David Hilbert's Naturalism

Curtis Franks

November 13, 2005

1 Introduction

In [1922c] David Hilbert delivered a series of lectures to the Hamburg University Mathematics Seminar outlining the methods for a program that would occupy him and his colleagues for roughly a decade. His report repeatedly emphasized the role that a certain type of consistency proof was to play in the realization of the program's aims. After the mathematical and philosophical communities came to understand the theorems of Gödel [1931d] as demonstrating the unavailability of this type of consistency proof, they rejected on that ground Hilbert's program as a failed attempt at securing the foundations of mathematics. Indeed Hilbert's program has come to be *identified* in many minds with the production of an appropriate sort of consistency proof–so much so that his contributions to the foundations of mathematics are simply, in the opinions of many, a glib "formalism" regarding the nature of mathematics, a somewhat vague, proto-constructivist "finitism" regarding mathematical existence, and a radical, "all eggs in one basket" thesis according to which mathematics is a terminally unfounded enterprise unless the consistency of a significant portion of it can be proven (impossibly) according to the demands of these two restrictive "isms".

This construal of Hilbert's program is difficult to reconcile with a continuous theme in his Hamburg lectures according to which one has every reason to believe in the consistency of mathematics, because of the clarificatory gains in the axiomatics of Weierstrass, Frege, Dedekind, Zermelo, and Russell and, most of all, because everything in our mathematical experience speaks for it's consistency. If this sentiment amounts only to an optimism that the carefully described consistency proof will eventually be carried through, then it is a remarkably cavalier sentiment owing to the fact that the proof called for was an open problem in a completely new and largely uncharted field of logic. Moreover there is no hint as to why the clarification gained through axiomatics or especially through "mathematical experience" should weigh in on the prospects for such a proof. Instead Hilbert appears to be articulating in the report a pre-theoretic belief in the consistency of mathematics that on the one hand does not ride on the promise of a realization of his foundational program, and on the other hand carries on unscathed by announcements of skepticism from the philosophical schools. This is hardly the mindset of one who sees the question of the consistency of mathematics depending entirely on the existence of a proof radically constrained by two philosophical scruples.

Even more difficult to reconcile with the received view of Hilbert's foundational contributions is his announcement that he intends his treatment, which he describes as epistemological, to probe more deeply than previous investigations in the foundations of mathematics. The technical component of his program no doubt was a tremendous move forward in mathematical sophistication. The report introduced for the first time the notion of metamathematics and the prescription of separate techniques for the objectual and metamathematical levels of investigation as well as–through the introduction of proof theory–a specific way to carry out metamathematical investigations. But if the philosophical position inspiring these technical achievements is merely the belief that mathematics consists ultimately of meaningless formulas and that the valid inferential moves regarding these are the purely finitary ones, then neither does Hilbert's position seem deeper than any other nor does it appear to be answering deeper, or even different, concerns than those that troubled Frege, Brouwer, and Weyl. Hilbert must, instead, be advancing a program of considerable philosophical insight if he takes himself to be advancing "a deeper treatment of the problem".

In fact Hilbert's epistemological position differs significantly from those of his intellectual adversaries. Foundational concerns had traditionally fallen directly out of skepticism concerning questions like whether mathematics is consistent, and the rigor and methodology of the investigations undertaken by Frege, Brouwer, and Weyl were responsible precisely to the nature and degree of there own skepticism. By contrast, Hilbert is less skeptical than they are concerning the consistency of mathematics but at the same time has much higher standards for what counts as a proof of it. This is because the question inspiring him to foundational research is not whether mathematics is consistent, but rather whether or not mathematics can stand on its own–no more in need of philosophically loaded defense than endangered by philosophically loaded skepticism. All the traditional "Hilbertian theses"–formalism, finitism, the essential role of a special proof of consistency–are methodological principles necessitated by this one question. When they are understood in that light, they appear no longer to be the glib scientistic principles of an expert mathematician's amateur dabbling in philosophy. They appear rather to be the constraints on method needed for probing a deep epistemological issue left untouched by rival programs.

If Hilbert's program is understood thus, the temptation to ignore it as a contribution to the philosophy of mathematics goes away. Hilbert's epistemological stance turns out to be one of philosophical subtlety and originality. Additionally, since it has been largely ignored in favor of a philosophically naïve, purely mathematical program, the viability of the program in the light of the development of logic in the last thirty-five years remains largely unexplored. We hope to show here that Hilbert's program vis-á-vis mathematical autonomy is philosophically instructive. In the next chapter we will take up the question of its logical viability.

2 Anti-foundationalism

The principal concern leading mathematicians and philosophers to consider the foundations of mathematics in the early twentieth century was the discovery of the paradoxes in nineteenth century set theory and in Frege's axiomatic system. These systems were designed to provide a conceptual framework for significant portions of mathematics in response to points of unclarity their designers sought to eliminate. Since the paradoxes played the ironic move of slipping in to notice precisely at the point of purported certitude, they were thought of as ushering in an epistemological crisis. For the paradoxical nature of mathematics had been chased directly to the conceptual framework on which the science was thought to rest. Since this framework itself had proven untenable, the new foundational task was to provide a replacement. The project was daunting. On the one hand, since mathematics was to be secured on a framework other than the one on which mathematicians had been basing their methods, there was no guarantee that the new, well-founded mathematics would resemble even closely the old. In addition, entirely new methods needed to be developed, both to build mathematics up from whatever new framework was decided on and to practice the science according to this framework. On the other hand, the prospects for deciding quickly on a new framework were dim. For the decision amounted to selecting an epistemological theory–the *correct* epistemological theory for mathematics–and the only unanimously accepted and scientifically informed principle of theory selection was to reject paradoxical ones. This opened the foundational search wide open to reflection on the nature of mathematics, consideration of the relationship between mental representation and the world, and other matters on which there was almost no consensus and equally little argumentative standard for how consensus might be reached.

