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William Ewald,
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REVIEW

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This is a study in history of philosophy of logic. Its author rejects the received view that Hilbert had no sustained interest in philosophy and foundations of mathematics, but was goaded into undertaking his foundational excursions by the "crisis" in mathematics precipitated by the Russell paradox and the debates on foundations which consequently emerged between logicism, formalism, and intuitionism. Ewald also opposes the contention of the received view, that the Hilbert program was specifically and explicitly a response to the Russell paradox, and he adduces evidence from Hilbert?s lectures and lecture notes, in support of his opposition to the contentions of the received view. What follows is not a study of the history of logic and foundations; nor is it a discussion of the technical aspects of the Hilbert program. It is *not* history of logic and foundations. It *is* history of philosophy of logic and foundations, examining, in large measure upon the basis of the Hilbert *Nachlaß*, the chronology of the motivation, origin and development of the Hilbert program.

Ewald's thesis in this article is that the traditional perception of Hilbert as a mathematician focused exclusively on problem solving and with little or no interest in philosophical or foundational issues at best, is misguided.

This perception is to be attributed initially to Otto Blumenthal and his biography of Hilbert, in which the emphasis is first of all on Hilbert's list of unsolved problems [Hilbert 1900] and to Blumenthal's account of the work which Hilbert had taken on, in particular with his predeliction to tackle difficult, but not unsurmountably unsolvable, problems in one field of mathematics and then moving on to another field. This perception is further suggested by Hermann Weyl, who emphasized [Weyl 1944] Hilbert's tenacity in working on, and succeeding in obtaining significant results in, various fields of mathematics. Ewald (p. 228) facetiously refers to Blumenthal's description of Hilbert's Problems list as 'the supreme "Mathematical Games and Puzzles" column.'

As evidence of Hilbert's lack of interest in or serious attention to foundational and philosophical issues, Ewald refers (p. 229) to Hilbert's arguments against Brouwer and Weyl and their intuitionism; his formalistic conception of logic nd the demand for a finitist approach to proof; to his worries over what he regarded as the unhealthy results of the set-theoretical paradoxes; and to his dismissal [Hilbert 1931] for their "twaddle" of foes of the Law of Excluded Middle as "philosophers disguised as mathematicians." These polemical attacks against philosophical stances were excised—Ewald thinks with Hilbert's explicit approval—by the editor from the material that was included in Hilbert's *Gesammelte Abhandlungen*. On this traditional view, Hilbert was, in Ewald's words (p. 229), "a mathematician's mathematician, a collector and solver of problems, moving from challenge to challenge like a mountain climber, but with no underlying theme beyond the desire to climb as many peaks as possible," anything but a "synthesizing intellect."

Ewald raises (p. 229), and endeavors to provide evidence of an affirmative reply to, the question of whether Hilbert's polemical attacks upon "the question of the foundations of mathematics as such," which he hoped can finally be got rid of, were merely the result of his impatience and frustration with the difficulties which they posed in the search to complete his problem-solving work, rather than an accurate expression of any real hostility to philosophical and foundational concerns. In fact, there is considerable evidence that Hilbert was interested in, and indeed influenced by, the work of Immauel Kant. His [1899a] *Grundlagen der Geometrie*, starting off with an explicit reference to Kant[1], provides *prima facie* evidence at the very least that Hilbert was

[1]The quote is from the "Elementarlehre" of Kant's *Kritik der reinen Vernunft*, 2. T. 2 Abt.; it is: "So fängt denn alle menschliche Erkenntnis mit Anschauung an, geht von da zu Begriffen und endigt mit Ideen." This conspicuoulsy eventuated in Hilbert's work with Stefan Cohn-Vossen (1902-1936), the *Anschauliche Geometrie* [Hilbert & Cohn-Vossen 1932]. The argument that this work was the responsibility of Cohn-Vossen and that Hilbert merely lent his name to it to assist Cohn-Vossen does not necessarily discredit the possibility that Hilbert was interested in, or credit

aware of Kant. And, having been both educated at Kant's Königsberg and on its faculty for a decade, he could not have been blind to the intellectual presence of Kant, even four-fifths of a century after Kant's decease. Following Gottfried Martin's analysis [Martin 1985, esp. 6-7] of various interpretations of attitudes stemming from Kant's philosophy of mathematics, we can at the very least confirm a confluence of assessments between Kant and Hilbert that geometry and arithmetic are axiomatic, though they would diverge in Kant's assessment that geometry and arithmetic are constructive and Hilbert's that they are deductive (keeping in mind that Kant's use of "synthetic" corresponds to Martin's use of "axiomatic and constructive"). If, however, Kant had any influence at all upon Hilbert, either in arousing in him an interest in philosophical or foundational issues, or in any actual undertakings or positions in such issues, Ewald does not mention it. Hardy [1929, 11], however, does tell his readers that Hilbert was, by Hilbert's own admission, influenced by Kant; he writes [Hardy 1929, 11]: "There is . . . some sort of concrete, perceptible basis for which Hilbert . . . claims the support of 'the philosophers and especially Kant'," although Hardy also admits that he is himself ignorant by what "justice" this claim can be made, and he consequently fails to identify any connection between Kant's philosophy and Hilbert. We can, nonetheless, point to Hilbert's use of a quotation from Kant for the motto of his *Grundlagen der Geometrie* as attesting to some influence of Kant upon Hilbert, however subtle or enigmatic that influence may prove to be.

