Relative Consistency

&

Accessible Domains

Wilfried Sieg

February 1, 2005

Technical Report No. CMU-PHIL-166

Philosophy

Methodology

Logic

Carnegie Mellon

Pittsburgh, Pennsylvania 15213

To appear in: *The Architecture of Modern Mathematics: Essays in History and Philosophy*, Edited by J. Ferreirós and J. Gray, OUP

RELATIVE CONSISTENCY

&

ACCESSIBLE DOMAINS*

Wilfried Sieg
Department of Philosophy
Carnegie Mellon University
Pittsburgh, PA 15213

---

* This paper was originally published in Synthese 84, 1990, pp. 259-297, an essay that in turn was a much-expanded version of *Relative Konsistenz* - written in German and published in (Börger, 1987). That collection of papers was dedicated to the memory of Dieter Rödding, my first logic teacher.

The text of the Synthese paper is essentially unchanged, except for the incorporation of some of the (still numerous) footnotes. In the meantime much illuminating historical research has been carried out and many significant mathematical results have been obtained. Some of these developments are reflected in four papers I have since published and that are most closely related to central issues in this essay; the references are found at the end of the bibliography.

Translations in this paper are my own, unless texts are taken explicitly from Englis editions. In the notes, some quotations that are not central to my arguments are given only the original German.

> ... weil Nichts in der Mathematik gefährlicher ist, als ohne genügenden Beweis Existenzen anzunehmen ... [1]

INTRODUCTION. The goal of Hilbert's program - to give consistency proofs for analysis and set theory within finitist mathematics - is unattainable; the program is dead. The mathematical instrument, however, that Hilbert invented for attaining his programmatic aim is remarkably well: *proof theory* has obtained important results and pursues fascinating logical questions; its concepts and techniques are fundamental for the mechanical search and transformation of proofs; and I believe that it will contribute to the solution of classical mathematical problems.[2] Nevertheless, we may ask ourselves, whether the results of proof theory are significant for the foundational concerns that motivated Hilbert's program and, more generally, for a reflective examination of the nature of mathematics.

The results I alluded to establish the consistency of *classical* theories relative to *constructive* ones and give in particular a constructive foundation to mathematical analysis. They have been obtained in the pursuit of a *reductive program* that provides a coherent scheme for metamathematical work and is best interpreted as a far-reaching generalization of Hilbert's program. For philosophers these definite mathematical results (should) present a profound challenge. To take it on means to explicate the reductionist point of constructive relative consistency proofs; the latter are to secure, after all, classical theories on the basis of more elementary, more evident ones. I take steps towards analyzing the precise character of such implicitly epistemological reductions and thus towards answering the narrow part of the above question. But these steps get their direction from a particular view on the question's wider part.

As background for that view, I point to striking developments within mathematics, namely to the emergence of set theoretic foundations, particularly for analysis, and to the rise of modern axiomatics with a distinctive structuralist perspective. These two developments overlap, and so do the problems related to them. Indeed, they came already to the fore in Dedekind's work and in the controversy surrounding it.[3]

---

[1] From Dedekind's letter to Lipschitz of July 27, 1876, published in (Dedekind, 1932), p. 477.

[2] Finally, there are real beginnings; see (Luckhardt, 1989).

[3] Dedekind played a significant role in the development of 19th century mathematics. As far a our century is concerned I mention his influence on Hilbert and Emmy Noether, thus

They were furthered by Hilbert's contributions to algebraic number theory and the foundations of geometry; the difficult issues connected with them prompted his foundational concerns during the late 1890's. Hilbert's program, though formulated only in the twenties, can be traced to this earlier "problematic". I argue that it was meant to mediate between broad foundational conceptions and to address related, but quite specific methodological problems. An example of the latter is the use of "abstract" (analytic) means in proofs of "concrete" (number theoretic) results: the program - in its instrumentalist formulation - attempts to exploit the formalizability of mathematical theories for a systematic and philosophically decisive solution.

This instrumentalist aspect, as a matter of fact equivalent to the program's consistency formulation, has been overemphasized in the literature and leaves unaccounted-for critical features of Hilbert's thought. The historical part of my paper brings into focus such neglected features and sets the stage for an analysis of proof theoretic reductions as *structural* ones. The philosophical significance of relative consistency results is viewed in terms of the *objective underpinnings* of theories to which reductions are (to be) achieved.[4] The elements of *accessible domains* that provide such underpinnings have a unique build-up through basic operations from distinguished objects; the theories formulate principles that are evident - given an understanding of the build-up and a minimalist delimitation of the domain. But note that (i) the objects in accessible domains need not be constructive in any traditional sense: certain segments of the cumulative hierarchy will be seen to be accessible, and (ii) the restriction of logical principles used is not central: the theories of interest turn out to be such that the consistency of their classical versions is established easily relative to their intuitionistic versions (by finitist arguments).

Even in mathematical practice relative consistency proofs are prompted by epistemological concerns. One wants to guarantee the

---

Bourbaki's conceptions. An illuminating analysis of Dedekind's work is given in (Stein, 1988); the major influences on Bourbaki are documented in (Dieudonné, 1970). In (Zassenhaus, 1975) one finds on p. 448 the remark: "... we can see in Dedekind more than in any other single man or woman the founder of the conceptual method of mathematical theorization in our century. The new generation of mathematicians ... after the First World War realized in full detail Dedekind's self-confessed desire for conceptual clarity not only in the foundations of number theory, ring theory and algebra, but on a much broader front, in all mathematical disciplines."

[4] The reduced theories have to be mathematically significant. Indeed, the consistency program has been accompanied from its inception by work intended to show that the theories permit the formal development of substantial parts of mathematics.

coherence of a complex (new) theory in terms of comprehended notions and does so frequently by devising suitable models. This general goal is pursued, e.g. when Euclidean models for non-Euclidean geometries are given. Proof theoretic reductions have two special features: (i) they focus on the deductive apparatus of theories, and (ii) they are carried out within theories that have to measure up to restrictive epistemological principles. The latter are traditionally of a more or less narrow "constructivist" character. In broadening the range of theories to "quasi-constructive" ones and concentrating on one central feature, namely accessibility, we will be able to evaluate their (relative) epistemological merits. And in this way, it seems to me, we can gain a deepened understanding of what is *characteristic of and possibly problematic* in classical mathematics and of what is *characteristic of and taken for granted as convincing* in constructive mathematics.

In the current discussion, some do as if an exclusive alternative between platonism and constructivism had emerged from the sustained mathematical and philosophical work on foundations for mathematics; others do as if this work were deeply misguided and did not have any bearing on our understanding of mathematics. Both attitudes prevent us from using the insights (of pre-eminent mathematicians) that underly such work and the significant results that have been obtained. They also prevent us from turning attention to central tasks; namely, to understand the role of abstract structures in mathematical practice and the function of (restricted) accessibility notions in "foundational" theories or "methodical frames", to use Bernays's terminology. I attempt to give a perspective that includes traditional concerns, but that allows - most importantly - to ask questions transcending traditional boundaries. This perspective is deeply influenced by the writings of Paul Bernays.

1. MATHEMATICAL REFLECTIONS. They are concerned with mathematical analysis and theories in which its practice can be formally represented. So I start out by describing attempts to clarify the very object of analysis and thus, it was assumed, the role of analytic methods in number theory. These attempts came under the headings *arithmetization of analysis* and *axiomatic characterization of the real numbers.* I discuss two kinds of arithmetizations put forward by Dedekind and Kronecker, respectively. Dedekind proceeded axiomatically and sought to secure his characterization by a consistency proof relative to logic broadly conceived, whereas

Kronecker insisted on a radical restriction of mathematical objects and methods. (Dedekind's arithmetization of analysis should perhaps be called *set theoretic* and Kronecker's by contrast *strict.*) Hilbert's axiomatization of the real numbers grew directly out of Dedekind's and was the basis for two proposals to overcome at least for analysis the set theoretic difficulties that had been discovered around the turn of the century. The second proposal, when suitably amended by the formalist conception of mathematics, led to Hilbert's program.

1.1. CONSISTENT SETS. A systematic arithmetization is to achieve, Dirichlet demanded, that *any* theorem of algebra and higher analysis can be formulated as a theorem about natural numbers.[5] If that had been clearly so, Dirichlet's introduction of analytic methods to prove his famous theorem on arithmetic progressions would have been methodologically innocuous. But in using properties of "continuous magnitudes" to prove facts concerning natural numbers, he pushed aside a traditional, partly epistemologically motivated boundary.[6] Dirichlet himself remarks: "The method I employ seems to me to merit attention above all by the connection it establishes between the infinitesimal analysis and the higher arithmetic..."[7] In another paper that explores further uses of analytic methods in number theory he writes: "... I have been led to investigate a large number of questions concerning numbers from an entirely new point of view, that attaches itself to the principles of infinitesimal analysis and to the remarkable properties of a class of infinite series and infinite products ..." The significance of these methodological innovations can be fathomed from remarks such as Kummer's, who compares them in his eulogy on Dirichlet to Descartes's "applications of analysis to geometry", or Klein's, who stated that they gave "direction to the entire further development of number theory".

The essays of Dedekind and Kronecker[8] seek an arithmetization satisfying Dirichlet's demand, but proceed in radically different ways. Kronecker admits as objects of analysis only natural numbers and constructs from them (in now well-known ways) integers and rationals.

---

5 That is reported in the preface to (Dedekind, 1888).

6 I allude, of course, to Gauss's attitude; compare (Sieg, 1984), p. 162.

7 (Dirichlet, 1838), p. 360, respectively (Dirichlet, 1839/40), p. 411.

8 Dedekind's relevant papers are the essays *Stetigkeit und irrationale Zahlen* (1872) and *Was sind und was sollen die Zahlen?* (1888); his letters to Lipschitz and Weber are also of considerable interest and were published in (Dedekind, 1932). As to Kronecker I refer to his *Über den Zahlbegriff* and Hensel's introduction to (Kronecker, 1901).

Even algebraic reals are introduced, since they can be isolated effectively as roots of algebraic equations. The general notion of irrational number, however, is rejected in consequence of two restrictive methodological requirements to which mathematical considerations have to conform: (i) concepts must be decidable, and (ii) existence proofs must be carried out in such a way that they present objects of the appropriate kind. For Kronecker there can be no infinite mathematical objects, and geometry is banned from analysis even as a motivating factor. Clearly, this procedure is strictly arithmetic, and Kronecker believes that following it analysis can be re-obtained. In (Kronecker, 1887) we read:

> I believe that we shall succeed in the future to 'arithmetize' the whole content of all these mathematical disciplines [including analysis and algebra] ; i.e. to base it [the whole content] on the concept of number taken in its most narrow sense..."

Kronecker did prove, to his great pleasure, Dirichlet's theorem on arithmetic progressions satisfying his restrictive conditions.[9] But it is difficult for me to judge to what extent Kronecker pursued a program of developing (parts of) analysis systematically. In any event, such a program is not chimerical: from mathematical work during the last decade it has emerged that a good deal of analysis and algebra can indeed be done in conservative extensions of primitive recursive arithmetic.[10] Finally let me mention that Kronecker begins the paper by hinting at his philosophical position - through quoting Gauss on the epistemologically special character of the laws for natural numbers; only these laws, in contrast to those of geometry, carry the complete conviction of their necessity and thus of their absolute truth.

