Aspects of Mathematical Experience

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October 1993

Report CMU-PHIL-42

Carnegie Mellon Philosophy Methodology Logic

Pittsburgh, Pennsylvania 15213-3890

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Contribution to: Proceedings of the conference on **Philosophy of Mathematics,**
Budapest, May 20-22, 1993

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In the current discussion on philosophy of mathematics some do as if systematic foundational work supported an exclusive alternative between Platonism and Constructivism; others do as if such mathematical and logical research were deeply misguided and had no bearing on our understanding of mathematics. Both attitudes prevent us from grasping insights that underlie such work and from appreciating significant results that have been obtained. In consequence, they keep us from turning attention to the task of understanding the role of *accessible domains for foundational theories* and that of *abstract structures for mathematical practice.*

This twofold task derives from a probing perspective that takes seriously traditional epistemological concerns, but that does not respect time-honored boundaries drawn for philosophical convenience. It will be approached mainly through work that has been done during the last seventy years on versions of Hilbert's Program. Such an avenue may be surprising, because the stand that was taken in the foundational discussion by Hilbert, Bernays, and their collaborators is widely perceived as extremely narrow and

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[1] My considerations are based on two papers of mine: the first, *Relative Consistency and Accessible Domains,* was published in Synthese 84, 1990, pp. 259-297; the second, *Mechanical Procedures and Mathematical Experience,* will appear in "Mathematics and Mind", edited by A. George, Oxford University Press. Here, I focus squarely on broader strategic points and rely for details concerning the relevant (meta-) mathematical results, historical connections, and conceptual analyses on those earlier papers.

technical. So I will give in section 2 a revisionary description of Hilbert's Program and sketch in section 3 some results that have been obtained within a *general reductive program.*

A prerequisite for Hilbert's Program is the effective or formal presentation of mathematical thought. Gb'del took his incompleteness theorems as refuting any form of "pure formalism", in particular the variety (he thought to be) underlying Hilbert's Program. The discussion of Gödel's reflections on this issue, in section 4, will lead me to focus on two aspects of mathematical experience. The first is the *quasi-constructive aspect,* and it has to do with accessible domains; the second is the *conceptional* aspect, and it deals with axiomatically characterized abstract structures. These two aspects are discussed in sections 5 and 6. In the seventh and final section I come back to the question of "mechanizing" mathematical thought and contrast Turing's views with Gödel's.

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Are the results of contemporary proof theory significant for the foundational concerns that motivated Hilbert's Program, and are those concerns connected to insightful reflections on the nature of mathematics? - Before we can assess answers to either question, we have to be clear about the specific foundational concerns and the general character of solutions proposed by the program. The broad background is provided by striking developments in 19-th century mathematics, namely the emergence of set theory, the discovery of set theoretic foundations for analysis, and the rise of modern axiomatics with a distinctive structuralist bent. These developments came to the fore in Cantor's and Dedekind's work. Some of the difficult issues connected with them were seen by Cantor, others were made explicit by Dedekind and by Kronecker (when criticizing Dedekind); and they clearly prompted Hilberfs foundational studies in the late 1890's. Dedekind and Kronecker were both deeply influenced by Dirichlet; their divergent development of algebraic number theory together with their general reflections pinpoint the central issues most clearly. Hilbert's Program was formulated only in the early twenties, but it evolved out of this earlier "problematic".

The program was to mediate between the opposing foundational views represented by Dedekind and Kronecker, and it was to address a methodological problem, to wit, the use of 'abstract' analytic means in proofs of 'concrete' number theoretic results (employed first by Dirichlet). The expressibility of parts of classical mathematics in axiomatic systems P was the basic datum for conceiving of *consistency proofs* for P as a possibly convincing approach. The basic datum, after such P's had been sharpened to formal theories, allowed Hilbert to think of classical mathematics *programmatically* as a formula game and, thus, of the consistency problem as a syntactic one. In this way Hilbert side-stepped the philosophical problems associated with the content of P and turned to restrictive demands on consistency proofs. He required that they be given in *finitist* mathematics, believed to be a philosophically unproblematic part of number theory and to coincide with the mathematics accepted by Kronecker and Brouwer.

