Gödel's First Incompleteness Theorem and Mathematical Instrumentalism

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(Note: This is a draft, and it's intended to be read. So no bibliography or footnotes, yet. Comments more than welcome!)

1 Introduction

Among those writing on the philosophy of mathematics, it is widely believed that Gödel's incompleteness theorems have shown that Hilbert's program cannot be carried out. Almost equally often, claims to this effect are qualified by pointing out that this belief is not universal, and that Detlefsen has argued that Hilbert's program survives the challenge posed by Gödel's theorems. It seems, however, that Detlefsen's challenge to those who think that Hilbert's program is dead has not been seriously taken up (with some exceptions, e.g., Ignjatovic), either positively or negatively. It seems that no-one has been persuaded by Detlefsen's arguments to a degree large enough so that they'd join the discussion on Detlefsen's side by trying to actually give a consistency proof of ideal mathematics. On the other hand, no decisive refutations of Detlefsen's "Hilbertian" instrumentalism are available.

I think part of the reason for this situation vis-a-vis Detlefsen's admirable work is that the differences between Hilbert's program and Detlefsen's Hilbertian program have not been appreciated enough. My (admittedly, completely subjective) feeling is that it is still thought that Gödel's theorems destroy Hilbert's program, so if Detlefsen tries to defend that program, something must be fishy. But the program Detlefsen is defending, despite its title, is not Hilbert's. There are many fundamental differences between what Hilbert thought needed to be done in order to secure classical mathematics, and what Detlefsen has argued is needed to successfully defend an instrumentalist philosophy of mathematics. However, the differences are hard to state completely and precisely.

What I want to do in this talk is to make a first step toward isolating some of those differences. No doubt there is a significant prior plausibility attached to the view that Gödel's theorems have an important, perhaps fatal impact upon instrumentalist philosophies of the kind Hilbert and Detlefsen (and Field!) have been proposing. So my strategy will be to see what must be done to escape the "Gödelian challenge." In contrast to Detlefsen, I think that already the first incompleteness theorem (G1) poses a significant challenge which instrumentalism must meet. Both in the 1986 book Hilbert's Program and in his 1990 paper "On an alleged refutation of Hilbert's program using Gödel's first incompleteness theorem," Detlefsen has argued that arguments against Hilbert's program put forward by Kreisel, Prawitz, and Smorynski fail. I will discuss an argument which is similar to these, but which, I hope, avoids some of the difficulties that Detlefsen has stressed. I do not know if the argument is what Kreisel, Prawitz, or Smorynski had in mind; Detlefsen tells me that he thinks it is at least different from Smorynski's. So I will refer to it as the "New G1 Argument," but lay no claim to priority over Kreisel or Prawitz.

(Judging by the volume of Kreisel's writing and correspondence, I wouldn't be surprised if every logical thought I ever had was thought earlier by Kreisel.) In particular, the New Argument does not rely on Gödel's arithmetization of provability: It, for instance, does not mention formalized consistency statements, and thus avoids the difficult questions of whether, say, $Con(\mathrm{PA})$ really formalizes the claim that PA is consistent. Those of you familiar with Detlefsen's work will recall that he devotes an entire chapter in the 1986 book as well as in the recent article I mentioned to attack this view. I don't want to get involved in this here; the payoff of focussing on G1, I hope, is that by avoiding such involvement some of the differences between Hilbert's Program and Detlefsen's Program will emerge more clearly.

My strategy will be to first remind you of the main problem for which Hilbert and Detlefsen alike require a solution: A proof of the real-soundness of ideal mathematics. I will then discuss the argument based on G1 I mentioned; I will argue that it does, in fact, destroy Hilbert's Program. Finally, I will assess the implications the argument has for Detlefsen's Program.

