Hilbert's Philosophy of Mathematics

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1 INTRODUCTION

The lens of history tends to diminish losers and magnify winners: because Hilbert's viewpoint seems to have been refuted as decisively as any philosophical view can be, there is a tendency to see his formalist programme and its philosophical basis as a relatively unimportant cul-de-sac in the history of the philosophy of mathematics. Against this, the contention of this paper is that Hilbert's contribution was of central importance in the development of mathematical philosophy and a paradigm of progressive philosophical practice.

It is not here argued that Hilbert's programme survives Gödel's incompleteness theorems. On the contrary, attempts to save the programme by watering down the notion of finitist reasoning do not seem to be philosophically helpful—which is not to deny the interest of seeing how far into the transfinite we must go in order to prove the consistency of given systems. The major claims of this paper are these:

1 Hilbert's formalist programme was a natural response to the crisis in foundations of mathematics, one which Cantor would have endorsed despite his unshakeable belief in the transfinite;
2 Brouwer's criticisms of the programme (those which don't presuppose acceptance of an intuitionist outlook) can be met;
3 Hilbert's firm belief prior to 1931 in the achievability of his programme was not unreasonable, given the high tide of positivism in that period;
4 While Hilbert's programme was fruitful and, at the time, plausible,

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2 A case for this is made in Detlefsen [1979].
the resuscitation of formalism by Robinson and Cohen (following Cohen’s proof of the independence of the Continuum Hypothesis) is untenable and barren.

2 THE GENESIS OF HILBERT’S PROGRAMME

For an undistorted view we must look at the historical context of the genesis of the formalist approach. A major feature of the development of mathematics in the nineteenth century was the dethronement of geometry: the discovery of non-Euclidean geometry by Bolyai, Lobachevsky and Gauss, which shattered the idea that Euclidean geometry was the paradigm of a clear and certain science; and surprising discoveries which shook past confidence in the reliability of geometrical intuition in analysis, such as the discovery of continuous but nowhere-differentiable functions. This paved the way for the elimination of geometrical intuition—methods and ideas of the mind’s eye—from analysis, dovetailing neatly with the abandonment of reasoning with infinitesimals in favour of the methods and definitions of calculus introduced by Bolzano and Weierstrass.

Now this process raised the question of the fundamental nature of the continuum. This was a question that mathematicians could not have become aware of as long as they were not conscious of the inadequacies of the geometrical ideas of line and point which had formerly been regarded as needing no mathematical clarification. The first and most important advances in response to this problem were made by Dedekind, Weierstrass and Cantor towards the end of the nineteenth century. In their work the new basis of the continuum was the conception of arbitrary (possibly undefinable) actually-infinite totalities, a conception developed by Cantor into a fully-fledged theory, with an accompanying theory of transfinite arithmetic.

Every revolutionary advance meets with reactionary opposition. Cantor’s theory of the infinite was no exception. But suspicion and rejection of the idea of the actual infinite was not new, and Cantor took pains to answer the specific arguments against the actual infinite that appeared in the writings of philosophers from Aristotle onwards. However, writers on Cantor seem to have overlooked the fact that his attitude towards finitism was by no means uniformly hostile.¹ He was irreconcilably opposed to those who, like Kronecker, objected to the direction analysis had taken under the influence of Bolzanos and Weierstrass; had their prohibitions against non-constructive methods been observed ‘science would have been retarded’, Cantor said. There was, on the other hand, a descriptive but non-revisionary finitist view of analysis which Cantor, while disagreeing, treated with respect.

On this view no numbers exist apart from the finite integers and perhaps

¹ Dauben for example, in his excellent mathematical biography of Cantor, Dauben [1979].
the rationals; signs for irrationals have ‘a purely formal meaning’. Cantor continues:

The actual material of analysis is composed, in this opinion, exclusively of finite integers and all truths in arithmetic and analysis already discovered or still to be discovered must be looked upon as relationships among the finite integers; the infinitesimal analysis and the theory of functions are considered legitimate only in so far as their theorems are demonstrable through laws holding for the finite integers (Cantor [1883]).

