Hermann Weyl, the Reluctant Revolutionary

David E. Rowe

"Brouwer—that is the revolution!"—with these words from his manifesto "On the New Foundations Crisis in Mathematics" [Weyl 1921], Hermann Weyl jumped headlong into ongoing debates concerning the foundations of set theory and analysis. His decision to do so was not taken lightly: this dramatic gesture was bound to have immense repercussions, not only for him but for many others within the fragile and politically fragmented European mathematical community. Weyl felt sure that modern mathematics was going to undergo massive changes in the near future. By proclaiming a "new" foundations crisis, he implicitly acknowledged that revolutions had transformed mathematics in the past, even uprooting the entire edifice of mathematical knowledge. At the same time he drew a parallel with the "ancient" foundations crisis commonly believed to have been occasioned by the discovery of incommensurable magnitudes, a finding that overturned the Pythagorean world view based on the doctrine "all is Number." In the wake of the Great War that changed European life forever, the *zeitgeist* appeared ripe for something similar, but even deeper and more pervasive.

Still, revolutions cannot occur without revolutionary leaders and ideologies, and these Weyl came to recognize in Egbertus Brouwer and his philosophy of mathematics, which Brouwer originally called "neo-intuitionism" (in deference to Poincaré's intuitionism, see [Dalen 1995]). Weyl had known Brouwer personally since 1912, and had studied his novel contributions to geometric topology as a prelude to writing *Die Idee der Riemannschen Fläche* [Weyl 1913]. But the Brouwer he and most others knew back then was the brilliant topologist, not the mystic intuitionist Dirk van Dalen acquaints us with in his rich biography [Dalen 1999]. Weyl simply had not known the whole Brouwer, and probably never did. True, he regarded him as a kindred philosophical spirit, but he seems never to have referred to Brouwer's *Leven, Kunst en Mystiek* (*Life, Art, and Mysticism*) or any of his other more general philosophical writings, presumably because he never read them (all were written in Dutch). If so, this surely precluded any chance of fully understanding the vision behind Brouwer's views. Nevertheless, he was swept off his feet both by Brouwer's personality and by his revolutionary message for mathematics.

Weyl had been teaching since 1913 at the ETH in Zurich (on his career there, see [Frei and Stammbach 1992]). His conversion experience took place in the summer of 1919 while vacationing in the Engadin, where Brouwer, too, was staying. Their encounter was brief, lasting only a few hours, but long enough for Weyl to see the light. Afterward, Brouwer lent him a copy of his 1913 lecture on "Formalism and Intuitionism," but Weyl returned it, commenting that he already had "a copy . . . from the old days," presumably an allusion to the pre-revolutionary era. He further confessed that "at the time I did not pay attention to it or understand it. . . . " (Weyl to Brouwer, 6 May 1920, quoted in [Dalen 1999, p. 320]), a remark befitting a new disciple of the faith.

Discipleship played a crucial role in the social relations among the mathematicians of this era, and no one felt this more keenly than Hermann Weyl when he studied under David Hilbert in Göttingen. Hilbert's aura as a youth leader—the "Pied Piper of Mathematics"—was perhaps the most distinctive quality that separated him from all his contemporaries. He must have felt a mixture of guilt and relief when, as he later described it, "during a short vacation spent together, I fell under the spell of Brouwer's personality and ideas and became an apostle of his intuitionism" (Weyl Nachlass, Hs 91a: 17). Even young Bertus Brouwer was

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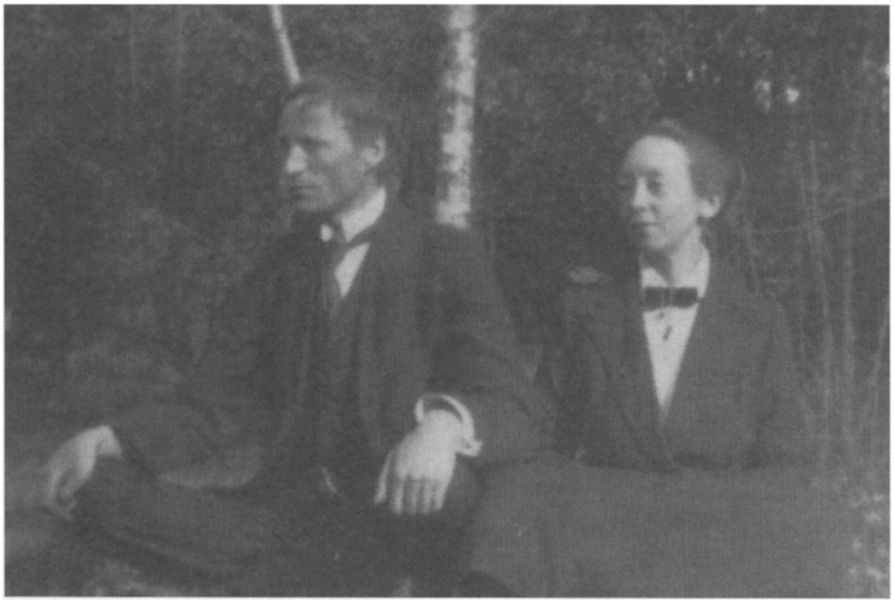
strongly attracted by Hilbert's alluring persona. He spent a considerable amount of time with him during the summer of 1909 when Hilbert was vacationing in Scheveningen, a seaside-resort town near the Hague. The first personal encounter left a deep impression on Brouwer, as he related to his friend, the poet Adama van Scheltema: "This summer the first mathematician of the world was in Scheveningen; I was already in contact with him through my work, but now I have repeatedly made walks with him, and talked as a young apostle with a prophet. He was only 46 years old, but with a young soul and body; he swam vigorously and climbed walls and barbed wired gates with pleasure. It was a beautiful new ray of light through my life." (Brouwer to Adama van Scheltema, 9 November 1909, quoted in [Dalen 1999, p. 128]).

