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Is There a "Hilbert Thesis"?

Abstract. In his introductory paper to first-order logic, Jon Barwise writes in the Handbook of Mathematical Logic (1977):

> [T]he informal notion of provable used in mathematics is made precise by the formal notion *provable in first-order logic*. Following a sug[g]estion of Martin Davis, we refer to this view as *Hilbert's Thesis*.

This paper reviews the discussion of (different variations of) Hilbert's Thesis in the literature. In addition to the question whether it is justifiable to use Hilbert's name here, the arguments for this thesis are compared with those for Church's Thesis concerning computability. This leads to the question whether one could provide an analogue for proofs of the concept of partial recursive function.

Keywords: David Hilbert, Formal proofs, Church's Thesis, Diagonalization.

1. Introduction

The relation of informal and formal notion(s) of proof is currently under discussion due to the challenges which modern computer provers present to mathematics. While mathematical proofs, as given in research papers and textbooks, are far from being formal in any sense of formalized proofs, it is generally assumed that they could be formalized *in principle*.

The assumption that formal proofs can faithfully represent *all* mathematical arguments was, in some sense, the very basis of *Hilbert's Programme*, the foundational enterprise with which Hilbert aimed to secure mathematical reasoning from the threat of paradoxes by giving consistency proofs for formal mathematical theories. Barwise coined, on the suggestion of Martin Davis, the term "Hilbert's Thesis" for such an assumption, narrowing it to first-order logic.

The term "Hilbert's Thesis" was taken up by other researchers only occasionally and also for certain variations of this Thesis. In the following section we review carefully the different uses of the term "Hilbert's Thesis" together with different arguments which were put forward to support such theses. In Section 3 we discuss whether it is justifiable to attribute such a Thesis, in one or another form, to David Hilbert. Section 4 compares (the justifications of) Church's Thesis with (those of the different variations of)

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"Hilbert's Thesis". The last section is devoted to a discussion of a particular argument for Church's Thesis stemming from the failure of diagonalization due to the partiality of recursive function. This leads us to the question whether there could be an analogous concept of partial proofs.

2. "Hilbert's Thesis" in the Literature

2.1. Barwise: Handbook of Mathematical Logic, 1977

The term "Hilbert's Thesis" appears for the first time in a paper of Barwise in the Handbook of Mathematical Logic [3, p. 41]:

> Many logicians would contend that there is no logic beyond first-order logic, in the sense that when one is forced to make all one's mathematical (extra-logical) assumptions explicit, these axioms can always be expressed in first-order logic, and that the informal notion of *provable* used in mathematics is made precise by the formal notion *provable in first-order logic*. Following a sug[g]estion of Martin Davis, we refer to this view as *Hilbert's Thesis*.

This Thesis is supported by two claims [3, p. 41]:

> The first part of Hilbert's Thesis, that all of classical mathematics is ultimatively expressible in first-order logic, is supported by empirical evidence. It would indeed be revolutionary were someone able to introduce a new notion which was obviously part of logic. The second part of Hilbert's Thesis would seem to follow from the first part and Gödel's Completeness Theorem.

The arguments for the "first" and "second part" are of quite different nature, namely "empirical evidence" versus a hard mathematical theorem. But Barwise provides also an important caveat concerning the Thesis [3, p. 41]:

> Even those who accept Hilbert's Thesis in theory, however, are a far cry from accepting it in practise. It would be completely impractical and, in fact, counter-productive, to always make all one's extra-logical assumptions explicit.

This impracticality is illustrated by the following example:

> *Example.* The axiom $\forall x \exists n \geq 1 (nx = 0)$ expressing the torsion property for abelian groups is not a first-order axiom [...]. If we were to apply Hilbert's Thesis in this case, we would have to axiomatize not

> only group theory but also the properties of natural numbers needed to carry out the arguments we are after. This would mean that the theory of torsion groups encompasses all of first-order number theory, something clearly not in the spirit of modern algebra.

Finally, Barwise gives additional evidence for "Hilbert's Thesis" by sketching the possibility to encode many-sorted logic in first-order logic.

It is important to note that, in the version given by Barwise, "Hilbert's Thesis" is not just about formalizability of proof, but formalizability *in first-order logic*. We will see below some other uses of the term "Hilbert's Thesis" in the literature,[1] but it is not only due to the fact that Barwise coined it first that one should probably follow first his reading.

2.2. Berk's Ph.D. Thesis, 1982

In 1982, Lon A. Berk wrote under George Boolos's supervision at the MIT a Ph.D. dissertation under the title *Hilbert's Thesis: Some Considerations about Formalizations of Mathematics*[2] [5, Abstract]:

> In this dissertation I discuss Hilbert's thesis, the thesis that all acceptable mathematical arguments can be formalized using no logic stronger than first-order logic.