Dedekind, a student of Gauss, emphasized already in his Habilitationsvortrag of 1854 a quite different and equally significant aspect of mathematical experience; namely, the introduction and use of new concepts to grasp composite phenomena that are being governed by the old notions only with great difficulty.[11] Referring to

---

9 (Kronecker, 1901), p. 11.

10 It is most plausible that such work would be enriched by paying attention to Kronecker's. For references to the contemporary mathematical work see (Simpson, 1988). As PRA is certainly a part of number theory unproblematic even for Kronecker, this work can be seen as a partial realization of "Kronecker's program" (and not, as it is done by Simpson, of Hilbert's).

11 Dedekind mentions that Gauss approved of the 'Absicht' of his talk. Kneser reports in his *Leopold Kronecker*, Jahresbericht der DMV, 33, 1925, that Dedekind referred often to a remark of Gauss that (for a particular number theoretic problem) notions are more important than notations. In pointing to the "Gaussian roots" of Dedekind's and Kronecker's so strikingly

this earlier talk, Dedekind asserts in the preface to his (1888) that most of the great and fruitful advances in mathematics have been made in exactly this way. He gives, in contrast to Kronecker, a general definition of reals: cuts are explicitly motivated in geometric terms, and infinite sets of natural numbers are used as respectable mathematical objects. Kronecker's methodological restrictions are opposed by him, in particular the decidability of concepts; he believes that it is determined independently of our knowledge, whether an object does or does not fall under a concept. In this way Dedekind defends general features of his work in the foundations of analysis and in algebraic number theory.[12] But, the reader may ask, how does Dedekind secure the existence of mathematical objects? To answer this question I examine Dedekind's considerations for real and natural numbers.

The principles underlying the definition of cuts are for us set-theoretic ones, for Dedekind they belong to logic[13]: they allow - as Dedekind prefers to express it - the creation of new numbers, such that their system has "the same completeness or ... the same continuity as the straight line". Dedekind emphasizes in a letter to Lipschitz that the *stetige Vollständigkeit* (continuous completeness) is essential for a scientific foundation of the arithmetic of real numbers, as it relieves us of the necessity to assume in analysis *existences*

---

different positions, I want to emphasize already here that they can (and should) be viewed as complementary.

12 Kronecker spurned Dedekind's algebraic conceptions. See, e.g. the note on p. 336 of his *Über einige Anwendungen der Modulsysteme*, Journal für Mathematik, 1886, and Dedekind's gentle rejoinder in, what else, a footnote of his (1888): "but to enter into a discussion [of such restrictions] seems to be called for only when the distinguished mathematician will have published his reasons for the necessity or even just the advisability of these restrictions." Kronecker expressed his views quite drastically in letters; for example in a letter to Lipschitz of August 7, 1883 he writes: "Bei dieser Gelegenheit habe ich das lange gesuchte Fundament meiner ganzen Formentheorie gefunden, welches gewissermassen 'die Arithmetisierung der Algebra' - nach der ich ja das Streben meines mathematischen Lebens gerichtet habe - vollendet, und welches zugleich mir mit Evidenz zeigt, dass auch umgekehrt die Arithmetik dieser 'Association der Formen' nicht entbehren kann, dass sie ohne deren Hülfe nur auf Irrwege geräth oder sich Gedankengespinste macht, die wie die Dedekindschen, die wahre Natur der Sache mehr zu verhüllen als zu klären geeignet sind." (Lipschitz, 1986), pp. 181-182.

13 Why then "arithmetization"? Dedekind views cuts as "purely arithmetical phenomena"; see the preface to (1872) or (1888), where Dedekind talks directly about the "rein arithmetische Erscheinung des Schnitts". In the latter work he immediately goes on to pronounce arithmetic as a part of logic: "By calling arithmetic (algebra, analysis) only a part of logic I express already that I consider the concept of number as completely independent of our ideas or intuitions of space and time, that I view it rather as an immediate outflow from the pure laws of thought." (Dedekind, 1932), p. 335. - The next three references in this paragraph are to (Dedekind, 1932), namely, p. 321, p. 472, and p. 477, respectively.

without sufficient proof. Indeed, it provides the answer to his own rhetorical question:

How shall we recognize the admissible existence assumptions and distinguish them from the countless inadmissible ones...? Is this to depend only on the success, on the accidental discovery of an internal contradiction? [14]

Dedekind is considering assumptions that concern the existence of individual real numbers; such assumptions are not needed, when we are investigating a complete system - *ein denkbar vollständigstes Größen-Gebiet*. By way of contrast, and in defense against the remark that all of his considerations are already contained in Euclid's *Elements*, he notices that such a complete system is not underlying the classical work. The definition of proportionality is applied only to those (incommensurable) magnitudes that occur already in Euclid's system and whose existence is evident for good reasons. And he argues in this letter of 1876 and later in the preface to *Was sind und was sollen die Zahlen?* that the algebraic reals form already a model of Euclid's presentation. For Euclid, Dedekind argues, that was sufficient, but it would not suffice, if arithmetic were to be founded on the very concept of number as proportionality of magnitudes.[15]

The question as to the existence of particular reals has thus been shifted to the question as to the existence of their complete system. If we interpret the essay on continuity in the light of considerations in *Was sind und was sollen die Zahlen?* and Dedekind's letter to Keferstein, we can describe Dedekind's procedure in a schematic way as follows. Both essays present first of all informal analyses of basic notions, namely of continuity by means of *cuts* (of points on the straight line and rationals, respectively) and of natural number by means of the components *system*, *distinguished object 1*, and *successor operation*. These analyses lead with compelling directness to the definitions of a complete, ordered field and of a simply infinite system. Then - in our terminology - models for these axiom systems are given. In *Stetigkeit und irrationale Zahlen* the system of all cuts of rationals is shown to be (topologically) complete and, after the introduction of the arithmetic operations, to satisfy the axioms for an ordered field. The parallel considerations for simply infinite systems in *Was sind und was sollen die Zahlen?* are carried out more explicitly. Dedekind gives in section 66 of that essay his "proof" of the existence

---

14 Letter to Lipschitz of July 27, 1876; in (Dedekind, 1932), p.477.

15 (1932), pp. 477-8, in particular top of 478.

of an infinite system. Such systems contain a simply infinite (sub-) system, as is shown in section 72.

Dedekind believes to have given purely logical proofs for the existence of these systems and thus to have secured the consistency of the axiomatically characterized notions.[16] With respect to simply infinite systems he writes to Keferstein in a letter of February 27, 1890:

After the essential nature of the simply infinite system, whose abstract type is the number sequence N, had been recognized in my analysis ... , the question arose: does such a system *exist* at all in the realm of our ideas? Without a logical proof of existence it would always remain doubtful whether the notion of such a system might not perhaps contain internal contradictions. Hence the need for such a proof (articles 66 and 72 of my essay).[17]

I emphasize that Dedekind views these considerations not as specific for the foundational context of the essays analyzed here, but rather as paradigmatic for a general mathematical procedure, when abstract, axiomatically characterized notions are to be introduced. That is unequivocally clear e.g. from his discussions of ideals in (Dedekind, 1877), where he draws direct parallels to the steps taken here.[18] The particular constructions leading to the general concept of real number provide an arithmetization of analysis: they proceed, as Dedekind believes, solely within logic and thus purely arithmetically (cp. footnote 13). Their specific logical character implies almost trivially that Dirichlet's demand is satisfied; any analytic statement can be viewed as (a complicated way of making) a statement concerning natural numbers. But Dedekind states, that it is nothing meritorious "to actually carry out this tiresome re-writing (mühselige Umschreibung) and to insist on using and recognizing only the natural numbers".

The very beginnings of the Hilbertian program can be traced back to these foundational problems in general and to Dedekind's proposed solution in particular. Hilbert turned his attention to them, as he recognized the devastating effect on Dedekind's essays of

---

16 That such a proof is intended also in *Stetigkeit und irrationale Zahlen* is most strongly supported by the discussion in (Dedekind, 1888), p. 338. - The Fregean critique of Dedekind in section 139 of *Grundgesetze der Arithmetik*, vol. II, is quite misguided. For a deeper understanding of Dedekind's views on creation (Schaffung) of mathematical objects see also his letter of January 24, 1888 to H. Weber in (Dedekind, 1932), p. 489. That, incidentally, anticipates and resolves Benacerraf's dilemma in *What numbers could not be.*

17 In (van Heijenoort, 1967), p. 101. The essay Dedekind refers to is (Dedekind, 1888).

18 (Dedekind, 1877), pp. 268-269; in particular the long footnote on p. 269.

observations that Cantor communicated to him in letters, dated September 26 and October 2, 1897.[19] Cantor remarks there that he was led "many years ago" to the necessity of distinguishing two kinds of totalities (multiplicities, systems); namely, *absolutely infinite* and *completed* ones. Multiplicities of the first kind are called *inconsistent* in his famous letter to Dedekind of July 28, 1899, and those of the second kind *consistent*. Only consistent multiplicities are viewed as sets, i.e. proper objects of set theory. This distinction is to avoid, and does so in a trivial way, the contradictions that arise from assuming that the multiplicity of all things (all cardinals, or all ordinals) forms a set.

In 1899 Hilbert writes *Über den Zahlbegriff*, his first paper addressing foundational issues of analysis. He intends - never too modest about aims - to rescue the set theoretic arithmetization of analysis from the Cantorian difficulties. To this end he gives a categorical axiomatization of the real numbers following Dedekind's work in *Stetigkeit und irrationale Zahlen*. He claims that its consistency can be proved by a "suitable modification of familiar methods"[20] and remarks that such a proof constitutes "the proof for the existence of the totality of real numbers or - in the terminology of G. Cantor - the proof of the fact that the system of real numbers is a consistent (completed) set". In his subsequent Paris address Hilbert goes even farther, claiming that the existence of Cantor's higher number classes and of the alephs can also be proved. Cantor, by contrast, insists in a letter to Dedekind, written on August 28, 1899, that even finite multiplicities cannot be proved to be consistent. The fact of their consistency is a simple, unprovable truth - "the axiom of arithmetic"; and the fact of the consistency of those multiplicities that have an aleph as their cardinal number is in exactly the same way an axiom, the "axiom of the extended transfinite arithmetic".[21]

Hilbert recognized soon that his problem, even for the real numbers, was not as easily solved as he had thought. Bernays writes in

---

19 In particular on section 66 of *Was sind und was sollen die Zahlen?*. That is clear from Cantor's response of November 15, 1899 to a letter of Hilbert's (presumably not preserved). Cantor's letter is published in (Purkert and Ilgauds), p. 154. See also remark A in section 1.3.

20(Hilbert, 1900), p. 261. The German original is: "Um die Widerspruchsfreiheit der aufgestellten Axiome zu beweisen, bedarf es nur einer geeigneten Modifikation bekannter Schlußmethoden." (Bernays, 1935) reports on pp. 198-199 in very similar words, but with a mysterious addition: "Zur Durchführung des Nachweises gedachte Hilbert mit einer geeigneten Modifikation der in der Theorie der reellen Zahlen angewandten Methoden auszukommen."

