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HILBERT'S PROGRAM REVISITED

ABSTRACT. After sketching the main lines of Hilbert's program, certain well-known and influential interpretations of the program are critically evaluated, and an alternative interpretation is presented. Finally, some recent developments in logic related to Hilbert's program are reviewed.

In its heyday in the 1920s, Hilbert's program was arguably the most sophisticated and progressive research program in the foundations of mathematics. However, after Gödel's celebrated incompleteness results it became an almost universally held opinion that Hilbert's program was dead and buried, and consequently interest in it diminished and the received picture of it became somewhat caricatured and unfair. But more recently, there has been lots of new serious interest in Hilbert's program. Consequently, there now exists some illuminating historical work on Hilbert's thought.[1] Moreover, there are also new systematic interpretations of Hilbert's program, which argue – in various ways – that there is a sound core in the program which was not affected by Gödel's results.[2] My aim in this paper is to critically evaluate these recent influential interpretations in the light of both historical (textual) and systematic logical facts and to hopefully settle some of the controversies. I shall first give a brief and relatively uncontroversial description of Hilbert's program, and then proceed to more detailed and controversial issues of interpretation.

On the one hand, the roots of Hilbert's program go back to the foundational debates in the late 19th century, especially to Kroenecker's attack on Cantorian set theory and the abstract analysis just developed. This debate affected Hilbert permanently. On the other hand, Hilbert's own thought went through various important changes, and it would be an error to simply equate Hilbert's views in, say, 1900, and his mature program, which was formulated only in the early 1920s.

Hilbert spoke about a consistency proof for arithmetic, or analysis, already in his famous 1900 talk on the open problems in mathematics (Hilbert 1900). This may give the wrong impression that Hilbert's program was already there. However, in 1900 Hilbert thought that this consistency

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proof would be carried through by exhibiting a realization, that is, a model. Only in 1904 did Hilbert consider the syntactical notion of consistency (Hilbert 1905). But, most importantly, both in 1900 and in 1904 he held that consistency is sufficient to guarantee the existence of the sets of natural and real numbers and the Cantorian alephs (cf. Sieg 1984, 170; 1988, 340–341), whereas in his mature program Hilbert believed that such infinistic existence claims are devoid of any meaning.[3] In 1917 Hilbert even endorsed Russell's logicism as the correct route in securing the foundations of mathematics (Hilbert 1918). Only in the early 1920s did Hilbert's program as we know it appear (cf. Sieg 1999, 23) – and even after that there were various modifications.

1. AN OUTLINE OF HILBERT'S PROGRAM

1.1. *The Skeptical Challenge*

According to Reid, Hilbert was becoming, in the early 1920s, "increasingly alarmed by the gains that Brouwer's conception of mathematics was making among the younger mathematicians. To him, the program of the Intuitionists represented quite simply a clear and present danger to mathematics". (Reid 1970, 154) Hilbert interpreted intuitionism as requiring that all pure existence proofs, a large part of analysis and Cantor's theory of infinite sets would have to be given up. In particular, this would rebut some of Hilbert's own important contributions to pure mathematics (Reid 1970, 154).

Hilbert was especially disturbed by the fact that Weyl, who was his most distinguished former student, accepted the radical views of Brouwer (see Reid 1970, 148, 155), who aroused in Hilbert the memory of Kroenecker. In Reid's words, "At a meeting in Hamburg in 1922 he came roaring back to the defence of mathematics" (Reid 1970, 155). This was the first public presentation of Hilbert's program.

Bernays has reported that:

> [F]or Hilbert's program [...] experiences out of the early part of his scientific career (in fact, even out of his student days) had considerable significance; namely, his resistance to Kroenecker's tendency to restrict mathematical methods and, particularly, set theory. Under the influence of the discovery of the antinomies in set theory, Hilbert temporarily thought that Kroenecker had probably been right there. But soon he changed his mind. Now it become his goal, one might say, to do battle with Kroenecker with his own weapons of finiteness by means of a modified conception of mathematics. (Reid 1970, 173)

Apparently Hilbert did not later see much difference between the views of Kroenecker and Brouwer: "What Weyl and Brouwer do amounts in principle to follow the erstwhile path of Kroenecker" (Hilbert 1922, 1119). Further, it seems that Hilbert never studied Brouwer's foundational work in detail, but he apparently identified Brouwer's view with that of Weyl (in Weyl 1921) – which he knew much better (cf. Mancosu 1998, 156). But in fact Weyl's view was more restricted than Brouwer's – as was Kroenecker's. Nevertheless, Hilbert attributed to Brouwer many views of Weyl. For example, Hilbert charged Brouwer with prohibiting existential statements. This charge fits Weyl's position, but not Brouwer's. For unlike Weyl, Brouwer did not think that existential statements have no meaning (cf. Van Dalen 1995, 158). Note, moreover, that this view, wrongly attributed to Brouwer, also resembles Hilbert's own finitism, according to which existential statements are likewise – and unlike in Brouwer – devoid of any meaning. More generally, Hilbert and his school simply identified intuitionism with finitism (see also Section 3.3; cf. Mancosu 1998, 169–170).

Although Hilbert often speaks about the question of the reliability of infinistic methods, one should not let this mislead one to think that Hilbert is primarily considering here some strong set-theoretical axioms such as the comprehension or power set axiom, or perhaps higher-order logic. What Hilbert has in mind is rather the very basic laws of the first-order predicate logic, the standard classical laws governing quantifiers. Indeed, in agreement with intuitionists, Hilbert granted that from the finitistic, contentual point of view, the law of the excluded middle "should not be uncritically adopted as logically unproblematic" (Hilbert 1923, 1140). Note, moreover, that he often formulated the law of the excluded middle as: $\neg(\forall x)P(x) \rightarrow (\exists x)\neg P(x)$ (e.g., Hilbert 1928, 466). However, unlike intuitionists, Hilbert aimed to demonstrate that the application of this law is, after all, harmless: "It is necessary to make inferences everywhere as reliable as they are in ordinary elementary number theory, which no one questions and in which contradictions and paradoxes arise only through our carelessness" (Hilbert 1926, 376). That is, Hilbert was convinced and aimed to show that "the application of terturium non datur can never lead to danger" (Hilbert 1923, 1144).