To describe the role of consistency proofs in greater detail, let **P** be a formal theory in which the practice of classical mathematics can be represented, and let **F** formulate the principles of finitist mathematics. The formal character of **P** allowed Hilbert to express in the language of **F** the proof predicate Pr for **P** and thus **P**'s consistency; Hilbert expected that an elementary proof of this elementary statement could be found! The consistency of **P** can actually be shown in **F** to be equivalent to the reflection principle

$$(\forall x)(Pr(x,'s') \rightarrow s);$$

where s is a finitist statement, and 's' is the corresponding formula in the language of **P**. A consistency proof in **F** would show, because of this equivalence, that the technical apparatus **P** can serve as a reliable instrument for establishing true finitist statements; after all, it would allow the transformation of any **P**-derivation of 's' into an **F**-proof of s. Finitist consistency proofs would thus resolve the methodological problem mentioned above, guaranteeing "Methodenreinheit" in a systematic manner. *And yet*, describing the program in this way truncates it by leaving out essential and problematic considerations.

Hilbert and Bernays attributed to consistency proofs a further philosophical significance: such proofs were thought to provide the last desideratum for *justifying* the *existential supposition* of infinite structures made by modern axiomatic theories[2]. This issue links the program of the twenties to Hilbert's first foundational

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[2] "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 p.19 of that work.

investigations. At the core of the strategic considerations was the perceived close connection between (truth in) mathematical structures and (provability in) syntactic formalisms; this connection was to be exploited as a crucial means of "reduction". Bernays expressed the central idea repeatedly through a mathematical image (in papers that span close to fifty years). In (1922) he observed after 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, (p. 96)

What is said here for geometry was stated in (1922A) for arithmetic and in (1970) for formal theories in general: "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 obtained in this way, Bernays emphasized then, will usually be different from the structure intended by the theory in essential ways. And yet, the projection has a point, because "... [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." Since Hilbert saw the axiomatic method as applying in identical ways to different domains, these projections are epistemologically uniform. That is explicitly 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.[3]

Hilbert's Program is thus seen to aim for *uniform structural reductions*: arbitrary mathematical structures are projected through their presumably "complete" formalizations into the properly mathematical domain of finitist mathematics. As the equivalence of consistency and satisfiability was assumed, or at least conjectured, the existence of such structures seemed to be secured by solving the finitist consistency problem.

It is often claimed that the difficult philosophical problems inherent in the axiomatic method and the associated structuralist view of mathematics were not addressed in the Hilbert school. That is incorrect; those problems were seen clearly and indeeed motivated the enterprise. But Hilbert and Bernays hoped, perhaps too naively, either to avoid them in a systematic-mathematical development by appropriate interpretations or to solve them for fundamental structures by finitist arguments. In any case, they envisioned an absolute reduction to a basis that was viewed by them, to repeat, as "the prerequisite for any theoretical investigation whatsoever". I assume, it is this clear reductive and philosophically motivated goal (to be reached by purely mathematical means) that made Hilbert's Program attractive; even Gödel admitted in his 1938 lecture at Zilsel's: "If the original Hilbert program could have been carried out, that would have been without any doubt of enormous epistemological value. The following

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

requirements would both have been satisfied: (A) Mathematics would have been reduced to a very small part of itself ... . (B) Everything would really have been reduced to a concrete basis, on which everyone must be able to agree." Note that (B) paraphrases Hilbert's characterization of the finitist basis in his (1926).