Though he disagreed with this view, Cantor responded favourably because he saw that its implications were not destructive, unlike Kronecker’s finitism, but wholly positive: he saw how this version of finitism, if correct, could provide a guarantee of the soundness and reliability of analysis:

If . . . only the finite integers exist and all else is nothing other than forms of relationships, then it can be expected that the proofs of analytical theorems can be tested for their ‘number-theoretic content’ and that every gap which appears in them can be filled out according to the basic principles of arithmetic; in the feasibility of such a ‘filling out’ is to be seen the true criterion for the genuineness and complete rigour of the proofs (Cantor [1883]).

To ‘fill out’ a proof of a theorem in analysis is to replace all non-finitary expressions and inferences by finitary ones. So Cantor is here saying that if finitism is correct a Hilbert-like programme for establishing the correctness of analysis would be achievable and useful. This proto-formulation of such a programme may even have been the actual source of Hilbert’s idea.

The significance of Cantor’s anticipation of Hilbert is that it discredits the prevailing idea that Hilbert’s programme was nothing more than a strategy to defeat Brouwer’s intuitionist ‘putsch’.¹ For it is evidence that a programme of finitist reduction was a natural response to the deeply felt epistemological uncertainty about the transfinite, whether or not one believed the ontological doctrine that infinite sets and numbers do not exist.

The paradoxes of the infinite increased this feeling of uncertainty. Cantor saw no obstacle in their paradoxes, for he believed that he had a satisfactory conceptual basis for the distinction between those totalities which could be regarded as having a cardinality, a power set etc. and those which could not be so regarded without inconsistency. But his conception was not widely understood and so solutions within the Cantorian framework appeared to many to be ad hoc manoeuvres.² Consequently it was felt that a new foundation for analysis was called for which did not depend on the notion of actual infinite totalities. Hilbert’s programme was a positive response to that challenge.

¹ See for example, Reid [1970].
² It is still sometimes asserted in modern text books that Cantor’s set theory is inconsistent, e.g. p. 184, R. Rogers [1971].
3 THE PHILOSOPHICAL BASIS OF HILBERT’S PROGRAMME

Hilbert described classical analysis as ‘a symphony of the infinite’ (Hilbert [1925]). He wanted to vindicate not revise it. But it was not only analysis he wanted to save. Though he did not believe in the existence of transfinite sets and numbers he regarded Cantor’s theory of the transfinite as a beautiful instrument of mathematical discovery. It seemed to him, Hilbert said, to be ‘the most admirable flower of the mathematical intellect and in general one of the highest achievements of purely rational human activity’ (Hilbert [1925]). So Hilbert’s aim was not to do away with Cantor’s theory, nor to replace set theoretic accounts of real analysis, but to show that, despite the paradoxes, Cantorian set theory harboured no inner contradiction.

The source of the paradoxes lay not in the concepts of Cantor’s theory, according to Hilbert, but in the misapplication of certain modes of inference to those concepts:

contetntual logical inference has deceived us only when we accepted arbitrary abstract notions, in particular those under which infinitely many objects are subsumed . . . we obviously did not respect necessary conditions for the use of contentual logical inference (Hilbert [1925]).

And in a passage repeated word for word in his article ‘The Foundations of Mathematics’ Hilbert specified these conditions for reliable logical inference:

(1) the elements of the domain must be extralogical concrete objects of which we have immediate awareness prior to all thought;
(2) the domain must be completely surveyable;
(3) the occurrence and arrangement of the objects must be immediately given (‘anschaulich’), as irreducible facts.

The reliable parts of mathematics, then, are those which remain within the bounds of mechanical reasoning about surveyable domains of concrete objects (such as mathematical symbols). Accordingly, mathematical sentences were divided into those with content (or meaning) and those without: those which are mechanically decidable in a finite number of steps have content and so does any universal schema whose instances are all finitarily decidable. The rest are meaningless strings of symbols. Hilbert gives the universal equation ‘\(a + 1 = 1 + a\)’, where ‘\(a\)’ ranges over the natural numbers, as an example of a meaningful claim whose negation is without meaning:

\[\ldots\] the proposition that there exists a number \(a\) for which \(a + 1 \neq 1 + a\) holds no finitary meaning; one cannot, after all, try out all numbers (Hilbert [1927]).

And so, he concludes, the law of excluded middle does not hold universally.