Hilbert's reaction to the paradoxes and ensuing threat of mathematical inconsistency differs radically from the general response just described, but the exact nature of his reaction is somewhat elusive. His addresses routinely cycle back and forth between announcements such as this one of literal uncertainty in the consistency of mathematical theories: "we can never be certain in advance of the consistency of our axioms if we do not have a special proof of it" ([1922c], pg. 201), and statements like the following of his utmost certainty in the validity of mathematical methods: "the paradoxes of set theory cannot be regarded as proving that the concept of a set of integers leads to a contradiction" since "[o]n the contrary, all our mathematical experience speaks for the correctness and consistency of this concept" ([1922c], pg. 199). This seeming tendency to second guess or contradict himself suggests that Hilbert had no fully developed view either of the impact of the paradoxes or of the state of foundations.

One way to resolve this tension is to attribute Hilbert's call for a concrete proof of consistency and claim that the consistency of mathematics depends entirely on the existence of such a proof to his considered philosophical position, while writing off his declarations of assurance in "the correctness and consistency of mathematics" as mere academic optimism that in due time such a proof would surface from his research circle. So prone to organizing his addresses around similar optimistic proclamations (the infamous call to arms against the "*Ignoramibus*" in mathematics and the suggestion that mathematics was destined to subsume within its scope all of human knowledge are two glaring examples) was Hilbert, that this picture would seem at first not at all unreasonable. And consistently with this reading one could place Hilbert's foundational philosophy alongside his contemporaries'. He would, in particular, be in agreement with them that the discovery of paradox in current foundational programs justifies skepticism in the consistency of mathematics which can only be answered by re-securing mathematics on new foundations. Distinguishing Hilbert's proposal would be only his insistence that the absence of paradox in the new foundations should be *mathematically proven* rather than justified somehow *a priori*, and the optimism–perhaps inspired by Hilbert's distinguished mathematical tenure–that all orthodox mathematics could in this way be grounded. The passage most suggestive of this reading is the following one from the 1931 article in *Mathematische Annalen* where Hilbert characterizes his belief in the consistency of mathematics as "faith" and proceeds to claim that "faith" in this case, does not suffice:

> It would be the death of all science and the end of all progress if we could not even allow such laws as those of elementary arithmetic to count as truths. Nevertheless, even today Kronecker still has his followers who do not believe in the admissibility of *tertium non datur*: this is probably the crassest lack of faith that can be met with in the history of mankind.
>
> However, a science like mathematics must not rely upon faith, however strong that faith might be; it has rather the duty to provide complete clarity. ([1931b], pg. 268)

There is another way to resolve the tension, however, that puts Hilbert directly at odds with his contemporaries' epistemological views. Unlike the attempt just described, moreover, this resolution is in keeping with Hilbert's opening statement in his Hamburg lectures:

> If I now believe a *deeper treatment of the problem* to be requisite, and if I attempt such a deeper treatment, this is done *not so much to fortify individual mathematical theories* as because, in my opinion, all previous investigations into the foundations of mathematics fail to show us a way of *formulating the questions concerning foundations so that an unambiguous answer must result.* But this is what I require: in mathematical matters there should be in principle no doubt; it should not be possible for half-truths or truths of fundamentally different sorts to exist. ([1922c], pg. 198 italics added)

That is, Hilbert deliberately intends a deeper foundational investigation than those of his contemporaries, and his chief aim in doing so is not to demonstrate the consistency of any branch of mathematics. It is, rather, to establish a mathematical autonomy according to which the reliability and correctness of ordinary mathematical methods does not rest on *any* epistemological background–neither the failed conceptual framework of nineteenth century set theory, nor any new philosophically informed framework–since these can only ever provide "ambiguous" foundations–foundations dependent in their conclusiveness on their underlying philosophical principles. Since philosophical principles are, according to Hilbert, eternally contentious, such a defense of mathematics would only be a "half-truth": a truth only in so far as one is willing to subscribe to the relevant philosophical principles. Hilbert shares his adversaries' goal: "The goal of finding a secure foundation of mathematics is also my own. ..." But finding secure foundations is for Hilbert just to cut through the fog of such half-truths: "...I should like to regain for mathematics the old reputation of *incontestable* truth, ..." since this reputation for objectivity more than anything else is that "which [mathematics] appears to have lost as a result of the paradoxes of set theory" ([1922c], pg. 200 italics added).

The apparent conflict between Hilbert's determined affirmation of the consistency of mathematics and his desperate call for a proof of it is properly settled in precisely the opposite manner of the received view: His considered philosophical position is that the validity of mathematical methods is immune to all philosophical skepticism and therefore *not even up for debate* on such grounds. The consistency proof of a precisely delineated sort just is a methodological tool designed to get everyone, unambiguously, to see this.

On what grounds might one reasonably retreat to skepticism about the veracity of mathematical methods? To recognize a contradiction in a mathematical system is straightforward once it has been discovered. For example one is able to verify Russell's paradox in Frege's system directly, and so skepticism about the veracity of the system in Frege's *Grundgesetze* is perfectly reasonable and insurmountable. It is questionable, though, how one might foster this same sort of skepticism about a system for which one cannot formally demonstrate any inconsistency. Presumably one would need to be reasoning from some quite elementary standpoint the security of which one takes to be granted for present purposes but from which mathematical methodology seems both in need of justification and under threat of instability. One option would be to articulate some non-mathematical standpoint from which the system indeed does appear to be plagued by some noxious failing. If one could successfully defend the epistemological security of this standpoint, say by showing that it accords with appropriately elementary principles of reason and is therefore sounder than the mathematical system one is attacking from it, then again skepticism seems a reasonable refuge.

Such was the attack that the Intuitionists and Predicativists waged on orthodox mathematical practice. Hilbert's reaction to their epistemology is illuminating, for he directly challenges, not the epistemological security of the Intuitionist or Predicativist standpoints, but the general strategy just described. In fact, Hilbert does not distinguish the Predicativist and Intuitionist positions in his lectures, though his particularly detailed remarks about the charge of circularity is more applicable to Predicativism. Therefore it will be most appropriate to refer to the view he is apposing as the Predicativist view. Ultimately, however, since Hilbert criticizes the general method of skeptical foundationalism and not the details of the Predicativist position, the same critique applies equally well to the Intuitionist and any other similarly conceived program.