From his rather comprehensive work we would like to highlight two different aspects. The first one concerns a standard argument for "Hilbert's Thesis", not before mentioned by Barwise:

> I present and criticize an argument for Hilbert's thesis that is often found in the literature. The argument concludes that Hilbert's thesis is true since all mathematics is reducible to set theory and set theory is a first-order theory.

The empirical fact, that essentially every mathematical argument can be coded in ZFC,[3] is, indeed, used as argument for one or another form of "Hilbert's Thesis"; we will come back to this in Sections 3.4 and 5.2.

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[1] In fact, the term shows up in many contexts for totally different theses related to Hilbert, such as, for instance, in invariant theory. It was also used for Hilbert's conviction that consistency implies existence. Here we are dealing, of course, only with different understandings concerning formalizability of informal proof. We should, however, clarify that this does not include the "Hilbert's Thesis" proposed by Parsons [39] which concerns the epistemic status of finitary reasoning.

[2] Available at: http://hdl.handle.net/1721.1/15650.

[3] In [35] one can find a detailed account how such an encoding works.

The second aspect concerns his placing of "Hilbert's Thesis" in a broader context, namely by identifying two more general theses:

Leibniz's Thesis. [5, p. 17]

(i) Every acceptable argument of (informal) mathematics is a proof; and

(ii) Every proof can be formalized as a derivation.

Frege's Thesis. [5, p. 19]

(i) Leibniz's thesis is true.

(ii) There is a formal language and a set of rules of inference that can be used to formalize adequately all proofs.

Only now, he presents "Hilbert's Thesis" as an particular instance of Frege's Thesis: [5, p. 19]

> I shall be especially interested in one version of Frege's thesis called *Hilbert's thesis*. It is, roughly, the view that all arguments of informal mathematics can be formulated adequately using only the first-order predicate calculus.

There are subsequent uses of the term "Hilbert's Thesis" in terms of formalizability of proof without reference to first-order logic, such as, for instance, by Rav (see Section 2.6) or Shapiro.[4] It would probably by more appropriate to follow in these instances Berk and use terms like Leibniz's or Frege's Thesis—if at all.

Attributing such theses to certain personal names is, in fact, rather problematic; in this sense, Berk provides the following caveat, [5, p. 19]:

> Three warnings should be given, perhaps unnecessarily. Leibniz's thesis was not explicitly endorsed by Leibniz, and Frege's thesis was not explicitly endorsed by Frege. Nor was Hilbert's thesis explicitly endorsed by Hilbert.

For "Church's Thesis"—or better for the jumble of "Church's Thesis", "Turing's Thesis", "Post-Turing Thesis", and "Church–Turing Thesis"—Soare argues vigorously in [46, pp. 244 and 246] for replacing the names by the subject matter:

[4] "We might define '*Hilbert's thesis*' to be the statement that a text constitutes a proof if and only if it corresponds to a formal proof (although any of half a dozen other names would have done just like well—including that of Alonzo Church)." [42, p. 158].

> Why all the fuss over names? Why not simply use the term "Church's thesis" invented by Kleene [[29]], and let it refer to the "computability thesis"? This is in fact what is widely (and incorrectly) done.

and later:

> Why not call it simply, "the computability thesis" and not "Church's thesis", or the "Church–Turing thesis?"

In the very same way, it would also be more accurate to call "Hilbert's Thesis", as promoted by Barwise, the "First-Order Formalizability Thesis"; and what Bark called "Leibniz's Thesis" the (simple) "Formalizability Thesis"—and we will tactically do so in the following. But if you like to stick to names, we think it would be advisable to follow at least Bark's disambiguation. As far as Hilbert is concerned, we will discuss his relation to the "First-Order Formalizability Thesis" below in Section 3.

2.3. Kripke, 1996/2006/2013

Saul Kripke briefly addresses a version of "Hilbert's Thesis" in a paper published in 2013 [33, p. 81], but going back to talks given in 1996 and 2006:

> Now I shall state another thesis, which I shall call "Hilbert's thesis", namely, that the steps of any mathematical argument can be given in a language based on first-order logic (with identity).

He uses it to reduce Church's Thesis to this form of "Hilbert's Thesis". For the present paper, a note [33, endnote 21, p. 97f] added by Kripke to "Hilbert's thesis" is of particular interest. It first stresses a certain weakening in comparison with Barwise:

> Martin Davis originated the term "Hilbert's thesis"; see Barwise (1977, 41). Davis's formulation of Hilbert's thesis, as stated by Barwise, is that "the informal notion of *provable* used in mathematics is made precise by the formal notion *provable in first-order logic* (Barwise 1977, 44). The version stated here, however, is weaker. Rather than referring to provability, it is simply that any mathematical *statement* can be formulated in a first-order language. Thus, it is about statability, rather than provability.