2 Instrumentalism and the New G1 Argument

So first, let me briefly remind you of the structure of the instrumentalist projects of Hilbert and Detlefsen. According to Hilbert, mathematics is to be secured by giving a complete formalization of it. For definiteness, let us call this formalization I (for "ideal mathematics"). I is a formal theory in the usual sense: Its sentences are finite sequences of symbols, its axioms and rules of inference are recursive. A proof in I is a finite, combinatorial object, and there is an (easy) procedure for determining if a given sequence of symbols constitutes a proof in I. We might think of I, for instance, as Zermelo-Fraenkel set theory, or the system of Principia. Opposite formalized ideal mathematics we have finitary meta-mathematics. This is not, at least not initially, a formalized theory. It is a body of mathematics of a special, finitary character; it incorporates all those concept-formations and methods of inference which can be justified on finitary grounds. For us, it won't be too important to characterize what "finitary grounds" are; suffice it to say that finitary mathematics is supposed to avoid infinite objects, avoid quantification over infinite totalities, and avoid methods of inference which in some way or other "presuppose" commitments to the existence of infinite totalities or even the meaningfulness of quantification over them. Depending on who you ask, finitary mathematics must guarantee intuitive knowledge of its theorems (Parsons), or it encompasses that part of mathematics which is implicit in the concept of number and hence is a—or the—minimal part of non-trivial mathematics (Tait). The main aim of Hilbert's program is to give a consistency proof, carried out in finitary meta-mathematics, of the theory I, that is, a finitary proof that I is not contradictory, that for no sentence ϕ it proves both ϕ and $\neg\phi$.

It is commonly assumed that behind this quest for consistency actually lies a more fundamental and more important issue: That of conservativity of "ideal" over "real mathematics." Ideal mathematics here may be taken as the theorems of I. However, the question of what counts as "real mathematics" is a little trickier. "Real mathematics" is supposed to be that part of ideal mathematics which corresponds to finitary meta-mathematics, and the idea is that if ϕ is a real sentence, then if $I \vdash \phi$ then ϕ can also be proved by finitary means. But because ideal mathematics is a formal theory and finitary meta-mathematics is not, a precise delineation of what sentences should count as real, and an account of what a finitary, non-formal proof of a formal statement amounts to are necessary. Hilbert's view was that there is a formal counterpart of finitary mathematics that "lives inside" I. Some sentences of I count as real sentences, and some proofs of I count as real proofs. On Tait's widely accepted construal, finitary mathematics is exhausted by what can be proved in primitive recursive arithmetic PRA; so we might think of I as being an extension of PRA, and the real sentences as the primitive recursive ones (i.e., formulas in the language of PRA without unbounded quantifiers, but perhaps with free variables; more or less equivalently, Π_1 sentences). But the details are not too important: Let's just say that R is the set of all real formulas which have proofs that count as formalizations of real proofs.

One detail about real sentences is important, though: Not all real sentences are alike. Detlefsen introduced the distinction between problematic and unproblematic real sentences: Unproblematic real sentences are those real sentences which are closed under the classical logical operators, e.g., those the negation of which is also real. We might think of the unproblematically real sentences as those not having free variables, e.g., sentences of PRA without quantifiers (or free variables), or Δ_0 sentences (only bounded quantifiers). Those real sentences that do express general propositions, on the other hand, make up the problematic real sentences.

We're now getting close to the New Argument, so let us state some premises. The first just makes explicit what I just said.

(1) Every unproblematic real sentence ϕ provable in R is also provable in I.

The reason (1) must be accepted by Hilbert is that I is supposed to be a formalization of <u>all</u> of mathematics, and since finitary meta-mathematics is <u>a part</u> of mathematics, indeed, a very restricted, minimal part of mathematics, everything R can prove must also be provable by ideal mathematics. Hilbert says that ideal mathematics "rounds out" real mathematics. And Detlefsen points out in Chapter 1 of his book that this is in fact an important premise of the instrumentalist program: It accounts for the perspicacity of ideal mathematics. He writes, "all truths of real mathematics are theorems of ideal mathematics" and: "[I]n constructing ideal mathematics, we start with real mathematics as a base and embed it in the ideal instrument. Hence, the fact that the ideal instrument generates a significant body of real truths is hardly to be wondered at. The perspicacity of an instrument is mysterious only if it is apparently constructed without reference to the subject-matter with respect to which it serves as a guide to truth. In keeping with the replicationist stratagem, the Hilbertian instrumentalist seeks to avoid this element of mystery by explicitly basing his ideal mathematics on real mathematics."

(2a) Every Π_1 sentence $\forall x\phi(x)$ is real.

(2b) Every Δ_0 sentence $\phi(n)$ (no unbounded quantifiers) is real.

