Curtis Franks, *The Autonomy of Mathematical Knowledge: Hilbert's program revisited.* Cambridge University Press, 2009

Reviewed by Solomon Feferman

According to Curtis Franks' preface, this book bundles his historical, philosophical and logical research to center around what he thinks are "the most important and most overlooked aspects of Hilbert's program … a glaring oversight of one truly unique aspect of Hilbert's thought," namely that "questions about mathematics that arise in philosophical reflection—questions about how and why its methods work—might best be addressed mathematically… Hilbert's program was primarily an effort to demonstrate that." The standard "well-rehearsed" story for these oversights is said to be that Hilbert's philosophical vision was dashed by Gödel's incompleteness theorems. But Franks argues to the contrary that Gödel's remarkable early contributions to metamathematics instead drew "significant attention to the then fledgling discipline," a field that has since proved to be exceptionally productive scientifically (even through the work of such logicians as Tarski, who mocked Hilbert's program). One may well ask how the author's effort to put this positive face on the patent failure of Hilbert's program can possibly succeed in showing that mathematical knowledge is autonomous, that mathematics has only to look to itself for its proper foundations. Let us see.

The first two chapters of Franks' elegantly written book (whose text, unfortunately, is marred by numerous typos) are devoted to the development of Hilbert's "new science" within his overall conception of mathematics and his philosophical naturalism.[1] In particular, much of Ch. 2 draws on the Hamburg lectures [Hilbert 1922] in which Hilbert threw down the gauntlet against his primary opponents, the foundationalist skeptics Kronecker, Brouwer and Weyl, and laid out the plans and means

[1] Franks takes Hilbert's famous 1899 work on the foundations of geometry as a point of departure. But no mention is made at all in the book or its references of the evolution of Hilbert's thought on the foundations of mathematics from the famous lecture in 1900 on problems of mathematics through various papers prior to 1922, especially those of 1900, 1905 and 1918. For such a survey cf., e.g., the introduction to Part III of Mancosu [1998] and/or Sieg [1999].

for his program.[2] Hilbert there rightly dismissed as a failure the efforts of Frege and Russell to reduce all of mathematics to logic, while crediting them for their elucidation of the quantifier logics of first and higher order. By contrast, he said that the foundationalists would seriously emasculate mathematics on the grounds of extra-mathematical philosophical scruples, Brouwer by rejection in general of the *tertium non datur* and of the completed infinite, and Weyl by the rejection of a supposed *circulus vitiosus* in the use of impredicative definitions in analysis via the lub principle for the real numbers. According to Franks:

> [It is] a continuous scheme in [Hilbert's] Hamburg lectures that one has every reason to believe in the consistency of mathematics…because everything in our mathematical experience speaks for its consistency." (pp. 29-30)

And, further on:

> According to Hilbert mathematics is justified, though not on any philosophical grounds. Mathematics is justified in application, through a history of successful achievement … This justification earns for mathematics a position of unassailability, but it does not earn for it the position of epistemic bedrock. (p. 44)

But both of these overstate what one finds in the text of the Hamburg lectures, whose primary target to begin with is the apostate Weyl:

> [T]here is scarcely any subject, either within or outside the mathematical sciences, that has been so thoroughly studied as real analysis. 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" …then he is seeing ghosts. Rather, despite the application of the

[2] However, significant features of the program would not be delineated until later in various of Hilbert's publications, in particular the essential distinction between formal induction and contentual induction for finitist reasoning. That is discussed by Franks in his Ch. 3.4, vis à vis Poincaré's argument that there is a circularity in Hilbert's program to prove the consistency of arithmetic.

> boldest and most manifold combinations of the subtlest techniques, a complete security of inference and a clear unanimity of results reigns in analysis. ([Hilbert 1922], p. 200).[3]

As Hilbert explains, the reference to sets of numbers has to do with Dedekind's construction of the continuum in terms of cuts (or sections) in the rational numbers, thus leading to a reduction of the concept of real number to that of set of natural numbers.

