mathematical methods. [Hilbert 1926, 184]

More specifically, on the standard account, Hilbert decided to undertake such an enterprise in order to protect the certainty of our mathematical knowledge from the double-threat of the set-theoretic paradoxes and the revisionist advocacy of Brouwer and Weyl.

So how, on this account, does he try to achieve this goal? The basic story runs as follows. Looking over the whole of mathematics, Hilbert realizes that, in mathematics, there are two kinds of sentences. On the one hand, there are those which are clearly meaningful and of whose truth and falsity we seem

[1] The very first sentence of Philip Kitcher's influential paper "Hilbert's Epistemology" reads:

> Hilbert's approach to the foundations of mathematics is designed to defend the thesis that we can have certain mathematical knowledge (and that we do have such knowledge of parts of mathematics). [Kitcher 1974, 99]

to have certain knowledge because of an intuitive access we have to their objects or to the states of affairs they represent. On the other hand, there are those which appear meaningful but of whose truth and falsity we seem to be denied such intuitive access and thus about which we do not have certain knowledge. One way to restore the absolute certainty of mathematics would then be to "reduce" all of the second, problematic kind of sentences to the first, intuitable kind through "translations." Or if such a reduction or translation cannot be carried out for all, one could attain the desired goal by banishing from mathematics altogether those which refuse reduction. Very crudely, this is the path that the intuitionists encourage us to take. But, unlike the intuitionists, Hilbert was never of the opinion that we should do away with the part of mathematics that is not "reducible" to or "constructible" from the first, non-problematic kind. For him, it simply was too great a sacrifice to pay.

Here an idea occurred to him, namely that the problematic, infinitary part of mathematics is nothing other than a particular instance of, what he termed in another context, "ideal elements." Ideal elements are introduced into a system merely for the purpose of simplifying or generalizing our thinking about a certain subject-matter, and thus they need not be considered to be tied to any intuitional basis. In projective geometry, points and lines at infinity are postulated so as to preserve the general validity of the principle of duality, and, in arithmetic, negative numbers are introduced so that the operation of subtraction may be performed universally. Analogously, it might be thought that sentences of infinitary mathematics are introduced as a useful means of deriving meaningful sentences of finitary mathematics. Just as with other types of ideal elements, sentences of infinitary mathematics are nothing more than purely formal devices and possess no "real" content of their own. The upshot of this is that there is no need to be concerned about the epistemological status of such meaningless formalism: they have no representational content and are neither true nor false.

There is one condition to be met, however. This is that, since, in the actual practice of mathematics, infinitary mathematics is freely employed in the production of finitary results, it must be made *certain* that the use of the *former* never leads to incorrect finitary results; and this amounts to the demanding for a proof that the use of the meaningless sentences of infinitary mathematics is *consistent* with meaningful sentences of finitary mathematics. In other words, the point of a consistency proof is to establish the "instrumental usefulness" of infinitary mathematics with regard to finitary mathematics. That Hilbert seems to have understood a proof of consistency precisely in this form can be seen from passages such as the following:

> To be sure, one condition, a single but indispensable one, is always attached to the use of the method of ideal elements, and that is the proof of consistency; for extension by the addition of ideal elements is legitimate only if no contradiction is thereby brought about in the old, narrower domain, that is, if the relations that result from the old objects whenever the ideal objects are eliminated are valid in the old domain. [Hilbert 1928, 471]

Moreover, since we must be certain of the truth of the consistency proof, such a *meta*mathematical consideration must be carried out within the boundary of what is knowable, i.e., within finitary mathematics.[2] In a nutshell, this is Hilbert's program.

Thus, in explaining why Hilbert's consistency program is, at the same time, an epistemological project, the standard account ascribes to his theory of mathematics a view that is comparable to what is usually called "instrumentalism" in the philosophy of science. In other words, within the totality of the sentences constituting the discipline of mathematics, only some count as genuine knowledge. The rest are merely formal devices that are introduced into the system of "real," "contentual" mathematics so as to facilitate the production of genuine knowledge.[3] It should be recognized here that, considered thus, what essentially distinguishes Hilbert's position from intuitionism seems to be neither its objective, nor its conception of what is genuinely knowable within the bounds of the intuitive,[4] but its instrumentalist treatment of

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[2] In his recent article on German philosophy of mathematics, Donald Gillies explain Hilbert's "formalist" philosophy in this way:

> To provide each branch with a foundation, all that was needed was to give a consistency proof for the corresponding formal system. For this purpose use had to be made of something which was not a formal system, but which had an indubitable character. This was intuitive, finitary arithmetic. [Gillies 1999, 189]

[3] Hence, according to the standard account, Hilbert's standpoint should not be confused with that of "strict" formalism, the view according to which mathematics is nothing but a combination of meaningless symbols.

[4] Strictly speaking, this is not correct, for, as Gentzen and Gödel independently showed, it turns out that Hilbert's finitist standpoint is more restrictive than Brouwer's intuitionism. This, however, does not affect my point here insofar as, on the standard account, Hilbert does rely on the notion of intuition for his finitism.

what goes beyond the knowable in mathematics.[5] It is on this ground that Hilbert is supposed to be able to characterize his own standpoint as the one that does not, contrary to Brouwer and Weyl, throw away infinitary mathematics altogether. Instead it rescues and preserves it.

A close look at the content of this "rescue," however, might lead one to wonder whether the standpoint attributed to Hilbert by the standard account is not at odds with what Hilbert himself claims to achieve with his consistency program. On the one hand, Hilbert seems to maintain repeatedly and invariably that, through the consistency program, it will be established that all, not just some, mathematical statements are "incontestable and ultimate truths."[6] In the beginning of the 1922 essay "The New Grounding of Mathematics: First Report," Hilbert illustrates the goal of his foundational project in this way:

> ... in mathematical matters there should be in principle no doubt; it should not be possible for half-truths or truths of fundamentally different sorts to exist. Thus--to give as an example a difficult and remote item on the agenda--it must be possible to formulate Zermelo's postulate of choice in such a way that, in the same sense of "valid" [gültig], it becomes just as valid and reliable as the arithmetical proposition that 2 + 2 = 4. [Hilbert

[5] William Ewald thus writes:

> Despite Hilbert's fiery polemics against Kronecker, Weyl, and Brouwer, it should be observed that the entire controversy is an internal feud among constructivists. [Ewald 1996, 1116]

[6] In this connection, it might also be recalled that Hilbert made his intent known with the oft-quoted remark:

> No one shall drive us out of the paradise that Cantor has created for us. [Hilbert 1926, 191]

1922, 198][7]

On the other hand, according to the standard, instrumentalist account, Hilbert's consistency program, if successfully carried out, would establish only the truth and certainty of finitary mathematics precisely because Hilbert considers the sentences of infinitary mathematics to be mere formal devices and thus devoid of cognitive content.

Yet, considering the polemical and partisan nature of the articles and addresses where one finds such "strong" claims by Hilbert about the goal of his project, it might seem only natural to conclude that no interpretative problems are posed by the apparent discrepancy between those claims and Hilbert's instrumentalist treatment of infinitary mathematics. As a matter of fact, until quite recently, the scholarship has been virtually unanimous in endorsing the instrumentalist reading and recognizing the existence of a discrepancy; Cantor's paradise is, after all, the paradise of "gadgets" that make life smoother.[8] This unanimity, however, has been broken by Michael Hallett's recent discussions of the issue.[9] Hallett, through the careful examination of Hilbert's mostly unpublished lecture notes, has argued that a strongly anti-instrumentalist tendency can be found in various aspects of Hilbert's thinking and that its existence cannot simply be brushed aside as something extrinsic or accidental. More specifically, Hallett argues that 1) for Hilbert, the use of the method of ideal

[7] Similarly, when Hilbert says that the consistency of the axioms of analysis, if obtained, establishes the "incontestable and ultimate truths" of *mathematical statements*, he does not add any qualification.

[8] Kreisel 1983, 209.

[9] Hallett 1990. See also Hallett 1994, Hallett 1995.

elements, or the introduction of ideal elements, is not, as is claimed by the standard account, arbitrary but, rather, in some sense basic to the human thought process; 2) the ideal extension, in Hilbert's view, is just as much meaningful as the real theory being extended (insofar as certain conditions are satisfied) and, thus, it is not true that it is a mere formal device with no representational content.

Provided that Hallett is right in pointing out the existence and importance of an anti-instrumentalist tendency in Hilbert's thinking, one might be inclined to reject the standard, instrumentalist account of Hilbert's program. But this, of course, would amount to abandoning what might seem to be the only interpretation that could account for the reason why Hilbert thinks the consistency program provides a definitive solution to the epistemological problem of grounding our mathematical knowledge. One might try, then, to find a way out by interpreting Hilbert's consistency program as a non-epistemological project. This, however, would require much explaining, to say the least, in the face of the ample textual evidence which seems to indicate otherwise.

Hence, if one would prefer the exegetical "cleanliness" of the instrumentalist account, there would seem to be no choice left but to conclude the existence of a discrepancy within Hilbert's text.[10] One recent commentator on Hilbert's program thus acknowledged the "difficultness" of the problem and alluded to the possibility that "Hilbert and Bernays might not have been completely consistent in their positions,"[11] whereas

[10] That is, insofar as one agrees with Hallett on the existence of an anti-instrumentalist tendency in Hilbert.

[11] Mancosu 1998, 160. It is to noted, however, that this is not Mancosu's conclusion. His attitude is rather to leave the issue open for further discussion.

another spoke of Hilbert's "vacillation" and eventually came to the conclusion that "there is a strange tension in the philosophy behind Hilbert's program":[12]

> On the one hand, it values highly the transfinite part of mathematics, but on the other hand, it is ready to discard transfinite interpretations and to take transfinite propositions as just formal instruments, retaining only their finitary interpretations when possible. [Prawitz 1993, 97]

In short, the general atmosphere surrounding the issue of Hilbert's instrumentalism might best be described as the state of stalemate, as it were. Two opposing interpretations have been presented for the solution of the problem and, yet, neither seems to be able to come up with a satisfactory account. Nor does any promising alternative seem forthcoming.

The goal of the present study is to attempt to provide a new approach to Hilbert's philosophy of mathematics. More specifically, in considering the so-called "Hilbert program" of the 1920s, the current study will return to the very origin of Hilbert's foundational investigation and to the *Problematik* within which it was framed and developed. By understanding the true purpose and scope of Hilbert's early research in the foundations of mathematics, and by carefully tracing its development in subsequent periods and his interactions with then-contemporary mathematicians, we will be able to recognize the general intellectual tendencies that invariably and continuously motivated Hilbert's research program throughout his long career. This understanding in turn will help us see clearly the "philosophical" pillars supporting the "technical"

[12] Prawitz 1993, 95, 97.

or "mathematical" suprastructure of Hilbert's consistency program.

In particular, the present study will show that Hilbert's early foundational investigation was directly related to the late nineteenth century movement in mathematics generally referred to as the "rigorization" of mathematics. It will also show that his axiomatic method, and his accompanying views of axioms and definitions, were adopted as a response and a solution to two difficulties. These were the then-contemporary dispute between two different methodological standpoints, represented by his two great predecessors, Dedekind and Kronecker respectively, over the "arithmetization" of analysis, and the recent discovery of set-theoretical paradoxes. Hilbert's axiomatization program constructively took over Dedekind's project, which emphasized the importance of deductive rigor and the freedom of concept-formation[13] in mathematics. This was in strong opposition to Kronecker's program of "strict arithmetization," which was primarily motivated by philosophical concerns for absolute certainty and the a prioricity of arithmetical knowledge. By carrying out the rigorous axiomatization of mathematics, and by constructing a "complete proof-structure," Hilbert's objective was twofold: to systematize, and thereby to increase our understanding of mathematics, and to demonstrate the objectivity of mathematical judgment and inference.

What can be seen from this is that Hilbert's foundational investigation was not driven by epistemological concerns in the

13 Throughout this study, the term "concept-formation" and its cognates are used to translate the German term "*Begriffsbildung.*" Accordingly, by "concept-formation," I do not mean the configuration of concepts existing in the timeless Platonic heaven. Rather, it means the *forming* of concepts.

"exalted" sense[14] and, indeed, that it combated against the intrusion of such concerns by relegating framework-independent elements through what the co-architecht of Hilbert's program, Paul Bernays called "the new methodological turn" in the conception of axiomatics. At the same time, however, the rejection of the "epistemic" character of mathematical propositions should not be taken as the elimination or suspension of the epistemological consideration of mathematics. Rather, as I shall argue, what is implicit in Hilbert's elimination of system-independent elements is the rejection of the standard notions of truth, existence, and knowledge which involve reference to such elements, and the relativization of these notions to the axiom system characterizing a field of knowledge.

This, naturally, will raise a question concerning Hilbert's insistence on the need for a consistency proof: given that such notions as truth and existence are relativized to the relevant axiom system, why is a consistency proof necessary? On the interpretation presented here, the demand for a consistency proof, which was already on Hilbert's mind before the emergence of the set-theoretical paradoxes, will be explained in terms of Hilbert's interest in rigor and, accordingly, without reference to any exalted epistemological concerns.

With such a non-standard interpretation of Hilbert's consistency program in mind, the present study examines the content of Hilbert's early attempt to obtain an absolute consistency proof for the axioms of arithmetic. We then

14 The phrase is borrowed from Kitcher 1984. For the ease of description, I will follow Kitcher in claiming that anyone concerned in the philosophy of mathematics to show the certainty and aprioricity of mathematical knowledge is doing epistemology (of mathematics) in "the exalted sense."

examine Poincaré's criticism of it. The main task of this part of the study will be to explain how the first attempt of a direct consistency proof, presented in the 1905 essay, was turned into the sophisticated, proof-theoretic consistency proof of the 1920s. It is argued that it was through Hilbert's effort to refute Poincaré's objection that the former's (implicit) use of induction principle at the level of metamathematics came to signify the ultimate failure of his consistency proof.

Hilbert's final answer to Poincaré came with his remarkable idea of proof-theory and the formulation of finitary mathematics as the theory providing the framework for proof-theoretic considerations. But, from a philosophical viewpoint, this methodological move meant that, despite the rejection of the framework-independent elements (and the subsequent relativization of those philosophical notions) executed in Hilbertian axiomatics, the notions of truth and existence in the absolute sense are re-introduced into mathematics. As a direct result of this "epistemological" turn, the "truth" of infinitary mathematics and the "existence" of the ideal elements occurring in it became problematic (because of their unintuitability), and thus a motivation for adopting (mathematical) instrumentalism as the philosophy of Hilbert's program arose. Nevertheless, even after the epistemological turn, the earlier view continued to be operative in Hilbert's thought and this, I shall argue, explains the aforementioned tension in the philosophy behind Hilbert's program.

Chapter I

Hilbert's Axiomatic Method

§1. In 1899, the year in which his celebrated *Foundations of Geometry* was published, Hilbert wrote a short paper entitled "*Über den Zahlbegriff* [On the Concept of Number]." It was his first essay on the foundations of arithmetic. Hilbert starts the paper by describing and contrasting two different methods of investigation, each of which has traditionally been tied to a particular branch of mathematics. The first is often thought to have a close tie to arithmetic and is employed there in the following manner:

> Starting from the concept of number 1, one usually imagines the further rational positive integers 2, 3, 4 ... as arising through the process of counting, and one develops their laws of calculation; then, by requiring that subtraction be universally applicable, one attains the negative numbers; next one defines fractions, say as a pair of numbers--so that every linear function possesses a zero; and finally one defines the real number as a cut or a fundamental sequence, thereby achieving the result that every rational indefinite (and indeed every continuous indefinite) function possesses a zero. [Hilbert 1900a, 1092]

Hilbert calls this method "genetic" because, in it, the most general concept to be introduced (i.e. that of real number) is "engendered" by the successive extension of the simple, primitive concept (of number). The second method of investigation, by contrast, is regularly used, as Hilbert himself so successfully employed it in his 1899 book, in the field of geometry:

> Here one customarily begins 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 on the pattern of Euclid--brings these elements into relationship with one another by means of certain axioms--namely the axioms of linking [Verknüpfung], of ordering, of congruence, and of continuity. 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 is adequate to prove all geometrical propositions. [Hilbert 1900a, 1092-93, Hilbert's emphasis]

For the obvious reason, Hilbert calls this procedure the "axiomatic" method.

Now, despite the customary practice of applying the genetic method to arithmetic and the axiomatic one to geometry, it is not Hilbert's opinion that any intrinsic ties exist between the two items forming such a couple. As a result, he proposes to examine the concept of number through the axiomatic method instead of the customary genetic method. What is to be noted here, however, is that Hilbert does not propose the switching simply for the reason that it has rarely been done in the past. Rather, as can be seen from the following passage, his proposal is made on the basis of the belief that the axiomatic method is more suitable for the purpose of his project, which is a *logical* investigation into the foundations of the theory of number or arithmetic:

> *Despite the high pedagogic and heuristic value of the genetic method, for the final presentation and the complete logical grounding* [Sicherung] *of our knowledge the axiomatic method deserves the first rank.* [Hilbert 1900a, 1093, emphasis in original]

The question is then why Hilbert thinks that the axiomatic method is generally to be preferred to the genetic one when it comes to the matter of the logical grounding of knowledge.

To answer this question, we need not only examine the nature of the two investigative methods more closely. We also need to clarify what precisely is meant by the "logical grounding" of knowledge. Before tackling these problems, one thing must be recognized. This is the fact that whenever Hilbert refers to the "axiomatic" method in the context of foundational topics, it appears that he has *his* own brand of axiomatic method specifically in mind. Here, without getting into much detail, I would like first simply to point to some essential differences between traditional and Hilbertian axiomatics. In general, the axiomatic method may be characterized by the fact that, when constructing a theory in accordance with this method, one sets down, at the outset, a collection of basic statements and then proceeds, by purely deductive reasoning, to derive from them all the other statements of the theory. Hilbert's axiomatic method shares this characteristic, but it differs from the traditional, pre-Hilbertian axiomatic method in its treatment of axioms and the basic concepts occurring in them. If the constructed theory is a non-logical one (e.g. geometry), both the initial and the derived statements of the theory are statements about its special subject matter (e.g. space) and thus involve special, non-logical terms (e.g. "point," "line"). Accordingly, in the traditional version of the axiomatic method, for the purpose of fixing the meanings of these non-logical terms, a list of explanations and definitions is usually introduced (for the basic terms of the theory) before the initial statements or "axioms" are set up.[1] The axioms are then formulated in such a way that their truth about the objects of the theory is self-evident in view of these explanations and definitions.

By contrast, in Hilbert's axiomatic method, the axioms of a theory do *not* express truths about any special subject matter, and the meanings of the non-logical terms occurring in the axioms are not fixed by previously given definitions as in the old axiomatics, but by (and within) the axioms which only state how they are related to each other.[2] In the quoted passage, where Hilbert describes the procedure of the axiomatic method as opposed to that of the genetic method, these distinctive characteristics of the Hilbertian axiomatic method are clearly visible: the basic concepts (point, line, plane), which are not previously anchored to any extra-theoretical understanding of them, are brought into relation to each other by means of the axioms (those of linking, of ordering, of congruence, and of continuity). Thus, when he talks there of "the axiomatic method," he is thinking of his version of the axiomatic method.

Yet, this fact alone does not imply that Hilbert has his own axiomatics specifically in mind when, in the 1900 essay, he stresses the superiority of the axiomatic method to the genetic one with regard to "the final presentation and the complete logical grounding of our knowledge." Indeed, it appears that,

[1] The meanings of the other non-basic, non-logical terms occurring in the theory (e.g. "segment," "triangle") are all (explicitly) defined by means of the basic terms, whereas the logical terms (e.g. "and," "if ... then," "there exists") are assigned their usual meanings.

[2] In other words, in Hilbert's axiomatics, the non-logical terms of the theory do not refer to objects that are thought to exist independently of the theory. The logical terms, by contrast, receive their usual meanings just as in the old axiomatics. For more on these points and details of the Hilbertian axiomatics, see below.

by construing Hilbert's position in the following manner, one could well argue that what he means there is the preferability of the axiomatic method *in general*, not that of his own in particular. What Hilbert means by the "complete logical grounding of our knowledge" is basically to explore and clarify the logical relations among the statements constituting a domain of our knowledge. The axiomatic method is then more advantageous compared to the genetic method for the reason that it is designed exactly to investigate such relations. By contrast, the latter makes it its job to introduce various concepts comprising a theory by defining them solely in terms of its "basic" concept and is not concerned with the clarification of the logical relations among these concepts. In other words, the chief merit of the axiomatic method, in Hilbert's view, consists in its power to organize a body of knowledge in such a manner that the logical relations holding (and not holding) among the statements constituting it are made perspicuous. It follows, then, that there is no compelling reason why we must think that by "the axiomatic method," he means his own method in particular, at least insofar as the pre-Hilbertian axiomatics seems capable of executing this task just as well as Hilbert's.

This reading, however, is incorrect for two reasons. First, while *systematization* accounts for at least part of what is involved in Hilbert's project of "logical grounding," it does not exhaust the whole content of this project.[3] Second, regardless of the precise nature of the project of logical grounding, it is not Hilbert's view that the two versions of the axiomatic method can be treated indiscriminately in this

[3] I shall come back to this point shortly.

context. As was mentioned above, one particular characteristic distinguishing his axiomatics from the traditional one is its conception of definition, according to which the meanings of the basic terms of a theory, such as "point," "line," and "plane" in geometry, are fixed by the collection of the initial statements or the axiom system *as a whole*. But how did he arrive at this non-standard view of definition in the first place? In his letter to Frege of 22 September 1900, Hilbert relates the circumstance in which he was led to it:

> I did not think up this view because I had nothing better to do, but *I found myself forced into it by the requirements of strictness in logical inference and in the logical construction of a theory*. I have become convinced that the more subtle parts of mathematics and the natural sciences can be treated with certainty *only* in this way; otherwise one is only going around in a circle. [Frege 1980, 51, my emphasis]

This remark by Hilbert is of great significance for our purpose. For, providing that, for Hilbert, the "strictness in logical inference and in the logical construction of a theory" cited here and the "complete logical grounding of our knowledge" mentioned in the 1900 essay refer basically to the same thing (or if the latter is thought to involve the former),[4] this clearly shows that, in his eyes, some sort of "necessary" connection exists between the "complete logical grounding" and the Hilbertian axiomatics, after all.[5]

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[4] It is also to be noted that these remarks come from precisely the same period. The talk upon which "*Über den Zahlenbegriff*" is based was first presented by Hilbert in September 1899 at the meeting of the *Deutsche Mathematiker-Vereinigung* held in Munich.

[5] In this connection, it is also to be mentioned that, in his letter of 29 December 1899 to Frege , Hilbert remarks that "[i]t was of necessity that I had to set up my axiomatic system". [Frege 1980, 38]

Moreover, as the second sentence in the above quotation further suggests, Hilbert seems to think that the foundational inquiries into various domains of our knowledge cannot be successfully carried out through a method of investigation that does not share the definitional procedure adopted in his axiomatics. It follows that, for Hilbert, the traditional axiomatic method must be distinguished, just as is the genetic method, from his version of axiomatics whenever the logical grounding of knowledge is at stake.

§2. A complete logical grounding of knowledge requires the adoption of the Hilbertian method of axiomatics as one's chosen method of investigation; any other method of investigation falls short of providing such a grounding. This, as we saw above, is the conclusion Hilbert reaches via his early foundational investigations. But why does he arrive at such a conclusion? In order to answer this question, I would like first to consider more carefully how Hilbert came to adopt the axiomatic method and the accompanying view of definition for his investigation into the foundations of geometry. As we just saw, Hilbert, by his own account, was led to the (then) non-standard view of definition as he pursued "strictness in logical inference and in the logical construction of a theory." Yet, this concern for *deductive rigor* itself was nothing unusual or extraordinary in the late nineteenth century; many of his contemporaries shared this concern, and some considered, just as did Hilbert, the axiomatic approach to be the most appropriate one to achieve rigor. But what then explains the idiosyncrasies of Hilbertian axiomatics? In considering this question, it is instructive to begin by comparing Hilbert's approach to those of his contemporaries who also adopted the axiomatic method as the procedure of investigation into the foundations of geometry; in particular, the philosopher Gottlob Frege's view gives us a good starting point precisely because of its profound differences from Hilbert's.

What ultimately explains Frege's interest in rigor and the axiomatic method is his quintessentially philosophical concern for the truth and certainty of our mathematical knowledge. Very crudely, Frege's idea is that a proper epistemic justification can be given if, by means solely of truth-preserving, logical deduction, geometry can be developed from a collection of basic propositions, of whose truth we have certain knowledge through pure intuition. For him, then, deductive rigor is important because it precludes the danger of "gaps" or "jumps" in the transition from one proposition to another which may harbor an unwarranted or possibly fallacious inference. On this view, the axiomatic approach is supposed to enable us to achieve rigor by making completely perspicuous the logical relations holding (and not holding) among the propositions of the theory.

Furthermore, Frege's concern for certainty determines the ways in which he conceives axioms and definitions. For him, not only must the geometrical axioms, as the initial elements of the deductive chain, express truths about their subject matter so that propositions derivable from them will also express only truths. It is also the case that their truth must be epistemically transparent and "self-evident." In this way, there will be no doubt about their truth. According to Frege, this fact must also be unambiguously represented in the constructed theory. That is, in the construction of a theory, it must be clear what the axioms are about, and that they are true of this subject matter. For this reason, Frege demands that every theory begin with definitions, whose job is to fix the meaning of previously meaningless terms by means of a properly regimented and precise language such as his own *Begriffsschrift.* In this way, "there is no doubt about the sense of the proposition and the thought it expresses" and "the only question can be whether this thought is true and what its truth rests on" [Frege 1980, 36]. Consequently, it is Frege's invariable opinion that

> axioms and theorems can never try to lay down the meaning of a sign or word that occurs in them, but it must already be laid down. [Frege 1980, 36]

Now, given such a view of definitions and axioms, it is no wonder that Frege was baffled when he found in Hilbert's monograph on geometry the idea that the meaning of the (non-logical) terms occurring in an axiom system is fixed by the axiom system as a whole:

> I have my doubts about the proposition that a precise and complete description of relations is given by the axioms of geometry (sect. 1)[6] and that the concept 'between' is defined by axioms (sect. 3). Here the axioms are made to carry a burden that belongs to definition. To me this seems to obliterate the dividing line between definitions and axioms in a dubious manner, and besides the old meaning of the word 'axiom', which comes out in the proposition that the axioms express fundamental facts of

[6] Hilbert writes there:

> The points, lines and planes are considered to have certain mutual relations and these relations are denoted by words like "lie," "between," "congruent." The precise and mathematically complete description of these relations follows from the *axioms of geometry.* [Hilbert 1899, 3, emphasis in original]

> intuition, there emerges another meaning but one which I can no longer quite grasp. [Frege 1980, 35-36]

In addition to the issue of definitions and axioms, Frege could not understand why Hilbert insisted on providing a consistency proof for the axiom system insofar as the self-evident truth of the geometrical axioms implies that they do not contradict each other.

Confronted with such reactions from Frege, the first thing Hilbert did was to make it absolutely clear that the intentions guiding their respective investigations "differ in kind."[7] While Hilbert did not dwell on what he took Frege's intention to be, he described the motivation guiding his *Festschrift*, *Foundations of Geometry* as follows:

> My intention in composing the *Festschrift* was: to make it possible to understand the most beautiful and important propositions of geometry (unprovability of the parallel axiom, of Archimedes'[s] axiom, provability of the Killing-Stolz axiom etc.), so as to make it possible to give definite answers (some of which turn out very unexpected). [Frege 1980, 41][8]

Hilbert's primary concern is not to provide a proper epistemic justification for our geometrical knowledge, but rather to understand and give definite answers to various theoretical and metatheoretical questions. It is presumably for this purpose that he tries to "rigorize" inferential process in geometry

[7] In the beginning of his letter to Frege of 29 December 1899, Hilbert writes:

> ... One more preliminary remark: if we want to understand each other, we must not forget that the intentions that guide the two of us differ in kind [Frege 1980, 38].

[8] The remark is found in the draft (or excerpt) by Hilbert.

through the employment of the axiomatic method. But in what sense is rigorization thought to be required for this purpose?

The important point to be noticed here is that Hilbert is interested in *understanding* as much as in establishing the cluster of the propositions, which he regards as "the most important results of geometrical inquiries."[9] Indeed, in the introduction of the *Foundations of Geometry*, Hilbert appears to claim that understanding and deducing go in tandem:

> This present investigation is a new attempt to establish for geometry a complete, and as simple as possible, set of axioms and to deduce from them the most important geometric theorems *in such a way that the meaning of the various groups of axioms, as well as the significance of the conclusions that can be drawn from the individual axioms, come to light.* [Hilbert 1899, 2, my emphasis]

What this means, I think, is that Hilbert's interest in rigor is motivated, at least in part, by his desire to *systematize* geometry.[10] The axiomatization of a field of knowledge, if successful, would introduce a small number of fundamental propositions and derive from them every (true) result in the field solely in accordance with the rules of logic. What is to be recognized is that, in so doing, the axiomatic method enables us to achieve an orderly and systematic presentation of

[9] The letter quoted above continues with the following words:

> It was of necessity that I had to set up my axiomatic system: I wanted to make it possible to *understand* those geometrical propositions [*ich wollte die Möglichkeit zum Verständnis derjenigen geometrischen Sätze geben*] that I regard as the most important results of geometrical inquiries: ... I wanted to make it possible to *understand* [*verstehen*] and answer such questions as why the sum of the angles in a triangle is equal to two right angles and how this fact is connected with the parallel axiom. [Frege 1980, 38, my emphasis]

[10] The following account is loosely based upon Philip Kitcher's discussion of "systematization" in [Kitcher 1984, 180-182, 217-220].

the theoretical results, which might otherwise be disordered and completely dispersed in the field, and gives us a unifying perspective on them. This is not all. Systematization by axiomatization would, on the one hand, enhance our understanding of the previously accepted results by exposing clearly their logical relations to each other and to the small number of fundamental propositions: On the other hand, it also lead us to, and enables us to understand, various hitherto unknown results by deriving them from the basic propositions. This fits quite well with Hilbert's own description of the goal of his geometrical investigation and, indeed, he is later to write that the axiomatic method serves the purpose of "orienting" [*Orientierung*] and "ordering" [*Ordnung*] a field of knowledge.[11] It would seem then that one sense in which rigor or strictness in logical inferences is important for Hilbert is that, in the absence of it, we would be unable to achieve a systematic presentation, and thus a clear understanding of, the propositions constituting a field of knowledge.

§3. Another point we should pay heed to concerning Hilbert's interest in rigor is his emphasis upon the "definiteness" of the manner in which various theoretical and metatheoretical questions are answered by means of *his*

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[11] Hilbert emphasizes the "ordering" and "unifying" function of the axiomatic method in the paper "Axiomatic Thought" published in 1918. In this connection, it is also to be noted that in the Paris address Hilbert maintains that "the rigorous method is at the same time the simpler and the more easily comprehended" [Hilbert 1900b, 1099].

axiomatic method.[12] Neither in the quoted letter to Frege nor in its draft, can we find a clear statement from Hilbert about what this definiteness consists in, but it does not seem so difficult to get an idea of his meaning. For instance, in the conclusion to the 1899 essay, Hilbert writes:

> The present treatment is a critical investigation of the principles of geometry. In this investigation the ground rule was to discuss every question that arises in such a way so as to find out at the same time whether it can be answered *in a specified way with some limited means.* [Hilbert 1899, 106, my emphasis]

Although Hilbert does not explicitly say so, it seems quite clear that what he says here captures at least part of what he means by "definiteness." That is, for him, to give a definite answer to a question is to answer it in a specified way with some limited means, whatever he may take such a means precisely to consist of. In fact, shortly after the above passage, Hilbert talks of the close relation between this "ground rule" and the proverbial demand for *purity* in mathematical reasoning:

> The ground rule according to which the principles of the possibility of a proof should be discussed at all is very intimately connected with the requirement for the "purity" of the methods of proof which has been championed by many mathematicians with great emphasis. This requirement is basically none other than a subjective form of the ground rule followed here. [Ibid., 107]

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12 In the letter to Frege, Hilbert writes:

> That my system of axioms allows one to answer such questions in a very definite manner, and that the answers to many of these questions are very surprising and even quite unexpected, is shown, I believe, by my Festschrift as well as by the writings of my students who have followed it up. [Frege 1980, 39]

But why does he think that such a rule is necessary for mathematical investigations? Or, to put it differently, in his view, what is supposed to be achieved through the fulfilment of the demand for purity?

A short answer to these questions is given when it is realized that Hilbert considers the "ground rule" or the demand for purity to be a general requirement for the *solution of a mathematical problem*. In his 1900 Paris address "Mathematical Problems," Hilbert writes:

> It remains to discuss briefly what general requirements may be justly laid down for the solution of a mathematical problem. I should say first of all, this: that it shall be possible to establish the correctness [*Richtigkeit*] of the solution by means of a finite number of steps based upon a finite number of presuppositions [*Voraussetzungen*] which are implied in the statement of the problem and which must always be exactly formulated. This requirement of logical deduction by means of a finite number of processes is simply the requirement of rigour in carrying out proofs [*Strenge in der Beweisführung*]. [Hilbert 1900b, 1099, translation modified]

Here, the "rule" is presented with a little more detail and is identified with the requirement of "rigor," but the message is clear: it is required for the solution of mathematical problems. To understand the exact meaning of this claim, however, we have to recognize a few more things yet. First, the fact that Hilbert considers the requirement of rigor to be a *general* requirement for the solution of a mathematical problem seems to imply that, for him, rigorization is not sought in order to solve any specific mathematical problem that has previously remained unsolved.[13] Indeed, here again, Hilbert refers to the importance of rigor for the enhancement of our

[13] For more on this point, see the next chapter.

understanding of mathematical propositions:

> ... only by satisfying this requirement [of rigor in reasoning] do the thought content [*gedankliche Inhalt*] and the fruitfulness [*Fruchtbarkeit*] of the problem attain their full effect [Ibid., 1099, translation modified].