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Gödel's Incompleteness Theorems blocked, however, the radical aspiration of Hilbert's Program. If the program was to be pursued in *some* form, the sharp restriction of the "properly mathematical" domain had to be given up; Gödel's second theorem implies, after all, that structural reductions even for arithmetic can be obtained only by strengthening the finitist basis. A suitable modification of the program has been pursued with remarkable success.[4] The crucial tasks of this *general reductive program* are: (1) find a formal theory **P*** for a significant part of classical mathematical practice, (2) formulate an unmistakably constructive theory **F***, and (3) prove in **F*** the *partial reflection principle* for **P***, i.e.

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

for each **P***-derivation d. Pr* is the proof-predicate of **P***, and s is an element of some class of formulas in the language of **F***. The provability of this partial reflection principle implies the consistency of **P*** *relative* to **F***. Clearly, for such a result to be of foundational significance, **F*** must be philosophically distinguished.

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[4] Hilbert and Bernays, Ackermann, von Neumann, Herbrand, Gödel, Gentzen, Schütte, Kreisel, Feferman, Tait, Takeuti, and many others contributed. For references and detailed discussions, in particular on the consistency proofs for impredicative theories, see (Buchholz e.a. 1981), (Sieg 1984), (Feferman 1988), (Pohlers 1989), or (Rathjen 1991).

The first contributions to the reductive program were the proofs given by Gödel, respectively Bernays and Gentzen, who established independently the consistency of classical arithmetic relative to its intuitionistic version; as a matter of fact, this result made the modification of Hilbert's Program at all plausible.

As I do not intend to sketch the development of proof theory, I will only comment on some central results concerning analysis, i.e. concerning theories for the mathematical continuum. Hilbert and Bernays considered analysis as the touchstone for the feasibility of the reductive program and took second order arithmetic as the framework for its formal development and its metamathematical investigation. In contemporary presentations the essential set theoretic principles are the comprehension principle

(3X)(Vy)(yeX <-> S(y))

and forms of the axiom of choice

(Vx)(3Y)S(x,Y) -> (3Z)(Vx)S(x,$(Z)_x$);

here S is an *arbitrary* second order formula.[5] The principles in this general form are impredicative, as the sets X and Z whose existence is postulated are characterized by reference to *all* sets of natural numbers - if S contains set quantifiers. *Subsystems* of second order arithmetic are defined by restricting S to particular classes of formulas; and subsystems that have been proved consistent contain, for example, the impredicative comprehension principle for *n*\- and Aj-formulas.

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[5] The lower case variables range over natural numbers, the upper case variables over sets of natural numbers. In the axiom of choice, ye$(Z)_x$ is understood as <y,x>eZ and <,> is a pairing function.

These subsystems are of genuine mathematical interest, since analysis can be formalized in them by refining the presentation in Supplement IV of *Grundlagen der Mathematik II.* (This presentation goes actually back to lectures of Hilbert's starting with those given in the winter term 1917/18, when Bernays had just started to work with him on foundational matters.) Really surprising refinements have been obtained during the last twenty years[6]: all of analysis can be formalized in conservative extensions of number theory (containing the comprehension principle for arithmetic formulas with set parameters); significant parts of analysis and of algebra can be developed already in conservative extensions of primitive recursive arithmetic, which is arguably the exact formal frame for finitist mathematics.[7] The further mathematical investigations, showing that ever weaker subsystems allow the formalization of at least significant parts of analysis, have been complemented by proof theoretic reductions of ever stronger subsystems of analysis to constructive theories. However, the treatment of full second order arithmetic is still an open issue: even the subsystem with Π^1_2-comprehension presents a formidable obstacle.

Before discussing the character of relative consistency proofs for impredicative theories, I want to recall that Brouwer's mathematical universe was richer than assumed in Göttingen. In his development of analysis Brouwer used infinite proofs and treated them mathematically as well-founded trees. Such trees can be

6 For references to the rich literature see (Feferman 1977 and 1988), (Simpson 1988) and (Sieg 1990).

7 This is the basis for Simpson's version of Hilbert's Program that should better be called "Kronecker's Program"; see (Simpson 1988) and (Sieg 1990).

viewed as inductively generated sets of sequences of natural numbers. For constructive ordinals the generation proceeds in a similar manner according to the rules OeO, aeO ->a'eO, and (Vn)a_ne0 -> a:=supa_ne0. With respect to the infinite proofs Brouwer wrote in his (1927): "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[11] (footnote 8, p.460). He added that this remark contains his "main argument against the claims of Hilbert's metamathematics".