This assumption just makes precise the idea that real sentences encompass at least everything that I can say about numbers using a single universal quantifier. One might object here by pointing out that the unbounded universal quantifier in $\forall x\phi(x)$ disqualifies it from being finitarily meaningful; however, the "syntactically" real formula $\phi(x)$ is interderivable with $\forall x\phi(x)$. So the admission of a single universal quantifier makes no difference as far as provability is concerned. And indeed, at least statements of this kind of generality must be admitted as real statements, since the statement of consistency is of such a kind.

(3) There is a real, finitary consistency proof of I.

This is the explicit claim of Hilbert's program. Detlefsen sees as the basic aim of instrumentalism not a consistency proof, but rather a proof of real soundness: Whatever real sentence is provable by ideal methods must be provable by real methods. In formalized terms, I is conservative over R as regards real sentences. A proof of this of course yields a consistency proof of I, since, say $0 = 1$, is a real sentence. So if I were inconsistent, $0 = 1$ would be provable by real methods, which is absurd.

(4a) R is (contained in) a recursively axiomatizable sub-theory of I.

(4b) R, or the recursively axiomatized subtheory of I in which it is contained, is much weaker than I.

This the most contentious assumption; I will return to it below. However, it has significant initial plausibility: For instance, it is a consequence of the widely accepted identification of finitary reasoning with PRA.

We are ready to look at the argument. The first intermediate conclusion is the converse to the implication noted before in the discussion of premise (3): That consistency of I implies its real soundness. This, as I mentioned, is not an explicit aim of Hilbert's program; which first and foremost is concerned with consistency. It is, however, an explicit goal of Detlefsen's Program.

(5) If $\forall x \phi(x)$ is real and $I \vdash \forall x \phi(x)$, then $R \vdash \forall x \phi(x)$.

By premise (2a), Π_1 sentences like $\forall x \phi(x)$ are real. Suppose $\phi(n)$ is a given numerical instance of $\forall x \phi(x)$. By (2b), $\phi(n)$ is also real, and indeed, unproblematically so: $\phi(n)$ contains no unbounded quantifiers, it can be verified or falsified by direct finitary computation. So suppose $\phi(n)$ is false. Then $\neg\phi(n)$ is true, and hence, because this can also be verified by finitary computation, it has a real proof, that is: $R \vdash \neg\phi(n)$. By (1), also $I \vdash \neg\phi(n)$. But I, we assumed, proves $\forall x \phi(x)$, and hence also $\phi(n)$ (by universal instantiation). So I is inconsistent. This is impossible by (3), which says that there is a finitary consistency proof of I.

Even though this proof uses reductio ad absurdum, this application is at least intuitionistically unproblematic, since the formula in question, $\phi(n)$, is a quantifier- and variable-free sentence mentioning only decidable predicates. In fact, this argument is Hilbert's own! ("Foundations of mathematics" (1927), van Heijenoort p. 474).

Since we have a finitary proof of "for all numbers n, $\phi(n)$," R—which formalizes finitary reasoning—should prove the formalized counterpart of the conclusion of the argument; so: $R \vdash \forall x \phi(x)$.

(6) There is a Π_1 sentence g which is provable in I but not in R.

(6) follows from (4a), (4b) and G1. Think of g as the Gödel sentence of R. And (6) obviosuly contradicts (5).

So that's the argument. Let's see now what implications it has for instrumentalism, and how one might possibly escape them.

3 G1 and Hilbert's Program

First let me focus on Hilbert's Program. As I mentioned before, premise (1) is accepted by Hilbert. Again the reasons are: First, real mathematics is a kind of mathematics, so every real sentence provable by real means is mathematically provable. And since ideal mathematics is a formalization of <u>all</u> mathematics, it must then also be provable by ideal mathematics. This is true a fortiori for unproblematically real sentences. (1) is also used in Hilbert's argument for (5), which is supposed to be a finitary proof. In any case, it seems that ideal mathematics should prove at least all the true numerical equalities and inequalities (which, perhaps, may contain symbols for arbitrary finitary functions, although as far as the argument is concerned, $+$ and $\times$ are all that's needed).