His interest in rigor, however, is not exhausted by such a concern for the enhancement of understanding alone. To see this, we should note, secondly, the fact that Hilbert's formulation of the requirement of rigor or purity seems to come directly from his notion of *effective process.* Accordingly, it shares certain general features with it. In his 1892 paper, Hilbert talks of a number that "can be actually found by means of calculation in a *finite number* of operations," while, in the paper published a year later, he describes a decision procedure as a way "in which one can decide whether or not δ is a completely algebraic function through processes that are finite and surveyable from the outset."[14] What can be seen from these descriptions is Hilbert's emphasis on the perspicuity and the finite character of the processes in question. As we saw above, these same characteristics are stressed by Hilbert when he describes the "ground rule" of his geometrical investigation as the one according to which every question that arises should be answered "in a *specified* way with some *limited* means." In keeping with this observation, Hilbert's requirement of rigor in the proof presented in the Paris address may then be interpreted to be comprised of two parts: a) that the rules

[14] My translation. The original reads respectively, "sich ... mittels Rechnung durch eine *endliche Anzahl* von Handlungen wirklich finden lässt" [Hilbert 1935 vol. 2, 275]; "wie man durch endliche und von vornherein übersehrbare Prozesse entscheiden kann, ob δ eine ganz algebraische Funktion ... ist oder nicht". [Ibid., 321] Cf. Webb 1980, 75.

of inference employed in the proof must be antecedently specified; b) that the proof-procedure must be "limited" in the sense that the solution is obtained from a finite number of premisses in a finite number of inferential steps. To be sure, in the Paris address, Hilbert does not clearly state what exactly these rules of inference are, but given his identification of the requirement of rigor with that of "logical deduction" in a finite number of steps, it seems reasonable to assume that he has in mind the rules of logic taken in the ordinary sense for such an antecedently specified means of proof.

Now, with a better understanding of Hilbert's notion of rigor in hand, our task is to find out what function rigor is to serve for him. But before going into this question, there is one more thing to be noted. This is the fact that Hilbert seems to think that the fulfilment of these points is required for something other than the mere solution of a mathematical problem. In the 1900 address, he describes rigor in the proof as a requirement for a "perfect" solution of a problem,[15] while, in the *Festschrift*, he says that a rigorous proof for a problem, or for the unprovability of the problem, is necessary if "the drive for knowledge" is to be satisfied:

> ... if in the course of mathematical investigations, a problem is encountered, or a theorem is conjectured, the drive for knowledge is then satisfied only if either the complete solution of the problem and the rigorous proof of the theorem are successfully demonstrated or the basis for the impossibility of success and hence the inevitability of failure are clearly seen. [Hilbert 1899, 106]

At first glance, it might be thought that Hilbert's reference

[15] Ibid., 1100.

to "knowledge" suggests that his interest in rigor stems ultimately from the standard, epistemological concern for certainty, just as in the case of Frege. That is to say, it might be thought that Hilbert insists on the thorough implementation of rigor in order to provide a justification for our believing the truth of mathematical propositions. Yet, given Hilbert's clear statement about the difference of the intentions guiding his investigation from Frege's, and his non-standard view of definition and axiom, it seems unlikely that this is the case. In fact, as he writes in a paper published in 1909, the requirement of rigor or of "logical deduction in a finite number of steps" is not only not motivated by the concern for truth, but even opposed to and hardly compatible with it in the context of *his* foundational investigations:

> In the case of modern mathematical investigations, ... I remember the investigations into the foundations of geometry, of arithmetic, and of set theory--they are concerned not so much with proving a particular fact or establishing the correctness of a particular proposition, but rather much more with carrying through the proof of a proposition with restriction to particular means or with demonstrating the impossibility of such a proof. [Hilbert 1935 vol.3, 72, my translation][16]

But what, then, is the point of "rigor" for Hilbert? What, if not truth or validity, does he think is achieved by a proof with some specified means? What is implicit in his insistence upon the requirement of rigor, I suggest, is his

[16] Bei gewissen modernen mathematischen Untersuchungen ... ich erinnere an die Untersuchungen über die Grundlagen der Geometrie, der Arithmetik, und der Mengenlehre--handelt es sich nicht sowohl darum, eine bestimmte Tatsache zu beweisen oder die Richtigkeit eines bestimmten Satzes festzustellen, sondern vielmehr darum, den Beweis eines Satzes mit Beschränkung auf gewisse Hilfsmittel zu führen oder den Nachweis für die Unmöglichkeit einer solchen Beweisfuhrung zu erbringen.

concern for the *objectivity* of mathematical judgment and reasoning. Hilbert's requirement of rigor, as we just saw, consists of two demands: one for the antecedently fixed rules of inference and one for the finitude of the whole proof-procedure. The fulfilment of these conditions guarantees the objectivity or intersubjectivity of a proof in the sense that it not only eventually terminates, but also that it terminates with the same result, no matter who carries it out. The result may or may not correspond to one's expectation: it may provide a solution to a problem, or it may demonstrate the insolubility of the problem. In either case, if the proof is constructed in full accordance with the requirement of rigor, its result is "definite" and "indisputable." This indisputability of the result, however, does not mean that its truth is indubitable and our knowledge of it certain; what it means is rather that insofar as one follows the antecedently specified rules, one will reach the same result in a finite number of steps.[17] For Hilbert, it is in this non-justificatory sense that rigor serves to provide a "perfect" or "complete" solution to a

[17] Concerning the notion of rule-following, Ludwig Wittgenstein raised such questions as "What is it to follow one rule rather than another?" "Can we distinguish the following of one rule incorrectly from the following of a different rule correctly?," and there has been much discussion among philosophers. This issue does touch upon my interpretation of Hilbert's concern for rigor, but I shall not get into it here.

problem and to satisfy the "drive for knowledge."[18] Thus he has no qualms in maintaining, at the same time, that his foundational investigations are not to be understood in the standard, epistemological sense. Correspondingly, for him, the main function of logic as rules of inference is not to preserve or transmit the truth and a prioricity of the initial propositions through transition from one proposition to another. Rather, it is to assure the objectivity of such a transition itself. Although it is slightly anachronistic to cite it, what Hilbert wrote about his proof theory in the late 1920s would help us see what is at stake in his pursuit of rigor:

> ... our understanding does not practice any secret arts, but rather always proceeds according to well-determined and presentable [*aufstellbar*] rules. And this is at the same time the guarantee for the absolute objectivity of its judging [*seines Urteilens*]. [Hilbert 1929, 233]

Indeed, such a concern for the objectivity of mathematical judgment and proof, I think, explains not only why Hilbert is interested in deductive rigor, but also why he is led to the sort of the view that he holds of definitions and axioms. Once again, a comparison with Frege's view is useful. As we saw

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[18] In the beginning of the 1900 address, Hilbert characterizes the notion of "completeness" or "perfectness" in terms of "clearness" and "ease of comprehension":

> An old French mathematician said: 'A mathematical theory is not to be considered *complete* [*vollkommen*] until you have made it so clear that you can explain it to the first man whom you meet on the street.' This clearness and ease of comprehension [*Diese Klarheit und leichte Faßlichkeit*], here insisted on for a mathematical theory, I should still more demand for a mathematical problem if it is to be *perfect* [*vollkommen*]; for what is clear and easily comprehended attracts, the complicated repels us. [Hilbert 1900b, 1097, my emphasis]

earlier, in accordance with his intended goal of justifying our geometrical knowledge, Frege demands that the axioms of geometry, from which the other geometrical propositions are derived by means of (truth-preserving) logical deduction, must be true of their subject-matter. He also demands that, in the construction of a theory, the meaning of the basic terms occurring in the axioms be antecedently fixed by using a precise language, such as his *Begriffsschrift,* so that there can arise no question about the propositional content of the axioms, and thus about their truth.

But how exactly is definition carried out on Frege's account? Given that the axioms are a true representation of a certain theory-independent realm of objects, it would follow that the basic terms occurring in them denote these theory-independent objects. This, in turn, would imply that in order for the definition of the basic terms to be possible, we must possess, *independently of the axioms*, the knowledge of what objects are denoted by them and, indeed, of what these objects essentially are. That is, on Frege's account, we define the meaning of the basic terms by means of our extra-theoretical knowledge of the subject-matter of geometry and, in accordance with this definition, formulate the axioms as self-evident truths about it.

The problem with such a procedure is that precisely because the subject-matter of geometry is conceived to be independent of the axioms, there seems to be no common framework within which we can give an answer to questions such as "What objects are denoted by the basic terms of geometry?" or "What are the essential properties of these objects?" Thus, once a disagreement arises over the acceptability of a proposed definition, there would be no way to settle the dispute; in the absence of a common framework, no party would be either correct or incorrect. By the same token, if, in such a case, one simply goes ahead and introduces a definition, this would amount to presupposing (extra-theoretical) knowledge of the subject-matter; and, consequently, one's construction of theory becomes dogmatic. In other words, the traditional view of definition makes the process of definition conditional upon, and thus relative to, one's background philosophical views. I think these are some of the points that Hilbert had in mind when he wrote to Frege as follows:

> You say my explanation in sect. 3 is not a definition of the concept 'between', since it fails to give its characteristic marks. But these characteristic marks are given explicitly in axioms II/1 to II/5. ... You say further: 'The explanations in sect. 1 are apparently of a very different kind, for here the meaning of the words "point", "line", ... are not given but are assumed to be known in advance.' This is apparently where the cardinal point of misunderstanding lies. I do not want to assume anything as known in advance; I regard my explanation in sect. 1 as the definition of the concepts point, line, plane--if one adds again all the axioms of groups I to V as characteristic marks. If one is looking for other definitions of a 'point', e.g., through paraphrase in terms of extensionless, etc., then I must indeed oppose such attempts in the most decisive way; one is looking for something one can never find because there is nothing there; and everything gets lost and becomes vague and tangled and degenerates into a game of hide-and-seek.

[Frege 1980, 39][19]

Now, Hilbert's claim that "one is looking for something one can never find because there is nothing there" might be thought to suggest that his concern has to do with the non-existence or abstract nature of mathematical objects and, therefore, that he would not object to the traditional method of definition in cases where a theory deals with concrete objects to which we have direct epistemic access. Such a reading, however, is mistaken. First, as Hilbert writes immediately after the quoted passage, it is his contention that the consistency of the geometrical axioms guarantees the truth of the axioms and the existence of the things defined by them.[20] Second, as we saw earlier, it is also Hilbert's opinion that not only "abstract" sciences such as mathematics but the natural sciences too must also be treated in the manner of the Hilbertian axiomatics:

> In my opinion, a concept can be fixed logically only by its relation to other concepts. These relations, formulated in certain statements, I call axioms, thus

[19] Letter to Frege of 29 December 1899. In the corresponding passage in the draft (or excerpt), Hilbert puts the points in this way:

> Instead of 'axioms' you can say 'characteristic marks' if you like. But if one is looking for another definition of, e.g., 'points', perhaps through paraphrase in terms of extensionless ..., then I reject such attempts as fruitless, illogical and futile. One is looking for something where there is nothing. The whole investigation becomes vague and tangled and degenerates into a game of hide-and-seek. [Frege 1980, 41]

Note Hilbert's use of the term "illogical" to describe the traditional way of definition.

[20] Is this not a sheer contradiction to Hilbert's denouncement of the epistemological character of his foundational investigation we saw above? I shall come back to the issues surrounding Hilbert's "identification" of consistency with truth and existence in Chapter 3.

> arriving at the view that axioms (perhaps together with propositions assigning names to concepts) are the definitions of the concepts. I did not think up this view because I had nothing better to do, but I found myself forced into it by the requirements of strictness in logical inference and in the logical construction of a theory. I have become convinced that the more subtle parts of mathematics and the natural sciences can be treated with certainty only in this way; otherwise one is only going around in a circle. [Frege 1980, 51]

Indeed, for Hilbert, the traditional method of definition, which involves reference to theory-independent objects, is objectionable regardless of the ontological and epistemological status of the definienda. For, insofar as the basic terms of a theory are thought to denote objects existing independently of its axioms, there would be no "rigorous" procedure by means of which we can settle questions about the referents of these terms and their nature, whether they be "abstract," mathematical terms such as "point" and "real number" or "concrete," physical terms such as "gold" and "water."[21] In short, such a definitional procedure is "illogical" and lacks objectivity.

But how, then, does Hilbert solve the difficulties associated with the traditional method of definition? As is indicated in the above remark, Hilbert gets to the root of the problem. That is, he tries to solve the problem by denying the underlying assumption that the non-logical (basic) terms

21 In this connection, it has been pointed out by Michael Hallett that Hilbert's position on meaning is to be examined against the backdrop of certain late 19th century developments in classical physics, in particular, the anti-metaphyical tendencies found in Heinrich Hertz's views of physical theories. According to Hallett, Hertz argued that questions about the metaphysical nature of an object or concept, or questions about the "essential" meaning of terms are futile if they are posed *over and above* the properties and relations ascribed to them by a system of physical theory. For more on Herz's influence upon Hilbert, see Hallett 1990, 219-223 and Webb 1980, 78-81.

occurring in the axioms have theory-independent denotations. Once it is denied that there are theory-independent objects to which the non-logical terms of the theory are supposed to refer, it is both unnecessary and impossible to formulate (theory-independent) definitions of these terms: their meanings need not and cannot be fixed in reference to theory-independent concepts or objects. As a result of this unanchoring, the non-logical terms in a theory constructed in accordance with Hilbert's axiomatic method become "place-holders," as it were. Even so, it is important to recognize that this does not mean that they are devoid of meaning or, simply, meaningless. Since these terms occur in sentences (i.e. in the axioms of the theory), they obtain (intra-systematic) meaning from their relations to each other. Consider, for instance, the following proposition, which is one of the geometrical axioms Hilbert presents in the *Foundations of Geometry*:

> For every two points *A, B* there exists a line *a* that contains each of the points *A, B*.[Hilbert 1899, 3]

The non-logical terms "point," "line," and "contains" here have no theory-independent meanings attached to them. Thus, they should be seen as mere place-holders. Nevertheless, they do obtain some sort of meaning or sense through their co-occurrence in the axiom: the axiom gives us information about what it is to be a point or a line in terms of the incidence-relation, whereas the sense of this incidence-relation is (partially) determined through the manner in which it holds (or does not hold) between these elements. At this stage, the "meanings" of these basic terms are quite general, but they will become more and more specific and determinate as they occur in a series of axioms. In Hilbert's own words,

> The points, lines and planes are considered to have certain mutual relations and these relations are denoted by words like "lie," "between," "congruent." The precise and mathematically complete description of these relations follows from the *axioms of geometry*.[Hilbert 1899, 3]

In the *Festschrift*, Hilbert lists five groups of axioms, some nineteen in total, each of which contributes to this meaning-fixing.[22]

The upshot of all this is that, in the Hilbertian axiomatics, the meaning of a (non-logical) term is determined by an axiom system *as a whole* and is not "complete" until the building of the system is complete.[23] Furthermore, it also follows that a change in an axiom system implies a change in the meaning of a term occurring within it; accordingly, in Hilbert's view, it is quite possible that one and the same non-logical term has different meanings in different axiom systems, as he remarks in the quoted letter to Frege:

> Every axiom contributes something to the definition, and hence every new axiom changes the concept. A 'point' in Euclidean, non-Euclidean, Archimedean and non-Archimedean geometry is something different in each case. [Frege 1980, 40]

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22 Hilbert explicitly states that "each of these groups expresses certain related facts basic to our intuition" [Hilbert 1899, 3]. But this reference to the alleged source of the geometrical axioms, as Ernest Nagel points out, "is essentially a biographic statement," and should not be thought to imply that Hilbert considers the technical terms occurring in the axioms as denoting certain theory-independent objects.

23 In the quoted letter to Frege, Hilbert writes that "the definition of the concept point is not complete till the structure [Aufbau] of the system of axioms is complete" [Frege 1980, 42].

This, then, is what is usually understood by "implicit definition" and "definition by axioms."

Furthermore, since, on Hilbert's conception, definition or meaning-fixing is executed entirely and solely by means of the logical relations formulated in a group of explicitly stated statements, the whole procedure is completely perspicuous. In fact, as a result of this methodological turn, questions about the properties of the defined concepts, which seemed previously to require metaphysical investigations into the essence of objects, are now expressible as questions about the properties of the axiom system as a whole and thus treatable as questions falling in the domain of mathematics and logic. To mention one example, instead of trying to decide the "correctness" of the proposed definition of a term in a field of knowledge through metaphysical investigations into what the term in question denotes and what its referent essentially is, Hilbert's axiomatic method allows us to formulate the question as one about whether it is possible to deduce, from the axioms fixing the meaning of the term, a class of statements which are commonly accepted as true in the field. Moreover, by providing such a means of question-formulation, Hilbert's axiomatic method, at the same time, guarantees the *freedom* of concept-formation: one is free to propose one's own definition of a concept insofar as it is put forward in the form of an axiom system and, hence, susceptible to rigorous and objective treatment.

§4. Let me recapitulate what I have argued so far. First, a careful reading of the text showed us that Hilbert thinks what he calls "a complete logical grounding of knowledge" requires the adoption of Hilbertian axiomatics as one's chosen method of investigation. Next, in order to understand this claim, we examined how Hilbert came to adopt the axiomatic method and his accompanying view of definition for his investigation into the foundations of geometry. In so doing, special attention was paid to Hilbert's emphasis upon "strictness in logical inference and in the logical construction of a theory," and we were led to the conclusion that there are at least two primary concerns motivating Hilbert's pursuit of rigor and the adoption of his (then) non-standard view of definitions and axioms. One of them has to do with Hilbert's desire to enhance our understanding of geometrical propositions. He tries to achieve this goal by systematizing the accumulated results of the science through axiomatization. The problem, however, is that while this project of systematization by axiomatization accounts partially for his interest in rigor, it alone does not seem capable of explaining the distinctive characteristics of Hilbertian axiomatics. To consider this point, we then examined Hilbert's notion of "rigor" more closely. What we learned from this examination is that, for Hilbert, rigor involves not only the strict observance of logical laws in all proofs, but also the finitude, and thus the perspicuity and executability, of the whole proof-procedure. This, I then argued, suggests that Hilbert's interest in rigor stems ultimately from his concern for the objectivity of mathematical judgment and reasoning. Finally, to support this claim, I tried to show that Hilbert's concern for objectivity led him to relinquish the assumption of theory-independent denotations which underlies the traditional conception of definitions and axioms.

The idea of banishing extra-systematic elements from the construction of a scientific theory thus essentially characterizes Hilbert's axiomatic method. However, I do not mean to claim that it is his concern for objectivity that is solely responsible for the introduction of this view. Here, we have, obviously, to take into account manifold developments which took place in geometry and mathematics in general, and in neighboring exact sciences such as physics, towards the late nineteenth century. In this connection, Hilbert's principal collaborator in the foundational investigations, Paul Bernays, refers to some extrinsic factors that were conducive to Hilbert's "new methodological turn" in the conception of axiomatics. Following Bernays's account, I go over these points briefly. First, Bernays notes that physicists around that time started to adopt both empirical statements and mere hypotheses as axioms of physical theories so that the results of multifarious experiences could be encompassed in a statement of general character; and, consequently, that the "demand that each axiom should express an a priori knowable truth was soon abandoned" [Bernays 1922b, 191].[24]

Second, the discovery of non-Euclidean geometry and Helmholtz's influential arguments for the empirical character of the geometrical axioms, according to Bernays, led many to give up on their belief in the *a priori* knowledge of geometry. Towards the mid-nineteenth century, the age-old effort to demonstrate the parallel postulate from the other assumptions of Euclidean geometry by means of the indirect, or *reductio ad*

[24] As an example of this, Bernays cites "the two propositions about the impossibility of a *perpetuum mobile* of the first and second type, which Clausius put at the top as axioms of theory of heat" [Bernays 1922b, 191].

absurdum, method of proof resulted in the construction of new systems of geometry, which, although no contradictions seemed to follow in them, seemed to be in violent contradiction to what habitual "intuitions" tell us about the nature of space if their non-logical terms are understood in the ordinary manner.[25] The first proof of the consistency of one of such new geometries was presented by Beltrami in 1868,[26] in which he showed that the plane non-Euclidean geometry of Lobachevsky and Bolyai can be represented, with certain restrictions, on a surface of constant negative curvature such as the pseudosphere, or tractoid.[27] The significance of Beltrami-style consistency proofs is twofold. In the first place, by giving a Euclidean "model" to the newly "invented," non-Euclidean geometries, they established conclusively that the "bizarre" non-Euclidean geometries are consistent if Euclidean geometry is consistent.[28] In the second place, as Ernest Nagel points out, these proofs, by offering an interpretation of the new geometries in terms of "intuitions" of familiar Euclidean surfaces, "took the wind out of the sails of those who insisted that the new systems could not be construed as 'geometries' even though they were consistent calculi, on the ground that they lacked an 'intuitive content'" [Nagel 1979, 243].

[25] Very roughly, the idea goes as follows. We start by assuming the truth of the negation of the parallel postulate, together with that of the other axioms of Euclidean geometry. If we succeed in deriving a contradiction in this non-Euclidean system, it will show the inconsistency of the system and thus that it is impossible for those statements to be true collectively. Since this means that it is impossible that the negation of the parallel postulate is true, or the parallel postulate is false while the others true, the parallel postulate is implied by the other axioms.

[26] "Saggio di interpretazione della geometria non-Euclidea," *Giornale di Matematiche* 6 (1868): 74-105.

[27] For more detailed discussion of Beltrami's and others' consistency proofs of non-Euclidean geometries, see Eves 1990, 65-70.

[28] For more on consistency proofs, see Ch. 4.

An important consequence of this course of events is the liberation of geometry from the question of validity and thus its separation from geometry as a physical science. Given that seemingly "incorrect" non-Euclidean systems are, in the logical sense, as legitimate as Euclidean geometry, there arises the possibility of studying various systems of "geometry" without regard to their applicability to physical space and, thus, without regard to the epistemic character [*Erkenntnischarakter*] of their axioms.[29] On the other hand, as Howard Eves observes,

> [w]ith the possibility of inventing such purely "artificial" geometries it became apparent that physical space must be viewed as an empirical concept derived from our external experience, and that the postulates of a geometry designated to describe physical space are simply expressions of this experience, like the laws of a physical science. [Eves 1990, 68][30]

Another important development occurred four years after

[29] Note the striking contrast between such a conception of *pure* geometry and the Kantian theory of geometry which dominated Europe's philosophical landscape at the time of the discovery of the non-Euclidean geometries. According to Kant, geometrical reasoning requires for its very possibility "construction in pure intuition," and since the pure intuition of space constitutes a condition for the possibility of our experience, Euclidean geometry, which is the only geometry representable by means of pure intuition, is necessarily valid of the objects of experience.

[30] This roughly corresponds to main theses of Helmholtz. Helmholtz's primary concern was to argue against the so-called "nativist" school of physiology, that human perceptions (such as our perception of space) are not simply determined by innate, physiological mechanisms, but that (conscious and subconscious) psychical processes are also involved. As regards our spatial perception, Helmholtz first thought (mistakenly) that, with the condition that space be three dimensional and infinite, our universal experience of the congruence of rigid bodies determined space as Euclidean. But when Beltrami's aforementioned paper appeared, he realized his mistake and added the principles of mechanics to the experiential basis for our geometry, and thereby made it dependent upon conscious experience subject to proof or disproof by experiment. For a detailed account of Helmholtz's investigations into the foundations of geometry, see Richards 1975.

the publication of Beltrami's paper when Hilbert's future colleague at Göttingen, Felix Klein, applied considerations drawn from the algebraic theory of groups to geometry.[31] Very crudely, the chief results of Klein's investigation may be explained as follows.[32] Within each geometry, there are certain transformations which may be carried out without changing the relations or properties characteristic of that geometry. For example, such transformations as translations, rotations, and reflections in lines leave unchanged or invariant those properties which are characteristic of ordinary Euclidean geometry, e.g., length, area, congruence, parallelism, perpendicularity, similarity of figures, collinearity of points, and concurrence of lines. Given such mutual relationships between the properties characteristic of a geometry and certain transformation groups, the idea suggests itself that a geometry can be characterized by either of the two. Thus, instead of saying that Euclidean geometry studies those metrical properties listed above, we may also say that it studies those properties which remain invariant under the group of transformations comprised of translations, rotations, and reflections in lines.

What we must see here is that Klein thereby opened a way to consider the concept of "geometrical" from an abstract perspective, that is, the "geometrical" simply as that which is characterized by an invariance with respect to a certain set of algebraic operations. With the help of this simple but powerful idea, some very surprising results follow. Here I

[31] "Vergleichende Betrachtungen über neue geometrische Forschungen," Mathematische Annalen, Bd. 43, 1872.

[32] A more detailed account of Klein's so-called Erlangen Program can be found in Eves 1990, 128-132 and Nagel 1979, 242-249. My account is basically an abridged version of these two writers'.

mention only three, which are of particular importance for our purpose. According to one such result, if the groups of transformations which characterize respectively two different geometries have the same formal content and thus are formally identical, then these geometries, whatever their respective fundamental elements might be, are structurally identical in the sense that "for every theorem about an invariant property in one geometry, there is a 'dual' theorem about a corresponding invariant property in each of the others."[33] Second, Klein established the structural identity, in this sense, of the Euclidean and non-Euclidean (Lobachevskian and Riemannian) geometries. Further he showed that the differences among these three types of geometries could be construed as stemming entirely from the differences in their definitions of metrical notions such as distance, angle, area, and volume. Klein thus succeeded in giving a conclusive answer to the question of the precise nature of the logical relations between Euclidean and non-Euclidean geometries, which Beltrami-style "model-theoretic" approaches were unable to answer.[34] Finally, the particular manner in which Klein demonstrated the structural identity of the three types of geometry provided, at the same time, a unitary and systematic picture of geometry. Klein obtained all of these geometries by using the concept of

33 Nagel 1979, 246. Of the abstract nature of group-theory, Klein writes elsewhere that the concept of groups is manifestly characteristic of "a wholly intellectual mathematics that has been purged of all intuition; of a theory of pure forms with which are associated not quantities or their symbols, numbers, but intellectual concepts, products of thought, to which actual objects or their relations may, but need not, correspond." F. Klein, *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, "Die Grundlehren der mathematischen Wissenschafteen," XXIV (1926), I, 335, quoted in Cassirer 1949, 30.

34 As we saw above, all that they showed was that certain non-Euclidean geometries (e.g., geometry of Lobachevsky and Bolyai) are consistent if Euclidean geometry is consistent.

"distance" defined entirely in the language of projective geometry (as a theory of invariance of projective transformations). This meant that it could be developed without employing any postulate of parallels or axioms of congruence, and thereby showed that these metric geometries could be taken to be "contained" in projective geometry.[35]

Apparently, Bernays had in mind these consequences, which had resulted from group-theoretic approaches such as Klein's,[36] when he referred to a "powerful change" brought about by the "systematic development of geometry":

> Mathematical abstraction had, starting with elementary geometry, raised itself far above the domain of spatial intuition and had led to the construction of comprehensive systems, in which ordinary Euclidian [sic] geometry could be incorporated and within which its lawlikeness appeared only as one particular among others of equal mathematical rights. With this a new sort of mathematical speculation opened up by means of which one could consider the geometrical axioms from a higher standpoint. [Bernays 1922b, 191]

The upshot of all this was a growing awareness among the scientists of the time that "this mode of consideration had nothing to do with the question of the epistemic character of the axioms":

> Accordingly, the necessity of a clear separation between

[35] To be more precise, the transformation group of the metric geometries is contained as a subgroup in the transformation group of projective geometry, under which, of the previously mentioned properties, only collinearity of points and concurrence of lines remain invariant. In this manner, Klein established a "sequence of nesting geometries," progressing from metric to affine and projective geometry and finally to *analysis situs* or topology in terms of their respective transformation groups.

[36] In this connection, the geometrical research of Sophus Lie must be also mentioned.

> the mathematical and the epistemological problems of axiomatics ensued. The demand for such a separation of the problems had already been stated with full rigor by Klein in his Erlangen Programme. [Ibid., 191-192]

The primary significance of Hilbert's *Festschrift*, on Bernays's view, consists in the fact that in it such a separation was executed, for the first time, with full consciousness and with full rigor, in the mould of the axiomatic method:

> The important thing, then, about Hilbert's "Foundations of Geometry" was that here, from the beginning and for the first time, in the laying down of the axiom system, the separation of the mathematical and logical [spheres] from the spatial-intuitive [sphere], and with it from the epistemological foundation of geometry, was completely carried out and expressed with complete rigor. [Ibid., 192]

Although it is arguable whether the content of the denial of extra-systematic meaning and denotation, which characterizes Hilbertian axiomatics,[37] can be exhausted by such a separation of the logico-mathematical from the epistemological, Bernays's account helps us see the general intellectual atmosphere in which Hilbert's investigation into the foundations of geometry is embedded and from which his view of definitions and axioms arose.

§5. So far my consideration has focused on how the two types of axiomatics differ, and we have yet to examine Hilbert's view on the genetic method. As we saw above, Hilbert explains in his 1900 essay that, in a theory-construction in accordance with this method, various general concepts are

[37] I shall consider this in some detail in Ch. 3.

engendered through the successive extensions of the basic or primitive concept(s) of the theory. But in what sense may concepts be said to be "basic" and "primitive" there? Apparently, one sense Hilbert attaches to these terms is *psychological* (and genetical). This is why, later in the essay, he speaks of "the high pedagogic and heuristic value of the genetic method": the genetic method delivers not only a method of theory-construction, but also a psychological aid which facilitates the learning of a scientific theory. Our question is then how the genetic method fares with regard to the problem of a complete logical grounding of knowledge. Before tackling this question, however, we need to have a better picture of this method of investigation; in particular, we must get clear about what is precisely meant in his seemingly figurative manner of speech: a concept is *engendered* by the *successive extension* of a theory's basic concept.

Generally speaking, in constructing a theory in accordance with the genetic method, one usually begins with the definition of its fundamental concepts. To put it slightly differently, here one usually begins by specifying the objects of the theory with their essential properties. After this is done, the fundamental operation(s) of the theory are introduced which are (universally) applicable to these objects, and the laws of operations are laid down as general truths, from which various results follow deductively. At this point, however, one may find that the theory constructed thus far falls short of one's goal of reconstructing an existing discipline, where numerous operations unknown to the constructed theory are freely and universally applied to the "objects" that are not found in its objective domain. It is at this point that a need arises for the "successive extension" of the theory's basic concept(s) in such a way that certain hitherto only limitedly executable operations become universally applicable.

To illustrate and to understand the "mechanism" of this procedure more clearly, let us consider its employment in a particular discipline. Suppose that we wish to reconstruct, in accordance with the genetic method, the mathematical discipline of arithmetic--conceived in such a broad sense that analysis is included--as a theory whose subject matter consists of the natural numbers and their interrelations. The first thing we would do is specify the objective domain of the theory by means of some essential properties of the natural numbers and define their relations (e.g. =, <, >). After this is done, the fundamental operations of arithmetic, in terms of which other possible operations on the natural numbers are definable, would be introduced: addition and multiplication, for instance, might be chosen as arithmetic's basic operations and introduced using definitions. Furthermore, certain relevant properties of these operations (e.g. commutativity, associativity) would then be established and laid down as the rules of operations, in accordance with which innumerous calculational results would follow in a purely deductive manner.

It goes without saying, however, that the constructed theory is, at this point, still far from exhausting the whole content of arithmetic. To mention one particular difference between the two: in ordinary arithmetic, we find, in addition to the natural numbers, such things as the integers (positive, zero, and negative), on which such an operation as subtraction is universally applied, whereas, in the constructed theory, we find none of these: although subtraction might be introduced into it as the inverse operation of addition, it could not be carried out universally on the natural numbers. Now, one way of amending the situation would be to extend the domain of the theory simply by *postulating* a new type of object called "integer" to which the hitherto only partially executable operation of subtraction is universally applicable, whatever "subtraction" might mean with regard to the newly introduced object.[38] This, however, is not how the situation is dealt with in the genetic method. According to this method, theory-construction is to be proceeded by means of so-called "constructive definitions," in which newly introduced objects, relations, and operations are defined solely in terms of the *basic* objects, relations, and operations specified at the outset of the theory-construction. The point here is not merely to capture all and only those properties of the objects, relations, and operations to be introduced, but to do this by employing *only* the initially specified and thus "authentic" concepts of the theory. In the case at hand, definitions of the integers, their relations, and operations on them are available which capture the content of what is understood by them in ordinary arithmetic *and* are formulated solely in terms

[38] To do this would amount to jettisoning the initial assumption that arithmetic is the theory of natural numbers and their interrelations.

of natural numbers, their relations, and operations on them;[39] consequently, the reconstruction of arithmetic would proceed by establishing laws of operations and deducing further results from them.