Brouwer had already criticized Hilbert's axiomatic methods in his doctoral dissertation, submitted in 1907, where he concluded "that it has nowhere been shown, that if a finite number has to satisfy a system of conditions of which it can be proved that they are not contradictory then the number indeed exists" (quoted in [Dalen 2000, p. 127]). For his part, Hilbert clearly recognized that the axiomatic method could never show more than consistency, but he emphatically asserted that this was all a mathematician needed to prove in order to assert that a mathematical object exists. As van Dalen has observed, it would not have been like Brouwer to pass up this golden opportunity to explain his foundational ideas to Hilbert firsthand. Unfortunately, neither apparently left any notes of what they talked about while strolling through the sand dunes of Scheveningen, but nearly twenty years later Brouwer did refer to these discussions while lamenting that Hilbert had in the meantime appropriated some of his key intuitionist principles [Brouwer 1928].

In May 1920, the ink of his "New Crisis" manuscript barely dry, Weyl sent it off to Brouwer along with the above-cited letter in which he explained his motives. "It should not be viewed as a scientific publication," he informed his new-found ally, "but rather as a propaganda pamphlet, thence the size. I hope that you will find it suitable for this purpose, and moreover suited to rouse the sleepers. . . . " [Dalen 1999, p. 320]. That it certainly did. Weyl's provocative broadside caused the long-bubbling cauldron of doubts about set theory and analysis to boil over into what came to be known as the modern "foundations crisis," a slogan taken directly from the title of this essay. Brouwer responded with almost gleeful delight: "your wholehearted assistance has given me an infinite pleasure. Reading your manuscript was a continuous delight and your exposition, it seems to me, will also be clear and convincing for the public . . . " [Dalen 1999, p. 321].



Brouwer and his wife communing with nature in their garden (from [Dalen 1999], p. 63).

Among such delights was Weyl's use of politically inspired metaphor to convey a heightened sense of urgency. The antinomies of set theory, he wrote, had once been regarded as "border conflicts" in "the remotest provinces of the mathematical empire" [Weyl 1921, p. 143]. But now they could be seen as symptomatic of a deep-seated problem, till now "hidden at the center of the superficially glittering and smooth activity," but which betrayed "the inner instability of the foundations upon which the structure of the empire rests" (*ibid.*). Weyl likened the ontological status of objects whose "existence" depends on proof by *reductio ad absurdum* to currency notes in a "paper economy," whereas true mathematical existence was surely a "real value, comparable to food products in the national economy." Nevertheless, "we mathematicians seldom think of cashing in this 'paper money.' The existence theorem is not the valuable thing, but rather the construction carried out in the proof. Mathematics, as Brouwer on occasion has said, is more an activity than a theory" [Weyl 1921, p. 157].

With Bismarck's mighty German Empire now in shambles, Weyl clearly thought that the empire of modern analysis would soon fall, too. Its mighty fortress in Göttingen, led by the fearless and often ferocious Hilbert, had weathered all assaults up until now, but Weyl saw its walls cracking and prognosticated that they would soon come a crumblin' down, while the sage of Amsterdam stood ready to ride in and assume power. Weyl's defection to his intuitionist camp was clearly undertaken in order to tip the scales in the Dutchman's favor, thereby preparing the overthrow of the old regime. His manifesto, penned during the period of the abortive Kapp *Putsch* and its aftermath, reflected the mood of the times, when thoughts of revolution and counter-revolution abounded in Weimar Germany. Its principal target, of course, was his former mentor, Hilbert, who needed no rousing to see

Hermann Weyl, circa 1910, around the time he first became skeptical of Zermelo's axioms for set theory.

what was at stake. He struck back quickly, hard, and with plenty of polemical punch:

What Weyl and Brouwer are doing amounts, in principle, to a walk along the same path that Kronecker once followed: they are attempting to establish the foundations of mathematics by throwing everything overboard that appears uncomfortable to them and erecting a dictatorship [Verbotsdiktatur] *à la Kronecker. This amounts to dismembering our science, which runs the risk of losing a large part of our most valuable possessions. Weyl and Brouwer ban the general concept of irrational number function, . . . the Cantorian numbers of higher number classes, etc.; the theorem that among infinitely many whole numbers there is always a smallest, and even the logical "Tertium non datur" . . . are examples of forbidden theorems and arguments. I believe, that just as earlier when Kronecker failed to do away with irrational numbers . . . so, too, today will Weyl and Brouwer not succeed; no: Brouwer is not, as Weyl contends, the revolution but rather only the repetition of a* Putsch *attempt with old means. If earlier it was carried out more sharply and still completely lost out, now, with the state so well armed and protected through Frege, Dedekind, and Cantor, it is doomed to failure from the outset [Hilbert 1922, pp. 159–160].*