> *The concept of set in the most general sense is not admissible without qualification*, but this does not in any way mean that there is anything amiss with the concept of a set of integers. And the paradoxes of set theory cannot be regarded as proving that the concept of a set of integers leads to contradictions. On the contrary, all our mathematical experience speaks for the correctness and consistency of this concept. (op. cit., p. 199, italics mine)

Hilbert goes on to say that it is not necessary to rest on experience or intuition, since one can ground analysis in an axiomatic theory of real numbers, and "the only thing that remains to be decided is whether a system of the requisite sort is thinkable, that is whether the axioms do not, say, lead to a contradiction." (ibid.) But he apparently acknowledges that the general concept of set is problematic considered on its own merits:

> The goal of finding a secure foundation of mathematics is also my own. I should like to regain for mathematics the old reputation for incontestable truth, which it appears to have lost as a result of the paradoxes of set theory; but I believe that this can be done while fully preserving its accomplishments. The method that I follow is none other than the axiomatic. (op. cit., p. 200)

In the fourth section of Ch. 2, Franks expounds his take on the two prime features of Hilbert's consistency program, "formalism" and "finitism," in order to demonstrate that "mathematics need not appeal to anything non-mathematical in its own defense, and that its truths are in that sense objective, 'ultimate' truths." (p. 45)

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[3] Page references to Hilbert [1922] in the following are to the English translation in Mancosu [1998].

> 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 reliance that he intends to demonstrate. (p. 48)

Moreover, there must be some restriction on the mathematical means by which such problems are to be settled, otherwise one risks arguing in a circle. Does that dictate a restriction to finitist methods? According to Franks:

> To step out of the circle, *any* restriction of techniques would do. The resulting justification just will only ever be relative to the techniques that are required. …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 to carry out the evaluation. (p. 50, italics mine)

Of Hilbert's emphasis, from 1922 on, on finitism as the "contentual" mathematics of the "concretely intuitive," Franks merely says that this is "better understood as a strategic advertisement for his program." (p. 58) The many problems of what Hilbert meant by finitism (and what ought to be meant by it) and exactly what kind of reasoning would be admitted under it are brushed aside as follows:

> [S]o 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. (p. 58)

Franks gives no attention to the *prima facie* extra-mathematical features of concrete intuition that Hilbert uses continually to promote finitism. Moreover, he makes no mention of the distinction between the "real" statements of finitary mathematics and the remaining "ideal" statements of mathematics that Hilbert emphasized beginning with his well known 1925 lecture, "On the infinite." This distinction would seem to grant second class citizenship to most of mathematics on philosophical, rather than mathematical grounds. In emphasizing what he thinks are the "most overlooked" aspects of Hilbert's program, Franks conveniently ignores the commonly recognized aspects that may not favor the overriding theme of the autonomy of mathematical knowledge. All of this is fodder for Hilbert scholars, of which I am not one, so I will leave it at that.

I turn now to the three more technical chapters of Franks's book, whose contents in my view prove to be increasingly odd. Ch. 3, titled "Arithmetization", concerns the proper formulation of the consistency of a formal system T as a mathematical problem. Franks insists that in order to be so, it must be expressed in arithmetical terms, i.e. as a problem about the natural numbers. The reason is that:

> [W]e look at techniques for the arithmetization of metatheory as the essential methodological contribution to Hilbert's program. It is through arithmetization that questions about a system of mathematics and its methods can be put to that system on its own terms, so that mathematics can "speak for itself." (p. 64)