It would seem, then, that, strictly speaking, there exists, in a theory-construction in accordance with the genetic method, neither an "extension" of the theory's basic concept(s) nor an "engendering" of new concept(s). To refer to the above example, what exists in providing constructive definitions of integers is neither that the basic concept of the theory, that of natural number, is extended in such a way that it now comprehends the previously heterogeneous concept of integer, nor that a new concept of integer is engendered from the concept of natural number. Rather, the definition, if successful, establishes that the term "integer" may be construed as a short-hand symbol for ordered pairs of natural numbers and that every statement about integers is expressible as, or translatable into, a statement about natural numbers. What is achieved in constructive definitions is then not so

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[39] The definition of the integers as *ordered pairs* of natural numbers, for instance, would do the job. The motivation for this definition stems from the fact that any integer, whether it is positive, zero, or negative, is representable as the difference $m - n$ of two positive whole numbers m and n, e.g., $2 = 4 - 2$, $0 = 1 - 1$, $-3 = 2 - 5$, etc. But since subtraction cannot be carried out on the natural numbers when $m \leq n$, the integers are introduced instead as ordered pairs $<m, n>$ of natural numbers, where by $<m, n>$ we really have in mind the difference $m - n$. Accordingly, an integer $<a, b>$ is said to be *equivalent to* another integer $<c, d>$ if and only if $a + d = b + c$ (because $a - b = c - d$ if $a + d = b + c$), where "a," "b," "c," "d," designate arbitrary natural numbers; "+" the addition of two natural numbers; and "=" the identity of two natural numbers. Similarly, since $(a - b) + (c - d) = (a + c) - (b + d)$, the *sum* of two integers $<a, b> + <c, d>$ is defined to be the ordered pair of natural numbers $<a + c, b + d>$, whereas $<a, b> - <c, d>$ may be defined to be $<a + d, b + c>$ because $(a - b) - (c - d) = (a + d) - (b + c)$. Other relations of the integers and operations on them could be constructively defined in a similar manner. For more details of such "constructions," see Eves 1990, 191ff.

much an "extension" as a "reduction" in the sense that the integers are theoretically dispensable: it is not necessary to consider them as something that is distinct from the objects and relations in the initial theoretical domain. By the same token, if one succeeds in giving constructive definitions to all the concepts used in a scientific discipline, this would establish that every statement comprising this discipline is translatable into a statement about those fundamental objects which are specified at the outset of the theory-construction and, therefore, that the discipline can be thought of as the theory of those objects and their interrelations.[40]

§6. But why would one seek to carry out such a reduction? As the case of the construction of an arithmetical theory might suggest, a primary reason for employing the genetic method is usually epistemological or ontological or both. The idea is that one begins a theory-construction with an epistemologically privileged class of objects as its basis, and that if one succeeds in establishing, by means of a stepwise introduction through constructive definitions, that all the higher-level concepts of a scientific discipline (including those which are of an epistemologically problematic nature) are theoretically dispensable, i.e., all its statements are translatable into statements about the basic objects, then one would thereby succeed in establishing the legitimacy and certainty of the "knowledge" in question in the sense that the correctness of all its (true) statements are, in principle, knowable or

[40] In the case of arithmetic, a successful theory-construction in accordance with the genetic method would yield what is usually referred to as the "arithmetization" of analysis: it would show that arithmetic (conceived in the broad sense) can indeed be construed as the theory of natural numbers and their interrelations.

verifiable. But the fact that the genetic method is usually adopted for epistemological purposes also means that its application usually comes with the assumption that the theory constructed according to this method has a theory-independent realm of objects as its subject matter; consequently, the fundamental concepts of the theory are defined in reference to these theory-independent objects by means of one's pre- or extra-theoretical understanding of them;[41] the fundamental operations are selected and formulated in accordance with the nature of the objects and their relations; and the laws of operations express general truths about these objects as well as about the operations. However, it would seem, then, that the genetic method (accompanied by such an assumption) would be considered by Hilbert as not suitable for the *logical* construction of a theory for precisely the same reason that he rejects traditional axiomatics.

Yet, provided that Hilbert's objection to the old axiomatics is that the assumption built into this method about the epistemic character of a theory and its axioms could hinder a thorough investigation into the logical structure of a scientific knowledge, it might well be argued that the objection is not applicable to the genetic method *per se,* since such an assumption is not embedded in the genetic method as a procedure of theory-construction. To be sure, the relinquishment of the assumption, and thus the elimination of extra-theoretical elements from a theory-construction, would

[41] This may mean either that the definition involved here is an explicit definition, in which the concept(s) are defined by means of (extra-theoretical) terms whose meaning is already known, or that the objects of the theory themselves are "exhibited" as in Hilbert's "concrete-contentual number theory," in such a way that the signs themselves constitute the theory's subject matter.

imply that the definition of basic concept(s) must be carried out without recourse to any extra-theoretical elements and, therefore, that it not be an *explicit* definition. But the "problem" seems surely to be soluble by defining the basic terms contextually, i.e. by implicit definition. To take the case of arithmetic as an example, instead of beginning the theory-construction with an explicit definition of "natural number," we might begin by listing a series of operational rules such as this:[42]

If a *and* b *are natural numbers, then* a + b = b + a;

where the terms "natural numbers" and "+" have no theory-independent meaning attached to them, and where the logical terms "if ... then" and "=" are assigned their normal meanings. Thus, these "laws" of operations collectively determine the meaning of operational signs, and the so-defined operations, in turn, define "natural numbers" as their operational results or simply as objects satisfying these operations. Once the concept of natural number is defined purely intra-theoretically in this manner, theory-construction would proceed by means of a stepwise introduction of a "new" concept through explicit or constructive definitions.

It is true that the axiomatic method is employed here in contextually defining the basic concept of arithmetic and thus providing the starting-point for the theory-construction. However, unless Hilbert goes as far as to identify the

[42] Strictly speaking, these operational "laws" are not sufficient for the construction of a theory of natural numbers, and we have to add a few more axioms, which include a partially formalized version of the principle of mathematical induction. For the complete list of such axioms or postulates, see Eves 1990, 184.

distinction between explicit and implicit definition with that between genetic and axiomatic method, there seems to be no reason why implicit definition should not be used with the genetic method, nor does there seem any reason to conclude that the genetic method is not capable of providing an appropriate procedure of investigation for what Hilbert calls the logical grounding of knowledge, insofar as the relevant metatheoretical proofs (e.g., completeness, consistency) are forthcoming.[43]

[43] In connection with Hilbert's aforementioned criticism of the "mistaken" procedure often found in the contemporary physicists' theoretical investigation, Frege presented the following objection to the stepwise introduction of concepts used in the genetic method:

> Indeed, the defect of the genetic method lies precisely in this: that the concepts are not ready [*fertig*] and are nevertheless used in this less than ready and hence not properly usable state, and that we never know whether a concept is finally ready. So it happens that after a proposition has been proved it becomes false again because of the continued development, for the thought contained in the proposition becomes a different one. Such changes are especially dangerous, for since the wording remains the same, one does not even become fully aware of the change. [Frege 1980, 44]

The concept of "number," for instance, appears to undergo certain changes in the theory-construction in accordance with the genetic method: at the beginning, "number" means natural numbers, whereas, in the subsequent stages of development, it would also mean integers as, say, ordered pairs of natural numbers; rational numbers as, say, ordered pairs of integers; real numbers as, say, Dedekind cuts in the set of rational numbers and so on. Thus, one and the same sign "1," for instance, might designate the natural number 1 in one context and the ordered pair $<1, 0>$ in another. This fact, however, does not create such a problem as Frege mentions, insofar as those members of a newly introduced class which correspond to the class constituting the domain of the theory at the immediately preceding stage have precisely the same (formal) properties as their counterparts. Take the case of integers as an example. Providing that all natural numbers, x, are defined as members of the class which have the form $<x + a, x>$, it can be shown that they behave precisely in the same manner in every context: that they are *isomorphic*.

Chapter II

Two Kinds of the Arithmetization of Analysis

§1. In the first chapter, we saw, among other things, that the pursuit of rigor or strictness in the logical construction of a theory led Hilbert to oppose the assumption of extra-systematic meaning and denotation which traditionally accompanies the construction of a scientific theory. The characteristic features of Hilbert's axiomatic method are to be understood in reference to this circumstance. Even so, the scope of Hilbert's remark in the 1900 essay about the superiority of the axiomatic over the genetic method for the complete logical grounding of our knowledge is not confined to this point. In considering Hilbert's intent in that remark, it is important to recognize that his early writings on the foundations of mathematics were intended to be his solution to a dispute between two different methodological standpoints. These two standpoints were motivated by two different concerns about foundational issues, as well as by the then-recent discoveries of the set-theoretical paradoxes. More specifically, it remains to be seen that Hilbert's early foundational project is directly related to the problem of the so-called "arithmetization" of analysis, in response to which two radically different attempts were put forward by Dedekind, Cantor, and Weierstrass on the one hand and Kronecker on the other. Furthermore, while these mathematicians all employed the genetic method for the implementation of their desired arithmetizations, Hilbert's axiomatic method was meant to articulate and develop what he took to be the true core of the former approach in opposition to the latter's advocacy of "strict arithmetization." Thus, if we are to grasp a proper sense of the *Problematik*, within which Hilbert's discussion of the axiomatic and genetic methods is framed, it is all but imperative that we first acquire a clear understanding of this dispute and its philosophical significance. Accordingly, in this chapter, I shall first briefly explain some pointed features of the problem of arithmetization of analysis and then outline two different kinds of arithmetization put forward by Dedekind and Kronecker. When this is done, I will consider how these two attempts differ and what explains the difference, with a special emphasis on Dedekind's project in his 1872 essay *Continuity And Irrational Numbers* in order better to understand the nature of Hilbert's early foundational investigation.

The problem of arithmetizing analysis found its formidable contenders through the personal charisma of the great nineteenth century number theorist, Dirichlet. Dirichlet, who was the successor of Gauss in Göttingen and whose influence on then-contemporary mathematicians can be characterized as a "spiritual" one,[1] was firmly convinced and repeatedly claimed that "every theorem of algebra and higher analysis, no matter how remote, can be expressed as a theorem about natural numbers."[2] If we, for the time being, leave aside the question of Dirichlet's intent with this claim and focus on the technical side, we can see that a first step toward the fulfilment of his "demand" had already been taken in the 1820s, when Cauchy succeeded in defining the basic concepts of

1 Concerning Dirichlet's influence, Howard Stein writes, "it is not too much to characterize Dirichlet's influence not only upon those who had direct contact with him . . . but upon a later generation of mathematicians, as a spiritual one (the German geistig would do better)." [Stein 1988, 241]

2 Dirichlet's remark is reported by Dedekind in the preface to his 1888 essay. See Dedekind 1888, 792.

analysis--continuity, differentiability, and series sum--in terms of the concept of limit, which itself is formulated in the language of algebra, i.e., a general science of *quantities*.[3]

Cauchy's proposal, however, fell short of full arithmetization insofar as his theory of limits relied ultimately upon an intuitive geometrical notion of the continuum or the real number system: the existence of limits asserted in its various principles could not be established without appeal to the intuitive notion. It seemed, then, that what was required to complete the arithmetization of analysis was to establish the arithmetizability of the real number system itself: that the real number system can be expressed solely in terms of the natural numbers, their interrelations, and (number theoretic) operations on them.[4] This challenge was taken up by some of the greatest minds of the day; as a result, different but equivalent ways of constructing real numbers were

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[3] By this I do *not mean* to say that Cauchy introduced these definitions in order to "arithmetize" analysis. For Cauchy's motive, see Kitcher 1984, 246-250. Cauchy's definition of "limit" reads:

> When the values successively attributed to the same variable approach indefinitely a fixed value, eventually differing from it by as little as one could wish, that fixed value is called the *limit* of all the others. [Cauchy 1821, 19, translated in Birkhoff 1973, 2]

Accordingly, Cauchy's definition of "continuity," for instance, may be paraphrased as follows:

> f is continuous on $[a, b]$ if and only if $|f(x + h) - f(x)|$ tends to 0 as h tends to 0.[Kitcher 1984, 246]

[4] Admittedly, the above is a very crude description of what is unquestionably one of the most interesting and important episodes in the history of mathematics. For more on the development of analysis, see Philip Kitcher's excellent account in Kitcher 1984, 229-71.

put forward before the turn of the century.[5]

Dedekind, a "student" and colleague of Dirichlet's at the University of Göttingen,[6] arrived at his definition of real numbers in 1858 and published it fourteen years later in the little monograph, *Continuity and Irrational Numbers*. In it, Dedekind says that a real number is defined *by* (not *as*)[7] a separation or a *cut* (A_1, A_2) of the (ordered) set of rational numbers into two non-empty subsets, A_1 and A_2, in such a way that (1) every rational number is either in A_1 or in A_2, (2) every element of A_1 is less than every element of A_2. As Dedekind himself reveals, he arrived at this definition through his insight that the essence of our intuitive, geometrical understanding of "continuity" is captured in the principle: that if all points of a straight line are divided into two classes in such a way that every point in the first class lies to the left of every point in the second class, then there always exists one and only one point of the line which produces this separation of the line into the two classes. Assuming the correctness of this principle, Dedekind thus tried to carry out the desired arithmetization of the notion of the real number by "translating" the geometrical statement into what he considered to be the language of number theory. He then proceeded to

[5] With regard to this effort, the names of Weierstrass and Cantor, in addition to that of Dedekind, must be mentioned.

[6] When in 1855 Dirichlet arrived from Berlin to succeed Gauss's professorship in Göttingen, Dedekind, who had become a member of the faculty a year before (the year in which Riemann too was habilitated), started to attend Dirichlet's lectures in number theory, differential equations, and the theory of definite integrals. For more on the historical background, see Ewald 1996, 753-54.

[7] According to Dedekind, while a real number can be regarded as "completely defined" by a cut, it is not to be identified with a cut itself: by means of such a definition, we "create" a "new" number which "corresponds to" or "produces" a cut [Dedekind 1872, 773]. I will come back to this point shortly.

define the ordering of the reals as well as operations on them in purely arithmetical expressions; finally, he established the continuity of the "new domain" of the real numbers and also demonstrated, by referring to this new domain only, and thus without appealing to intuitive geometrical evidence, some fundamental theorems of analysis, which are concerned with the existence of limits.

Now, in connection with Dirichlet's remark about the arithmetizability of analysis, two things might be mentioned concerning Dedekind's definition of the real number. First, in the 1872 essay, Dedekind defined the real number system in terms of the *rational* numbers and simply assumed the definability of the latter in terms of the natural numbers. Second, and more importantly, in Dedekind's construction of the real number system, the notion of what we now call "set," together with various set theoretical operations, is employed in an essential manner. The question arises, then, whether his definition is purely "arithmetical"--whether it might not contain an element foreign to arithmetic or the theory of the natural numbers.[8] Dedekind's reply to the question could be found in his other foundational essay, *Was sind und was sollen die Zahlen*, published sixteen years after *Continuity and Irrational Numbers*. To put it simply, it was a logicist one: that arithmetic is a part of logic, and that the set theoretical principles required for the definition of the real numbers are available as the laws of logic.

A proposal for a radically different way of arithmetizing analysis was put forward by Kronecker, Dirichlet's student from his Berlin period. As we saw above, in view of Cauchy's (both

8 Dedekind himself considered a cut as a "purely arithmetical phenomenon" [eine rein arithmetische Erscheinung]. See Dedekind 1888, 793.

explicit and implicit) appeal to the intuitive geometrical notion of the continuum in justifying his theory of limits, various ways of constructing the real number system were proposed; and Dedekind's construction exemplifies one such proposal. Kronecker, however, was simply dismissive about all these attempts "to grasp and to give foundations to the concept of 'irrational' in all its generality" [Kronecker 1886, 156]. But, given that, in Dedekind's definition, the real number system is constructed from the natural numbers in a purely arithmetical fashion, what would be Kronecker's reason for dismissing such an attempt altogether?[9] To put it simply, his reason is that Dedekind's construction, just as other "arithmetization" attempts, makes essential use of completed infinities or infinite totalities. But why, then, does he object to the use of completed infinities in the definition of a concept?

Kronecker's answer to this question is that it might make the concept *undecidable*. That is, once completed infinities are employed in the definition of a concept, there might be no effective method for telling whether a given object falls under the concept so defined. In the Dedekindian definition, a real number is defined by a cut of rationals, or equivalently by a "left set," i.e., one of the two infinite (sub)sets of rationals generated by the cut. It would seem, then, that, in general, there is no effective method for deciding whether a given object falls under the concept of real number so defined-

[9] In this connection, the following episode might be indicative of Kronecker's attitude toward Dedekind's attempt: when in 1880 Kronecker nominated Dedekind for membership in the Berlin Academy, he submitted a report which described Dedekind's work in detail, but did not bother to mention the latter's *Continuity and Irrational Numbers*. For more on this, see Ewald 1996, 942.

-whether an infinite set of rationals has all the properties listed in the definition.[10]

Accordingly, Kronecker maintained that the arithmetization of analysis must be effected without involving undecidable concept-formations and hence without the use of completed infinities.[11] Furthermore, as the following passage from the 1887 essay "On the Concept of Number" clearly indicates, it was his contention that such a "strict" arithmetization of analysis was not only theoretically necessary but also technically *realizable*:

> ... The word 'arithmetic' is here not to be understood in the usual restricted sense, but rather as including all mathematical disciplines with the exception of geometry and mechanics--especially, therefore, algebra and analysis. And I also believe that we shall one day succeed in 'arithmetizing' the entire content of all these mathematical disciplines--that is, in grounding them solely on the number-concept taken in its narrowest sense, and thus in casting off the modification and extensions of this concept, which were mostly occasioned by the application to geometry and mechanics. [Kronecker 1887, 949][12]

In particular, in the 1887 essay, he provided an effective method for constructing from the natural numbers, the integers,

10 Similarly, Kronecker objected to Weierstrass's definition of the irrational numbers on the ground that there is no decision procedure for telling whether an infinite set characterizing a sequence defines an irrational or not. See Webb 1980, 73 and Mancosu 1998, 155. In this connection, Kronecker's rejection of the Cantorian set-theoretical mathematics and the applications deriving from it is also often mentioned, while the "myth" of his personal attack on Cantor has recently been questioned by Edwards. See Edwards 1995.

11 As for the reason why Kronecker considers undecidable concepts to be inadmissible, see below.

12 In a footnote attached to the above passage, Kronecker remarks that, by "the modifications and extensions of this concept," he means "especially the addition of irrational and continuous quantities" [Kronecker 1887, 949].

rationals, and algebraic reals,[13] on the basis of which (at least parts of) analysis could be re-obtained.[14] Even so, at the time of the publication of his *Lectures on the Theory of Numbers* in 1901, full continuity seems to have remained beyond the reach of his strict arithmetization program, as he writes in the introductory chapter that "from the entire domain of this branch of mathematics [i.e. analysis], only the concept of limit or bound has thus far remained alien to number theory" [Kronecker 1901, vol.1, 4-5].[15]

§2. Now, given that what essentially distinguishes Kronecker's arithmetization of analysis from Dedekind's (and others') is its insistence on the decidability of the concepts employed in the attempt, and that this methodological restriction is precisely what makes the desired goal of arithmetization much more difficult, if not impossible, for

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[13] An algebraic number is any number x, real or complex, that satisfies some algebraic equation of the form:

$$a_n x^n + a_{n-1} x^{n-1} + \ . \ . \ . \ a_1 x + a_0 = 0 \qquad (n \geq 1,\ a_n \neq 0)$$

where the a_k are integers. For example $\sqrt{2}$ is an algebraic (real) number, since it satisfies the equation:

$$x^2 - 2 = 0.$$

See Courant & Robbins 1941, 103.

[14] As has been pointed out by some commentators, Kronecker's program has recently been "revived" in spirit in the investigation in Reverse Mathematics: they have shown that a good deal of analysis and algebra can be done in conservative extensions of primitive recursive arithmetic. See Sieg 1990 and Marion 1995.

[15] Translated and quoted in Stein 1988, 257. Stein offers a stronger reading of the passage; according to him, the last phrase "the concept of limit ... has remained *alien to number theory*" must be taken "to mean *irreducible to the finitary theory of the natural numbers*--which reducibility is what Kronecker's constructive program aimed at" [ibid., 257-58].

Kronecker, we naturally wonder why he is so emphatic about imposing this condition.[16] In addition, we are not alone in wondering about Kronecker's motive. Dedekind, who was made aware, in a different context,[17] of Kronecker's criticism against his methodological procedure, decided to reply to it in the 1888 essay. According to Dedekind, in setting a veto on the use of undecidable concepts, "Kronecker ... has endeavoured to impose certain limitations upon the free formation of concepts in mathematics which I do not believe to be justified" [Dedekind 1888, 797]. A concept, thinks Dedekind, is "completely determined" when it is determined whether an object does or does not fall under it.[18] Moreover, for him, whether an object does or does not fall under a concept is determined independently of our knowledge. That is, it is a matter of indifference for this determination in what manner it is brought about, and whether we are in possession of a procedure for deciding upon it. It would follow that, in Dedekind's view, a concept could be completely determined whether or not there is such a decision procedure available to us: the absence of the decision procedure has no bearing upon whether a concept is completely determined or not. He thus cannot agree

[16] It is to be noted that Kronecker arrived at this view *before* the "discovery" of set-theoretic paradoxes.

[17] In his 1886 essay "Über einige Anwendungen der Modulsysteme auf elementare algebraische Fragen," Kronecker criticized Dedekind's introduction of the concepts of "module," "ideal," etc. for their undecidability. Incidentally, it is also on this occasion that Kronecker expressed his disagreement with "the introduction of various new concepts, which have been used in many recent attempts (first of all by Heine) to grasp and to give foundations to the concept of 'irrational' in all its generality" [Kronecker 1886, 156, translated and quoted in Marion 1995, 192]. See also Stein 1988, 251.

[18] In the 1888 essay, a "system," instead of a concept, is used to make this point: a system or a set *S* is "completely determined when, for every thing, it is determined whether it is an element of *S* or not" [Dedekind 1888, 797].

with Kronecker that a concept must be decidable. What is crucial in mathematics, according to Dedekind's methodological standpoint, is the determinacy, not the decidability, of a concept.[19]

Given Dedekind's emphasis on the independence of mathematical "state of affairs" from decidability, it might seem that what is at stake here between the two is the *ontology* of mathematical objects: Dedekind, a supporter of realism or Platonism, thinks that the objects of mathematics exist independently of the mathematician, whereas Kronecker, an advocate of a sort of idealism, holds that what exists in mathematics is what can be constructed by the mathematician and only that.[20] The discrepancy between such an interpretation and the text, however, seems quite apparent when, for instance, it is noticed that the alleged "Platonist" proclaims in the preface to his 1888 essay that "numbers are free creations of the human mind" [Dedekind 1888, 791]; whereas the "quasi-idealist" seems to affirm the mind-independent existence, at least, of the natural numbers in his famous aphorism that "God created the integers; everything else is the work of man."[21]

What then explains the difference in the two mathematicians' attitudes toward the method of concept-formation and of the arithmetization of analysis? In the case

[19] This, however, does not mean that Dedekind sees no further requirements for concept-formations in mathematics. For more on this, see below.

[20] Ewald, for instance, seems to suggest that it is Kronecker's "strong opinions about mathematical ontology" that is the ultimate source of his intolerance for completed infinite collections and for non-constructive definitions. See [Ewald 1996, 942].

[21] Incidentally, these words do not appear anywhere in Kronecker's published writing. Its first occurrence in print is believed to be Heinrich Weber's obituary of the former published in 1893, according to which the remark in question was made by Kronecker in a lecture to the Berlin Naturforscher Versammlung in 1886.

of Kronecker, the answer seems to lie in his concern for the epistemological status of arithmetic in the broad sense. Immediately after the above quotation from the 1887 essay, where Kronecker expresses his hope for a strict arithmetization of analysis, we find him conclude the preface with the following remark:

> The difference in principle between geometry and mechanics on the one hand and the remaining mathematical disciplines (here gathered together under the term 'arithmetic') on the other is, according to Gauss, that the object of the latter, number, is *merely* our mind's product, while space as well as time also have *outside* of our mind a *reality*, whose law we cannot completely prescribe *a priori*. [Kronecker 1887, 949, emphasis in original][22]

As the footnote attached by Kronecker to this passage might indicate, what he presents here seems, on the surface, to be nothing more than a brief description of Gauss's view of mathematics, which was also the standard one among his contemporaries. However, a careful reading of the text would make us realize that it is much more than just a customary salute, as it were, toward his great predecessor: rather, it expresses a wholehearted aspiration on Kronecker's part to complete a task bequeathed by Gauss. More specifically, in explicitly classifying analysis (and algebra) under the title

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22 In a footnote, Kronecker quotes from Gauss's letter to Bessel of 9 April 1830, which reads:

> It is my deepest conviction that the theory of space has a completely different position in our *a priori* knowledge than does the pure theory of quantity. Our knowledge of the former utterly lacks the complete conviction of necessity (and also of absolute truth) that belongs to the latter; we must in humility grant that, although number is *merely* the product of our mind, space also possesses a reality outside of our mind, and that we cannot entirely prescribe its laws *a priori*. [(Quoted in) Kronecker 1887, 949]

of arithmetic and, at the same time, emphasizing the *a priori* character of arithmetic so considered (or at least by contrasting it to the *a posteriori* nature of geometry and mechanics), Kronecker is revealing his intention of providing a solution to an epistemological problem through the mathematical discussions developed in the subsequent sections of the essay. In a nutshell, his attempt to achieve strict arithmetization is designed to achieve the epistemological "grounding" of analysis.

For many mathematicians in the nineteenth century (including Gauss), the "discoveries" of (consistent) non-Euclidean geometries meant two things: first, geometrical propositions do not express necessary truths; second, their objective validity cannot be known or justified independently of experience. Some concluded from this that the (then) predominant Kantian doctrine of the pure intuition of space and time, which was supposed to account precisely for the (non-logical) necessity and aprioricity of geometrical truths, was mistaken and should, accordingly, be abandoned. This chain of events, however, put the epistemological status of analysis in limbo. As Cauchy showed, various basic concepts of analysis are definable in terms of the limit-concept, and this latter presupposed full continuity. But once the Kantian notion of the pure intuition of the continuum is rejected,[23] it would appear that the notion of continuity, which is essential to analysis, must come from perceptual intuition, as it seems

[23] See Friedman 1992, 71-80 for how Kant tried to "ground" analysis through his theory of construction in pure intuition. The calculus was traditionally considered as an extension of algebra, i.e., a general science of quantities: accordingly, Euler, for instance, rejected geometry as a basis for the calculus and tried to base it on arithmetic and algebra. For the historical background, see Kline 1980.

indeed to be the case with Cauchy's foundational studies, and therefore that analysis has an empirical, *a posteriori* element, just as geometry does.

Kronecker could not tolerate such a consequence. In his view, analysis, while it originated from geometry, "has been developed independently on its own ground," and cannot be demarcated from arithmetic as the theory of the natural numbers [Kronecker 1901, vol.1, 5].[24] Moreover, it is his (and Gauss's) invariable position that we have direct epistemic access to the objects of arithmetic and are capable entirely of prescribing its laws independently of experience. Thus, Kronecker attempts to establish the a prioricity and "absolute truth" of analysis by constructing it from arithmetic in the strict sense: with an epistemologically privileged class of objects as its basis, he attempts, by means of a stepwise introduction through constructive definitions, to show that all the higher-level concepts of analysis are theoretically dispensable, and hence that all the statements of analysis are translatable into those of number-theory, whose correctness is, in principle, knowable *a priori*. Now, in view of the epistemological nature of Kronecker's project, it is quite understandable why he opposes the use of completed infinities and undecidable concepts in general in the arithmetization of analysis. To quote Hilbert, the use of such a concept "makes statements possible whose correctness is not decidable in a finite number of operations" [Hilbert 1920b, 944]. In other words, Kronecker cannot accept the use of undecidable concepts because it makes the truth of

[24] Quoted and translated in Stein 1988, 243.

mathematical knowledge unverifiable.[25]

§3. Dedekind did not consider the main objective of his foundational investigations to be epistemological (at least in the sense of Kronecker). This much seems certain given the fact that he thought it quite unnecessary and, in fact, mistaken to impose, as Kronecker does, the restriction upon the concept-formation with regard to its decidability. But, on the other hand, it seems also to be the case that Dedekind, like Kronecker, engages in some sort of "reductionist" project when he attempts to characterize the real number system in the language of arithmetic in the celebrated *Continuity and Irrational Numbers*. In the preface to the first edition of his second foundational essay published sixteen years later, he describes his earlier project as follows:

> In my earlier memoir on continuity (1872) I have already shown (at any rate for the example of irrational numbers) how the step-by-step extension of the number-concept is

[25] An objection to the philosophical or epistemological interpretation of Kronecker's writings has recently been put forward by Mathieu Marion. According to Marion, Kronecker's lack of interest in philosophical or foundational questions should indicate that "his foundational stance was not dictated by some kind of misplaced philosophical thesis" [Marion 1995, 206]. Rather, the kernel of his polemic lies in his emphasis on the "algorithmic aspects of mathematics" as opposed to the non-algorithmic or "descriptivist" style exemplified in Dedekind's, or more recently in Bourbaki's approach. Marion's interpretation gives a coherent, unitary picture to sundry aspects of Kronecker's mathematical works and deserves a careful consideration. However, I will not do so here since my goal is to delineate the Kronecker/Dedekind dispute in order better to understand what is behind Hilbert's early foundational investigations and also to provide a "rational construction" of *Hilbert's* view of this controversy. As I shall argue later, and as the above quotation seems to indicate, Hilbert did think that Kronecker conceived and constructed his program essentially from the epistemological perspective. Moreover, it seems to me that even if Marion is correct in emphasizing the "algorithmist" style of Kronecker's standpoint, the question still remains why Kronecker insists on this. The reason seems to lie in his philosophical and epistemological concern for aprioricity and certainty.

> subsequently to be carried out--the creation of zero, of the negative, rational, irrational, and complex numbers--always by a reduction to earlier concepts, and indeed without any introduction of foreign concepts ....[Dedekind 1888, 792]

The obvious exegetical problem for us here is what, if not an epistemological endeavour, is the point of such a "reduction" for Dedekind. Indeed, this question becomes even more pressing when we realize that he recognizes no general methodological value or need in a reduction as such:

> I see nothing meritorious--and this was just as far from Dirichlet's thought[26]--in actually performing this wearisome circumlocution and insisting on the use and recognition of no other than natural numbers. [ibid., 792]

It would seem then that a successful interpretation of Dedekind's enterprise in the 1872 essay must be able to account for two things: first, its goal is not epistemological in the sense of justifying the truth and certainty of analysis; second, while Dedekind seems to employ reduction as a means to achieve this goal, he does not attach any general significance to reductionism as such.

In considering these points, I would like to begin by examining Dirichlet's claim about the arithmetization of analysis: "every theorem of algebra and higher analysis, no matter how remote, can be expressed as a theorem about natural numbers." Although the emphasis here is laid upon the reducibility of algebra and analysis to arithmetic, the claim can be taken to contain another sub-thesis, which is logically

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[26] Dedekind's report of Dirichlet's remark about the arithmetization of analysis occurs between the above quotation and this one.

independent of the reductionist one. That is, in claiming that theorems of analysis are *theorems* of number-theory, I think Dirichlet is also implying that analytical theorems can be demonstrated in "gap-free" proofs. To be sure, in Dedekind's formulation of Dirichlet's claim, analytical theorems are said to be deducible from *arithmetical* ones, but the demand for gap-free proofs does not in itself involve their reduction to arithmetical ones. Conversely, the expressibility of analytical statements in the language of arithmetic does not in itself call for their demonstration in gap-free proofs. In typical accounts of the development of analysis in the nineteenth century, the terms "arithmetization" and "rigorization" and their cognates are often used virtually interchangeably, as was the case in the works of the analysts themselves. Despite such a common practice, however, it is important to see that the demands for these two ideals do not imply each other and can be carried out independently. In fact, among the mathematicians of Dedekind's day were some exceptions who were aware of these points and who emphasized the importance of the distinction.

Here I quote a passage from the lecture Felix Klein delivered to the Royal Academy of Sciences of Göttingen in 1895, which was entitled "*Über Arithmetisierung der Mathematik*":

> ... since I consider that the essential point [of the arithmetizing of mathematics] is not the mere putting of the argument into the arithmetical form, but the more rigid logic obtained by means of this form, it seems to me desirable ... to subject the remaining divisions of mathematics to a fresh investigation based on the arithmetical foundation of analysis. [Klein 1895, 967]

Klein here clearly recognizes that what has been brought about by the recent attempts of arithmetizing analysis can be separated into two components--"putting of the argument into the arithmetical form" and "the ... rigid logic obtained by this form"--and that the latter moment is applicable to the fields which do not deal with numbers in the strict sense. To be sure, Klein here seems to be saying that such a "rigid logic" is to be obtained upon "the arithmetical foundation of analysis" and hence that rigorization *requires* a translation into, or reduction to, arithmetic. But, when actually discussing the execution of rigorization in the domain of geometry, he explains that

> ... this might very well be done, as it was originally, on purely geometrical lines; but in practice on account of the overwhelming complications that present themselves, recourse must be had to the processes of analysis, that is to the methods of analytical geometry. [Ibid., 967]

In other words, on Klein's view, it is not in the logical, but merely in the practical, sense that a translation into the language of arithmetic is required for the rigorization of geometry.[27]

If we now go back to Dedekind's project in the 1872 essay, we also notice that while there Dedekind attempts to characterize the concept of the real number in the language of

[27] As we shall see in the next chapter, Hilbert vigorously argued for the possibility and necessity of rigorization in the fields of geometry and other non-arithmetical (and non-mathematical) disciplines. Incidentally, Hilbert was offered a professorship at the University of Göttingen in 1895 (due partially to a strong recommendation from Klein) and was there by the March of the same year. Since Klein's lecture was delivered on 2 November 1895, it is most probable that Hilbert heard the lecture. For more on Hilbert's appointment at Göttingen, see Reid 1996, 45ff.

arithmetic and, consequently, opens a way to reducing analysis to the theory of numbers, he does not consider such a reduction or arithmetization itself to be the main objective of his project. His primary goal there, rather, is to rigorize analysis, and arithmetization is used as a means for rigorization. This point is made quite clear at the very beginning of the essay, where he relates to the reader the course of events and the train of thought that led him to the foundational issues:

> The considerations which form the subject of this pamphlet date from the autumn of 1858. I was then professor in the Polytechnic School in Zurich, and I found myself for the first time obliged to lecture upon the elements of the differential calculus; I felt more keenly than ever before the lack of a truly scientific foundation for arithmetic. In discussing the concept of the approach of a variable magnitude to a fixed limiting value--in particular, in proving the theorem that every magnitude which grows continually, but not beyond all limits, must certainly approach a limiting value--I took refuge in geometrical evidence. Even now I regard such invocation of geometric intuition [*Anschauung*] in a first presentation of the differential calculus as exceedingly useful from a pedagogic standpoint, and indeed it is indispensable, if one does not wish to lose too much time. But no one will deny that this form of introduction into the differential calculus can make no claim to being scientific. For myself this feeling of dissatisfaction was so overpowering that I resolved to meditate on the question until I should find a purely arithmetical and perfectly rigorous foundation [Begründung] for the principles of infinitesimal analysis. [Dedekind 1888, 767][28]

Here Dedekind talks, in general terms, of the lack of a "truly scientific" foundation for arithmetic and of the need to replace the customary appeal to geometrical intuition with a

[28] Note the similarity in tone between this passage and Hilbert's 1900 paper, in which he emphasizes the preferability of the axiomatic over the genetic method for the complete logical grounding of our knowledge.