This tense encounter, pitting the all-powerful Hilbert against his most gifted pupil, stands out as one of the more dramatic episodes of twentieth-century mathematics. Yet despite all its high drama, Hermann Weyl's commitment to Brouwer's intuitionist program soon lost its intensity. By the mid-twenties, Brouwer had put an immense amount of energy into his program for revolutionizing mathematics, while in Göttingen Hilbert and Paul Bernays were just as busy developing proof theory as a bulwark of defense for classical analysis. By 1924, Brouwer had proved a series of results culminating in the theorem that every full function is uniformly continuous. Because these intuitionist findings had no counterparts in classical mathematics, Brouwer, who wasn't one to mince words, concluded that "classical mathematics is contradictory" [Dalen 1999, p. 376].

So where was Weyl? He largely stood by and watched this lively action from the sidelines, albeit with considerable interest. His flirtation with intuitionism seemed to many just that, a fleeting affair doomed from the start to end in disappointment. Weyl felt differently. To understand why, it will be helpful to glance back at his earlier interest not only in foundations of analysis but in mathematical physics as well. It was hardly an accident that these two fields coincided with Hilbert's principal research interests after 1910, as both men shared high hopes for breakthroughs in these two realms. In Weyl's case these took concrete form in 1918 with the nearly simultaneous publication of *Das Kontinuum* [Weyl 1918a] and *Raum-Zeit-Materie* [Weyl 1918b].

On the Roots of Weyl's Ensuing Conflict with Hilbert

Hilbert set out his early foundational views on a number of prominent occasions, but for the most part he preferred to evade direct controversy [Rowe 2000]. After 1904, when he delivered a highly polemical address on foundations issues at the Heidelberg ICM [Hilbert 1904], he remained virtually mute about these matters for over a decade. He did not return in earnest to research in this field until the late war years. Still, this hardly meant that he had lost interest. As Volker Peckhaus has described, Hilbert's longstanding efforts on behalf of Ernst Zermelo, who held a modest position in Göttingen teaching mathematical logic, as well as his support for the philosopher Leonard Nelson were part of Hilbert's long-term strategy aimed at providing institutional support for research in set theory, foundations, and mathematical logic [Peckhaus 1990, pp. 4–22]. Hilbert, now at the height of his career, had emerged as Göttingen's second great empire-builder. He did so, however, not so much by building on the groundwork Felix Klein laid in various branches of applied mathematics [Rowe 2001], but rather by promoting research that extended the territorial claims he himself had already staked out in analysis, number theory, foundations of geometry, and mathematical physics. Compared with the Hilbertian production lines in these fields, Göttingen research efforts in set theory and foundations resembled a mere cottage industry. Presumably Hilbert hoped that by delegating this research to specialists he could turn to other matters, in particular the foundations of physics, which dominated his attention after the death of Hermann Minkowski in 1909 [Corry 1999].

Skúli Sigurdsson has addressed the theme of "creativity in the age of the machine" in connection with Weyl, who during his student days had close associations with Hilbert's "factory" for integral equation theory [Sigurdsson 2001, pp. 21–29]. Hermann Weyl clearly never wanted an ordinary job on this fast-moving assembly line, and he later downplayed the value of much that came off it. In one of his two obituaries for his mentor, he wrote that it had been due to Hilbert's influence that "the theory of integral equations became a world-wide fad in mathematics . . . producing an enormous literature of rather ephemeral value" [Weyl 1944, pp. 126–127]. Nevertheless, the young Weyl took a keen interest in the work of E. Schmidt, E. Hellinger, O. Toeplitz, et al., and he also did a fair amount of mingling with his peers in the Göttingen mathematical community. This gave him ample opportunity to participate in discussions on set theory and foundations with Zermelo, whose proof of Georg Cantor's well-ordering theorem in 1904 led to an intense debate regarding the admissibility of Zermelo's axiom of choice [Moore 1982]. Four years later, in an effort to quell this controversy, Zermelo presented his well-known system of axioms for set theory [Zermelo 1908].

Soon thereafter, Weyl began to take a serious and active interest in set theory. Writing to his Dutch friend, Pieter Mulder, on 29 July 1910, he characterized his standpoint as closer to that of Emile Borel and Henri Poincaré than to Zermelo's views. But he also indicated that he would have to think these matters through very carefully, especially because he feared the controversy that typically ensued whenever issues in set theory and the foundations of analysis were addressed. Recalling these times, Weyl would later write: "I grew up a stern Cantorian dogmatist. Of Russell I had hardly heard when I broke away from Cantor's paradise; trained in a classical gymnasium, I could read Greek but not English" (Weyl Nachlass, Hs 91a: 17). Eight years later, Weyl alluded to the difficulties he encountered in trying to make sense of Zermelo's axiom system for set theory: "My investigations began with an examination of Zermelo's axioms for set theory. . . . Zermelo's explanation of the concept 'definite set-theoretic predicate,' which he employs in the crucial 'Subset'-Axiom III, appeared unsatisfactory to me. And in my effort to fix this concept more precisely, I was led to the principles of definition of §2 [in *Das Kontinuum*]" [Weyl 1918a, p. 48]. These principles were already enunciated in [Weyl 1910], a paper Solomon Feferman has discussed in connection with Alfred Tarski's ideas [Feferman 2000, p. 180].