He faults Hilbert and Bernays for defining a formal theory *T* to be consistent if no formally correct derivation in *T* ends with $\perp$(falsity), since that is just an ordinary non-mathematical statement. According him later on, this presented an obstacle to Hilbert's program, since "as a statement about syntax, a mathematical verification of the statement [of consistency] was *prima facie* not possible." (p. 102) In these terms, Gödel's method of arithmetization of syntax is regarded as "perhaps the most significant positive contribution to Hilbert's program" (ibid.). If that were the case, one would have expected the development of the program thenceforth to be carried out in some way in purely arithmetical terms. But in fact it had no such effect, as is seen from inspection of the contributions to that program, among others those of Ackermann, von Neumann, Herbrand and of course the two volumes of Hilbert and Bernays' *Grundlagen der Mathematik* [1934, 1939]; the only use of arithmetization in the latter is in the exposition in Vol. II of Gödel's incompleteness theorems and of an arithmetized version of his completeness theorem. Rather, one must say that one of the achievements of the program from the beginning is that it opened up formal syntax as the basis for a precise mathematical treatment of the properties of formal systems such as consistency, completeness, independence, decidability, not to say its use by Tarski in mathematically defining notions of truth and definability. And besides being treatable in arithmetic as shown by Gödel, that could be done even more directly in concatenation theory and set theory. Already in 1922, Hilbert had written:

> [W]e need to have a strict formalization of the entire mathematical theory, inclusive of its proofs, so that—following the example of the logical calculus—the mathematical inferences and definitions become a formal part of the edifice of mathematics. The axioms, formulae, and proofs that make up this formal edifice are precisely what the number signs were in the construction of elementary number theory…; and with them alone, as with the number-signs in number theory, contentual thought takes place—i.e., only with them is actual thought practiced. (Hilbert [1922], p. 204).

While Franks stresses the supposed good news for Hilbert's program of Gödel's method of arithmetization, he is casually dismissive of the negative significance of the second incompleteness theorem for Hilbert's program.[4] He says that as ordinarily understood, the theorem shows that "the only way to prove the consistency of a mathematical system is to use mathematical techniques that extend those of the system itself. Therefore any mathematical defense of mathematics is circular in the sense that Hilbert hoped to show it need not be—hardly good news to a Hilbertian." (p. 65) But this ignores the kind of consistency proof of *PA* provided by Gentzen, which is non-circular in that it uses a principle not provable in *PA* (transfinite induction up to $\varepsilon_0$) but otherwise uses only a very restricted part of *PA*. In any case, according to Franks, Gödel's theorem should not have been unduly disturbing to Hilbert, since he "would not conclude from a

[4] On p. 67, Franks seems curiously to credit the formulation of a general fixed point theorem in Carnap [1934] as the technical basis for Gödel's incompleteness theorems. Though Gödel [1934] acknowledged that theorem in his expository Princeton lectures, it is clear that Carnap would not have known how to establish it without the crucial specific fixed point construction provided in Gödel [1931].

system's inability to prove its own consistency that its consistency was dubious…[though] he would have welcomed a non-circular, purely mathematical demonstration of the consistency of analysis and even that he expected such a result." (ibid.) Nothing is said about Hilbert's later public dismissal of the significance of Gödel's second incompleteness theorem:

> This situation of the results that have been achieved thus far in proof theory at the same time points the direction for the further research with the end goal to establish as consistent all our usual methods of mathematics. With respect to this goal, I would like to emphasize the following: the view, which temporarily arose and which maintained that certain recent results of Gödel show that my proof theory can't be carried out, has been shown to be erroneous. In fact that result shows only that one must utilize the finitary standpoint in a sharper way for the farther reaching consistency proofs… (Hilbert, *Einführung* to [Hilbert and Bernays 1934])[5]

The second part of the chapter on arithmetization is devoted to Herbrand's work in logic and its relation to Hilbert's program. Besides logic, Herbrand was also deeply interested in algebra and number theory during his student days, and following the completion of his dissertation at the École Normale Supérieure in 1928 published important work in class-field theory, an advanced part of algebraic number theory. This came in the few years before his tragic death in a mountain-climbing accident in 1931 at the age of 23. The mathematicians at the ENS were hostile to the study of logic, viewing it as a part of philosophy. It had thus been difficult for Herbrand to assemble a thesis committee for his work in logic, despite the general recognition of his mathematical brilliance. Perhaps for these reasons, as well as his own view of the matter, Herbrand was at pains to disassociate himself from Hilbert's philosophical aims, in particular the idea that a mathematical concept exists once it has been shown how to formulate it axiomatically and prove its consistency. But he went along with the Hilbert school in his

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[5] As we know, Gödel himself briefly entertained the same possibility at the conclusion of his 1931 paper, but soon after changed his mind; cf. Feferman [2011].

emphatic restriction to finitist methods and reasoning (called by him, "intuitionistic"). Herbrand also faulted the development of Hilbert's program for its lack of rigor and even more for claiming proofs that turned out to be false. (Ironically, no mention is made by Franks of the substantial errors in Herbrand's thesis [1930] that needed real work years later to correct; cf. van Heijenoort [1967], pp. 525-526 and 567-581.)