"purely arithmetical" and "perfectly rigorous" grounding of this discipline. But in order better to understand his point in the passage, special attention must be paid to the remark that, in proving a certain fundamental theorem of analysis, he took refuge in geometrical evidence, but that such a procedure can in no way be considered "scientific." With regard to Dedekind's meaning of "scientific," we can find a similar use of the term in the opening remark of the 1888 essay:

> In science nothing capable of proof ought to be believed without proof. [Dedekind 1888, 790][29]

In a "scientific" theory, or in the construction of one, no proposition that can be inferentially justified should be accepted unless it is given a proof. It would seem, then, that the overpowering feeling of dissatisfaction which drove Dedekind to foundational investigations stemmed from the fact that he had to accept a proposition *which is capable of proof* on the basis of geometrical evidence rather than proof. To amend such an "unscientific" state of analysis, he set himself the task of finding a foundation from which the fundamental

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[29] Dedekind continues the passage with the following words:

> Though this demand seems reasonable, I cannot regard it as having been met even in the most recent methods of laying the foundations of the simplest science; viz., that part of logic which deals with the theory of numbers.

What is of interest for our purpose is the fact that, in a footnote attached to this passage, Dedekind cites as examples of recent works on the topic Kronecker's 1887 essay, along with Helmholtz's 1887 essay on counting and measuring and E. Schröder's *Lehrbuch der Arithmetik und Algebra* (1873), and writes that "the appearance of these memoirs has induced me to publish my own views" [Dedekind 1888, 790]. In other words, Dedekind sees these works as lacking in rigor in the sense that they accept what is capable of proof unproved. I shall come back to this point in connection with Hilbert's criticism of Kronecker.

theorems of analysis can be proved. Thus, when he later actually embarks on the project, he describes his task in the following terms:

> the problem is to indicate a precise characteristic of continuity that *can serve as the basis for valid deductions*. [Dedekind 1872, 771, my emphasis]

In other words, it is important for Dedekind not so much to capture the notion of continuity in a vocabulary of arithmetic as to characterize it in such a way that the fundamental theorems of analysis could be deduced from it. Such a characterization, once found, would provide a "truly *scientific*" and "perfectly *rigorous*" foundation for analysis because it would make valid deductions possible: those fundamental theorems of analysis, which previously had to be accepted without proof, would be given an inferential justification in a "gap-free" proof.

This point can be further elaborated against the background of the technical issues involved. As we saw above, in the preface to the 1872 essay, Dedekind refers specifically to a theorem of analysis: that if a magnitude grows continually but not beyond all its limits, then it approaches a limiting value. This theorem, or any one equivalent to it, he writes later, can form "a more or less sufficient foundation for infinitesimal analysis" [Dedekind 1888, 767]; accordingly, it, together with other limit existence theorems Cauchy accentuated, plays a crucial part in the development of this discipline. What is to be recognized here is that, by the mid nineteenth century, i.e., by the time of Dedekind's composition of *Continuity and Irrational Numbers*, precisely these fundamental theorems of analysis came to appear as problematic to the eyes of mathematicians. I said earlier that Cauchy's attempt in the 1820s to formulate the basic concepts of analysis in terms of the algebraic (quantitative) notion of limit marked a first step toward the arithmetization of analysis. Even so, it fell short of full arithmetization because, for some reason, Cauchy did not try to establish theses about convergence, continuity, and differentiability (to wit, theorems asserting the existence of limits) in a rigorous manner. Instead he implicitly and explicitly appealed to geometrical evidence for their acceptance.[30] Initially, Cauchy's eclectic, unrigorous procedure was considered acceptable because of the presumed isomorphism between analytic (algebraic or quantitative) and intuitive (geometrical) notion of continuity; in fact, the applicability of these theorems to the intuitive domain was thought to confirm the correctness of Cauchy's algebraically formulated principles. But as the research progressed, the disparity between the two became more and more conspicuous, and "mathematicians became more wary of the traditional geometrical reasonings to which Cauchy had helped himself when the algebra became difficult" [Kitcher 1984, 262]. Consequently, there arose amongst mathematicians

[30] For instance, Cauchy "proves" the Intermediate Zero Theorem by appealing to a geometrical representation of a continuous curve. His "proof" begins with the assumption that a continuous function $\mathbf{f}$ can be represented by a continuous curve. Given that $\mathbf{f}(a) < 0 < \mathbf{f}(b)$, geometrical considerations show that there is a c such that $a < c < b$ and $\mathbf{f}(c) = 0$: a continuous curve must cross the axis between a and b. This argument is fallacious for the content of Cauchy's algebraically defined notion of continuity cannot be exhausted by the geometrical representation of continuous curves: there are functions satisfying Cauchy's criterion for continuity which cannot be represented by continuous curves. This point can be clearly shown, for instance, by the existence of everywhere continuous nowhere differentiable functions, the first example of which was given by Bolzano in 1834 and which were later made famous by Weierstrass's investigation in 1872 [Boyer 1949, 269-270].

the demands for the rigorization of analysis and, in particular, for the "gap-free" proof of the limit existence theorems.[31]

Dedekind's attempt in the 1872 essay was designed to do just this, to *prove* the fundamental theorems of analysis in a rigorous manner.[32] And his solution to this problem? To put it simply, he found his solution when he hit upon the idea that the desired basis for valid deductions is a "purely arithmetical" foundation. More specifically, he tried to solve the problem by characterizing the notion of the real numbers in such a manner that there are real numbers satisfying the conditions laid down in those limit existence theorems. What is to be noticed here, however, is this: his primary goal being to provide a gap-free proof for the fundamental theorems of analysis, he *could as well have* argued for a non-arithmetical basis.[33] To put this point in a modern manner, the adoption of arithmetic as the linguistic framework for the rigorization of analysis, and indeed the translation of its sentences into any other language, is not implied by the nature of his task, and that this explains his negative attitude toward reductionism in general.

§4. Against this interpretation, however, it might be argued that while not being motivated by epistemological

[31] Thus, when Dedekind talks, in the preface to the 1872 essay, of the feeling of dissatisfaction over his inability to establish the fundamental theorems of analysis, it could well be seen as representing the general atmosphere surrounding the mathematical community of the time.

[32] It is therefore no mere accident that Dedekind ends the 1872 essay with the attempted proofs for the limit existence theorem mentioned in the preface and another one equivalent to it. This point is duly observed in Kitcher 1984, 263.

[33] As Kitcher points out, Bolzano, for instance, argued for an *algebraic* version of analysis [Kitcher 1984, 263-264].

concerns in the exalted sense of Kronecker, Dedekind has a different reason to wish to carry out a reduction of analysis to arithmetic. More specifically, it might be said that what he is after in the 1872 essay is to establish the *uniformity* of analysis and arithmetic. For him, analysis is a form of knowledge which is concerned with numbers and constitutes a part of arithmetic, the science of numbers. This fact, however, is obscured by the presence of non-arithmetical, non-numerical notions such as extensive quantity in the foundations of analysis. As a result, it might even be thought that analysis depends upon a type of knowledge other than arithmetic. Thus, Dedekind attempts to establish the monolithic nature of analysis and arithmetic by translating the heterogeneous mixture of analytic vocabulary uniformly into the language of number. Indeed, the fact that Dedekind sees a certain special connection holding between the two disciplines is quite apparent. After remarking on the foundational character of the limit existence theorem cited in the preface for the other propositions of analysis, Dedekind writes that his goal is "to discover its *true origin in the elements of arithmetic*" [Dedekind 1872, 767, my emphasis]. Furthermore, as can be seen in the following passage in the third section of the 1872 essay, Dedekind appears at least occasionally to affirm the *necessity* of arithmetization (i.e., the reduction of analysis to arithmetic) while stressing the uniformity of the two disciplines:

> It may in a general way be granted that such comparisons with non-arithmetical notions have furnished the immediate occasion for the extensions of the number-concept ... ; but this is certainly no reason for introducing these foreign notions into arithmetic itself, the science of

> numbers. Just as negative and fractional rational numbers are formed by a free creation, and just as the laws of operating with these numbers must and can be reduced to the laws of operating with positive integers, so *we must strive to give a complete definition of the irrational numbers using the rational numbers alone.* [Dedekind 1872, 771, my emphasis]

Despite such an appearance, however, I think it is a mistake to claim along the lines of the above interpretation that, by reducing analytical to arithmetical propositions, Dedekind wants to show that analysis, as a part of arithmetic, has numbers as its proper objects, and that for him the reduction must, therefore, be to arithmetic. First and foremost, as we just saw, on this account the reduction of analysis to arithmetic would be necessary for the realization of Dedekind's goal. But if this were really the case, it would mean that, in denying the methodological importance of a reduction elsewhere, Dedekind is confused about his own standpoint.

Second, it is to be noted that Dedekind, unlike Kronecker, never states in unambiguous terms that the proper object of analysis is numbers in the strict sense. This would be rather a strange thing if his primary goal were to establish that analysis deals with numbers. From antiquity (e.g., Aristotle, Euclid) through the late eighteen and early nineteenth centuries (e.g., Kant, Gauss), it was commonly thought that there are two distinct kinds of "quantity"--the discrete and continuous--and that they are mathematically represented by the theories of *number* and of *continuous magnitude* respectively.[34] In fact, even at the time of Dedekind's composition of the essay on continuity, it was still a prevalent view that

[34] Stein 1988, 242.

analysis is concerned with continuous magnitude.[35] Thus, if Dedekind thinks that number is the authentic object of analysis and wanted to demonstrate this fact, the 1872 essay would be *the* place to express this against the prevalent view of the time. Certainly, he is opposed to the introduction of non-arithmetical notions such as extensive quantity into analysis and does not hesitate to make this known. In the preface to the 1888 essay, he states that "I wholly reject the introduction of measurable quantities,"[36] and refers the reader to the third section of the 1872 essay, where, he says, some reasons for this rejection are advanced. It turns out, however, that his criticism of non-arithmetical notions put forward there is not directed toward the foundational view held by many then contemporary mathematicians, namely that analysis deals with continuous magnitude. If, as the proposed account claims, his goal was to establish that analysis is concerned with numbers, the introduction of non-arithmetical notions would be rejected for the reason that these notions do not pertain to numbers and thus fall within "foreign" disciplines.[37]

Third, it is to be recalled that, in presenting his (arithmetico-set-theoretical) characterization of the real numbers, Dedekind emphasized that while a real number is completely defined by a "cut," it is not to be identified with the cut itself. Rather, according to him, by means of such a

[35] In the preface to the 1872 essay, Dedekind writes, "[t]he statement is frequently made that the differential calculus deals with continuous quantities, yet an explanation of this continuity is nowhere given" [Dedekind 1872, 767].

[36] See Dedekind 1888, 793. Dedekind's statement in the 1888 essay was a response to Dini's remark to the effect that the former's theory of the real numbers rests upon the notion of measurable quantity.

[37] I shall consider Dedekind's criticism against the introduction of non-arithmetical concepts in more detail later.

definition, we "create" a "new" number which corresponds to, or produces, a cut [Dedekind 1872, 773]. This talk of "creation," however, would be quite unintelligible if Dedekind's primary goal were to show that the proper objects of analysis are (the natural) numbers, insofar as such newly "created" real numbers would be (ontologically) distinct from the natural numbers.[38]

§5. But how then can we understand these aspects of Dedekind's foundational view which are inexplicable in terms of reductionism? In my opinion, these points can only be given a satisfactory account when we recognize that Dedekind's primary goal is not reduction, but rather rigorization. Let us first consider his rejection of the introduction of non-arithmetical notions into a scientific construction of analysis. As we just saw, Dedekind's discussion turns to the issue of "foreign" elements in the context where he comments on the (then) customary expositions of analysis. Correspondingly, his criticism against the introduction of non-arithmetical elements is framed in terms of the notion of extensive or measurable quantities, which is often employed in such expositions.[39] More specifically, in them, the concept of the real number is defined, with a direct reference to the (intuitive) notion of

[38] Apparently, the same point applies to the other types of numbers. In the above quotation, Dedekind says that while the calculational rules for negative and rational numbers are "reduced" to those for positive integers, negative and rational numbers themselves are "formed by a free creation." And thus, it would seem that they are thought to be distinct from positive integers. For more on this, see below.

[39] Thus, in §3 of the 1872 essay, after having described the customary characterization of the irrational numbers in terms of "the conception of extensive magnitudes," he immediately continues that "... such comparisons with *non-arithmetical* notions have furnished the immediate occasion for the extension of the number-concept ... but this is certainly no reason for introducing these *foreign* notions into arithmetic itself, the science of numbers [Dedekind 1872, 771, my emphasis].

extensive quantity, as the ratio of two such quantities of the same kind. Apparently, the idea behind this definition is that every number, including those which cannot be represented as ratios of whole numbers, is representable as a ratio between similar quantities, e.g., line segments, planes, volumes, etc., which, presumably, have the property of continuity. In other words, this definition attempts to "construct" the continuous number-domain by means of *something continuous* (without clearly explaining wherein this continuity consists). In so doing, it is committed to two tacit assumptions: 1) that extensive quantities exist (or we possess intuitive representations of them); 2) that they are continuous.

As Dedekind sees it, the problem, however, is that, granting the real existence of such quantities (or of their representations in us), there is no guarantee that they are, in fact, continuous. To illustrate this, he chooses particularly to consider (the representation of) space, which is commonly thought inconceivable to be anything else than continuous. His argument is simple and right to the point. Contrary to the common belief in the full continuity of space, all that we know of space through Euclidean geometry does not entail its continuity: it is logically possible that the theorems of Euclidean geometry be all true; and space be discontinuous.[40] To put it in modern terms, Dedekind presents here an informal proof to the effect that Euclidean geometry has a *model* in which all ratios of length of straight segments are algebraic

[40] It appears, then, that, for Dedekind, Euclidean geometry *is* the science of space.

numbers.[41] Now, given that this argument succeeds in showing that extensive quantity (or its intuitive representation) might lack the property of continuity, it would follow that the proposed definition of the real numbers might also lack the relevant properties. This definition, as we saw, draws directly on the presumed continuity of such quantity. Thus, if extensive quantity fails to be continuous, then the domain consisting of the real numbers so defined would also fail to be continuous.

But why does Dedekind think that such a failure constitutes a ground for rejecting the introduction of the notion of extensive quantity? To answer this, we have only to recall how Dedekind has come to what is understood here by the notion or the principle of continuity: Dedekind formulated it precisely in such a manner that it "can serve as the basis for valid deductions" of the fundamental theorems of analysis (asserting the existence of limits). Thus, to fail to provide a number-domain possessing the property of continuity, in his view, is to fail to provide such a deductive basis. Insofar as Dedekind's primary goal in the 1872 essay is to provide a gap-free proof for those theorems, a definition failing to fulfil its intended function must be rejected. In other words, Dedekind's interest in rigour leads him to reject the introduction of non-arithmetical notions such as extensive magnitude into the concept-formation in analysis.

Yet, it must be recognized at the same time that when Dedekind argues that space might not have the property of full continuity and, thus, that the desired characterization of the

[41] To be more precise, Dedekind argues that the constructive procedures found in Euclidean geometry (construction with straight-edge and compass) could be carried out in such a *discontinuous* space and, therefore, that full continuity is not required for their successful execution.

real numbers might not be obtained by defining them in terms of extensive quantity, he does *not* mean to say that such a characterization might not be formulated in a theory within which the notion of extensive quantity falls, or in a "language" whose vocabulary includes, among others, the term "extensive quantity."[42] That is, in so saying, his point is *not* that (the language of) a non-arithmetical theory such as geometry or algebra (as a general science of quantity) is incapable of providing a framework for the rigorization of analysis. His point is rather that if we are to succeed in proving the fundamental theorems of analysis, intuitive elements should not enter the process of concept-formation (or of inference). What Dedekind is really opposing in his criticism against non-arithmetical notions is not the introduction of non-arithmetical (e.g., geometric or algebraic) vocabulary into the language of analysis but, rather, the intrusion of an intuitive element into a system of language.[43] Dedekind himself, of course, did not have a methodological language to convey his meaning in this manner. But that he understands the notion of "non-arithmetical" or "foreign" from such a perspective can be confirmed by the fact that he occasionally contrasts it with the notion of "conceptual" or "logical," rather than with that of "numerical." Toward the end of his discussion of measurable quantities in the preface

[42] Once again, it is to be noted that Dedekind himself would not put the point in this manner. What is presented here and in what follows is meant to be a rational reconstruction in order to make explicit what is involved in Dedekind's reasoning.

[43] Accordingly, it is my contention that Dedekind's emphasis upon "uniformity" and "purity" is to be understood in the sense that definition should be free from an extra-linguistic or extra-theoretical element.

to the 1888 essay, Dedekind writes:[44]

> ... without any notion of measurable quantities and simply *by a finite system of simple steps of thought*, man can advance to the creation of the pure continuous number-domain; and only by this means is it in my opinion possible for him to render the notion of continuous space clear and definite. [Dedekind 1888, 793-794, my emphasis]

Given that "the pure continuous number-domain" is what provides a basis on the ground of which the fundamental theorems of analysis can be given a proof, I take his point in (the first half of) this statement to amount to this: that, despite the common belief that it takes an appeal to extra-theoretical, intuitive elements such as measurable quantities to formulate the characterization of continuity, we *can* actually "construct" the desired (deductive or inferential) basis without any appeal to intuition through a stepwise procedure of (logical) thinking. As a matter of fact, the second half of the quotation seems to suggest that, in Dedekind's view, a characterization of continuity that can serve as a basis for valid deductions can be obtained *only* by means of thought, and thus *never* through an appeal to intuition.[45] Thus, in opposing the introduction of non-arithmetical elements into analysis, Dedekind is not denying the possibility of a non-arithmetical version of analysis. This stems from the circumstance that the main goal of his project, i.e., rigorization, does not necessitate the adoption

44 Note, once again, that, both in §3 of the 1872 essay and in the preface to the 1888 essay, Dedekind uses the term "measurable quantity" all but interchangeably with the term "non-arithmetical" or "foreign" notion.

45 I shall explain shortly the reason why Dedekind thinks such a characterization can make the notion of continuous *space* clear and definite.

of arithmetic as a framework for the theory-construction.

§6. On the other hand, in Dedekind's view, certain requirements are to be imposed upon the process of concept-formation in consequence of his demand that the basic concepts of a theory be formulated so as to be able to function as a basis for the valid deductions of its fundamental theorems. In a long footnote attached to his 1877 essay, Dedekind lists three items to be met when introducing new arithmetical elements: 1) "arithmetic must be kept pure of any mixture of foreign elements"; 2) all such elements must be "generated at the same time from one common definition"; 3) the definition must be "of such a nature as to permit as well a perfectly clear definition of the calculations (addition, etc.) that one will perform on the new numbers"[46] [Dedekind 1877, 784]. In contrast to Kronecker's emphasis upon the decidability of concept-formation, Dedekind's thinking on this matter is not motivated by any epistemological concerns (in the exalted sense): rather, as can be clearly seen in the second and third items, the requirements imposed by Dedekind are directly related to his interests in the rigour and systematicity of theory-construction; the demand for the "purity" of arithmetic, as we just saw, is made because of his belief that a definition based upon a foreign (intuitive) element is incapable of formulating a characterization required for valid

46 Dedekind thus criticizes the customary definition of the real number in terms of extensive quantity on this ground as well: that, on such a definition, we would not be able to formulate a clear definition of the operations on them so that even a simple theorem such as $\sqrt{2} \cdot \sqrt{3} = \sqrt{6}$ could not be demonstrated in a rigorous manner. See Dedekind 1888, 794 and Dedekind 1877, 784.

deductions.[47]

In addition, Dedekind's demand upon definition has the following consequence with regard to the nature of a concept defined. If the basic concepts of a theory, as he insists, are formulated in such a way that they can collectively provide a basis for the valid deductions of its theorems, this would, of course, mean that the theorems can be deduced, solely in accordance with logic as the laws of thought, from a set of sentences (including those) which are generable from basic concepts. But, since the process of inference (and of the generation of sentences from concepts) is independent in all its parts from the content of the non-logical terms of the theory, it follows that the basic concepts possess certain *formal* or *relational* properties which make the valid deductions of theorems possible. This, in turn, means that while the non-logical terms used to define the basic concepts may have certain theory-independent referents (because of their ordinary meaning), they need not be considered to be concerned particularly with these objects. These concepts are applicable to any domain that exhibits the relational properties expressed in them, and the theorems of the theory, which are "deducible" from them, would, under an appropriate interpretation, all express truths about the objects constituting that domain. This, then, is the reason why Dedekind thinks that his characterization of continuity can provide a "scientific basis for the investigations of *all* continuous domains"[48] and render

[47] Dedekind later comes to the view that definition must not only be able to serve as a basis for valid deductions, but also be a deductive basis harbouring no internal contradictions. For more on this, see below.

[48] Dedekind 1872, 771, emphasis in original. Thus, for Dedekind, it is not, as for Frege, the contentual generality of numbers (or of the logical objects to which they are to be reduced) that explains the applicability of analytical (and, probably, arithmetical) principles.

the notion of continuous *space* clear and definite.[49]

Furthermore, Dedekind's view of mathematical objects seems also to be conditioned by his conception of definition and, thus, by his interests in rigour. For him, the real numbers, for instance, are not things that exist independently of analysis: they are introduced as things that constitute a domain satisfying the continuity-principle, which, in turn, is formulated in such a way that the fundamental theorems of analysis can be established in a gap-free proof; they are nothing more or less. In Dedekind's view, then, any other properties which might be ascribed to them (because of the extra-theoretical meaning attached to the non-logical terms employed in their definition) are irrelevant and even undesirable. One way to avoid this would be to deny non-logical terms of their (extra-theoretical) meanings and denotations;[50] however, Dedekind chooses instead to speak as though "new" objects are "created" through our definition of them. In his letter to Heinrich Weber of 24 January 1888, Dedekind defends this procedure by referring to two points, the first of which, as Stein observes, very much anticipates the one Paul Benacerraf makes in a well-known paper:[51] 1) for the

[49] Stein characterizes Dedekind's project in the 1888 essay in this way:

> ... it is not what numbers "are" *intrinsically* that concerns Dedekind. He is not concerned, like Frege, to identify numbers as particular "objects" or "entities"; he is quite free of the preoccupation with "ontology" that so dominated Frege, and has so fascinated later philosophers. [Stein 1988, 247, emphasis in original]

[50] This, of course, is the route Hilbert takes by means of his axiomatic method and implicit definition. Note, however, that Dedekind himself writes elsewhere that "All technical expressions [are to be] replaced by arbitrary newly invented (heretofore nonsensical) words; the edifice must, if it is rightly constructed, not collapse" [van Heijenoort 1967].

[51] "What Numbers Could Not Be"(1965), *Philosophical Review* **74**, 47-73.

sake of the homogeneity of all numbers, it is more expedient to proceed in this way; 2) there are many attributes of cuts that "would sound in the highest degree peculiar were they to be applied to the numbers themselves."[52] It would seem then that what leads Dedekind to the seemingly idealistic manner of speech is not any philosophical (ontological, epistemological) concerns but rather his interests in (re)constructing analysis in one uniform linguistic framework.

§7. Let us now go back to the exegetical problem posed earlier concerning Dedekind's foundational investigations in his 1872 essay and see how it can be answered. Our task was to find an account of Dedekind's project without ascribing to it any exalted epistemological objectives. Through a careful examination of the text, I have argued that what primarily inspires Dedekind's enterprise is his interests in providing a gap-free proof for the fundamental (limit-existence) theorems of analysis; that he tries to achieve this goal by constructing analysis in the language of arithmetic; but that since rigorization does not in itself imply reduction, his negative attitude toward reductionism is not inconsistent with the seemingly reductionist outlook of the 1872 essay. But, what, then, explains his interest in rigor? In the first chapter, I argued that the interest in rigor in the case of Hilbert's investigation into the foundations of geometry is explicable in

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[52] Dedekind 1888b, 835. Similarly, concerning the definition of cardinal number as a class, Dedekind writes in the same letter:

> one will say many things about the class (e.g. that it is a system of infinitely many elements, namely, of all similar systems) that one would apply to the number only with the greatest reluctance; does anybody think, or won't he gladly forget, that the number four is a system of infinitely many elements? [Ibid., 835]

terms of his desire to enhance the understanding of geometrical theorems by means of systematization and his concern for establishing the objectivity of mathematical judgment and inference.

The circumstance is somewhat different in Dedekind's case. First and foremost, Dedekind, unlike Hilbert, had a specific theoretical problem he wanted to solve and, in trying to find a solution to the problem, he found it impossible to achieve his goal by those non-arithmetical, intuitive elements which were (tacitly) employed in Cauchy's foundational investigation. As a result, he replaced them with the purely arithmetical concepts of continuity and real number. Here, then, the pursuit of rigor appears to be driven primarily by technical interests of mathematical research and have no further, deeper motivations. Such an interpretation has been put forward by Philip Kitcher. Kitcher's account is presented in the course of his attempt to provide a proper account of the history of research in the foundations of various mathematical branches and is supposed to explain, at one swoop, all the rigorization attempts which appeared in the history of mathematics.[53] Another point characterizing Kitcher's account of rigorization is its explicitly anti-epistemological and anti-philosophical stance:

> ... despite the "philosophical" remarks mathematicians sometimes insert into their preface, I see no reason to assume that those mathematicians have any exalted epistemological interests and that they become concerned when the reasonings in some branch of mathematics are incapable of furnishing a priori knowledge. [Kitcher 1984, 213]

[53] One notable exception is Frege's attempt, which, according to Kitcher, was motivated by "misguided epistemological ideals."

What, then, leads mathematicians to foundational issues? In Kitcher's view, mathematicians attend to foundational issues "when [they] recognize that important problems cannot be solved without some clarification of language and techniques of reasoning" [Ibid., 215]. For Dedekind (and other nineteenth century analysts), it was the two "loose ends" left dangling in Cauchy's earlier attempt of the rigorization of analysis that constituted such problems: more specifically, one was to seek a definite resolution of the so-called Fourier question;[54] the other was to give algebraic demonstrations of the existence of limits. For the purpose of carrying out the latter task, Dedekind rigorized analysis. His pursuit of rigor was thus "motivated by the need to fashion tools for continuing mathematical research" [Ibid., 271].

While it seems clear that this reading fits quite well with the actual content of the 1872 essay, one more instance might be cited, where Dedekind stresses the non-philosophical, technical nature of mathematical and scientific developments in general:

> ... the greatest and most fruitful advances in mathematics and other sciences have invariably been made by the creation and introduction of new concepts, rendered necessary by the frequent recurrence of complex phenomena which could be mastered by the old notions only with difficulty. [Dedekind 1888, 792]

Given the fact that this remark immediately follows the passage in which he denies the "meritoriousness" of a reductionist

[54] Roughly speaking, it asks whether and how any function can be given a trigonometric series representation. For more on this, see Kitcher 1984, 249ff.

enterprise, he is most probably counting his "creation and introduction" of the real numbers among those "fruitful advances in mathematics." That is to say, he considers his own project to be essentially a response and a solution to a research problem which has been barely tameable by the old notions. What is important to Dedekind is not any such exalted epistemological goals as the demonstration of the certainty and a prioricity of analysis, but a successful solution of a problem of research, and thus it matters little to him whether, say, the newly introduced arithmetical concept of real numbers is decidable.

Against Kitcher's account, however, it might be argued that Dedekind's attempt in the 1872 essay, although it is not motivated by any exalted epistemological interests, does contain an epistemological element. More specifically, it might be said that for him the attempt constitutes an (epistemological) project of establishing the former's *independence* from other types of knowledge such as geometry and kinematics.[55] In connection with Kronecker's foundational investigations, I said earlier that the rejection of the Kantian notion of the pure intuition of space and time occasioned by the discoveries of consistent non-Euclidean geometries created a sort of epistemological vacuum in the foundations of analysis, and that Kronecker tried, by means of a reductionist program, to avoid the incursion of an *a posteriori* (geometrical) element into analysis and save the a

[55] Such an interpretation has been presented by William Demopoulos in his account of Frege's investigation into the foundation of arithmetic. This interpretation is intended by Demopoulos also to apply to the foundational interests of other nineteenth century analysts such as Cauchy, Bolzano, Weierstrass, Cantor and Dedekind. See Demopoulos 1994, 71. The interpretation of Dedekind's view presented in what follows is meant to develop Demopoulos's idea.

prioricity and absolute truth of this essential component of arithmetic. But there is another epistemological sense in which it is desirable to eschew any appeals to a foreign element--be it *a posteriori* or *a priori*--in the foundations of a scientific discipline: the dependence of a fundamental principle of the discipline upon some foreign element would mean that the discipline lacks autonomy since this element presumably falls within the domains of "foreign" disciplines.[56]

When faced with such "threats," one might try to establish the autonomy (and generality) of a discipline by "banishing" any foreign elements from its foundations. That Dedekind might have something like this in mind when he tried to arithmetize the real number system can be seen in the following passage:

> For our immediate purpose, however, another property of the system *R* [of real numbers] is still more important; it may be expressed by saying that the system *R* forms a well-ordered domain of one dimension extending to infinity on two opposite sides. What is meant by this is sufficiently indicated by my use of expressions borrowed from geometric ideas; but just for this reason it will be necessary to bring out clearly the corresponding purely arithmetical properties *so as to avoid even the appearance that arithmetic is in need of such foreign ideas* [fremden Vorstellungen]. [Dedekind 1872, 768, my emphasis][57]

In other words, if the notion of continuity or dimensionality, which is essential to analysis, must come from non-arithmetical notions such as space, time, and "measurable quantities," it

[56] Moreover, in the case where the discipline in question is arithmetic (or analysis), this would further imply that the *generality* or *universality* usually associated with arithmetic will be lost along with its autonomy in case its "host" discipline should lack such generality.

[57] Later in the essay, Dedekind also writes that the fact that "comparisons with non-arithmetical notions have furnished the immediate occasion for the extension of the number-concept" is "certainly no reason for introducing these *foreign* notions into arithmetic itself, the science of numbers [Dedekind 1872, 771, my emphasis].

would follow that analysis depends essentially upon those disciplines (e.g. geometry, kinematics, mechanics) to which these non-numerical notions properly belong. Dedekind thus attempts to establish the independence of analysis from these alien disciplines by defining the real number system solely in terms of numbers (and set-theoretic notions) and thereby demonstrating that the possibility of analysis does not presuppose any other type of knowledge than the theory of numbers (and set theory). Consequently, in engaging the seemingly "reductionist" project, Dedekind is not concerned with justification.

I find this account ultimately unsatisfactory for two reasons. First, as I argued above, what Dedekind is really opposing in his criticism of foreign notions is not the introduction of non-arithmetical or non-numerical (e.g., geometrical or mechanical) vocabulary into the language of analysis, but rather the intrusion of intuitive or non-conceptual elements into the purely "logical" process of mathematical reasoning. This point was made more explicit in Dedekind's second foundational essay, where he investigated the foundations of arithmetic:

> In speaking of arithmetic (algebra and analysis) as merely a part of logic I mean to imply that I consider the number-concept entirely independent of the notions or intuitions of space and time--that I rather consider it an immediate product of the pure laws of thought. [Dedekind 1888, 790-791]

Very roughly, in Dedekind's view, our knowledge of numbers is based upon certain set-theoretical principles which, in turn, derive ultimately from "the ability of the mind to relate things to things, to let a thing correspond to a thing, or to represent a thing by a thing, an ability without which no thinking is possible" [Dedekind 1888, 791].[58] Accordingly, in the 1888 essay, Dedekind rigorizes arithmetic on the basis of logic in this broad sense; more specifically, he formulates some basic principles for the theory of "system," his term for set, and characterizes the notion of natural number (up to isomorphism) solely by means of the set theoretical, and thus logical, apparatus.[59]

It might be thought, then, that the above "minimally" epistemological interpretation of Dedekind's rigorization project could be salvaged by saying that its aim is to establish the independence or autonomy of our "logical" knowledge from intuitive elements. Yet, there seems to be another reason why the "minimalist" reading does not work. It is true that if Dedekind's interest in rigor were mainly motivated by his concern for the epistemological independence of logic (taken in his broad sense), his project of rigorization would not be epistemological in the sense of justifying the truth of arithmetical and analytical theorems. What is to be recognized, however, is that, in arguing for the independence of our knowledge of these propositions, he is at least committed to the *possibility* of such knowledge: were the knowledge of arithmetic and analysis impossible, there would be

[58] As Hallett points out, this remark by Dedekind indicates that for him "set theory of some sort forms part of the 'laws of thought'" and thus of logic [Hallett 1990, 230]. In other words, for Dedekind what we might today call set theory and logic collectively or conjointly constitutes the necessary conditions for the possibility of thinking in general. And this is (part of) the reason why Dedekind, despite his unambiguous "logicist" statement, does not attempt at an reduction of the former to the latter.

