Saving Hilbert's Program?

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1 Introduction

It has been widely accepted that the hope of constructing a secure foundation of mathematics in the sense of Hilbert's Program has been destroyed by Gödel's incompleteness theorems in 1931. The fact that a system strong enough to express interesting mathematics is impossible to prove its own consistency, shattered Hilbert's hope in providing consistency proofs of finitist number theory, which he has reduced all the mathematics into, with method specified in the finitist way.

However, it is worth noting that the clash between Gödel's result and Hilbert's program can not be a purely technical one – while the former is a well-defined technical result, the latter is a vague program that has been revised by Hilbert himself along the years.

In the current paper we follow a line led by Detlefsen's works, in particular the book Detlefsen[1986]. Detlefsen's goal is two-fold. On the one hand, he argues against existing approaches and try to open up the possibility that the gap between Gödel's theorem and the failure of Hilbert's program is significant; and on the other hand, he proposes directions that people could follow so that Hilbert's program can be carried on.

We will first examine the gap in the received view that Gödel's second incompleteness theorem shattered the possibility of carrying out Hilbert's Program. Several attempts have been made to fill in the gap, most noteably Mostowski's argument that the proof conditions established the only interesting proof predicate, and hence the incompleteness result is unavoidable. Detlefsen targets rightly at the defects of Mostowski's argument, but there are possibilities of avoid amending Mostowski's proposal to avoid Detlefsen's attack. We then go on to examine Detlefsen's problematic positive proposals for reconstructing Hilbert's program. His key idea is to have a different notion of proofs based on a radical reading of Hilbert's instrumentalism. Detlefsen takes Rosser's provability definition and shows that there is possibly an interesting proof predicate whose consistency can be trivially proved. We will then see that precisely because of such triviality, the proof predicate in Rosser's sense technically contradicts Hilbert's very idea of the use of mathematics. Since Detlefsen provided nothing more than the Rosser proof predicate as a candidate for a nonstandard predicate, it is puzzling where Detlefsen can make positive proposals in a meaningful sense. We further show that it is very unlikely that any reasonable proposals can be made out of the strict instrumentalist reading of Hilbert, since consequences of such reading contradicts with Hilbert's original ideas.

2 Gap in the Standard Argument

The standard argument for that Gödel's second theorem has defeated the hope of carrying out Hilbert's program is easy to state, without introducing all the complications introduced in Detlefsen.

First, given Gödel's numbering, there exists a formula $\mathrm{Con}(T)$ that expresses the metamathematical property of T that $T$is consistent. Then Gödel's second theorem shows that, if T is consistent and interesting enough to express elemenetrary number theory, then $\mathrm{Con}(T)$ can not be established as a theorem of T.

The very problem that the technical nature of Gödel's theorem does not fully block the possibility of carrying on Hilbert's program has been noticed once the theorem came out, which was in the writing of Gödel himself:

> ... to note expressly that Theorem XI (and the corresponding results for M and A) do not contradict Hilbert's formalistic viewpoint. For this viewpoint presupposes only the existence of a consistency proof in which nothing but finitary means of proof is used, and it is conceivable that there exist finitary proofs that cannot be expressed in the formalism of P. (Gödel[1931], p615)

That is, Gödel held the view that a consistency proof of PA could be obtained by using methods that are not formalizable in PA, but are still finitary. Hence the way that Detlefsen's battle against "the received view" is to some extent targeting at a straw-man in the first place – the received view is not formed instantly after Gödel's theorem came out, but in fact out of a long time of attempts of finding amendments to the original program, during which no really interesting and useful positive results have been made. Well-known methods that have been considered as candidates for making such amendments include Gentzen's defence of the use of transfinite induction up to ϵ_0 in his consistency proof for PA. Gödel, Takeuti and Kreisel also presented another extension of the finitist standpoint.