Premise (2) is likewise unproblematic. Simple general statements must be allowed as part of real mathematics, for otherwise, no real proofs of anything interesting are possible—indeed, one couldn't even state the consistency of I as a real sentence.

Premise (3) is the main aim of Hilbert's program. It's a (indeed, I argue, the only) candidate among the premises which should be rejected.

Premises (4a) and (4b) are the critical assumptions that allow the application of G1 in the proof of conclusion (6) to go through. So they deserve more attention. But let me first point out that, since the proof of (5) is Hilbert's own and relies only on premises (1)–(3), which Hilbert is committed to, he is also committed to (5). And the inference from (4) to (6) relies only on G1, a mathematical result of impeccable pedigree, often checked (indeed, even verified by computer). The argument Gödel used in proving G1 is entirely constructive; so not only is there a sentence g with the stated properties (provable in I, not provable in R), but the finitist must agree that there is.

So let us turn to (4a): It states that real meta-mathematics is a recursively axiomatizable sub-theory of I. (4a) is needed to apply G1 in the proof of (6) to conclude that there is a real Π_1 sentence not provable in R. (4b), again, states that R is much weaker than I, and is needed to conclude that the unprovable-in-R sentence g is provable in I.

To be sure, neither (4a) nor (4b) are conclusively established, or even universally accepted. But both have strong reasons in their favor: (4a), the claim that there is a recursive sub-theory of I which proves all the real theorems, has in its favor the reason given above for (1): If it can be proved by real mathematics, it must be provable by ideal mathematics also. So, at the very least, I

itself is a recursively axiomatized sub-theory of I containing R which proves all real sentences. But is there a recursively axiomatizable proper sub-theory of I doing the job? A definitive answer cannot be given without a clear characterization of the extent of real reasoning as Hilbert conceived of it. Some convincing characterizations are available, especially Tait's characterization of finitism as being exhausted by the theorems of PRA. Others, e.g., Parson's and Kreisel's proposals likewise provide strict limits on real reasoning: Parson concludes that finitism is exhausted by $I\Delta_0$ induction; Kreisel gives provability in first-order PA as the limit of finitism. Hilbert's (and Ackermann's and Bernays's and Gentzen's) examples of finitary procedures, either presented explicitly as such examples, or used implicitly in putatively finitary consistency proofs, are all formalizable in systems close to PA. This all counts in similar measure in favor of (4b), the claim that R is essentially weaker than I. If, e.g., we take I to be all of set theory, there is a strong intuitive presumption that I must be a lot stronger than real mathematics. And for all the serious candidates for precise characterizations of real mathematics (Tait's, Parson's, Kreisel's), it is a mathematical fact that they satisfy (4a) and (4b).

Owing to the vagueness of the notion of "finitary proof," the thesis expressed by (4)—"thesis" used here in the same sense that "Church's Thesis" is a thesis—perhaps can never be supported by the same kind of overwhelming support the latter has. Nevertheless, it seems so plausible as to at least shift the burden of proof: Anyone wishing to claim that there is no weak recursively axiomatizable theory that formalizes all real proofs, is, I think, under obligation to exhibit the finitary procedures or finitary modes of inference which cannot be formalized in I. No-one working on Hilbert's Program has done that successfully, certainly not Hilbert himself, and most of them have implicitly or explicitly endorsed thesis (4).

So where does this leave Hilbert's program? (1) and (2) are integral parts of Hilbert's conception; it is very difficult to see how Hilbert's Program can escape (4), (5) follows by Hilbert's own proof, and (6) is as solid a mathematical fact as any. So, the only premise that can go is (3): There is, after all, no consistency proof for ideal mathematics of the kind Hilbert had in mind.

4 G1 and Detlefsen's "Hilbertian" instrumentalism

Now, on to Detlefsen's Program. Detlefsen's conception of mathematical instrumentalism differs in some very important respects from Hilbert's. First of all, his philosophical framework is much more developed. From Detlefsen's writings we get a lot we don't get from Hilbert: philosophical arguments why a consistency proof, or a proof of real soundness is important, and how it answers various challenges. But he himself, as well as his critics, have taken his defense of instrumentalism against Gödel's <u>second</u> incompleteness theorem as central. What I hope my talk shows is that the challenge from the first incompleteness theorem is a real challenge; and I think attempts to answer it will bring into sharper focus some aspects of Detlefsen's Program, which, first of all, are crucial for a defense against the challenge from G1, and in the end pose genuine problems for the viability of the instrumentalist enterprise.