[59] I shall discuss Dedekind's project in the 1888 essay in more detail in the next chapter.

no sense in talking of its independence from intuitive notions. It would seem to follow from this that Dedekind would then have a very strong reason to be concerned about the decidability of concepts occurring in those propositions. But this clearly conflicts with the textual evidence. As we saw earlier, Dedekind vehemently argued against Kronecker's imposition of such a methodological restriction upon the "freedom" of concept-formation in mathematics.

Should we conclude with Kitcher that Dedekind's foundational interest in rigor is motivated entirely by technical interests of mathematical research and has no epistemological or philosophical dimension to it? Admittedly, this is a difficult question to answer, but there are a few things which might be mentioned in this connection. First, Dedekind's notion of rigor, like Hilbert's, seems to involve something more than just "gap-free" proofs. In one of the passages quoted above, Dedekind emphasizes the importance of the finitude of proof-process:

> ... without any notion of measurable quantities and simply *by a finite system of simple steps of thought*, man can advance to the creation of the pure continuous number-domain; and only by this means is it in my opinion possible for him to render the notion of continuous space *clear* and *definite.* [Dedekind 1888, 793-794, my emphasis]

Although Dedekind finds the Kroneckerian requirement of the decidability of concepts unnecessary, he does seem to think that the finitude of deductive basis and the perspicuity of inferential process bring about clarity and definiteness in the foundations of the discipline in consideration. Elsewhere Dedekind links the simplicity of foundations with the themes of systematicity, unity, and generality:

> My efforts in number theory have been directed towards basing the work not on arbitrary representations or expressions but on simple foundational concepts, and thereby--although the comparison may sound a bit grandiose--to achieve in number theory something analogous to what Riemann achieved in function theory .... [Dedekind 1932, 477, quoted and translated in Marion 1995, 197]

These remarks, together with his emphasis on rigor as a requirement for the "scientificity" of arithmetic, seem to indicate that concerns very similar to Hilbert's are also operative in Dedekind's foundational investigation.

Chapter III

The Logical Grounding of Arithmetic

§1. In chapter 1, we saw, in reference mainly to Hilbert's investigation into the foundations of geometry, that what motivates his pursuit of rigor and thus his conception of definitions and axioms, which characterizes the Hilbertian axiomatic method, is his concern for the systematicity and objectivity of mathematics. In chapter 2, we then considered two fundamentally different types of application of the genetic method exemplified in the so-called arithmetization of analysis by the two influential late nineteenth century mathematicians, Kronecker and Dedekind, on the assumption that Hilbert's introduction of the axiomatic method into the field of arithmetic was meant to be a response to the methodological dispute between the two. In the present chapter, I shall consider how precisely Hilbert's grounding of arithmetic is related to the attempts of his predecessors and how he actually tries by means of the axiomatic method to provide a decisive solution to the dispute and also to the emergence of set-theoretical paradoxes. In particular, I shall argue that Hilbert's attempt of the logical grounding of arithmetic can be understood as a direct offspring of Dedekind's foundational approach and is designed to refute Kronecker's standpoint, which is dictated by the epistemological concern for the aprioricity and absolute truth of arithmetic. This then lead us to the question as to Hilbert's view on the epistemological status of mathematics, and I shall consider this question towards the end of the chapter.

In our discussion of Dedekind's foundational project, we saw that what is usually meant by "arithmetization" can be distinguished into two, logically independent, components--translation into the language of arithmetic and rigorization--and that Dedekind's project should be viewed primarily as an attempt of the rigorization of analysis by means of arithmetization in the first sense. Moreover, it was also noted there that a clear awareness of the distinction was shared by Hilbert's teacher at Leipzig and later colleague at Göttingen, Felix Klein. Klein, while quite clear on the logical possibility of rigorization without arithmetization as reduction, nevertheless, thought that, for the rigorization of geometry, recourse to analytical methods was inevitable for technical reasons. On one level, Hilbert's 1899 *Foundations of Geometry* can then be regarded as a realization of the Kleinian ideal of rigorized geometry "on purely geometrical lines." From fairly early on, Hilbert clearly understood the formal or content-independent nature of deductive relations and recognized that the possibility of rigorization is not limited to those parts of mathematics which are concerned with numbers. This insight led him to the adoption of the new kind of axiomatic method, in which the technical terms of a theory are stripped of any system-independent meaning and denotation and given their meaning through the axiom system as a whole. And he decided to apply this method to the relatively simple case of elementary geometry in order to carry out a complete "logical" grounding in this field.[1]

[1] In this connection, we should also pay attention to the enthusiasm Hilbert shows towards Hermann Minkowski's *Die Geometrie der Zahlen* (1896), which demonstrates the possibility of "an arithmetical theory operating rigorously with geometrical ideas and signs" [Hilbert 1900b, 1101]. This fact, I think, confirms Hilbert's awareness that the possibility of rigorization does not necessitate the adoption of one particular linguistic framework.

What is to be noted, however, is that Hilbert's rigorization of geometry carried out in his *Festschrift* has a polemical aspect as well. As Hilbert's first doctoral student, Otto Blumenthal later pointed out in a biographical sketch of his great teacher, in carrying through "his ideal of a complete proof-structure [*Beweisgebäude*]" outside the theory of numbers, Hilbert, at the same time, presented a counterexample to the view represented by Kronecker that "all mathematics that cannot be immediately tied to the natural numbers is contaminated with impure 'earthly remains' [Erdenrest]."[2] One year after the publication of the *Festschrift*, in his Paris address *Mathematische Probleme*, Hilbert made his disagreement with the Kroneckerian view explicit:

> While insisting on rigour in the proof as a requirement for a perfect solution of a problem, I should like, on the other hand, to oppose the opinion that only the concepts of analysis, or even those of arithmetic alone, are susceptible of a fully rigorous treatment. This opinion, occasionally advocated by eminent men, I consider entirely erroneous. [Hilbert 1900b, 1100]

To be sure, Hilbert here does not name names, but whom precisely he means by "eminent men" becomes quite obvious when he continues the remark by saying that "such a one-sided interpretation of the requirement of rigor" leads, as a last consequence, to "the rejection of the ideas of the continuum and of the irrational number" [Ibid., 1100]. In contrast to Kronecker's narrow conception of rigor, Hilbert's view of rigor sees no such boundary:

[2] Blumenthal 1922, 68, translated and quoted in Hallett 1990, 211.

On the contrary I think that wherever, from the side of the theory of knowledge or in geometry, or from the theories of natural or physical science, mathematical ideas come up, the problem arises for mathematical science to investigate the principles underlying these ideas and so to establish them upon a simple and complete system of axioms, that the exactness of the new ideas and their applicability to deduction shall be in no respect inferior to those of the old arithmetical concepts. [Hilbert 1900b, 1100][3]

Kronecker's conception of geometry, as we saw above, stems ultimately from his Gaussian view that the object of geometry, space, has a mind-independent existence and thus that we are unable to have *a priori* knowledge in this domain. It is to be recognized, however, that what Hilbert finds particularly objectionable here is not Kronecker's rejection of the aprioricity of geometrical knowledge; nor is Hilbert's objective with the rigorization of geometry the demonstration of its aprioricity. Rather, Hilbert's objection is directed toward the primacy of epistemological concerns in Kronecker's thinking over mathematical ones. Accordingly, Hilbert tries to refute Kronecker's claim about the impossibility of rigorization in non-arithmetical branches of mathematics on the mathematical ground, by actually constructing a rigorous, deductive system of geometry. Indeed, to present a philosophical argument against the assumption underlying Kronecker's position and thus confront him at the philosophical level would be tantamount to leaving the objective tribunal of mathematics and falling into a "game of hide-and-seek."

Hilbert's fight against Kronecker does not end here. In Hilbert's eyes, Kronecker's emphasis upon the epistemological is harmful not only to geometry but also to mathematical (and

[3] Indeed, Hilbert lists as the sixth problem the "mathematical treatment of the axioms of physics" among his celebrated twenty three problems.

scientific) investigation in general. And, as a matter of fact, the threat of the Kroneckerian tendency is nowhere more serious than in the field of arithmetic, Kronecker's paradigmatic example of *a priori* knowledge. The threat is twofold. First, with regard to the foundations of arithmetic (in the narrow sense), Kronecker sees neither the necessity nor the possibility of giving a (gap-free) proof for the principles of arithmetic because of his philosophical conviction that "the integer--and, in fact, the integer as a general notion (parameter value)--is directly and immediately given."[4] Second, as we saw above, the possibility of giving such a proof to the fundamental theorems of analysis is denied by Kronecker on two philosophical grounds: a) we do not have the *a priori* intuition of continuity; b) the proof would involve the use of undecidable concepts, and thus its correctness cannot be verified. What is happening here, as in the case of geometry, is that the demand for rigor, which, because of the "formal" character of deductive relations, is executable independently of any extra-systematic considerations, is overridden by the beliefs about the ontological/epistemological status of the (system-independent) objects the theory in consideration is supposed to be concerned with. Moreover, since such beliefs are accepted for some extra-systematic reason and are not susceptible to any objective treatment, they are nothing other than "dogmas."

[4] Hilbert 1905a, 130. Hilbert continues the remark with the following words:

> ... this prevented him [Kronecker] from recognizing that the notion of integer must and can have a foundation. I would call him a dogmatist, to the extent that he accepts the integer with its essential properties as a dogma and does not look further back. [Ibid., 130]

Needless to say, Hilbert cannot tolerate such a standpoint: it is his invariable contention that both arithmetical and analytical principles "must and can have a foundation." Thus, as Hilbert embarks on the rigorization or "logical grounding" of arithmetic and analysis, he explicitly identifies the implementation of this goal with the refutation of Kronecker's "dogmatic" standpoint.[5] How then does he try to achieve this goal? To put it simply, Hilbert once again attempts to refute Kronecker's position by constructing a "complete proof-structure" for these disciplines. Yet, circumstances are somewhat different this time around as a consequence of the recent developments in the foundational investigations and set theory in particular. Thus, before considering Hilbert's project, I shall briefly go over some of the points particularly relevant for our later discussion.

§2. As we have seen in passing, before Hilbert, Dedekind tackled, *contra* Kronecker, the problem of rigorizing arithmetic in his 1888 essay in the belief that the principles of numbers can and must be given a proof. Dedekind's solution exactly parallels his previous attempt in the foundations of analysis: he defines ("creates") the natural numbers as the things which constitute a (infinite) domain possessing those and only those characteristics which are required for the valid deductions of arithmetical principles. In outline, Dedekind's set-theoretical definition of the natural numbers presented in the 1888 essay proceeds as follows. After introducing various basic concepts of set theory (e.g., set, subset, union,

[5] See the closing remark of the 1905 essay.

intersection, mapping, one-to-one mapping and so on),[6] Dedekind first defines in terms of these set-theoretical ideas two important notions. The first is a *chain* [*Kette*] *relative to a mapping* ϕ or, simply, a ϕ-chain: a set K is a chain relative to ϕ if the image [*Bild*] of K under ϕ, i.e., what results from K by the mapping [*Abbildung*] ϕ, is a subset of K. For instance, let ϕ be $x \to x$. Then, any set K is a chain relative to ϕ since its image under ϕ, i.e., K, is a subset (although not a *proper* subset) of K itself. The second is *the* chain of A relative to ϕ, alternatively, $\phi_0(A)$ or A_0: A_0 is the intersection of all ϕ-chains containing A. If we use the above example and let A be 1, then A_0 will be {1} since {1} is the intersection, i.e., the common part, of all the ϕ-chains that contain 1.

With these two notions in hand, Dedekind next introduces the notion of *simply infinite system*: by this he means a set N together with a mapping ϕ of N and an element 1 of N satisfying the following conditions: a) ϕ is a mapping of N into itself;[7] b) N is the chain 1_0 relative to ϕ; c) 1 is not contained in the image N' of N under ϕ; and d) ϕ is a one-to-one mapping. The clauses a), c), and d) collectively state that N is an *infinite* set: by a), what results from N by ϕ, i.e., N', is a *subset* of N; and, by c), N' does not contain one element of N, i.e., 1, and therefore N' is a *proper* subset of N; and finally, by d), there exists a one-to-one mapping

[6] Here I basically follow the standard terminology of modern set theory. To mention some instances in which Dedekind's terminology differ, "system" is used for "set," "similar mapping" for "one-to-one mapping."

[7] A mapping ϕ is said to be a mapping of a set S *into itself* if and only if the image of S under ϕ is a subset of S itself, that is to say, what results from S by ϕ is a subset of S.

between the elements of N and N'. In other words, the elements of N can be matched up one-to-one with those of its proper subset N', which is possible only if N contains infinitely many elements.[8] The role of the clause b) is to make sure that N is the "smallest" of all the ϕ-chains containing 1.

Finally, Dedekind introduces his definition of the natural numbers in this way:

> If in the consideration of a simply infinite system N ordered by a mapping ϕ we entirely neglect the special character of the elements, simply retaining their distinguishability and taking into account only the relations to one another in which they are placed by the ordering mapping ϕ, then these elements are called *natural numbers* or *ordinal numbers* or simply *numbers*, and the base-element 1 is called the *base-number* of the *number-series N*. With reference to this liberation of the elements from every other content (abstraction) we are justified in calling the numbers a free creation of the human mind. The relations or laws which are derived entirely from the conditions [a), b), c), d) above], and therefore are always the same in all ordered simply infinite systems, whatever names may happen to be given to the individual elements ... form the first object of the *science of numbers* or *arithmetic*. [Dedekind 1888, 809]

As we saw above, in the 1872 essay, Dedekind defined the real numbers as what produce (Dedekind) cuts and thus are distinct from cuts themselves. Similarly, here, in defining the natural numbers, Dedekind does not identify the natural number system with "instances" of simply infinite systems. For him, the natural number system is rather the *structure* instantiated in such instances. At any rate, Dedekind, with this definition in hand, goes on to prove, among other things, the principle of

[8] In fact, Dedekind defines the notion of infinity precisely in these terms: a system S is said to be infinite if and only if there exists a one-to-one mapping between S and a proper subset of S. See Dedekind 1888, 806.

mathematical induction and the legitimacy of definitions by primitive recursion and provides the standard recursive definitions of arithmetical operations (e.g., addition, multiplication, and exponentiation). In this way, he demonstrates that the whole science of arithmetic can be developed from the "uniform" foundation (i.e., the concept of number) through purely logical processes.

Now, while Dedekind's two foundational attempts presented in the 1872 and 1888 essays respectively share their overall goal and methodology, the later project differs from the earlier one in one respect: in the 1888 essay Dedekind not only tries to demonstrate the possibility of a gap-free proof for the principles of arithmetic by constructing a deductive basis, but also points, in clear terms, to the need for establishing that such a deductive basis does not contain "internal contradictions." More specifically, Dedekind thinks it necessary to establish that a simply infinite system, in terms of which the definition of the natural numbers is formulated, has an existence "in the realm of our ideas":[9]

> 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

[9] Although it might be said that Dedekind expresses his concern for consistency already in the 1872 essay in connection with his criticism of the notion of extensive magnitude, his concern there has to do with the "real existence," and not with an "inner contradiction in the concept." On the other hand, Dedekind's concern for consistency can be clearly seen in his letter to Lipschitz of 27 July 1876:

> 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? [Dedekind 1932, 477, quoted and translated in Sieg 1999, 4]

> not perhaps contain internal contradictions. Hence the need for such a proof (article 66 and 72 of my essay). [van Heijenoort 1967, 101, emphasis in original][10]

Dedekind's somewhat peculiar, "logical" proof for the existence of infinite sets itself reads as follows:

> Theorem. There exist infinite systems.
> Proof. My own realm of thoughts, i.e., the totality *S* of all things which can be objects of my thought, is infinite. For if *s* signifies an element of *S*, then the thought *s′*, that *s* can be object of my thought, is itself an element of *S*. [Dedekind 1888, 806-807]

The mapping $s \rightarrow s'$ is a one-to-one mapping between *S* and a proper subset of *S* itself, and therefore *S* is infinite. This, apparently, is Dedekind's point here.

This "proof" has been widely criticized.[11] In particular, the problematic nature of the notion of the totality of everything thinkable, which Dedekind employed in the proof, was soon pointed out by his long time friend Georg Cantor. Cantor, who is often considered the sole founder of set theory, had for some time been aware of the problems surrounding the use of such a notion through his set-theoretical investigation into infinite set and transfinite numbers. One such problem can be explained, roughly, as follows. According to Cantor, two sets have the same *cardinal number* when there exists a one-to-one mapping between them. So, for instance, the two sets {a, b, c}

[10] Letter to Keferstein of 27 February 1890. The essay Dedekind refers to is Dedekind 1888. In Hallett's view, a source of Dedekind's concern for consistency is found in his thesis that mathematical objects are "created" according to certain principles and on the basis of nothing else. Thus, the question naturally arises whether the creation are "possible" at all. See Hallett 1995, 146 and Hallett 1990, 226ff.

[11] Subsequently it came to be seen that the existence of infinite sets is a matter of postulation, rather than of proof.

and {d, e, f} have the same cardinal number, more precisely, the cardinal number three. The same applies to infinite sets, that is, sets containing infinitely many elements: two infinite sets have the same cardinal number if they can be placed in one-to-one correspondence. For instance, the set of natural numbers {1, 2, 3, ...} have the same cardinal number as the set of even numbers {2, 4, 6, ...} since the elements of the two sets can be placed into one-to-one correspondence, e.g., 1 with 2, 2 with 4, 3 with 6 and so on. The cardinal numbers of such infinite sets are called *transfinite numbers*. In his theory of sets, Cantor established that, for any given infinite set, there is an infinite set of a greater cardinality, and hence that, just as there exists no greatest natural number, so there exists no greatest transfinite number. Consider, however, the notion of the totality of all sets, i.e., the "set" which includes all sets as its members. By definition, no set can have more members than such a "set" of all sets. But if so, it seems to follow that no transfinite number is greater than the cardinal number of this "set," which clearly contradicts Cantor's result that there exists no greatest transfinite number.

After seventeen years of blank, Cantor resumed regular correspondence with Dedekind in 1899 and, in his letter of 3 August of that year, informed the latter of the paradoxical consequences which would result from the use of notions such as the "totality of everything thinkable." In the letter, Cantor further explained that, of all the (definite) multiplicities, there are ones such that the assumption that all of their elements "are together" leads to a contradiction and it is impossible to consider them as "one finished thing."[12] Cantor called these *absolutely infinite* or *inconsistent multiplicities* and stressed the need for distinguishing them from *consistent multiplicities* or *sets*, the totality of whose elements can be thought of without contradiction as "being together" and which can therefore be considered as "one thing." In his reply to this letter, Dedekind did not try to dispute the claim that his employment of the notion of the totality of everything thinkable in the proof for the existence of infinite systems leads to a contradiction, but complained about Cantor's distinction between consistent and inconsistent multiplicities:

> ... You will certainly sympathize with me if I frankly confess that, although I have read through your letter of 3 August many times, I am utterly unclear about your distinction of totalities [*Inbegriffe*] into consistent and inconsistent; I do not know what you mean by the 'co-existence of all elements of a multiplicity', and what you mean by its opposite.[13]

§3. As we shall see shortly, Hilbert also found Cantor's criterion for the distinction utterly unclear and tried to improve it by means of his axiomatic method. But before considering that, we must understand how those problematic multiplicities were thought to form genuine sets in the first place. This question leads us to what Hilbert calls the "fundamental principle" in traditional logic: "a concept (a set) is defined and immediately usable if only it is determined for every object whether the object is subsumed under the concept or not" [Hilbert 1905a, 130]. Traditionally, this so-called comprehension principle was relied upon as the sole

[12] Letter to Dedekind of 3 August 1899 in Ewald 1996, 931.

[13] Letter to Cantor of 29 August 1899 in Ewald 1996, 937.

criterion for the formation of concepts and, through them, classes. As we saw earlier, Dedekind, for one, appealed to this principle against Kronecker's imposition of methodological restrictions upon concept-formation. Apparently, Hilbert too was convinced of the legitimacy of the principle (in an unrestricted sense) until Cantor communicated to him of the said distinction between consistent and inconsistent multiplicities and paradoxical consequences resulting from the introduction of the latter.[14]

What the principle of comprehension allows one to do, when used in an unrestricted manner, is to introduce a concept or a set solely by dint of its determinacy. Thus, on this principle, Dedekind's alleged set of all thinkable objects would be allowable and indeed exist insofar as it is determinate for any object whether it belongs to it or not. But, of course, the introduction of this "set," as we just saw, would result in a contradiction. Another example in which the unrestricted use of the comprehension principle led to a devastating result is found in Frege's foundational investigation. Very roughly (and anachronistically), it can be described as follows.[15] In attempting to provide a foundation for arithmetic using only the notions of logic, Frege assumed that, for any given property $P(x)$, one could speak of the

[14] See Letter to Hilbert of 2 October 1897 in [Ewald 1996, 927]. According to Ivor Grattan-Guinness, Hilbert had been the main contact of Cantor's on this issue and told of the distinction between "ready" ("*fertig*") [i.e. consistent] sets and "absolutely infinite" sets as early as 1896. For more on the historical background, see Grattan-Guinness 2000, 117-119 and Ewald 1996, 923-926.

[15] For the sake of brevity and clarity, here I do not rehearse how the so-called Russell paradox arises from Frege's infamous Basic Law V. For a concise account of this important episode in the history of the philosophy of mathematics and the "derivation" of a contradiction from Basic Law V, see, for instance, Bell 1999, 196-199. The account presented here is based loosely upon Paolo Mancosu's presentation in Mancosu 1998, 67.

totality of objects which satisfy $P(x)$. In other words, he assumed that to any (determinate) property $P(x)$ there corresponds a set of objects satisfying $P(x)$.[16] Symbolically,

$$\exists X\ (P(x) \leftrightarrow x \in X),$$

where X is a set, P a property, and x an object. Now, let $P(x)$ be the property that applies to an object x just in case x is not an element of itself, i.e., $P(x) = x \notin x$.[17] It follows then that since, by assumption, to any property there corresponds a set of objects satisfying it, there must exist a set corresponding to this property as well. To put it symbolically,

$$\exists X\ (x \notin x \leftrightarrow x \in X).$$

Let us call "R" the set to which an object x belongs if and only if x is not a member of itself, that is,

$$x \in R \leftrightarrow x \notin x.$$

Moreover, since x is supposed to be any arbitrary object, R itself can be substituted for x. We then obtain:

$$R \in R \leftrightarrow R \notin R,$$

from which a contradiction follows by the rules of logic.

[16] This, of course, is equivalent to the unrestricted form of the comprehension principle.

[17] The property is determinate since it must be the case that for any given set either it is a member of itself or it is not a member of itself.

Frege was informed of the paradox by Bertrand Russell in 1902 and thereupon annexed an appendix to the second volume of his *Basic Laws of Arithmetic*, devoting it to the attempted solution to the problem. Russell himself published the paradox in his *Principles of Mathematics* in 1903, and, subsequently, there followed a series of the discoveries of new paradoxes by mathematicians and philosophers. In consequence, as Ewald reports, "the view became widespread that the paradoxes had shaken the foundations of Cantorian set-theory" [Ewald 1996, 924].

Thus, it appeared to the mathematical community at the beginning of the twentieth century that Kronecker's flat dismissal of set theory a few decades before might have had some basis after all.[18] According to Volker Peckhaus, around 1900, Hilbert, despite his wholehearted support for the Cantorian set theory since its inception, did not consider it as an independent subdiscipline of mathematics but rather merely as an "alternative methodological approach to arithmetic":

> In accordance with his "pragmatic" viewpoint, the appearance of contradictions was nothing to be alarmed about so long as the stock of accepted mathematical knowledge could be preserved by other means. [Peckhaus 1994, 96]

[18] In the 1920 lectures, Hilbert describes Kronecker's attitude toward "the newly arisen Cantorian set theory" as that of "an ostrich-politics," wishing to know nothing about its accomplishments. In the lectures he also remarks: "The first in the younger generation who seriously took Cantor's side were Minkowski and I" [Hilbert 1920, 946]. According to Bernays, however, "[U]nder the influence of the discovery of the antinomies in set theory, Hilbert temporarily thought that Kronecker had probably been right there" [Reid 1996, 173]. The young Hilbert visited Kronecker twice (1886, 1888) at Berlin, a few years before the latter's death at the age of sixty-eight in 1891.

But, by 1904, Hilbert came to the realization that it was impossible to axiomatize arithmetic in the broad sense by purely mathematical means and, accordingly, the paradoxes of set theory loomed larger in his mind. In addition, given that a source of the paradoxes could, in Hilbert's view, be traced back to the unrestricted use of the comprehension principle, which had traditionally been accepted as the reliable criterion for concept-forming, the threat of the paradoxes clearly was not confined to the theory of sets alone.[19] Thus, it was an urgent task for him to diagnose correctly the cause of the problem and prescribe a means to remove it. Yet, at the same time, this therapeutic means must not be something that unnecessarily restricts the freedom of concept-formation in mathematics.

Now, to return to Hilbert's diagnosis of the cause of the paradoxes, he recognized that a contradiction arose when the notion of totality ("all" or "every") was applied, in an unrestricted manner, in a domain of objects which was formed in accordance with the comprehension principle.[20] In the 1905 paper, he explains the failure of Frege's logicism precisely in these terms:

> *G. Frege* sets himself the task of founding the laws of arithmetic by the devices of logic, taken in the traditional sense. ... But, true to his plan, he accepts among other things the fundamental principle [i.e., the

[19] Note, however, that Hilbert's concern for consistency actually precedes and hence was not simply a reaction to the emergence of set-theoretical paradoxes. I shall come back to this point shortly.

[20] In his 1905 lectures, Hilbert writes:

> ... [T]he most difficult concept is the concept 'all' or 'every', since through its use all the contradictions known to us arise, at least if one applies it in the traditional ways.... [Hilbert 1905b, 254, quoted and translated in Hallett 1995, 156]

> comprehension principle] ..., and here he imposes no restriction on the notion "every"; he thus exposes himself to precisely the set-theoretic paradoxes that are contained, for example, in the notion of the set of all sets. [Hilbert 1905a, 130][21]

However, this recognition helps us little in itself, for, as Hilbert points out in the 1905 lectures, all thinking depends upon such a collecting together [*Zusammenfassung*] of totalities into a set, and yet we are not always led to a contradictory result: "The problem here is rather that of distinguishing the permissible collections from the impermissible."[22] As was briefly noted above, an "answer" to this question was already given by Cantor in his distinction between consistent and inconsistent (or *fertige* and *nichtfertige*) multiplicities. A multiplicity, according to Cantor, can be "assembled together" into a set if it can be thought of as "ready," that is, "if it is possible without contradiction (as can be done with finite sets) to think of all its elements as existing together":

> ... an 'assembling together' [*Zusammenfassung*] is only possible if an *'existing together'* [*Zusammensein*] is *possible*.[23]

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21 In a similar vein, Hilbert writes of Dedekind's project:

> ... I would call his method transcendental insofar as in proving the existence of the infinite he follows a method that, though its fundamental idea is used in a similar way by philosophers, I cannot recognize as practicable or secure because it employs the notion of the totality of all objects, which involves an unavoidable contradiction. [Hilbert 1905a, 131]

22 Hilbert 1905b, 215, quoted and translated in Hallett 1995, 156. See also Peckhaus 1994, 97.

23 Cantor's letter to Hilbert of 2 October 1897 in Ewald 1996, 927-928, emphasis in original.

The obvious problem with Cantor's criterion for distinguishing permissible from impermissible collections, however, is its utter unclarity: how are we to decide whether, say, the elements of the set {the Empire State Building, $\sqrt{2}$} can "exist together"? We have already seen Dedekind confessing himself completely at a loss to comprehend Cantor's meaning. Hilbert, while acknowledging Cantor's awareness of the problem, also found the latter's treatment lacking in clarity and thus ineffective:

> *G. Cantor* sensed the contradictions just mentioned [i.e. the paradoxes of set theory] and expressed this awareness by differentiating between "consistent" and "inconsistent" sets. But since in my opinion he does not provide a precise criterion for this this distinction, I must characterize his conception on this point as one that still leaves latitude for *subjective* judgment and therefore affords no objective certainty". [Hilbert 1905a, 131, emphasis in original]

Hilbert's task then is to give an articulate expression to Cantor's notion of "can be thought of as existing together without contradiction" and to provide a criterion, according to which the permissibility of set- or concept-formations can be determined in an objective, and thus "rigorous" manner.

"What is decisive is the recognition [*Erkenntnis*] that the axioms that define the concept are free from contradiction."[24] In this simple and confident statement, Hilbert's solution is implied in its entirety. Hilbert captures the Cantorian notion of the coexistence of all elements of a multiplicity as the consistency of a concept (or property). And since, in his axiomatic method, a concept is defined by and within an axiom

[24] Letter to Frege of 7 November 1903 in Frege 1980, 52.

system, the consistency of the concept, in turn, can be understood in terms of the consistency of the axiom system as a whole. This insight, together with his analysis of the notion of consistency, finally leads him to the formulation that a concept-formation is permissible if it is impossible to deduce a contradiction in the axiom system defining it "by the application of a finite number of logical inferences."[25] In thus characterizing the notion of consistency in terms of deducibility in a finite number of steps, the axiomatic method enables us not only to articulate Cantor's notion of "coexistence of the elements of a multiplicity" in terms of the consistency of an axiom system, but also to obtain a precise criterion for distinguishing permissible (consistent) from impermissible (inconsistent) concept-formations.[26]

We saw earlier that the Hilbertian axiomatics is designed to make the rigorous construction of a theory possible in such a way that the correctness of a solution to a mathematical problem can be established by means of antecedently fixed rules of inference from a limited number of assumptions in a finite number of steps. What is to be recognized here is that, at the same time, the axiomatic method also enables us to formulate the criteria, according to which it can be determined in a precise manner whether the requirement of rigor is indeed satisfied by a system. To put the point simply, the axiomatic method made possible, for the first time, the objective treatment of what we today call "metatheoretical" questions.

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[25] Hilbert 1900b, 1105. See also Hilbert 1899, 29.

[26] To mention another example, as we saw above, the traditional notion of the "correctness" of a definition is understood in terms of the *completeness* of the axiom system: a definition is "correct" if all the theorems of the relevant domain of knowledge can be derived in the axiom system formulating it.

More specifically, Hilbert opened a way to a rigorous metatheoretical investigation by defining the key notions of metatheory in terms of "deduction," "derivability," and "logical consequence."[27] In addition to the notion of consistency, those of *independence* and *completeness* are also defined in these terms and are considered by Hilbert as constituting, together with the consistency requirement, the adequacy conditions on axiom systems.[28] As a quick look at the content of these conditions makes us see, they correspond to and further develop what Hilbert calls in a different context the requirement of rigor. Consequently, when it is demonstrated *in a rigorous manner* that an axiom system fulfils these requirements, we are entitled to claim that a complete logical grounding has been given to the field of knowledge in question.

In conclusion, Hilbert's axiomatics thus takes over and gives a clear expression to the main objectives of Dedekind's foundational investigations (e.g., a gap-free proof for the arithmetical and analytical theorems from a consistent deductive base) and, in so doing, also points to ways to determine, in an objective manner, the successful implementation of these goals. This, I think, is at least part of Hilbert's meaning when he says in the 1900 essay that the axiomatic method is to be preferred for the "complete logical grounding [*Sicherung*] of our knowledge." As it turned out,

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27 In the next chapter, I shall consider in more detail how such a metatheoretical investigation proceeds with respect to the consistency of arithmetic.

28 In the 1905 lectures, Hilbert states that "axioms are independent if none can be deduced from another," whereas an axiom system is complete if "all the remaining facts of the field of knowledge that lies before us are consequences of the axioms" [Hilbert 1905b, 11-13, quoted in Peckhaus 1990, 59].

Hilbert would spend the rest of his long career trying to fulfil the ideal of complete logical grounding with regard to arithmetic in the broad sense. Yet, the important thing here is that, given that the relevant metamathematical proofs are forthcoming, a successful axiomatization of the real numbers would demonstrate against Kronecker that arithmetic in the broad sense *can* be rigorized and that the laws of number *can* have a (consistent) foundation from which they are deducible solely in accordance with rules of logic. Towards the execution of this rigorization program, in the 1900 essay,[29] Hilbert presented a (categorical) axiomatization for the reals following Dedekind's attempt in *Continuity and Irrational Numbers*, and, in the 1905 essay, being convinced of his imminent success in finding a consistency proof for his axiom system, Hilbert confidently spoke of the "real refutation" [*sachliche Widerlegung*] of Kronecker's dogmatic standpoint.

§4. Now, given that one of Hilbert's main goals in his foundational investigation is to fight against the methodological restrictions Kronecker imposes upon the formation of concepts for the epistemological concerns and demonstrate the possibility of rigorization and thus the solvability of mathematical problems in a definite, objective manner, one naturally wonders how Hilbert himself sees the philosophical issues surrounding the foundations of mathematics. In this connection, I argued above that the primary concern with Hilbert's foundational investigation is to establish the objectivity of mathematical judgment and

[29] Wilfried Sieg points out that the title of the 1900 paper "*Über den Zahlbegriff* [On the Concept of Number]" is Hilbert's polemical allusion to Kronecker's essay of the same title. See Sieg 1984, 165.

reasoning by means of rigorization, and that his project is not only not motivated by the concern for truth, but even opposed to it.