Given this austere philosophy of logic, the natural if not inescapable course would seem to be to abandon classical logic in favour of a constructivist logic. But Hilbert was convinced that he could not follow this
course without losing analysis. How then, if classical logic is indispensable for analysis but not valid over infinite domains, could Hilbert avoid the conclusion that classical analysis ought to be abandoned? Hilbert proposed that we continue using sentences without finitary content as ideal elements in a formal system. Just as ideal points and a line ‘at infinity’ are used in projective geometry, thus preserving the principle of duality (interchangeability of ‘line’ and ‘point’ in all theorems salva veritate) and the simplifying law that any two lines meet at exactly one point, so we must use sentences without finitary content as ideal propositions ‘in order to maintain the formally simple rules of ordinary Aristotelian logic’ (Hilbert [1925]).

We cannot, of course, guarantee mathematics against paradox and inconsistency simply by regarding sentences without finitary content as ideal elements. What now had to be done was to show by strictly finitist means that the classical use of ideal propositions would never lead us to accept a falsehood among the genuine propositions. This is the heart of the matter. Hilbert felt sure this could be done: ‘the demonstration can in fact be given’, he said (Hilbert [1925]).

This was to be done by setting out arithmetic/analysis/set theory as a formal system and showing that no proof in the system has ‘1 ≠ 1’ (or some other evidently false finitary proposition) as its last formula. The essence of the programme was to find a way of replacing every proof of a real proposition by a proof of that proposition which is purely finitary (containing no ideal propositions). Since there is no finitary proof of ‘1 ≠ 1’ this would ensure that this proposition could not be derived in the system, and therefore that the system is consistent (since any formula of the form ‘(A & ¬A) → B’ is provable).

It was known that the formal systems of Principia Mathematica and Zermelo-Fraenkel set theory encompassed classical analysis and all the mathematics based on methods of proof then in use. Once Hilbert and his collaborator, Paul Bernays, had settled on a precise first-order formalisation, they could focus their attention on a recursive domain of concrete objects, viz., finite arrays of symbols, formulas being finite strings of symbols, proofs being finite lists of formulas. Using only the quantifier-free logic of Skolem arithmetic (primitive recursive arithmetic)—this is what finitary reasoning amounts to—their task was to show the impossibility of exhibiting a proof whose last formula is ‘1 ≠ 1’, a decidable property of finite lists of formulas. This was Hilbert’s programme.

4 BROUWER’S OBJECTIONS

Hilbert had thus formulated a clear foundational programme which could not be ignored by any school. If successful the programme would have shown not only that belief in the existence of actual infinite totalities was unnecessary for the defence of classical mathematics as a rational enterprise; it would also have shown that the destruction of classical mathematics
entailed by intuitionism was pointless, saving us nothing in terms of intuitive certainty.

Of course, Brouwer, the founder of intuitionism, disputed this, arguing that the proposed consistency proof was insufficient as a justification for classical mathematics:

the (contentual) justification of formalistic mathematics by means of the proof of its consistency contains a vicious circle, since the justification rests upon the (contentual) correctness of the proposition that from the consistency of a proposition the correctness of the proposition follows, that is upon the (contentual) correctness of the principle of excluded middle (Brouwer [1927]).

Brouwer’s charge of circularity was not justified. Hilbert did not rely on the principle that the consistency of the propositions of a system entail their correctness. For Hilbert the propositions of a system are divided into the real propositions and the ideal propositions. There is no sense, on Hilbert’s view, in claiming or denying correctness for ideal propositions, since they are meaningless strings of symbols. Hilbert’s task was to show that any real proposition provable in the system is correct.

Hilbert did assume, quite explicitly, that a real proposition provable by finitary reasoning is correct. Given this uncontroversial assumption, it would be enough to show, by finitary means, that the system in question is a conservative extension of finitary mathematics, i.e. that any real proposition provable in the system is also provable by purely finitary means. Brouwer seems not to have realised that showing this is equivalent to showing, by finitary means, the consistency of the system. This claim of equivalence can be given precise formulation and proof. Taking the real propositions to be those expressed by $\pi_1^0$-formulas and finitary methods to be those of primitive recursive arithmetic (PRA), and if $T$ is an axiomatic extension of PRA, the arithmetised statement of the consistency of $T$ is equivalent over PRA to $\pi_1^0$-reflection for $T$. For proof see Smorynski [1977], §4.1.4.