So where does the important difference between Hilbert and Detlefsen lie? As I see it, it lies in the, admittedly, initially plausible restriction, that the instrumentalist need not ground all of ideal mathematics, but only those parts that are instrumentally valuable. Specifically, a proof of real soundness—that is, a proof that if $I \vdash \phi$ for a real statement ϕ, then $R \vdash \phi$—must only cover those ϕ which have proofs in I which are more efficient than any real proof thereof.

Digression: Unfortunately, Detlefsen has not yet explained precisely what he means by "more efficient." Initially, it seemed that it was perhaps simply length of proofs that was at issue. Ignjatovic has done some interesting and important work on this, generally negative. I'm not sure, but it was perhaps in response to that work that Detlefsen has since emphasized that it is not speed-up as regards length of proofs that makes ideal math more efficient than real math, but speed-up, if it can be so called, as regards conceptual resources, or what he calls "discovermental" speed-up. (End Digression)

The reason for why only those parts of ideal math must be grounded by a real-soundness proof that are of instrumental value is that those that aren't, namely, those statements where ideal mathematics offers no speed-up over real mathematics, are simply in no need of being grounded: We just take their real proofs as our grounds for their truth; considering less efficient instrumental proof would be useless. Furthermore, if, so Detlefsen, we take the aim of the instrumentalist project seriously, we must realize that the instrument we are considering is an instrument for us. If it is an instrument for us humans, then nothing in it that goes beyond human capability of comprehension could conceivably be of any utility. And so there is no requirement on the instrumentalist to ground those parts of ideal math that go beyond what is of instrumental value to us, in the sense of being of any practical value whatsoever. As I said, this view has some initial plausibility. Let's see how it can help against the New Argument from G1.

Detlefsen introduced the term "Hilbertian residue" for that part of ideal mathematics that is in need of grounding via a proof of real-soundness. The humanly useful ideal proofs of *I* are those of *I* less:

- "all ideal proofs of real formulae that are too long or complex to be of any human epistemic utility

- all ideal proofs of real formulae that have an equally short and simple real proof, and finally,
- all real proofs of real formulae." (HP, p. 89)

The Hilbertian residue I_H of I is obtained by closing the remainder under the logic of I. It is only I_H that the Detlefsonian instrumentalist needs a consistency proof for. So, replace I in the New Argument from G1 everywhere by I_H.

Premise (1) of the New New Argument is the first to go: Not all, indeed, perhaps no unproblematically real sentence provable in R need be provable in I_H. For the unproblematically real sentences are those that just express basic relations between numbers, not involving generality either explicitly in the form of a universal quantifier or implicitly as a free variable. And it is not implausible to assume that for many theorems of this sort, direct, finitary calculation is the best possible (shortest) method of proof. If so, they are excluded from I_H.

Premise (4a) is likewise in trouble. Since we expressly excluded those parts of R from I_H which have no more efficient proofs in I than they do in R itself, I_H is presumably not a sub-theory of I_H—since I_H lacks a whole lot of real theorems (those whose most efficient real proofs are not less efficient than any of their ideal proofs). And premise (4b) is in no better shape: we've severely cut down I to I_H, and perhaps, indeed, the strength of I_H as regards provability of Π_1 sentences with no more efficient real proofs does not go beyond what we can prove in R in the first place.

So it looks like Detlefsen's Program escapes the New G1 Argument on two counts: The intermediate conclusion (5) cannot be derived, because the necessary premise (1) is false. And conclusion (6)—the contradiction to (5) based on G1—cannot be derived because it isn't clear that either (4a) or (4b) must hold.