One possible answer to the above question would then be that, as a mathematician, Hilbert is simply not interested in tackling such philosophical problems as truth, knowledge, existence, and, accordingly, has nothing particular to say. Indeed, here we might recall Hilbert's "cautionary" remark to Frege that "if we want to understand each other, we must not forget that the intentions that guide the two of us differ *in kind*."[30] Given the explicitly philosophical nature of Frege's inquiries about Hilbert's *Festschrift*, the latter's remark such as this might seem to suggest that not only is he not concerned with the epistemological questions in that work, but he has no intention of getting himself involved with them. As we briefly saw above, Paul Bernays, who was Hilbert's chief collaborator in the 1920s, understood the latter's early foundational investigation in this way and, in fact, maintained that its significance consists precisely in the clear separation between the mathematical and the epistemological problems of axiomatics carried out in it. It might seem, then, that it is not only futile but even mistaken to try to find a theory of knowledge in Hilbert's early foundational study.

This interpretation seems to receive a further support when we look at general trends in mathematics in the late nineteenth century. As we saw earlier, in the field of geometry around that time, the view became widespread that geometry as a demonstrative or formal science is to be distinguished from geometry as a physical science. Seen from

[30] Letter to Frege of 29 December 1899 in Frege 1980, 38, my emphasis.

one angle, this distinction represented the mathematicians' demand for the autonomy and freedom of mathematics: by freeing geometrical terms from their assumed references and thus by relegating the question of objective validity, geometry can be developed in such a way that various operations, which previously could not be performed because of the restrictions arising from the tie to its "authentic" domain, can be carried out in a uniform and general manner. Seen from a philosophical perspective, however, this freeing of geometry from its special subject-matter was thought by many to imply that the "new" geometry is devoid of epistemic potency, as it were, and hence that questions concerning the epistemological status of geometry, if they were to be given a satisfactory answer, must be posed and considered with regard to the "old" geometry, which had its proper subject-matter.

As a representative of this trend, Moritz Pasch's view presented in his influential *Lectures on Modern Geometry* [*Vorlesungen über neuere Geometrie*] (1882) might be mentioned. According to Pasch, geometry is, first and foremost, a natural science, whose concepts correspond to empirical objects, and, as such, receives justification from the facts of sensory intuition.[31] On the other hand, with his clear awareness of the content-independent nature of deductive relations, Pasch recognized that geometry may be developed as a rigorously deductive science, which deals exclusively with the logical

[31] Pasch's view, while representative of the late nineteenth century tendency toward the separation between "formal" and "authentic" geometry, differs from other "empiricist" views of the time in that it considered the object of (authentic) geometry to be relations between *bodies*, rather than space or extension as such; accordingly, the validity of the geometrical axioms or what he calls "nuclear propositions" is established by sensory observation on bodies. For more on Pasch's view, see Nagel 1978, 235-239.

relations among geometrical propositions. Taken in this latter sense, geometry is a branch of logic and has no subject-matter and may be pursued without regard to epistemological concerns.[32] A similar tendency toward the separation between "formal" and "material" science is also detectable in other fields of mathematics in the mid to late nineteenth century. Algebra, which had traditionally been considered to be the general science of quantity and thus directly related to arithmetic as the theory of number, came to be viewed as a discipline which studies the transformations of symbols in accordance with certain specified rules, and, consequently, separated from general arithmetic or arithmetical algebra as the general theory of integers.[33] Here too symbolic algebra as a formal science was considered as having no subject-matter (taken in the traditional sense) and thus no epistemological issues of its own.

Accordingly, it might be thought that Hilbert's early foundational investigation is also to be understood against the background of this general tendency in the late nineteenth century mathematics, and, therefore, that it is in no way surprising if we should find no philosophy of mathematics in

[32] Pasch himself does not put his points in these terms. The view attributed to him here is rather "implied" by his formulation of issues.

[33] Here, I have in mind in particular the distinction between arithmetical and symbolic algebra introduced by George Peacock in his *Treatise on Algebra* (1842). According to Ernest Nagel,

> Peacock thus laid the basis for distinguishing between a system of marks having an explicit *extrasystemic* reference, concerning which questions of truth and falsity are significant; and a system of marks with no such *explicit* extrasystemic interpretation, concerning which questions of truth and falsity are meaningless. [Nagel 1978, 180, emphasis in original]

For a brief survey of the history of modern algebra, which focuses primarily on the works of English algebraists, see Nagel 1978, 166-193.

it. What is to be emphasized here, however, is that, unlike those nineteenth century mathematicians who viewed "pure" mathematics to be free from epistemological concerns, Hilbert does not seem to distinguish "logical" or "formal" geometry from "authentic" or "material" geometry, with regard to which philosophical questions can and must be posed and considered. Thus, insofar as Hilbert's foundational investigation is not motivated by concerns for truth, we would have to conclude that, for him, what is philosophical is not simply separated or bracketed but rather *nonexistent*.

The above account, however, is not the only interpretation that can accommodate the absence of philosophical elements in Hilbert's early writings on the foundational issues. In the current literature in the philosophy of mathematics, it is sometimes claimed that Hilbert (temporarily) held the view called "deductivism" or "if-then-ism."[34] According to deductivism, mathematics is nothing but the business of deducing various results from axioms. It would follows that since deductive relations are independent of the content of statements, a deductivist does not have to commit himself to any particular view on the nature of mathematical objects: all that matters in mathematics is a deductive relation between statements, and it is irrelevant for him what the non-logical terms occurring in them represent or what it is for them to be true. Thus, on the deductivist reading, Hilbert's early foundational investigation does not contain any "philosophical" considerations (taken in the standard sense) not because he distinguishes "formal" mathematics as a branch of logic from

[34] To mention a couple of examples, Michael Resnik states that "Hilbert went through a deductivist period" [Resnik 1980, 105]. Stewart Shapiro also seems to endorse such a reading, entitling one section of Shapiro 2000 "Deductivism: Hilbert's *Grundlagen der Geometrie*."

"material" mathematics with its special subject-matter and considers them as pertaining to the investigation into the latter alone, but because, for him, mathematics *just* is a logical discipline and, as such, requires no philosophical discussions on such topics as the existence of mathematical objects and truths.

What is puzzling, however, is the fact that, later in the aforementioned letter to Frege, Hilbert emphatically maintains that the consistency of a given system of axioms guarantees the "truth" of these axioms and the "existence" of the things defined by them:

> ... as soon as I have laid down an axiom, it exists and is 'true'; and this brings me now to a further important point in your letter. You write: 'I call axioms propositions ... From the truth of the axioms it follows that they do not contradict one another.' I found it very interesting to read this very sentence in your letter, for as long as I have been thinking, writing and lecturing on these things, I have been saying the exact reverse: if the arbitrarily given axioms do not contradict one another with all their consequences, then they are true and the things defined by the axioms exist. This is for me the criterion of truth and existence. [Frege 1980, 39-40]

And this is no slip of the pen on Hilbert's part. A similar remark can be found in almost every work he composed in this early period.[35] Given such textual evidence, we seem to have to reject as false the first, Bernaysian interpretation according to which, for Hilbert, pure mathematics distinguished from material mathematics has no subject-matter, and hence his early investigation into the former has no philosophical discussions on truth and existence. By contrast, on the second, deductivist account, Hilbert takes mathematics to consist in

[35] E.g. Hilbert 1900a, 1095, Hilbert 1900b, 1105, Hilbert 1905a, 134.

deductions from axioms, and thus he could adopt a "stance of agnosticism concerning the existence of mathematical objects and truths" insofar as "a mathematical deduction from axioms can be carried out without any presuppositions concerning the truth of the axioms or their ontology [Resnik 1980, 105]. On this reading, then, Hilbert need not but *can* maintain without contradicting himself that mathematics has its subject-matter.

However, given that the chief merit of adopting the deductivist standpoint is such freedom to remain agnostic about the notoriously difficult philosophical issues, one might wonder why Hilbert would not take advantage of this freedom. Another point that seems to escape the deductivist account is the notion of truth and existence Hilbert presents in the quotation. Provided that he really means that the consistency of axioms guarantees their truth and the existence of the objects defined by them, it would follow that two consistent but mutually incompatible axiom systems would both be true. But how could, say, Euclidean and non-Euclidean geometries both be true?[36] It seems obvious then that Hilbert is not using these concepts in the standard manner. And this explains why he could possibly speak of truth and existence while, at the same time, denying that his foundational investigation is motivated by the standard philosophical concerns for truth and so on. But, how, then, could we make sense of his remark? The deductivist account seems unable to explain Hilbert's intent insofar as it, just as other standpoints in the philosophy of mathematics, assumes the standard understanding of these notions.

36 That is, assuming that they are both consistent.

§5. In my opinion, what is implicit in Hilbert's approach is best understood as the relativization of the notions of truth and existence to the axiom system characterizing a field of knowledge. First of all, it is to be recalled that while, in Hilbert's axiomatic method, the non-logical terms occurring in the axioms of a theory are denied of system-independent meanings and denotations, they obtain (intra-systematic) meanings from their relations to each other formulated in the axioms. In accordance with this holistic conception of meaning and definition, Hilbert, like Dedekind, comes to the view that mathematical objects are not something existing independently of mathematical theories in which they occur and are waiting to be picked out by these theories, as it were. Rather, they just are (a system of) things that constitutes a domain satisfying the relational properties expressed in the axiom system and nothing more or less. Hilbert makes this point explicit in the 1900 Paris address:

> The totality of real numbers, i.e., the continuum according to the point of view just indicated, 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. It is rather a system of things whose mutual relations are governed by the axioms set up and for which all propositions, and only those, are true which can be derived from the axioms by a finite number of logical inferences. In my opinion, the concept of the continuum is strictly logically tenable in this sense only. [Hilbert 1900b, 1105][37]

This view of mathematical objects is a crucial component of his "philosophy" of mathematics and appears to have remained invariable throughout his career. Still in 1922 Hilbert

[37] See also Hilbert 1900a, 1095.

writes: "a real number is conceptually just a thing belonging to our system" [Hilbert 1922, 199].

As a consequence, for Hilbert, the notions of truth and existence should also be considered in relation to an axiom system. In the quoted letter to Frege, it is said that "as soon as I have laid down an axiom, it exists and is 'true'." In other words, if the existence of an object is implied by the axioms constituting a system, it exists, and, similarly, a statement is true if it is a logical consequence of the axioms. Since the axioms are consequences of themselves, on this conception, they are true and the objects defined by them exist as soon as they are laid down. With regard to the notion of truth, Hilbert thus writes elsewhere that "all propositions [*Tatsachen*], and only those, are true [*wahr*] which can be derived from the axioms by a finite number of logical inference" [Hilbert 1900b, 1105].

But if, for him, truth and existence are relativized to an axiom system, why does he also maintain the need for a consistency proof? Would it not be the case that anything whose existence is implied by the axioms exists? Furthermore, how can we account for Hilbert's frequent remark about the equivalence of consistency and (mathematical) existence?[38] To quote one such instance, Hilbert claims in the 1900 address:

> ... the proof of the consistency of the axioms [of real numbers] is at the same time the proof of the mathematical existence of the complete system of real numbers or of the continuum. [Hilbert 1900b, 1105]

[38] See Hilbert 1900a, Hilbert 1900b, Hilbert 1905a. On one account, Hilbert identifies consistency and existence for most of his career. See Mancosu 1998, 178.

On this question, Michael Hallett made the suggestion that Hilbert's view is best understood as a form of "internal realism."[39] That is, instead of starting from the assumption that certain extra-theoretical objects and states of affairs exist and arguing that the objects that theories talk about correspond to these and their axioms express truths about these, Hilbert proposes that "if we are to continue to use such notions as 'existence' and 'truth of axioms', then these must be dealt with in a non-mysterious way via the notion of theory acceptance" [Hallett 1990, 225]. And once the question of the acceptability of a theory is settled in accordance with such a non-mysterious method, the only meaningful question about existence will be the one taken in the "internal" sense, which amounts to demonstrating, within an axiom system, a proposition of the form $\exists xA$, even by non-constructive means. The proof of consistency, then, provides the required criterion for the acceptance of a theory in cognitively accessible terms. Understood thus, Hilbert is not precisely proposing to relativize the notion of existence (and of truth). But this does not mean that with the requirement of consistency, he is trying to answer the question of truth and existence considered in the traditional, external realist sense. Rather, his suggestion is to *replace* the standard, metaphysical notion of existence with the notion of theory acceptance formulated in terms of the consistency or the impossibility of deducing a contradiction in a finite number of logical steps. And he makes such a suggestion presumably because its alternative, i.e., the traditional manner of considering the issue of truth and existence, involves the reference to extra-theoretical

[39] Hallett 1990, 223-243, Hallett 1995, 147-148.

elements and has a danger of degenerating into the "game of hide-and-seek"; in short, it is motivated by his concern for the objectivity of mathematics.

Hallett's account fits quite well with the textual evidence and seems capable of explaining why Hilbert insists on the necessity of a consistency proof while considering the notion of existence as being relative to the axiom system. However, as far as the notion of truth is concerned, it is not so clear whether Hilbert, as Hallett claims, thinks it necessary to justify the truth (taken in the external sense) of axioms and the propositions deducible from them through a consistency proof. That is to say, for the establishment of their truth, Hilbert does not seem to demand nothing over and above a proof within the relevant axiom system. For instance, consider the following remark in the 1922 essay:

> ... despite the application of the boldest and most manifold combinations of the subtlest techniques, a complete security of inference and a clear unanimity of results reigns in analysis. We are therefore justified in assuming those axioms which are the basis of this security and agreement; to dispute this justification would mean to take away in advance from all science the possibility of its functioning.... [Hilbert 1922, 200]

Hilbert here seems to be claiming that the fulfilment of the requirement of rigor (conceived in the sense we saw earlier) is just sufficient for the truth of the axioms of analysis.

But why, then, does he insist on the need for a consistency proof? Admittedly, this is not an easy question to answer. In considering this, I want to call attention to the not so remarkable point that Hilbert's axiomatization/rigorization program is designed to be applied to an existing body of "knowledge" and hence is not a revisionist program. In other words, it is assumed in advance that there is a set of "correct" propositions, whose "truth" his program is supposed to establish, and these propositions function as "norms," as it were.[40] It would seem then that an inconsistent axiomatization is undesirable for the reason that in it the negation of a "correct" proposition is deducible and thus the falsity of the "correct" one is provable (while, of course, the "correct" one is also deducible in such a system). In the 1905 lectures, Hilbert explains the source of our interest in the requirement of consistency along these lines:

> ... from any contradiction, no matter how far removed, we can prove the falsehood of every correct statement. Hence, we could say that *one* contradiction in the whole realm of our knowledge [*Wissen*] acts like a spark in the gunpowder barrel and destroys everything. Therefore, every science [*Wissenschaft*] must have an interest in dealing with a contradiction, no matter how far removed. [Hilbert 1905b, 217, quoted and translated in Hallett 1995, 151, emphasis in original]

Given Hilbert's denial of any extra-systematic denotation and meaning in his axiomatic method, the term "correct" here cannot be taken in the sense of some sort of correspondence between representation and the framework-independent object, and thus what he means by a "correct statement" should be read as a statement that is commonly accepted as a truth by the practitioners of the science in question. It would seem then that the requirement of consistency arises as a part of the general requirement of rigor, which states that the correctness

[40] In this connection, it might be recalled that, in the 1905 lecture notes, Hilbert defined the notion of completeness of an axiom system in terms of the deducibility of all the propositions commonly accepted as truths from its axioms.

of (all and only) correct results be established by means of a finite number of steps based upon a finite number of assumptions.[41]

41 In the 1935 article "Hilbert's investigations into the foundations of arithmetic," Bernays reports that Hilbert considered consistency as a requirement of rigor:

> ... the complete certainty of consistency is regarded by Hilbert as a requirement of mathematical rigor. [Bernays 1935, 201-202, my translation]
>
> [Die völlige Gewißheit der Widerspruchsfreiheit erachtet aber Hilbert als ein Erfordernis der mathematischen Strenge.]

Chapter IV

The Path to Hilbert's Program

§1. In the last chapter, we saw how, for Hilbert, the need for a consistency proof could and did arise from the reasons that have nothing to do with the epistemological concerns for certainty and truth (taken in the standard, direct realist sense). In the present chapter, we will examine how Hilbert actually tries to demonstrate the consistency of arithmetic in the 1905 essay. But before getting into this, I will first take a brief look at his first consistency proof presented in the *Foundations of Geometry* and consider some important points related to the problem of consistency. Now, if we start our discussion with the commonly accepted account in the current literature of this proof, it would typically look like this:

> Using techniques from analytic geometry, Hilbert (1899) constructed a model of all of the axioms using real numbers, thus showing that the axioms are 'compatible', or consistent. In contemporary terms, he showed that the axioms are satisfiable. [Shapiro 2000, 153]

That is to say, Hilbert proved the (relative) consistency of the axiom system of geometry by assigning to its primitive terms certain arithmetical objects in such a manner that its axioms all come out true under the proposed interpretation. The end of the story. To be sure, the standard, model-theoretic reading of Hilbert's consistency proof, I think, captures the kernel of his argument, but, in simply pigeonholing it in accordance with the modern understanding of semantics, we might be in danger of overlooking something of philosophical importance. To begin with a minor point, the Hilbert of *circa* 1900, as was briefly noted above in passing, had yet to arrive at what we today call the "syntax/semantics" distinction, and his conception of consistency in this early period is, strictly speaking, neither syntactic nor semantic, but, simply, deductive: in the 1899 essay, it is stated that a set of axioms is consistent if "it is impossible to deduce from them by logical inference a result that contradicts one of them" [Hilbert 1899, 29]. It should not be thought, then, that Hilbert's proof there consists in finding a "model" for the *disinterpreted* formulae of the formalized Euclidean geometry.

How then does Hilbert's proof proceed? As we saw above, while, in Hilbert's axiomatic method, the non-logical terms occurring in an axiom system are denied of extra-systematic denotation and meaning, they are not empty formalisms and do retain their descriptive character. But, on the other hand, since their meaning is determined solely by the logical relations formulated in the axioms, they can be considered as place-holders or variables, for which any concepts can be substituted. Hilbert thus writes in a letter to Frege that a theory can be considered as a "scaffolding or schema of concepts together with their necessary relations to one another" and that "the basic elements can be thought of in any way one likes."[1] It would seem then that the axioms of a theory, taken in the sense of the Hilbertian axiomatics, can be construed as *propositional functions*, from which we can obtain (true or false) propositions by substituting certain concepts

[1] Letter to Frege of 29 December 1899 in Frege 1980, 40.

for the variables occurring in them.[2] And, as the quotation indicates, Hilbert himself seems to consider axioms in this manner when dealing with questions concerning their independence and consistency.[3]

On this understanding of axioms, one way of establishing their consistency would indeed be to appeal to the so-called method of models, that is, to find a set of terms or concepts which, when substituted for the variables occurring in the axioms, converts all of these propositional functions into true propositions.[4] In the *Festschrift*, Hilbert constructs a model for his geometrical axioms using the concepts of the theory of the real numbers. As an illustration, let us consider (part of) the interpretation (call it I) and see how one of Hilbert's axioms comes out true on I. Of the five primitive terms found in Hilbert's axiom system, i.e., "point," "line," "on," "between," and "congruent" (as applied to segments and to angles), the two terms, "point" and "congruent" (as applied to segments), for instance, receive the following assignments by I:[5]

i) By a "point" we mean an ordered pair (x, y) of real numbers;

ii) The segment, denoted by $(x_1, y_1)(x_2, y_2)$, is said to

[2] For example, from the propositional function "x is an F," we can obtain a (true) proposition "Kafka is a cat" by substituting the individual concept *Kafka the cat* for x and the concept *cat* for F.

[3] By contrast, Resnik is of the opinion that Hilbert viewed the primitives as *schematic letters* rather than variables. See Resnik 1980, 112.

[4] In the 1905 essay, he calls this method "the method of a suitable specialization, or of the construction of examples" [Hilbert 1905a, 135].

[5] For the sake of brevity, here I list only the assignments for those two terms. In the full list, each of the five primitives receive an assignment by I.

be "congruent" to the segment, denoted by $(x_3, y_3)(x_4, y_4)$, if and only if

$$(x_2 - x_1)^2 + (y_2 - y_1)^2 = (x_4 - x_3)^2 + (y_4 - y_3)^2.$$

Under the assignments made by I, one of the axioms characterizing the concept of congruence, which states that "if two segments are congruent to a third one they are congruent to each other," would read: if two segments $(x_1, y_1)(x_2, y_2)$ and $(x_3, y_3)(x_4, y_4)$ are congruent to a third segment, denoted by $(x_5, y_5)(x_6, y_6)$, then they are congruent to each other. That is, if $(x_2 - x_1)^2 + (y_2 - y_1)^2 = (x_6 - x_5)^2 + (y_6 - y_5)^2$ and $(x_4 - x_3)^2 + (y_4 - y_3)^2 = (x_6 - x_5)^2 + (y_6 - y_5)^2$, then $(x_2 - x_1)^2 + (y_2 - y_1)^2 = (x_4 - x_3)^2 + (y_4 - y_3)^2$. It is fairly easy to see that the truth of this proposition follows solely from the relational properties of "=" by the rules of logic.[6] This is rather a trivial case, and, of course, there are many cases where much care and ingenuity is required to demonstrate the truth of the (arithmetical) propositions resulting from the substituting various concepts for the (predicate) variables occurring in the geometrical axioms in accordance with the proposed interpretation.[7] But the important point here is that, on the proposed interpretation, each of the geometrical axioms (qua propositional functions) is converted to a true proposition of the theory of real numbers, and that Hilbert

[6] Let us call the three segment "*a*," "*b*," and "*c*" respectively. What we have to do here then amounts to showing that if $a = c$ and $b = c$, then $a = b$. Suppose $a = c$ and $b = c$. It follows from $b = c$ by the reflexivity of "=" that $c = b$. Then, by the transitivity of "=," it follows from $a = c$ and $c = b$ that $a = b$.

[7] For more "interesting" cases, see Eves 1990, 92-98.

succeeded in establishing its truth in some appropriate manner and thereby proving the consistency of the axiom system.

This, however, is not the end of the story: we have yet to understand why and how precisely the *truth* of the geometrical axioms *on I* amounts to their consistency. One might think that truth clearly implies consistency (possibility of truth) and that no further explanation is necessary, but that is not so. To see this, consider in what sense the "interpreted" axioms are said to be "true" here. In the above example, we saw that one of the axioms characterizing the notion of congruence, when interpreted according to the assignments made by *I*, is true because it can be proved from the relational properties of "=" by the use of the logical rules. What we must recognize, however, is that, strictly speaking, both the axiom(s) defining the meaning of "=" and the rules of logic are a part of the axiom system characterizing the theory of real numbers. In other words, the proposition resulting from the proposed interpretation is true in the sense that it is implied by the axioms of this theory; it is a truth *of* the theory of real numbers. This circumstance might be more easily seen in cases where the demonstration of (the truth of) an "interpreted" proposition clearly involves the use of various axioms of arithmetic, but, the point is the same.

It follows that what we are really doing when constructing a model for an axiom system *A* is to assign to the primitive terms of *A* concepts of some other axiom system *B* in such a way that the axioms of *A* are logical consequences of the axioms of *B*. What is implicit in the process of "constructing" a model is then the recognition that the logical relations holding among the non-logical concepts occurring in the axioms of *A* are *structurally similar* to those holding among the non-logical concepts occurring in a certain set Γ of theorems of *B*. Furthermore, since the process of deduction depends in all its parts solely upon these logical relations, and thus completely independent of the non-logical content of the propositions, if a certain proposition P_A is deducible from the axioms of *A*, then its counterpart or dual P_B (i.e., the proposition structurally similar to P_A) is deducible from Γ, and hence from the axioms of *B*. Thus, if a contradiction is ever deducible in *A*, then a contradiction must arise in *B* as well. To put this contrapositively, a contradiction never arises in *A* insofar as it is impossible to deduce one in *B*; or, more simply, *A* is consistent if *B* is consistent. That is, in assigning to the basic terms of an axiom system *A* concepts of some other axiom system *B* and further demonstrating the truth of the resultant propositions on this interpretation, we thereby establish the consistency of *A conditional upon* the consistency of *B*, or, to put it differently, we thereby reduce the consistency of *A* to that of *B*. That Hilbert sees the matter in this way can be confirmed by his remark in the Paris address "Mathematical Problems," where he summarizes the content and scope of his consistency proof for the geometrical axioms:

> In geometry, the proof of the consistency of the axioms can be effected by constructing a suitable field of numbers, such that analogous [*analoge*] relations between the numbers of this field correspond to the geometrical axioms. Any contradiction in the deductions from the geometrical axioms must thereupon be recognizable in the arithmetic of this field of numbers. In this way the desired proof for the consistency of the geometrical axioms is made to depend upon the theorem of the consistency of the arithmetical axioms. [Hilbert 1900b, 1104]

Now, does this mean then that any consistent proof by the method of models is a relative consistency proof and that the method of models is incapable of establishing the consistency of an axiom system in the absolute sense? Yet, there seems to be cases in which we feel convinced to have established the absolute consistency of a set of sentences in this method. More specifically, we seem to feel this way especially when the domain of a model is finite, that is, the domain specified by an interpretation consists of only finite number of elements. In such a case, we can, in principle, *verify* the truth of a sentence on some interpretation *I* without resorting to the method of proof (which involves the use of the rules of inference) even if the sentence in question is a quantificational sentence. One might think then that if it is possible to construct a finite model for an axiom system, such a model, when constructed, would establish the absolute consistency of the system. This, however, is not true. Neither the finitude of the size of a model nor the verifiability of the "axioms"[8] on *I*, in itself, implies the possibility of an absolute consistency proof. The first point to be stressed is that the verifiability of the axioms of a system (on *I*), while enabling us to establish their truth without resorting to the method of deduction from the axioms of some other system, does *not* make their "truth" absolute or system-independent.

Suppose we try to establish the consistency of the sentence(-form) "There exists an *x* such that *x* is *R*-related to

[8] I.e., the propositions resulting from the assignments by *I* of certain concepts to the variables occurring in the axioms (which are taken to be propositional functions).

b" by giving the interpretation *I* which assigns 2 to "*b*" and the "less-than" relation to "*R*" and whose domain is {1, 2, 3}. The sentence(-form), when interpreted, would be converted to something like: "There exists a number (in the domain) such that it is less than 2." This sentence is logically equivalent to the sentence "1 is less than 2 or 2 is less than 2 or 3 is less than 2," and its truth (on *I*) can be verified by checking the truth-value of the three disjuncts one by one and showing that at least one of them is true (on *I*). What is to be seen here is that, while, in this case, the truth of "$(\exists x)Rxb$" under the proposed interpretation can be established without any explicit reference to an axiom system, this does not mean that its truth is to be taken in some system-independent sense. Indeed, considered strictly, the truth of the interpreted sentence is relative to the theory of number insofar as what those objects and relations which the sentence is about are formulated by the axioms of that theory. Consequently, the impossibility of deducing a contradiction from the sentence in question and thus its consistency, despite the verifiability of its truth (on *I*), depends upon the consistency of the background theory, i.e., the theory of number, in terms of whose concepts the proposed model is constructed.

In fact, this is nothing more than the recapitulation of what we saw above. That is, constructing a model for an axiom system *A* is tantamount to finding a set of propositions Γ which are logical consequences of the axioms of some other system *B* and are structurally similar to the axioms of *A*. Hence, no matter whether the domain of the theory characterized by the axiom system *B* consists of finite number of elements,[9] the fact

[9] More precisely, as a set may be finite, denumerable, or uncountable, so a model is finite, denumerable, or uncountable.

remains that whatever is implied by the axioms of *A* is implied by Γ and by the axioms of *B*, and thus, *A* is consistent *if B* is consistent. It is true that if we had already established the consistency of *B* in the absolute sense, i.e., without reducing it to the consistency of yet another system, then the construction of a model for *A* using concepts of *B*, in conjunction with the knowledge of *B*'s consistency, would establish the absolute consistency of *A*. But since the domain-size of a theory obviously has no bearing on its consistency, the finitude of (the domain of) *B* does not imply the (absolute) consistency of *B*, and thus all that is achievable by the construction of a finite model of *A* in *B* is no more than the consistency of *A relative to* that of *B*.

Despite all this, however, it is to be noted that there exists, or so we commonly believe, a way in which we can establish the absolute consistency of an axiom system by means of the method of models. As we just saw, the construction of a model could establish the absolute consistency of an axiom system *A* if (and only if) it is accompanied by the knowledge of the absolute consistency of the background theory from which the meanings assigned to *A*'s primitive terms are adapted. What is to be seen here is that we are in possession of such a system whose absolute consistency is "given" to us, as it were. It is the real world, or more precisely, the set *W* of the propositions representing the states of affairs constituting it. To be sure, *W* is not a deductive system. But, no matter what exactly is meant by the term "the real world," we believe that in it there exists no contradictions. Correspondingly, the system consisting of the propositions representing the real world, we believe, is free of contradiction and is consistent in the absolute sense.[10] Thus, if an interpretation is devised for the axioms of *A* in such a manner that the resulting propositions are members of *W*, then, given the consistency of *W*, *A* is shown to be consistent.[11] This perhaps explains why we feel to have established the absolute consistency of a collection of propositions when we have come up with a "concrete" model. It is to be recognized, however, that, even in such a case, what accounts for the consistency of the propositions in consideration is not their "truth" on the proposed interpretation *per se*, but rather the structural similarity holding between them and some members of *W* and the consistency of *W*.

§2. At this point, let us go back to Hilbert's consistency proof for his geometrical axioms in the 1899 *Festschrift*. As we saw above, it consists in assigning objects and relations adapted from the theory of real numbers to the geometrical axioms considered (implicitly) as a set of propositional functions. But since it is conducted in accordance with the method of models, all that it achieves is a consistency proof for geometry in the relative, conditional sense. The question then arises of the consistency of the arithmetical axioms, to which the consistency of the geometrical axioms is reduced. Once again, one might try to

[10] That is, W's consistency does not depend upon the consistency of some other system.

[11] It might be said that such a model need not correspond to the *actual* states of affairs and that all that is required here is its correspondence to some "possible" states of affairs. This is probably so. But my sole intention here is to point to our belief in the possibility of an absolute consistency proof by means of the method of models and to explain how this is supposed to work, and, accordingly, here I do not embark on the logico-metaphysical considerations which are required to unpack the exact content of the "possibility" contained in our "intuition."

achieve this by constructing a model and reducing the consistency of arithmetic to that of some other branch of mathematics, but we cannot go on like this for ever: in this way we would never be able to "discharge" the antecedent of the conditional statement "*A* is consistent *if B is consistent*." For this and the other reasons we saw earlier, Hilbert came to think that not only is a consistency proof necessary for the arithmetical axioms, but also "a *direct* method is needed for the proof of the consistency of the arithmetical axioms" [Hilbert 1900b, 1104, my emphasis]. How, then, did Hilbert tackle this task?

In light of the above consideration, a consistency proof in the absolute sense can be obtained for an axiom system if a concrete model is constructible. But, needless to say, as far as the axioms of arithmetic is concerned, this path does not seem open. As Dedekind's and others' investigations into the foundations of analysis revealed, the construction of arithmetic (in the sense of the theory of real numbers) requires, at minimum, the notion of the totality of natural numbers as a completed set. Dedekind, as we saw, believed to have established the consistency of this notion by the method of models, but it involved the use of the problematic concept and resulted in complete failure. As he was later to maintain vehemently and repeatedly, Hilbert was, from the outset, of the view that "the infinite is nowhere to be found in reality" [Hilbert 1926, 201] and did not consider model-construction as a solution to the problem. Thus, when he says in the quotation that a "direct" method is needed for the task, he specifically means a method that does not involve modelling.

In the Paris address, Hilbert claims that he is "convinced" that a direct proof for the arithmetical axioms can be obtained "by means of a careful study and suitable modification of the known methods of reasoning in the theory of irrational numbers" [Hilbert 1900b, 1104].[12] But it was not until in the 1905 essay that he presented the outline of a direct consistency proof. Its main idea is expressed in a deceivingly simple statement: we can "consider the proof itself as a mathematical object" [Hilbert 1905, 137]. As we saw above, the method of definition by axioms enabled Hilbert to formulate the consistency problem as that of the consistency of an axiom system and thus of the impossibility of deducing a contradiction in it. With this "deductional" conception of consistency in hand, Hilbert came to the realization that the consistency of an axiom system can be established without reducing it to the consistency of some other system if it is possible to show that, by following the rules of inference employed in mathematics, no two propositions can be derived in the axiom system that contradict each other. More precisely, his task is to prove a (metatheoretical) statement *about* the proofs constructible in the axiom system of arithmetic that, for every such proof, it is not the case that a finite number of inferential steps based upon the axiom set can lead to contradictory results. Formulated thus, this way of proving consistency is relative to the *logical* framework of the axiom system in consideration insofar as any small change in the rules of inference could affect the result of the proof, but it can establish the desired result directly, i.e., without regard

12 In "On the Concept of Number," Hilbert writes: "To prove the consistency of the above axioms [for arithmetic], one needs only a suitable modification of familiar methods of inference" [Hilbert 1900a, 1095].

to the consistency of a second system.[13]

And this is not all. In attempting to achieve this goal, Hilbert introduces a completely new way of tackling the problem. Instead of meditating on the content of the propositions (or propositional functions) constituting an axiom set and the totality of the propositions deducible from them by the inference rules in some *conceptual* manner, Hilbert proposes to consider the axiom system in its outer garments, as it were. More precisely, he assumes[14] that propositions are made up of what he calls "thought-objects" [*Gedankendinge*] and proposes to consider the "syntactic" properties of the propositions qua the combinations of such objects by abstracting from their meaning-content. With this switch of the viewpoint, Hilbert tries to solve the problem of consistency.

Let us now briefly consider how he thought this "direct" consistency proof would proceed by following the sketch he drew in the 1905 paper.[15] According to this sketch, the proof starts with the introduction of two "thought-objects," 1 (one) and = (equals), and we consider all the (finite) combinations generable by concatenation from these simple objects. For example,

$$1 =,\ (11 =)(= =1),\ ((11)(1)(=))(= =),\ 1 = 1$$

are such combinations. (Simple) thought-objects and the combinations generable from them are the "building blocks" of

[13] Or so it seemed to the Hilbert of *circa* 1905.