And in the continuation, it reflects briefly on the attribution of such a thesis to Hilbert:

> Very possibly the weaker thesis about statability might have originally been intended. Certainly Hilbert and Ackermann's famous textbook (Hilbert and Ackermann 1928 [[25]]) still regards the completeness of conventional predicate logic as an open problem, unaware of the significance of the work already done in that direction. Had Gödel not solved the problem in the affirmative a stronger formalism would have been necessary, or conceivably no complete system would have been possible. It is true, however, that Hilbert's program for interpreting proofs with ε-symbols presupposed a predicate calculus of the usual form. There was of course "heuristic" evidence that such as a system was adequate, given the experience since Frege, Whitehead and Russell and others.
>
> Note also that Hilbert and Ackermann do present the "restricted calculus," as they call it, as a fragment of the second-order calculus, and ultimately of the logic of order ω. However, they seem to identify even the second-order calculus with set theory, and mention the paradoxes. Little depends on these exact historical points.

Surely, "little depends on these exact historical points", except, maybe, the name (see below, Section 3).

Insofar as Kripke's weakened version—"statability rather than provability"—is concerned, statability and provability will differ—for true formulas—only in the case of incompleteness.[5] As far as semantic (in)completeness—for first-order logic!—is concerned, Gödel's Completeness Theorem would actually show that Kripke's version is only *apparently* weaker.[6]

But if we consider syntactic incompleteness, Gödel's First *In*completeness Theorem, gives us, for every *fixed* first-order axiom system, a true first-order sentence, not provable in this system (but, of course, first-order statable). However, "Hilbert's Thesis" (as given by Barwise) is not bound to *one* fixed first-order system. On the contrary, as the mathematical assumptions of a given proof have to be made explicit, one can clearly choose different sets of axioms for different proofs (see Sections 2.5 and 3.4).

[5] Bernays [6, p. 60f] identified this as one of the lessons of Gödel's incompleteness theorems: "die Behauptung der Widerspruchsfreiheit [läßt] sich im finiten Sinne formulieren [...]. Daraus aber ergibt sich noch keineswegs, daß das Problem mit finiten Methoden lösbar ist." ("the assertion of consistency can be formulated in a finitistic sense. But this does not at all mean that the problem is solvable by finitistic methods.")

[6] And Kripke's historical remarks support the idea that *after* the completeness result, the difference may be disregarded; but this applies, of course, only to first-order logic.

Thus, the distinction of statability and provability appears to be marginal and Kripke might have argued for the weaker version only, because he doesn't need a stronger one in the particular argument of his paper.[7] It could be of interest, however, when one fixes just one particular first-order theory, with *Zermelo–Fraenkel set theory* as the most natural candidate. In this case, for instance, the Continuum Hypothesis is not provable, but statable; but even for this case, Kripke's distinction of statability and provability left no further trace in the literature.

2.4. Boolos, Jeffrey. Computability and Logic, 3th Edition, 1989

"Hilbert's Thesis" might have received attention from a broader audience when it was addressed in the textbook on *Computability and Logic* by Boolos and Jeffrey [8][8]. In a chapter called "Proofs and Completeness" the 4th edition [7] contains a paragraph "14.3 Other Proof Procedures and Hilbert's Thesis". There "Hilbert's Thesis" is given as the

> assertion, that if there is a proof in the ordinary sense, then there will be a deduction in our very restrictive format.

The restricted format is, indeed, first-order logic. And the authors then give three arguments for "Hilbert's Thesis":

> Before the completeness theorem was discovered, a good deal of evidence of two kinds had already been obtained for the thesis. On the one hand, logicians produced vast compendia of formalizations of ordinary proofs. On the other hand, various independently proposed systems of formal deducibility, each intended to capture formally the ordinary notion of provability, had been proved equivalent to each other by directly showing how to convert formal deductions in one format into formal deductions in another format; and such equivalence of proposals originally advanced independently of each other, while it does not amount to a rigorous proof that either has succeeded in capturing the ordinary notion of provability, is surely important evidence in favor of both.
>
> The completeness theorem, however, makes possible a much more decisive argument in favor of Hilbert's thesis.

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[7]He writes [33, endnote 21, p. 97]: "For the purposes of the present paper, [the Hilbert Thesis] could be restricted to steps of a computation."

[8]As far as we were able to verify, it was first included in the 3rd edition; references are to the 4th edition published by John P. Burgess.

The first argument (empirical evidence) is as in Barwise. The second is, however, new, but we will reject it later, as we don't see the alleged independence of the formal systems. The third argument is, again, as in Barwise. But it is also used, by contraposition, to dismiss explicitly ordinary proofs in second-order logic, when the authors write:

> And when in later chapters we show that there can be no formal deduction in certain circumstances, it will follow that there can be no ordinary proof, either.