1.2. *Finitistic and Ideal Mathematics*

Hilbert agreed with the long tradition according to which there is no such thing as an actual or completed infinite (see e.g., Hilbert 1926). Hilbert thought that it is – in Kantian terms – only a regulative idea of reason (see e.g., Hilbert 1926, 1931; cf. Detlefsen 1993a, b). But, unlike Kroenecker and Brouwer, he did not therefore want to rebut all the achievements of infinistic mathematics. What Hilbert rather aimed at was to bring together the safety, or reliability, of critical constructive mathematics and the liberty and power of infinistic set theoretical mathematics – to justify the use of the latter by the uncontroversial methods of the former. More exactly, Hilbert's program for a "new grounding of mathematics" was planned to proceed as follows:

First, all of mathematics so far developed should be rigorously formalized. In Hilbert's own words: "All the propositions that constitute mathematics are converted into formulas, so that mathematics proper becomes an inventory of formulas .... The axioms and provable propositions, that is, the formulas resulting from this procedure, are copies of the thoughts constituting customary mathematics as it has developed till now" (Hilbert 1928, 465).

Next, Hilbert intended to isolate what he viewed as an unproblematic and necessary part of mathematics, an elementary part of arithmetic he called "finitistic mathematics", which would certainly be accepted by all parties in the foundational debate, even the most radical skeptics such as Kroenecker, Brouwer and Weyl. Indeed, Hilbert stated explicitly that Kroenecker's view "essentially coincided with our finitist standpoint" (Hilbert 1931, 1151). Further, Hilbert divided mathematical statements into ideal and real statements. The latter are finitistically meaningful, or contentual, but the former are strictly speaking just meaningless strings of symbols that complete and simplify the formalism, and make the application of classical logic possible.

Finally, the consistency of the comprehensive formalized system is to be proved by using only restricted, uncontroversial and contentual finitistic mathematics. Such a finitistic consistency proof would entail that the infinistic mathematics could never prove a meaningful real statement that would be refutable in finitistic mathematics, and hence that infinistic mathematics is reliable.

Hilbert's characterization of both finitistic mathematics and real statements are somewhat unclear, and the exact extension of these notions remains a subject of debate. Nevertheless, it is in any case clear that the real statements include numerical equations and their negations, bounded existential and universal quantifications of these, and at least some universal generalizations. They are thus included in – if not coextensive with – the sentences that logicians nowadays call Π^0_1 sentences. For simplicity, I shall assume below (although this is not essential to my argumentation), in agreement with the great majority, that Hilbert's real sentences coincide with Π^0_1 sentences.[4]

William Tait (1981) has in turn argued that Primitive Recursive Arithmetic (PRA) captures exactly the part of mathematics that Hilbert took to be finitistic; this interpretation has received quite a wide acceptance. I tend to agree with this, but let me point out certain new, logical grounds for it.

First, PRA can be formulated as a "logic-free" equational calculus, and consequently it does not matter whether one adopts classical or constructive logic as one's logical basis for PRA (see e.g., Troelstra and van Dalen 1988, 125). This satisfies nicely Hilbert's need for a neutral, unproblematic background theory.

Second, in replying to Poincaré's charge that his attempt to justify arithmetical induction by a consistency proof must necessarily use induction and is thus circular, Hilbert emphasized that finitistic (meta-)mathematics only uses limited contentual induction (cf. Mancosu 1998, 164–165). This seems to imply that finitistic mathematics must be weaker than PRA with the unrestricted induction scheme. Now what could this contentual, restricted induction be? The standard formulations of PRA have the induction scheme only for the quantifier-free formulas. But, one may ask, perhaps Hilbert would have allowed more. Perhaps – but how much more? It happens to be a logical fact, although a less known one, that, first, Σ^0_1-induction is conservative over PRA (Parsons 1970), and second, that Π^0_1-induction is equivalent to Σ^0_1-induction (Paris and Kirby 1978); therefore, if the induction scheme is restricted to contentual real statements, in Π^0_1, which would certainly be the most natural choice, one does not go beyond PRA.

Hence there are various reasons to assume that Hilbert's finitistic mathematics is well captured by PRA.[5] Nevertheless, my discussion below does not essentially depend on this assumption. In what follows, I shall denote finitistic mathematics – whatever it is (I only assume that it is axiomatizable) – by F, and an ideal formalized theory under consideration by T.

Now there is no question that Gödel's incompleteness theorems presented a serious challenge to Hilbert's program. But the question of whether Gödel's results definitely refuted Hilbert's program is somewhat vague – the answer obviously depends on what exactly one takes to be the truly essential parts of Hilbert's program. I turn next to some different interpretations of this issue.

2. NEW INTERPRETATIONS OF HILBERT'S PROGRAM

2.1. *Hilbert's Program and Gödel's First Theorem*

Earlier it was generally thought that it is primarily Gödel's second incompleteness theorem which challenged Hilbert's program. But more recently a number of leading foundational thinkers, e.g., Kreisel (1976), Smorynski (1977, 1985, 1988), Prawitz (1981) and Simpson (1988), have claimed that even Gödel's first theorem is enough to refute Hilbert's program. Michael Detlefsen, on the other hand, has argued vigorously against this interpretation (Detlefsen 1990).

Essentially, Detlefsen denies that the Hilbertian has to require that an ideal theory is a conservative extension of finitistic mathematics – note that this requirement is obviously impossible by Gödel's first theorem. (One says that a theory T is a conservative extension of a theory F, or that T is conservative over F, for a set of sentences R, if for every sentence φ in R, $T \vdash \varphi \Rightarrow F \vdash \varphi$.) More exactly, he argues that an argument based on Gödel's first theorem must commit the Hilbertian to the assumption that an ideal theory decides every real sentence ("real-completeness"); without this assumption the conservation condition cannot, according to Detlefsen, be derived from the consistency. And, the argument continues, the Hilbertian need not assume this. Detlefsen concludes that the conservation condition is too strong and should be replaced by the weaker condition that an ideal theory is a consistent extension of finitistic mathematics, that is, one only requires that it does not prove any finitistically refutable real sentence. I agree with some of Detlefsen's criticism of Prawitz and Smorynski, but I think I must disagree with his overall conclusions.

To begin with, one could argue that the conservation condition can be derived from the consistency without requiring the real completeness of the ideal theory T (cf. Murawski 1994). The relevant fact is a theorem due to Kreisel (see Smorynski 1977, 858), which says that

THEOREM (Kreisel). $T \vdash \varphi \Rightarrow F + Con(T) \vdash \varphi$ (where φ is in Π^0_1).

If then $F \vdash Cons(T)$, as the Hilbertians arguably assume, this reduces to: $T \vdash \varphi \Rightarrow F \vdash \varphi$. But this is just conservativity – and moreover, the real-completeness of T is nowhere assumed! However, the proof of this theorem depends essentially on the derivability conditions,[6] which makes this theorem a relative of Gödel's *second* theorem in being intensional. Hence it is somewhat problematic to appeal to it in arguing that even Gödel's *first* theorem is sufficient to refute Hilbert's program.[7]

But, more importantly, Hilbert explicitly and repeatedly required that an ideal theory is conservative over finitistic mathematics for real sentences. Thus we read, for example: "[T]he modes of inference employing the infinite must be replaced generally by finite processes that have *precisely the same results* .... That, then, is *the purpose of my theory*. Its aim is to endow mathematical method with the definitive *reliability*" (Hilbert 1926, 370, my italics). Note that Hilbert here both identifies reliability with conservativity and states that a proof of conservativity is the purpose of his theory.