Leaving the latter aside for the moment, let's look at (5) again. (5) states what Detlefsen in the 1986 book calls real soundness: Every real statement provable by I must also be provable by R. So perhaps we don't need (1) for (5) after all: Detlefsen already accepts (5) independently, or so it seems. Now, fortunately for him, he does not seem to hold the view anymore that (5) is what must be established in the service of the instrumentalist project. In the 1990 paper, Detlefsen calls the condition expressed in (5), "real-conservation" and gives the condition of real-soundness thus: "For any real sentence ϕ, if $I \vdash \phi$ then $R \nvdash \neg\phi$" (p. 363). He also argues that real conservation—that is, our (5)—is not something the instrumentalist is committed to proving. This continues to puzzle me, for several reasons: (i) The statement of the new soundness condition is vacuously satisfied for Π_1 sentences (which are real sentences), since the negation of a Π_1 is not real. Hence, $R \nvdash \neg\phi$ for any Π_1 sentence ϕ simply because $\neg\phi$ is not the kind of thing that R talks about. (ii) The old definition of real-soundness seems to be still what the instrumentalist project requires; in any case, the new definition does not yield the kind of philosophical payoff that Detlefsen wanted out of it in the 1986 book. There, for instance, he said that real soundness shows that "real mathematic is contentually exhaustive of ideal mathematics (i.e., only truths of real mathematics are contentual theorems of ideal mathematics)" and that "since the standard of truth that hold sway in real mathematics can be used to verify every real consequent of ideal mathematics, every such consequent must be regarded as true when judged by those standards." (pp. 30–31). At least, one needs to verify that what Detlefsen wanted to accomplish in 1986 using real conservation can also be accomplished using the new real soundness condition. (iii) Lastly, later on in the same paper, Detlefsen still claims that the Hilbertian needs "a real metamathematical proof showing of ϕ that if it is provable in I_H, then it is also provable by real means at the object level" and "the Hilbertian must be able to show that every sentence provable in *R* is also provable in *I*" (p. 369). Perhaps I'm missing something very subtle, but this sounds to me like Detlefsen does want real conservation after all.

So if real-conservation is required for Detlefsen's Project, then there is still the worry about (4); and if only (the new version) of real-soundness is required, then there is a worry about (1). For if (1) were true, real conservation would still <u>follow</u> from real soundness as in Hilbert's argument for (5).

Both (1) and (4), as noted above, are deflected by Detlefsen's insistence that I_H, the theory that the instrumentalist must prove real soundness for, contains only those parts of ideal mathematics which are both humanly usable and which provide an appreciable gain over real mathematics. The New G1 Argument shows, I think, that this is in fact a crucial limitation on what the "Hilbertian residue" of the ideal theory can look like. But what it can look like, and with it, the scope of Detlefsen's Project, is a decisive factor for its viability as a philosophy of mathematics.

Detlefsen excludes all real proofs, and all ideal proofs of real theorems from I_H where a more efficient real proof is available. This is what avoids (1). But this is a really unsatisfactory picture of instrumentalism as an philosophy of <u>actual</u> mathematics. After all, instrumentalism is supposed to give an account of why the procedures of ideal mathematics are reliable, i.e., yield true real results. If instrumentalism only yields an account of why instrumentally useful proofs (i.e., proofs more efficient than any real proof of the same result) are reliable, then this account is of no use for actual mathematics at all. For a mathematician would like to have assurance that <u>whatever</u> she proves is a reliably true, not that whatever she proves is reliably true provided that there is no more efficient way to prove it by real methods. Let me give an example: Suppose I find a proof of Goldbach's Conjecture in ZF. That's a real sentence. So I'd like the instrumentalist to provide assurance that once I've done that, I am entitled to believe that Goldbach's Conjecture is true. But if I only have a proof of real-soundness of the Hilbertian residue of ZF, I don't have this assurance. More is required: I have to also make sure that there is no more efficient real proof of Goldbach's Conjecture. The difficulties of doing this are obvious: First of all, we have no account of what "more efficient" amounts to. And even if we did, the number of candidates for more efficient real proofs and the resources required to check all of them are astronomical. Others, e.g., Steiner in his review of Detlefsen's book, have suggested similar criticisms of the ideal theories Detlefsen has suggested as possible candidates for the kind of theory for which real soundness can in fact be proved, e.g., consistency minded theories. But I think the difficulty lies deeper: Restricting the scope of instrumentalism to only the instrumentally useful ideal proofs produces a very unattractive instrumentalist epistemology unsuited to mathematical practice.