2.5. Beklemishev and Visser, 2005

As Kripke, Beklemishev and Visser discuss a "Hilbert's Thesis" in connection with the Church–Turing Thesis (and an additional Gurevich's Thesis). In course of this discussion they make explicit a distinction which we already identified as relevant for the (non-)applicability of Gödel's First Incompleteness Theorem [4, p. 85f]:

> the non-uniform version of Hilbert's Thesis – stating that every proof can be represented in a suitable axiomatic system – as opposed to a uniform version related to, say, a fixed system of set theory ZFC.

As we will discuss below, a uniform version of a "Formalizability Thesis" can hardly be attributed to Hilbert; but it deserves some attention and could probably be better subsumed under the term "Set-theoretical Formalizability Thesis".[9]

2.6. Rav, 1999

With a paper entitled *Why Do We Prove Theorems?*, Yehuda Rav triggered a discussion on "Hilbert's Thesis" in the more philosophical community. Without referring to first-order logic, he proposes a non-uniform—as we read it—Formalization Thesis:

> it has been suggested to name *Hilbert's Thesis* the hypothesis that every conceptual proof can be converted into a formal derivation in a suitable formal system.

It is his goal to argue that normal mathematical proofs carry important information which is allegedly lost when moving to the formal derivations. For this he uses the picture of a bridge:

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[9] In this way, Shapiro [42, p. 159] treats "*ZF-proofs*" explicitly apart from his earlier mentioned "Hilbert's thesis" (see Footnote 4).

> One immediately observes [...] that [...] Hilbert's Thesis is [...] a one-way bridge: from a formalized version of a given proof, there is no way to restore the original proof with all its semantic elements, contextual relations and technical meanings.

For this bridge he coins the term *Hilbert Bridge*.

We will not enter into the discussion concerning epistemological and ontological differences of informal and formal proofs. But it goes without saying that Hilbert himself was not a naïve formalist; in consequence the term "Hilbert Bridge" is dangerous if it suggests that Hilbert would invite you to cross it blindly—or, even worse, if you would place Hilbert just on the formal side of the bridge. Rav explicitly warns the reader of his paper not to dismiss proof theory which is acknowledged as an important branch of mathematical logic, and the subsequent discussion—initiated by a reply of Jody Azzouni [2]—is just concerned with the conceptional questions regarding informal and formal proofs, but not with Hilbert's own views. The problem is that the somehow careless use of his name in this discussion might place him on a side he doesn't really belong to.

This impression is caused, for instance, by Weir, who suggested the following expanded version of Rav's "Hilbert's Thesis" [49, p. 30]:

> Hilbert's Thesis II: In any cogent mathematical practice there is a systematic process of transformation (not necessarily known to the practitioners) which turns any correct proof into a (suitably related) finite derivation in a formal system S. The system S in question is determined by the informal practice and its transformation process; in particular, the formal rules of S are rules which are implicit in the mathematical practice.

At a first glance, one could think of a non-uniform reading of this Thesis, choosing different formal systems S for different proofs; the following discussion, however, makes clear that the author thinks of just one formal system, and dismisses the Thesis on the bases of Gödel's First Incompleteness Theorem. We will argue below that Hilbert could be associated, at best, with a non-uniform version; thus it is, at least, unfortunate to use his name in this context. And using Gödel's First Incompleteness Theorem to argue against a (necessarily) uniform version of "Hilbert's Thesis" comes to nothing when one considers a non-uniform version.[10]

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[10] This problem appears to be repeated, in particular, in the philosophical discussion. We just like to mention Marfori who writes [34, p. 263]:

3. Hilbert and a "Hilbert Thesis"

Is it accurate to attribute the "First-Order Formalizability Thesis" to David Hilbert? Let us first look at the first-order aspect of the thesis.

3.1. First Order

Both Berk and Kripke made a caveat concerning Hilbert and first-order logic. And it is, indeed, beyond question that Hilbert himself never explicitly identified first-order logic as against higher-order logic as the distinguished formal framework for mathematical arguments. On the contrary—and as clearly stated by Kripke—Hilbert and Ackermann, in their seminal textbook [25], were advocating second-order logic.[11] While Gödel's Completeness Theorem shows that first-order logic is a suitable framework for formalization, it was only a corollary of his First Incompleteness Theorem, that second-order logic does not admit a complete axiomatization.[12] Certainly, this was not known to Hilbert at the time he was working on his foundational programme, and it would be mere speculation to consider what Hilbert would think about the fundamental difference between first- and second-order logic with respect to axiomatizability. But it seems to be conceivable that, if second-order logic were to admit an axiomatization,[13] Hilbert would actually subscribe a "Second-Order Formalizability Thesis". In any case, we are not aware of any textual evidence which would link Hilbert to a "first-order restriction" in a "Formalizability Thesis". At best, one could argue that he

Footnote 10 continued

> the incompleteness theorems undermined the claim that mathematical provability was indeed reducible to provability within a formal system.