Weyl described his initial orientation as similar to Dedekind's theory of chains, in that he sought to establish the principle of complete induction without recourse to the primitive notion of the natural numbers. This quest "drove me to a vast and ever more complicated formulation but, unfortunately, not to any satisfactory result." He finally abandoned this as a "scholastic pseudo-problem" after achieving "certain general philosophical insights," presumably derived from reading Edmund Husserl and distancing himself from Poincaré's conventionalism. Nevertheless, he concluded that Poincaré had been right regarding the status of the sequence of natural numbers as "an ultimate foundation of mathematical thought" [Weyl 1918a, p. 48].

What prompted Weyl to reenter this arena in 1918, a move that took his good friend, Erich Hecke, by surprise? Probably he had several motives, but he surely kept a keen eye on his mentor's activities, about which he had first-hand knowledge. On 11 September 1917, Hilbert delivered a lecture on "Axiomatic Thought" [Hilbert 1918] before a meeting of the Swiss Mathematical Society in Zurich. This gave the first clear signs that he was about to take up the foundations of mathematics once again. Probably no one in Hilbert's audience listened more attentively than Hermann Weyl, who discussed this lecture in detail many years later. For Hilbert's talk offered a sweeping panorama of mathematical and physical ideas that stressed not only their mutual interdependence but the role of axiomatics in both realms (see [Corry 1997] on Hilbert's background interests).

Like many of his contemporaries, Hilbert regarded the growth of mathematical knowledge as an essentially teleological process in which thought obeys higher, transcendental laws. As such, his positivism had something like an Hegelian flavor, only with the mathematician replacing the metaphysician as the highest human form of Reason. In his lecture, Hilbert described the manner in which axiomatization took place as part of a natural, organic process starting from an informal system of ideas ("*Fachwerk von Begriffen*"). These ideas, which arose spontaneously in the course of the theory's development, were merely provisional in nature. Only during the next stage, when researchers attempted to provide deeper foundations for the theory, did axiomatization actually begin. By invoking architectonic imagery, Hilbert suggested how this process structured scientific thought:

> *Thus arose the actual, present-day so-called axioms of geometry, arithmetic, statics, mechanics, radiation theory, and thermodynamics. These axioms build a deeper-lying layer of axioms than the axiom layer that was earlier characterized by the fundamental theorems of the individual fields. The process of the axiomatic method . . . thus amounts to a* deepening of the foundations *of the individual fields just as it becomes necessary to do with any building to the extent that one wants to make it secure as one builds it outward and upward [Hilbert 1918].*

In his concluding remarks, Hilbert mentioned two particularly pressing problems confronting the foundations of mathematics: proving the consistency of his axioms for arithmetic (the second of Hilbert's 23 Paris problems), and doing the same for Zermelo's system of axioms for set theory. He emphasized that both of these problems were wedded to a whole complex of deep and difficult epistemological questions of "specifically mathematical coloring": (1) the problem of the *solvability of every mathematical question in principle,* (2) the problem of the subsequent *verification* of the results of a mathematical investigation, (3) the question of a *criterion for the simplicity* for mathematical proofs, (4) the question of the relationship between *content and form* in mathematics and logic, and (5) the problem of the *decidability* of a mathematical question by means of a finite number of operations. Hilbert then summed up his position regarding all these complex issues as follows: "All such fundamental questions . . . appear to me to form a newly opened field of research, and to conquer this field—this is my conviction—we must undertake an investigation of the concept itself of the specifically mathematical proof, just as the astronomer must take into account the movement of his position, the physicist must care for his apparatus, and the philosopher criticizes reason itself" [Hilbert 1918, p. 155].

While conceding that, for the present, these ideas remained but a sketch for future research, Hilbert retained his optimistic outlook for his program:

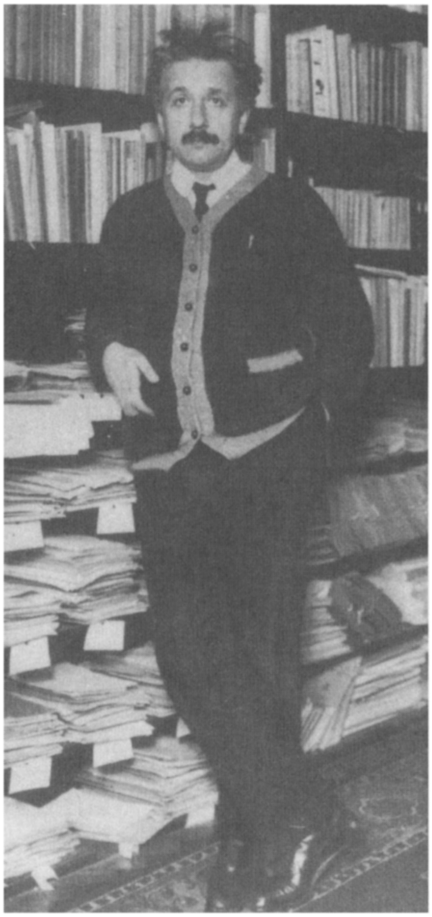
I believe that everything which can be the subject of scientific thought, as soon as it is ripe enough to constitute a theory, falls within the scope of the axiomatic method and thus directly to mathematics. By pursuing ever deeper-lying layers of axioms . . . we gain ever deeper insights into the essence of scientific thought itself, and we become ever more conscious of the unity of our knowledge. In the name of the axiomatic method, mathematics appears called upon to assume a leading role in all of science [Hilbert 1918, p. 156].