The relative consistency proofs for impredicative theories I alluded to, ironically use infinitary logical calculi; the syntactic objects constituting them, i.e. infinitary formulas and derivations, are treated as well-founded trees 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 i.d. [inductively defined] classes of natural numbers. As the process of inductive generation for constructive ordinals can be expressed by an arithmetic formula A(X,x), the two principles are in this case

(01) (Vx)(A(O,x)-> O(x)) , and

(02) (Vx)(A(F,x) -> F(x)) -> (Vx)(O(x) -> F(x)) .

These principles are correct from an intuitionistic point of view.

There is no doubt that (meta-) mathematically and prima facie also philosophically significant results 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. And this is not trivial, if one bears in mind that strong non-constructive principles seemed to be necessary for analysis. As to the metamathematical results it can be noted: the constructive principles formalized in intuitionistic theories for special i.d. classes allow us to recognize the relative consistency of some 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.

The relative consistency proofs provide material for critical philosophical investigations. After all, they press on us the question, "What is the special evidence of the mathematical principles used in these proofs?" The principles for special i.d. classes are recognized by classical and constructivist mathematicians alike: they are more elementary than the principles used in their set theoretic justification, but they cannot be given a direct intuitive foundation. In section 5 I will formulate some tasks for an analysis that attempts to clarify the *objective underpinnings* for extensions of the finitist standpoint and to explicate, relative to them, the epistemological significance of particular results.

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The task of assessing the epistemological significance of particular proof theoretic results was briefly taken up by Gödel: in his lecture at Zilsel's Seminar of January 29, 1938, he investigated several ways of extending the finitist basis and (the possibility of) proving the consistency of arithmetic and analysis on that basis. The lecture extended the considerations of a talk Gödel had given in Cambridge on December 30, 1933. In both lectures he was sympathetic to a reductive program of the sort I sketched; cp. the discussion in (Sieg and Parsons 1993). However, this line of research was not pursued and the underlying "methodische Einstellung" was not adopted by Gödel.[8] He tried later, most explicitly in his Gibbs Lecture of 1951, to use the incompleteness theorems as a starting point for an argument in favor of Platonism.[9] Central features of Gödel's argument are, first, the fact that formal theories are being investigated and, second, the belief that the concept of formality had been captured adequately through Turing's analysis. The first point is also important for the very formulation of Hilbert's Program, and the second is crucial for the generality of the incompleteness theorems -- used for the program's refutation!

The insistence on the *effective* presentation or the *formal* nature of theories had been motivated by epistemological concerns; and it is quite clear that a restriction on our cognitive, more particularly mathematical, capacities had been intended. For this reason it is surprising that some of the logical pioneers interpreted the incompleteness and undecidability results in a quite dramatic way. Post, for example, emphasized in 1936 that these theorems

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[8] Indeed, not properly appreciated.

[9] To understand this development in Gödel's views is most important, particularly in light of the critical remarks on Platonism made in his 1933 lecture quoted below.

exemplify "a fundamental discovery in the limitations of the mathematizing power of Homo Sapiens"; a few years later he remarked with respect to the same results:

> Like the classical unsolvability proofs, these proofs are of unsolvability by means of given instruments. What is new is that in the present case these instruments, in effect, seem to be the only instruments at man's disposal. (1944, p. 310)

Turing's work provided for Gödel "a precise and unquestionably adequate definition of the general concept of formal system"; consequently, the incompleteness theorems hold for *arbitrary* formal systems (satisfying the usual conditions). Yet in contrast to Post, Gödel did not see them as establishing "any bounds for the powers of human reason, but rather for the potentialities of pure formalism in mathematics" (1965, pp. 72-73).[10]

In his Gibbs Lecture Gödel argued that human reason goes beyond the bounds for formalism in mathematics. To begin with he stated, if mathematics is viewed as a body of propositions that "hold in an absolute sense", then the incompleteness theorems bring to light the fact that mathematics is not exhaustible by a mechanical enumeration of its theorems. Already the first theorem yields for any consistent formal system P, containing a modicum of number theory, a simple arithmetic sentence that is independent of P. But he emphasized that the second theorem makes particularly evident the phenomenon of inexhaustibility.