Let me conclude with a brief comment on the status of (4) in Detlefsen's Program. Detlefsen hasn't given us much by way of a characterization of finitary, real reasoning. His strategy of avoiding the Gödelian Challenge has been, not to strengthen finitary reasoning, but to weaken the body of ideal mathematics the reliability of which must be established. So my guess is that the kind of real mathematics he has in mind is restricted enough so as to be recursively axiomatizable in a weak subtheory of I. He has suggested to me that it's not central for his project that R be part of I, although I have doubts how he can escape this requirement given what he says about "fashioning the instrument after the contentual subject matter with respect to which it serves as a guide." So until there's an argument for why we shouldn't take R to be included in a recursively axiomatizable proper sub-theory of I, I'm going to adhere to the received view that this is essential to the instrumentalist project. This was premise (4a) of the argument; and it yields that there is a real sentence undecidable in R—the Gödel sentence for R (or its recursively axiomatized super-theory). Premise (4b) is one that is still up for grabs; it is required for the conclusion that I does prove the Gödel sentence for R. Now here's something that I think counts in favor of (4b), if not provides a conclusive argument: Consider g, the Gödel sentence for R. The proof of G1 is a perfectly fine mathematical proof, not too hard, not too long, quite elegant. Take the recursively axiomatized (super-theory of) R, and carry out the proof of G1 for that axiom system. Now with a little technical machinery (formalized truth definitions, etc.) I should be able to formalize this proof in I, including the part of the proof that says that g is true. So that gives me a proof in I of g. It seems to me that this proof must be part of I_H: It is something that humans actually prove, so it isn't excluded from I_H in virtue of being "too long or complex to be of any human epistemic utility." It's not a real proof, since it uses lots of high-level math. Say we formalized it in set theory, and our formalization uses the natural way of talking about sequences of formulas as certain sets—which aren't real objects. Our ideal proof doesn't have a more efficient real proof, since g doesn't have any real proofs. So we have a theorem of I_H not provable in R.

5 Conclusion

So where do we stand? I think that the New G1 Argument is as a convincing argument against the viability of Hilbert's Program. As far as Detlefsen's Program is concerned, there are some open questions, and we have trained spotlights on some aspects which haven't so far been highlighted. One such aspect is the relation of the instrumentalist project to mathematical practice. I think Detlefsen still owes us an explanation of how a proof of real soundness for only the instrumentally useful part of ideal mathematics stands any chance of being a satisfactory account of mathematics as practiced. And I mean this of course in a an idealized sense: I'm not looking for an instrumentalist account of mathematical psychology. But mathematicians, say, when they work in analytic number theory, don't care if the theorems they prove have more efficient real proofs. So what justifies the reliability of their proofs, given that no-one has verified, and perhaps no-one can verify whether their proofs fall into the Hilbertian residue of whatever ideal theory they are using? An answer to this question will have to include an answer to the question: What is this "efficiency" exactly that qualifies sentences for inclusion in the Hilbertian residue? It would be exciting to have answers, but I must admit that I am skeptical that these answers can be given.

His incompleteness theorems destroyed the search for a mathematical theory of everything. Nearly a century later, weâ€™re still coming to grips with the.Â The first step in this process is to map any possible mathematical statement, or series of statements, to a unique number called a GÃ¶del number. The slightly modified version of GÃ¶delâ€™s scheme presented by Ernest Nagel and James Newman in their 1958 book, GÃ¶delâ€™s Proof , begins with 12 elementary symbols that serve as the vocabulary for expressing a set of basic axioms. For example, the statement that something exists can be expressed by the symbol âˆƒ, while addition is expressed by +. The first incompleteness theorem states that in any consistent formal system \(F\) within which a certain amount of arithmetic can be carried out, there are statements of the language of \(F\) which can neither be proved nor disproved in \(F\). According to the second incompleteness theorem, such a formal system cannot prove that the system itself is consistent (assuming it is indeed consistent). These results have had a great impact on the philosophy of mathematics and logic.Â (See also the entry on Kurt GÃ¶del for a discussion of the incompleteness theorems that contextualizes them within a broader discussion of his mathematical and philosophical work.) 1. Introduction. 1.1 Outline. 1.2 Some Formalized Theories. 1.3 The Relevance of the Church-Turing Thesis.