Just one year before Hilbert's Hamburg lectures, Weyl published a report "On the new foundational crisis of mathematics" outlining precisely the understanding of the impact of the set-theoretic paragraphs described above as well as his attempt, and another due to Brouwer which Weyl had recently embraced, at re-centering mathematical practice on new, philosophically informed foundations. The introductory comments to this report are the primary philosophical target of Hilbert's address:

> The antinomies of set theory are usually treated as border conflicts concerning only the most remote provinces of the mathematical realm, and in no way endangering the inner soundness and security of the realm and its proper core provinces. The statements on these disturbances of the peace that authoritative sources have given (with the intention to deny or to mediate) mostly do not have the character of a conviction born out of thoroughly investigated evidence that rests firmly on itself. Rather, they belong to the sort of one-half to three-quarters honest attempts of self-delusion that are so common in political and philosophical thought. Indeed, any sincere and honest reflection has to lead to the conclusion that these inadequacies in the border provinces of mathematics must be counted as symptoms. They reveal what is hidden by the outwardly shining and frictionless operation in the center: namely, *inner instability of the foundations on which the empire is constructed*. (Weyl [1921] pg. 86[1])

It is interesting that Weyl here charges the defenders of traditional mathematical techniques with trading in half-truths and self-deception, since Hilbert raises a very similar complaint with the idea of grounding mathematics on *a priori* principles. These charges seem indicative of research programs operating under fundamentally different conceptions of adequacy in scientific foundations. Weyl's main point is nonetheless clear: He takes himself to have identified the source of the antinomies, not in the experimental far reaches of mathematics, but in basic principles like impredicative definitions. Since classical mathematics abounds with such techniques even in its core research areas, Weyl took this circularity to undermine totally the veracity of ordinary mathematical practice.

Hilbert begins his objection to Weyl's skepticism by noting the artificiality of Weyl's standpoint:

> [O]ne sees that for the mathematician various methodological standpoints exist side by side. The standpoint that Weyl chooses and from which he exhibits his vicious circle is not at all one of these standpoints; instead it seems to me to be artificially concocted. ([1922c], pg. 199)

[1] Added italics follow Ewald's translation of Hilbert [1922c], where this passage is quoted.

Specifically, Hilbert criticizes Weyl's argument for resting on patently non-mathematical grounds. Immediately one wonders at the relevance of Hilbert's complaint. The standpoint of Weyl's criticism is non-mathematical, but couldn't it be reliable all the same? And if it is reliable, then should not the circularities demonstrable within it impugn classical mathematics? Moreover, what is one to make of Hilbert's charge of artificiality? Elsewhere he repeatedly emphasizes that mathematical systems themselves are fully arbitrary, that they earn their credence simply by virtue of their consistency. Why, then, should the artificiality of Weyl's standpoint implicate it?

Hilbert's elaboration of his criticism is extraordinary:

> Weyl justifies his peculiar standpoint by saying that it preserves the principle of constructivity, but in my opinion precisely because it ends with a circle he should have realized that his standpoint (and therefore the principle of constructivity as he conceives it and applies it) is not usable, that it blocks the path to analysis. ([1922c], pg. 199)

Hilbert rejects Weyl's standpoint and the philosophical principles behind it *because* of the circularity that from this standpoint appears in classical mathematics. There is no discussion of the reliability of Weyl's constructivism, no analysis of the degree to which the Predicativist standpoint is epistemically secure or elementary. Neither does Hilbert propose an alternative to Predicativism or explain where he thinks its philosophical underpinnings go wrong. Weyl's foundational program fails, in Hilbert's estimation, simply because "it blocks the path to analysis", because classical mathematics is not recoverable in it.

By the artificiality of a standpoint, then, Hilbert means that it is not native to mathematical practice. Whatever can be said for such a standpoint must in some way betray mathematical standards and therefore the mathematician is under no obligation to his science to take heed. The same year Bernays expressed the point as follows: "Thus we find ourselves in a great predicament: the most successful, most elegant, and most established modes of inference ought to be abandoned just because, from a specific standpoint, one has no grounds for them" ([1922b], pg. 218). In such a predicament, there is only one way to turn, as Hilbert memorably explains in his celebrated [1926] address to the Westphalian Mathematical Society, "no one, though he speak with the tongue of angels, could keep people from negating general statements, or from forming partial judgments, or from using *tertium non datur*" because these principles are the mathematician's fundamental resources and arguments against them simply are of no weight next to our compulsion to work with them and the achievements attainable by them.

In short, since "[t]he standpoints usually taken by mathematicians do not rest on the principle of constructivity at all, nor do they exhibit Weyl's circle" ([1922c], pg. 199), the Predicativist must entice us to jump ship, to opt for the skeptic's subtle philosophy over mathematical methodology. But if from the mathematical mode of thinking nothing seems out of line, then the skeptic's call is just so much rhetorical sport and we are destined as a matter of fact to ignore it in favor of the clarity and naturalness of our science. One is reminded of Descartes' reaction at the end of his First Meditation to his own skeptical tendencies:

> But this undertaking is arduous, and a certain laziness brings me back to my customary way of living. . . . I fall back of my own accord into my old opinions, and dread being awakened, lest the toilsome wakefulness which follows upon a peaceful rest must be spent thenceforward not in the light but among the inextricable shadows . . .

For Hilbert, though, the toilsome wakefulness of skeptical foundationalism is not a challenge, from which the promise of new levels of mathematical certitude awaits all who overcome their intellectual laziness. It is an unwelcome interruption of the mathematical dream that puts one in the contrived and unhelpful state of puzzlement and ineptitude where before all was in perfect order.