Combining the supposition that Hilbert had no native philosophical or foundational interests to speak of with the supposition that he was exasperated by the animadversions of the intuitionists and frustrated by the disarray to the process of axiomatizing mathematics that was created by the paradoxes, the third aspect of the traditional view of Hilbert is that he was led to (or perhaps better, goaded into) his explicitly foundational researches by his discomfitures. In Ewald's words (p. 230), "Hilbert's work specifically in foundations was inspired by the so-called crisis in the foundations of mathematics."

The crisis was of course created by the logical and set-theoretic paradoxes, and more particularly the Russell paradox, which goes significanly more deeply than the semantic paradoxes, and deeper even than

the possibility that Hilbert disagreed with, the main tenets of the work. The explicit quotation from Kant's passage arguing that knowledge begins with intuition, made more than three decades before publication of the *Anschauliche Geometrie*, could appear credibly to speak for Hilbert's interest in, if not unadulterated acquiescence in, a Kantian philosophy of mathematics.

the other set-theoretic paradoxes, the Cantor and Burali-Forti paradoxes in partticular; because, unlike those, the Russell paradox bears on the very notions of *set* and *elementhood*. The final straw, one may say the death-blow, for the Hilbert program was of course Gödel's incompleteness theorems which, despite the title of Gödel's [1931] with its explicit reference to *Principia* systems, served to unconditionally and irrevocably undermine the Hilbert program. —Or so, we should say, it would appear. As Ewald (p. 248) remarks, Gödel did not conceive of his incompleteness results as necessarily applying to Hilbert's conception of proofs. For as Gödel himself asserted in his remark [Gödel 1931, 197] to Theorem XI of his article on incompleteness: "Die entsprechenden Resultate über (M, A) in keinen Widerspruch zum Hilbertschen formalistischen Standpunkt stehen." Stephen Kleene, in his introductory note to Gödel's incompleteness papers [Gödel 1986, 138] explained: that Gödel's remark regarding Theorem XI make it "conceivable that there exist finitary proofs that *cannot* be expressed in the formalism of P."

Hilbert's only goal in undertaking to deal with the foundational challenges, Ewald explains (p. 230) in sketching the traditional view, was to eliminate those which had become problematic for mathematics; and he worked to provide technical solutions to specific problems, rather than to devise a systematic and philosophically satisfying underpinning for mathematics. Consequently, his foundational and philosophical significance is only of historical interest, and limited to the fate of his consistency program.

As sketched by Ewald (pp. 230?231), Weyl's assessment of Hilbert's foundational work has it advance in two separate and distinct stages. The first centers around the work in axiomatization of geometry. The second—and for Ewald more interesting—stage, taken up several years after the first stage, involves Hilbert's duel with logicism on the one hand and intuitionism on the other. Frege's abandonment of his efforts to complete his foundational work in the compelling face of the the Russell paradox signals the onset of the foundational crisis. What Weyl's account failed to take cognizance of is, as Ewald correctly tells us, that Hilbert entered again into foundational work in 1902, whereas Russell did not publish his critique of Frege and his announcement of the Russell paradox and an attempt at a way out until 1903, in the *Principles of Mathematics*. Ewald also correctly notes that by this time Hilbert was already cognizant of the Cantor paradox (and the Burali-Forti paradox), so that he did not have to wait until 1903 for Russell to publish his paradox to know that naïve Cantorian set theory was exhibiting difficulties. This discrepancy signals to Ewald that the traditional perception of Hilbert is not entirely correct.

Much of the remainder of Ewald's account of the received view of Hilbert is devoted to rejection of the contention, attributed first of all, but not exclusively, to Frank Ramsey [1925, 338] that Hilbert's was, in Ewald's words (pp. 231-232) a "crude" formalism. This is the view that claims that for Hilbert, all of mathematics consists merely of manipulation of empty, meaningless symbols. To counter this view, Ewald invokes G. H. Hardy?s "Mathematical Proof" [Hardy 1929], and in particular the passage in which Hardy [1929, 11] is prepared to accept that Hilbert's philosophy is inadequate, even grossly inadequate, but categorically rejects the mathematical structures erected from our axioms are meaningless, empty symbols rather than expressions concerning mathematical entities that have a reality for us. They are neither trivial nor ridiculous, and in Hardy's mind it is "impossible to suppose that Hilbert denies the significance and reality of mathematics" [Hardy 1929, 11]. To the contrary, as Hilbert himself stated, "the axioms and demonstrable theorems which arise in our formalistic game are the images of ideas which form the subject matter of ordinary mathematics" [Hilbert 1923, 153].[2] It would seem but one more step from this gnoseological attitude towards the Platonistic-Pythagorean view that behind these images are *real* Ideas that although epistemologically abstract, are more real than the images themselves; and that these Ideas are the mathematical structures which our formal system erects from its axioms. But Ewald is not yet prepared to make this next step, or to speculate on how wide or narrow the stride required to cover it is.

Ewald also invokes, in support of Hardy's contention, Hilbert's actual work, including material from his lecture notes, as confirmation that he contributed to areas of mathematics, *e.g.* invariant theory, which were actively employed in practical applications, to a reality outside of mathematics itself. Hilbert's contributions to physics is another example.

We might momentarily stop our examination of Ewald at this juncture and ponder over (1) the 'why?' of the "unreasonable effectiveness" of mathematics (see, *e.g.* [Wigner 1960]; [Hamming 1980]; [Sarukkai 2005]); and (2) the 'how?' and 'why?' of Hilbert's axiomatization of relativity theory, the role of Hilbert space in quantum theory, *etc.*, if

[2] "Die Axiome und beweisbaren Sätze, d.h. die Formeln, die in diesem Wechselspiel entstehen, sind die Abbilder der Gedanken, die das bliche Verfahren der bisherigen Mathematik ausmachen..." See also [Hilbert 1932- 35, 180].