This *tour de force* performance clearly signaled Hilbert's intentions to take up once again the foundations program he had sketched thirteen years earlier in his speech at the Heidelberg ICM. Indeed, his rhetorical flourishes clearly echoed themes Weyl and others would have recognized from Hilbert's even more famous address at the Paris ICM in 1900. Just as striking, however, were the parallels with his concluding remarks from his first contribution to the general theory of relativity, in which he made similarly sweeping claims regarding the strength and resilience of the axiomatic method:

As one sees, the few simple assumptions expressed in Axioms I and II suffice by sensible interpretation for the development of the theory: through them not only are our conceptions of space, time, and motion fundamentally reformulated in the Einsteinian sense, but I am convinced that the most minute, till now hidden processes within the atom will become clarified through the fundamental equations herein exhibited and that it must be possible in general to refer all physical constants back to mathematical constants—just as this leads to the approaching possibility, that out of physics in principle a science similar to geometry will arise: truly, the most glorious fame of the axiomatic method, while here, as we see, the mighty instruments of analysis, namely the calculus of variations and invariant theory, are taken into service [Hilbert 1915, p. 407].

Tilman Sauer has recently noted how Hilbert, quite ironically, made only a vague allusion in his Zurich lecture to this vision for a unified field physics [Sauer 2002]. Weyl could not have failed to notice that Hilbert sounded very subdued about these prospects on this occasion. He also knew very well what Einstein thought of Hilbert's methodological approach. In a letter from November 1916, Einstein confessed:

To me Hilbert's Ansatz *about matter appears to be childish, just like an infant who is unaware of the pitfalls of the real world. . . . In any case, one cannot accept the mixture of well-founded considerations arising from the postulate of general relativity and unfounded, risky hypotheses about the structure of the electron. . . . I am the first to admit that the discovery of the proper hypothesis, or the Hamilton function, of the structure of the electron is one of the most important tasks of the current theory. The "axiomatic method," however, can be of little use in this (Einstein to Weyl, 23 November 1916 [Einstein 1918a, p. 366]).*



Einstein relaxing in his home office in Berlin. By 1918 he was carrying on an extensive scientific correspondence.

Weyl took up these problems around this very time. In the summer semester of 1917 he offered a lecture course on general relativity, and, on the advice of Einstein's close friend Michele Besso, he decided to adapt his notes into a book on special and general relativity. This was published the following year by Julius Springer Verlag as the first edition of *Raum-Zeit-Materie* [Weyl 1918b]; a second soon followed, and three substantially revised editions appeared between 1919 and 1923. A few months before it came out, however, Weyl had proofs sent to both Einstein and Hilbert. Their respective reactions reveal a good deal about both men.

Einstein was euphoric: "it's like a symphonic masterpiece. Every word has its relation to the whole, and the design of the work is grand" (Einstein to Weyl, 8 March 1918 [Einstein 1918b, pp. 669–670]). A week earlier, Hilbert wrote also, but he merely acknowledged receipt of the proofs (Hilbert to Weyl, 28 February 1918, Weyl Nachlass, Hs. 91: 604). Because he was on his way to Bucharest to attend a meeting on space and time in physics, he had no time to read them. Still, he expressed regret that he would not be able to meet Weyl in Switzerland over the semester break, but hoped to do so during the summer or early the following year. He then added some remarks about the professorship in Breslau recently offered to Weyl.

Hilbert had been consulted during the deliberations over potential candidates, and he had apparently recommended Weyl for the post. But he now counseled him against accepting the offer "*im Interesse des Reichsdeutschtum*," because if he left Zurich this would probably leave some worthy German mathematician without a job. Hilbert took a very active role in this game of mathematical musical chairs, and he apparently thought that Weyl should pass on this round. He also thought the Prussian government would be receptive to this argument, so that declining would not have unfavorable consequences for Weyl's future. Apparently Weyl was not inclined to follow this advice; at any rate, in April he accepted the chair in Breslau (although he would later turn it down for health reasons). Hilbert, having returned from Bucharest, wrote him on 22 April, sending "congratulations on accepting the Breslau position," but quickly added "unfortunately this again creates a vacant mathematical position in Switzerland that will be difficult to fill and unlikely so with a suitable personality."

Miffed that Weyl had ignored his wishes, Hilbert added some curt praise for *Raum-Zeit-Materie*: "I have looked more carefully at the proofs of your book, which gave me great pleasure, especially also the refreshing and enthusiastic presentation. I noticed that you did not even mention my first Göttingen note from 1915. . . ." He then proceeded to rattle off a litany of complaints bearing on this omission, remarks that provide insight into what Hilbert himself saw as the main achievements in his controversial paper on "Foundations of Physics" [Hilbert 1915]. (For details, see the accompanying box.)

