

Volker Peckhaus, *Hilbertprogramm und kritische Philosophie*
(Göttingen, Vandenhoeck & Ruprecht, 1990).

A Review by C. Smoryński

The firm of Vandenhoeck and Ruprecht in Göttingen has recently published a number of fine books on the history of modern mathematics in Germany, with particular emphasis on Göttingen. Their works on David Hilbert are *Der Briefwechsel David Hilbert - Felix Klein (1886 - 1918)* (1985) edited by Günther Frei; *Über die Entstehung von David Hilbert's „Grundlagen der Geometrie“* (1986) by M.-M. Toepell; and now *Hilbertprogramm und kritische Philosophie* (1990) by Volker Peckhaus. The first of these has very little for the logician: a pair of letters (numbers 55 and 56) and an accompanying excerpt from a letter from Paul Gordan to Klein will satisfy those claiming a psychological background to Hilbert's programme. The book by Toepell should serve to dispel the myth that Hilbert's *Grundlagen der Geometrie* arose in a vacuum: Hilbert concerned himself with geometry throughout the 1890s, lecturing on it a number of times. However, except as background to the development of Hilbert's foundational views, Toepell's book is not essential reading for the historian of logic. This historian cannot, however, ignore the book of Peckhaus, which is the first booklength study of Hilbert's foundational activity in the period between Hilbert's *Grundlagen* and the end of the First World War. *A la* Toepell Peckhaus dispels the myth of Hilbert's 1904 - 1917 foundational vacuum; he offers the clearest perspective on the rôle of the paradoxes in Hilbert's programme I've ever seen; and he offers a very powerful challenge to those of us who stubbornly cling to the belief that there wasn't a philosophical bone in Hilbert's body.

All but the most perfunctory reader will notice a shift in Hilbert's attitude toward the paradoxes. When, in 1900, he originally called for consistency proofs for his axiomatisation of arithmetic and, eventually, for an axiomatisation of set theory, the paradoxes were not a major concern to him. Indeed, of his two publications on the matter, only the one— his famous address on problems— even hinted at the existence of the paradoxes when he mentioned that some collections— like that of all alephs— are inconsistent. Hilbert had already known of the set-theoretic paradoxes for several years. In his 1904 lecture at Heidelberg, he actually used the word "paradox" and declared that "the avoidance of such contradictions and the clarification of those paradoxes is rather to be kept in the eyes as a principal goal from the start in the investigation of the number-concept". Noting that logic depends on arithmetic and arithmetic on logic, he added that "for the avoidance of paradoxes a piecewise simultaneous development of the laws of logic and arithmetic is therefore necessary". It is not at all clear from these remarks whether the paradoxes are now an impetus or merely a constraint. In his later popular accounts of his foundational views, the paradoxes would become not merely an impetus, but the prime mover. Indeed, in his 1917 lecture on axiomatic thinking Hilbert was so taken with the paradoxes that he considered them to be the reason Kronecker and Poincaré rejected Cantor's set theory. Dropping the chronologically absurd reference to Kronecker, Hilbert would again wax poetic over the paradoxes in his other great popular account— his 1925

Münster lecture on the infinite. Does the fact that the most extensive remarks on the paradoxes that Hilbert made in print occur in popular accounts, coupled with the careless invention of Kronecker's knowledge of the paradoxes tell us something? If we look at Hilbert's technical papers of the 1920s, instead of the earlier demanded piecemeal simultaneous development of the laws of logic and arithmetic as necessitated by the paradoxes, we find a simple dichotomy between a safe metamathematics and the not-yet-safe mathematical practice. The paradoxes seem to have lost their ability to shape Hilbert's programme. And, if one looks to the timing of Hilbert's mature programme and one checks to whom or what Hilbert reacts during this period, one is drawn to the inevitable conclusion that it was Weyl's defection and not the paradoxes directly that triggered and sustained Hilbert's new burst of foundational activity in the 1920s. Only insofar as Weyl took the paradoxes seriously and believed analysis to be inconsistent did the paradoxes assume a secondary causal importance to Hilbert— a small rôle seemingly incommensurable with the rhetoric devoted to them in „Axiomatisches Denken" or „Über die Unendliche". Extrapolating the 1920s Hilbert back to 1904, or interpolating between the 1900 and 1920s Hilberts to that year, one is inclined to minimise the importance of the paradoxes to Hilbert and Hilbert's programme then as well. This, according to Peckhaus, would be a mistake. In his view, and the newly published evidence seems to warrant such, Hilbert took the paradoxes so seriously that he undertook a second phase of his axiomatic programme because of them. And the reason he suddenly took the paradoxes seriously is the gravity with which everyone else took them in 1903 when they first came to be widely known.

Peckhaus notes that in the various libraries and archives of Göttingen there can be found no fewer than ten sets of notes of Hilbert's lectures on the foundations of mathematics, exclusive of the foundations of geometry, covering the period between 1898 and 1917/1918. Among these are two write-ups by Ernst Hellinger and Max Born, respectively, of a course given by Hilbert in the 1904/1905 academic year. Of particular interest in these manuscripts are Hilbert's extensive remarks on the paradoxes. Here we find it quite explicitly stated that contradictions are the normal course of events in the helter-skelter developments of new areas of mathematics, but that in set theory one had gone deeper into the "theoretic philosophical side" than had anyone in previous mathematical developments and that in set theory one had mistakenly accepted Aristotelian logic as obvious. There is even the declaration that the laws of logic themselves are in doubt. Of course, Hilbert would say such things in the 1920s, but in this latter period so much of what he said is readily dismissed. However, his remarks of 1905, made when he was at the height of his powers must be taken seriously.