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[10] As a footnote to this remark Gödel discussed in (1972) a "philosophical error in Turing's work". Gödel claimed that Turing intended to show that "mental procedures cannot go beyond mechanical procedures" and pointed to page 136 (in (Davis 1965)) of Turing's "On Computable Numbers", where a very brief argument is to show that the number of states of mind that need be taken into account for a computation is finite. As mechanical procedures, not mental procedures in general, are analyzed there, I do not see a philosophical error in Turing's work, but rather in Gödel's interpretation.

For it makes it impossible that someone should set up a certain well-defined system of axioms and rules and consistently make the following assertion about it: all of these axioms and rules I perceive (with mathematical certitude) to be correct, and moreover I believe that they contain all of mathematics. (1951, pp. 5-6)

If someone claims this he contradicts himself, because recognizing the correctness of all the axioms and rules means recognizing the consistency of the system. Thus, a mathematical insight has been gained that does not follow from the axioms.

To explain the meaning of this situation Gödel distinguished between "objective" and "subjective" mathematics: objective mathematics is the body of all *true* mathematical propositions, subjective mathematics is that of all *humanly provable* ones. There clearly cannot be complete formal systems for objective mathematics. For subjective mathematics the existence of a finite procedure yielding all its evident axioms cannot be excluded. But if there were such a procedure, then we could not be certain that all of the generated axioms are correct, and -- as far as mathematics is concerned -- the human mind would be equivalent to a Turing machine. Furthermore, there would exist simple arithmetical problems that could not be decided by a mathematical proof intelligible to the human mind. Calling such problems *absolutely undecidable* Gödel thus established: ***either*** *mathematics is inexhaustible in the sense that its evident axioms cannot be generated by a finite procedure* **or** (in case there is a procedure generating the axioms of subjective mathematics) *there are absolutely undecidable arithmetic problems.* (1951, p. 7)

This disjunction is of "great philosophical interest" to Gödel; not surprisingly, because he rejects the second alternative and explicates the first in the following way: "... that is to say, the human mind (even within the realm of pure mathematics) infinitely surpasses the powers of any finite machine". Gödel's elucidation of this remark invokes his Platonism; already in (1933, p. 50) he had claimed that the axioms of set theory, "if interpreted as meaningful statements, necessarily presuppose a kind of Platonism". But at that time he added the relative clause "which cannot satisfy any critical mind and which does not even produce the conviction that they [the axioms of set theory] are consistent".

I would go too far afield, if I presented the reasons why I do not find Gödel's considerations for Platonism convincing. In any event, my criticism does not start with his treatment of set theory, but at the point where he contrasts the objects of finitist and intuitionistic mathematics in his *Dialectica* paper of 1956. (The basic considerations go back to 1938 and 1941.) According to Gödel, finitist mathematical objects are required to be "finite space-time configurations whose nature is irrelevant except for equality and difference"; furthermore, in proofs of propositions concerning them one uses only insights that derive from the combinatorial space-time properties of sign combinations representing them.[11] These remarks, though consonant with Hilbert's very early views, stand in sharp conflict with Bernays' position to which Gödel appealed in his *Dialectica* paper. Bernays stressed already in 1930 the uniform character of the generation of natural numbers, the local structure

[11] (Gödel 1958), in Collected Works II, p. 240. It is informative to compare this statement with the incorrect translation on p. 241 and, most significantly, with the corresponding remark in (Gödel 1972), p. 273. In the latter Gödel expanded "insights that derive from" by "a reflection upon" in this remark.

of the schematic "iteration figure", and the need to *"reflect* on the general features of intuitive objects". Indeed, our *understanding* of natural numbers as being generated in a uniform way allows us to grasp *laws* concerning them. This observation is also correct for more general inductively defined classes, and it points to the first of two critical aspects of mathematical experience I want to describe now.