21 (Cantor, 1932), p. 447-448.

his (1935) on p. 199, "When addressing the problem [of proving the above consistency claims] in greater detail, the considerable difficulties of this task emerged". It is the realization, I assume, that distinctly new principles have to be accepted; principles that cannot be pushed into the background as "logical" ones.[22] Dedekind's arithmetization of analysis has not been achieved without "mixing in foreign conceptions", after all;[23] a rewriting, however tiresome, of analytic arguments in purely number theoretic terms is seemingly not always possible.

1.2. CONSISTENT THEORIES. In his address to the International Congress of Mathematicians, Heidelberg 1904, Hilbert examines again and systematically various attempts of providing foundations for analysis, in particular Cantor's. The critical attitude towards Cantor that was implicit in *Über den Zahlbegriff* is made explicit here. Hilbert accuses Cantor of not giving a rigorous criterion for distinguishing consistent from inconsistent multiplicities; he thinks that Cantor's conception on this point "still leaves latitude for subjective judgment and therefore affords no objective certainty". He suggests again that consistency proofs for suitable axiomatizations provide an appropriate remedy, but proposes a radically new method of giving such proofs: develop logic (still vaguely conceived) together with analysis in a common frame, so that proofs can be viewed as *finite* mathematical objects; then show that such formal proofs cannot lead to a contradiction. Here we have seemingly in very rough outline Hilbert's program as developed in the twenties; but notice that the point of consistency proofs is still to guarantee the existence of sets, and that a reflection on the mathematical means admissible in such proofs is lacking completely. Before describing the later program, let me mention that this address and *Über den Zahlbegriff* are squarely directed against Kronecker. In his Heidelberg address Hilbert claims

---

22 This general concern comes out in Husserl's notes on a lecture that Hilbert gave to the Göttingen Mathematical Society in 1901 and, in very similar terms, in (Hilbert, 1904), p. 266; Husserl's notes are quoted in full in Wang's *Reflections on Kurt Gödel,* Cambridge, 1987, p. 53.

23 Dedekind points out emphatically, e.g. in the letter to Lipschitz (1932, p. 470) and in the introduction to (1888), that his constructions do not appeal anywhere to "fremdartige Vorstellungen"; he has in mind appeals to geometric ones. - Bernays has again and again made the point that a "restlose strikte Arithmetisierung" cannot be achieved. In (Bernays, 1941) one finds on p. 152 the remark: "... one can say - and that is certainly the essence of the finitist and intuitionist critique of the usual mathematical methods - that the arithmetization of geometry in analysis and set theory is not a complete one." It is through the powerset of the set of natural numbers that our geometric conception of the continuum is connected to our elementary conception of number; e.g. in: *Bemerkungen zu Lorenzen's Stellungnahme in der Philosophie der Mathematik*, 1978.

that he has refuted Kronecker's standpoint - by partially embracing it, as I hasten to add. I will explain below that this is by no means paradoxical. Indeed, a genuine methodological shift had been made; Bernays remarks that Hilbert started, clearly before giving this address, "to do battle with Kronecker with his own weapons of finiteness by means of a modified conception of mathematics".[24]

There are a number of general tendencies that influenced the Heidelberg address and the further development towards Hilbert's program. First of all, the *radicalization of the axiomatic method*; by that I mean the insight that the linguistic representation of a theory can be viewed as separable from its content or its intended interpretation. That was clear to Dedekind, was explicitly used by Wiener, and brought to perfection by Hilbert in his *Grundlagen der Geometrie*.[25] Secondly, the *instrumentalist view of (strong mathematical) theories*; the earliest explicit formulation I know of is due to Borel discussing the value of abstract, set theoretic arguments from a Kroneckerian perspective.

> One may wonder what is the real value of these [set theoretic] arguments that I do not regard as absolutely valid but that still lead ultimately to effective results. In fact, it seems that if they were completely devoid of value, they could not lead to anything ... This, I believe, would be too harsh. They have a value analogous to certain theories in mathematical physics, through which we do not claim to express reality but rather to have a guide that aids us, by analogy, in predicting new phenomena, which must then be verified.

Can one systematically explore, Borel asks, the sense of such arguments. His answer is this:

> It would require considerable research to learn what is the real and precise sense that can be attributed to arguments of this sort. Such research would be useless, or at least it would require more effort than it would be worth. How these overly abstract arguments are related to the concrete becomes clear when the need is felt.[26]

---

24 The quotation is taken from a longer remark of Bernays in (Reid, 1970). It is preceded by: "Under the influence of the discovery of the antinomies in set theory, Hilbert temporarily thought that Kronecker had probably been right there. *(I.e. right in insisting on restricted methods.)* But soon he changed his mind. Now it became his goal, one might say, to do battle with ..."

25 Dedekind describes, on p. 479 of (1932), such a separation before making the claim that the algebraic reals form a model of the Euclidean development. For a penetrating discussion of the general development see (Guillaume). Such a separation appears to us as banal, but it certainly was not around the turn of the century, as the Frege-Hilbert controversy amply illustrates.

26 (Baire e.a.), p. 273. A striking, but different suggestion along these lines was made already in (Cantor, 1883), p. 173: "If, as is assumed here [i.e. from a restrictive position], only the natural numbers are real and all others just relational forms, then it can be required that the

To grapple with this problem clearly one has to use, thirdly, the *strict formalization of logic* that had been achieved by Frege (Peano, and Russell/Whitehead). That is a moment not yet appreciated in Hilbert's Heidelberg address, where one finds a discussion of logical consequence (Folgerung) quite uninformed by this crucial aspect of Frege's work. Hilbert succeeded to join these tendencies into a sharply focused program with a very special mathematical and philosophical perspective.

The *modified conception of mathematics* underlying the formulation of the program is characterized by Hilbert in the twenties most pointedly and polemically: classical mathematics is a *formula game* that allows "to express the whole thought content of mathematics in a uniform way"; its consistency has to be established within finitist mathematics, however. Finitist mathematics is taken to be a philosophically unproblematic part of number theory and, in addition, to coincide with the part of mathematics accepted by Kronecker and Brouwer.[27] Not every formula of this "game" has a meaning but only those that correspond to finitist statements, i.e. universal sentences of the kind of Fermat's Theorem. For a precise description of the role of consistency proofs let **P** be a formal theory that allows the representation of classical mathematical practice and let **F** formulate the principles of finitist mathematics. The consistency of **P** is in **F** equivalent to the reflection principle

$$(\forall x)(Pr(x,'s') \Rightarrow s).$$

Pr is the finitistically formulated proof predicate for **P**, s a finitist statement, and 's' the corresponding formula in the language of **P**. A consistency proof in **F** was programmatically sought; it would show, because of the above equivalence, that the mere technical apparatus **P**

---

proofs of theorems in analysis are checked as to their 'number-theoretic content' and that every gap that is discovered is filled according to the principles of arithmetic. The feasibility of such a supplementation is viewed as the true touchstone for the genuineness and complete rigor of those proofs. It is not to be denied that in this way the foundations of many theorems can be perfected and that also other methodological improvements in various branches of analysis can be effected. Adherence to the principles justified from this viewpoint, it is believed, secures against any kind of absurdities or mistakes." This is in a way closer to Hilbert's belief that finitist statements must admit a finitist proof. That belief is implicitly alluded to in (Bernays, 1941), p. 151: "The hope that the finitist standpoint (in its original sense) could suffice for all of proof theory was brought about by the fact that the proof theoretic problems could be formulated from that point of view."

27 See remark B in section 1.3. below.

can serve reliably as an instrument for the proof of finitist statements. After all, the consistency proof would allow to transform any **P**-derivation of 's' into a finitist proof of s (and thus give a quite systematic answer to Borel's question). Hilbert believed that consistency proofs would settle foundational problems - once and for all and by purely mathematical means. Bernays judged in (1922), p. 19:

> The great advantage of Hilbert's method is precisely this: the problems and difficulties that present themselves in the foundations of mathematics can be transferred from the epistemological-philosophical to the properly mathematical domain.

Because of Gödel's incompleteness theorems this advantage proved to be illusory, at least when finitist mathematics is contained in **P**:[28] for such **P**'s the Second Incompleteness Theorem just states that their consistency cannot be established by means formalizable in **P**. The radical restriction of what was "properly mathematical" had to be given up; a modification of the program was formulated and has been pursued successfully for parts of analysis.[29] The crucial tasks of this general reductive program are: (i) find an appropriate formal theory **P*** for a significant part of classical mathematical practice, (ii) formulate a "corresponding" constructive theory **F***, and (iii) prove in **F*** the partial reflection principle for **P***, i.e.

$$Pr^*(d,'s') \Rightarrow s$$

for each **P***-derivation d. Pr* is here the proof-predicate of **P*** and s an element of some class *F* of formulas. The provability of the partial reflection principle implies the consistency of **P*** relative to **F***. (For the theories considered here, this result entails that **P*** is conservative over **F*** for all formulas in *F*.) Gödel and Gentzen's consistency proof of classical number theory relative to Heyting's formalization of intuitionistic number theory was the first contribution to the reductive program; as a matter of fact, their result made that program at all plausible.

I do not intend to sketch the development of proof theory and, consequently, I will comment only on some central results concerning

---

28 And that is a more than plausible assumption for those **P**'s Hilbert wanted to investigate and that contain elementary number theory. Consider the practice of finitist mathematics, for example in volume I of *Grundlagen der Mathematik*, the explicit remarks on p. 42 of that book, but also the analyses given by (Kreisel, 1965) and (Tait, 1981).

29 Hilbert and Bernays, Gentzen, Lorenzen, Schütte, Kreisel, Feferman, Tait, and many other logicians and mathematicians have contributed; for detailed references to the literature see (Buchholz, e.a., 1981) or (Sieg, 1985).

theories for the mathematical continuum. Second order arithmetic was taken by Hilbert and Bernays as the formal framework for analysis. The essential set theoretic principles are the comprehension principle

$$(\exists X)(\forall y)(y \varepsilon X <=> S(y))$$

and forms of the axiom of choice

$$(\forall x)(\exists Y)S(x,Y) => (\exists Z)(\forall x)S(x,(Z)_x);$$

S is an arbitrary formula of the language and may in particular contain set quantifiers. These general principles are impredicative, as the sets X and Z whose existence is postulated are characterized by reference to all sets (of natural numbers). Subsystems of second order arithmetic can be defined by restricting S to particular classes of formulas. The subsystems that have been proved consistent contain for example the comprehension principle for Π^1_1- and Δ^1_2- formulas; the latter have the shape $(\forall X)R$, respectively are provably equivalent to formulas of the shape $(\forall X)(\exists Z)R$ and $(\exists Z)(\forall X)T$, where R and T are purely arithmetic.[30] These particular subsystems are of direct mathematical interest, as analysis can be formalized in them by (slightly) refining the presentation of Hilbert and Bernays in supplement IV of *Grundlagen der Mathematik II*. The proof theoretic investigations have been accompanied by mathematical ones, showing that even weaker subsystems will do. Really surprising refinements have been obtained during the last fifteen years: all of classical analysis can be formalized in conservative extensions of elementary number theory, significant parts also of algebra already in conservative extensions of primitive recursive arithmetic.[31] These two complexes of results indicate corresponding complexes of problems for future development; namely, (1) to give constructive consistency proofs of stronger subsystems of analysis, first of all for the system with Π^1_2-comprehension, and (2) to find weaker, but mathematically still significant subsystems (whose consistency is easily seen from the finitist standpoint and) whose provably recursive functions are in complexity classes. These are mathematically and logically most fascinating problems.