Hilbert was "gaming" without a commitment of some sort to the reality of the underlying mathematical structures. Richard Hamming [1980] and Sundar Sarukkai [2005] for example, both think that it has in part at least to do with the game-theoretical aspect of language, in this case a specifically mathematical language; Sarukkai, who admittedly, adopts a view of Hilbert as a "crude" formalist, in particular reflects upon how mathematical models are constructed. These are models not of the universe, however, but of our linguistic picture of the universe.

We can understand, then, that when Hilbert famously stated that it does not matter whether we take points, straight lines and planes or beer mugs, tables and chairs as the undefined elements of our axiom system,[3] it is the case nevertheless that these primitives of our axiomatic system have a reality of their own, and so do the structures that we raise from them within our axiom system. Hilbert was, in his own way, therefore, a mathematical Platonist, rather than a formalist in the crude sense as being concerned with what Gustav Bergmann [1949, 71] called the "physics of papers and pencils,"[4] or Hilbert's and Hardy's "marks on paper". Hilbert, that is to claim, was not playing language games with these marks on paper that have no meaning or significance outside of the game itself. Neither did he conceive of himself as doing just that and no more. Against the Ramseyan view of Hilbert as a crude formalist working with and manipulating meaningless marks on paper, Ewald (p. 232) points at Hilbert's preface to the *Zahlberichte* in which Hilbert wrote that he "attempted to avoid the great calculating apparatus" so that "one should execute proofs, not by calculations, but solely by thought."

Hilbert, Ewald tells us (p. 233), followed consistently the Riemannian tack of avoiding, insofar as possible, Kummer's "great calculating apparatus" in favor of Riemann's principle that proofs be carried out by thought rather than computation. This, Ewald adds (p. 233), should be sufficient to illustrate the erroneousness of the received view of Hilbert?s formalism, as a crude reliance on mere symbol manipulation in which the symbols are of themselves meaningless. (We might also add: this approach was, we can conjecture, embedded in Hans Hahn's remark, preserved by Gustav Bergmann [1964, 243], that: "Just

[3]See [Reid 1986, 57], from a remark made by Hilbert to Hermann L. G. Wiener (1857-1939), see also [Weyl 1944, 635]; but, according to [Fang 1970, 81], to Ernst Kötter (1859-1922) and Arthur Moritz Schoenflies.

[4]Bergmann's full statement [Bergmann 1949, 71] avers "that as long as one studies symbolic systems *as such*, all one can learn from them about the world belongs to the physics of pencils and papers and to the psychology of those who play these fascinating and intricate games" [my emphasis].

knowing how a proof goes, you know nothing. When you know why it goes this way rather than that way or that other way, then you begin to know something.")

Hilbert's lecture notes illustrate that he often remarked to his classes on the interconnectedness of various branches of mathematics with one another, and that logical and set-theoretical concerns also appeared throughout both his lectures and his thinking, that although these types of discussions did not appear in his publications, they had a significant role in his lectures and his intellection. Moreover, Ewald notes (p. 233), many of his courses on various topics included substantial introductory discussion of their logical and set-theoretical foundation, and most especially on the axiomatic method by which the course material was to be unfolded. And lectures and courses on a particular topic, Ewald further notes (p. 233), did not begin or end only during the period in which Hilbert produced his research publications on that topic; rather, his courses covered the many fields in which he worked throughout the length of his teaching career. To divide Hilbert's career into discrete segments, marked by interest in and occupation with just one or a few branches of mathematics during and only during that career segment, presents, therefore, a distortion of his intellectual biography.

If this aspect of the received view of Hilbert is incorrect, what about other salient aspects of that view?

Ewald notes (pp. 231, 233) that the chronology of Hilbert's lectures and researches belie the received view that it was only the foundational crisis in mathematics that precipitated Hilbert?s embarkation upon his own foundational excursions. He points out (p. 230), for example, that Hilbert was already cognizant of the Cantor paradox by 1897, that is, prior to the publication of the Russell paradox, but that he nevertheless called for a proof of the consistency of arithmetic in the second item of his problem list of 1900, and alluding to to it at the end of "Über den Zahlbegriffe" [Hilbert 1899b]. Cantor's Continuum Hypothesis received its place at the top of Hilbert?s problem list. This chronology belies the received view that the Russell paradox provided the stimulus for the recognition of the need for a consistency proof, and for the consequent development of the Hilbert program. On the other hand, it does not count as evidence, we are bound to claim, that the set-theoretical paradoxes led Hilbert to conceive of the need for a search for consistency. For that, we require first Ewald's contention (p. 232) that Hilbert thought the Cantor paradox already settled in 1908 by Zermelo's axiomatization of set theory, with its *Fundierungs- und*

Auswahlaxiom, designed to ban pathological sets such as the Russell set, which came a decade prior to Hilbert's main onslaught on the problem in and by the Hilbert program, as articulated in "Axiomatisches Denken" [Hilbert 1918].