22 April 1918

Dear Herr Weyl,

above all my congratulations on accepting the Breslau position, unfortunately this again creates a vacant mathematical position in Switzerland that will be difficult to fill and unlikely so with a suitable personality.

Having returned from Bucharest I have looked more carefully at the proofs of your book which gave me great pleasure, especially also the refreshing and enthusiastic presentation. I noticed that you did not even mention my first Göttingen note from 1915 even though the foundations of the gravitational theory, in particular the use of the Riemannian curvature in the Hamiltonian integral which you present on page 191, stems from me alone, as does the separation of the Hamiltonian function in H-L, the derivation of the Maxwellian equations, etc. Also the whole presentation of Mie's theory is precisely that which I gave for the first time in my first note on the foundations of physics. For Einstein's earlier work on his definitive theory of gravitation appeared at the same time as mine (namely in November 1915); Einstein's other papers, in particular on electrodynamics and on Hamilton's principle appeared however much later. Naturally I am very gladly prepared to correct any of my mistaken assertions. It also appears odd to me that where you do take note of the fact that there are four fewer differential equations for gravitation than unknowns you only cite my second communication in the *Göttinger Nachrichten*, whereas precisely this circumstance served as the point of departure for my investigation and was especially enunciated as a theorem whose consequences were pursued there. . . .

With best greetings to you and your wife,

Your Hilbert

Draft of letter from Hilbert to Weyl, NSUB, Hilbert Nachlass 457, 17

Weyl received this letter in time to make modifications in the text before the book went to press. He added a few citations and brief remarks on Hilbert's first note, but these remained shadowy features of his book compared with his own contributions and, of course, Einstein's. Hilbert could not have felt gratified by this, especially after reading the preface, which Weyl wrote while vacationing at his in-laws' home in Mecklenburg. He could hardly have failed to notice his protégé's animus against his views on the axiomatization of physics. At the same time, Weyl announced that he had found a new avenue to a truly unified field theory:

The Einsteinian theory in its present form ends with the duality of electricity and gravitation, "field" and "ether"; these remain totally isolated and stand next to one another. Just now a promising path has opened for the author for the derivation of both realms of phenomena from a common source by an extension of the geometrical foundations. Evidently the development of the general theory of relativity had not yet been concluded. However, it was not the intention of this book to take the powerful, stirring life that stems from the field of physical knowledge to the point it has presently reached and transform it with axiomatic rigor into a dead mummy. [Weyl 1918b, p. vi]

If Weyl saw general relativity as opening new vistas for the geometrization of physics, he also took up the challenge Hilbert had laid down for foundations of analysis. Indeed, immediately after Hilbert spoke about this in Zurich, Weyl began a lecture course in the winter semester of 1917–18 titled "Logical Foundations of Mathematics." One of his potential auditors, Paul Bernays, was unable to attend since Hilbert had already snatched him up as his new assistant in Göttingen. Thus began the collaboration that enabled Hilbert to push forward proof theory in earnest [Sieg 2000]. In the meantime, Weyl used his lectures as a platform for writing a booklet on *The Continuum* [Weyl 1918a]. This aimed to salvage the foundations of analysis within the more modest scope of a constructivist program that avoided the complexities of a full-blown axiomatic set theory à la Zermelo. Its approach to the continuum thus clearly exposed the differences between Weyl's constructivist views and those of Hilbert. Weyl only gradually became aware of this rift, but by 1918 he stood firmly on the other side of a deep abyss that separated his approach to foundations of analysis from conventional wisdom on this subject.

Even in Zurich, Weyl's ideas encountered strong opposition. His younger colleague, Georg Pólya, whom he deeply respected as an analyst, reacted incredulously, so much so that Weyl challenged him to a mathematical wager. He rounded up a dozen witnesses who validated a document stating that:

Within 20 years Pólya and the majority of representative mathematicians will admit that the statements

1. *Every bounded set of reals has a precise supremum*
2. *Every infinite set of numbers contains a denumerable subset*

contain totally vague concepts, such as "number," "set," and "denumerable," and therefore that their truth or falsity has the same status as that of the main propositions of Hegel's natural philosophy. However, under a natural interpretation (1) and (2) will be seen to be false. [Pólya 1972]

Thus, already by this time, Weyl's heretical views were well known within Zurich's mathematical circles. They would become even better known after he completed the manuscript of [Weyl 1918a] and followed it up with [Weyl 1919], a note on the "vicious circle" in conventional foundations of analysis that appeared in the widely read *Jahresbericht* of the German Mathematical Society.