Hilbert also insisted that a consistency proof entails this sort of conservativity: "If one succeeds in carrying out this proof [of *consistency*], then ... this means that if a numerical statement that is finististically interpretable is derived from it, then it is indeed *correct* every time" (Hilbert 1929, 232, my italics). Clearly "correct" here can only mean finitistic provability. The same view is expressed also by Bernays:

> [B]y recognizing the *consistency* of application of these postulates [of arithmetic], it is established at the same time that an intuitive proposition that is interpretable in the finitistic sense, which follows from them, can never contradict an intuitively recognizable fact. In the case of a finitistic proposition, however, the determination of its *irrefutability* is equivalent to determination of its *truth*. (Bernays 1930, 259, my italics)

Finally, in order to show that the conservativity or soundness requirement is not just a later, unessential addition to Hilbert's program, let us note that it was present already in Hilbert's 1921/22 Lectures: "[I]t is exactly *the task* for the foundational investigation to recognize why it is that the application of transfinite inference methods as used in analysis and (axiomatic) set theory leads always to *correct* results" (quoted from Sieg 1999, 29, my italics).

To recapitulate: According to Hilbert, the consistency proof guarantees finitistic correctness of all finistically meaningful theorems provable by infinistic means, not just their non-refutability, as Detlefsen suggests. In various passages, Hilbert even straightforwardly presents the conservativity proof as the main aim of his proof theory.

Moreover, Hilbert viewed the expected consistency proof but an application of the more general conservativity property. That is, Hilbert was convinced that one can always eliminate, from a given proof of a real statement, all the applications of infinistic methods, and thus obtain a purely finitistic proof of the real statement. A canonical contradiction, e.g., 0 = 1, is a false real statement. Therefore a hypothetical infinistic proof of contradiction could be in particular transformed to a finitistic proof of contradiction – but the latter was considered to be obviously impossible (see e.g., Hilbert 1923, 1141–1142; 1928, 477).

Note, by the way, that precisely speaking, the idea here is to show that $Cons(F) \Rightarrow Cons(T)$ (that is, $T \vdash 0 = 1 \Rightarrow F \vdash 0 = 1$), not that $F \vdash Cons(T)$, as the popular view assumes. Accordingly, the relevant form of Gödel's second theorem is not e.g., PA $\nvdash Cons(PA)$, familiar from textbook presentations, and trivially $Cons(PA) \Rightarrow Cons(PA)$, nor PRA $\nvdash Cons(PA)$, etc. The point is rather that, e.g., $Cons(PRA) \nRightarrow Cons(PA)$, $Cons(PRA) \nRightarrow Cons(Z_2)$, etc. However, it is not completely clear to me whether Hilbert and his school clearly distinguished between these two cases. (In any case, if F would prove $Cons(F)$, as the Hilbertians arguably assumed (see below), then the two cases would coincide.)

In summary, Hilbert thought both that a consistency proof guarantees the conservativity of the ideal theory, and that the consistency is to be proved via conservativity, that is, the consistency proof allows one to eliminate ideal elements from a proof of a real sentence, and the consistency should be proved by showing that an ideal proof of inconsistency in particular could always be turned to a finitistic proof of inconsistency. Hence, consistency and conservativity were, for Hilbert, just two sides of the same coin. And in as much as this requirement of conservativity is essential to Hilbert's program, the program is refuted already by Gödel's first incompleteness theorem. Avoiding this conclusion requires at least some modifications in Hilbert's original formulations.

2.2. *Gödel's Second Theorem and the Consistency Program*

More traditionally, it has been generally thought that Gödel's second incompleteness theorem shows the impossibility of carrying through Hilbert's program. But Michael Detlefsen has argued that this is not the case (Detleflsen 1986, cf. 1990).

Detlefsen's considerations are based on certain sophisticated technical issues related to Gödel's second theorem. Namely, unlike in Gödel's first theorem, where it is enough that one has a predicate that extensionally represents provability, one must now require that the particular way one formalizes provability satisfies certain intensional extra conditions (see e.g., Smorynski 1977), most necessarily the so-called second derivability condition

$$F \vdash Prov_T(\ulcorner\varphi\urcorner) \rightarrow Prov_T(Prov_T(\ulcorner\varphi\urcorner))$$

But there are various extensionally correct formalizations of provability which do *not* satisfy this condition. The earliest and most well known is Rosser's provability predicate (Rosser 1936). Detlefsen calls theories with such a notion of provability "consistency-minded theories", and he suggests that the Hilbertian could well switch to such theories, and demonstrate their consistency in accordance to Hilbert's program. These considerations are very interesting and highly relevant for the issue at stake here, but I am not convinced that a Rosser-like consistency-minded approach can really be used to save Hilbert's program from Gödel's second theorem.

First, Hilbert did not aim at a consistency proof at any price: what he wanted was to justify, also in the infinite domains, the use of the simple laws of Aristotelian logic that "man has always used since he began to think" (Hilbert 1926, 379), "all the usual methods of mathematical inference", "the usual modes of inference that we find in analysis" (Hilbert 1923, 1140) without which the construction of analysis is impossible (Hilbert 1928, 471). Indeed, Hilbert wrote that "we just do not want to renounce the use of the simple laws of Aristotelian logic, and no one, though he speak with the tongues of angels, will keep people from" using it (Hilbert 1926, 379). Hilbert stated that "the fundamental idea" of his 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" (Hilbert 1928, 475).

In the light of such statements, I think it would be quite totally against the spirit of this aim of Hilbert to gerrymander the notion of provability in order to obtain a "consistency proof". Hilbert's purpose was to justify just the ordinary laws of logic when applied to the infinite, not to devise some *ad hoc* logic (notion of provability) allowing an apparent consistency proof for the axioms of, say, analysis.

Second, every theory not containing a trivial, explicit contradiction already at the level of axioms can be proved, even in PRA, to be Rosser-consistent. These theories include, for example, certain theories by Frege, Church, Quine and Curry, that is, all the famous proposed foundational theories that have turned out to be *inconsistent*. Thus the resulting consistency predicate is not even extensionally correct. Therefore a consistency proof in terms of Rosserian consistency predicate has absolutely no value in securing the foundations of mathematics. It is very doubtful that such an empty victory would have satisfied Hilbert – and certainly it would not have convinced the Kroeneckerian and Brouwerian skeptics of the reliability of infinistic mathematics.[8]

Further, I think that Hilbert did not aim to prove finitistically just the consistency of some particular theory or a couple of chosen theories. It is plausible to think that he rather thought that finitistic mathematics is able to prove the consistency of any consistent theory. This expectation seems to be present in some statements of Hilbert: "[T]he development of mathematical science as a whole takes place in two ways that constantly alternate: on the one hand we derive new provable formulae from the axioms by formal inference; on the other, we adjoin new axioms and prove their consistency by contentual inference" (Hilbert 1923, 1138; cf. Hilbert 1922, 1132). In a similar vein, Hilbert wrote later: "The consistency proof for the inclusion of a statement must be carried out every time according to the principles just discussed" (Hilbert 1929, 232).