There is no reference to a *Grundlagenkris*, or to the intuitionistic challenges of Brouwer and Weyl in Hilbert's lectures on foundations of mathematics of 1917/18. Neither do the paradoxes come up for discussion, let alone urgent discussion, until Hilbert, near the end of his course, turns to consideration of higher-order logic: —and then only as an object-lesson about the need for care to be excersied in formulating one's axiom set. The whole of Hilbert's 1917/18 lecture course on foundations revolves around problems induced by the axiomatic method itself. And the course is concerned specifically with what Ewald describes (p. 233) as a "detailed examination" of the axiomatic method. More to the point, in making the case that the paradoxes did not drive Hilbert into thinking about and working in philosophy and foundations, Ewald avers (pp. 233-234) that nowhere in his lectures are the paradoxes cited as motivation, or even as one motivating factor among others, for Hilbert's excursions into logical investigations, and "still less" are they presented "as a shattering disaster for mathematics as a whole." Perhaps needlessly, and not of itself sufficient to advance his case, but more, perhaps, as an indication of Hilbert's personality, Ewald adds (p. 234) that the discussion is "cool and a bit dry, without the slightest taint of hysteria." This bit of effluvia by Ewald may perhaps tell us something about the psychology of those who play the "fascinating and intricate games" of the study of symbolic systems, and of Hilbert in particular; but, although Ewalds may think it so, it is otherwise not Ewald's best argument against the notion of the Hilbert program as a panicky defense against the paradoxes.

Ewald (pp. 234?235) finds that the traditional view tends strongly and wrongly to denigrate Hilbert's accomplishments, and in particular their importance for and role in the history of logic. In accordance with the traditional view, which sees Hilbert's search for consistency as a sidelight to his research and as a response to the paradoxes, the Hilbert program, and more specifically the development of *Beweistheorie* arising from it, because it was carried out in haphazard and piecemeal fashion and under duress, is inferior to the work of Löwenheim and Skolem. It is a failure also, as having been undermined by Gödel's incompleteness proofs. But Ewald fails to note that Hilbert's work in proof theory has another root than did the work of Löwenheim and Skolem. The latter came directly out of the algebraic tradition of the

Boole-Schröder calculus, and its purpose was to establish the model-theoretic properties of finitary systems. That is to say, the work of Löwenheim and Skolem was what we would today designate as belonging to model theory, and designed to examine and establish the properties of categoricity of models for specific universes of discourse. The Löwenheim-Skolem Theorem, for example, establishes the satisfiability of κ-ary models, and asserts that if a formula F of a first-order logical system with identity is κ-satisfiable for every finitary κ-ary model, then it is $\aleph_0$-satisfiable. (See [Brady 2000], [Anellis 2004], and [Badesa Cortéz 1991; 2004] of the connection of the work of Löwenheim and Skolem to the classical Boole-Schröder calculus and to its contribution to model theory.) The root of Hilbert's later work in proof theory is not in the Boole-Schröder calculus itself, but in the semantic approach that Löwenheim applied in establishing the soundness of universes of discourse for the Boole-Schrder calculus and applying this to the study of the relationships between validity, satisfiability, and provability. Hilbert took Löwenheim?s method "as a proof procedure and used it to reopen the study of the character and concept of *formal system*" [Anellis 1994, 120]. What drove the work on proof theory and the development of alternative theories of quantification in the 1930s (by Herbrand and Gentzen, as well as by Hilbert himself and his followers, including especially Bernays) was the investigation and elaboration of what Hilbert meant by "being a proof" (see [Anellis 1991]). All this, too, however, is left untouched by Ewald, who, for the sake of brevity, focuses on Hilbert's work in proof theory in and around 1917/18.

Ewald is intent upon establishing the conception that the Hilbert progam, and in particular Hilbert's work in proof theory, was motivated by his over-all conception of mathematics as a unified and systematic discipline, and that the importance of establishing its consistency was in consonance with this conception of mathematics. Proof theory, that is, was not an emergency repair, and was not an isolated technical response merely to technical difficulties arising from the paradoxes. The problem list of 1900, under this interpretation, was both a response to broader developments of the nineteenth century and a call for the effort, in the coming twentieth century, to provide a sound structure for the entire mathematical edifice as an archetectonic. The problem of the consistency of arithmetic was the centerpiece of that vision.

It is not so much the paradoxes that as such, Ewald thinks (pp. 235-236) undermine mathematics. They undermine set theory and logic, but not mathematics as a whole. If, with Kronecker and Poincaré, one accepts that mathematics rests upon the solid foundation of the integers, which mathematicians can believe in and know directly, then arithmetic remains soundly on solid ground—and consistent. Hilbert's student, colleague, and partner in the creation of *Beweistheorie*, Paul Bernays, Ewald tells us (p. 236), held that it was Russell's logicism, his reduction of the rest of mathematics to logic, and of logic to set theory, that rendered the paradoxes so devastating. In this case, it is also reasonable to ask whether, and if so how, Gödel's incompleteness results impact, and undermine the Hilbert program at all. The devastation done to the Hilbert program, according to Ewald, was more in the mind of Hans Freudenthal [1976] and others like him, than in the mind of Hilbert.