Yet some of Detlefsen's efforts may be justified in that he targets at several attempts that have been made for strengthening Gödel's theorem to rule out the possibility of finding amendments, in particular in the work of Mostowski:

> If we assume that every combinatorial proof can be formalized within arithmetic, then Gödel's second theorem shows that Hilbert's theorem of proving in a purely combinatorial way the consistency of arithmetic is not realizable. This assumpition is open for discussion, however, as we shall see later when we discuss Gentzen's theorem. Another objection which can be raised against such interpretation of Gödel's second undecidability theorem is this: There are many formulae F strongly representing Z (T's proof relation) in T; Gödel's theorem is valid only for some such formulae. It is not immediately obvious why the theorem proved for just this formula should have a philsophical importance while a similar theorem obtained by a different choice of a formula strongly representing the same set Z is simply false. (Mostowski[1966], p23)

What Mostowski then went on to propose is a way of strengthening G2 such that the worry that he described above can be eliminated. First, for strengthening G2, it is worthwhile to take a look at what properties of the proof predicate are in fact required for the proof of incompleteness theorem to go through. As is well-known, the proof predicate used in Gödel's theorem satisfies the following conditions. For any formula A in the theory T, the proof predicate $\mathrm{Prov}_T()$ should satisfy:

$$\text{(LPC) If} \vdash_T A, \text{then} \vdash_T \mathrm{Prov}_T([A]). \tag{1}$$

$$\text{(LPS) If} \vdash_T \mathrm{Prov}_T([A]), \text{then} \vdash_T A. \tag{2}$$

$$\text{(Diagonalization) There exists } G \text{ of } T \text{ such that} \vdash_T G \leftrightarrow \neg\,\mathrm{Prov}_T([G]). \tag{3}$$

$$(F - \mathrm{LPC}) \vdash_T \mathrm{Prov}_T([A]) \rightarrow \mathrm{Prov}_T([\mathrm{Prov}_T([A])]). \tag{4}$$

The Gödel's Incompleteness theorems can be obtained from these conditions easily and we do not need to recap the proof. Now the natural idea of strengthening Gödel's theorem is to consider the properties of the proof predicate as listed here, and argue that they express the necessary properties of any natural notion of proofs.

Mostowski's defence for the Gödelian attack rests on establishing a measure on the "representability" of T-theoretic formulations of metamathematical notions, so that different ways of representing the same notion in T can be compared, and the desirable conclusion should be that the Gödelian representation of the metamathematical notions is the best possible representation with respect to such measure. Along this line, Mostowski proposed that the best T-theoretic representation of a metamathematical notion M should be provided by a formula of T that can represent the intuitive truth regarding M as theorem of T as many as possible.

> The general problem ... can be described as follows: there is given, on the one hand, a set X of integers (of pairs, triples, etc.) and, on the other hand, a formal language. We are looking for the best possible definition of X in T, i.e., for a definition which makes, of all the intuitive true formulae involving X, as many as possible provable in T. (Mostowski [1966], p25)

Mostowski employed the notion of "strongly represent" to clarify this idea. A property S of n-tuple of numbers is defined to be *strongly represent*ed by a T-formula $F(x_{1,}\ldots, x_n)$ if for every n-tuple of numbers $\langle k_1, \ldots, k_n\rangle$,

$$\text{If} \langle k_1, \ldots, k_n\rangle \in S, \text{then} \vdash_T F(k_1, \ldots, k_n);$$
$$\text{If} \langle k_1, \ldots, k_n\rangle \not\in S, \text{then} \vdash_T \neg F(k_1, \ldots, k_n).$$

Mostowski's argument is the following. Consider the proof predicate $\mathrm{Prov}(x)$, in light of Gödel's theorem, there is no way of directly making $\mathrm{Prov}(x)$ strongly represent the set S of numerals that are be decoded to formulas that have proofs. However, if we recall the definition of the proof predicate,

$$\mathrm{Prov}(x) \equiv_{\mathrm{df}} \exists y\, \mathrm{Der}(y, x)$$

where $\mathrm{Der}(y, x)$ is the derivability predicate expressing that there is a derivation, coded as y, from axioms of PA to x. Then we can immediately see that $\mathrm{Der}(y, x)$ strongly represents the notion of derivability, because:

$$\text{If } y \text{ is a derivation from axioms to } x, \text{then} \vdash_T \mathrm{Der}(y, x);$$
$$\text{If } y \text{ is not } a \text{ derivation from axioms to } x, \text{then} \vdash_T \neg\, \mathrm{Der}(y, x).$$

In this way, it is plausible to say the the proof predicate in Gödel's definition enforces the extensional adequacy of our representation of the notion. Hence, Mostowski has argued that the proof predicate in Gödel's definition is the best we can get and therefore Gödel's second incompleteness theorem rules out the possibility of establishing consistency proofs with any "better" notions of proofs.