[14] Strictly speaking, it is not quite clear whether he "assumes" that this is the case or is actually of the opinion that propositions consists of thought-objects.

[15] For the sake of clarity and brevity, the proof is slightly modified.

arithmetical propositions, but, at this point, none of these should be thought to have meaning. At this point, Hilbert introduces the logical connectives: "$\neg$" for negation,[16] "*u.*" for conjunction, "*o.*" for disjunction, "$|$" for implication, and the "$A(x^{(o)})$" and "$A(x^{(u)})$ for existential and universal quantification over proposition *A* containing the "arbitrary" *x*, respectively. Thus, well-formed formulae of the theory are, presumably, generated in the usual, recursive manner.[17] He then lists the two axioms characterizing the notion of = (equals):

1. $x = x$,
2. $[x = y \ u. \ w(x) \,|\, w(y)]$,[18]

where "*w*" denotes some (unexplained) propositional function. The following three axioms are later introduced with the addition of three further thought-objects, **u** (infinite set, infinity), **f** (successor), and **f**′ (accompanying operation):[19]

3. $\mathbf{f}(\mathbf{u}x) = \mathbf{u}(\mathbf{f}'x)$,
4. $\mathbf{f}(\mathbf{u}x) = \mathbf{f}(\mathbf{u}y) \,|\, \mathbf{u}x = \mathbf{u}y$,
5. $\neg[\mathbf{f}(\mathbf{u}x) = \mathbf{u}1]$,

[16] In Hilbert's paper, "-" (bar) is put on the top of a "formula" or a combination, rather than "$\neg$" being attached in front.

[17] No axioms are set up for the logical connectives, and their meanings seem to be understood in the customary sense. Incidentally, here the existential quantification is considered as the abbreviation of disjunction while the universal quantification as the abbreviation of conjunction.

[18] That is to say, if $x = y$ and $w(x)$, then $w(y)$.

[19] In Hilbert's original, *Fraktur* characters are used instead of bold letters.

where the "arbitrary" object x stands for any of the five thought-objects and the combination **u**x (e.g., **u**1, **u**(11), **uf**) is an element of the infinite set **u**. Although Hilbert does not say so, these axioms correspond to (part of) the axiom set Peano gave for the natural number system.[20] Very crudely, Axiom 3 states that the successor of an element of the infinite set **u** (or, more simply, the successor of a natural number) is a natural number; Axiom 4 states that two natural numbers are equal if their successors are; and Axiom 5 says that 1 is not the successor of any natural number.[21] As in the case of the first two axioms, these axioms contextually fix the meanings of the basic terms occurring in them.

How then is a proof carried out in this system? Hilbert mentions two rules of "inference." According to the first, a new formula can be introduced by substituting for the "arbitrary" objects x and y occurring in the above axioms any arbitrary combination of the five simple objects taken as primitive. The second corresponds to the so-called "law of syllogistic reasoning": from A|B and B|C, A|C can be obtained.[22] These rules are apparently considered as what we today call the rules of transformation; for here there is no mention of "implication" or "truth-preservation."

With all this machinery set up, we are now ready to explain Hilbert's direct consistency proof. As was stated earlier, his goal is to demonstrate that, by following the rules of inference antecedently specified, no two propositions

20 Peano, in turn, based his axiom set upon Dedekind's presentation.

21 The point was duly observed in Poincaré 1906a, 1041. Incidentally, the Peano axiom set includes two more axioms, the principle of complete induction and the one that states that 1 is a natural number.

22 Later in the 1905 essay, Hilbert calls these two rules those of "specialization" and "combination," respectively.

can be derived in the axiom system that contradict each other. And since, in this system, two contradictory thoughts are expressed by two combinations of the forms a and $\neg a$, respectively, its consistency can be established by showing that such a pair of combinations can never be obtained from the axioms (taken in the sense of the axiom-schemata) by the applications of the inference rules (taken in the sense of the transformation rules).

Now, the idea behind the proof is as follows. As a close look at the five axiom(-schemata) and the transformation rules will reveal, Axiom 5 is the only one giving rise to propositions or combinations of the form $\neg a$ (since the other four axiom-schemata are all of the form a and since there is no rule allowing us to introduce $\neg$). It follows that a contradiction arises if and only if it is possible to derive a combination of the form:

$$6.\quad \mathbf{f}(\mathbf{u}x^{(o)}) = \mathbf{u}1.$$ [23]

But such a combination cannot result from Axioms 1-4 in any way. More specifically, Hilbert tries to establish this by introducing the definition of the following syntactic property: the equation or the combination of the form $a = b$ is called a "homogeneous" equation if a and b consists of the same number of thought-objects. For example,

$$(11) = (\mathbf{f}\mathrm{u}),\ (\mathbf{f}11) = (\mathbf{u}1 =),\ (\mathbf{fu}111 =) = (\mathbf{uu}111\mathbf{u})$$

[23] (The associated proposition of) 6 states that the combinations of the form $\mathbf{f}(\mathbf{u}x^{(o)}) = \mathbf{u}1$ belong to the class of beings, whereas (the associated proposition of) Axiom 5 states that they belong to the class of non-beings.

are all homogeneous equations. With this property in hand, Hilbert then proves the impossibility of deriving a contradiction by showing that every combination derivable from Axioms 1-4 by the transformation rules is a homogeneous equation while any combinations of the form specified in 6 are not a homogeneous equation and thus cannot be obtained from Axioms 1-4 by the use of the transformation rules.

To recapitulate the general features of this proof, it is designed to establish the consistency of an axiom system through the consideration of the syntactic entities employed to represent this system. For this purpose, a certain syntactic property is defined. To be more precise, it is defined to be such a property that if it belongs to the combination expressing a proposition *A*, it does not belong to the combination expressing the proposition that forms a contradiction with *A*.[24] It is then shown that every combination derivable from the axioms through the inference rules has this property.[25] More specifically, it is shown that the combinations obtainable through the application of the substitution rule to the axiom-schemata have the property and that the transformation rules, when applied to these combinations, produce combinations that also have the property. Thus, if a combination is derivable from the axiom-schemata, it has the property. But since, in such a case, its "contradictory" combination, by definition, does not have the property, it is not derivable from the axiom-schemata. Hence, it is impossible that combinations expressing a proposition and

[24] In Hilbert's proof, a complication arises because the sentences of the theory are associated with certain "artificially" concocted propositions.

[25] For the sake of explanation, the description slightly differs from Hilbert's proof in the 1905 essay.

any one contradictory to it are both derivable from the axiom-schemata.

There are a few things to be noted about Hilbert's proposal for a direct consistency proof made in the 1905 essay. First and foremost, as the above observation seems to indicate, some of the main features of his later consistency program, albeit very vaguely, are already detectable in this early attempt. The axioms of arithmetic (in the sense of the theory of natural numbers) are first presented with the rules of inference which are employed in proofs. We then completely abstract from the meaning-content of these propositions, focus on the syntactic features of these complex objects, and investigate proofs themselves as finite (syntactic) objects. By means of this "abstractive" procedure, which Hilbert later calls "formalization," and the subsequent syntactic consideration, the problem of consistency is then formulated and treated as the purely syntactic problem of the (un)derivability of a certain formulae in the axiom system as formalized. Second, the idea of studying mathematical proofs by abstracting from their content requires that the logic and the logical language employed in mathematical reasoning be specified. In keeping with his axiomatic treatment of geometry and arithmetic, what Hilbert should have done is first to "rigorize" our knowledge of logic in the manner of his axiomatic method and put them into such a condition that purely syntactic consideration is applicable. To be sure, in the essay, Hilbert emphasizes the need for "a partly simultaneous development of the laws of logic and arithmetic" and develops "logic" with arithmetic proper within one common frame. But the logic developed there is obviously too meager to cover all those inferential processes found in mathematical proofs, and the extra-systematic meanings of the logical terms are still presupposed.[26] [without axiomatization, logical terms become symbols without "formal" meaning.]

This lax attitude toward the logical framework of arithmetic, combined with the absence of a clear distinction between theory and metatheory, accounts for the ultimate failure of Hilbert's attempt in the 1905 essay. As we saw above, at the heart of Hilbert's direct, syntactic consistency proof lies the insight that there is a certain (syntactic) property that is shared by every formula derivable from the axioms of a (formal) system but not by its "contradictory" formula. Hilbert tries to establish this by showing that the initial items have such a property and that the formal rules of inference transmit the property, that is, items obtained through the application of the rules also have the property. In other words, in trying to establish the consistency of a (partial) system of arithmetic, Hilbert employs the so-called principle of mathematical induction. This, however, raises a serious problem for him. For since his ultimate goal is to obtain a proof for the consistency of full arithmetic and since any appropriate axiomatization of arithmetic would include the

[26] Towards the end of the essay, Hilbert talks of the need for adding "the familiar modes of logical inference" such as $\{(a \rightarrow b) \& (\neg a \rightarrow b)\} \rightarrow b$ and $\{(a \vee b) \& (a \vee c)\} \rightarrow \{a \vee (b \& c)\}$ [Hilbert 1905a, 131].

induction principle,[27] to use this principle in the proof would be to presuppose the legitimacy of one of the very principles he is supposed to establish; in short, his approach to the foundations of arithmetic would be circular.

§3. As is well known, the apparent circularity involved in the use of the induction principle in Hilbert's 1905 attempt was first pointed out by another great mathematician of the time, Henri Poincaré, in a series of articles which he published in 1905 and 1906 in response to the recent rise of the logicist movement in all over Europe.[28] Seen from a technical perspective, Poincaré's criticism gave Hilbert an occasion to reflect upon what is involved in metatheoretical investigations and thus to develop the inchoate effort of 1905 into the sophisticated, proof-theoretic project of the 1920s. Accordingly, in the literature, it is usually touched upon in

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[27] That is, if one does not choose to take a set-theoretical approach. As was briefly noted above, in Dedekind's (and in Frege's) attempt, the induction principle is deducible from the concept of number, which, in turn, is defined in terms of logic (including set theory). In addition to the discovery of set-theoretical paradoxes, Hilbert refers to the following point to explain his reservation about the (traditional) logicist standpoint:

> If we observe attentively, however, we realize that in the traditional exposition of the laws of logic certain fundamental arithmetic notions are already used, for example, the notion of set and, to some extent, also that of number. Thus, we find ourselves turning in a circle, and that is why a partly simultaneous development of the laws of logic and of arithmetic is required if paradoxes are to be avoided. [Hilbert 1905a, 131]

Note, however, that Hilbert here considers the notion of set to be arithmetical.

[28] Three articles are published under the title "*Les mathématique et la logique.*" Louis Couturat, who was a chief proponent of the tenet in France, was apparently Poincaré's main target in these articles. In addition, Cantor, Zermelo, Russell, Peano, and Hilbert, among others, were all criticized to varying degrees.

the context of the pre-history of Hilbert's program and often explained as merely a technical obstacle which is to be overcome by Hilbert's later distinction between two types of induction. To treat Poincaré's objection in this way, however, is to fail to recognize the philosophical significance it has for the subsequent development of Hilbert's foundational views. For this reason, I shall discuss the dispute briefly in what follows.

At first glance, it might seem that not only does Poincaré's interest in foundational issues of mathematics agree with Hilbert's, but also the former approaches these issues in essentially the same manner. Of the "formalist" tendency found in the "new mathematics," Poincaré says the following:

> What strikes us in the new mathematics is its purely formal character: 'We think,' says Hilbert, 'three sorts of things, which we shall call points, lines, and planes. We stipulate that a line shall be determined by two points, and that in place of saying this line is determined by two points, we may say it passes through these points, or that these two points are situated on this line.' What these things are, not only we do not know, but we should not seek to know. We have no need to, and one who never had seen either point or line or plane could geometrize as well as we. ... Thus, be it understood, to demonstrate a theorem, it is neither necessary nor even advantageous to know what it means. ... I do not make this formal character of his geometry a reproach to Hilbert. This is the way he should go, given the problem he set himself. He wished to reduce to a minimum the number of the fundamental assumptions of geometry and completely enumerate them .... [Poincaré 1905, 1024]

And how should we understand such fundamental assumptions? According to Poincaré, (some of) these assumptions should be thought of as the so-called "definition by postulates,"[29] which formulate the fundamental relations uniting the basic notions of a system and which enable us to demonstrate all their other properties:[30]

> Thus certain indemonstrable axioms of mathematics would be only disguised definitions. This point of view is often legitimate; and I have myself admitted it in regard for instance to Euclid's postulate. [Ibid., 1026]

Given that these postulates are merely "disguised definitions," they are not, as were traditionally thought, self-evident truths about a certain subject-matter and are neither true nor false. This, however, does not mean, says Poincaré, that the introduction of a system of postulates is a completely arbitrary matter and that there needs to be no justification for their introduction. What we must see here, in his view, is that, while postulates are nothing more than definitions, definitions, in fact, involve the assumption of the existence of the defined object. But, since mathematics is independent of the existence of material objects, the meaning of the term "existence" in mathematics can mean only "freedom from contradiction." It thus follows that "in defining a thing, we affirm that the definition implies no contradiction" [Ibid., 1026]:

> 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.

[29] For the reason we shall see shortly, Poincaré reserves the term "axiom" for some other use.

[30] I.e., axioms taken in the sense of Hilbertian axiomatics.

[Ibid., 1026]

Moreover, because elsewhere Poincaré claim that "to have the right to lay down a system of postulates, we must be sure they are not contradictory," it appears that he considers consistency as a necessary and sufficient condition for the legitimacy of concept-formations.

How then does he think the consistency of a postulational system can be established? Poincaré lists three possible ways of showing that a definition implies no contradiction. The first is the method of models. More specifically, he seem to have in mind here a proof by concrete model, as is indicated by the remark that "such a direct demonstration by example is not always possible."[31] If a concrete model cannot be found, "it is necessary," says Poincaré, "to consider all the propositions deducible from these postulates considered as premisses, and to show that, among these propositions, no two are contradictory."[32] This then corresponds to Hilbert's direct method. According to him, this method can be subdivided into two cases. On the one hand, the number of the propositions deducible from the postulates in question might be finite. In such a case, we can directly verify the non-occurrence of such a pair. This case, however, is "infrequent and uninteresting," says Poincaré. The number of the deducible propositions, on the other hand, might be infinite. If this is the case, a direct verification is no longer possible. And thus, in order to establish the underivability of contradictory propositions, we must resort to the so-called "principle of complete

31 Poincaré 1905, 1026.

32 Poincaré 1905, 1026.

induction," which, according to Poincaré's formulation, reads:

> If a property be true of the number 1, and if we establish that it is true of $n + 1$ provided it be of n, it will be true of all the whole numbers. [Poincaré 1905, 1025]

Now, as was noted above, a problem seems to arise in the case where the "postulates" whose consistency we are trying to establish by this method are those of arithmetic. Since, in this case, the first two methods are unable to provide the desired result, the third method must be employed for the proof. But to do so is to try to establish the consistency of a system of postulates by appealing to the very principle whose legitimacy is in question. Poincaré points out that this is what happens and what is overlooked in the consistency proof Hilbert put forward in the 1905 essay. Poincaré illustrates his point by referring to one part of Hilbert's proof, where the latter says that the two axioms characterizing the notion of = (equals) do not lead to a contradiction. To list those axioms once again, they are:

1. $x = x$, and
2. $[x = y\ u.\ w(x) \,|\, w(y)]$.

Very roughly, Hilbert's proof shows that all the propositions deducible from these axioms by the two inference rules are of the form $\alpha = \alpha$[33] and hence that these propositions cannot be contradictory. To this Poincaré poses the following question:

[33] Hilbert says that this is the case if we select from all the "consequences" of the axioms those that have the simple form of the proposition *a* (assertion without supposition), and thus excluding conditionals.

> But how does he know that all these propositions are identities? We consider a series of consequences deduced from our axioms, and we stop at a certain stage in this series; if at this stage we have so far obtained nothing but identities, we can verify that, by applying to these identities any of the operations permitted by logic, we can obtain only new identities.
> One concludes that one can never obtain anything but identities; but *to reason thus is to employ complete induction.* [Poincaré 1906a, 1041, emphasis in original][34]

Certainly, given that Hilbert's goal is to prove the universal statement that, for every proof in arithmetic, it is not the case that a contradiction is deducible from the axioms by means of the usual rules of inference in a finite number of steps, it appears necessary to apply complete induction. But we are not entitled to make this move because, as Poincaré accentuates, "*we do not yet know the principle of complete induction.*"[35]

Now, providing Poincaré is right about this, does it mean that a proof for the absolute consistency of the arithmetical axioms cannot be obtained? As far as Poincaré himself is concerned, this might appear to be the case. Towards the end of the second essay, he states that, for the principle of induction, the demonstration of consistency is impossible.[36] But, here we should pay as much attention to the *conclusion* Poincaré draws from this impossibility. As we saw above, in his view, postulates are "disguised definitions"; definitions involve the assumption that they imply no contradiction; and thus the laying down of a postulational system, if it is to be

[34] Later in the same paper, Poincaré points to another use of complete induction in Hilbert's proof for the consistency of the five axioms for arithmetic, which make use of the notion of "homogeneous equations."

[35] Poincaré 1905, 1033, emphasis in original.

[36] Poincaré 1906a, 1049. However, as we shall see shortly, Poincaré's claim is, in truth, a conditional one.

legitimate, requires the demonstration of consistency. A closer look at the text, however, would reveal that with this we have seen only one half of Poincaré's thesis. In fact, the passage quoted above, when continued, reads as follows:

> 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. *If we cannot demonstrate that, it must be admitted without proof, and that then will be an axiom; so that, seeking the definition under the postulate, we should find the axiom under the definition.* [Poincaré 1905, 1026, my emphasis]

Provided that we cannot demonstrate the (absolute) consistency of a certain indemonstrable proposition of a science, what this means, argues Poincaré, is not that we cannot establish the legitimacy of the definition (implicitly) formulated by the proposition (qua a postulate). Rather, it means that, in this particular case, the assumption underlying the demand for a consistency proof is false. That is, it means that the proposition in question must not be viewed as a postulate or a definition in disguise. And, in Poincaré's view, this, in turn, means that the proposition in question is an "axiom"--a *truth* in some robust sense;[37] to be sure, there is only one such case, i.e., the principle of complete induction,[38] but the

[37] I shall shortly try to explain what precisely is meant by the claim that the principle of complete induction is an axiom, a truth, and a synthetic *a priori* judgment.

[38] More precisely, it is what Poincaré calls "pure intuition," which underlies the truth of induction principle and of all other analogous principles employed in mathematics.

point is that there *is* such a case.[39]

Indeed, to argue for the truth of induction principle, this is one of the primary purposes of Poincaré's polemic against the "logicist" standpoint, to which, he thinks, Hilbert's foundational attempt belongs. Accordingly, we should not think that Poincaré's point is just that the demonstration of consistency is impossible in the case of the arithmetical axioms. Rather, his argument is to be seen as having the form of disjunctive syllogism. Induction principle is *either* a postulate (and thus a disguised definition) *or* an axiom. Suppose the former is the case, and that induction principle is a postulate. We then have to obtain a consistency proof for its legitimacy. But since this leads to the unacceptable result, i.e., the impossibility of demonstrating the (absolute) consistency of arithmetic and thus of the whole mathematics, we should reject the assumption that induction principle is a disguised definition and accept that the principle is an axiom. Consequently, we find Poincaré maintaining the impossibility of such a demonstration in one context, while speaking of its possibility in another. This can be clearly seen in his dispute with Couturat.

Against the claim Poincaré made in the first essay that the logicists's attempt to view the induction principle as a disguised definition requires a justification by a consistency proof, Couturat retorted that postulates are presumed to be free from contradiction until the contrary is proved, and thus that the *onus probandi* actually rests upon "those who believe

39 By appealing to the first half of the quoted passage, Sieg speaks of Poincaré's agreement with Hilbert on the "fundamental" point that mathematical existence means only freedom from contradiction. But, in simply ignoring the second half, I think that he misrepresents the former's position. See Sieg 1999, 7.

that these principles are contradictory."[40] To this Poincaré replied in the third essay with the following words:

> Needless to add, I do not assent to this claim. But, you say, the demonstration you require of us is impossible, and you cannot ask us to jump over the moon. Pardon me; that is impossible *for you*, but *not for us*, who admit the principle of induction as a synthetic judgement *a priori*. And that would be necessary for you, as for us. [Poincaré 1906b, 1056, my emphasis]

Poincaré's point here is twofold. First, there are cases in which the basic propositions of a field of knowledge are postulates and mere disguised definitions, as in the case of geometry. And, in such cases, the legitimacy of postulates or, more precisely, of the assumption made in them about the (mathematical) existence of the defined objects *must* be justified through a proof that they imply no contradiction. Second, the demonstration of consistency (in the absolute sense) requires the truth of induction principle and is impossible *insofar as* one takes it to be a postulate. But once the truth of the induction principle is acknowledged, a consistency proof can be obtained.

Immediately after the quoted passage, Poincaré also explains what precisely his criticism of Hilbert's attempt in the 1905 essay consists in. Speaking strictly, Poincaré's intent is neither to dismiss the latter's consistency proof on account of the circularity involved in the (implicit) use of

40 Louis Couturat, "*Pour la logistique* (*résponse á M. Poincaré*)," *Revue de métaphysique et de morale*, **14** (1906), 208-250; quoted in Poincaré 1906a, 1056. By this remark, Couturat seems to insinuate that Poincaré's claim about the impossibility of a consistency proof implies that the latter is of the opinion that the postulates in question are contradictory. This, I think, suggests that Couturat failed to see the true intent of Poincaré's polemic.

the induction principle in it nor to argue for the impossibility of a consistency proof for the arithmetical axioms. Rather, Poincaré's objection has to do with Hilbert's failure to acknowledge the truth of the induction principle:

> What I have blamed Hilbert for is not his having recourse to it [the principle of complete induction] (a born mathematician such as he could not fail to see a demonstration was necessary and this the only one possible), but his having recourse without recognizing the reasoning by recurrence. [Poincaré 1906b, 1056]

In other words, Poincaré is prepared to accept the correctness of Hilbert's "direct" (syntactic) method as a solution to the problem of consistency once the latter recognizes the synthetic *a priori* character of the induction principle.

§4. So how did Hilbert respond to Poincaré's "suggestion"? Given Poincaré's conception of mathematical existence in terms of freedom from contradiction and his recognition of the need for a consistency proof, one might wonder whether Hilbert could not concede to Poincaré's claim without giving up his goal, that is, whether he could not accept the (extra-systematic) truth of the principle of complete induction (at the level of metamathematics) *and* try to construct a consistency proof for arithmetic on this minimal basis. What we must recognize here, however, is this. Hilbert adopted the axiomatic method in order to implement what he called the logical grounding of our mathematical knowledge. And in so doing, he considered the axioms of a deductive system as having no extra-systematic denotations and meanings. As a result of this methodological move, the problem of consistency was brought to the fore, and

Hilbert subsequently introduced, although still in an unclear manner, the method of formalization so that an absolute consistency proof for arithmetic could be obtained through metamathematical investigations into syntactic properties of formalized axiom systems. It would follow that, for Hilbert, to accept Poincaré's suggestion and to admit, albeit for the restricted purpose of metamathematical investigations, that the principle of induction represents a certain system-independent content is nothing other than to abandon the methodological principle underlying the axiomatic method and the very need for a consistency proof. In fact, without a clear distinction between theory and metatheory, the system-independent truth of induction principle would mean that arithmetic (even in the sense of the theory of the natural numbers) could not be treated in the manner of the axiomatic method.

Hilbert himself sees Poincaré's objection in this light and clearly understands that it not only is directed toward the alleged shortcoming in his consistency proof but also signifies a clash between two foundational standpoints, which are motivated by two fundamentally different concerns:

> Poincaré was from the start convinced of the impossibility of a proof of the consistency of the axioms of arithmetic. According to him, the principle of complete induction is a property of our mind--i.e. (in the language of Kronecker) it was created by God. [Hilbert 1922, 201][41]

In Hilbert's view, Poincaré is convinced of the primitive truth of the induction principle primarily on the philosophical ground that the knowledge of this principle is directly and immediately given to us, and he tries to establish this

[41] See also Hilbert 1928, 472-473.

philosophical thesis by arguing that a consistency proof cannot be obtained for the arithmetical axioms without presupposing the truth of the induction principle. As can be surmised from the above quotation, Hilbert thus sees a return of the Kroneckerian dogmatism in Poincaré's foundational standpoint:[42] just as Kronecker's failure to recognize the possibility of and the need for a logical grounding of arithmetic stems from his belief in the *a priori* knowledge of the natural numbers with their essential properties, so Poincaré's rejection of the possibility of a consistency proof for the arithmetical axioms stems from his adherence to what he calls the "Kantian" theory of mathematics, according to which our knowledge of mathematics is ultimately based upon synthetic *a priori* judgments and thus upon pure intuition.

It follows that if Hilbert is to maintain the foundational approach embodied in his axiomatic method, it is imperative that he provide a solution to the problem of consistency without presupposing the extra-systematic truth of induction principle. But how does Hilbert deal with Poincaré's argument which states otherwise? A short answer to this question is: in essentially the same manner as he attempted to refute Kronecker's dogmatism. That is, Hilbert tries to refute Poincaré's claim about the impossibility of a consistency proof for arithmetic by actually proving it in a *rigorous* manner, rather than by confronting it in the philosophical arena. More specifically, Hilbert tries to achieve this by formulating the question of consistency in such a way that it could be given a univocal solution in strict accordance with antecedently

[42] In the lectures of the summer term 1920, Hilbert portrays Poincaré as a "successor" and an "advocate" of Kronecker's foundational approach [Hilbert 1920b, 945]. On the other hand, a positive appraisal of Poincaré's work by Hilbert can be found in Hilbert 1930, 1164.

specified rules in a finite number of operations. Now, given the fact that what he has been seeking to achieve is to *prove* the consistency of the arithmetical axioms, this might seem to be a trivial point to make. But, as Hilbert was later to write, "all previous investigation into the foundations of mathematics fail to show us a way of formulating the questions concerning foundations so that an unambiguous [*eindeutige*] answer must result" [Hilbert 1922, 198], and thus, it was an epoch-making idea to formulate and treat such *meta*theoretical questions as that of consistency as purely *mathematical*[43] questions. How exactly, then, does Hilbert go about this task? Given its precise objective, the answer seems to suggest itself: a solution to the consistency problem should be sought by means of the axiomatic method. That is, set up an axiom system for the metatheory of ("formalized") arithmetic and formulate the problem of consistency as a problem pertaining to this system.[44] As it turns out, however, this is not the path Hilbert actually took. And to see why not, we must first take a closer look at what is really involved in the so-called "*petitio principii*" charge Poincaré raises against Hilbert's attempt in the 1905 essay.

To reiterate the point we saw above, the problem seems to

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43 Here I am using the term "mathematical" for whatever is capable of a rigorous treatment, "rigorous" being taken in the sense I explained in Ch.1. In this connection, it is of importance to note that, in one of the papers published in 1922, Bernays describes "the great advantage of Hilbert's procedure" as resting on the fact that "the problems and difficulties that present themselves in the grounding of mathematics are transferred from the epistemologico-philosophical domain into the domain of what is properly mathematical" [Bernays 1922a, 220]. Earlier in the same paper, Bernays characterizes mathematics as "the general theory of the *formal* relations and properties" [Ibid., 217].

44 More precisely, set up an axiom system for metatheory, derive the consistency statement from the axioms of this metatheory, and *apply* it to formalized axiom system of arithmetic.

arise for Hilbert in the following way. Hilbert's goal is to prove the consistency of the arithmetical axioms by carrying out a syntactic consideration on the formalized axiom system of arithmetic and by showing that a pair of formulae of the forms a and $\neg a$ are not derivable in this system. And since there are infinitely many "consequences" which are derivable from the axioms by the use of the inference rules, it appears necessary to apply the induction principle in order to establish that a "contradictory" pair of formulae are not derivable. But the induction principle is one of the axioms whose consistency is to be established by the proof, and thus the proof is circular.

The argument being so understood, one thing it calls our attention to is the inferential means employed in such a metatheoretical consideration and its reliability. To paraphrase this in the context of Hilbert's axiomatic method, one thing Poincaré's circularity charge brings out is the question as to the consistency of the axiom system for metatheory. Let us call the formalized axiom system of arithmetic, *A*, and the axiom system for metatheory, *MA*, in which the consistency proof is carried out. One objection to Hilbert's procedure in his 1905 proof would, then, be that the proof for the consistency of *A* in *MA* establishes nothing in the absence of the consistency of *MA*, for any proposition is provable in an inconsistent system. In other words, the question regarding the reliability of *MA* arises whether or not the induction principle is contained in *MA* as one of its axioms; for what is at stake is the consistency of *MA* as the framework for metatheoretical investigations.

What follows from this is that, strictly speaking, Hilbert's "direct" method, i.e., the method in which the consistency of an axiom system is established by means of the syntactic consideration on the formalized object-theory, provides a relative or conditional *proof* for the absolute consistency of the axiom system in consideration: it is a relative or indirect consistency proof in the sense that the validity of a proof for the consistency of an axiom system in consideration depends upon the consistency of the axiom system constituting the metatheoretical framework. By contrast, what we earlier called the method of models, if successful, provides us with a (direct) proof for the consistency of an axiom system relative to the consistency of another. As we saw above, what takes place in the model method when we construct a model for (a set of) axioms *A* is that we find (a set of) propositions Γ of some other theory *B* which are structurally similar to *A* and thereby show that a contradiction does not follow from *A* if a contradiction does not follow from the axioms of *B*, which imply Γ. In short, here we prove that A is consistent if B is consistent. Needless to say, such a logical relation does not necessarily obtain, in Hilbert's direct method, between an axiom system in consideration and the axiom system for metatheory. Accordingly, we might note the following point. In constructing a model for *A* in *B*, we "embed" (the structure defined by) *A* into *B*. Consequently, the structural complexity of *B*, i.e., the "host" theory relative to which the consistency of *A* is established, varies in accordance with the structural complexity of *A*. By contrast, in Hilbert's "direct" method, one and the same axiom system could provide a framework for metatheoretical investigations because, once formalized and considered as a sort of "alternating game"[45] of formulae, object-theories which differ widely in their "contentual" complexity would all be reduced to the same level of structural complexity.[46]

Now, given that Hilbert's "direct" method, if conducted in accordance with his axiomatic method, would require the consistency of the axioms of metatheory, our next question is whether the axiom system providing the inferential means for metatheoretical considerations is, in fact, consistent. And, of course, this is where Poincaré's claim about the indispensability of the induction principle in the syntactic consistency proof comes into consideration. Evidently, if the consistency of this principle could be established independently, its use at the meta-level would cause no concern. But, argues Poincaré, "we do not yet know the principle of complete induction," and therefore the consistency of the axiom system of metatheory, which includes the induction principle, must be proved in order to fulfil Hilbert's goal, i.e., the demonstration of the consistency of arithmetic. This, however, appears impossible since such a demonstration would have to be conducted in a direct manner and thus require a syntactic consistency proof, which, in turn, would presuppose the consistency of its meta-theoretical framework (i.e., meta-

45 Hilbert was later to use this phrase frequently in characterizing the nature of the formalized object-theory. See Hilbert 1923, 1138 and Hilbert 1928, 475.

46 Towards the end of the 1905 essay, Hilbert thus writes that, considered "as a stipulation expressible by formulas":

> the axioms for the totality of real numbers do not differ qualitatively in any respect from, say, the axioms necessary for the definition of the integers. In the recognition of this fact lies, I believe, the real refutation of the conception of the foundations of arithmetic associated with L. Kronecker and characterized at the beginning of my lecture as dogmatism. [Hilbert 1905a, 138]

meta-theory) and *ad infinitum.*

What we should do, then, might seem to be either to draw the negative conclusion that we are not certain of the consistency of arithmetic, and thus of the whole mathematics, or to assume or postulate the consistency of arithmetic, or of "reasoning by recurrence," as given.[47]

As we saw above, this, however, is not how Poincaré sees the state of affairs. According to him, the demand for a consistency proof arises only because the axioms of arithmetic, and especially the induction principle, are treated in the manner of Hilbert's axiomatic method. What the impossibility of a consistency proof (a la Hilbert) implies, in his view, is, then, the falsity of the underlying assumption that the arithmetical axioms can be considered as "disguised definitions." In other words, for Poincaré, they are truths. Furthermore, for him, since the induction principle is neither logical nor empirical, it "presupposes a new and independent act of our intuition and (why not say it?) a veritable synthetic judgement *a priori*" [Poincaré 1905, 1034].[48]

It appears, then, that if Poincaré is right, Hilbert has either to give up on the idea of providing a consistency proof for arithmetic or to accept the system-independent truth of the

[47] Janet Folina describes Poincaré's polemic from a slightly different angle:

> ... in order to justify the claim that a formal system is consistent--even one which encodes a nonstandard version of arithmetic--we effectively presuppose arithmetic (induction) at a higher level (in the metatheory). According to Poincaré, the necessity of induction at higher logical levels, belies its (and also arithmetic's) deep epistemological status. [Folina 1994, 213]

[48] According to Poincaré, pure intuition underlying induction principle constitutes a precondition for systematic thinking in general and thus even for logic. For more on his notion of intuition, see Folina 1992 and Folina 1994.

induction principle.[49] The former option implies that Hilbert's ideal of complete proof-structure and of the logical "grounding" of mathematics must also be abandoned. The latter, on the other hand, means that something must be assumed to be "known" in advance in mathematics.[50] This is the dilemma Hilbert was made to face by Poincaré's polemic.

[49] Strictly speaking, there is a third option: to maintain the Hilbertian axiomatic approach while considering the consistency of the induction principle as given or primitive. I shall come back to this point later.