But certainly this idea appears today to be highly problematic, to put it mildly, for consistency is known to be as strongly undecidable as a Π^0_1 property can be. That is, it is Π^0_1-complete property, and every Π^0_1 property can be reduced to it. Consequently, if finitistic mathematic would be able to prove the consistency of every consistent theory, it would be complete for all Π^0_1-statements (and for all real statements) – and under the plausible assumption that finitistic mathematics is effectively axiomatizable, this would contradict Gödel's first theorem. This more abstract, recursion-theoretic way of viewing the matter also emphasizes the basic difficulty in Hilbert's consistency program: Whatever puzzling technical details there are in Gödel's second theorem, it is an unquestionable logical fact that no consistent effectively axiomatizable theory can prove the consistency of every consistent theory – this would also entail a decision procedure for the logical validity and contradict the classical undecidability theorem of Church and Turing.

And I would add, in contradistinction to certain more charitable interpretations, that I think that Hilbert and most of his collaborators really expected there to be a decision method for the first-order logic. For, even if Hilbert did allow, in his 1900 talk on open problems, also a proof of the impossibility of solution as a possible settlement of a mathematical problem, this seems to apply only to a problem formulated in terms of some definite, restricted methods. But there simply was no definition of a general decision method in the 1920s. Moreover, the classical formulation of the Decision Problem by Hilbert and Ackermann, in 1928, strongly suggest that only a positive solution was expected: "The Decision Problem is solved when one knows a procedure which will permit one to decide, for any given logical expression, its validity or satisfiability .... The Decision Problem must be considered the main problem of mathematical logic" (Hilbert and Ackermann 1928, 73).

2.3. *Reflection Principles – Logicians' Interpretations of Hilbert's Program*

There is a widely accepted, rather sophisticated logical explication of Hilbert's program that is essentially due to Kreisel (1958, 1968, 1976) and endorsed by various distinguished logicians, e.g., by Feferman (1988, 1994), Sieg (1984, 1988, 1990a, 1999), Prawitz (1981), Smorynski (1977,1985, 1988), and Simpson (1988), as well as by philosophers such as e.g., Kitcher (1976) and Giaquinto (1983).

This interpretation relates Hilbert's requirement of consistency proof closely to the soundness and conservativity properties. It is based essentially on the so-called reflection principles, which may be considered as a sort of soundness statements ("a provable sentence is true"). More exactly, reflection principles are instances of the scheme:

$$\text{(Ref)} \qquad Prov_T(\ulcorner\varphi\urcorner) \rightarrow \varphi.$$

Most closely related to Hilbert's program is the reflection scheme that is restricted to Π^0_1-sentences, in short, Π^0_1-Ref. Under rather weak assumptions, it is the case that Π^0_1-Ref and *Cons(T)* are equivalent.[9] Moreover, reflection principles allow one to eliminate ideal elements from a proof of real statements. For, assume that $F \vdash Prov_T(\ulcorner\varphi\urcorner) \rightarrow \varphi$. If $T \vdash \varphi$, and $Prov_T(x)$ formalizes (weakly represents) in F provability-in-T, then $F \vdash Prov_T(\ulcorner\varphi\urcorner)$; by Modus Ponens, one obtains φ in F. This approach thus manages to explicate Hilbert's claim that a finitistic consistency proof would also entail soundness and conservativity for all real statements. Note, however, that Hilbert's program, thus viewed, is refuted by Gödel's theorems.

This is certainly a very interesting rational reconstruction of Hilbert's program. Nevertheless, I think that it is somewhat anachronistic to attribute such sophisticated logical ideas to Hilbert. For, if such a line of thought really were behind his consistency program, one would certainly expect Hilbert to explain it in detail. But there is hardly any hint of such reasoning in Hilbert's work. The only exception is Hilbert's relatively late discussion, in his 1927 Hamburg address (Hilbert 1928), where there is indeed an informal sketch of the idea of how the consistency proof would allow one to eliminate the infinistic elements from a proof of a real sentence. This passage seems to be the only basis for the later logical interpretation.

But certainly, if Hilbert's reasons were as ingenious as the later proof-theoretical tradition tends to interpret them, there would be more traces of it in Hilbert's publications. And yet, for example in his paper 'On the Infinite' from 1926, which is the most extensive mature exposition of Hilbert's program, Hilbert simply stated that a proof of consistency amounts to real-soundness and real-conservativity, without a word of explanation (the ideal "objects" here are just ideal statements):

> For there is a condition, a single but absolutely necessary one, to which the use of the method of ideal elements is subject, and that is *the proof of consistency*; for, extension by the addition of ideals is legitimate only if no contradiction is thereby brought about in the old, narrower domain, *that is*, if the relations that *result for the old objects* whenever the ideal objects are eliminated are *valid in the old domain*. (Hilbert 1926, 383, my italics)

It is important to note that the reflection principles etc. occur neither in this paper nor elsewhere in Hilbert's writing (the first occurrence of anything like the reflection principles I have been able to find in the literature is in Gödel's paper on intuitionistic and modal logic (Gödel 1933)). This is a puzzling state of affairs, and presents a problem of interpretation: Why was Hilbert so convinced, and why did he insist repeatedly with no further argument, that a finitistic consistency proof guarantees real-soundness and real-conservativity?

My hypothesis is the following: I think that Hilbert simply assumed that finitistic mathematics is deductively complete with respect to the real sentences (i.e., is "real-complete").[10] This would have made everything smooth: if an ideal theory extending finitistic mathematics would prove some real sentence that finitistic mathematics does not prove, it would be inconsistent: the real-conservativity and real-soundness follow immediately from the consistency. That is, in the presence of real-completeness of finitistic mathematics, the properties of consistency, real-soundness and real-conservativity almost trivially coincide.

Hilbert once remarked that "in my proof theory only the real propositions are directly capable of verification" (Hilbert 1928, 475), but I am not certain whether one can interpret this as expressing a commitment to real-completeness. However, the following statement seems to do that: "In mathematics there is no ignorabimus. On the contrary, we can always answer meaningful questions" (Hilbert 1929, 233). And "No answer" is clearly not an answer. (One should also note that there is, as such, something odd with the idea of a statement which is meaningful but does not have a truth-value.) Bernays, in any case, explicitly assumed real-completeness: "In the case of a *finitistic proposition* however, the determination of its *irrefutability* is equivalent to determination of its *truth*" (Bernays 1930, 259, my italics). One may presume that this also reliably reflects Hilbert's view.