Thus is Hilbert's naturalistic epistemology. The security of a way of knowing is born out, not in its responsibility to first principles, but in its successful functioning. The successful functioning of a science, moreover, is determined by a variety of factors–freedom from contradiction is but one of them–including ease of use, range of application, elegance, and amount of information (or systemization of the world) thereby attainable. For Hilbert mathematics is the most completely secure of our sciences because of its unmatched success, and this unambiguous certainty is all the justification that any way of knowing should call for. If from some external perspective mathematics appears to be in jeopardy, this is evidence *against the tenability of that perspective*, not in favor of a skepticism about mathematics. Hilbert articulates this epistemic stance in a succinct summation of his analysis of Weyl's Predicativism:

> Mathematicians have pursued to the uttermost the modes of inference that rest on the concept of sets of numbers, and not even the shadow of an inconsistency has appeared. If Weyl here sees an "inner instability of the foundations on which the empire is constructed," and if he worries about "the impending dissolution of the commonwealth of analysis," then he is seeing ghosts. Rather, despite the application of the boldest and most manifold combinations of the subtlest techniques, a *complete security of inference and a clear unanimity of results* reigns in analysis. We are therefore *justified* in assuming those axioms which are the basis of this security and agreement; *to dispute this justification would mean to take away in advance from all science the possibility of its functioning . . .* ([1922c], pg. 200 italics added)

3 Mathematical autonomy

If Hilbert recognized no *Grundlagenkrise* in mathematics, what, after all, was the point of his elaborate foundational program? Why endeavor so assiduously to demonstrate what one takes to be the unshakable starting point of all inquiry–the consistency of one's own methods?

For a certain type of naturalist, these questions may have no satisfactory answer, and so to answer them one must further sharpen one's understanding of Hilbert's epistemology.

The contrast here is with the anti-foundationalism of Wittgenstein's remarks *On Certainty* and the naturalistic epistemology depicted there–the view according to which "at the foundation of well-founded belief lies belief that is not founded" (253). Wittgenstein suggests that, for anyone, *some* system of belief must be completely immune from doubt because it is the system from which the person weighs the truth or falsity of claims, the ground on which he or she stands in order even to express doubt: "I have a world picture. Is it true or false? Above all it is the substratum of all my enquiring and asserting" (162). "But I did not get my picture of the world by satisfying myself of its correctness; nor do I have it because I am satisfied of its correctness. No: it is the inherited background against which I distinguish between true and false" (94).

If one could simply take mathematical methods to be constitutive of argumentative grounds, then it is clear both why the skepticism of certain foundational programs would not appear threatening and why doubt about mathematical methods would not in general arise. Should the skeptic point out that justification for certain principles, say mathematical induction, was lacking, one could only wonder at the question. Whatever justification there could be would have to be more certain than the principle itself in order to gain any ground, and that is unthinkable: Wittgenstein's remarks that "[his] not having been on the moon is as sure a thing ... as any grounds [he] could give for it" (111) and that "[his] having two hands is, in normal circumstances, as certain as anything that [he] could produce in evidence for it" (250) would apply just as well to mathematical induction.

Similarly, so long as one continues to work within the ordinary mathematical framework, the kind of doubt that is directed *at* that framework would be impossible:

> All testing, all confirmation and disconfirmation of a hypothesis takes place already within a system. And this system is not a more or less arbitrary and doubtful point of departure for all our arguments: no, it belongs to the essence of what we call an argument. The system is not so much the point of departure, as the element in which arguments have their life. (105)

Because it wouldn't be couched within one's own argumentative standards, doubt directed at mathematical methods would be lifeless. Mathematical methods, then, just are never subject to serious doubt by virtue of their especial certitude.

To an extent this Wittgensteinian naturalism accords with Hilbert's position. For Hilbert objects to the foundational programs of Brouwer and Weyl simply because they are rooted on non-mathematical grounds from which ordinary mathematics appears to be in need of justification. That is reason enough for Hilbert to object to those programs and reject the grounds on which they are rooted. Hilbert recognizes no rite of arbitration in any standpoint from which the legitimacy of the mathematician's methods falls into question. The legitimacy of those methods cannot reasonably be questioned.

But a thoroughgoing Wittgensteinian attitude about mathematics precludes any need for any foundational program. No epistemic gains are available if mathematics already is "the element in which arguments have their life". Yet Hilbert offers a foundational program and promises from it epistemological gains. Hence his position cannot be in full accordance with the Wittgensteinian's.

Distinctive of Hilbert's position is that mathematical methods are *neither* subject to scrutiny from any non-mathematical standpoint *nor* constitutive of our argumentative standards. Since mathematics is not subject to scrutiny, there is no foundational crisis to overcome. But the reason they are not subject to scrutiny is not the Wittgensteinian reason that such scrutiny would be lifeless or senseless by virtue of all meaningful or gainful scrutiny taking place already within the mathematical framework. For Hilbert not even mathematics plays the role of first principles. Certainly, at least, mathematics' high mark of certitude is not due to it playing this role.

This distinction between Hilbertian and Wittgensteinian naturalism is most evident in Hilbert's claim that mathematical methods *are* justified, in contrast with the Wittgensteinian principle that our epistemic bedrock is not and cannot be justified. According to Hilbert mathematics is justified, though not on any philosophical grounds: Mathematics is justified in application, through a history of successful achievement, through the naturalness with which its methods come to us, through its broad range of applicability, etc. This justification earns for mathematics a position of unavailability, but it does not earn for it the position of epistemic bedrock.

Indeed when Hilbert claims that the mathematical methods "which Aristotle taught and which men have used ever since they began to think" cannot be challenged because "no one . . . could keep people from negating general statements, or from forming partial judgments, or from using *tertium non datur*" he says also that these methods "do not hold" in all the contexts in which mathematicians use them ([1926] pg. 219). That the methods "do not hold" is precisely the skeptical challenge, and Hilbert's response again is extraordinary. He refutes the skeptic, not by disagreeing with the content of the challenge–that mathematical methods do not hold. He *agrees* with the skeptic and yet *still* declares the challenge inappropriate. The mathematician's duty is not to find laws that "hold" but ones that get justified in practice. Once they are so justified, not even the concession that they betray the most fundamental philosophical principles amounts to foundational crisis.