---

30 For details concerning the theories with versions of the axiom of choice see (Feferman and Sieg, 1981). The character of the consistency proofs is indicated below. Foundationally significant results are also described in (Feferman, 1988).

31 For the discussion of these results and detailed references to the literature see (Simpson, 1988) and (Sieg, 1988).

1.3. REMARKS. They are partly of historical, partly of systematic character and concern mostly the axiomatic method underlying Hilbert's program. Though they are intended to ease the transition to the philosophical reflections in the second part of this essay, they are also to defuse some of the widely held misconceptions of the program: e.g. its "crude formalism" or its ad-hoc-character to serve as a "weapon" against Brouwer's intuitionism.

A. PARADOXICAL BACKGROUND. The concern with consistent sets and the explicit use of Cantorian terminology in *Über den Zahlbegriff* show clearly that Hilbert was informed about the set theoretic difficulties Cantor had found and communicated to Dedekind in the famous letter of July 28, 1899. The recently published earlier letters of Cantor's to Hilbert I mentioned above throw light on this background. (They provide also surprising new information on the early history of the set theoretic paradoxes and on the circumstances surrounding Cantor's letter to Dedekind.) There is no doubt that Hilbert was prompted by these difficulties to think seriously about foundational issues. After all, as I pointed out, he recognized the impact of Cantor's observations on Dedekind's logical foundations of arithmetic presented in *Was sind und was sollen die Zahlen?*. Here I just want to recall Dedekind's reaction to the Cantorian problems, reported in a letter of F. Bernstein to Emmy Noether and published in (Dedekind, 1932). Bernstein had visited Dedekind on Cantor's request in the spring of 1897. The express purpose was to find out what Dedekind thought about the paradox of the system of all things; Cantor had informed Dedekind about it already by letter in 1896. Bernstein reports: "Dedekind had not arrived yet at a definite position and told me, that in his reflections he almost arrived at doubts, whether human thinking is completely rational."[32] Strong words from a man as sober and clear-headed as Dedekind.

B. ASSUMPTION. How is it possible to reconcile Hilbert's programmatic formalism with his deep trust in the correctness of classical

---

32 (Dedekind, 1932), p. 449. Even six years later, in 1903, Dedekind still had such strong doubts that he did not allow a reprinting of his booklet. In 1911, he consented to a republication and wrote in the preface, "Die Bedeutung und teilweise Berechtigung dieser Zweifel verkenne ich auch jetzt nicht. Aber mein Vertrauen in die innere Harmonie unserer Logik ist dadurch nicht erschüttert; ich glaube, daß eine strenge Untersuchung der Schöpferkraft des Geistes, aus bestimmten Elementen ein neues Bestimmtes, ihr System zu erschaffen, das notwendig von jedem dieser Elemente verschieden ist, gewiß dazu führen wird, die Grundlagen meiner Schrift einwandfrei zu gestalten." (Dedekind, 1932), p.343.

mathematics? - Most easily, when the formal theories of central significance are complete or deductively closed, as the Hilbertians used to say. This completeness assumption is already found in *Über den Zahlbegriff*. Hilbert writes there that the set of real numbers should be thought of as "... a system of things, whose mutual relations are given by the above finite and closed system of axioms, and for which statements have validity, only if they can be deduced from those axioms by means of a finite number of logical inferences". Hilbert talks about a non-formalized axiomatic theory. But if it is adequately represented by a formal theory **P**, then **P** must naturally be deductively closed. As a matter of fact, it was believed in the Hilbert-school - until Gödel's incompleteness theorems became known - that the formalisms for elementary number theory and analysis were complete. For the purpose of obtaining a completeness proof Hilbert suggested in his Bologna address (1928) to reinterpret finitistically the familiar arguments for the categoricity of the Peano-axioms and of his axioms concerning the real numbers. The assumed completeness and the ensuing harmony of provability and truth help understand how Hilbert could take his radical formalist position, in order to simply bypass the epistemological problems associated with the classical infinite structures.[33] - The finitist mathematical basis was thought to be co-extensive with the part of arithmetic accepted by Kronecker and Brouwer. As to Kronecker, Hilbert mentions in his (1931): "At about the same time [i.e. at the time of Dedekind's (1888)] ... Kronecker formulated most clearly a view, and illustrated it by numerous examples, that essentially coincides with our finitist standpoint." The relation to intuitionism is discussed explicitly at a great number of places by Bernays; e.g. (Bernays, 1967), p. 502. A particularly concise formulation was given by Johann von Neumann in his *Formalistische Grundlegung der Mathematik*, Erkenntnis 2 (1931), pp. 116-7.

C. DOUBTS. Two mathematicians with quite different foundational views criticized Hilbert's formalism at exactly this point; i.e. they criticized the assumption that parts of mathematics can be represented (completely) by formal theories. The first of them was Brouwer, the second Zermelo.

Brouwer used in his development of analysis infinite proofs and treated them mathematically as well-founded trees. He wrote with respect to

---

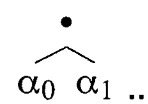
33 Compare (Hilbert, 1929), pp. 14-15, (Bernays, 1930), pp. 59-60, and the discussion in section 2.1.

them: "These *mental* mathematical proofs that in general contain infinitely many terms must not be confused with their linguistic accompaniments, which are finite and necessarily inadequate, hence do not belong to mathematics."[34] He added that this remark contains his "main argument against the claims of Hilbert's metamathematics". The well-founded trees of Brouwer's can be viewed as inductively generated sets of sequences of natural numbers; that is the essential claim of the bar-theorem. In the case of the constructive ordinals the inductive generation proceeds by the following rules (on the right-hand side I indicate the graphic representation of the ordinal):

$0 \ \varepsilon \ \mathbf{O}$

$\alpha \ \varepsilon \ \mathbf{O} \ \Rightarrow \ \alpha' \ \varepsilon \ \mathbf{O}$

$(\forall n) \ \alpha_n \ \varepsilon \ \mathbf{O} \ \Rightarrow \ \alpha := \sup \alpha_n \ \varepsilon \ \mathbf{O}$



Notice, it is the bar-theorem together with the continuity principle that implies the fan-theorem and thus the properties of the intuitionistic continuum so peculiar from a classical point of view; e.g. the uniform continuity of all real-valued functions on the closed unit interval.

Also Zermelo claimed that finite linguistic means are inadequate to capture the nature of mathematics and mathematical proof. In a brief note he argued: "Complexes of signs are not, as some assume, the true subject matter of mathematics, but rather *conceptually ideal relations* between the elements of a conceptually posited *infinite manifold*. And our systems of signs are only *imperfect* and *auxiliary means* of our *finite* mind, changing from case to case, in order to master at least in stepwise approximation the infinite, that we cannot survey *directly* and *intuitively*."[35] Zermelo suggested using an infinitary logic to overcome finitist restrictions. The concept of well-foundedness is fundamental for Zermelo's infinitary logic as well, but in

---

[34] In (Brouwer, 1927), footnote 8, p. 460.

[35] (Zermelo, 1931), p. 85.

an unrestricted set theoretic framework. Zermelo's investigations of infinitary systems can be found in (Zermelo, 1935).

D. REDUCTION. Ironically, the constructive consistency proofs of impredicative theories, mentioned at the end of section 1.2, use infinitary logical calculi; but the syntactic objects constituting them (namely, formulas and derivations) are treated in harmony with intuitionistic principles. The theories **F*** - in which the infinitary calculi are investigated and to which the impredicative theories are reduced - are extensions of intuitionistic number theory by definition and proof principles for constructive ordinals or other accessible i.d. [inductively defined] classes of natural numbers.[36] The above process of inductive generation for constructive ordinals can be expressed by an arithmetical formula A(X,x). The two crucial principles are

(O1) $(\forall x)(A(\mathbf{O},x) \Rightarrow \mathbf{O}(x))$, and

(O2) $(\forall x)(A(F,x) \Rightarrow F(x)) \Rightarrow (\forall x)(\mathbf{O}(x) \Rightarrow F(x))$.

The former expresses the closure principle for **O**, the latter the appropriate induction schema for any formula F. These principles and corresponding ones for other inductively defined classes are correct from an intuitionistic point of view; the theories **F*** are based on intuitionistic logic. Because of these facts we can claim that the consistency of the impredicative theories has been established relative to constructive theories.

2. PHILOSOPHICAL REFLECTIONS. The reductions of impredicative subsystems of analysis to intuitionistic theories of higher number classes or other distinguished inductively defined classes are certainly significant results; were not all impredicative definitions supposed to contain vicious circles? The question is, nevertheless, what has been achieved in a general, philosophical way. Gödel remarked that giving a constructive consistency proof for classical mathematics means "to replace its axioms [i.e. those of classical mathematics] about abstract entities of an objective Platonic realm by insights about the given

---

36 See (Buchholz e.a., 1981) and for an informal introduction the second part of (Sieg, 1984). - Accessible or deterministic i.d. classes are distinguished by the fact that all their elements have unique construction trees. If the construction trees for all elements of an i.d. class are finite, we say that the class is given by a finitary inductive definition. For a detailed discussion of these notions see (Feferman and Sieg, 1981), pp. 22-23, and Feferman's paper *Finitary inductively presented logics* for Logic Colloquium '88.

operations of our mind".[37] This pregnant formulation gives a most dramatic philosophical meaning to such proofs; it seems to me to be mistaken, however, in its radical opposition of classical and constructive mathematics and even in the very characterization of their subject matters. I prefer to formulate the task of such proofs as follows: they are to relate two aspects of mathematical experience; namely, the impression that mathematics has to do with abstract objects arranged in structures that are independent of us, and the conviction that the principles for some structures are evident, because we can grasp the build-up of their elements. I will argue that this is indeed central to the mediating task of the (modified) Hilbert program. The starting point of my argument is a re-analysis of the reductive goals of the original program; that will lead to the notion of "structural reduction" and to questions concerning its epistemological point.

2.1. STRUCTURAL REDUCTION. The description of Hilbert's program in section 1.2 brings out, appropriately, the goal of justifying the instrumentalist use of classical theories for the proof of true finitist statements; it captures also important features of Hilbert's approach in a natural way, for example his concern with "Methodenreinheit" and the method of ideal elements. And yet, it truncates the program by leaving out essential and problematic considerations. Hilbert and Bernays both argue for a more direct mathematical significance of consistency proofs: such proofs are viewed as the last desideratum in justifying the existential supposition of infinite structures made by modern axiomatic theories.[38] It is clearly this concern that links the program to Hilbert's first foundational investigations and to Dedekind's attempted consistency proofs. Dedekind considers consistency proofs also as a last desideratum, but there seems to be a decisive difference as to the nature of theories: for him the theories (of natural and real numbers) are not just formal systems with some instrumentalist use. On the contrary, they are contentually motivated, have a materially founded necessity, and mathematical efficacy. They play an important epistemological role by giving us a conceptual grasp of composite mathematical as well as physical phenomena; Dedekind claims, for

---

37 (Reid, 1970), p. 218; compare also Gödel's remarks in Hao Wang's *From Mathematics to Philosophy*, London, 1974, pp. 325-326.