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If one takes seriously the reformulation of the first alternative in Gödel's disjunction, one should try to see ways in which the human mind goes beyond the limits of mechanical computors. Gödel suggested in (1972) that there may be humanly effective, but non-mechanical mental procedures; yet even the most specific of his proposals, he admitted then, "would require a substantial advance in our understanding of the basic concepts of mathematics". That proposal concerned extensions of the cumulative hierarchy or, rather, of Zermelo Fraenkel set theory by axioms of infinity. The problem of extending what I call *accessible domains* is not special to the case of set theory: there are completely analogous issues, e.g., for the theory of primitive recursive functional and for the theory of constructive ordinals.

Accessible domains comprise elements that are inductively and uniquely generated. They are most familiar from mathematics and logic: the natural numbers, the formulas of first order logic, the constructive ordinals, and the sets in segments of the cumulative hierarchy are generated in this way and form accessible domains.

The generating procedures allow us in all these cases to grasp the build-up of the objects and to recognize mathematical principles for the domains constituted by just them. For it is the case, I suppose, that the definition and proof principles for such domains follow directly from the comprehended build-up.

A broad framework for the inductive generation of mathematical objects is described by Aczel (1977). It is indeed so general that it encompasses all the examples I mentioned, and allows us to compare and explicate the difficulties (in our understanding) of generating procedures. This echoes considerations of Gödel's in his 1933 lecture in Cambridge, when discussing varieties of constructive mathematics as follows:

> ... it is certainly true that there are different notions of constructivity and, accordingly, different layers of intuitionistic or constructive mathematics. As we ascend in the series of these layers, we are drawing nearer to ordinary non-constructive mathematics, and at the same time the methods of proof and construction are becoming less satisfactory and less convincing, (p. 51)

Let us continue, I suggest, the ascent to classical mathematics and investigate, in what way the methods of proof and construction are becoming "less satisfactory and less convincing[11]; let us consider, in particular, (extensions of) Zermelo Fraenkel set theory! It seems that, *if* we understand the generating procedure for a segment of the cumulative hierarchy, then it is the case that the axioms of ZF*[12] together with a suitable axiom of infinity "force themselves upon us as being true" (in GödePs famous phrase). They formulate, after all, the principles underlying the "construction" of the objects in the segment; this reason for accepting the axioms is consonant with

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[12] ZF* denotes ZF set theory without the axiom of infinity

Gödel's analysis in *What is Cantor's continuum problem?*[13] and does not rest on the Platonism advocated in the later supplement of the paper.

By broadening the range of foundational theories for relative consistency proofs from constructive to "quasi-constructive" ones and concentrating on one central feature of objects in the intended domains, namely accessibility, we can understand better what is characteristic of and considered as problematic in classical mathematics, and what is characteristic of and taken for granted as convincing in constructive mathematics. I want to raise issues concerning the second conjunct and start at a very elementary level. A finitist standpoint that is to serve as the basis for Hilbert's Program cannot be founded on just the intuition of concretely given objects, but has to incorporate *reflection* as Bernays explained in (1930)[14]. Thus a first task presents itself.

(I) Analyze Bernays' reflection for the natural numbers (and 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.

For Bernays, the natural numbers are the simplest formal objects that can be (partially) represented by concrete objects. That representation has a special feature: the representing things, numerals, contain 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. This feature has to be given up when we extend the finitist standpoint; symbols are no longer carrying their meaning on their

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[13] The conceptual kernel of the analysis goes back to Zermelo's penetrating 1930.