However, a problem with Mostowski's proposal is that only the truths that can be arithmetized inside T have been considered, while there can be more "general" truth of the derivability relation that can not be represented in the language of T at all. As Detlefsen put it:

> But, Mostowski notes, not all truths concerning the proof-of-relation are particular truths (i.e., truths either of the form "A is a proof-of-T" of B or of the form "A is not

> a proof-in-T of B"). There are also general truths to be considered. And it is not the case that all formulae strongly representing the relation "X is a proof-in-T of Y" do an equally good job of registering the general truths regarding this notion as theorems of T. Hence, when judged according to the basic view of adequate representation suggested by Mostowski, the condition of strong representation turns out to be insufficiently discriminating. (Detlefsen[1986], p102)

I find this objection reasonable. The notion of "strong representation", after all, is a bit contrived. If the fact that the derivability relation can be easily decided is enough to show that the proof predicate chosen is the only reasonable one, the whole problem would become quite vacuous. That the extensional adequacy of the derivability relation entails the representational adequacy of the proof predicate can also be a matter of dispute, since it is not totally obvious why the existential abstraction operation would transfer the desirable properties.

However, as has also been noted in Steiner[1991], Detlefsen's objection is also flawed. Although Mostowski's defence is based on the notion of arithmetization, it is not necessarily a crucial part in Mostowski's argument. All that Mostowski needs to assume is that there is a certain way of representing the properties of the proof predicate in the theory T. The fact that there are more "general" truths conceivable of the proof predicate is simply because "proof", as a notion of finite sequence of formulas, is a metamathematical notion and arithmetization is a way of bridging its properties into T. But note that we can not argue against the reliability of PA in proving mathematical facts, and we do not worry that there are more properties of, say, "addition" that can not be registered in PA. Therefore, if we consider a system that can express the notion of proof *naturally* without any auxiliary techniques, then we should agree that in that system the notion of proof is just as direct as the notion of "addition" in PA. An easy candidate of such a system would be ZFC, where a sequence of formulas is just a well-defined set. Then Mostowski's argument can in principle be modified to apply to ZFC, where Detlefsen's objection can not arise, and the incompleteness theorem can be strengthened in that way.

There are other attempts for strengthening G2, such as that provided by Kreisel-Takeuti, which will not be discussed here further. In general, Detlefsen's objections to these proposals are quite well-posed. However, as we mentioned in the beginning, the fact that there can be gaps between the technical result of Gödel and the more or less vague program of Hilbert is not surprising, which can also be reasonably hard to amend. However, strong as Gödel's result is, the burden of proof is laid on those who wish to carry on Hilbert's Program. The received view that the program fails is not only the consequence of the technical result, but more of seeing the failed efforts of continuing the original program in an intuitively appealing form as Hilbert has imagined. Hence the effort of Detlefsen has to be judged by his positive proposals on how to find interesting enough nonstandard notions of proofs which can stimulate the interest in the original program. This, however, is more poorly done, as we will see in the following sections.

3 Hilbert the Instrumentalist

As a well-known major controversy among philosophers of science, the instrumentalists view theories as tools for systemizing observational statements, while realists think of theories as true descriptions of reality and theories can bare truth values. Although problems in philosophy of mathematics can usually be different from those in philosophy of science, Hilbert himself enjoyed the analogy between physics and mathematics, and often drew insights from the former in formulating his views on the latter. It is commonly received that Hilbert's view of mathematics is closest to an instrumentalist's one.

Yet the contents of instrumentalists' views can be varied, and Hilbert's view is not an extreme one. Instead of claiming that the whole mathematics is tool-like and just a game of symbols for no good purposes, he introduced the distinction between real mathematics and ideal mathematics, and proposed that the relation between the two should be that ideal mathematics is a (conservative) extension of real mathematics, and the use of ideal mathematics is to serve epistemological goals:

> In my proof theory, the transfinite axioms and formulae are adjoined to the finite axioms, just as in the theory of complex variables the imaginary elements are adjointed to the real, and just as in geometry the ideal constructions are adjoined to the actual. The motivation and the success of the procedure is the same in my proof theory as it is there: that is, the adjoining of the transfinite axioms results in the simplification and completion of the theory.

In Hilbert[1926], the notion of an ideal proposition is clearly introduced. In Hilbert[1927], when he first speaks of real propositions in addition to the ideal, he made it clear that the real part of mathematics should consist only of decidable, variable-free formulas.