We may follow up by asking whether it is really the case that Hilbert felt as challenged by the incompleteness theorems as much of the historical and philosophical literature insist, and, if so, why he took it as a serious challenge. (As a matter of fact, Ewald does not think that Russell thought that the paradoxes were as serious as he did after Wittgenstein convinced him that all of mathematics was a mere tautology rather than than about objective truth.) Ewald does not, for the nonce, take up, or even mention, these questions. Rather, he asks why, in light of Berkeley's attacks upon Newton's fluxions as "ghosts of departed quantities" and Bolzano's paradoxes of the infinite, Hilbert's question did not arise a century before Hilbert propounded it. Ewald's approach to this apparent failure on the part of mathematicians to ask the Hilbertian question on the heels of Berkeley's criticisms or Bolzano's account of paradoxes, is to explain it in terms of the nature of nineteenth-century mathematics: *viz.*, relying on arithmetic and geometry as guarantors of the whole of mathematics, one would merely retrace one's steps and search for one's intellectual or computational error if a discernible difficulty arose.[5] As Ewald expressed it (p. 236), if an imaginary number occurred with which one had a problem, one merely need only to track down the mathematical entity to which it evidently referred. This, Ewald declares (pp. 236-237) led to nineteenth century mathematicians persistently confusing their symbols with their objects: now they manipulated the one, now the other. Not until the efforts of the very late nineteenth century when mathematicians encountered the problem of providing for the legitimacy of non-Euclidean geometries did they have to worry about axiomatically

[5]Ewald does not consider that the failure to respond to Bolzano's account of the paradoxes of the infinite arose because Bolzano's work was not well known to mathematicians. Nor is it clear that Ewald fails to consider this possible explanation because he disagrees with [Schubring 1993] that Bolzano's work was better known to his colleagues than than has generally been supposed, or simply because the paradoxes were not considered a significant issue at that time.

presented deeductive theories rather than finding interpretations under which the theorems of the theory would be true. Thus, in the face of the Cantor and Russell paradoxes, early twentieth-century mathematicians had a conception of mathematics that, while new, presented them with a *foundational*, rather than an *epistemological*, problem. Brouwer's route took him to intuitionism, to an epistemologucal solution. Hilbert's called for a technical, metamathematical solution.

This old approach, of going back to rehearse and double-check one's work to locate an errant object if and when the symbol obtained in computation or conceptualization seems out of place or otherwise suspicious, could and might have led to the strategy of checking and, when required revising the problematic axioms or offending underlying suppositions of one's theory. Although Ewald does not say so, this strategy might well have been employed as a corrective to the paradoxes stemming from what Ewald has called "lack of care" (p. 237) in Cantor's naïve set theory. Granted it would not solve the philosophical problem of the possibility of the existence of a completed infinite that worried Kronecker, as Ewald admits (p. 237). Neither does Ewald suggest that this is the sort of strategy that may have underlay Zermelo's development of an axiomatic set theory with the inclusion of a *Fundierings- und Auswahlaxiom* to banish pathological sets. And since he does not consider whether this was Zermelo's strategy, neither does he say whether Hilbert would have been satisfied with this maneouver. But he does indicate that Hilbert was, for the time being apparently satisfied that Zermelo's 1908 set theory had adequately dealt with the problem. From the standpoint of philosophy, neither Zermelo's axiomatization nor Russell's theory of types, however, provide a satisfactory solution to the problem of the paradoxes, Ewald says (p. 237). With regard to the theory of types, it was devised specifically—and exclusively—for dealing with the paradoxes, not for underwiting set theory or number theory. We might well ask whether, when Russell queried Leon Henkin as to whether Gödel incompleteness meant that it was permissible in "school-boy arithmetic" to obtain $2+2=4.001$, he, either facetiously or unwittingly seriously, was acknowledging that the *Principia* axiom system, with—or despite—its theory types, was not the solution to the paradoxes that was required from the philosophical perspective.[6]

[6] Leon Henkin recorded that in response to his [Henkin's] request for comments on his article "Are Logic and Mathematics Identical?" [Henkin 1962], Russell expressed the thought that Gödel's incompleteness theorems mean that $2+2=4.001$ is permissible in "school-boy arithmetic". Russell's reply 1963 (ts. [Russell 1963], transcribed in [Grattan-Guinness 2000, 592-593]) was that Gödel's theorem showed, not that (primitive recursive) arithmetic is incomplete, but that it is inconsistent,

All this being the case, we have to ask why Hilbert had thought it necessary to pursue further a program to develop metamathematics if he thought that Zermelo had repaired the problem and thereby eliminated the damage; unless he also thought that more was required than merely a technical treatment. Moreover, we must ask how deeply Gödel's incompleteness results impacted Hilbert, and whether, if satisfied with Zermelo's axiomatization, he would find it necessary to continue foundational research after 1908 unless he had some additional philosophical concerns, or such concerns as had led to a need to respond to Brouwer or Weyl. Because Ewald does not examine Hilbert?s work after 1917/18, we cannot be certain to what extent Gödel's incompleteness results, in Ewald's estimation, affected Hilbert. And for that we have to turn therefore to our own consideration of Hilbert in the post-1918 period, rather than rely upon Ewald. For the most part, Hilbert's response to Gödel comes from Bernays. For Hilbert between 1918 and 1931, and after, Ewald leaves us to our own devices. But we can extrapolate from what Ewald writes and what we otherwise know of Hilbert and his work.

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that it permitted "school-boy" arithmetic to allow that "$2+2=4.001$". This reply (and its "April Fool's" date) prompted Henkin to ask me ([Henkin 1983]) whether Russell was joking; but the entire tenor of the letter, together with the philosophical background on which Russell drew to conclude that Gödel's results allowed school-boy arithmeticians to have $2+2=4.001$, shows that Russell really was in earnest. In his reply to [Russell 1963, ts. 2], Henkin [1963a] therefore actually found it necessary to explain to Russell that Gödel's results did not say that arithmetic is inconsistent, but that it is incomplete.