This sentence would have profited from replacing "*a* formal system" by "*one* formal system".

[11] The opening section of of Chapter 4 is entitled: *Necessity of an extension of the calculus* ("Notwendigkeit einer Erweiterung des Kalküls") [25, p. 82]; the title was changed for the second edition [26] (see also the following footnote).

[12] Gödel actually mentions this result (at least, according to the surviving manuscript) as early as 1930 when he presented the Completeness Theorem in his talk at the history-making conference in Königsberg, [15, pp. 28/29]. But it doesn't seem to have received any particular attention (maybe because the discussion turned soon to Gödel's Second Incompleteness Theorem). One can find it emphasized and with explicit reference to Gödel, but without proof, in the second edition of Hilbert and Ackermann's textbook *Grundzüge der Theoretischen Logik* [26, p. 104] which was published in 1938. Some historical considerations concerning this result can be found in [28].

[13] We understand "axiomatization" always as "recursive axiomatization".

must have had a complete calculus in mind when he argued for formalization of mathematical arguments. With hindsight, knowing Gödel's results, we know now that this amounts essentially to first-order logic.

An immediate lesson is that it would be, of course, a historical misconstruction to direct arguments against the first-order aspect of the Formalizability Thesis to Hilbert. A more subtle argument could be constructed, however, against formalizability in general, if *second-order logic* could be shown to be necessary for mathematical reasoning. Such a claim is often attributed to Georg Kreisel, who argued forcefully for *informal rigor* [32, p. 138f]: "Informal rigour wants [...] not to leave undecided questions which can be decided by full use of evident properties of these intuitive notions." And he puts forward a specific example: the Continuum Hypothesis might be decidable as a *second-order consequence* from additional intuitive axioms. As second-order logic is not axiomatizable, we may simply overlook it: "most people in the field are so accustomed to working with the restricted [first-order] language that they may simply not succeed in taking other properties seriously" [32, p. 152]. But one may observe that Kreisel is discussing here the choice of the appropriate set-theoretical axioms, but *not* any notion of proof. Only if one would consider arguments for the "evidence of properties of intuitive notions"—that is what Kreisel's is chasing—as parts of mathematical proofs one could get out of Kreisel's informal rigour an argument against a Formalizability Thesis. If, however, the choice of the axioms is separated from the proofs within the resulting axiom system, there is no further point here.[14]

3.2. Formalizability and Finiteness

Insofar as any general Formalizability Thesis is concerned, there exists overwhelming evidence that Hilbert promoted such a Thesis (as much as Leibniz and Frege and many others before and after him). As an example we may cite from his second Hamburg lecture [23, p. 464; our emphasis]:

> With this new way of providing a foundation for mathematics, which we may appropriately call a proof theory, I pursue a significant goal, for I should like to eliminate once and for all the questions regarding the foundations of mathematics, in the form in which they are now posed,

[14] Of course, the very use of "axioms" suggests that one already subscribes to some kind of formalization; but we would like to make the point that axioms are not allowed to be questioned and, thus, don't require a proof—whether they are understood traditionally as evident truths or being taken in a modern sense as arbitrary hypotheses.

> by *turning every mathematical proposition into a formula that can be concretely exhibited and strictly derived*, thus recasting mathematical definitions and inferences in such a way that they are unshakable and yet provide an adequate picture of the whole science.

We have also textual evidence where Hilbert stresses that proofs have to be *finite*, see, for instance, [18, p. 184] and [17, p. 264]. This appears to be rather part of the definition of proof than any form of Thesis; in this way Hilbert commented polemically in his talk *Über das Unendliche* [24, p. 370]:

> some stress the stipulation, as a kind of restrictive condition, that, if mathematics is to be rigorous, only a finite number of inferences is admissible in a proof—as if anyone had ever succeeded in carrying out an infinite number of them!

In a Post-Gödelian perspective, a "Finiteness Thesis" could, however, be taken as another evidence for the "First-Order Formalizability Thesis": we know now that second-order logic would require, to be formalized in a complete way, infinite proofs. Such proofs Hilbert had ruled out.[15]

Interestingly, it was Hilbert himself who suggested, in 1930 [22, p. 491], to consider infinitary derivations which are today rendered by use of an ω-rule.[16] Such a rule is obviously incompatible with the finiteness condition. The ω-rule became later an important tool in proof theory, but as a purely technical instrument, and the resulting theories are intentionally called "semi-formal".