On this account, though, skeptical foundationalism has still a foothold. Hilbert feels that the justification of a measure arises solely from the measure's demonstrated success, and on that account his confidence in mathematics is as high as it could be. But unlike the Wittgensteinian naturalist who can perpetually check the skeptic, Hilbert has no recourse to the meaninglessness of skepticism about mathematics, he has only his particularly naturalistic grounds of justification against theirs. In the end, his position seems to him unshakable, because he can see how the skeptic's path leads to the death of all science and that mathematicians are unlikely to follow down it. But against the persistent skeptic this is not an argument of the sort that the Wittgensteinian could offer. For the skeptic simply could call for the death of all science in deference to his philosophical scruples. Hence, even as it cannot mount a viable reformation, skepticism may endure. Hilbert would like the legitimacy of mathematics to speak for itself, but the skeptic has him appealing to certain standards of justification to defend his science.

This is the setting of Hilbert's program. Though the skeptic fails to make a case against the consistency and reliability of mathematics, his attack does enough damage if it exposes a dependence of the veracity of mathematical methodology on *any* justificatory standards. For even if questioning those standards is unreasonable, even if, that is, to do so "would mean to take away in advance from all science the possibility of its functioning," the status of mathematics is diminished if its veracity is shown to rest, through however circuitous a route, on non-mathematical grounds.

This explains Hilbert's at first puzzling approach to foundational research, his endeavor to prove what he emphasizes is in no way in doubt: the consistency of mathematics. For the epistemological gain to be earned with such a proof isn't the knowledge that mathematics is consistent, it is the knowledge that mathematics need not appeal to anything non-mathematical in its own defense and that its truths are in that sense objective, "ultimate" truths:

> Accordingly, a satisfactory conclusion to the research into these foundations can only ever be attained by the solution of the [mathematical] problem of the consistency of the axioms of analysis. If we can produce such a proof, then we can say that mathematical statements are in fact incontestable and ultimate truths–a piece of knowledge that (also because of its general philosophical character) is of the greatest significance for us. (Hilbert [1922c], pg. 202)

Thus it is evident why Hilbert takes his program to probe deeper than other foundational efforts. His adversaries' programs all treat as open questions whether and how much mathematics is consistent, and they aim to settle these questions by setting some portion of mathematics securely on some principles of first philosophy. By contrast, Hilbert *begins* his foundational research assuming that all orthodox mathematics is consistent and asks instead whether mathematics is autonomous in the sense that its consistency, and therefore its legitimacy, depends ultimately on *no* principles of first philosophy. Success would "regain for mathematics the old reputation or incontestable truth" by making its truths welcome to everyone *regardless of* the philosophical principles they endorse.

Since Hilbert's "consistency question" ultimately is the challenge of taking mathematics out of any philosophically informed setting, his estimation of others' attempts at solving the question is low:

> The importance of our question about the consistency of the axioms is well recognized by philosophers, but in [the philosophical literature] I do not find anywhere a clear demand for the solution of the problem in the mathematical sense. ([1922c], pg. 201)

Without settling the problem mathematically, it is unclear what a "solution" to the problem even could gain, since, after all, Hilbert has no doubts about mathematics' consistency. Thus Bernays explains that

> [t]he great advantage of Hilbert's procedure rests precisely on the fact that the problems and difficulties that present themselves in the grounding of mathematics are transformed from the epistemologico-philosophical domain into the domain of what is properly mathematical. ([1922b], pg. 222)

This is such a great advantage, he says elsewhere, because "mathematics [thereby] takes over the role of that discipline which was earlier called *mathematical natural philosophy*" ([1931a], pg. 236). With mathematics itself in that role, Hilbert's defense can achieve a kind of unambiguity that mathematics deserves. Again Hilbert's words in his introduction to the Hamburg lectures are key: "in mathematical matters . . . it should not be possible for half-truths or truths of fundamentally different sorts to exist." In particular it should not be possible to settle mathematical matters in ways that essentially favor any particular set of philosophical assumptions. Thus Hilbert's program embodies philosophical subtlety, for the gains he envisions are philosophical, but the program's realization depends on turning philosophical inquiry over to purely mathematical methods.

4 Formalism and finitism

The ingenuity behind Hilbert's "formalism" and "finitism" lies in the role that these theses play in securing the transfer of philosophical inquiry into the mathematical domain. Each thesis amounts to a methodological guideline designed to ensure that the foundational program delivers the kind of unambiguous mathematical self-sufficiency described in the last section. That is, should the foundational program betray either principle, the program would fail, but Hilbert argues that if the consistency result can be proven in accordance with these principles, one will have shown that mathematics is beholden to no philosophical framework.

Thus it will not do to interpret "formalism" as the doctrine that mathematics is meaningless or that its subject matter consists just of formal symbols and rules of formula manipulation, as the term is often used in current philosophical discussions. Neither is it correct to understand Hilbert's "finitism" as the doctrine that only decidable methods are veracious and that only finitary propositions are contentful, as has been alleged in various forms since Kronecker. Hilbert explicitly wants to avoid appealing to any doctrines about the subject matter or ontology of mathematics. Indeed, as argued above, if research constrained by theses such as these proved inadequate to lay a foundation for all ordinary mathematics, then Hilbert would abandon those theses before yielding any of the mathematics. And if, alternatively, such a program succeeded, the resulting "defense" of mathematics would have the mathematical edifice resting on these metaphysical principles, which Hilbert hardly would consider an improvement over the defense already available in terms of mathematics' success in application. Both Hilbert's "formalism" and his "finitism", instead of being philosophical perspectives from which he intends to justify mathematical techniques, are methodological constraints *forced* by the type of mathematical self-reliance that he intends to demonstrate.