38 "Existential supposition" is to correspond to the term "existentielle Setzung" that is used by Hilbert and Bernays as a quasi-technical term. The problem pointed to is presented as a central one in *Grundlagen der Mathematik I*; see the summary of the discussion on p.19 of that work. As to the role of the reflection principle, compare the informative remarks on pp. 43-44.

example, that it is only the theory of real numbers that enables us "to develop the conception of continuous space to a definite one".[39]

None of these points are lost in the considerations of Hilbert and Bernays. The contentual motivation of axiom systems, for example, plays a crucial role for them, as is clear from the very first chapter of *Grundlagen der Mathematik I* where the relation between contentual and formal axiomatics ("inhaltliche", respectively "formale Axiomatik") and its relevance for our knowledge is being discussed. "Formal axiomatics," they explain, "requires contentual axiomatics as a necessary supplement; it is only the latter that guides us in the selection of formalisms and moreover provides directions for applying an already given formal theory to an objective domain".[40] The basic conviction is that the contentual axiomatic theories are fully formalizable; formalisms, according to Hilbert (1928), provide "a picture of the whole science". Bernays (1930) discusses the completeness problem in detail and conjectures that elementary number theory is complete. Though there is "a wide field of considerable problems", Bernays claims, "this 'problematic' is not an objection against the standpoint taken by us". He continues, arguing as it were against the doubts of Brouwer's and Zermelo's:

We only have to realize that the [syntactic] formalism of statements and proofs we use to represent our conceptions does not coincide with the [mathematical] formalism of the structure we intend in our thinking. The [syntactic] formalism suffices to formulate our

---

39 (Dedekind, 1932), p. 340. - The underlying general position is persuasively presented in (Dedekind, 1854). Dedekind viewed it as distinctive for the sciences (not just the natural ones) to strive for "characteristic" and "efficacious" basic notions; the latter are needed for the formulation of general truths. The truths themselves have, in turn, an effect on the formation of basic notions: they may have been too narrow or too wide, they may require a change so that they can "extend their efficacy and range to a greater domain". Dedekind continues, and that just cannot be adequately translated: "Dieses Drehen und Wenden der Definitionen, den aufgefundenen Gesetzen und Wahrheiten zuliebe, in denen sie eine Rolle spielen, bildet die größte Kunst des Systematikers". In mathematics we encounter the same phenomenon, e.g. when extending the definition of functions to greater number domains. In contrast to other sciences, however, mathematics does not leave any room for arbitrariness in how to extend definitions. Here the extensions follow with "compelling necessity", if one applies the principle that "laws, that emerged from the initial definitions and that are characteristic for the notions denoted by them, are viewed as generally valid; then these laws in turn become the source of the generalized definitions..." What a marvelous general description of his own later work in algebra (in particular the introduction of ideals) and in his foundational papers, the guiding idea of which is formulated clearly on p. 434 of this very essay.

40 *Grundlagen der Mathematik I*, p. 2. - I translated by "an objective domain" the phrase "ein Gebiet der Tatsächlichkeit". - Similar remarks can be found in earlier, pre-Gödel papers; see especially the comprehensive and deeply philosophical (Bernays, 1930).

ideas of infinite manifolds and to draw the logical consequences from them, but in general it cannot combinatorially generate the manifold as it were out of itself.[41]

The close, but not too intimate connection between intended structure and syntactic formalism is to be exploited as the crucial means of reduction. This idea is captured in papers by Bernays through a mathematical image. (The papers are separated by close to fifty years; I emphasize this fact to point out that the remarks are not incidental, but touch the core of the strategy.) The first observation, from 1922, follows a discussion of Hilbert's *Grundlagen der Geometrie*.

Thus the axiomatic treatment of geometry amounts to this: one abstracts from geometry, given as the science of spatial figures, the purely mathematical component of knowledge [Erkenntnis]; the latter is then investigated separately all by itself. The spatial relations are *projected* as it were into the sphere of the mathematically abstract, where the structure of their interconnection presents itself as an object of purely mathematical thinking and is subjected to a manner of investigation focused exclusively on logical connections.

What is said here for geometry is stated for arithmetic in (Bernays, 1922) and for theories in general in (Bernays, 1970), where a sketch of Hilbert's program is supplemented by a clear formulation of the epistemological significance of such "projections".

In taking the deductive structure of a formalized theory ... as an object of investigation the [contentual] theory is *projected* as it were into the number theoretic domain. The number theoretic structure thus obtained is in general essentially different from the structure intended by the [contentual] theory. But it [the number theoretic structure] can serve to recognize the consistency of the theory from a standpoint that is more elementary than the assumption of the intended structure.

Recalling that - according to Hilbert - the axiomatic method applies in identical ways to different domains, these projections have a uniform character. Thus Hilbert's program can be seen to seek a *uniform structural reduction*: intended structures are projected through their assumed complete formalizations into the properly mathematical domain (of Kronecker's and Brouwer's), i.e. finitist mathematics. The equivalence of consistency and satisfiability was claimed or at least conjectured;[42] consequently, it seemed that the existence of intended

---

41 (Bernays, 1930), p. 59. The words in brackets were added by me to make the translation as clear as the German original. - The next longer quotations are taken from (Bernays, 1922 A), p. 96, and (Bernays, 1970), p. 186.

42 In (Bernays, 1930), p. 21, one finds the following phrase: "It is for this reason necessary to prove for every axiomatic theory the *satisfiability*, i.e. the *consistency* of its axioms." Compare also *Grundlagen der Mathematik I*, p. 19. - Minc and Friedman have shown that Gödel's completeness theorem for predicate logic can be established in a conservative extension of

structures would be secured by the mathematical solution of the purely combinatorial consistency problem. The principles used in the solution were of course to be finitist; the epistemological gain of such reductions is described in *Grundlagen der Mathematik I*:

> Formal axiomatics, too, requires for the checking of deductions and the proof of consistency in any case certain evidences, but with the crucial difference [when compared to contentual axiomatics] that this evidence does not rest on a special epistemological relation to the particular domain, but rather is one and the same for any axiomatics; this evidence is the primitive manner of recognizing truths that is a prerequisite for any theoretical investigation whatsoever.[43]

This reconstruction of the intent of Hilbert's program is supported most explicitly by Bernays (1922 and 1930). Let me focus briefly on the earlier paper, not to report on all its detailed points, but rather to depict the structure of its argumentation. The problem faced by the program is seen in the following way. In providing a rigorous foundation for arithmetic (taken in a wide sense to include analysis and set theory) one proceeds axiomatically and starts out with the assumption of a system of objects satisfying certain structural conditions. But in the assumption of such a system "lies something so-to-speak transcendental for mathematics, and the question arises, which principled position is to be taken [towards that assumption]". Bernays considers two "natural positions". The first, attributed to Frege and Russell, attempts to prove the consistency of arithmetic by purely logical means; this attempt is judged to be a failure. The second position is seen in counterpoint to the logical foundations of arithmetic: "As one does not succeed in establishing the logical necessity of the mathematical transcendental assumptions, one asks oneself, is it not possible to simply do without them". Thus one attempts a constructive foundation, replacing existential assumptions by construction postulates; that is the second position and is associated with Kronecker, Poincaré, Brouwer, and Weyl. The methodological restrictions to which this position leads are viewed as unsatisfactory, as one is forced "to give up the most successful, most elegant, and most proven methods only because one does not have a foundation for them from a particular standpoint". Hilbert takes from

---

primitive recursive arithmetic. (That is an obvious improvement of Bernays' proof of the completeness theorem in elementary number theory.) The result is of considerable interest in this connection, as it justifies the equivalence of consistency and satisfiability from a finitist point of view - at least for formal first order theories.

43 *Grundlagen der Mathematik I*, p. 2. The parenthetical remark is mine. - Bernays uses the term "primitive Erkenntnisweise" which I tried to capture by the somewhat unwieldy phrase "primitive manner of recognizing truths".

these foundational positions what is "positively fruitful": from the first, the strict formalization of mathematical reasoning; from the second, the emphasis on constructions. Hilbert does not want to give up the constructive tendency, but - on the contrary - emphasizes it in the strongest possible terms. The program, as described in 1.2, is taken as the tool for an alternative constructive foundation of all of classical mathematics.

It is not the case - as is so often claimed - that the difficult philosophical problems brought out by the axiomatic method and the associated structural view of mathematics were not seen. They motivated the enterprise and were seen perfectly clearly; however, it was hoped, perhaps too naively, to either avoid them directly in a systematic-mathematical development (by presenting appropriate models) or to solve them in the case of "fundamental" structures on the finitist basis. In any case, a so-to-speak absolute epistemological reduction was envisioned. These radical, philosophically motivated aspirations of Hilbert's program were blocked by Gödel's incompleteness theorems: according to the first theorem it is not possible, even in the case of natural numbers, to exclude systematically all contentual considerations concerning the intended structure; the second theorem implies that formal theories can be used at most as vehicles for partial structural reductions to strengthenings of the finitist basis. Bernays wrote in the epilog to his (1930), p. 61:

On the whole the situation is like this: Hilbert's proof theory - together with the discovery of the formalizability of mathematical theories - has opened a rich field of research, but the epistemological views that were taken for granted at its inception have become problematic.

At the inception of Hilbert's program, it seems, the epistemological views had not been dogmatically and unshakably fixed. As will be pointed out in the next section, Hilbert's original position had to be and was extended; in addition, to judge from (Bernays, 1922), the focus on finitist mathematics was viewed as part of an "Ansatz" to the solution of a problem. Having formulated the question as to a principled position towards the transcendental assumptions underlying the axiomatic foundations of arithmetic (see above), Bernays remarks, p. 11:

Under this perspective[44] we are going to try, whether it is not possible to give a foundation to these transcendental assumptions in such a way that only primitive intuitive knowledge (primitive anschauliche Erkenntnisse) is used.

Viewing the philosophical position in this more experimental spirit, we can complement the metamathematical reductive program by a philosophical one that addresses two central issues: (i) what is the nature and the role of the reduced structures? and (ii) what is the special character of the theories to which they are reduced? As to the latter issue, our greater metamathematical experience allows us to point to perhaps significant general features.

2.2. ACCESSIBLE DOMAINS. The reductive program I described in section 1.2. has been pursued successfully. I think there can be no reasonable doubt that (meta-) mathematically and, prima facie, also philosophically significant solutions have been obtained. As to the mathematical results it can be observed:

- A considerable portion of classical mathematical practice, including all of analysis, can be carried out in a small corner of Cantor's paradise that is consistent relative to the constructive principles formalized in intuitionistic number theory. This is not trivial, if one bears in mind that in particular for analysis non-constructive principles seemed to be necessary.

The metamathematical results concerning the relative consistency of impredicative theories speak also for themselves.

- The constructive principles formalized in intuitionistic theories for i.d. classes[45] allow us to recognize the relative consistency of certain impredicative theories. This is again not trivial, if one takes into account that any impredicative principle, from a broad constructive point of view, seemed to contain vicious circles.[46]

These relative consistency results provide material for critical philosophical analysis. After all, they raise implicitly the traditional question: "What is the (special) evidence of the mathematical principles used in (these) consistency proofs?" - The intuitionistic theories for i.d. classes formulate complex principles that are recognized by classical and constructivist mathematicians alike. On the one hand

---

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

[45] i.e. very special ones: higher number classes and, more generally, accessible i.d. classes.