Further, it is a fact that Hilbert believed that both the axioms of elementary arithmetic and those of real analysis are deductively complete (see Hilbert 1929, 1931; cf. Bernays 1930). Although he is nowhere that explicit with respect to finitistic arithmetic, it is not at all implausible to assume that Hilbert believed also in this kind of real-completeness. Moreover, the former alleged completeness of full arithmetic would actually provide a decision method for all real statements, which in turn would naturally entail their decidability in finitistic mathematics. Finally, the assumptions that first-order logic is decidable and that finitistic mathematics can prove the consistency of any consistent theory, arguably alleged by Hilbert, both entail that finitistic mathematics is complete for real sentences. Therefore, we even have several different reasons to assume that Hilbert believed in real-completeness of finitistic mathematics.

Given Gödel's first incompleteness theorem, we know now that no recursively axiomatizable theory – however strong and infinistic – is complete for real sentences. Thus the assumption I have ascribed to Hilbert is false. However, one may argue that this assumption is not as such really essential for Hilbert's program. I tend to think that, in itself, it is not. Consequently, the question whether, and in what sense, Hilbert's program is shown to be impossible by Gödel's results, is decided by other issues, that is, by the prospect of consistency, soundness and conservativity proofs. However, the conclusion on these issues must be, if my argumentation above has been successful, that in its original form and in its full generality, Hilbert's program has been definitely shown, by Gödel's theorems and related results, to be impossible to realize.

3. RECENT LOGICAL RESEARCH RELEVANT TO HILBERT'S PROGRAM

On the other hand, there has been more recently certain very interesting developments in logic which show that there are, after all, certain admittedly more local and restricted and/or modified but still foundationally highly relevant variants of Hilbert's program that can be successfully carried through.

Many of the relevant results are due to Harvey Friedman, although he himself has never interpreted them as contributing to Hilbert's program. This has been done, however, by his collaborator Steven Simpson, who is speaking about a "partial realization of Hilbert's program". Solomon Feferman and Wilfrid Sieg have in turn studied the relationships between various constructive and classical theories; they have called their line of research a "relativized Hilbert's program". But there are also other results less well known but highly relevant to our topic; in what follows I'll try to give my own view of this field.

But first, let me mention in passing a pair of older results that in my mind together exemplify nicely, in the sphere of pure logic, Hilbert's view according to which one can complete and simplify one's mathematics by adding ideal, infinistic objects. Namely, Gödel's standard completeness theorem famously shows that one can have a complete formalization of logical truth (truth in every model). However, it is illuminating to compare this well-known fact to Trakhtenbrot's theorem, according to which it is not possible to give a complete axiomatization of truth in every finite model (Trakhtenbrot 1950). Only by adding "ideal elements" (here: infinite models) can one obtain a completely formalizable notion of logical truth. I think that this pair of old results provide an amusing realization of Hilbert's ideas. However, let us now turn to certain more seriously and deeply relevant achievements in the foundational studies.

3.1. *WKL_0 and "Partial Realization of Hilbert's Program"*

The first and perhaps the best-known case of the logical developments mentioned above concerns a theory standardly called WKL_0. Its name derives from the fact that Friedman originally isolated it as a form of a weak König's lemma (for binary trees) (Friedman 1976). Simpson has later formulated this theory in a more natural form in terms of the Σ_1^0 separation scheme (Simpson 1984).

Friedman and Simpson have shown that a considerable proportion of ordinary mathematics can be proved in this theory. It is strong enough to prove a great number of theorems of classical infinistic mathematics, including some of the best-known nonconstructive theorems (see Simpson 1988). Hence, WKL_0 is mathematically quite strong.

But in 1977 Friedman showed – using certain very sophisticated methods of model theory – that WKL_0 is conservative over PRA with respect to Π_2^0 sentences. Later, in 1985, Sieg gave a purely proof-theoretical reduction (Sieg 1985). According to Simpson (1988), this means that any mathematical theorem that can be proved in WKL_0 is finitistically reducible in the sense of Hilbert's program. Any Π_2^0 consequence of such a theorem is finitistically true. Hence, a large and significant part of mathematical practice is finitistically reducible. Thus we have in hand, as Simpson interprets this, a rather far-reaching partial realization of Hilbert's program (Simpson 1988).[11]

3.2. *Real Closed Fields and Hilbert's Program*

Very recently (this result has not yet been even published) Friedman managed to prove another striking result concerning the theory of real closed fields (RCF), although again he himself has not interpreted it as relevant to Hilbert's program – this was noted by Matthew Frank. Namely, Friedman proved a result that in fact implies, together with certain results by Simpson, the following:

$$PRA \vdash Cons(RCF).$$

The decidability and completeness of RCF was shown by Tarski already in 1948, but the possibility of a strictly finitistic consistency proof has been demonstrated only now. Note next that RCF can be viewed as the natural first-order arithmetic of real numbers – with full classical logic. Therefore this is indeed a remarkable local realization of Hilbert's program. The question of the exact relevance of this very fresh result must, however, still await a detailed examination.

3.3. *Intuitionism and the Modified Hilbert's Program*

Recall that Hilbert's aim was, in Bernays' words, to do battle with Kroenecker with his own weapons, and note then that when one moves from the 1890s to the 1920s, it is Brouwer – not Kroenecker – who is Hilbert's main target: "Of today's literature on the foundations of mathematics, the doctrine that Brouwer advanced and called intuitionism forms a greater part" (Hilbert 1928, 473). Hilbert's strategy is that we can use whatever methods our opponent uses – here Brouwer's methods – to do battle with Brouwer with his own weapons, so to say. Hilbert and his school just wrongly identified Brouwer's view with Kroenecker's much more restricted view (cf. Section 1.1). Thus Von Neumann, for example, explained Hilbert's program as follows: "[I]f we wish to prove the validity of classical mathematics, which is possible in principle only by reducing it to the *a priori* valid finitistic system (i.e., Brouwer's system), then we should investigate, not statements, but methods of proof" (Von Neumann 1931).