John Dawson [1991, 96], upon examining this episode, wondered whether Russell's response reflected Russell's momentary bewilderment upon learning of Gödel's theorems, or a continuing "puzzlement". Dawson [1991, 96] asked whether Russell was saying that "intuitively, he had recognized the futility of Hilbert?s scheme of proving the consistency of arithmetic but had failed to consider the possibility of rigorously *proving* that futility", or if he actually was "revealing a belief that Gödel in fact had shown arithmetic to be *inconsistent*," and he notes that Henkin, for one, assumed that Russell supposed Gödel to have proven the inconsistency of arithmetic. He adds [Dawson 1991, 96-97] that, in either case, Gödel eventually received a copy of Russell's letter and consequently remarked to Abraham Robinson on 2 July 1973 that "Russell evidently misinterprets my result; however he does so in a very interesting manner ... ." What manner of misinterpretation Gdel may have had in mind as so interesting is, regrettably, left unstated.

Russell himself, wrote in reply to Gödel (addendum to "My Reply to Criticisms" in *The Philosophy of Bertrand Russell*) that "I had always supposed that there are propositions in mathematical logic which can be stated, but neither proved nor disproved. Two of these had a fairly prominent place in *Principia Mathematica*—namely the axiom of choice and the axiom of infinity."

Both Hilbert and Russell agree that we cannot take the concept of natural numbers for granted. Russell was in search for apodictic certainty, an epistemological concern, whereas Hilbert is in search of a mathematical foundation for mathematics, not merely an epistemic certainty, but mathematically grounded foundations which provide for the consistency, completeness, and categoricity of the mathematical theory. It is thus little wonder that Zermelo's proof that every set can be well-ordered would initially have seemed to Hilbert to have sufficed for his purposes, as well as for preserving the validity of the theorems of mathematics. In model-theoretic terms, Hilbert's purpose was to develop axiomatic systems which were categorical. And, as defined by Oswald Veblen [1904, ?2], an axiomatic system Σ is *categorical* if for every model of Σ of countable cardinality κ, if every every first-order model of Σ is identical, *i.e.* if every model is isomorphic up to κ.

For Hilbert, as Ewald notes (p. 239), it is crucial that one's axiom set could prove every theorem—whether in Euclidean geometry or some other theory—that needed to be proven for the theory for which the axioms were established. Thus, Ewald distinguishes (p. 239) between deducing the theorems from the axioms and determining whether or not the axioms in fact are sufficient for their task. Metalogic was, that is to say, required to decide whether or not a given axiom set was complete, and for determining whether or not the axioms of the axiom set were dependent or independent. A related question, requiring metalogic rather than logic alone, is the categoricity of the system, that is, whether the axioms chosen not only derived, for a particular model, the theorems required, but also whether the axioms allowed deduction of all, and only those, theorems, for the particular model, and were valid for *every* such model appropriate to the axiom system. One of the purposes of Hilbert's work in the *Grundlagen der Geometrie*, indeed the main purpose, was to find the smallest set of axioms capable of deriving both Euclidean and non-Euclidean geometries, and was at once inclusive enough to derive those geometries without allowing derivation of extraneous or inconsistent theories, and still strong enough to allow for the deduction of the complete set of theorems across all possible models of those geometries. Therein lay the root of Hilbert's claim that it makes no difference whether the elements of our system, the primitives to which one?s axioms are applied, are interpreted as points, straight lines and planes, or beer mugs, tables and chairs. Categoricity, then, can be comprehended as validity across models or interpretations. In Ewald's words (p. 239), "Hilbert's investigations required him to sever the link between geometrical axioms and any particular domain of objects." For Hilbert, then, mathematics is no longer the science either of numerical quantity or of spatial magnitude, not even about any objects of any kind, but a hypothetical science about constructs postulated on the basis of, and deduced from an axiomatic set, and about the undefined entities that are the subject of the axioms. Hence the centrality of the problem of the consistency and completeness of the axiom system, of categoricity and the independence of the axioms. Whereas, Ewald says (p. 242), Russell's concern remained with epistemologicl certainty, Hilbert's was with "*Fruchtbarkeit*." Axiomatization, then, was but one step. Reduction of geometry seemed to work in the *Grundlagen der Geometrie*. The next step was to reduce all of mathematics, pure and applied, to arithmetic and to axiomatize each. "For Hilbert," Ewald tells us (p. 243), "as for Gauss, arithmetic was the fundamental mathematical science *überhaupt*..." And, although a relative consistency proof was employed by Hilbert to prove the consistency of geometry provided arithmetic is consistent, some other proof procedure would be requireed for proving the consistency of arithmetic itself. Ewald suggests (p. 243) that Hilbert had in mind a syntactic approach, but, in 1900, details were scarce and unclear. Hilbert's remarks in Königsberg in 1930, "Wir müssen wissen. Wir werden wissen.", was, after three decades, still a promise, and one whose path was still only partially mapped, a path before which Gödel, a day earlier at the same conference, had lain his incompleteness results.