[46] That point is clearly and forcefully made in Gödel's paper *Russell's Mathematical Logic*; see pp. 455-456 in the second edition of *Philosophy of Mathematics*, Benacerraf and Putnam (eds.), Cambridge, 1983.

they are more elementary than the principles used in their set theoretic justification, but on the other hand they cannot be given a direct (primitive) intuitive foundation. For a philosophical analysis that attempts to clarify extensions of the finitist standpoint and to explicate - relative to them - the epistemological significance of these particular results some clear and concrete tasks can be formulated.

At the very beginning of the development of Hilbert's program one finds an extension not of, but rather towards the finitist standpoint. Originally, Hilbert intended to make do with a mathematical basis that did not even include the "Allgemeinbegriff der Ziffer": all mathematical knowledge (Erkenntnis) was to be reduced to primitive formal evidence.[47] This extremely restricted undertaking was given up quickly: how could the central goal of the program, consistency, be formulated within its framework? A "finitist standpoint" that is to serve as the basis for Hilbert's investigations cannot be founded on the intuition of concretely given objects; it rather has to correspond to a standpoint, as Bernays explained, "where one already *reflects* on the general characteristics of intuitive objects".[48] A first task presents itself.

---

47 This is reported in (Bernays, 1946), p. 91; an example of the form of this quite primitive evidence can be found l.c. p. 89, but compare also (Bernays, 1961), p. 169. As to the historical point see (Bernays, 1967), p. 500: "At the time of his Zurich lecture Hilbert tended to restrict the methods of proof-theoretic reasoning to the most primitive evidence. The apparent needs of proof theory induced him to adopt successively those suppositions which constitute what he then called the 'finite Einstellung'."

48 From (Bernays,1930), p. 40. The context is this: "Diese Heranziehung der Vorstellung des Endlichen [used from the finitist standpoint] gehört freilich nicht mehr zu demjenigen, was von der anschaulichen Evidenz notwendig in das logische Schließen eingeht. Sie entspricht vielmehr einem Standpunkt, bei dem man bereits auf die allgemeinen Charakterzüge der anschaulichen Objekte *reflektiert*." - This is a clearer and more promising starting point for an analysis than the one offered through Hilbert's own characterization in (van Heijenoort), p. 376. Important investigations, in addition to those of Bernays, have been contributed by i.a. Kreisel, Parsons, and Tait. See (Tait, 1981) and the references to the literature given there. However, in this systematic context (and also for a general discussion of feasibility) I should point out that weakenings of the finitist standpoint are of real interest; a penetrating investigation is carried out by R. Gandy in *Limitations of mathematical knowledge*, in: Logic Colloquium '80, Amsterdam 1982, pp. 129-146. Notice that the type-token problematic has to be faced already from weakened positions. - The crux of the additional problematic was compressed by Bernays into one sentence: "Wollen wir ... die Ordnungszahlen als eindeutige Objekte, frei von allen unwesentlichen Zutaten haben, so müssen wir jeweils das bloße Schema der betreffenden Wiederholungsfigur als Objekt nehmen, was eine sehr hohe Abstraktion erfordert." (Bernays, 1930, pp. 31-32.) It is for these formal objects that the "Gedankenexperimente" are carried out, that play such an important role in *Grundlagen der Mathematik I* (p. 32) for characterizing finitist considerations.

(I) Analyze this reflection for the natural numbers (and the elements of other accessible i.d. classes given by finitary inductive definitions) and investigate, whether and how induction and recursion principles can be based on it.

Without attempting to summarize the extended (and subtle) discussion in (Bernays, 1930) I want to point to one feature that is crucial in it and important for my considerations. For Bernays the natural numbers (as ordinals) are the simplest formal objects; they are obtained by formal abstraction and are representable by concrete objects, numerals. This representation has a very special characteristic: the representing things contain ("enthalten") the essential properties of the represented things in such a way that relations between the latter objects obtain between the former and can be ascertained by considering those.[49] It is this special characteristic that has to be given up when extending the finitist standpoint: symbols are no longer carrying their meaning on their face, as they cannot exhibit their build-up.[50] For the consistency proofs mentioned in Remark D above one uses accessible i.d. classes of natural numbers; numerals for the elements of such a class are now understood as denoting infinite objects, namely the unique construction trees associated effectively with the elements. So we have as a generalization of (I) - to begin with - the task:

(II) Extend the reflection to constructive ordinals and the elements of other accessible i.d. classes and investigate, whether and how induction and recursion principles can be based on it.

One delicate question has not been taken into account here. For the consistency proofs of strong impredicative theories the definition of

---

49 This is found on pp. 31-32, in particular in footnote 4 on that very page. The general problematic is also discussed in (Bernays, 1935 A), pp. 69-71. Compare the previous footnote to recognize that not an isomorphic representation by a particular, physically realized object is intended. The uniform character of the generation and the local structure of the schematic "iteration figure" are important.

50 Gödel described in his *Über eine bisher noch nicht benützte Erweiterung des finiten Standpunktes*, Dialectica 12 (1958), pp. 280-287, a standpoint that extends the finitist one and that is appropriate for the consistency proofs for number theory given by Gentzen and Gödel himself. The starting point of this proposal are considerations of Bernays - e.g. in his (1935) and (1941) - concerning the question, "In what way does intuitionism go beyond finitism?" Bernays's answer is "Through its abstract notion of consequence." And it is this abstract concept that is to be partially captured by the computable functionals of finite type. - Note that the specifically finitist character of mathematical objects requires, according to Gödel, that they are "finite space-time configurations whose nature is irrelevant except for equality and difference". This seems to conflict with Bernays's analysis pointed to in the previous two footnotes.

i.d. classes has to be iterated uniformly; that means the branching in the well-founded construction trees is not only taken over natural numbers, but also over other already obtained i.d. classes. These trees are of much greater complexity. For example, it is no longer possible - as it is in the case of constructive ordinals - to generate effectively arbitrary finite subtrees; that has to be done now through procedures that are effective relative to already obtained number classes. Thus we have to modify (II).

(III) Extend the reflection to uniformly iterated accessible i.d. classes, in particular to the higher constructive number classes.

Buchholz and Pohlers used in their investigations of theories for i.d. classes systems of ordinal notations. It is clearly in the tradition of Gentzen and Schütte to use for consistency proofs the principle of transfinite induction along suitable ordinals (represented through effective notation systems). But in parallel to proving the consistency of formal theories by such means, Gentzen wrote in a letter to Bernays of March 3, 1936, one has to pursue a complementary task, namely " ... to carry out investigations with the goal of making the validity of transfinite induction constructively intelligible for higher and higher limit numbers". It is only through such investigations that the philosophical point of consistency proofs can be made, namely, to secure a theory by reliable ("sichere") means. Gentzen's task is included in (III), since the systems of notations used in Buchholz and Pohlers' work are generated as accessible i.d. classes, and their well-ordering is recognized through the proof principle for these i.d. classes. These systems of notations were quite complicated, but in their latest and conceptually best form they are given by clauses of the same character as those for higher number classes (Buchholz, 1990). I mentioned earlier the consistency problem for the subsystem of analysis with Π^1_2-comprehension; this is not only a mathematical problem, but also an open conceptual problem, as new "constructive" objects are needed for a satisfactory "constructive" solution.[51]

The number classes provide special cases in which generating procedures allow us to grasp the intrinsic build-up of mathematical

51 Work of Pohlers and his students to extend the method of local predicativity make it most likely that a close connection to set theory (in particular the study of large cardinals and the fine structure of the constructible universe L) is emerging.

objects.[52] Such an understanding is a fundamental and objective source of our knowledge of mathematical principles for the structures or domains constituted by those objects: is it not the case that the definition- and proof principles follow directly this comprehended build-up? Clearly, we have to complement an analysis of this source - as requested in (I)-(III) - by formulating (the reasons for the choice of) suitable deductive frames in which the mathematical principles are embedded. Thus there are substantial questions concerning the language, logic, and the exact formulation of schematic principles. But notice that for the concerns here these questions are not of primary importance. For example, the restriction to intuitionistic logic is rather insignificant: the double-negation translation used by Gödel and Gentzen to prove the consistency of classical relative to intuitionistic arithmetic can be extended to a variety of theories to yield relative consistency results. Indeed, Friedman showed for arithmetic, finite type theories, and Zermelo-Fraenkel set theory that the classical theories are Π^0_2-conservative over their intuitionistic version. Using Friedman's strikingly simple techniques Feferman and Sieg (1981) established such conservation results for some subsystems of analysis and also for the theories of iterated inductive definitions.[53] In the latter case it is the *further* restriction to accessible i.d. classes that is (technically difficult and) conceptually significant.

Disregarding the traditional constructive traits of the objects considered up to now we can extend the basic accessibility conception from i.d. classes of natural numbers to broader domains. A comprehensive framework for the "inductive or rule-governed generation" of mathematical objects is given in (Aczel, 1977); it is indeed so general that it encompasses finitary i.d. classes, higher number classes, the set-theoretic model of Feferman's theory T_0 of explicit mathematics and of other constructive theories (like Martin-Löf's), but also segments of the cumulative hierarchy. Clearly, not all of Aczel's i.d. classes have the distinctive feature of accessible i.d. classes; those whose elements do have unique associated well-founded "construction" trees are called deterministic and, here again, accessible. Segments of the cumulative hierarchy - that contain some

---

52 By calling their build-up "intrinsic" I point again to the parallelism between the generating procedure and the structure of the intended object; compare the case under discussion here, for example, with that of the computable functionals of finite type.

53 The theory of arithmetic properties and ramified systems were shown to be Π^0_2-conservative over their intuitionistic versions in (Feferman and Sieg), pp. 57-59. - The generality of Friedman's techniques was brought out by Leivant in *Syntactic translations and provably recursive functions*, Journal of Symbolic Logic 50 (3), 1985, 682 - 688.

ordinals (0, ω, or large cardinals) and are closed under the powerset, union, and replacement operations - are in this sense accessible: the uniquely determined transitive closure of their elements are "construction" trees.[54] Here, as above, we have the task of explicating (the difficulties in) our understanding of generation procedures. After all, for accessibility to have any cognitive significance such an understanding has to be assumed. The latter is in the present case relatively unproblematic, if we restrict attention to hereditarily finite sets; then we have an understanding of the combinatorial generation procedures and, in particular, of forming arbitrary subcollections. Indeed, ZF^-, i.e. Zermelo-Fraenkel set theory without the axiom of infinity, is equivalent to elementary number theory. The powerset operation is the critical generating principle; its strength when applied to infinite sets is highlighted by the fact that ZF without the powerset axiom is equivalent to second order arithmetic.[55] But if we do assume an understanding of the set theoretic generation procedure for a segment of the cumulative hierarchy, then it is indeed the case that the axioms of ZF^- together with a suitable axiom of infinity "force themselves upon us as being true" - in Gödel's famous phrase; they just formulate the principles underlying the "construction" of the objects in this segment of the hierarchy.[56] In summary, we have a wealth of accessible domains, and it seems that we can understand the pertinent mathematical principles quasi-constructively, as we grasp the build-up of the objects constituting such structures.