Paul Bernays, who was the most important collaborator of Hilbert, has later described the subsequent progress in proof theory as follows:

> It soon became apparent that proof theory could be fruitfully developed without fully keeping to the original program. It was discovered that a proof of consistency for the formal system of number theory, although not a finitist one, is possible by methods of proof admitted by Brouwer's intuitionism . . . as Gödel and Gentzen independently observed, there is a relatively simple method of showing that any contradiction derivable in the formal system of classical number theory would entail a contradiction in Heyting's system [Intuitionistic Arithmetic]. Hence from the consistency of Heyting's formal system the consistency of the classical system follows . . . . (Bernays 1967)

More formally, the result of Gödel and Gentzen from 1933 Bernays is speaking of is that (although *Cons*(PRA) $\not\Rightarrow$ *Cons*(PA)) it holds that:

$$Cons(HA) \Rightarrow Cons(PA), \quad \text{or} \quad PA \vdash \bot \Rightarrow HA \vdash \bot.$$

Note that this is *not*: HA $\vdash$ *Cons*(PA). Bernays continues:

> In this way it appeared that intuitionistic reasoning is not identical with finitist reasoning, contrary to the prevailing views at that time . . . It thus became apparent that the 'finite Standpunkt' is not the only alternative to classical ways of reasoning. An enlarging of the methods of proof theory was therefore suggested. (Bernays 1967)

This way of viewing the situation, initiated by Bernays, has led to a rich line of proof theoretic research pursued, e.g., by Kreisel, Feferman and Sieg – labelled as "relativized Hilbert's program" by Feferman (see e.g., Feferman 1988). However, they have mostly focused on rather strong classical and intuitionistic theories not necessarily needed in ordinary mathematical practice. There is, however, a combination of results rarely isolated and combined explicitly, mainly due to Friedman, that deserves, in my mind, to be called the modified Hilbert's program.[12]

First, Gödel and Gentzen had established, already in 1933, that PA is conservative over HA for all negative arithmetic formulas (which are either atomic or use in their build-up only the logical connectives $\neg$, $\wedge$, $\forall$), and Kreisel (1958b) extended this for Π^0_2 sentences. Finally, Friedman (1978) gave a uniform method, extending the double negation method of Gödel and Gentzen, which enables one to show Π^0_2-conservativity for various classical theories of arithmetic, analysis and set theory, over their intuitionistic counterparts. This is, already as such, a rather remarkable realization of Hilbert's aim to justify the use of classical logic in mathematics. (Note that all "real sentences" in Hilbert's sense are certainly included in Π^0_2.)

Further, Friedman has isolated a particularly important subsystem of second-order arithmetic, which is obtained by restricting the comprehension scheme to arithmetical ("first order") formulas. This theory of "Arithmetical Comprehension Axiom" is abbreviated as ACA_0. Friedman and Simpson have shown that ACA_0 is actually equivalent to certain key mathematical theorems, e.g., The Bolzano-Weierstrass Theorem, König's Lemma, and many others (see e.g., Friedman 1976, Friedman et al. 1983, Simpson 1984, 1985a, b). In fact, it has turned out that all the results of ordinary analysis and algebra can be proved in ACA_0. Thus, it is a sufficient and necessary formal theory for standard classical mathematics: it is the exact amount of set theoretical existence (the arithmetically definable sets) presupposed by ordinary mathematics. But how much is this? Surprisingly little, in fact. Namely, one can show, by a simple model-theoretical argument, that ACA_0 is actually a conservative extension of the ordinary PA – a strictly proof-theoretical reduction is slightly more complicated. Hence its consistency can be reduced to the consistency of simple intuitionistic arithmetic HA; and it is – like PA –conservative over HA for Π^0_2 sentences. And HA is the basic intuitionistic theory certainly accepted by all strands of constructivism. Further, Feferman (see e.g., Feferman 1977) has in turn formulated certain theories of finite types, more suitable for a direct formalization of ordinary mathematics, which are likewise conservative over PA etc.

Hence, even if Hilbert was wrong about finitistic reducibility of all of classical mathematics, one can after all justify ordinary classical analysis intuitionistically along the lines Hilbert thought – and arguably that was (at least a part of) what Hilbert really aimed; he only wrongly interpreted intuitionism to be equivalent to more restrictive finitism.

DISCUSSION. How really relevant is the last mentioned successful modified Hilbert's program? On the one hand, it comes quite close to fulfilling some key aims of Hilbert's program: as was just noted, it really justifies ordinary classical mathematics in the way that Hilbert suggested. On the other hand, the distinction between real statements and ideal statements – so central for Hilbert's program – is relevant perhaps only for Weyl's version of intuitionism (cf. Section 1.1). That is, without the particular way in which Hilbert and Weyl demarcated meaningful and meaningless, not shared by most constructivists, the focus on real-soundness and real-conservativity may appear rather arbitrary – and mere consistency does not guarantee more.

This leads to a more general, slightly pessimistic conclusion: it may be that Hilbert's program was, even if it had been fully successful, from the beginning much less relevant as a reply to the intuitionistic criticism of infinistic mathematics than Hilbert and his school thought. Only by taking Weyl's rather specific variant of intuitionism as a representative of the whole intuitionistic camp could one conclude that a proof of consistency, and real-soundness, should silence the intuitionistic critics.

However, these rather pessimistic afterthoughts are in no way meant to imply that the various results described above have no foundational interest. They certainly have. A more positive conclusion would be that a limited proof of conservativity at least demonstrates that the use of classical logic cannot lead to a result that contradicts a constructively proved fact.

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NOTES

[1] See e.g., Kitcher (1976), Giaquinto (1983), Detlefsen (1993a, b), Sieg (1984, 1988, 1990a, 1999), and Mancosu (1998).

[2] Detlefsen (1986, 1990; cf. Sections 2.1 and 2.2); Simpson (1988), (Sieg 1984, 1988), Feferman (1988; cf. Sections 3.1 and 3.3).

[3] It is difficult to judge, however, to what degree this is a matter of a substantial change of view, and to what degree just a verbal change of terminology.

[4] For a detailed discussion on ideal and real sentences, see Detlefsen (1990).

[5] Later, however, Hilbert and Bernays were prepared to widen the scope of finitistic mathematics considerably, but I think that this should be considered as an *ad hoc* reaction to unwelcome logical discoveries (first, the non-primitive recursivity of Ackermann function, and most importantly, of course, Gödel's results) rather than as evidence for the view that finitistic mathematics was understood to be that comprehensive from the beginning. Indeed, in the early 1920s, it was understood very narrowly indeed (but see note 11).

[6] For the derivability conditions, see e.g., Smorynski (1977).

[7] This was emphasized by Michael Detlefsen during a discussion. I am very grateful to him for pointing out the relevance of this to the issue.

[8] However, I think that Detlefsen's real point is, rather than to really claim that a Rosser-like consistency-minded approach should be used to rescue Hilbert's program, to emphasize the fundamental but rarely noted fact that at the moment our understanding of the whole issue is seriously incomplete (cf. Detlefsen 1998). Here I completely agree. Namely, there is no conceptual analysis which shows that every natural notion of provability necessarily satisfies the derivability conditions. There is only some partial inductive evidence that the natural candidates suggested so far do, and that the few negative cases are clearly unnatural. What is badly needed is a conclusive analysis of the notion of provability in general, an analysis like Turing's for the notion of effective computability, and consequently, something like the Church-Turing thesis for the concept of provability. Still, I am modestly optimistic that we'll someday have one.