Echoing Poincaré (Poincaré [1905]), Brouwer also criticised Hilbert’s use of primitive induction as circular (Brouwer [1913]). The principle of induction belongs to the set of axioms of arithmetic to be proved consistent. But such a consistency proof is impossible without the use of induction. As induction is not finitistically provable nor, according to Brouwer, self-evident from the finitist viewpoint, its use in Hilbert’s metamathematics was taken to be illegitimate.

In making this criticism Brouwer was ignoring the distinction between the unrestricted use of induction in a non-finitary formal system and its use to establish decidable properties of a surveyable domain of concrete objects. The use of induction in the latter case can fall within the scope of finitary reasoning. Suppose there is an effective list of the elements of the domain and suppose $F$ is a decidable property; then it does seem quite evident that any element $x$ has the property $F$, provided that (a) the first element in the list has $F$ and (b) if an element has $F$ so does the next one on the list. For if we
know that these premisses are satisfied we know that $F(x)$ can be established without induction by a finite number of steps of modus ponens (as many as there are predecessors of $x$); so we know that for any $x$, $F(x)$ is finitistically provable. As the objects of Hilbert’s metamathematics, formulas and proofs, form a recursively enumerable domain of concrete objects and as their metamathematical properties are decidable, Hilbert’s use of induction was quite valid from the finitist point of view.

The intuitionists appear to have found no other specific objections to Hilbert’s programme which do not presuppose an intuitionist point of view, nor any persuasive general arguments against the plausibility of Hilbert’s outlook. In the absence of cogent objections from the intuitionists it is hardly surprising that Hermann Weyl was persuaded in 1927 to abandon his constructivist convictions and accept Hilbert’s outlook (Weyl [1927]). For Hilbert’s philosophy was both stricter than intuitionism and mathematically less damaging: intuitionism was based on an abstract phenomenology of mental constructions yet to be made precise, whereas Hilbert’s formalism was based on mechanical (primitive recursive) reasoning about finite arrays of symbols; intuitionism bans the convenient and intuitive methods of classical mathematics and destroys analysis even after its rigorous reformulation by Bolzano and Weierstrass, while Hilbert sought to justify it. If successful Hilbert’s programme would have provided greater certainty for classical mathematics than the intuitionists could provide for the restricted mathematics acceptable to them.

5 HILBERT’S PROGRAMME AND LOGICAL POSITIVISM

Even if it is acknowledged that prior to 1931 Hilbert’s programme was an attractive and not implausible avenue of research in the foundations of mathematics, the feeling remains that Hilbert had an unreasonable faith in the achievability of his programme. I think that this assessment is quite in error, resulting from a failure to place Hilbert’s optimism in its historical context.

Having had several decades in which to digest Gödel’s theorems and to assess Hilbert’s programme in the light of those theorems and subsequent advances in proof theory, we may confidently assert that Hilbert’s programme is unachievable. Yet the intellectual pull of the belief in Hilbert’s programme was, at least until 1931, so strong that even after his proofs of incompleteness and unprovability of consistency, Gödel himself felt disposed to defend the programme in the following way:

I wish to note expressly that Theorem XI (and the corresponding results from $M$ and $A$) do not contradict Hilbert’s formalist viewpoint. For this viewpoint presupposes

1 Confirmation of the claim that finitism is stricter than intuitionism is provided by Gödel’s discovery that while there is no consistency proof of first order arithmetic within finitary arithmetic, there is a proof of its consistency afforded by intuitionistic arithmetic: Gödel [1933].
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 $P$ (or of $M$ or $A$) (Gödel [1931]).

'Theorem XI' states that if $P$ is consistent its consistency is not provable in $P$, $P$ being Peano arithmetic extended by simple type theory ($M$ is set theory, $A$ is classical analysis). Even when Gödel had come to accept the impossibility of a finitary consistency proof of number theory, he described this as a 'surprising fact' (Gödel [1958a]).