3.3. Formalizability *in Principle*

When we attribute a Formalizability Thesis to Hilbert, we have to stress that he thought of such a formalization *in principle* only. Hilbert's work in proof theory leaves no doubt that concrete formalizations of existing proofs are not his business. The rationale is evidently something else, based on a *modus tollens* argument: if (informal) mathematical reasoning would be subject to hidden contradictions—as the set-theoretical paradoxes around the turn of the 20th century suggested—such contradictions would carry over to the formalized mathematical definitions and inferences. If one could show—as it

[15]In this perspective, Boolos, Burgess, and Jeffrey (see Section 2.4) are in line with Hilbert.

[16]It remains unclear whether Hilbert was already informed of Gödel's incompleteness result by the time of his talk in December 1930 and, if so, if his infinitary rule was an "answer" to it; we consider it as quite possible that it came to Hilbert's mind independently of Gödel's result. For his infinitary rule (and the modern ω-rule) see also [44, p. 160ff].

was the aim of Hilbert's Programme—that formalized Mathematics is free of contradiction, informal mathematical reasoning would be secured.[17]

The aim of Hilbert's proof theory is to be able to investigate mathematical proof(s) by mathematical means; investigations which may consider correctness or consistency or other "meta-properties".[18] Outside of such an investigation, however, the original informal proof does not lose anything of its interest, and it is not supposed to be replaced by a formal proof.

3.4. Non-uniformity

Finally, Hilbert should also only be related to a *non-uniform* Thesis (in the sense of Beklemishev and Visser, see Section 2.5 above). There are no indications that Hilbert thought of one universal formal theory to formalize (encode) all kind of mathematics; he is, in fact, surprisingly silent concerning ZFC. Tapp [48] points out that Ackermann, in his (flawed) proof of the consistency of Arithmetic, which was his Ph.D. thesis under Hilbert's supervision, does not even properly specify the formal system he is concerned with, but allows picking-out certain axioms "as needed" ([48, § 10.2] with reference to [1, p. 5]).

Thus, although uniform versions of a "Hilbert's Thesis", in particular based on ZFC or the like, are of interest (see also below Section 5.2), they can hardly be attributed to Hilbert.

4. Church's Thesis

"Hilbert's Thesis" may be associated with Church's Thesis, the well-known thesis that informal computability is captured by any Turing-complete formal notion of computability as, for instance, λ-calculus, partial recursive functions, or Turing machines. Kleene, who coined the term "Church's Thesis", gave the authoritative discussion of it in his seminal book *Introduction to Metamathematics*, [29, § 62].[19]

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[17] From this perspective, it is obvious that a "Hilbert bridge" as proposed by Rav (see Section 2.6) is without further relevance for mathematical practice, which—also for Hilbert—can, of course, differ substantially from formalized mathematics.

[18] A list of such "meta-properties" was already presented by Hilbert in 1917, [19, p. 412f].

[19] Today the thesis also comes under the name "Church–Turing Thesis", stressing correctly Turing's contribution in the clarification of the notion of computability. One may note that Kleene reserved the term "Turing's Thesis" for Turing's justification of his machine model [29, §70], which is explicitly used as strong support for his Church's Thesis

The fact that a "Hilbert's Thesis" tries to give a formal definition (formalized proof) of an informal notion (mathematical proof) is a rather superficial parallel with Church's Thesis. An association of them would be better supported by analogous justifications. Kleene, in his book, gives four arguments, under the following titles (of course, with substantial arguments spelled out in detail):

(A) Heuristic evidence.

(B) Equivalence of diverse formulations.

(C) Turing's concept of a computing machine.

(D) Symbolic logics and symbolic algorithms.

Let's have a look for similar arguments in the case of a "Hilbert's Thesis".

(A) One can only agree with Barwise, that the heuristic evidence for "Hilbert's Thesis" in its non-uniform first-order version is overwhelming. In contrast to computability, a notion which has barely 100 years of history, there is no example known in the several 1000 years history of mathematics, which would contradict this form of "Hilbert's Thesis". Even recent developments concerning "big" proofs support, in some way, such a Thesis rather than questioning it.[20]

(B) It was argued—see Section 2.4—that also the different formalizations of first-order logic give support for "Hilbert's Thesis" (in the first-order version). In our view, this argument is rather weak. In the case of computability, the different formalization were developed independently and largely unrelated, but turned out to be equivalent only later. In the case of calculi for first-order logic, namely Hilbert-style calculi (based on Frege's and Whitehead–Russell's axiomatizations), Gentzen-style sequent calculi, and natural deduction, the development was not based on independent approaches but rather by explicit reflection on shortcomings of one or the other, notably in the work by Gentzen himself.[21]

Footnote 19 continued
[29, p. 321, item (C)]. Kleene also pointed out that Post [40] arrived at a similar formulation (see also [9]).