Herbrand considered the *Entscheidungsproblem*—i.e. the question to determine whether or not a formula $A$ of the first order predicate calculus is *valid* in all domains—to be the fundamental problem of mathematical logic. A number of procedures to decide special cases of it (given by the logical form of $A$) had been settled by the time of Herbrand's work, but the general problem remained open until Church and Turing independently showed in the mid-1930s that it is effectively unsolvable. Herbrand extended the problem to the question, given an axiomatic theory $T$, to determine whether or not $A$ is a logical consequence of $T$. For $T$ consisting of closed formulas, this holds if and only if there is some finite sequence $T_1,\ldots,T_n$ of axioms of $T$ such that $\bigwedge_{j=1,..,n} T_j \rightarrow A$ is valid. In particular, the consistency problem falls under this extension of the *Entscheidungsproblem*, since $T$ is consistent if and only if there is *no* sequence $T_1,\ldots,T_n$ such that $\bigwedge_{j=1,..,n} T_j \rightarrow \bot$ is valid. The Fundamental Theorem in Herbrand's thesis [1930] provides a criterion for validity which, in view of the later Church-Turing result, is generally non-effective. Franks states a series of results related to Herbrand's Fundamental Theorem, one of which generalizes it; this closely follows an exposition due to Buss [1995]. Those familiar with Herbrand's theorem may not recognize it in Buss' more general version, while those not familiar with it will find this section (pp. 90ff) difficult to comprehend. What the theorem comes down to basically in the special case of a prenex formula $A$ with quantifier free matrix $B$ is that $A$ is valid if and only if there is a disjunction $B^*$ of substitution instances of $B$ that is tautologous; moreover one may deduce $A$ from $B^*$ by a series of universal and existential quantifier introductions. Such a $B^*$, if it exists, is called an *Herbrand proof* of $A$ (ignoring those quantifier introductions). Furthermore, if $T$ is a set of universal axioms, $T \vdash A$ iff there is such a $B^*$ for which $T \vdash B^*$. In both cases (not noted by Franks), there is in general no effective way to associate $B^*$ with $A$ except if we begin with a derivation of $A$ (or of $A$ from $T$) in the first order predicate calculus. The situation in which T is not universal is more complicated, since then the best we can say is that $T \vdash A$ iff there is some finite sequence $T_1,\ldots,T_n$ of axioms of $T$ for which there is an Herbrand proof $C^*$ of $\bigwedge_{j=1,..,n} T_j \rightarrow A$. And, finally, $T$ is consistent iff there is *no* Herbrand proof $C^*$ of $\bigwedge_{j=1,..,n} T_j \rightarrow \bot$ for *any* sequence $T_1,\ldots,T_n$ of axioms of $T$.

Herbrand grandly claimed that his Fundamental Theorem "permit[s] a reduction of the most general case of the *Entscheidungsproblem* to the remarkable form of a problem about number-theoretic functions that is but a generalization of the problem of the effective solution of diophantine equations. By means of this, all questions which can be raised in metamathematics are 'arithmetized'." ([Herbrand 1931], 275-276) This is justified to the extent that the test whether a given $B^*$ is tautologous comes down to an effective calculation via truth tables. But it is not justified as a reduction of each instance of the *Entscheidungsproblem* to arithmetic since as we have seen there is no effective method to tell whether or not there is such a $B^*$ associated with a given $A$ (resp., $C^*$ associated with given $A$ and $T$). While Franks takes Herbrand's contribution to the mathematization of Hilbert's program at face value, he rightly faults him for not actually producing a statement of the consistency of a formal theory $T$ in ordinary arithmetical terms. But he is not satisfied with Gödel's method of arithmetizing the statement of consistency of $T$ because it simply formalizes the informal definition of consistency given by Hilbert and Bernays.