More important still for assessing the rationality of Hilbert's belief in the achievability of his programme was the contemporary high tide of empiricism in the philosophy of science. At the turn of the century, after roughly two hundred years of unperturbed rule by Newtonian mechanics, the conceptual foundations of physics were destroyed by the genesis of quantum theory and the theories of relativity: the apparently invincible framework of Newtonian physics had proved inadequate, and so it was felt that the only reliable or certain part of physics was what was observed or in some other way directly experienced. This feeling was transformed into a return to empiricism: the only facts are the observable facts; the theories of physics incorporating allusions to what cannot be observed are simply more or less useful instruments for guiding the choice and design of experiments and deducing testable hypotheses. Hilbert's view of his proof theory was at one with this philosophy and he was aware of the parallel between instrumentalism in physics and his own view:

What the physicist demands precisely of a theory is that particular propositions be derived from laws of nature or hypotheses solely by inferences, . . . , without extraneous considerations being adduced. Only certain combinations and consequences of the physical laws can be checked by experiment—just as in my proof theory only the real propositions are directly capable of verification (Hilbert [1927]).

The idea that the real propositions are those which are verifiable or falsifiable is well known as a doctrine of the Vienna Circle i.e. of positivism. Behind it lay the empiricist doctrine that the only facts are the observable facts, the only truths those which can be verified by observation (and finitary computation). This outlook gave rise to a number of reductionist programmes, which aimed to show that the purely theoretical or unobservable parts of a particular science are ultimately eliminable in favour of statements about experimental operations and regularities in what could be observed.

One of the clearest expressions of this outlook was Bridgman's programme for an operationist reduction of physics, as formulated in The Logic of Modern Physics, published in 1927. This outlook was not confined to physics; in fact it had wide acceptance in the scientific community, also finding expression in Watson's programme for the behaviourist reduction of psychology, originally proposed in 1913 (Watson [1913]) and comprehensively stated in his Behaviourism, published in 1925; and it pervaded the
philosophy of the Vienna Circle. Its most generalised expression was Carnap’s attempt to show that the language of science was translatable into a purely phenomenalistic language, in *Der Logische Aufbau der Welt*, published in 1928.

It is no accident that the finest philosophical expressions of Hilbert’s view, his articles of 1925 and 1927, appear in the same period as the major programmatic statements of Bridgman, Watson and Carnap, and the period in which the Vienna Circle flourished. The point here is that Hilbert’s belief in the achievability of his programme was no less reasonable than the belief in the achievability of the other foundational programmes. To characterise Hilbert’s attitude as unreasonable is, therefore, either to ignore the intellectual context in which his attitude was formed or to condemn as unreasonable the philosophical outlook of an entire generation of philosophers and scientists. By what supra-historical standards can one make such judgements?

It is true that some reductionist proposals were naive and crude, even by the standards of the time. Heisenberg, for example, was justly criticised by Bridgman for demanding an operationally defined measurement for every numerical term occurring in a physical theory and an operational interpretation, expressible in ordinary language, of every equation (pp. 64–6 Bridgman [1936]; p. 149 Heisenberg [1958]). Heisenberg’s exaggerated demand runs parallel, not to Hilbert’s formalism, but to the intuitionist demand that every sentence of a mathematical theory should have a constructive interpretation. Hilbert explicitly rejected this demand in a passage responding to Brouwer’s reproach that Hilbert’s view reduces mathematics to a game:

This formula game enables us to express the entire thought-content of mathematics in a uniform manner. . . . To make it a universal requirement that each individual formula be interpretable by itself is by no means reasonable; on the contrary a theory by its very nature is such that we do not need to fall back on intuition in the midst of some argument. What the physicist demands of a theory is that propositions be derived from laws . . . solely by inferences, hence on the basis of a pure formula game, without extraneous considerations. Only certain consequences of the physical laws can be checked by experiment—just as in my proof theory only the real propositions are directly capable of verification (Hilbert [1927]).