[9] See e.g., Smorynski (1977). Note, however, that whereas *Cons*(T) is a Π^0_1 sentence, Ref is a scheme and not a particular sentence (instances of Π^0_1-Ref are Π^0_1 sentences). Note, moreover – and apparently this has not been generally noted – that the sentences expressing naturally the basic properties of being an extension of a theory and being conservative over another theory are not Π^0_1 but only Π^0_2 and thus ideal, not real sentences; more exactly, these properties are expressed by $(\forall x)\left[(\exists y)Prf_F(y,x) \rightarrow (\exists z)Prf_T(z,x)\right]$, and $(\forall x)\left[(\exists y)Prf_T(y,x) \rightarrow (\exists z)Prf_F(z,x)\right]$, respectively.

[10] I thus disagree here with Detlefsen's view in his (1990, 364).

[11] See (Sieg 1990b, 1998) for certain qualifications. Indeed, I would claim that Hilbert certainly was not ready to rebut parts of the classical analysis even if they turned out not to be provable in a conservative extension of PRA. In personal correspondence, agreeing with the way I put the point, Sieg clarified his view further as follows: "Hilbert aimed to secure *all* of classical mathematics; Hilbert did not seek to develop as much mathematics as possible in a conservative extension of PRA". And indeed, Simpson's discussion in his (1988) might lead one to think the contrary.

[12] Simpson (in private correspondence) does not accept this strategy, for he thinks that it would be a good strategy only for someone who accepts the basic ideas of intuitionism, that is, that the strategy implicitly accepts "the preposterous idea" that the law of the

excluded middle is false. I disagree, however. One can stick, for the sake of argument, to intuitionistic logic, and show that actually classical logic proves no real sentence unjustified intuitionistically. Thus this concession to intuitionism is just a strategic, temporary move, an assumption that can later be discharged.

REFERENCES

Page references given are to English translations whenever a translation is available (in case of Hilbert 1922, 1931, which have appeared in English both in Ewalds (ed.) 1996 and Mancosu (ed.) 1998, the page references are to the former).

Bernays, P.: 1930, 'Die Philosophie der Mathematik und die Hilbertische Beweistheories', *Blätter für deutsche Philosophie* **4**, 326–367. English translation in P. Mancosu (ed.) 1998.

Bernays, P.: 1967, 'Hilbert, David', in P. Edwards (ed.), *The Encyclopedia of Philosophy*, Vol. 3, MacMillan, New York, pp. 496–504.

Browder, F. E. (ed.): 1976, *Mathematical Developments Arising from Hilbert's Problems*, Proceedings of the Symposia in Pure Mathematics 28, American Mathematical Society, Providence.

Detlefsen, M.: 1986, *Hilbert's Program. An Essay on Mathematical Instrumentalism*, D. Reidel, Dordrecht.

Detlefsen, M.: 1990, 'On an Alleged Refutation of Hilbert's Program using Gödel's First Incompleteness Theorem', *Journal of Philosophical Logic* **19**, 343–377.

Detlefsen, M.: 1993a, 'Hilbert's Formalism', *Revue Internationale de Philosophie* **47**, 285–304.

Detlefsen, M.: 1993b, 'The Kantian Character of Hilbert's Formalism', in J. Czermak (ed.), *Philosophie der Mathematik. Akten des 15. Internationalen Wittgenstein Symposiums*, Verlag Hölder-Pichler-Tempsky, Vienna, pp. 195–205.

Ewald, W. (ed.): 1996, *From Kant to Hilbert. Readings in the Foundations of Arithmetic*, Oxford University Press, Oxford.

Feferman, S.: 1977, 'Theories of Finite Types Related to Mathematical Practice', in J. Barwise (ed.), *Handbook in Mathematical Logic*, North-Holland, Amsterdam, pp. 913–971.

Feferman, S.: 1988, 'Hilbert's Program Relativized: Proof-Theoretical and Foundational Reductions', *The Journal of Symblic Logic* **53**, 364–383.

Feferman, S. and W. Sieg: 1981, 'Proof Theoretic Equivalences between Classical and Constructive Theories of Analysis', in Buchholz, Feferman, Pohler and Sieg, *Iterated Inductive Definitions and Subsystems of Analysis*, Lecture Notes in Mathematics, Vol. 897, Springer-Verlag, Berlin, pp. 78–142.

Friedman, H.: 1976, 'Systems of Second Order Arithmetic with Restricted Induction I, II' (abstracts), *The Journal of Symbolic Logic* **41**, 557–559.

Friedman, H.: 1978, 'Classically and Intuitionistically Provably Recursive Functions', in Müller and Scott (eds.), *Higher Set Theory*, Lecture Notes in Mathematics, Vol. 669, Springer-Verlag, Berlin, pp. 21–27.

Friedman, H., S. G. Simpson, and R. L. Smith: 1983, 'Countable Algebra and Set Existence Axioms', *Annals of Pure and Applied Logic* **25**, 141–181.

Gödel, K.: 1933, 'Einen Interpretation des intuitionistischen Aussagenkalküls', *Ergebnisse eines mathematischen Kolloquiums*, Heft 4, pp. 39-40.

Giaquinto, M.: 1983, ‘Hilbert’s Philosophy of Mathematics’, *British Journal of the Philosophy of Science* **34**, 119–132.

Hilbert, D.: 1900, ‘Mathematische Probleme’, *Nachrichten von der Königlichen Gesellschaft der Wissenschaften zu Göttingen*, pp. 253–297. English translation in F. E. Browder (ed.) 1976.

Hilbert, D.: 1905, ‘Über die Grundlagen der Logik und der Arithmetik’, *Verhandlungen des Dritten Internationalen Mathematiker-Kongresses in Heidelberg vom 8. bis 13. August 1904*, Teubner, Leipzig, pp. 174–185. English translation in J. van Heijenoort (ed.) 1967.

Hilbert, D.: 1918, ‘Axiomatisches Denken’, *Mathematische Annalen* **78**, 405–415. English translation in W. Ewald (ed.) 1996.

Hilbert, D.: 1922, ‘Neubegründung der Mathematik. Erste Mitteilung’, *Abhandlungen aus dem mathematischen Seminar der Hamburgischen Universität* 1, pp. 157–177. English translation in W. Ewald (ed.) 1996 and in P. Mancosu (ed.) 1998.

Hilbert, D.: 1923, ‘Die logischen Grundlagen der Mathematik’, *Mathematische Annalen* **88**, 151-1165. English translation in W. Ewald (ed.) 1996.