[50] It might be recalled here that, in his letter to Frege, Hilbert made the following remark:

> I do not want to assume anything as known in advance [*Ich will nichts als bekannt voraussetzen*]. [Frege 1980, 39]

Chapter V

Mathematics as a Presuppositionless Science

§1. "For decades I have never lost sight of it."[1] This is what Hilbert wrote of the problem of the consistency of the arithmetical axioms in 1922, some seventeen years after the publication of his Heidelberg lecture, in which the idea of a "direct" proof was first put forward. As we saw above, the circularity involved in the use of induction principles in his "direct" proof was soon pointed out by Poincaré, and this presented a seemingly insurmountable obstacle for Hilbert's logical grounding of arithmetic. The only way out appeared to be to abandon the methodological principle guiding his foundational investigation: the total elimination of extra-systematic elements from mathematics. Hilbert would eventually "return" to the proof-theoretic approach sketched in the 1905 essay and finally find a way to escape Poincaré's dilemma but, in the interim, he explored alternative foundational approaches seeking a solution to the problem.[2] I cannot attempt here to delve into the complex and intriguing development Hilbert's foundational views underwent in the years between 1905 and 1922. Yet, in order to facilitate the subsequent discussion, I will briefly explain the importance of Russell and Whitehead's logicist program as developed in their *Principia Mathematica* (1910-1913).

In the late 1910s Hilbert thought (for a short period) that the logicists' proposal of reducing arithmetic to logic

[1] Hilbert 1922, 200.

[2] Sieg 1999 presents a detailed account of the development of Hilbert's thought between 1905 and 1922 through a careful examination of his lecture notes.

was *the* solution to the consistency problem. In "Axiomatic Thought" (1918), which is the only work on the foundational issues published during those seventeen years, Hilbert expressed his "conversion" to logicism in one single sentence:[3]

> The problem of the consistency of the axiom system for the real numbers can likewise be reduced by the use of set-theoretic concepts to the same problem for the integers: this is the merit of the theories of the irrational numbers developed by Weierstrass and Dedekind.
>
> In only two cases is this method of reduction to another 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 which it would be possible to invoke.
>
> *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.*
>
> This method was prepared long ago (not least by Frege's profound investigations); it has been most successfully explained by the acute mathematician and logician Russell. One could regard the completion of this magnificent Russellian enterprise of the axiomatization of logic as the crowning achievement of the work of axiomatization as a whole. [Hilbert 1918, 1113, my emphasis]

From Hilbert's viewpoint, a main advantage of the logicist approach lies in the following circumstance. The central component of Poincaré's polemic consists in the alleged indispensability of the induction principle as a means for carrying out a consistency proof. Thus, if it is possible to *prove* this principle within the theory which is going to receive the proof, we could take the sting out of Poincaré's *petitio* charge. Of course, in such a case, the consistency of

[3] It is to be recalled that, in the 1905 essay, Hilbert explicitly denied the viability of this alternative.

the object-theory in which the principle is proved would still be in question, but this would presumably pose no problem providing the object-theory is logic. Hilbert's fascination with the logicist program, however, did not last long, as he soon came to the realization that Russell and Whitehead's attempt to construct analysis from the resources of logic goes beyond the bound of what is logical with their introduction of the axiom of reducibility, and hence that the proposed "reduction" to logic is given only nominally.[4] Bernays explains the circumstance in this way:

> When Russell ... introduced the very cautious procedure of the calculus of types, it turned out that analysis and set theory in their usual form could not be obtained in this way. And thus Russell and Whitehead, in *Principia Mathematica*, saw themselves forced to introduce an assumption about the system of predicates "of the first type," the so-called "*axiom of reducibility.*"
>
> But hereby one again returned to the axiomatic standpoint and gave up the goal of the logical grounding. [Bernays 1922a, 216][5]

In short, the axioms of "logic" themselves require a consistency proof, and hence nothing is really gained by this

[4] A remark by Hilbert to this effect can be found in his lecture notes from this period. See Sieg 1999, 19.

[5] According to the standpoint motivating Russell's ramified theory of types, one takes for granted a domain of individuals with basic properties and basic relations between them, and, from this basis, all further predicates and relations are obtained, constructively, by the logical operations. The problem with the axiom of reducibility, in Hilbert's (and Bernays's) view, lies in the fact that, with the introduction of this axiom, the system of basic properties and relations must be expanded in such a way that the demand made by the axiom can be met, but such an expansion cannot be executed by a logical procedure. For more on this see Sieg 1999, 19. Mancosu 1999 provides additional, important information on Russell's influence on Hilbert and the Hilbert school by way of a detailed account of Heinrich Behmann's 1918 doctoral dissertation, *Die Antinomie der transfiniten Zahl und ihre Auflösung durch die Theorie von Russell und Whitehead*.

move. Thus, the idea of establishing the consistency of arithmetic by *reducing* it to logic was considered ultimately a failure by Hilbert.

Nevertheless, the axiomatization of logic carried out in *Principia* was of special importance to Hilbert's subsequent foundational investigations. As he later emphasizes, with the axiomatization (and the formalization) of logic, the modes of inference employed in mathematical proofs can be perfectly captured as purely symbolic operations, and thus "the mathematical *inferences* and *definitions* become a formal part of the edifice of mathematics."[6] The upshot is that mathematical proofs (or their formal counterparts) themselves become amenable to a theoretical consideration. In Bernays's words

> This procedure of the logical calculus supplements the method of the axiomatic grounding of a science, to the extent that such a procedure makes possible, along with the exact laying down of the *presuppositions* as it is brought about by the axiomatic method, an exact pursuit of the *inference modes* with the aid of which one proceeds from the principles of a science to its conclusions. [Bernays 1922b, 195-196, emphasis in original]

To be sure, this idea of studying (the syntactic properties of) proofs was already present in the 1905 essay, but, despite Hilbert's emphasis there of the need for a "partly simultaneous development of the laws of logic and of arithmetic," the execution remained still very crude; and the logical frame of mathematics was left virtually untouched. The logical work of Peano, Frege, and Russell thus provided a ready means for

[6] Hilbert 1922, 204, my emphasis.

Hilbert's proof-theory.[7]

§2. Now, with this remarkable idea of proof-theory, we are ready to consider Hilbert's "new grounding of mathematics." But before saying anything about it, I first simply present Hilbert's "solution" to the problem of consistency in his own words:

> We turn to the solution of this problem [of the consistency of the axioms of analysis].
>
> As we saw, abstract operations with general concept-scopes [*Begriffsumfängen*] and contents has proved to be inadequate and uncertain. Instead, as a precondition for the application of logical inferences and for the activation [*Betätigung*] of logical operations, something must already be given in representation [*in der Vorstellung*]: certain extra-logical discrete objects, which exist [*da sind*] intuitively as immediate experience before all thought. If logical inference is to be certain, then these objects must be capable of being completely surveyed in all their parts, and their presentation [*Aufweisung*], their difference, their succession (like the objects themselves) must exist for us immediately, intuitively, as something that cannot be reduced to something else. Because I take this standpoint, the objects [*Gegenstände*] of number theory are for me--in direct contrast to Dedekind and Frege--the signs [*Zeichen*] themselves, whose shape [*Gestalt*] can be generally and certainly recognized by us--independently of space [*Ort*] and time, of the special conditions of the production of the sign, and of insignificant differences in the finished product. The solid philosophical attitude that I think is required for the grounding of pure mathematics--as well as for all scientific thought, understanding, and communication--is this: *In the beginning was the sign.* [Hilbert 1922, 202]

[7] The names of the three logicians are mentioned by Bernays in Bernays 1922b. In this connection, Hilbert frequently speaks of the "pre-established harmony," the "most wonderful and magnificent example" of which, according to him, can be found in Einstein's use of the results of Riemann's mathematical investigations in his theory of relativity. See Hilbert 1931, 266, for instance.

Following Marcus Giaquinto's reading, Hilbert's claim in the quotation may be summarized as follows. First, Hilbert presents what he understands as the basic philosophical standpoint necessary for the "grounding" of mathematics (as well as for scientific thinking in general). It consists of three demands:

(1) the elements of the domain must be extralogical concrete objects of which we have immediate awareness prior to all thought;
(2) the domain must be completely surveyable;
(3) the occurrence and arrangement of the objects must be immediately given ('*anschaulich*') as irreducible facts.[8]

Hilbert then states that, on the basis of these requirements, he considers the objects of *number theory*--*numbers*--*to* be signs.

Our question, naturally, is why he thinks the adoption of this standpoint provides a solution to the problem of consistency. To remind ourselves of the point at issue with Poincaré's objection to Hilbert's "first" direct consistency proof, it is concerned with the circularity involved in the use of the induction principle in the proof: Hilbert's direct consistency proof, argues Poincaré, requires the use of the induction principle, but the induction principle is one of the very principles that constitute the axiom system for arithmetic and thus whose legitimacy is at issue. With the "invention" of proof-theory, however, Hilbert comes to hold that the use of the induction principle as formulated in the axiom system for

[8] The list comes from Giaquinto 1983, 122.

arithmetic is not necessary for the proof and that there is no circularity in it, as Poincaré alleges.

The idea, roughly, goes as follows. As in the case of the 1905 proof, Hilbert's goal is to show the consistency of the arithmetical axioms by demonstrating that no contradiction is deducible from them by the specified rules of inference. In the earlier attempt, he considered the propositions constituting mathematical proofs as being composed of thought-objects and, by abstracting from their meaning-content, conducted a study on the syntactic properties of thought-objects and their combinations generable by the rules of inference. This time around, by contrast, thanks to recently accumulating results in the field of logic, Hilbert is able completely to reproduce mathematical proofs with the reasoning processes occurring in them by means of purely symbolic operations. And, by abstracting from the meaning-content of such a symbolic system, he obtains the objects of his investigation. To quote Bernays's account,

> He [Hilbert] obtains this by taking the systems of formulas that represent those proofs in the logical calculus, detached from their contentual-logical interpretations, as the immediate object of study, and by replacing the proofs of analysis with a purely formal manipulation that takes place with certain signs according to definite rules. [Bernays 1922b, 196]

In other words, Hilbert's proof-theory studies not the objects that the proofs in analysis denote, but rather the proofs themselves, or more precisely, proof-*figures*. What is to be noticed is then this: that while, in principle, there is no bound to the complexity of the proof-figures that are to be considered in proof-theory, they do not form a continuous, infinite manifold that constitutes the domain of the theory of real numbers. On this conception of his project, Hilbert formulates the problem of consistency as that of showing that a formula of the form $\boldsymbol{a} \neq \boldsymbol{a}$ is not derivable from the axioms of analysis by means of its methods of inference. More specifically, a consistency proof, in his formulation, proceeds first by assuming that a proof terminates with a formula of the form $\boldsymbol{a} \neq \boldsymbol{a}$ and then shows, by means of a *reductio* argument, that this assumption cannot be the case. Hilbert's insight is then that, no matter how complex the proof-figure under consideration may be, the form of induction employed is finite. Why? Paolo Mancosu explains the situation as follows:

> ... if I am working with a given proof-figure that ends with $0 \neq 0$, I can certainly look for the first occurrence of the expression $0 \neq 0$ and there is no circularity here, since this appeal to the least number principle in the case of a finite proof figure is as harmless as the principle of induction on the finite proof figure. [Mancosu 1998, 167]

For this reason, Hilbert (and Bernays) argues that we must distinguish *two types* of complete induction:

> the narrower form of induction, which relates only to something completely and concretely given, and the wider form of induction, which uses either the general concept of whole number or the operating with variables in an essential manner. [Bernays 1922a, 221]

Poincaré argues that a consistency proof is impossible (without presupposing the legitimacy and, in fact, the *truth* of the induction principle) since it requires the use of the induction principle. In saying this, he, of course, meant that the induction principle that is used in the proof is the very same principle whose consistency is to be established (as an axiom of arithmetic). But if, as Hilbert argues, two types of induction are to be distinguished, and if the one used in a consistency proof is the narrower form, whereas the one receives the proof is the wider form, it is no longer apparent that there is any circularity involved in this procedure.[9]

With the recognition of these points, we are in a position to understand at least part of Hilbert's meaning in the long quotation from the 1922 essay. When, in that passage, he talks of certain extra-logical discrete objects and their (epistemic) immediacy, complete surveyability, and so on, what he actually has in mind is a formalized axiomatic system, or more specifically, proof-figures in terms of which the problem of consistency is formulated and will be solved. His point is that, insofar as the consistency problem is formulable as a question concerning the concretely given objects of such characteristics, the use of the induction principle as formulated in number theory is not necessary, and thus a first step can be taken toward the refutation of Poincaré's objection.

This, however, is only a first step: for provided that Hilbert is right about the two different forms of the induction

[9] But can Hilbert really dispense with the use of full induction in metamathematics? A short answer to this question is "No": as Gödel later showed, not even full induction is sufficient to prove the consistency statement for arithmetic. As regards the viability of Hilbert's reply to Poincaré, a concise and informative discussion can be found in Mancosu 1998, 165-167 and van Heijenoort 1967, 480-482. My goal here is not to disentangle technical details of this debate or to decide whether Hilbert succeeds in refuting Poincaré, but rather to consider *philosophical* consequences that are brought about by Hilbert's adoption of the so-called "finitist standpoint." Accordingly, in the following, I will proceed without further pursuing this issue.

principle and that the one employed in a consistency proof for the axiom system of arithmetic is, in some sense, more restrictive and less problematic than the one contained in the arithmetical axioms, it would still have to be shown that the axiom system for metatheory, which, presumably, includes the narrower form of the induction principle, is consistent as well. Furthermore, in order that the "consistency statement" proved in metatheory, in fact, expresses a truth about a certain fact about extra-logical objects constituting formal system of arithmetic--the fact that a formula of the form $\boldsymbol{a} \neq \boldsymbol{a}$ is not derivable in it, it would also have to be established that all the axioms of metatheory are *applicable* to these objects.

Accordingly, it would seem that what Hilbert must do next is set up an axiom system (for metatheory) in such a way that various "facts" of the domain consisting of such extra-logical discrete objects are logical consequences of its axioms (which contains, among others, the narrower form of the induction principle).[10] And after actually proving that the consistency statement is a theorem of this system,[11] he would still have two more things to establish: a) that the axiom system for metatheory is consistent, and b) that the axioms constituting the system can be interpreted in such a way that they all express truths about those extra-logical objects.[12]

[10] It is to be recalled here that, as an adequacy condition of an axiom system, Hilbert states that its axioms are to be laid down in such a way that "all the remaining facts of the field of knowledge that lies before us are consequences of the axioms". [Hilbert 1905b, 11-13, quoted in Peckhaus 1990, 59]

[11] What sort of reasoning procedure would be admissible in such a system? I will come to this question shortly.

[12] Needless to say, once it is established that these objects provide a model for the axiom system, it would follow from this that the system is consistent.

Only when all this is done, could Hilbert claim to have refuted Poincaré's objection. Quite obviously, this would mean that, contrary to Poincaré's claim, a direct or absolute consistency proof can be given to arithmetic, to whose consistency that of all other branches of mathematics may be reduced, and thus that Hilbert's ideal of "complete proof-structure" has been realized. This, however, is not all. What we should note here is the following. According to Poincaré, the need for a consistency proof arises in the first place *because* it is assumed that the arithmetical axioms can be considered "disguised definitions" and are treatable in the manner of the Hilbertian axiomatics just as those of various geometrical systems. Upon this understanding, he argues that what follows from the (alleged) impossibility of obtaining a (direct) consistency proof for arithmetic is, in fact, the falsity of the background assumption that arithmetic is axiomatizable in the Hilbertian manner. In other words, Poincaré argues from the impossibility that the axioms of arithmetic, or, at least, the induction principle, are synthetic, framework-independent truths. The acquisition of a consistency proof for arithmetic would, then, refute this philosophico-epistemological part of Poincaré's polemic: given the consistency proof, there would be no reason to think that the axioms of arithmetic differ from those of geometry in their epistemological status, nor would there be any need to introduce system-independent elements into the practice of mathematics.

So what do Hilbert (and Bernays) actually do to deal with Poincaré's objection? Do they set up an axiom system for metatheory, prove the consistency of arithmetic in it, and further establish the applicability of the metatheoretical system to the domain of formal objects? As was briefly mentioned above, it turns out that the route Hilbert and Bernays take is not the one described above. To be sure, they do try to produce a consistency proof for arithmetic, but, as we shall see shortly, they present the theory which serves as the framework for metamathematical investigations in a non-axiomatic, "direct intuitive" manner. The reason for this is usually explained as a matter of mere convenience. Indeed, Hilbert himself at one place speaks in such a manner:

> In the definitive presentation of my theory, the grounding of elementary number theory also takes place by means of axioms; but here, merely for the sake of brevity, I appeal to the direct intuitive grounding. [Hilbert 1923, 1139 footnote 3][13]

A closer look at the text, however, indicates that the fact of the matter is slightly more complicated than that. As we just saw, if one takes the axiomatic approach to the theory which provides the framework for metatheoretical reasoning, it becomes necessary to establish the consistency of the axioms of this metatheory (in order that the consistency proof for the object-theory carried out there has any significance at all). An apparent problem with this axiomatic procedure would then be that even if the axioms of the metatheory do not contain the wider form of the induction principle, and thus that Poincaré's circularity charge may be thwarted at the "first" level, it will hit hard at the "second" level. That is, provided that the consistency of the object-theory can be proved in the metatheoretical axiom system, it is still necessary to

[13] The significance of this passage will be discussed later.

establish the consistency of the latter. But given that a Hilbert-style syntactic consistency proof requires the use of the narrower form of the induction principle, this same principle needs to be applied in proving the consistency of the axioms of the metatheory which, presumably, contains this very principle as one of its members. Hence, once again, we are faced with the possibility of an infinite regress.[14]

Apparently, Hilbert (and Bernays) think that the circularity at the level of metatheory was a real "circle," and they conclude from this that metatheory cannot be treated in the Hilbertian axiomatic method. This seems to be at least what is suggested in the following remark Bernays made in the article written as an introductory essay for Hilbert's complete works:

> Furthermore, the methodological standpoint of Hilbert's proof-theory is not yet developed to its full clarity in the Heidelberg lecture [i.e., the 1905 essay]. Some passages suggest that Hilbert wants to avoid the intuitive representation of number and replace it with the axiomatic introduction of the number-concept. *Such a procedure would result in a circle in the proof-theoretic considerations.* [Bernays 1935, 200, my emphasis, my translation][15]

As we saw earlier, in the 1905 essay, simple thought-objects such as = (equals) are considered in the manner of Hilbert's

[14] Admittedly, it is not clear what exactly such an axiom system would look like. But, given the existence of the induction principle (in the narrower form) in the system, the model method would not be available here.

[15] Außerdem ist auch der methodische Standpunkt der Hilbertschen Beweistheorie in dem Heidelberg Vortrag noch nicht zur vollen Deutlichkeit entwickelt. Einige Stellen deuten darauf hin, daß Hilbert die anschauliche Zahlvorstellung vermeiden und durch die axiomatische Einführung des Zahlbegriffes ersetzen will. Ein solche Verfahren würde in den beweistheoretischen Überlegungen einen Zirkel ergeben.

axiomatic method and are defined by the axioms of the system. Thus, the above remark by Bernays, who collaborated with Hilbert for the formulation of the Hilbert program, does seem to indicate that this was their shared view.[16]

In any case, it is to be stressed here that, in presenting the theory that provides the framework for proof-theoretical investigations, Hilbert chose a non-axiomatic approach. Another point to be noted, as is indicated in the above passage--as well as in the long quotation from Hilbert's 1922 essay--is that Hilbert formulated this *theory* of extra-logical, discrete objects as the theory of *numbers*. More specifically, he introduced what he calls "elementary number theory" as the theory whose object domain consists exclusively of signs[17] built up from 1's, i.e., unary numerals and identified these signs with numbers. Why Hilbert made such an identification is a very complex issue, and I do not consider the question here.[18] For our purpose, what is important is as follows. First, in formulating the theory providing the framework for

[16] But, as is evinced by the above quotation from the 1923 essay, there seems to exist a vacillation on Hilbert's part concerning the axiomatic approach to metatheory. More on this later.

[17] Hilbert later dropped the expression "number-sign [*Zahlzeichen*]" for "numeral [*Ziffer*]" because in his view these "signs" have no meaning [*Bedeutung*] of any sort and are *not* signs of anything. On this issue, see Bernays's footnote to Hilbert 1922 and Mancosu 1998, 172.

[18] As Hilbert was later to maintain, behind the identification there exists his conjecture that the basic linguistic and the basic arithmetic abilities are identical. In this connection, we might recall that Dedekind, in his logico-set-theoretical construction of arithmetic, emphasized the basicness of our ability of "mapping" as a precondition for rational thinking in general. Furthermore, the influence of the Kantian philosophy on Hilbert's thought need also to be taken into consideration. For an illuminating discussion on these issues, see Hallett 1994, 176-181.

metatheoretical considerations as a (contentual)[19] theory of numbers/signs, Hilbert thought that he had finally refuted Poincaré's circularity charge. His view seems to be that, in this theory, we make true statements on the basis of the epistemic access we have to the (extra-systematic) objects constituting its domain, and there is no need to worry about its consistency. To be sure, this theory does not consist solely of statements describing the results of such "number"-theoretical operations as appending, say, "111" to "11." Instead it also contains such genuine theoretical statements as **a** + **b** = **b** + **a**, which states that the result of appending a certain number-sign **a** to another number-sign **b** is the same as the result of appending **b** to **a**, and also deductions of a statement from other statements by the use of "logic."[20] The point is that all those statements which constitute the theory of elementary arithmetic are true and their truth is verifiable or checkable in a finite number of operations. Indeed, Hilbert designed them to function in such a way.

As the theory providing the framework for proof-theoretical considerations, elementary number theory must admit the potential infinite so that, when employed as the metatheory, it can accommodate the maximal complexity of proof-

[19] Here, by the term "contentual," I do not mean a theory which is not formalized. A theory, when axiomatized in the manner of Hilbert's axiomatics, has no extra-systematic denotation or meaning, but, as we saw earlier, this does not mean that it is meaningless; in fact, it has intra-systematic denotation and meaning formulated by its axioms. In this sense, Hilbert sometimes talks of an axiomatized but not formalized theory as being contentual. But, on the other hand, he also talks of a theory considered as having certain extra-systematic denotation and meaning as contentual. Thus, in the above sentence, I am using the term "contentual" in this latter sense.

[20] That elementary number theory *must* contain all these statements is clear when it is realized that this theory is designed to provide the necessary apparatus for conducting a consistency proof.

figures. But, if such logical terms as "all" and "there exists" are applied to the totality of infinitely many number of objects without restrictions, we will no longer be able to verify the truth of statements formulable in the theory. For this reason, Hilbert limits the modes of inference admissible in the theory within a certain bound.[21] Thus, if we are able to *formulate* and actually *prove* the consistency statement within elementary number theory by means of the restricted, "finite" logic, Poincaré's objection could be finally disposed of in a satisfactory manner. For, in such a case, first, no inferential apparatus employed in full arithmetic would be used in the proof, and thus we find no such circularity alleged by Poincaré; second, any statement established in elementary number theory is a genuine truth. This roughly corresponds to the second half of the long quotation from the 1922 essay.[22]

As I noted above, it is not my intention to get into the issues surrounding Hilbert's finitary method and decide whether he is right in claiming the dispensability of the full induction principle at meta-level. Rather, what I want to call attention to here is a *philosophical* consequence that appears to be brought about by Hilbert's non-axiomatic approach to metatheory and his identification of numbers with signs. To put it simply, what the adoption of this so-called finitary standpoint seems to entail is the (re-)introduction of the system-independent notion of truth and existence into mathematics. As we saw above, Hilbert appears to be claiming

[21] But, at the same time, care must be taken not to impose too restrictive conditions upon the logic employed in elementary number theory; for this would lead to the consequence that metatheoretical investigations cannot be carried out to a satisfactory degree.

[22] As it turned out, much more powerful means than Hilbert expected are required to prove the consistency statement for a consistent theory that contains a minimal amount of arithmetic.

that, in it, we have before us a domain of extra-logical, discrete objects, which, presumably, exist independently of the theory, and, on the basis of the epistemic access we have to this domain of theory-independent objects, we make true statements about them. But is this not precisely the sort of extra-mathematical, philosophical assumption that he tried continually to avoid? Did he not object to Frege that to introduce such extra-theoretical elements into mathematics is to play a game of hide and seek?

What Hilbert should recognize here, it might be argued, is that, in equating the impossibility of obtaining a consistency proof for the axiom system for metatheory with the impossibility of axiomatizing metatheory in the manner of the Hilbertian axiomatic method, he is subject to the same mistake as Poincaré. To see this, we have only to consider what would happen if we start by *assuming* the consistency of the axiom system for metatheory *MA* and its applicability to the domain of the extra-logical objects constituting the formal system of arithmetic. Would it be the case, then, that, by making such an assumption, we have abandoned the Hilbertian axiomatics? Not at all. Indeed, in such a case, we would assume that the axioms, or more precisely, axiom-forms of *MA* are applicable to, and thus true of a domain of system-independent objects (under a certain appropriate interpretation *I*). It is to be realized, however, that this is *not* to assume that *MA* has some proper, extra-theoretical subject-matter, which it is supposed to be *about*. To use the expressions we saw earlier in connection with the mid to late nineteenth century trend of "algebraization" or "formalization" in various branches of mathematics, to assume that *MA* is applicable to a certain domain of objects is not to assume that it is a "material" or "authentic" science.[23] It might seem, then, that Hilbert should have axiomatized what he calls elementary number theory and maintained the philosophical standpoint underlying his axiomatic method even if no (direct) consistency proof is possible for the axiom system for metatheory.

One might want to object to this, however, that it blatantly goes against Hilbert's strong belief in mathematics as a *presuppositionless* science: to take this route would be to base the whole edifice of mathematics upon a dogma or a blind faith. But what, then, is the path Hilbert should have taken? Here we might want to reflect upon what originally led him to the banishment of extra-systematic elements from the construction of a scientific theory and thus to the adoption of the Hilbertian axiomatics. His goal in drastically modifying the traditional axiomatics, it is to be remembered, was to achieve a complete rigor in mathematics and thereby secures its objectivity. I also argued earlier that, for Hilbert, a consistency proof is pursued because it constitutes a part of the general requirement of rigor.[24] But given that his ultimate goal is the securing of the objectivity or intersubjectivity of mathematics in the sense that a proof not only eventually

[23] For the same reason, Poincaré is mistaken in claiming that the unavailability of a consistency proof for the induction principle entails its system-independent truth: the two issues are logically independent of each other.

[24] Very roughly, my argument for this claim went as follows. For Hilbert, rigor means that the correctness of (all and only) correct results are established by means of a finite number of inferential steps based upon a finite number of assumptions. Moreover, since his axiomatization/rigorization program is designed to be applied to an existing body of knowledge, this means that it is assumed in advance that there is a set of "correct" propositions, whose truth it is supposed to establish. It would seem then that an inconsistent axiomatization is undesirable because, in it, not only a "correct" proposition but also its negation is deducible.

terminates but also terminates with the same result, no matter who carries it out, what is absolutely essential for achieving it is the antecedently fixed rules of inference and the finitude of the whole proof-procedure, but not so much the provability of all and only "correct" results, and hence such properties of an axiom system as completeness and consistency are of secondary importance. It might be argued, then, that what Hilbert should have done is to stop placing so much importance on the requirement of consistency altogether.

But before conjecturing various possible paths Hilbert could and should have taken, we should ask once again whether, in adopting the non-axiomatic approach to elementary number theory, he really gave up on the idea motivating his axiomatic method: the banishment of framework-independent elements from the construction of a scientific theory. In considering this question, we might, first, take a little closer look at Hilbert's (and Bernays's) attitude toward the question of the axiomatizability of finitary mathematics as the framework for metamathematics. While Bernays seems quite adamant in maintaining the necessity of the non-axiomatic, intuitive approach, Hilbert himself, as we saw above, appears to have thought at least in the early 1920s that the axiomatic treatment of elementary number theory is possible and in fact theoretically desirable, as is indicated in a footnote attached to the 1923 paper:[25]

> In the definitive presentation of my theory, the grounding of elementary number theory also takes place by means of axioms; but here, merely for the sake of brevity, I appeal to the direct intuitive grounding [Hilbert 1923, 1139 footnote 3].

That Hilbert meant by "elementary number theory" finitary mathematics can be seen from the text to which the above footnote is attached:

> The elementary theory of numbers can also be obtained from these beginnings by means of 'finite' logic and purely intuitive thought [durch rein anschauliche Überlegungen] (which includes recursion and intuitive induction for finite existing totalities); here it is not necessary to apply any dubious or problematical mode of inference [Hilbert 1923, 1139].

Second, and more importantly, we should recognize that, contrary to what appears to be suggested by Hilbert's such remarks as "certain extra-logical discrete objects, which exists intuitively as immediate experience before all thought" and so on, it is questionable whether what he takes to constitute the subject-matter of elementary number theory is really physical objects existing independently of any (epistemic) conditions. In this connection, it has recently

[25] In addition to the aforementioned passage from his 1935 article, Bernays writes in the first volume of *Grundlagen der Arithmetik*:

> In number theory, we have an initial object and a process of succession. Both *must* be intuitively represented in a particular manner. [Hilbert and Bernays 1934, 20-21, my emphasis, my translation]

> [In der Zahlentheorie haben wir ein Ausgangsobjekt und einen Prozeß des Fortschreitens. Beides müssen wir in bestimmter Weise anschaulich festlegen.]

been argued by some scholars that, for Hilbert, the object of elementary number theory is not either a physical or mental object, but rather an *iterative process*, and this iterative process is representable in intuition.[26] Although I do not get into the details of this interpretation, I want to emphasize one point which it brings up. That is, while Hilbert does not axiomatize elementary number theory in the manner of the Hilbertian axiomatics, he in no wise conceive of the object of this theory as something that exists in itself. Whether the object of elementary number theory is taken by him to be the iterative process itself or what is constructed through the process, its nature is completely determined in terms of the successive iteration of operations. For our purpose, what this means is that, regardless of Hilbert's non-axiomatic construction of elementary number theory, the philosophical core of his axiomatic method, i.e., the banishment of system-independent elements, is preserved in it.

Once number theory and its subject-matter is understood in reference to the iterative process or the iterativistic objects, however, we will have to distinguish two different classes of objects in arithmetic: those elements which can be constructed through the iterative process and those which cannot. And this seems to entail two different notions of truth and existence within Hilbert's philosophy of mathematics. For the first class, truth is no longer defined in terms of deducibility from axioms but rather of "checkability," and the notion of existence is not defined in terms of the relevant axiom system but now interchangeable with that of constructible. To put it in the most general of terms, this

[26] E.g. Hand 1989, 1990, and Zach 1998.

may seem to be precisely the circumstance in which one might begin to consider the possibility of an instrumentalist solution, that is, to dispose, as mere formal devices, of the elements which are formulated by the axioms of a theory constructed in the manner of Hilbert's axiomatics but not constructible through an iterative process.

As was noted in the introduction, however, things are not so clear cut with the issue of Hilbert's alleged instrumentalism. To cite one instance which indicates the complexity of the issue, Michael Hallett's recent discussion of Hilbert's method of ideal elements shows that for Hilbert the distinction is a relative one. According to Hallett, Hilbert begins the section on ideal elements in the 1919 lectures by saying that the ideal is taken as opposed to the real

> ... on the one hand as the un-actual [*unwirklich*] (the merely thought) rather than the actual [*wirklich*], on the other hand as the complete rather than the incomplete existing.

Hilbert says he will deal only with the first characterization, for it is the one which plays the important role in mathematics. And in mathematics, continues Hilbert, the question of the actual/unactual arises in the form of the question of *existence*. But then:

> What do we mean by existence here? If one looks more closely, then one sees that existence is always meant with respect to the system which is taken as the starting point [*das zugrunde gelegtes System*], ...

In other words, for Hilbert, the real/ideal distinction is only a relative one. In fact, according to Hallett, Hilbert writes two pages later:

> The terminology of ideal elements thus properly speaking only has its justification from the point of view of the system we start out from. In the new system we do not at all distinguish between actual and ideal elements.

An element, say $\sqrt{-1}$, is of a different status, i.e. ideal or unactual, when first added to the system of reals. However, once one has given a set of laws for the integration of this element into the previous system, then what was previously an ideal element is real and does exist as are and do the other elements.

In the above I have tried to indicate the complexity of the issue and the need to re-examine Hilbert's philosophy of mathematics as a whole by identifying philosophical cores which underlie his various foundational attempts throughout his long career. In so doing, we have learned that what ultimately motivates Hilbert's foundational programs is his concern for the objectivity of mathematics and that he tries to achieve this goal through the realization of complete rigor. Accordingly, we must recognize that elementary number theory is designed, above all, to provide a means for the proof-theoretic considerations of formal axiomatic systems, and, consequently, it becomes possible to "talk of" mathematics by means of mathematical language within mathematics. In this way Hilbert's desire to treat such metatheoretical matters as the question of consistency in a rigorous manner is given a clear expression.[27] That is to say, what might be called the

[27] I do not mean by this, however, that for Hilbert rigor is possible only in mathematics.

"epistemological" turn that is brought about with the formulation of a finitary consistency proof can be seen as an expression of Hilbert's invariable concern for rigor and of his attempt to realize the ideal of complete "proof-structure" in the science of mathematics. In this sense, I think it appropriate to conclude my discussion with the following quotation from his 1928 essay "The Foundations of Mathematics":

> ... mathematics is a presuppositionless science. To found it I do not need God, as does Kronecker, or the assumption of a special faculty of our understanding attuned to the principle of mathematical induction, as does Poincaré, or the primal intuition of Brouwer, or, finally, as do Russell and Whitehead, axioms of infinity, reducibility, or completeness, which in fact are actual, contentual assumptions that cannot be compensated for by consistency proofs [Hilbert 1928, 479].

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