Following Mancosu ([1998], pg. 163)[2] let us note first that Hilbert nowhere describes himself or his outlook as "formalist". The label seems to originate instead in the polemic from representatives of other foundational schools intended to draw into question the legitimacy of "Hilbert's philosophical perspective". Aside from the notorious correspondence between Hilbert and Frege on the foundations of geometry, which in any case predates the foundational perspective that characterizes Hilbert's program[3], the most often cited passage in support of attributing a "formalist" philosophy to Hilbert is the following: "The solid philosophical attitude that I think is required for the grounding of pure mathematics ... is this: *In the beginning was the sign*" ([1922c], pg. 202). It will become clear, however, that even this proclamation must be understood as a description of the philosophical attitude that Hilbert feels one must adopt in order properly to engage in the foundational pursuit of mathematical autonomy, and not as a description of the correct theory concerning the nature or origin of mathematics.

Bernays describes the position explicitly in his reply to Aloys Müller's criticism of "Hilbert's conception of numbers as signs":

> Hilbert's theory does not exclude the possibility of a philosophical attitude that conceives of the numbers as existing, nonsensical objects [as Müller would have them be]. ... Nevertheless the aim of Hilbert's theory is to make such an attitude dispensable for the foundations of the exact sciences. ([1923], pg. 226)

Thus if formalism is supposed to be a type of nominalist or anti-realist metaphysical doctrine, then such cannot be consistent with this description of Hilbert's program. According to Bernays, success for Hilbert's program would not weigh

[2] Mancosu attributes the label primarily to Brouwer's [1928].

[3] Detlefsen [1993] distinguishes developmental stages leading to Hilbert's invention of proof theory. Specifically he separates Hilbert's early remarks about the formal nature of axiomatics and the "hypothetical" role that axiomatic systems generally played from the more thoroughgoingly formalist perspective of Hilbert's foundational investigations in the 1920's according to which even the logical symbols are treated as meaningless.

in on the question of whether numbers exist, or whether alternatively mathematics consists solely in meaningless signs. Adopting the attitude that "in the beginning was the sign" serves rather to separate all answers to such questions from the foundational program.

Similarly Hilbert's call for a restriction to purely finitary or constructive techniques for the sake of foundational research is a strategy needed in order to secure mathematical self-reliance. Since Hilbert harbors no doubts about the reliability of any mathematical techniques, in a sense all of them are at his disposal. But on pain of circularity, some restriction is due for the purposes of evaluating and justifying the techniques themselves. In his own words:

> We therefore see that, if we wish to give a *rigorous* grounding of mathematics, we are not entitled to adopt as logically unproblematic the usual modes of inference that we find in analysis. Rather, our task is precisely to discover *why* ... we always obtain correct results from the application of transfinite modes of inference of the sort that occur in analysis and set theory. (Hilbert [1923], pg. 1140 italics added)

Hence for programmatic purposes those same modes of inference that we seek to evaluate cannot figure in to the evaluation. For in case they should, the original question as to why a result is correct could be put to the result *of the evaluation*.

To step out of this circle, any restriction of techniques would do. The resulting justification just will only ever be relative to the techniques that are required. One must begin somewhere, however, so the relativity of the evaluation *per se* is not a complaint against it. The foundational task, as Hilbert saw it, was to step back far enough that only techniques that everyone recognized as mathematically acceptable were used, *while at the same time retaining resources sufficient to carry out the evaluation.* From Hilbert's point of view, everyone's demands weigh in equally on this matter, because their several perspectives are constitutive of the skepticism that we wants to ward off. The appeal to finitary or constructive techniques, therefore, is not so much a recourse to foundations that Hilbert would argue were epistemologically secure, as a measure to ensure that all mathematics gets justified wholly within mathematics. Again, Bernays articulates the position:

> One thus arrives at the attempt of a purely constructive development of arithmetic. And indeed the goal for mathematical thought is a very tempting one: Pure mathematics ought to construct its own edifice and not be dependent on the assumption of a certain system of things. ([1922b], pg. 217) ...
>
> For Hilbert in no way wants to abandon the constructive tendency that aims at the self-reliance of mathematics. ([1922b], pg. 219)

Thus our development supports the interpretation of Howard Stein:

> I think it is unfortunate that Hilbert, in his later foundational period, insisted on the formulation that ordinary mathematics is "meaningless" and that only finitary mathematics has "meaning". Hilbert certainly never abandoned the view that mathematics is an organon for the sciences: he states this view very strongly in the last paper reprinted in his *Gesammelte Abhandlugen*, called "Naturerkennen und Logik"; and he surely did not think that physics is meaningless, or its discourse a play with "blind" symbols. His point is, I think, this rather: that the mathematical *logos* has no responsibility to any imposed *standard* of meaning: not to Kantian or Brouwerian "intuition," not to finite or effective decidability, not to anyone's metaphysical standards for "ontology" .... ([1988], pgs. 254-255)

The question remains as to *how*, according to Hilbert, the methodological constraints of formalism and finitism are forced on one by the demands of an earnest attempt at establishing mathematical autonomy. Answering this question brings out how under the naturalistic conception of Hilbert's program there is a lively interaction between the two principles.

The general method underlying Hilbert's program is familiar. One first fully formalizes a branch of mathematics as an axiomatic system so that one is dealing, not with mathematical statements and inferences, but with formulas and admissible sequences of formulas. Already one's subject matter has been "formalized", but the next step brings out the particularly "formalist" nature of the method: When one sets out to study this axiomatic system, and specifically when one undertakes to demonstrate its consistency, one must suspend throughout the investigation the original "meanings" of the statements that have been formalized. Bernays describes this suspension of interpretation as necessary:

> Accordingly, in Hilbert's theory we have to distinguish sharply between the formal image of the arithmetical statements and proofs as *object* of the theory, on the one hand, and the contentual thought about this formalism, as *content* of the theory, on the other hand. The formalization is done in such a way that formulas take the place of contentual mathematical statements, and a sequence of formulas, following each other according to certain rules, takes the place of an inference. And indeed no meaning is attached to the formulas; the formula does not count as the expression of a thought .... (Bernays [1922b], pg. 219)

That is, the branch of mathematics one investigates becomes for the sake of the investigation a purely formal object. One "has to" precede in this way, as Bernays says, in order that the branch of mathematics fit entirely under the lens of mathematical investigation. For if, for example in one's demonstration that a branch of mathematics is consistent, one falls back to the pre-theoretic interpretation of formulas as statements, then the semantic assumptions behind that interpretation will have polluted the would-be purely mathematical achievement. In the Hamburg lectures, Hilbert describes this transition to proof theory in similarly normative terms: "To reach our goal, we *must* make the proofs as such the object of our investigation; we are thus *compelled* to a sort of 'proof theory' which studies operations with the proofs themselves" ([1922c], pg. 208 italics added).