Moving on from this, Franks is preoccupied in Ch. 4, "Intensionality", with the question—given a formal system $T$—as to which arithmetical statements $C$ may be regarded as correctly expressing the consistency of $T$ within $T$; this is crucial for the possible significance of Gödel's second incompleteness theorem for Hilbert's program. The chapter is mainly devoted to a critical evaluation of my own work on the method of arithmetization in Feferman [1960]; as we shall see, that is misconstrued in an essential way. The work there dealt with theories $T$ extending $PA$ whose underlying logic is fixed to be that of the classical first-order predicate calculus with Hilbert style logical axioms and rules of inference; the set $Ax_T$ of Gödel numbers of non-logical axioms of $T$ is assumed to be recursive, or at worst, recursively enumerable (r.e.).[6] The method of arithmetization defines within arithmetic various syntactic notions concerning *T*, beginning with a definition of $Ax_T$. In the case that that is infinite, there are many possible non-equivalent definitions $\tau(x)$ which hold just in case $x$ belongs to $Ax_T$. A minimal formal condition on $\tau$ is that it is numerically correct in the sense that it numerates or binumerates the set $Ax_T$ in *T*. Given $\tau$ and the assumptions as to the underlying logic, one constructs a statement $Con_\tau$ that canonically expresses the consistency of *T* in arithmetic.

In the 1960 paper I began with the observation, quoted by Franks, that "the applications of the method [of arithmetization] can be classified as being *extensional* if essentially only numerically correct definitions are needed, or *intensional* if the definitions must more fully express the notions involved, so that various of the general properties of these notions can be formally derived." Hilbert and Bernays [1939] had produced three so-called derivability conditions on a provability predicate for a formal system that would insure that Gödel's second incompleteness theorem would hold for it using the associated consistency statement. But they provided no sufficient syntactic criteria on a provability predicate that insure its meeting those conditions. My main concern in the 1960 paper was in general to localize such criteria to the form of $\tau$ and see when weaker criteria might suffice for other applications of the method of arithmetization. In particular, I was able to show that if $\tau$ is what I called an RE formula (thus provably equivalent to a $\sum^0_1$ formula) and *T* is a consistent extension of *PA*, and if $\tau$ numerates the axioms of *T* in *T* then *T* does not prove $Con_\tau$. Even more, under the same conditions, $T+\{Con_\tau\}$ is not interpretable in any finite subsystem of *T*. The standard formulas such as $\pi$ for *PA* are RE formulas, while I gave an example of a non-RE formula $\pi^*$ binumerating the axioms of *PA* in *PA* for which *PA* proves $Con_{\pi^*}$. On the other hand (not mentioned by Franks) I established an arithmetization of the Löwenheim-Skolem theorem (alternatively, Gödel's completeness theorem) under much weaker conditions than RE formulas, having interesting consequences such as Orey's compactness theorem for interpretability.

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[6] The weakening of the assumption that *T* extends *PA* is discussed below.

It was shown in my 1960 paper that various closure conditions on a provability predicate $Pr_\tau(x)$ can be established in $PA$ no matter what the form of $\tau$; these include the first two Hilbert-Bernays derivability conditions. But the third derivability condition was only shown to hold there for RE formulas $\tau$. Franks claims p.121 that I require an intensionally correct axiomatization of provability from $T$ to provably satisfy in $T$ a specific list of five such statements, and he calls this "Feferman's notion of intensionality." But that is completely mistaken. I nowhere make such a statement or even claim that any such list is a sufficient condition for intensionality. Rather, all I say on the subject is the following:

> With a given theory $T$ we can associate the class of all formulas $\tau(x)$ which numerically define the set of axioms of $T$. Then, by formally copying our notion of logical proof, we can associate with each such formula $\tau$, in a uniform way, a formula $Prf_\tau(x, y)$ and, in turn a sentence $Con_\tau$. It will be evident from the construction … that $Prf_\tau(x, y)$ correctly expresses that $y$ is a proof of $x$, via the first order logic, from the set of axioms defined by $\tau$. In other words, whenever a formula $\tau(x)$ can be recognized to express correctly that $x$ is an axiom of $T$, the associated sentence $Con_\tau$ will be recognized to be a correct expression of the proposition that $T$ is consistent. In this way all intensionally correct statements of consistency for formal theories can be obtained as special cases. ([Feferman 1960], p. 38)

This is only four pages in from the beginning of the paper. Franks seems instead to have taken (with slight modifications) what he claims to be my criteria for intensionality from the list of five statements in Buss [1998] p. 116, though that is not ascribed to me there.

In section 4.4, Franks rightly points out that my 1960 approach to arithmetization was not sufficiently general and that the use of logical bases other than that of Hilbert-style first-order predicate calculus used there ought to be considered.[7] An immediate candidate that he suggests is replacing provability by cut-free provability in Gentzen's

[7] In that respect he does not seem to be aware of my return to the issue of canonical formulations of consistency statements in Feferman [1989]. That is achieved there for all finitarily inductively generated formal systems $T$ in a way that allows for wide alternatives in the choice of basic logic for $T$.

sense.[8] He thinks that the use of other such logical bases could make a significant difference, especially in connection with the consistency statements for weak theories where the derivability conditions become problematic to establish, beginning with R. M. Robinson's finitely axiomatized system $Q$ that had been introduced in Tarski, Mostowski and Robinson [1953] to facilitate far-reaching undecidability results. [*NB.* Franks's list of axioms of $Q$ is missing the axiom $\forall x( x \times 0 = 0)$.] Considerable work over the years succeeded in reducing the amount of arithmetic needed to carry out the method of arithmetization involved in Gödel's second incompleteness theorem to $I\Delta_0 + \Omega_1$, where $I\Delta_0$ is the extension of $Q$ by the scheme of induction restricted to bounded formulas and $\Omega_1$ states the existence of the function $x^{log(y)}$; cf., e.g. Buss [1998], where also his system $S^1{}_2$ is shown to suffice. But these too are based on standard first-order logic. Franks proposes in their place taking a form of Kreisel's no-counterexample interpretation derived from Herbrand's theorem as the basic logical apparatus because it is supposed to be "combinatorially simpler" (p. 128) and thus more directly adapted to the limited computational strength of the above systems. But one must question the possible relevance of all this to Hilbert's program, since that is supposed to apply to actual mathematics through the process of more or less direct formalization, and the kind of basic system that most directly formalizes logic in practice is a natural deduction system like Gentzen's *NK*, from which one obtains a Hilbert style system by putting the conditional → to double duty.[9] In either case, a crucial rule is that of Modus Ponens, without which the formalization of mathematics as actually practiced would simply not get off the ground. But demonstration of the closure of Herbrand proofs or of the no-counterexample interpretation under Modus Ponens is beyond the reach of the weak systems in question and in any case is non-trivial. So all of this seems to me to be a real distraction from the question of the significance of Hilbert's program for the autonomy of

[8] Indeed, Kreisel and Takeuti [1974] proved the consistency of a certain cut-free formalization of analysis within that system.

[9] Hilbert's axiomatization of first-order logic involving forms of introduction and elimination axioms and rules via the conditional predates considerably Gentzen's work. It is of course to be found in Hilbert and Ackermann [1928], but that is already based closely on lectures that Hilbert gave, with notes by Bernays, in the period 1917-1920; cf. Sieg [1999], pp. 12-13.

mathematical knowledge.

For the same reason, I shall skip commenting on the most technical chapter in the book, Ch. 5, which deals with work on the question raised by Kreisel in the 1950s as to whether the unprovability of $Con_Q$ in $Q$ or more generally of *any* arithmetization of consistency of $Q$ could be considered to be a demonstration of the informal statement that $Q$ does not prove its own consistency, since $Q$ is *prima facie* too weak to carry out any of the arithmetization necessary for Gödel's second incompleteness theorem. The work of Wilkie (1986) and Pudlák (1985) with the surprising result that the system $I\Delta_0 + \Omega_1$ is ("cut") interpretable in Q has been argued by Pudlák to give a positive answer to Kreisel's question. Franks' Ch. 5 is an extended critique of this argument, but I don't see that it adds any force whatever to what is supposed to be the main aim of this book.