The question of the combination of Hilbert's 1930 prescription of the impossibility of unsolvable mathematical problems and his 1900 problem list raise the issues for Ewald (p. 244) of the criteria of inclusion and exclusion employed by Hilbert in compiling his problem list, and of how Hilbert's first problem, of the consistency of arithmetic fits into this list and into Hilbert's conception of mathematics. Ewald notes (p. 244) that Fermat?s Last Theorem [FLT] was not included. This, he thinks, gives us a clue to the answers to each of these issues, of deciding what problems to include or exclude, and to deciding that—and how—consistency, or the decidability problem, is the key to Hilbert's view of mathematics. Ewald is convinced that Hilbert excluded FLT not because he did not think it an important problem, and certainly not because he thought it to be unsolvable; but because he thought that it had already made significant contributions to mathematics in the efforts already expended in the search for its solution, by creating much new mathematics and many new mathematical structures along the way, in the development of algebraic number theory. (On the impact of FLT for Hilbert?s program, see, *e.g.* [Velleman 1997].) So, does Ewald think that the decidability of FLT (solved in 1995 by Andrew Wiles [1995] with the assistance of Richard L. Taylor [Taylor & Wiles

1995]), and other hard problems, such as the Goldbach Conjecture, or the Riemann Conjecture in number theory, which Gödel himself gave as examples of undecidable theorems that counted as evidence for his incompleteness proof, or of such more recent hard problems as the Poincaré Conjecture (formulated in 1904, solved in 2006 by Grigorii Perelman), in fact support Hilbert?s contention? Ewald does not say.

The focus then, is evidently not on the assertion that we will, and must know, or the concomitant rejection out of hand of *ignoramibus*, but of the expectation of what new mathematics can be created by turning our attention to the problems that were selected, and, in particular, on the consistency problem, for providing an understanding of the nature of mathematics and a foundation for arithmetic and for all of mathematics. With Wilhelm Ackermann, and especially with Paul Bernays, the challenge, which Hilbert undertook in working for the remainder of his life on the first—and foremost—problem of consistency, led to the development of metamathematics, to concern for metalogica—or model-theoretic characteristics of logic, and to proof theory.

Before 1931, we had Hilbert-type axiomatic systems and Frege-type axiomatic systems, the most notable of the latter being Whitehead and Russell?'s *Principia*. But, simultaneously in 1932, Herbrand, and then Gentzen, undertook the investigation of what *being a proof* meant in Hilbert's system (see [Anellis 1991]), and this led to application of the Löwenheim-Skolem theorem to proof theory as well as to model theory and to the establishment of first-order logic as *the* mathematical logic (see [Moore 1987; 1988; 1997]), to Herbrand's work in quantification theory, and to the development by Gentzen of the sequent calculi and natural deduction (see [Anellis 1991]). Herbrand undertook to develop globally, as *validity*, Löwenheim's concept of model-theoretic *satisfiability*. *Validity* is now the proof-theoretic correlate of categoricity; it is *satisfiability* invariant with respect to any particular model, just as *categoricity* is *consistency* invariant with respect to any particular model.

In "The Hilbert program of metamathematical study of proofs", [Anellis 1994, 139] wrote, "just because the axiomatic method fails to study proofs even while it provides an analysis of theorems." Moreover [Anellis 1994, 139]:

> It was Hilbert who undertook to define and systematically carry out the construction of mathematics within his system. This required a fully developed concept of proof. The Hilbert program had two aspects. One was

> to define the technical apparatus which would permit a finitist construction of all mathematics, The other was to ensure that the set of (mathematical) sentences derived within the axiomatic system was consistent.

If Hilbert was not as certain as might have been expected that Gödel's incompleteness would undermine, if not altogether devastate, his own effort to carry out his program, it was because he anticipated that he (and Bernays) could redefine their finitism in such a way as to rescue the categoricity, validity, and completeness of arithmetic. Bernays, for one, agreed that Gödel's theorem demonstrated that the *Principia* could not formalize all of mathematics. Neither he, nor Hilbert, nor yet even Gödel himself, were prepared to suggest the same for Hilbert-type systems. Thus, for example (see [Zach 2003]) 'Gödel [1931] left open the possibility that there could be finitary methods which are not formalizable in these systems and which would yield the required consistency proofs. Bernays's first reaction in a letter to Gödel in January 1931 was likewise that "if, as von Neumann does, one takes it as certain that any and every finitary consideration may be formalized within the system P—like you, I regard that in no way as settled—one comes to the conclusion that a finitary demonstration of the consistency of P is impossible" [Gödel 2003, 87].'

For others, such as Saul Kripke, the impact of Gödel's incompleteness results was unconditionally definitive, however, and Kripke [2005] speaks of the "collapse of Hilbertism", writing:

> Gödel's own proof of the collapse of Hilbert's program is indirect in the sense that he produces an auxiliary statement (first incompleteness theorem) that leads to the collapse (second incompleteness theorem). I will show by a syntactic argument (infinite descent of actual numbers) that Hilbert's original program directly implies its own collapse. Another unusual feature of Gödel's independence proofs is, unlike other such proofs, they do not proceed by producing a model where the statement is false. I will show that this can be done, using a modified ultrapower construction or arbitrary initial segments of nonstandard models, that this can be done. ...

Ewald does not explicitly refer to this or to any other concrete discussions which take the position that Gödel's incompleteness theorems irremediably wrecked the Hilbert program, however.

Ewald's summary (p. 344) of the significant differences between Russell and Hilbert begins by noting that the paradoxes were, after all, important to Hilbert, and the need to deal with them resulted in much expended energy on Hilbert's part. But they played a significantly different role for Hilbert than for Russell. For Russell, Ewald tells us (p. 244), "the paradoxes were the *sole* reason for much of his logical work, and without them he would have had no reason to develop the theory of types of the axiom of reducibility." For Hilbert, however, "the paradoxes are a *subordinate* issue within a consistency program that has much deeper reasons for existing."