[14] Cp. the discussion of Bernays' views at the end of section 4.

face, as they cannot exhibit their intended build-up. Numerals for the elements of accessible i.d. classes, for example, are understood as denoting infinite objects, namely the unique construction trees associated effectively with the elements. So we generalize (I) to a second task:

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

For the consistency proofs of impredicative theories the definition of i.d. classes has to be iterated; that means, branchings in the well-founded construction trees are not only taken over natural numbers, but also over already obtained i.d. classes. These trees are of much greater complexity. Thus, modifying (II) we have a third task.

(III) Extend the reflection to iterated accessible i.d. classes, in particular to the higher constructive number classes.[15]

The reflective analyses have to be complemented by reasoned choices of 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 these questions are of only secondary importance for my concerns here. The restriction to intuitionistic logic, for example, is rather insignificant, as the consistency proofs for classical arithmetic relative to intuitionistic arithmetic can be extended to a variety of theories. Indeed, Friedman showed for arithmetic, finite type theories, and Zermelo Fraenkel set theory that the classical theories are n^-conservative over their intuitionistic versions.

[15] As I am presenting only broad strategic considerations, I do not discuss the use of systems of ordinal notations in the work of Gentzen, Schütte, Feferman, e.a.; cp. (Sieg 1990), p. 281.

With Friedman's strikingly simple techniques such results were also established for some subsystems of analysis and for the theories of iterated inductive definitions.[16] In the latter case it is the *further* restriction to accessible i.d. classes that is technically difficult and conceptually significant.

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We have a wealth of accessible domains and seem to understand the pertinent mathematical principles, because we grasp the build-up of the objects constituting these domains. I did not discuss at all the ontological status of mathematical objects, as I agree with the subtle considerations of Bernays in his essay *Mathematische Existenz und Widerspruchsfreiheit* and suggest only one amendation: the objects of "methodical frames" (methodische Rahmen) should constitute accessible domains. In this way methodical frames may be epistemologically differentiated from each other and from "abstract" theories formulated within particular frames. I want to focus on this latter differentiation and contrast now the *quasi-constructive* aspect of mathematical experience (I sketched in the previous section) with its *conceptional* aspect.

In my paper "Relative consistency and accessible domains" (cp. note 1) I pointed out methodological parallels between Dedekind's treatment of natural and real numbers; here I want to emphasize a striking difference. Dedekind's analysis of natural numbers is based on a clear understanding of their accessibility through the successor

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[16] in (Feferman and Sieg 1981), pp. 57-59; the subsystems that were shown to be Π^0_2-conservative over their intuitionistic versions include the theory of arithmetic properties and ramified systems.

operation. Given the build-up of objects in their domains, it is quite obvious that any two simply infinite systems have to be isomorphic, indeed, via a unique mapping. 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 the density condition and the non-existence of endpoints, not any build-up of objects. This observation provides also the reason, why these axioms do not have an *intended model*: the accessibility of objects via operations gives us such models, not the categoricity of a theory. Similar 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. The crucial point is illustrated 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.(1950, p. 229)

The general structures fall under *abstract notions* that are distilled from mathematical practice for the purpose of comprehending complex connections (in the case of the reals, connections to geometry), of making analogies between different theories precise, and thus to obtain a more profound understanding. Notions like group, field, topological space, differentiable manifold are abstract in this sense, and *relative consistency proofs* have here indeed the *task of establishing the consistency of these notions relative to accessible domains.* Bourbaki's enterprise might be seen as being pursued relative to (a segment of) the cumulative hierarchy. The abstract, structural concepts are properly and in full generality investigated in category theory; Groethendieck's introduction of universes and MacLane's distinction between small and large categories can be viewed as attempts to establish the consistency of the general theory relative to extended segments of the cumulative hierarchy.[17] -- These broad considerations pertain not only to notions of classical mathematics, but apply also to notions distinctive for constructive mathematics. A prime example is the (abstract, axiomatically characterized) concept of a choice sequence that was introduced by Brouwer into intuitionistic mathematics in order to capture the essence of the continuum. Kreisel and Troelstra's consistency proof for the theory of choice sequences relative to the theory of O can be viewed as fulfilling exactly the above reductive task.