We can conclude that Hilbert resisted the destructive excesses of the empiricist revival. His programme was by contemporary standards a sophisticated expression, within the philosophy of mathematics, of an outlook that was widely accepted in the scientific community of the time. If now we find it difficult to understand why, before Gödel’s results, Hilbert seemed to entertain no doubts about the possibility of achieving his programmatic objectives, it may be because we find ourselves in a philosophical climate generally hostile to the empiricism (specifically, positivism) which flourished before the Second World War.
6 AGAINST THE FORMALIST REVIVAL

The aims of Hilbert’s programme, to provide a complete formalisation of classical mathematics and a finitary consistency proof of the formal systems thus obtained, cannot be fulfilled. Even the weaker objective of establishing by purely finitary methods the consistency of the (necessarily incomplete) formal theory of arithmetic cannot be achieved, and the same is true, a fortiori, for the theory of real numbers and set theory. If this was not established conclusively by Gödel’s second incompleteness theorem—Gödel himself thought it was not—it did become clear as a result of subsequent work by Gentzen. Gentzen proved the consistency of number theory by means of induction up to $\varepsilon_0$, but he also proved that induction up to ordinals strictly less than $\varepsilon_0$ was provable in number theory (Gentzen [1943]), and so by Gödel’s second incompleteness theorem, for no ordinal $\alpha$ less than $\varepsilon_0$ would induction up to $\alpha$ plus purely finitary methods be sufficient for proving the consistency of number theory. Yet induction up to $\varepsilon_0$ takes us beyond the realm of what is directly surveyable and therefore beyond that part of mathematics which the formalists accept as meaningful.

In an era of foundational programmes for science and mathematics, Hilbert’s was the most clearly and precisely formulated of all. For this reason it was also the one capable of the most decisive refutation. This must be seen as a merit, for it serves mathematical as well as philosophical progress: Hilbert’s programme gave rise to completely new fields of study within mathematical logic concerning the problems of consistency, completeness and decidability of formal theories.

Gödel’s results of 1931 can be regarded as theorems of limitation on what can be achieved in the direction of Hilbert’s objectives. After 1931 research was undertaken to see what can be achieved within the limits discovered by Gödel: for example the successful attempts to prove the consistency of number theory keeping within constructive bounds and straying as little as possible from what is immediately evident (see Gödel [1933], [1958] and Gentzen [1936]). In order to mitigate the effects of incompleteness Turing introduced the idea of a succession of formal systems of increasing strength, indexed by recursive ordinals, such systems being known as ordinal logics (Turing [1939]). These two lines of research are major components of proof theory. Within recursion theory the study of the decidability of theories was also a natural outgrowth of Hilbert’s programme, as Rabin explained:

... implied by Hilbert’s programme is the belief that the process of theorem-proving is mechanizable or, in modern parlance, that mathematical theories are decidable. Failing to implement Hilbert’s plan for mathematics as a whole, by proving the consistency and decidability of, say, set theory, researchers turned their attention to more restricted segments of mathematics. Many of the decidability results... such as Presburger’s decision method for the theory of addition of natural numbers, were obtained in the Twenties and early Thirties and were motivated by Hilbert’s plan (Rabin [1977]).
So Hilbert’s formalist programme was not only a natural and plausible product of the positivist era: it was also very fruitful.

This is a paradigm example of how a mistaken philosophical outlook articulated at the right time can be progressive. But the timing is crucial; a return to the formalist outlook today can have no progressive consequence, for the formalist foundational programme is irredeemable, as the modern formalists have acknowledged. In ‘Formalism 64’ Robinson wrote:

I cannot see at this time how a form of reasoning which attempts to escape the consequences of Gödel’s second theorem (such as Gentzen’s consistency proof or any other consistency proof for Arithmetic) can remain strictly finitistic and hence interpreted (Robinson [1964]).

The other eminent proponent of formalism in recent times, Paul Cohen, also believes that ‘the Hilbert programme can in no sense be resuscitated’ (Cohen [1967]). Formalism today does not offer a means of securing classical mathematics. On the contrary, it offers the pessimistic message that no foundation whatever is possible.

The revival of formalism was a reaction to Cohen’s discovery that the cardinality of the continuum is not determined by the current axioms of set theory (Cohen [1963]). The train of thought motivating this response is roughly as follows: even though the question of the cardinality of the continuum is a fairly simple and natural one, our most far-reaching conceptions as encapsulated in the axioms of set theory provide us with no means of settling it; there is, therefore, no sense in supposing that Cantor’s hypothesis that the cardinality of the continuum is the second infinite number (CH) has a truth-value; and if CH has no truth-value it has ‘no intrinsic meaning’ as Cohen puts it; if it has no intrinsic meaning this must be so in virtue of some feature which CH shares with other sentences in the language of set theory, namely, purported reference to infinite sets.