It is to just that that we finally return in the concluding Ch. 6, "Autonomy in context". In part that chapter is a harangue against foundationalism of any kind, even though it is claimed that the main foundational programs of the early twentieth century have largely been eroded and "are now treated more like historical attractions than like viable ways to enrich our understanding of mathematics." This ignores, for example, the great strides made in the constructivist redevelopment of mathematics (thus giving it in principle systematic computational content) in the schools of Bishop and of Martin-Löf, the success of modern forms of predicativism by myself and others as a direct account for scientifically applicable mathematics, and the pursuit of new foundational programs such as those of structuralism in its many varieties by Resnik, Shapiro, Hellman and others. In part, too, this last chapter is an attempt to assimilate Hilbert to forms of mathematical naturalism, especially as espoused by Maddy. Hilbert is said to be a mathematical naturalist *avant la lettre*, despite his pursuit of the finitary consistency program as the means to "secure" mathematics. After all, shouldn't one say that that's just another foundational program; why else entitle his great work with Bernays, *Grundlagen der Mathematik*? Yet Franks insists that "Hilbert was not, as he is commonly described as being, trying to demonstrate that modern mathematics is ultimately grounded in finitary reasoning about concrete signs. Like Wittgenstein, he did not think it worthwhile, or even coherent, to look for mathematics' foundations in anything at all." (p. 195)[10] If so, what was all the fuss about?

Throughout the book, Franks has quoted Hilbert selectively to support as much as possible his revisionist view of what Hilbert's program was really important for accomplishing. At the very end there is one more selective quotation, in this case from Hilbert's apostate student Weyl in his book *The Philosophy of Mathematics and of Natural Science* [1949]. That one is supposed to show that Weyl had "abandoned foundationalism after many years of resisting the draw of Hilbert's program." It is true that after giving up his own effort (in the 1918 monograph, *Das Kontinuum*) at a predicative foundation of analysis despite its success as far as it went and its further promise, Weyl took up Brouwerian intuitionism but later despaired of that.[11] Apropos of his own teacher's program Weyl writes (op. cit., p. 61), that "Hilbert's mathematics may be a pretty game with formulas…but what bearing does that have on cognition, since its formulas admittedly have no material meaning by which they could express intuitive truths?" However he says on the preceding page that at least a consistency proof of axiomatic arithmetic "would vindicate the standpoint taken by the author in *Das Kontinuum*, that one may safely treat the sequence of natural numbers as a closed sequence of objects." (op. cit., p. 60). Moreover, though Weyl questioned whether and how one would ever arrive at "ultimate foundations" of mathematics, he took a definite foundational stance throughout the years in his criticism of set-theoretical platonism and its assumption of closed transfinite totalities:

> The leap into the beyond occurs when the sequence of numbers that is never complete but remains open toward the infinite is made into a closed aggregate of objects existing in themselves. The vindication of this transcendental point of view forms the central issue of the violent dispute … over the foundations of mathematics." ([Weyl 1949], p. 38).

[10] There is a long, approving, mid-section of Ch. 6 on Wittgenstein's rejection of the idea of the critique of philosophy as a kind of "second-order" philosophy.

[11] I wish Weyl had been alive to witness how Bishop succeeded in advancing constructive analysis much, much farther than Brouwer by dispensing with his theory of choice sequences.

The autonomy of mathematical knowledge in practice is a fact: mathematicians have no need of a book like this to convince them that philosophers have nothing to tell them about what ought and ought not to count as mathematics. And they do not question its carrying an apparent body of truths within and—far and wide—outside of mathematics. But that will not and should not stop philosophers from asking: "Knowledge of what? And by what means?" whether or not any mathematicians are listening.

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