[20] We are thinking here of the successful computer-aided verification of Thomas Hales's proof of the Kepler Conjecture. A brief overview of the state of the art in this respect is given in [16].

[21] Thus, we plainly deny the independence alleged by Boolos, Burgess, and Jeffrey (see Section 2.4). Even if they refer to developments before the discovery of the Completeness Theorem, which would exclude Gentzen, it is evident that already the approaches of Frege, Whitehead–Russell, and Hilbert (Ackermann/Bernays) are build on each other. Boole and

(C) As the name "natural deduction" suggests, Gentzen thought of an analysis of how proofs are actually carried out, [12, p. 176].[22] Still, there is a huge difference with the very detailed step-by-step analysis of computation provided by Turing. In addition, Gentzen is only concerned with *logical reasoning*, not mathematical; and the former one is even the part usually suppressed in mathematical arguments.[23] We, thus, don't see that "Hilbert's Thesis" is supported by any analysis similar to Turing's, nor do we expect that it could be done: it is widely acknowledged that the way Mathematicians perform proofs is far away from any formalized notion of proof. But formalized proofs are not even intended to mimic the Mathematician's work while carrying out a proof, but rather to represent the end product of this procedure.

(D) Any reference to symbolic logic would, in the case of "Hilbert's Thesis", be circular. Thus, there is no direct counterpart of (D) for a "Hilbert's Thesis". But, at another occasion, Kleene put forward an interesting, albeit quite heuristic, argument for Church's Thesis which we will discuss in detail in the next section.

Before, we still like to recall the difference of the non-uniform and uniform version of "Hilbert's Thesis" (see Section 2.5). Church's Thesis speaks about *one* concrete model of computation—"one" in the sense that each of λ-definability, partial recursive functions, Turing machines, etc., fixes one well-defined class of functions. It, therefore, corresponds at best to a uniform version of "Hilbert's Thesis".[24]

In its non-uniform form, "Hilbert's Thesis" speaks about first-order logic, which is open to add non-logical axioms, and, thus, allows for a set of highly different theories (like Peano–Arithmetic or Zermelo–Fraenkel Set Theory). This gives it an *open texture*,[25] and, in particular, makes it immune to

Footnote 21 continued
Schröder, which developed the impressive *Algebra of Logic*, however, do not even have a proper theory of quantification and could, therefore, not be used as argument here.

[22]See [41] for the authoritative modern presentation of natural deduction.

[23]We do not claim that Gentzen was not interested in mathematical reasoning (see, for instance, [13, §4. Example of a proof from elementary number theory]). It is rather a result of the study of mathematical proofs that provides the detailed analysis of the (hidden) logical reasoning used in mathematical arguments.

[24]Beklemishev and Visser introduce the distinction of uniform and non-uniform explicitly to obtain a parallel to their "Gurevich's Thesis" which is presented as a non-uniform version of the (uniform) "Church–Turing Thesis" [4, p. 85].

[25]We borrow this term from Shapiro [42] who uses it in a discussion of Church's Thesis with explicit reference to earlier work by Waismann.

counter arguments using Gödel's First Incompleteness Theorem: albeit that there is, for every concrete first-order axiomatic theory, an unprovable true sentence, we may always switch to a stronger—but still first-order—theory, which decides this sentence.

One could try to put in parallel the different first-order axiom systems with the different functions calculated by different Turing machines, such that the different non-logical axioms would correlate to the different states and transition tables of a Turing machine. This parallel is insofar defective, as there exist a *universal Turing machine* which can encode the different machines in just one, while—due to Gödel—such a unified first-order axiom system cannot exist.

5. Diagonalization

In a paper on the origins of recursive function theory [30], Kleene recalls his first reaction to Church's Thesis:

> When Church proposed this thesis, I sat down to disprove it by diagonalizing out of the class of the λ-definable functions. But, quickly realizing that the diagonalisation cannot be done effectively, I became overnight a supporter of the thesis.

Since Cantor diagonalization is one of the most powerful tools to reason about formal notions; it is applied, as a rule, to show the unboundedness of formal notions, by passing any proposed bound by an appropriately constructed diagonal element.

Thus, a formal counterpart of an informal notion proposed in theses like "Hilbert's" and Church's should be, in one or the other way, immune to diagonalization.