In *Das Kontinuum*, he described the programmatic features of his new treatment of the continuum as follows:

[I]n spite of Dedekind, Cantor and Weierstrass, the great task which has been facing us since the Pythagorean discovery of the irrationals remains today as unfinished as ever; that is, the continuity *given to us immediately by intuition (in the flow of time and in motion) has yet to be grasped mathematically as a totality of discrete "stages" in accordance with that part of its content which can be interpreted in an "exact" way. [Weyl 1918a, pp. 24–25]*

Weyl insisted on the need to avoid the problem of impredicativity which arose when the set of real numbers was defined by means of arbitrary Dedekind cuts. Hilbert had tried to finesse this problem with his *Vollständigkeitsaxiom*, but in later editions of his *Grundlagen der Geometrie* he was forced to fall back on Dedekind cuts to prove that his axiom system characterized analytic geometry over the field of real numbers. Weyl's general philosophical orientation stood in sharp contrast to Hilbert's brand of rationalism, so it should not be surprising that he distanced himself from the fundamental tenets underlying his mentor's foundations research. Still, *Das Kontinuum* represented only a relatively mild break with Hilbertian precepts. Unlike Brouwer's approach, which took the continuum to be an irreducible given requiring the mathematician to master the new techniques of choice sequences, Weyl's goal in [Weyl 1918a] was far more pragmatic. As an analyst, he was less concerned with capturing the "essence" of the continuum than he was in extracting from it a sufficiently rich arithmeticized substructure which the "working mathematician" could use to recover most of the results of classical analysis (for more on this, see [Feferman 2000]).

Immediately after publishing *Das Kontinuum* and *Raum-Zeit-Materie*, Weyl began elaborating his gauge-invariant approach to a unified field theory. Einstein called it "a first-class stroke of genius," but nevertheless a useless one for physics (Einstein to Weyl, 6 April 1918 [Einstein 1918b, p. 710]). Arnold Sommerfeld had no such reservations, at least initially:

What you are saying there is really wonderful. Just as Mie had grafted a gravitational theory onto his fundamental theory of electrodynamics that did not form an organic whole with it . . . Einstein grafted a theory of electrodynamics . . . onto his fundamental theory of gravitation that had little to do with it. You have worked out a true unity (Sommerfeld to Weyl, 7 July 1918, Weyl Nachlass, 91: 751).

Apparently Weyl had no difficulty persuading Hilbert, either. As he had promised, his former mentor visited Weyl during September when he was vacationing in Switzerland. Whether or not they discussed foundational issues is unclear, but Weyl did report to Einstein that Hilbert "gave me his unqualified support" for the new unified field theory (Weyl to Einstein, 18 September 1918 [Einstein 1918b, p. 879]). Nevertheless, Einstein continued to express skepticism, so much so that Weyl wrote him in December: "I am now in a really difficult position; by nature so conciliatory that I'm almost incapable of discussion, I now must fight on all fronts: my attack on analysis and attempt to found it anew is encountering much more fierce opposition from the mathematicians who work on these logical things than my efforts in theoretical physics have received from you" (Weyl to Einstein, 10 December 1918 [Einstein 1918b, p. 966]).

By now Weyl was grappling not only with the foundations of classical analysis and mathematical physics but also with core issues that led to dramatic new developments in differential geometry. Weyl described his research program in 1921 as follows:

It concerns, on the one hand, impulses to a new foundation of the analysis of the infinite, the present foundation of which is in my opinion untenable; on the other hand, in close connection with this *[my emphasis] and in connection with the general theory of relativity, a clarification of the relation of the two basic notions with which modern physics operates, that of* field of action and of matter, the theories of which can at present by no means be joined together continuously *[Weyl's emphasis] [Dalen 2000, p. 311].*

Erhard Scholz has emphasized the extent to which Weyl's mathematical

A gathering of Zurich mathematicians during the visit of Jósef Kűrschák (right front row), who came from Budapest along with Frédéric Riesz (third from left). Pictured, from left to right, are Hermann Weyl, Louis Kollros, Riesz, Georg Pólya, Michel Plancherel, Jerome Franel, Mrs. Kűrschák, (unknown), Andreas Speiser, Kűrschák, and Rudolf Fueter. (From George Pólya, *The Pólya Picture Album: Encounters of a Mathematician,* ed. G. L. Alexanderson, Boston: Birkhäuser, 1987, p. 75.)

work was intertwined with physical and epistemological problems [Scholz 2001b]. All three of these fast-converging interests were clearly very much on his mind when he first learned about Brouwer's intuitionism.

Weyl's Emotional Attachment to Intuitionism

After meeting Brouwer in the summer of 1919, Weyl presented his provisional ideas on the "new crisis" the following December at three successive meetings of the mathematics colloquium, which he co-chaired with Pólya. The topics he covered were identical with those eventually presented in his "propaganda pamphlet," and apparently he made no attempt to tone down his enthusiasm for intuitionism when he delivered these lectures. Not surprisingly, Georg Pólya reacted just as vigorously in criticizing Weyl's philosophical claims in defense of intuitionism. According to notes taken by Ferdinand Gonseth, they exchanged words like these:

> PÓLYA: You say that mathematical theorems should not only be true, but also be meaningful. What is meaningful?
> WEYL: That is a matter of honesty.
> PÓLYA: It is erroneous to mix philosophical statements in science. Weyl's continuum is emotion.
> WEYL: What Pólya calls emotion and rhetoric, I call insight and truth; what he calls science, I call symbol pushing (*Buchstabenreiterei*). Pólya's defense of set theory . . . is mysticism. To separate mathematics, as being formal, from spiritual life, kills it, turns it into a shell. To say that only the chess game is science, and that insight is not, *that* is a restriction [Dalen 1999, p. 320].