As an example of the procedural guidelines that emerge from the formalist constraint, Bernays describes how the program's ultimate goal is shaped by it:

> What in particular emerges from this consideration about the requirement and the purpose of the consistency proof is that this proof is only a matter of seeing the consistency of arithmetic theory in the literal sense of the word, that is, *the impossibility of its immanent refutation*. ([1931a], pg. 260)

This is in contrast to the at the time more familiar means to establishing consistency, which was to determine whether "the conditions formulated in the axioms can at all be satisfied by means of a system of objects with certain properties that are related to them" ([1931a], pg. 237). This route to the consistency of arithmetic is easily established through reference to the standard model of natural numbers. But the goal of mathematical autonomy demands a purely syntactic demonstration. For on the one hand the consistency proof by way of reference to the standard model rests on the semantic assumptions underpinning one's grasp of that model and its accordance with the arithmetic axioms. And on the other hand even if one's model theory were fully mathematized so that the demonstration of consistency in this way became rigorously mathematical, the amount of mathematics involved would of course far extend the arithmetic theory under investigation, resulting again in justificatory circularity.

Meanwhile, since if the evaluation is to be genuinely mathematical then some mathematics must be assumed through the course of one's proof theoretic investigations, this base mathematics need not, and in fact cannot, be stripped of its meaning. One must use it and work within it in order to reason about the formal axiomatization that one is studying, thereby attaining results relative to the reliability of that base mathematics:

> [I]n addition to this proper mathematics, there appears a mathematics that is to some extent new, a *metamathematics* which serves to safeguard it by protecting it from the terror of unnecessary prohibitions as well as from the difficulty of paradoxes. In this metamathematics–in contrast to the purely formal modes of inference in mathematics proper–we apply contentual inference; in particular, to the proof of the consistency of the axioms. (Hilbert [1922c], pg. 212)

Hence Hilbert's famous two-tiered approach to foundational studies. One distinguishes the formal object of investigation and the contentual base in which the investigation is carried out:

> In this way the contentual thoughts (which of course we can never wholly do without or eliminate) are removed elsewhere–to a higher

> plane, as it were; and at the same time it becomes possible to draw a systematic distinction in mathematics between formulae and formal proofs on the one hand, and the contentual ideas on the other. ([1922c], pg. 204)

The formalist nature of Hilbert's *Beweistheorie* therefore arises from the need to eliminate philosophical assumptions from one's metamathematical investigations. With the semantic assumptions stripped away, what remains for one's scrutiny are only "signs", uninterpreted formulas. The remaining question is where to delineate the contentual base of metamathematics, that bit of mathematics that is spared strict formalization. One point of consideration is that the contentual base be weaker than the "proper mathematics" in order that the justification not exhibit circularity. Another is that, ideally, this base should be weak enough not to be the target of the skepticism from the rival foundationalist schools. In addition to these, a third constraint now arises, which is that the contentual base theory should be *strong* enough to allow one to reason within it effectively about signs and sequences of signs. At first it is not evident whether all three conditions can be met. That is, there is a question whether any mathematics could exhibit the deductive strength needed for robust investigation of formulas as such without extending in strength the arithmetic theory Hilbert intends to defend. And even if this circularity is avoidable, the degree of achievement is only partial if the metamathematics needed still is strong enough to be in need of its own defense. Hilbert claims that something called finitary mathematics meets all three criteria.

Bernays gives the clearest statement of how the contentual base theory is determined by the demands of formalism:

> Now the only question still remaining concerns the means by which this proof should be carried out. In principle this question is already decided. For our whole problem originates from the demand of taking only the concretely intuitive as a basis for mathematical considerations. Thus the matter is simply to realize which tools are at our disposal in the context of the concrete-intuitive mode of reflection. ([1922b], pg. 221)

Exactly what Bernays means by "concretely intuitive" is the subject of considerable debate, both in terms of the philosophical nature of this mode of reflection[4] and in terms of the answer, in the mathematical sense, to his question as to which tools are available in this reflection[5]. When he speaks of "the demand of taking only the concretely intuitive as a basis for mathematical considerations," however, Bernays can only mean the demand imposed on foundational studies by

[4] i. e. whether this mode corresponds with an empirical faculty or, as Hilbert and Bernays suggest in some later writings with a Kantian intermediate faculty between experience and thought

[5] Tait [1981] argues that Hilbert's finitary mathematics is Primitive Recursive Arithmetic. Others, for example Volker Halbach in private conversation, have argued that it is hard to imagine Hilbert rejecting as foundationally significant Gentzen's arithmetic consistency proof, had it been available, on grounds that it was not finitary.

the fact that these studies must proceed uninfluenced by any philosophical considerations. That is, Bernays is referring to the demand of investigating proper mathematics purely formally. Hence the proper delineation of metamathematics is to be determined by isolating that minimal fragment of proper mathematics sufficient to investigate purely syntactic aspects of formal, axiomatic theories, and not by unpacking the exact nature of "concretely intuitive" reflection in the philosophical sense.

On the other hand, Hilbert does want to say something about the philosophical nature of finitary mathematics, specifically that it is unassailable on skeptical grounds. Thus he argues that not only is this amount of mathematics necessary for metatheoretic evaluation because of the requirement that one be able to reason effectively about formulas, but that also it is sufficiently minimal to be beyond criticism in something more like the Wittgensteinian sense:

> If logical inference is to be certain, then these objects must be capable of being completely surveyed in all their parts, and their presentation, their difference, their succession (like the objects themselves) must exist for us immediately, intuitively, as something that cannot be reduced to something else. (Hilbert [1922c], pg. 202)

That is, since the subject matter of metamathematics is purely formal, it is fully concrete, finite, survey-able, and immediate. Thus metamathematical reasoning need *only* deal with the recognition of and distinction between concrete, immediately present objects and need *not* recapture any interpretation of these objects. Hilbert claims that this is a kind of bedrock of reasoning, irreducible and consequently unchallengeable.