> Hence even elementary mathematics contains, first, formulas to which correspond contentual communications of fintary propositions (mainly numerical equations or inequalities, or more complex communications composed of these) and which we may call the *real propositions* of the theory... (Hilbert[1926], p470)

On the other hand, ideal propositions do not "refer" but are only tools that are used to help clarify and simply the theory for real mathematics. Further, they should justify what the current mathematicians are doing as much as possible.

> How, then, do we come to the ideal propositions? It is a remarkable circumstance, and certainly a propitious and favorable one, that to enter the path that leads to them we need only continue in a natural and consistent way the development that the theory of the foundations of mathematics has already taken. Where we had propositions concerning numerals, we now have formulas, which themselves are concrete objects that in their turn are considered by our perceptual intuition, and the derivation of one formula from another in accordance with certain rules takes the place of the number-theoretic proof based on content.(Hilbert[1926], p379)

> ... and, second, formulas that – just like the numerals of contentual number theory – in themselves mean nothing but are merely things that are governed by our rules and must be regarded as the *ideal objects* of the theory.(Hilbert[1926], p470)

Hence, the distinction between real and ideal mathematics is crucial to the general framework that Hilbert tried to provide for mathematics. Given this distinction, abstract methods and notions of the ideal mathematics are no longer problematic, they can all be justified as long as useful for simplifying real mathematics and making more facts about real mathematics epistemologically available to us. All that we need to justify is that they can never give rise to false statements about real mathematics. At this point, it is crucial that real mathematics is a subset of ideal mathematics – whenever a false statement in real mathematics is obtained from ideal methods, they immediately give rise to inconsistency in the theory. Hence all we need to show is that the ideal mathematics is consistent, which will then never give rise to false statements in the realm of real mathematics. To conclude this section we cite Detlefsen's summarization of Hilbert's instrumentalism:

> For present purposes, we shall take instrumentalism with regard to a given body T of apparent theorems and proofs to consist in the belief that the epistemic potency of T (i.e., the usefulness of items of T as devices for obtaining valuable epistemic attitudes toward genuine propositions of some sort) can be accounted for without treating the elements of T literally (i.e., as genuine propositions and proofs), but rather as "inference-tickets" of some sort. ... a theorem (or pseudo-theorem) t functions as an inference-ticket with respect to a genuine proposition P when it is used to acquire an epistemic attitude (e.g., belief, justified belief, or knowledge) toward P, but not by dint of our adopting an epistemic attitude toward t itself; similarly, a proof (or pseudo-proof) p of genuine proposition P functions as an inference-ticket when it is used to obtain an epistemic attitude with respect to P, but not by virtue of our having adopted an attitude of belief regarding the truth of its premises and the truth preservingness of its inferences. (Detlefsen[1986], p3)

4 Strict Instrumentalism and Nonstandard Proof Predicates

As we have mentioned in Section 2, it is not enough to just re-claim the gap between Gödel's theorems and the impossibility of Hilbert's program, since this very fact has already been noticed by Gödel himself and the received view is not merely a result of the technical theorems. To make his work interesting, Detlefsen has to provide a possible way of carrying on Hilbert's program. In particular, a nonstandard notion of consistency, Cons'(T) of PA should be provided and shown to satisfy at least three requirements:

1. Cons'(T) truly expresses a reasonable notion of consistency;
2. This notion of consistency is itself *consistent* with Hilbert's proposals;
3. Cons'(T) has to be provable in T.

The second requirement is crucial, maybe more so than the other two, but has been somehow neglected by Detlefsen, as we will show in the next section. In saying that the new notion should be consistent with Hilbert's original proposals, we do allow new extensions and strengthening of his original ideas (such as the strict interpretation). However, the new notion should not imply anything that would contradict what Hilbert has clearly formulated; since otherwise it can no longer be thought of as a continuation of his original program.

The positive proposal of Detlefsen is built upon a radical reading of Hilbert's instrumentalism. Detlefsen proposes that if mathematics is really supposed to be used as an "instrument" for reasoning, it has to accomadate to the reasoning capacity of human.