Hilbert, D.: 1926, ‘Über das Unendliche’, *Mathematische Annalen* **95**, 161–190. English translation in J. van Heijenoort (ed.) 1967.

Hilbert, D.: 1928, ‘Die Grundlagen der Mathematik’, *Abhandlungen aus dem Mathematischen Seminar der Hamburgischen Universität* **6**, 65–85. English translation in J. van Heijenoort (ed.) 1967.

Hilbert, D.: 1929, ‘Probleme der Grundlegung der Mathematik’, *Mathematische Annalen* **102**, 1–9. English translation in P. Mancosu (ed.) 1998.

Hilbert, D.: 1931, ‘Die Grundlegung der elementaren Zahlentheorie’, *Mathematische Annalen* **104**, 485–494. English translation in W. Ewald (ed.) 1996 and P. Mancosu (ed.) 1998.

Hilbert, D. and W. Ackermann: 1928, *Grundzüge der theoretischen Logik*, Springer-Verlag, Berlin.

Kitcher, P.: 1976, ‘Hilbert’s Epistemology’, *Philosophy of Science* **43**, 99–115.

Kreisel, G.: 1958a, ‘Hilbert’s Programme’, *Dialectica* **12**, 346–372.

Kreisel, G.: 1958b, ‘Mathematical Significance of Consistency Proofs’, *The Journal of Symbolic Logic* **23**, 155–182.

Kreisel, G.: 1968, ‘A Survey of Proof Theory’, *The Journal of Symbolic Logic* **33**, 321–388.

Kreisel, G.: 1976, ‘What have we Learned from Hilbert’s Second Problem?’, in F. E. Browder (ed.) 1976, pp. 93–130.

Mancosu, P.: 1998, ‘Hilbert and Bernays on Metamathematics’, in P. Mancosu (ed.) 1998, pp. 149–188.

Mancosu, P. (ed.): 1998, *From Brouwer to Hilbert, the Debate on the Foundations of Mathematics in the 1920s*, Oxford University Press, New York.

Murawski, R.: 1994, ‘Hilbert’s Program: Incompleteness Results vs. Partial Realizations’, in J. Wolenski (ed.), *Philosophical Logic in Poland*, Kluwer Academic Pulishers, Dordrecht, pp. 103–127.

Paris, J. and L. Kirby: 1978, ‘Σ_n Collection Schema in Arithmetic’, in A. McIntyre et al. (eds.), *Logic Colloquium ’77*, North-Holland, Amsterdam, pp. 199–209.

Parsons, C.: 1970, ‘On Number Choice Schema and its Relation to Induction’, in Kino et al. (eds.), *Intuitionism and Proof Theory*, North-Holland, Amsterdam, pp. 459–473.

Prawitz, D.: 1981, ‘Philosophical Aspects of Proof Theory’, in G. Flöistad (ed.), *Contemporary Philosophy, a New Survey*, Vol. 1, Martinus Nijhoff, The Hague, pp. 235–277.

Reid, C.: 1970, *Hilbert*, Springer-Verlag, Berlin.

Rosser, J. B.: 1936, 'Extensions of Some Theorems of Gödel and Church', *The Journal of Symbolic Logic* **1**, 87–91.

Sieg, W.: 1984, 'Foundations for Analysis and Proof Theory', *Synthese* **60**, 159–200.

Sieg, W.: 1985, 'Fragments of Arithmetic', *Annals of Pure and Applied Logic* **28**, 33–71.

Sieg, W.: 1988, 'Hilbert's Program Sixty Years Later', *The Journal of Symbolic Logic* **53**, 338–348.

Sieg, W.: 1990a, 'Relative Consistency and Accessible Domains', *Synthese* **84**, 259–297.

Sieg, W.: 1990b, 'Review of Stephen G. Simpson. Friedman's Research on Subsystems of Second Order Arithmetic', *Journal of Symbolic Logic* **55**, 870–874.

Sieg, W.: 1998, 'Proof Theory', in E. Craig (ed.), *Routledge Encyclopedia of Philosophy*, Routledge, London and New York.

Sieg, W.: 1999, 'Hilbert's Programs: 1917–1922', *The Bulletin of Symbolic Logic* **5**, 1–44.

Simpson, S.: 1984, 'Which Set Existence Axioms are Needed to Prove Cauchy/Peano Theorem for Ordinary Differential Equations?', *The Journal of Symbolic Logic* **49**, 783–802.

Simpson, S.: 1985a, 'Reverse Mathematics', in Nerode and Shore (eds.), *Proceedings of the Recursion Theory Summer School*, Proc. Symp. Pure Math., AMS 42, pp. 461–471.

Simpson, S.: 1985b, 'Friedman's Research on Subsystems of Second Order Arithmetic', in Harrington et al. (eds.), *Harvey Friedman's Research on the Foundations of Mathematics*, North-Holland, Amsterdam, pp. 137–159.

Simpson, S.: 1987, 'Subsystems of Z_2 and Reverse Mathematics', Appendix in G. Takeuti, *Proof Theory*, 2nd edn., North-Holland, Amsterdam, pp. 432–446.

Simpson, S.: 1988, 'Partial Realizations of Hilbert's Program', *The Journal of Symbolic Logic* **53**, 349–363.

Smorynski, C.: 1977, 'The Incompleteness Theorems', in J. Barwise (ed.), *Handbook in Mathematical Logic*, North-Holland, Amsterdam, pp. 821–866.

Smorynski, C.: 1985, *Self-Reference and Modal Logic*, Springer-Verlag, Berlin.

Smorynski, C.: 1988, 'Hilbert's Programme', *CWI Quarterly* **1**, 3–59.

Tait, W.: 1981, 'Finitism', *Journal of Philosophy* **78**, 524–546.

Trakhtenbrot, B. A.: 1950, 'Nevozmoznost'algorifma dla prblemy razresimosti na konecnyh klassah' ('Impossibility of an Algorithm for the Decision Problem in Finite Classes'), *Doklady Akademii Nauk SSSR* **70**, 569–572.

Troelstra, A. and D. van Dalen: 1988, *Constructivism in Mathematics: An Introduction*, Vols. I–II, North Holland, Amsterdam.

Van Dalen, D.: 1995, 'Herman Weyl's Intuitionistic Mathematics', *The Bulletin of Symbolic Logic* **1**, 145–169.

Van Heijenoort, J. (ed.): 1967, *From Frege to Gödel: A Source-Book in Mathematical Logic, 1879–1931*, Harvard University Press, Cambridge, MA.

Von Neumann, J.: 1931: 'Die formalistische Grundlegung der Mathematik', *Erkenntnis* **2**, 116–134.

Weyl, H.: 1921, 'Über die neue Grundlagenkrise der Mathematik', *Mathematische Zeitschrift* **10**, 39–79. English translation in P. Mancosu (ed.) 1998.

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