5.1. Finite Proofs

As example, let us consider the finiteness condition for proofs (see Section 3.2); it is immune to diagonalization, as it is not to see which normal (informal!) proof principles would effectively construct out of finite proofs an infinite one. One would need something like an ω-rule which, clearly, is not considered as normal mathematical proof principle (but rather an idealization to reason in semi-formal systems).[26]

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[26] The same holds for propositional logic; it is surely immune to diagonalization, in the sense that one cannot diagonalize over all propositional proofs to obtain a new propositional proof of a previously unproven propositional formula. From this example, it follows

5.2. ZFC

If we consider, for a moment, a uniform version of "Hilbert's Thesis", one sees immediately that ZFC—or any of its extensions, considered in higher set theory—is subject to diagonalization: just propose the existence of a set (large cardinal), closed under all set-theoretic formation principles used so far.[27] It is, however, an ongoing discussion[28] whether extensions of ZFC are of "mathematical relevance" and whether extensions, which transcend the initial set-theoretic realm, are indeed needed to formalize mathematical arguments. Thus, a defender of a uniform "Hilbert's Thesis" would have to deny the mathematical relevance of higher set theory (at least from a certain point on).

5.3. Partial Functions

But let us go back to Kleene's attempt to diagonalize over the λ-definable functions. It fails, due to the *partiality* of the functions; i.e., due to the possibility that a function may not return a value for all its inputs.[29] In our view, the introduction of *partial functions* is a necessary condition to make Church's Thesis plausible, as any inductive definition of a set of total functions, is subject to diagonalizability.[30] By its time, it was seen as essential that Ackermann's function was a total function (and one may observe that it can be constructed by diagonalization over the definition of the primitive-recursive functions). There existed a strong bias towards total functions in Mathematics, and Kleene [31, p. 57] reports even about a puzzled Gödel asking him in 1939: "What is a partial recursive function?" But these partial recursive functions provide exactly the firm ground for Church's Thesis.

Footnote 26 continued
that immunity to diagonalization alone is not a sufficient condition to capture the informal notion of mathematical proof; propositional logic is clearly much too weak for it.

[27]This could be considered as an echo of *Cantor's Absolute*. Even without references to the construction of large cardinals, one could invoke here Gödel's First Incompleteness Theorem.

[28]See, for instance, [11].

[29]Tactically, one also needs some form of *strictness*, which forbids a function to return a defined value for an "undefined argument", at least when the argument is, indeed, used in the function. In general, this form of strictness could only be overturned, if one were able to solve the halting problem.

[30]It is, of course, possible to define the set of (total) recursive functions not purely inductively, invoking additional conditions; see, for instance, [43, p. 109] (it uses the additional condition that a function used for the unrestricted μ-operator has the zero one is searching for). Such a definition, however, looses the conceptional clarity of a purely inductive definition.

5.4. Partial Proofs?

Similarly it would be of interest for a "Hilbert's Thesis" whether it could be supported by a similar argument; i.e., by one which would block diagonalization, but now over (formal) proofs.

The fact leaps out that Gödel's First Incompleteness Theorem is based on diagonalization over his provability predicate;[31] it even provoked the ironical remark of Reinhold Baer in a letter to Zermelo [10, p. 213]: "Hurrah, logicians have also discovered diagonalization!" But this concerns *provability in a fixed system*, rather than (first-order) proofs by themselves.

We like to draw attention to another fact: there is an obvious analogy between[32]:

1. The proof of the Diagonalization Lemma;
2. The proof of Kleene's Second Recursion Theorem;
3. Curry's Paradoxical Operator Y
   (in λ-notation definable as: $\lambda y.(\lambda x.y\,(x\,x))\,(\lambda x.y\,(x\,x))$).

Arguably, Y can be held responsible for the emergence of partiality in the λ-calculus; technically, this is related to the possibility of performing *self-application* in the type-free λ-calculus.[33] For typed λ-calculus, the Curry–Howard isomorphism provides us with a clear relation between algorithms and proofs. The question is now whether one could extend the Curry–Howard isomorphism to the untyped world such that, at least, Y would correspond to some kind of proof object (in a yet-to-be-defined extended sense).[34] Such proofs objects might show the same immunity to diagonalization as λ-terms, and could help to answer the question:

What is a partial proof?

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[31] According to Gödel, it was Carnap who extracted the useful *diagonalization lemma* out of his original proof, [14, Fn. 23, p. 63].

[32] The relation of 1. and 2. is discussed, for instance, in [45]. The analogy of 2. and 3. is made explicit in [38, p. 155].

[33] One may note that self-application occurs twice in Y, by $x\,x$ and by $(\lambda x.y\,(x\,x))\,(\lambda x.y\,(x\,x))$.

[34] It is, of course, known that Y is typeable in the polymorphic lambda calculus or by recursive types; a deeper analysis of such types with respect to our question is still a desideratum. The work of Naibo, Petrolo and Seiller [36,37], which proposed a framework of *Untyped Proof Theory* might be a possible starting point for further clarification. A quite different account of partial proofs could probably be build on the notion of *pre-proof* which Jäger [27] introduced as a syntactic counterpart to the pre-models of Streett and Emerson [47] in order to unfold fixed points in the modal μ-calculus.

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