> Yet the very limitations on human epistemic resources which attract the Hilbertian to the ideal method also pose clear limits to its utility. Thus, only ideal proofs falling below a certain level of length and/or complexity will be of any human utilit. Ideal proofs exceeding that level will be of no value as devides of epistemic acquisition... Put more plainly, the Hilbertian instrumentalist is obliged to establish the reliability only of such ideal proofs as are of feasible length and complexity. (Detlefsen[1986], p83)

Detlefsen argues that Gödel's theorem shows the incompleteness of *the* ideal mathematics which is not fully regarded as an instrument, but more like the real mathematics over which we may not have epistemological control. Instead, all that the Hilbertian need to do is to show that an interesting subclass of the full ideal mathematics is consistent, which can then be used to derive true statements about real mathematics.

What Detlefsen then went on to provide is an example for nonstandard proof predicates, which is the Rosser-provability predicate. Note that to provide a nonstandard notion of consistency it is enough to provide a nonstandard notion of provability, since consistency is always expressed as $0 = 1$ is not provable.

The starting point of Rosser-provability is that since we have this resource limit on the syntactical complexity of formulas, it may make sense to have a partial order on the set of all the proofs, and cut out the subset that we need as the usable part of ideal mathematics. Therefore, we suppose that there is an ordering on the syntactical complexity of the formulas in a theory T. A formula x is then called Rosser-provable, if:

1. x is T-provable;
2. There is no shorter proof, w.r.t. the syntactical ordering we have fixed on the set of all proofs, that proves $\neg x$.

A few consequences follow from this definition.

1. If T is consistent, then all the propositions are provable if and only if they are Rosser-provable.
2. However, if T is inconsistent, not all formulas are Rosser-provable.
3. Rosser-provability is not closed under Modus Ponens.

The reason for 2 is easy to see: if a theory is consistent, then all the formulas are T-provable, in particular both x and $\neg x$ are provable. Now the higher-ranked proof for one of them can be used to refute the Rosser-provability of the other. The reason for 3 is hence also clear, suppose we have, trivially in an inconsistent theory, both x and $x \to y$ are Rosser-provable. Now y is provable, but since $\neg y$ is also provable, and without loss of generality we can assume that the proof for $\neg y$ is ranked higher than that of y (otherwise the two can be swapped), it follows that y is not Rosser-provable.

Naturally, we define Rosser-consistency to mean that $0 = 1$ is not Rosser-provable. Now it is easy to see that the Rosser-consistency of T can be trivial to establish. This is because, by definition, $0 = 1$ is not Rosser-provable if and only if it is either not provable, or it is refuted by a shorter proof. Since under syntactical considerations, the refutation of $0 = 1$ should be ranked highly in the ordering of the proofs, the consistency of T can be established by just showing that such a refutation can be obtained within certain syntactical limit.

Whether Detlefsen himself has taken Rosser-provability seriously as a proposal for a nonstandard proof predicated that is usable for the continuation of Hilbert's program is unclear. He gave the following remark in a footnote:

> To avoid misunderstanding, let us now register some disclaimers concerning our use of Rosser variants. First, we are not claiming that they show that Hilbert's Program *can* be carried out. ... The standard argument makes crucial use of the claim that every reasonable notion of provability must satisfy the Derivability Conditions. For it is from this claim that it derives the unprovability of every reasonable consistency formula. Our use of Rosser variants is intended to oppose this basic claim by showing that Rosser provability is an instrumentalistically reasonable notion of provability. In other words, our use of Rosser variants is designed to show that the anti-Hilbertian who employes

> the current version of G2 cannot solve the stability problem. And that is all that we claim for it.

This somewhat embarrassing footnote seems to have refuted the constructivity in Detlefsen's proposal already. As we have already mentioned, it is not enough to show what Detlefsen claims that he is only trying to show. The received view that Hilbert can not be further carried on is exactly formed by realizing the problems of any variant proof predicates that have been proposed. In particular, the problem with Rosser-provability is not only epistemological – in that there is not clear way of doing proofs under this notion since we can not always carry around the long list of proofs that have been examined – but indeed technical: even if we have established the Rosser-consistency of our theory T, we can not do ideal mathematics simply because what we have already established maybe have a refutation that will only later come up in the ordering of proofs. Hence, ideal mathematics can not be applied at all to prove statements in real mathematics, its "instrumental" value does not exist at all.

5 Problems with Strict Instrumentalism

We have seen the problem of Detlefsen's constructive proposal, and may conclude with the impression that he proposed some open direction which he himself does not have a good view of the outcome. However, the problem with his proposal seems to be even more serious. Recall that in the previous section we noted that, if the Hilbertian want to "continue" with Hilbert's Program instead of rewriting the whole thing, then the new extensions of the original program should at least not be contradictory with Hilbert's original proposals.