---

54 Here is the basis for ε-induction and recursion. It seems to me that in this context the discussion and results concerning Fraenkel's Axiom of Restriction would be quite pertinent.

55 Using powerset one obtains not the elements of a subclass from a given set, but rather all subclasses in one fell swoop. It is this utter generality that creates a difficulty even when the given set is that of the natural numbers; see the comprehensive discussion of Bernays's views in (Müller). The difficulty is very roughly this: in terms of the basic operations one does not have "prior" access to all elements of the powerset, unless one chooses a second order formulation of replacement. That would allow the joining of arbitrary subcollections, but "arbitrary subcollection" has then to be understood in whatever sense the second order variables are interpreted. - A focus on definable subsets leads to the ramified hierarchy, to Gödel's constructible sets, and to the consideration of subsystems of ZF. The investigations concerning subsystems of analysis can be turned into investigations of natural subsystems of set theory. That was done by G. Jäger. His work is presented in *Theories for admissible sets - A unifying approach to proof theory;* Bibliopolis, Naples, 1986.

56 This reason for accepting the axioms of ZF seems to be (at least) consonant with Gödel's analysis in *What is Cantor's continuum problem?* and does not rest on the strong Platonism in the later supplement of the paper. The conceptual kernel of this analysis goes back to Zermelo's penetrating paper of 1930. A discussion of the rich literature on the "iterative conception of set" is clearly not possible here. - Notice that the length of iteration is partly determined through the adopted axiom of infinity built into the base clause of the i.d. definition.

2.3. CONTRASTS. The "ontological status" of mathematical objects has not been discussed. The reason is this: I agree with the subtle considerations of Bernays in the essay *Mathematische Existenz und Widerspruchsfreiheit* and suggest only one amendation, namely to distinguish the "methodical frames" (methodische Rahmen) by having their objects constitute accessible domains. The contrast between "platonist" and "constructivist" tendencies is then not localized in the stark opposition formulated by Gödel; it comes to light rather in refined distinctions concerning the admissibility of operations, of their iteration, and of deductive principles. In this way, it seems to me, methodical frames are not only distinguishable from each other, but also epistemologically differentiated from "abstract" theories formulated within particular frames. I want to focus on this differentiation now and contrast the quasi-constructive aspect of mathematical experience I have been analyzing to - what I suggest to call - its "conceptional" aspect. The latter aspect is most important for mathematical practice and understanding, but also for the sophisticated uses of mathematics in physics; it is quite independent of methodical frames.

As a first step let us consider Dedekind's way of comprehending the accessible domain of natural numbers. The informal analysis underlying *Was sind und was sollen die Zahlen?* described in his letter to Keferstein, (Dedekind 1890), starts out with the question:

What are the mutually independent fundamental properties of the sequence N, that is, those properties that are not derivable from one another but from which all others follow? And how should we divest these properties of their specifically arithmetic character so that they are subsumed under more general notions and under activities of the understanding *without* which no thinking is possible at all but *with* which a foundation is provided for the reliability and completeness of proofs and for the construction of consistent notions and definitions?

One is quickly led to infinite systems that contain a distinguished element 1 and are closed under a successor operation ϕ. Dedekind notes that such systems may contain non-standard "intruders" and that their exclusion from **N** was for him "one of the most difficult points" in his analysis; "its mastery required lengthy reflection."[57] The

[57] The minimalist understanding is taken for granted when it is claimed that the induction principle is evident from the finitist point of view. Thus, even this seemingly most elementary explanation of induction leaves us with a certain "impredicativity". "The same holds", Parsons rightly argues, "for other domains of objects obtained by iteration of operations yielding new objects, beginning with certain initial ones. It seems that the impredicativity will loose its

notion of chain allows him to give "an unambiguous conceptual foundation to the distinction between the [standard] elements n and the [non-standard] t". By means of this notion he captures the informal understanding that the natural numbers are just those objects that are obtained from 1 by finite iteration of ϕ, or rather the objects arising from any simply infinite system "by entirely disregarding the special nature of its elements, and retaining only their distinguishability and considering exclusively those relations that obtain between them through the ordering mapping ϕ".[58] He continues: "Taking into account this freeing of the elements from every other content (abstraction), we can justifiably call the [natural] numbers a free creation of the human mind." How startlingly close is this final view of natural numbers to that arrived at by Bernays through "formal abstraction"!

For Dedekind the considerations (concerning the existence of infinite systems) guarantee that the notion "simply infinite system" does not contain an internal contradiction.[59] The "purely logical" and presumably reliable foundation did not, of course, allow this goal to be reached. In 1.1 I emphasized methodological parallels between Dedekind's treatment of the natural and real numbers; here I want to bring out a most striking difference. We just saw that Dedekind's analysis of natural numbers is based on a clear understanding of their accessibility through the successor operation. This understanding allows the distinction between standard objects and "intruders" and motivates directly the axioms for simply infinite systems. Given the build-up of the objects in their domains, it is quite obvious that any two simply infinite systems have to be isomorphic via a unique isomorphism. By way of contrast consider the axioms for dense linear orderings without endpoints; their countable models are all isomorphic, but Cantor's back-and-forth argument for this fact exploits broad structural conditions and not the local build-up of objects. The last observation gives also the reason, why these axioms do not have an "intended model": it is the accessibility of objects via operations not just the categoricity of a theory that gives us such a model. Similar

---

significance only from points of view that leave it mysterious why mathematical induction is evident." (Parsons, 1983), pp.135-136.

58 Section 73 of (Dedekind, 1888).

59 What is so astonishing in every re-reading of Dedekind's essay is the conceptual clarity, the elegance and generality of its mathematical development. As to the latter, it really contains the general method of making monotone inductive definitions explicit. The treatment of recursive definitions is easily extendible; that it has to be restricted, in effect, to accessible i.d. classes is noted. Compare (Feferman and Sieg), footnotes 2 and 4 on p. 75.

remarks apply to the reals, as the isomorphism between any two models of the axioms for complete, ordered fields is based on the topological completeness requirement, not any build-up of their elements. (That requirement guarantees the continuous extendibility of any isomorphism between their respective rationals.)

This point is perhaps brought out even more clearly by a classical theorem of Pontrjagin's stating, that connected, locally-compact topological fields are either isomorphic to the reals, the complex numbers, or the quaternions. For this case Bourbaki's description, that the "individuality" of the objects in the classical structures is induced by the superposition of structural conditions, is so wonderfully apt; having presented the principal structures (order, algebraic, topological) he continues:

> Farther along we come finally to the theories properly called particular. In these the elements of the sets under consideration, which, in the general structures have remained entirely indeterminate, obtain a more definitely characterized individuality. At this point we merge with the theories of classical mathematics, the analysis of functions of real or complex variable, differential geometry, algebraic geometry, theory of numbers. But they have no longer their former autonomy; they have become crossroads, where several more general mathematical structures meet and react upon one another.[60]

Here we are dealing with abstract notions, distilled from mathematical practice for the purpose of comprehending complex connections, of making analogies precise, and to obtain a more profound understanding; it is in this way that the axiomatic method teaches us, as Bourbaki expressed it in Dedekind's spirit (l.c., p. 223),

> to look for the deep-lying reasons for such a discovery [that two, or several, quite distinct theories lend each other "unexpected support"], to find the common ideas of these theories, buried under the accumulation of details properly belonging to each of them, to bring these ideas forward and to put them in their proper light.

Notions like group, field, topological space, differentiable manifold fall into this category, and (relative) consistency proofs have here indeed the task of establishing the consistency of abstract notions relative to accessible domains. In Bourbaki's enterprise one might see this as being done relative to (a segment of) the cumulative hierarchy. But note, this consideration cuts across traditional divisions, as it pertains not only to notions of classical mathematics, but also to some of constructive mathematics. A prime example of the latter is that of

---

60 p. 229 of N. Bourbaki, *The architecture of mathematics*, Math. Monthly (57), 1950, pp. 221-232. The natural numbers are not obtained at a crossroad.

a choice sequence introduced by Brouwer into intuitionistic mathematics to capture the essence of the continuum; the consistency proof of the theory of choice sequences relative to the theory of (non-iterated) inductive definitions can be viewed as fulfilling exactly the above task.[61] The restriction of admissible operations (and deductive principles) can lead to the rejection of abstract notions; that comes most poignantly to the surface in the philosophical dispute between Kronecker and Dedekind, but also in Bishop's derisive view of Brouwer's choice sequences. Bishop is not only scornful of the "metaphysical speculation" underlying the notion of choice sequence, but he also views the resulting mathematics as "bizarre". (Bishop, 1967), p. 6.

CONCLUDING REMARKS. The conceptional aspect of mathematical experience and its profound function in mathematics has been neglected almost completely in the logico-philosophical literature on the foundations of mathematics.[62] Abstract notions have been important for the internal development of mathematics, but also for sophisticated applications of mathematics in physics and other sciences to organize our experience of the world. It seems to me to be absolutely crucial to gain genuine insight into this dual role, if we want to bring into harmony, as we certainly should, philosophical reflections on mathematics with those on the sciences.

Results of mathematical logic do not give precise answers to large philosophical questions; but they can force us to think through philosophical positions. Broad philosophical considerations do not provide "foundations" for mathematics; but they can bring us to raise mathematical problems. We shall advance our understanding of mathematics only if we continue to develop the dialectic of mathematical investigation and philosophical reflection; a dialectic that has to be informed by crucial features of the historical development of its subject. In Brecht's *Galileo Galilei* one finds the remark:

A main reason for the poverty of the sciences is most often imagined wealth. It is not their aim to open a door to infinite wisdom, but rather to set bounds to infinite misunderstanding.[63]

---

61 Kreisel and Troelstra, *Formal systems for some branches of intuitionistic analysis*, Annals of Mathematical Logic 1(3), 1970, pp. 229-387. It is really just the theory of the second constructive number-class that is needed.

62 The exception are papers of Bernays; there it is absolutely central.

63 The German text is: "Eine Hauptursache der Armut der Wissenschaften ist meist eingebildeter Reichtum. Es ist nicht ihr Ziel, der unendlichen Weisheit eine Tür zu öffnen,

What is said here for the sciences holds equally for mathematical logic and philosophy.

BIBLIOGRAPHY

Abrusci, V. M.

1980 'Proof', 'Theory', and 'Foundations' in Hilbert's mathematical work from 1885 to 1900; in: Dalla Chiara (ed.), *Italian Studies in the Philosophy of Science,* Dordrecht 1980, 453-491.

Aczel, P.

1977 An introduction to inductive definitions; in: *Handbook of Mathematical Logic*, J. Barwise (ed.), Amsterdam, 739-782.

Aspray, W. and Kitcher, P. (eds.)

1988 *History and Philosophy of Modern Mathematics*, Minnesota Studies in the Philosophy of Science, Volume XI, Minneapolis.

Baire, R. ; Borel, E.; Hadamard, J.; Lebesgue, H.

1905 Cinq lettres sur la théorie des ensembles; Bulletin de la Société Mathématique de France 33, 261-273. (Translated in Moore's book *Zermelo's Axiom of Choice*, New York, Heidelberg, Berlin 1982, 311-320.)

Bernays, P.