The 1905 lecture notes document a genuine change in Hilbert's attitude. In 1900 he had barely mentioned the paradoxes, and in a 1903 letter to Frege he more-or-less downplayed them, noting that he had known them for some years and had personally discovered even stronger ones. In the 1905 lectures we find Hilbert's own paradox— and the announcement that it is "especially significant" because of its "purely mathematical nature": Let U be the collection of all sets obtainable from the set of natural numbers by union ($A \cup B \cup \dots$) and the reflexive formation of function spaces (from A obtain A^A), and

consider UU ... As paradoxes go, this one now seems a bit laboured and soporific. I would consider it a mixture of the Burali-Forti paradox and Cantor's proof of the inconsistency of the set of all sets: Look at the collection U of all things generated. This occurs at some stage α . Now look at stage $\alpha + 1$. Cantor's proof that 2^X is bigger than X applies to give the contradiction... The "purely mathematical nature" of this paradox lies presumably in the fact that U is not a vague collection of all sets, but is the result of a precisely defined inductive generation. That the precision is lacking probably tells us something about the general level of development of set theory at the time, although it is exactly the sort of lack of precision found in all of Hilbert's foundational papers and one is tempted to read a bit more into it than perhaps one should.

Unfortunately, Peckhaus says nothing about the other eight write-ups of Hilbert's foundational lectures to be found in the archives. However, he does give those of us who are more than an hour's train ride from Göttingen a more accessible source for further information, namely a paper by Vito Michele Abrusci, "David Hilbert's *Vorlesungen* on logic and foundations of mathematics" (in: Corsi, Mangione, Mugnai, eds., *Atti del convegno internazionale de storia della logica. Le teoria delle modalita* (1989), Bologna; no publisher given). Thoughtful as this is, one cannot help but fault Peckhaus for his failure to provide even the simplest chronological information. Are these lectures uniformly distributed over the period 1898 to 1917/1918 or is there a gap between 1905 and 1917/1918? Considering Brouwer's remark that Hilbert got the idea of the mathematics/metamathematics distinction from him in the summer of 1909, a datable documented transition from the call for a piecewise simultaneous development of logic and arithmetic to that for a metamathematical consistency proof for mathematics would be most interesting. Indeed, the date and cause of this transition would be of great interest even had Brouwer not accused Hilbert of plagiarism. Perhaps Abrusci settles the matter.

What Peckhaus is writing is as much a social history as a problem history. The three chapters following the discussion of Hilbert's 1905 lectures concern themselves with the social aspects of Hilbert's foundational involvement in the period stretching from 1904 to the end of the First World War. The two major involvements are his support of and relationships with Ernst Zermelo and Leonard Nelson. That there should be a strong connexion between Hilbert, Göttingen's champion of the axiomatic method, and Zermelo, Göttingen's axiomatiser of set theory, is obvious and I need say no more about it than that Peckhaus devotes an entire chapter to Zermelo. The name "Leonard Nelson" is not so widely known and Peckhaus gives him two chapters. The first of these discusses Nelson, his followers, and their „kritische Philosophie"— in particular „kritische Mathematik"; the second chapter elaborates on Hilbert's support of Nelson.

Hilbert's involvement with Nelson is particularly intriguing. The impression one gets from Hilbert's dismissal of Frege is one of a lack of patience with philosophers. The anecdote (apocryphal?) about Hilbert's turning to his friends after mentioning Kant in his famous Königsberg radio address and saying that, of course, it is nonsense but one must mention Kant when in Königsberg affirms any impression one has that Hilbert did not think highly of the discipline. If one adds to this the generally low level of philosophical sophistication of his foundational papers of the 1920s, one will surely feel justified in

declaring Hilbert to be aphilosophical. Against all of this compelling evidence for such a conclusion Peckhaus documents Hilbert's involvement with Nelson. There is, of course, also the fact that Hilbert's initial interest in Bernays lay in the latter's knowledge of philosophy. How do we eliminate the evident inconsistency? The answer may well be provided by the account of the Hilbert-Nelson alliance. Although Hilbert's own paradox was of a "purely mathematical nature", much of the problem with set theory had been that it had gone deeply into the "theoretical philosophical side" and logic itself had to be looked into. Just as Hilbert involved himself in physics during this period, he saw the need for an interdisciplinary involvement of mathematicians and philosophers in dissolving the paradoxes. If Hilbert were *aphilosophical*, he was not *antiphilosophical* and felt he needed the help of philosophers and of Nelson in particular. In any event, Hilbert's involvement with Nelson, coupled with the latter's work on the paradoxes, underlines once again the importance Hilbert attached to the paradoxes.

For the documentation of Hilbert's genuine concern over the paradoxes and all the information it gives about Hilbert's philosophical involvement, Peckhaus' book deserves a place on the bookshelf of every logician interested in the history of his field. One would hope, however, that the position would not be permanent and the book would give way to a more definitive second edition, one answering every conceivable question concerning the period: What is in those other eight write-ups of lectures? Did Hilbert make any progress after 1905? And Brouwer: In the years 1909 - 1919 of friendship with Hilbert, did the two of them discuss only topology during Brouwer's frequent visits to Göttingen? It would seem most unlikely. Finally, how did Hilbert react to Poincaré's criticism of his 1904 lecture? Surely he didn't wait until the 1920s and his first published response?

Seriously, there is a lot of scholarly research yet to be done on Hilbert's foundational work. A preliminary first step would seem to be the collection of his papers: The lecture notes remain unpublished; the collected works are incomplete and, to date, the only complete published collection of his foundational papers is Abrusci's Italian translation. Some of these papers have not yet been translated into English (one isn't even mentioned in the *Omega Bibliography*). Considering the important rôle that Hilbert, the greatest mathematician of the first quarter of our century, played in the development of modern logic, one would hope that this first step would be taken soon.