In Hilbert[1927], a proof theory for ideal mathematics was explicitly given. In particular, it has been made very clear that modus ponens should be an indispensable rule:

> A proof is an array that must be given as such to our perceptual intuition; it consists of inferences according to the schema
>
> $$\frac{\mathfrak{S} \quad \mathfrak{S} \to \mathfrak{T}}{\mathfrak{T},}$$
>
> where each of the premises, that is, the formulas $\mathfrak{S}, \mathfrak{S} \to \mathfrak{T}$ in the array, either is an axiom or results directly from an axiom by substituion.(Hilbert[1927], p465)

Hence the use of Rosser-provability predicate should be refuted quite easily. Now we show that further, Detlefsen's radical reading of Hilbert's instrumentalism as strict instrumentalism is in general problematic in the same way.

This is not so hard to show, as long as we take a look at some of the implications of having only a finite set of statements in our ideal mathematics. Suppose we still work with the signature $\langle 0, s, +, \cdot \rangle$. Let the finite theory be named FT. We show that Detlefsen either has to refuse any universal statements in FT, or has to withdraw the rules for instantiations of universal statements, or modus ponens.

Let us first exclude one case. Suppose we do not allow any universal statement in FT. That is apparently unsettling, because the whole program of finitist mathamatics is to do justice to the ideal mathematics that mathematicians do on a daily basis. By ruling out the use of universal statements, there can be no generalizations in the ideal mathematics, which can no longer be the "epistemological tool" that we use for deducing facts about real mathematics. Apparently we want to be able to derive laws like "additions are commutative" in PA, hence universal statements can not be excluded from FT.

Now let $\forall x \varphi(x)$ be any universal statement in FT. In the ε-axioms given by Hilbert, it is made clear that:

$$\forall x \varphi(x) \to \varphi(a)$$

should be valid (Hilbert[1928], p466). Here for our use we only need a to be a constant in the language – i.e., a is of the form $s(\cdots(s(0)\ldots)$. Now since FT is finite, we let n be the maximum length of formulas in FT, and consider instantiating the universal formula with the constant n,i.e., the term that has n times of application of s in front of 0. Now the problem is apparent, $\varphi(n)$, by definition of FT, can not be a member statement of FT. But it surely follows from modus ponens and the instantiation rule for the universal formula. Hence, either the instantiation rule or modus ponens has to be dropped from the rules for ideal mathematics. But this contradicts with Hilbert's own formulation of the proof theory of ideal mathematics.

Hence, at least a syntactical interpretation of strict instrumentalism is always contradictory with Hilbert's original program. We can further consider counter-moves by strict instrumentalists, which may be to give semantic interpretations of the idea. A possible proposal is that, the definition of a finite class of statements does not need to be taken *syntactically*, rather, it should be taken semantically that the subclass of statements are only needed to talk about semantically finite sets of objects – i.e., finite models. In this way, we do not have a direct limit on the syntactical apparatus that we can use, by may still have a possible consistency proof if all our models are assumed to be finite, since the semantic interpretation of the proof predicate would then be a finite set of numerals.

However, this can only make things worse. If only finite models are considered, then we need to take into account of all the negative results on first order logic that has been uncovered by finite model theory. To say the least, first order logic is incomplete with respect to finite models (Trakhtenbrot's Theorem). Other highly useful properties such as compactness also fail over finite models. Hence it is very unlikely that semantic variants of strict instrumentalism can be viable either.

6 Conclusion

We have reviewed the major proposals in Detlefsen[1986]. It is undeniable that Detlefsen has provided a clear explanation of Hilbert's program, and convincing arguments against that the Gödel's Second Incompleteness shows the impossibility of Hilbert's Program. However, we have also questioned the novelty of such negative claims, as well as the possibility of positive proposals under his radical interpretation of Hilbert's instrumentalism. In conclusion, it is quite impossible to continue Hilbert's Program in the way that Detlefsen imagined. Although it is admitted that there are always gaps between Gödel's theorems and the failure of Hilbert's Program, it has become a more and more unattractive project for anyone who wishes to provide variant notions of consistency in a way that the program could be continued, and Detlefsen's work is not really able to provide new excitement.

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