For Hilbert, the axiomatic method was the new tool for unifying and deriving all of mathematics, and, more essentially, for establishing thereby the validity of the theorems derived therefrom. Moreover, many of the mathematical structures to be deduced took their initial intuitive inspiration from the natural world. Real mathematical problems, including problems from both pure mathematics and applied, physics foremostly in the case of the latter, supplied the grist for Hilbert's axiomatic mill and, more importantly, provided one of the main supports for the axioms. Russell meanwhile continued to rely upon Platonic Ideals as the quintessential mathematical objects. Thus, when consistency failed in his system, so did certainty; and, more to the point, when certainty failed, so also did his world.

Ewald can be said, however, to have denigrated the full impact of the paradoxes in order the more to emphasize the radical differences between him and Hilbert and to emphasize thereby the mathematically deeper and more acute purposes of Hilbert. For Russell, the paradoxes challenged not merely the consistency of set theory, but the very possibility of the truth of mathematics, and therefore of the existence of mathematics itself and of mathematical objects. In effect, although Russell's purpose may be philosophical, even existential rather than merely epistemological, and what is at stake for Russell is more than the constructibility of mathematical objects, structures, and theories, as it was for Hilbert. What is at stake for Russell is the possibility of mathematics as the fundamental aspect of reality. Mathematical truth for Russell is absolute truth, and mathematical reality, although a Pythagorean-Platonic Ideal, *is* absolute reality, and hence the basis of and sole guarantor of epistemological apodictcity, of certainty, and not merely the certainty of empty tautology. (If Russell's encounter with Wittgenstein and Wittgenstein's *Tractatus Logico-Philosophicus* was so painful for Russell, as painful perhaps even as the paradoxes, it was because, according to the *Tractarian* understanding, logic, and by extension mathematics, was *empty* truth, comprised of nothing but tautologies behind which stood no Platonistic reality that could provide the certainty that Russell craved.) In this sense, Ewald is wrong to downplay, even if merely for the sake of contrast with Hilbert's conception of logic, the significance of Russell?s philosophical conception of logic, mathematics, and the import of the paradoxes for his views of logic and mathematics. Should we distinguish the *philosophical* purpose of Russell's logicism and logical approach to a foundation for mathematics on the one hand and Hilbert?s formalism and foundational, *metalogical*, approach to foundations of logic and mathematics on the other, we need be more attuned than Ewald apparently is, to the immense significance which their respective philosophical/metalogical concerns in their technical contributions, whether the focus of those concerns were with the technical apparatus designed to disarm the paradoxes and its consequences, as they were for Russell, or with the technical apparatus of proof theory developed for investigation of the conditions and characteristics of axiomatization designed to provide for the consistency of one's axiomatic system.

Still, for Hilbert, the selection of axioms and the fruitfulness of his axiomatic system did not alone suffice to underwrite the consistency of the system. The "*narrow*" Hilbert program had as its purpose, Ewald explains (p. 248), to "prove the consistency ultimately of formalized second-order arithmetic by finitary means." This concept of Hilbert's endeavor was what led the intuitionist to insist that Hilbert merely studied "meaningless" formal systems. Narrowly construed, Ewald concludes (p. 248), this program would indeed be demolished by Gödel's incompleteness proof. But the wider program, of deriving the theorems of all branches of mathematics, pure and applied, within an axiom system, deduced from a well-chosen set of axioms, such that the mathematical structures of pure and applied mathematics could be obtained, was the purpose of *Beweistheorie*, which would provide knowledge of the limitations of what could be achieved with, and within, the axiomatic system, and supplied the tools required for checking the system itself, the axioms and derivation rules of the system, to ascertain that the theorems obtained within the system withstand the test of validity, and of consistency. To Ewald?s characterization we may also add the following: Existence, in this approach, is constructibility. We no longer deal, as Russell sought to do, with mathematical entities that are abstract Ideals existing in a Platonic realm of pure Being.

It is this broader conception of the Hilbert program that Hilbert and Bernays adopted and was the source for their researches and which, as carried out by them and their successors, brought contemporary proof theory to its current state and made proof theory a central and pivotal feature of mathematics. Proof theory, then, however else Ewald might characterize it, is Hilbert's technical answer to the older problem of philosophy and foundations of mathematics. And Hilbert's successors, starting with Bernays, have moved it beyond a philosophical concern. Ewald (p. 248) quotes Hilbert [1922, §59] thus: "Just as the physicist investigates his apparatus and the astronomer investigates his location; just as the philosopher practices the critique of reason; so, in my opinion, the mathematician must secure his theorems by a critique of his proofs, and for this he needs proof theory."

Finally, we must add that, in the remaining years of Hilbert's lifetime, and despite the incompleteness proofs and the devastation it wreaked on *Principia* and *Principia*-like systems and wreaked or was supposed to have wreaked upon Hilbert's efforts, logicians, Gödel among them, were proving the consistency and completeness of important parts of logic. Gödel, just prior to proving his incompleteness results for first-order logic with identity, had proven the consistency of the propositional calculus and first-order logic without identity [Gödel 1929; 1930]. It was not yet clear that Gentzen's [1936] proof of the consistency of arithmetic was erroneous. By the mid-1950s, Leon Henkin would soon provide a reasonable expectation that there is a notion of finitary systems in which consistency could be achieved when he published his "Generalization of the Concept of ω-consistency" [Henkin 1954]. Henkin's "Generalization..." at least opens the possibility that there indeed could be finitary methods which are not formalizable in Hilbert-type systems and which would yield the required consistency proofs, with the concept of finitary properly redefined and refined. But Ewald, taking his history up to Hilbert's researches of 1917/18, does not take proper account either of Gödel's challenges or of the researches of Hilbert and his successors after 1917/18 to deal with the challenges posed either by Gödel or by Hilbert himself.

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