Hilbert's claim that there need be no defense for finitary mathematics is of course controversial–the majority of the controversy due to his unclarity with respect to what exactly finitary mathematics amounted to. A central example occurs with the reasoning involved in a demonstration of consistency. For there is something characteristically finitary about the verification that a single proof involves no contradiction as well as the verification that if any proof of a particular form is free of contradiction then so is another attainable from it through a constructive transformation. But one must use some principle of induction in order to reason from these points to the consistency claim that no proof contains a contradiction. And this principle of induction is not obviously finitary in any philosophically uncontroversial sense. This very point was the crux of the debate between Oskar Becker and Hilbert with respect to the nature of induction admissible in metamathematical reasoning[6].

What should strike one as even more controversial about Hilbert's claim, however, given his steadfast commitment to a naturalistic view of mathematics, is the very fact that he wants at this point to appeal to the epistemic status of

[6]See Mancosu [1998], pgs. 165-67. Importantly, Becker focuses his criticism on the need for Hilbert to move beyond the purely finite in his metamathematics, while Hilbert replies only that metamathematical induction is different and weaker than the "full induction axiom" of proper mathematics.

finitary reasoning. He cannot, after all, simply be hoping to secure mathematics on a foundation of the "concrete intuitive mode of reflection". Despite all his remarks in favor of the solidity of finitary reasoning, such a conception would just reduce his program to a fraudulent attempt at *ignotum per ignotius*–explaining what is unknown by what is more unknown–and Hilbert's principal aim is to avoid precisely this sort of foundationalism.

Rather, Hilbert's appeal to the especially basic status of the "concrete intuitive" is better understood as a strategic advertisement for his program. He is confident that he has found a way to provide a purely mathematical evaluation of mathematics itself, the essential device in doing so being the formalist perspective in proof theoretic investigations. And he sees that the evaluation can proceed thus in a philosophically gainful way, since the metamathematics can thereby be constrained to principles weaker than those comprising the formalized theory of mathematics proper. Already, then, the program amounts to a significant achievement. But this accomplishment might be lost on the broader philosophical and mathematical communities if the techniques involved cannot be shown to be everywhere sound, not by Hilbert's standards but by theirs. It is therefore something of a happy accident if in fact the metamathematics falls within a "finitary" rubric acceptable even to the most skeptical Predicativists, Intuitionists, and other mathematical cautionaries. If it does not, then still the degree of self-sufficiency that is attainable makes headway at demonstrating the needlessness of philosophical grounding for mathematics.

So just as Hilbert's formalism is a procedural consequence of the demands of his unique epistemological goals, his finitism is necessitated by that formalism. That is, so long as one's metamathematical evaluation steers away from any principles other than those needed to reason directly about the strictly formalized axiomatization of ordinary mathematics, then one is on track to uncover a purely mathematical appraisal of mathematics itself. Whether or not the "finitism" inherent in that course can truly be said to be finitary in every philosophically informed understanding of the term, and whether or not as a consequence it seems as epistemically innocuous as Hilbert describes it as being are at most secondary considerations.

5 Conclusion

In another report from 1922 entitled "Hilbert's significance for the philosophy of mathematics" where he focuses specifically on the innovation of rigid axiomatization, Bernays discusses the nature of Hilbert's earlier achievements in the foundations of geometry:

> [A] new sort of mathematical speculation [had] opened up by means of which one could consider the geometrical axioms from a higher standpoint. It immediately became apparent, however, that this mode of consideration had nothing to do with the question of the epistemic character of the axioms, which had, after all, formerly been

> considered as the only significant feature of the axiomatic method. Accordingly, the necessity of a clear separation between the mathematical and the epistemological problems of axiomatics ensued. ([1922a], pgs. 191-92)

We have seen that a very similar separation between mathematics and epistemology characterizes the foundational innovations that Hilbert was introducing that very year in his pursuit of arithmetic consistency, specifically by deepening foundational research "not so much to fortify individual mathematical theories as because, in my opinion, all previous investigations into the foundations of mathematics fail to show us a way of formulating the questions concerning foundations so that an unambiguous answer must result".

In the next sentence Bernays identifies the origin of this mathematical, as opposed to epistemological, attitude in foundational studies to Felix Klein: "The demand for such a separation of the problems had already been stated with full rigor by Klein in his Erlangen Programme". Here is Klein's [1908] description of his conception of the nature of foundational research:

> Mathematics has grown like a tree, which does not start at its tiniest rootlets and grow merely upward, but rather sends its roots deeper and deeper at the same time and rate as its branches and leaves are spreading upwards. Just so–if we may drop the figure of speech–mathematics began its development from a certain standpoint corresponding to normal human understanding and has progressed, from that point, according to the demands of science itself and of the then prevailing interests, now in the one direction toward new knowledge, now in the other through the study of fundamental principles.

It would be incorrect to infer from this image, however, that Hilbert's foundational pursuits were not philosophically motivated, that his naturalistic conception of the formal sciences amounted simply to a disinterested rejection of epistemological concerns about mathematics. Bernays' point, rather, is this: that Hilbert's efforts in axiomatization and studies of fundamental principles are not efforts directed at uncovering epistemic foundations *in the axioms*; the legitimacy of an axiomatization is earned purely mathematically, through its ability to realize mathematically prescribed goals, and not by way of the *epistemic character* of the axioms. However, the mathematical goals put to any particular foundational program can very well arise from epistemological concerns. Hilbert's own was the desire to demonstrate that mathematics was a fully self-supporting science, and that mathematical certainty therefore enjoyed an epistemic status privileged beyond that of any way of knowing that rested on a philosophical conception of justificatory grounds.

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