This line of thought is questionable at every step. Suffice it to say, for now, that any inquiry into the philosophical significance of Cohen’s discovery (and associated independence results—see Lévy and Solovay [1967] and Easton [1970]) must take into account the historical fact that our conceptions are not static but constantly developing; what may be presently unanswerable may become answerable in the future as our conceptual grasp of infinite structures grows and new axioms are added to the current axioms of set theory. So the independence results do not really warrant a retreat to formalism.

Right or wrong, the formalist’s belief that non-finitary mathematics is manipulation of meaningless formulas cannot rationally be combined with the formalist recommendation that classical practice be continued just as if infinite totalities actually existed. For there is no reason, given the formalist belief, for believing in the correctness of the meaningful (i.e. finitary) statements derivable within a formalisation of classical mathematics, because infinitely many of these statements have no finitary proof. Surely it
is irrational to use a theory whose consequences we have no reason to believe are correct.

Robinson considers two alternative ‘criteria of acceptability’ of theories: the possibility of using the theory ‘as a foundation for the natural sciences’, and the possession by the theory of mathematical beauty and ‘internal relevance’ (Robinson [1964]). Whatever it is for a theory to serve as a foundation for science, one requirement would surely be consistency of the theory; but the formalist cannot establish the consistency of classical mathematics or even first order number theory, so it is impossible for him to justify its use on the grounds that it can serve as a foundation for science. As for mathematical beauty, is it not in the eye of the beholder? In any case, it is too elusive a property to serve as a criterion; we would first need a criterion of beauty. This aside, there can hardly be any beauty or internal relevance in a theory from which every formula is derivable. Yet this is a possibility that cannot be ruled out until consistency has been established. Thus Robinson failed to find criteria by which a formalist could justify his acceptance in practice of classical mathematics.

Cohen takes a quite different line from Robinson:

Since I feel it incumbent on me, having chosen Formalism, to explain why I do not advocate abolishing all infinitistic mathematics, I should like to put forward the view that we do set theory because we feel we have an informal consistency proof for it (Cohen [1967]).

A formalist can hardly take this feeling (that we have an informal consistency proof) seriously, unless he believes that corresponding to the informal proof there is a finitary consistency proof. Of course Cohen does not believe this; and he admits that the intuition behind the feeling that we have an informal consistency proof cannot be made precise. Thus there is an inner tension, if not a self-contradiction, in Cohen’s philosophical view. This again finds expression in what he says about the arithmetical consequences of set theory:

[Gödel’s incompleteness theorem] still is the greatest barrier to any attempt to totally understand the nature of infinite sets. At the same time, since it shows that higher infinities have repercussions in number theory, allowing us to prove otherwise unprovable statements, it makes it extremely difficult to maintain that higher infinities can merely be dismissed (Cohen [1967]).

An example of such a repercussion is the $\pi_1^0$-formula which encodes the claim that $ZF$ set theory is consistent, which can be derived from $ZF$ plus the axiom that there is an inaccessible cardinal. However, the formalist has absolutely no reason to believe in the correctness of the arithmetical statements derivable from set theory which are not otherwise provable, for $ZF$ and its extensions are, from the formalist point of view, merely systems for generating formulas, systems which there is no reason to believe will not generate inconsistent formulas. Thus Cohen’s regard for the repercussions of set theory within number theory is merely evidence that his commitment
to the formalist outlook is not wholehearted. Hilbert, in contrast, sought to justify classical practice, and set theory in particular, by showing that it had no new repercussions in finitary arithmetic.

7 CONCLUSION

The impossibility of reconciling the formalist view of the non-finitary theories of classical mathematics with the formalist prescription to continue just as if they were known truths about infinite structures makes formalism today an untenable philosophy. This observation does not at all detract from the importance of Hilbert’s philosophical contribution. Hilbert’s optimistic outlook was fruitful because it offered a clear foundational programme for mathematics. It was philosophically as well as mathematically fruitful because it was precise enough to be clearly refuted: we have learnt something we would not have learnt without Hilbert’s contribution, namely, the irreducibility of mathematics to finitary computation. In order to reap the full benefit of Hilbert’s work we must face up to this fact and accept its practical consequences, that is, we should take seriously our higher-order intuitions and conceptions as a means of developing our theories and establishing their soundness.

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