1922 Über Hilberts Gedanken zur Grundlegung der Arithmetik; Jahresberichte DMV 31, 10-19.

---

sondern eine Grenze zu setzen dem unendlichen Irrtum." In its "application" to philosophy it mirrors (for me) the views of the man who influenced so deeply Dedekind, Kronecker, Hilbert, ... ; they are reported in (Kummer), p. 340: "Er [Dirichlet] pflegte von der Philosophie zu sagen, es sei ein wesentlicher Mangel derselben, dass sie keine ungelösten Probleme habe wie die Mathematik, dass sie sich also keiner bestimmten Grenze bewusst sei, innerhalb deren sie die Wahrheit wirklich erforscht habe und über welche hinaus sie sich vorläufig bescheiden müsse, nichts zu wissen. Je grössere Ansprüche auf Allwissenheit die Philosophie machte, desto weniger vollkommen klar erkannte Wahrheit glaubte er ihr zutrauen zu dürfen, da er aus eigener Erfahrung in dem Gebiete seiner Wissenschaft wusste, wie schwer die Erkenntnis der Wahrheit ist, und welche Mühe und Arbeit es kostet, dieselbe auch nur einen Schritt weiter zu führen."

1922A Hilberts Bedeutung für die Philosophie der Mathematik; Die Naturwissenschaften 4, 93-99.

1930 Die Philosophie der Mathematik und die Hilbertsche Beweistheorie; in: (Bernays, 1976), 17-61.

1935 Hilberts Untersuchungen über die Grundlagen der Arithmetik; in: (Hilbert, 1935), 196-216.

1935A Über den Platonismus in der Mathematik; in: (Bernays, 1976), 62-78.

1941 Sur les questions méthodologiques actuelles de la théorie hilbertienne de la démonstration; in: *Les entretiens de Zurich sur les fondements et la méthode des sciences mathématiques* , F. Gonseth (ed.), 1938, Zurich 1941, 144-152.

1946 Gesichtspunkte zum Problem der Evidenz; in: (Bernays, 1976), pp. 85-91.

1950 Mathematische Existenz und Widerspruchsfreiheit; in: (Bernays, 1976), 92-106.

1967 Hilbert, David; in: *Encyclopedia of Philosophy*, P. Edwards (ed.), New York and London, 1967, 496-504.

1970 Die Schematische Korrespondenz und die Idealisierten Strukturen; in: (Bernays, 1976), 176-188.

1976 *Abhandlungen zur Philosophie der Mathematik*; Wissenschaftliche Buchgesellschaft, Darmstadt.

Börger, E. (ed.)

1987 *Computation Theory and Logic*; Springer Lecture Notes in Computer Science 270, Berlin, Heidelberg, New York.

Brouwer, L.E.J.

1927 Über Definitionsbereiche von Funktionen; Mathematische Annalen 97, 60-75; translated in: van Heijenoort, 446-463.

Buchholz, W.

1990 Proof theory of iterated inductive definitions revisited; to appear in: Archive for Mathematical Logic.

Buchholz, W., Feferman, S., Pohlers, W. Sieg, W.

1981 *Iterated Inductive Definitions and Subsystems of Analysis: Recent Proof-theoretical Studies*; Springer Lecture Notes in Mathematics 897, Berlin, Heidelberg, New York.

Cantor, G.

1932 *Gesammelte Abhandlungen mathematischen und philosophischen Inhalts*; E. Zermelo (ed.); Berlin.

Dedekind, R.

1854 Über die Einführung neuer Funktionen in der Mathematik; in: (Dedekind, 1932), 428-438.

1872 *Stetigkeit und irrationale Zahlen*; in: (Dedekind, 1932), 315-324.

1877 Sur la théorie des nombres entiers algébriques; Bulletin des Sciences mathématiques et astronomiques, 1-121; partially reprinted in (Dedekind, 1932), 262-296.

1888 Was sind und was sollen die Zahlen? in: (Dedekind, 1932), 335-391.

1890 Letter to Keferstein; in: van Heijenoort, 98-103.

1932 *Gesammelte mathematische Werke*, Dritter Band; Fricke, Noether, and Ore (eds.), Braunschweig,1932.

Dieudonné J.

1970 The work of Nicholas Bourbarki; American Mathematical Monthly 77, 134-145.

Dirichlet, G. P. Lejeune

1837 Beweis des Satzes, dass jede unbegrenzte arithmetische Progression, deren erstes Glied und Differenz ganze Zahlen ohne gemeinschaftlichen Faktor sind, unendlich viele Primzahlen enthält; reprinted in (Dirichlet, 1889), 313-342.

1838 Sur l'usage des séries infinies dans la théorie des nombres; reprinted in (Dirichlet, 1889), 357-374.

1839/40 Recherches sur diverses applications de l'analyse infinitésimale à la théorie des nombres; reprinted in (Dirichlet, 1889), 411-496.

1889 *Gesammelte Werke I*, edited by L. Kronecker, Berlin, 1889.

1897 *Gesammelte Werke II*, edited by L. Kronecker and continued by L. Fuchs, Berlin 1897. (Both volumes were reprinted by Chelsea in one volume, New York, 1969.)

Edwards, H.

1988 Kronecker's place in history; in: (Aspray and Kitcher, 1988), 139-144.

Feferman, S.

1977 Theories of finite type related to mathematical practice; in: J. Barwise (ed.), *Handbook of Mathematical Logic*, Amsterdam, 913-971.
1988 Hilbert's Program relativized: proof-theoretical and foundational reductions, Journal of Symbolic Logic, 53 (2), 364-384.

Feferman, S. und Sieg, W.
1981 Iterated inductive definitions and subsystems of analysis; in: (Buchholz, e.a., 1981), 16-77.

Guillaume, M.
1985 Axiomatik und Logik; in: *Geschichte der Mathematik, 1700-1900*, J. Dieudonné (ed.), Braunschweig, 748-882.

Hilbert, D.
1900 Über den Zahlbegriff; Jahresberichte DMV 8, 180-194.
1902 Sur les problèmes futurs des mathématiques; in: Compte Rendu du Deuxième Congrès International des Mathématiciens, Paris, 59-114.
1904 Über die Grundlagen der Logik und Arithmetik; in: *Grundlagen der Geometrie*, 5. edition, Leipzig and Berlin, 1922, 243-258; translated in: van Heijenoort, 129-138.
1928 Die Grundlagen der Mathematik; Abhandlungen aus dem mathematischen Seminar der Hamburgischen Universität 6 (1/2), 65-85.
1929 Probleme der Grundlegung der Mathematik; in: K. Reidemeister (ed.), *Hilbert*, Berlin, 9-19; appeared first in: Math. Annalen 102(1), 1-9.
1931 Die Grundlegung der elementaren Zahlenlehre; Math. Annalen 104(4), 485-494.
1935 *Gesammelte Abhandlungen*, 3. Band; Berlin.

Klein, F.
1926 *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, Teil I, Berlin.

Kreisel, G.
1958 Hilbert's programme; Dialectica 12, 346-372; reprinted in: *Philosophy of mathematics*, Benacerraf, P. and Putnam, H. (eds.), Cambridge, 1983, 207-238.
1965 Mathematical Logic; in: *Lectures on modern mathematics*, vol. III, T.L. Saaty (ed.), New York, 95-195.
1968 Survey of Proof Theory; Journal of Symbolic Logic 33, 321-388.

Kronecker, L.

1886 Über einige Anwendungen der Modulsysteme; Crelles Journal für die reine und angewandte Mathematik 99, 329-371, also: Werke III, 145-208.

1887 Über den Zahlbegriff; in: Werke, Band III, 251-274.

1895/1930 *Werke*, 5 volumes, Leipzig.

1901 *Vorlesungen zur Zahlentheorie*; K. Hensel (ed.), Leipzig.

Kummer, E.E.

1860 Gedächtnisrede auf Gustav Peter Lejeune Dirichlet; reprinted in (Dirichlet, 1897), 311-344.

Lipschitz, R.

1986 *Briefwechsel mit Cantor, Dedekind, Helmholtz, Kronecker, Weierstrass*; Vieweg, Braunschweig.

Luckhardt, H.

1989 Herbrand-Analysen zweier Beweise des Satzes von Roth: polynomiale Anzahlschranken; Journal of Symbolic Logic 54(1), 234-263.

Moore, G.H.

1988 The emergence of first-order logic; in: (Aspray and Kitcher), 95-138.

Müller, G.H.

1981 Framing mathematics; Epistemologia 4(1), 253-286.

Parsons, C.D.

1983 The impredicativity of induction; in: *How many questions?* - Essays in honor of Sidney Morgenbesser, Cauman, Levi, Parsons, Schwartz (eds.), Indianapolis, 132-153.

Purkert, W. and Ilgauds, H. J.

1987 *Georg Cantor*; Basel, Boston, Stuttgart.

Reid, C.

1970 *Hilbert*; Springer-Verlag, Berlin, Heidelberg, New York.

Sieg, W.

1984 Foundations for Analysis and Proof Theory; Synthese 60 (2), 159-200.

1985 Reductions of Theories for Analysis; in: *Foundations of Logic and Linguistics*, G. Dorn and P. Weingartner (eds.) New York and London, 199-230.

1988 Hilbert's Program Sixty Years Later; Journal of Symbolic Logic, 53(2), 338-348.

Simpson, S.G.

1988 Partial Realizations of Hilbert's Program; Journal of Symbolic Logic, 53(2), 349-363.

Smorynski, C.

1977 The incompleteness theorems; in: *Handbook of Mathematical Logic*, J. Barwise (ed.), Amsterdam, 821-866.

Stein, H.

1988 Logos, Logic, Logistike: Some philosophical remarks on the 19th century transformation of mathematics; in: (Aspray and Kitcher), 238-259.

Tait, W.W.

1981 Finitism; Journal of Philosophy 78, 524-546.

van Heijenoort, J. (ed.)

1967 *From Frege to Gödel, a source book in mathematical logic, 1879-1931*; Harvard University Press, Cambridge.

Wiener, H.

1891 Über Grundlagen und Aufbau der Geometrie; Jahresberichte der DMV 1, 45-48.

Zassenhaus, H.J.

1975 On the Minkowski-Hilbert dialogue on mathematization; Canadian Math. Bulletin 18, 443-461.

Zermelo, E.

1930 Über Grenzzahlen und Mengenbereiche; Fund. Math. 16, 29-47.

1931 Über Stufen der Quantifikation und die Logik des Unendichen; Jahresberichte DMV 41, 85-88.

1935 Grundlagen einer allgemeinen Theorie der mathematischen Satzsysteme; Fund. Math. 25, 136-146.

Here are the references to the four papers I mentioned in the first note concerning the re-publication of this essay:

1999 Hilbert's Programs: 1917-1922; Bulletin of Symbolic Logic 5, 1-44.

2002 Beyond Hilbert's Reach?; in: *Reading Natural Philosophy*, D.B. Malament (ed.), Open Court, Chicago, 363-405.

2004 (with Dirk Schlimm) Dedekind's Analysis of Number: systems and axioms; Synthese.

2005 (with Mark Ravaglia) David Hilbert and Paul Bernays, *Grundlagen der Mathematik*; in: *Landmark Writings in Western Mathematics: Case Studies, 1640-1940*, I. Grattan-Guinness (ed.), 19 pp.