The conceptional aspect of mathematical experience and its profound function in mathematics has been entirely neglected in the logico-philosophical literature on the foundations of mathematics,

[17] To review in this context the earlier discussion on the foundations of category theory seems very much worthwhile; cp. for example (Feferman 1969).

except in the writings of Bernays. Among the major contributors to the foundational discussion in our century, it was Bernays who steered clear of divisionary formulations and emphasized the complementary character of seemingly conflicting aspects of mathematical experience (and philosophical positions). We have been discussing, implicitly and in his spirit, a redirected Hilbert Program searching for *structural reductions of abstract concepts to accessible domains*. Such structural reductions are most significant for any methodical frame: the traditional contrast between "platonist" and "constructivist" tendencies in mathematics comes to light in refined distinctions concerning the admissibility of operations, of their iteration, and of deductive principles considered as fundamental for a particular frame.[18]

7

The sharpening of axiomatic theories to formal ones was motivated by normative epistemological demands: checking of proofs ought to be done in a radically intersubjective way and ought to involve only operations similar to those used by a human computor when carrying out an arithmetic calculation. Turing analyzed the processes underlying such operations and formulated a notion of computability by means of his machines; that was in 1936. In a paper written about ten years later and entitled *Intelligent Machinery*, he stated what really is *the* problem of cognitive psychology:

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[18]Abstract notions have been important for the internal development of mathematics and for sophisticated applications in the sciences to organize our experience of the world. It seems to me to be absolutely crucial to gain insight into this dual role -- to bring into harmony philosophical reflections on mathematics with those on the sciences.

> If the untrained infant's mind is to become an intelligent one, it must acquire both discipline and initiative. ... But discipline is certainly not enough in itself to produce intelligence. That which is required in addition we call initiative. This statement will have to serve as a definition. Our task is to discover the nature of this residue as it occurs in man, and to try and copy it in machines. (1948, p. 21.)

The task of copying may be difficult, and Gödel would argue that it is impossible for mathematical thinking. But before we can start copying, we have to discover at least partially the nature of the residue, and we are led back to the questions: What are essential aspects of mathematical experience? Are they mechanizable?

I tried to give a very tentative and partial answer to the first question. As far as the second question is concerned, I don't have even a conjecture on how it will be answered. To come closer to an answer, we should investigate aspects of mathematical experience vigorously: by historical case studies, theoretical analyses, psychological experimentation, and by machine simulation. That the latter is still a real issue is counter to Turing's expectations. In 1947 he expressed this view:

> As regards mathematical philosophy, since the machines will be doing more and more mathematics themselves, the centre of gravity of the human interest will be driven further and further into philosophical questions of what can in principle be done etc. (1947, p. 122)

Even now, machines don't do much mathematics themselves — when doing mathematics includes: finding intelligible proofs of given theorems, introducing appropriate defined notions, formulating motivated conjectures, discovering new abstract concepts, and recognizing new axioms for accessible domains. For the first, relatively easy question, calculi that were developed in the Hilbert school provide the necessary logical framework. And with respect

to the other issues Hilbert, I assume, would have been very optimistic; he claimed in 1927:

The formula game that Brouwer so deprecates has, besides its mathematical value, an important general philosophical significance. For this formula game is carried out according to definite rules in which the technique of our thinking is expressed. These rules form a closed system that can be discovered and definitively stated. The fundamental idea of my proof theory is none other than to describe the activity of our understanding, to make a protocol of the rules according to which our thinking actually proceeds.

And he added, "If any totality of observations and phenomena deserves to be made the object of serious and thorough investigation, it is this one ... ." This remark of Hilbert's is undoubtedly correct (and independent from his claims for proof theory).

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