Weyl was not vituperative by nature, but he clearly hoped his ideas would create a stir. He especially wanted to arouse Hilbert from his dogmatic slumbers, something Brouwer had been unable to accomplish himself. Hard at work on his "New Crisis" paper, Weyl wrote Bernays in Göttingen (9 February 1920, Weyl Nachlass, 91: 10) that he had returned to foundations of analysis, and had "substantially modified [his] standpoint" thanks to Brouwer's work. Referring to their meeting the previous summer, he wrote, "I was completely happy during the few hours we spent together." And he described Brouwer as a real "devil of a fellow (*Mordskerl*)" and a wonderfully intuitive person, adding that "if you get him to come to Göttingen as Hecke's successor, then I envy you." The departures of Hecke and Carathéodory, two of Hilbert's most prominent protégés, reflected the fragile state of German academic life amid the surrounding political uncertainties. Brouwer turned down both chairs, but not before negotiating improved conditions in Amsterdam [Dalen 1999, 300–304].

Weyl's attraction to intuitionism and his feelings toward its founder were confirmed when he visited Brouwer at his home in Blaricum a few months after completing his "New Crisis" essay. Writing to Felix Klein, he proclaimed that "Brouwer is a person I love with all my heart. I have now visited him in his home in Holland, and the simple, beautiful, pure life in which I took part there for a few days, completely and totally confirmed the impression I had made of him" [Dalen 1999, p. 298]. Even after this passionate phase had passed, Weyl always extolled Brouwer's positive achievement, which he saw as clarifying the essential tension between form and content, a dichotomy he took to be characteristic of mathematical thought.

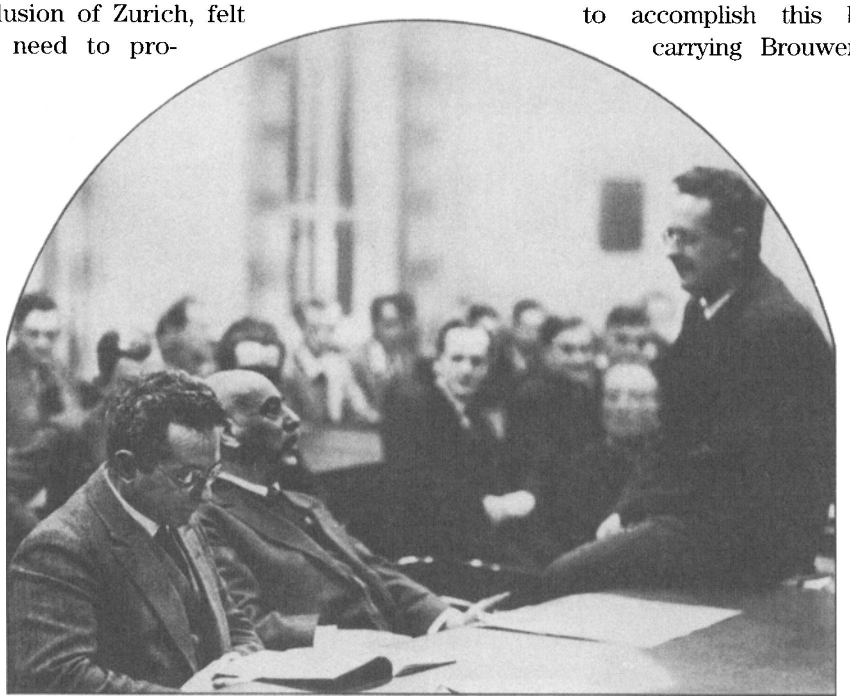
Yet Weyl was never an orthodox intuitionist, not even when he presented himself as a Brouwerian in his "New Foundations Crisis" [Scholz 2000, pp. 199–200]. What appealed to him most about Brouwer's philosophy was its honesty and its human dimension. He shared Brouwer's aversion to Hilbert's overly sanguine, winner-take-all approach to mathematics that bordered on intellectual dishonesty, just as he deplored the tendency to reduce the quest for mathematical meaning and truth to a meaningless formalist game. Brouwer refused to accept *carte blanche* the paper currency of pure existence proofs unless it could first be demonstrated that this money could actually be used to purchase a genuine mathematical article. Intuitionism thereby threatened the weakest flank in Hilbert's fortress by exposing the ontological assumptions of formalist dogma while advancing a vision for a new mathematics based on constructive principles. For Brouwer, Hilbert's formalist doctrine amounted to an impoverished view of mathematical activity, for it aspired merely to show that certain purely formal procedures could never produce a contradiction.

Brouwer's views thus had a twofold significance for Weyl. First, they helped him focus on conflicts he had long been bothered by but had never quite confronted, including his general dissatisfaction with Hilbert's claims regarding the axiomatic method. Second, the clarity of Brouwer's critique gave Weyl the courage to articulate his own views in strong, provocative language that could not be ignored. In his eyes, intuitionism was an ideology that dignified mathematical research as a human activity during a time of crisis. Still, it was only one element within the larger framework of ideas Weyl hoped to synthesize.

Many intellectuals—Weyl, Brouwer, Hilbert, and Einstein included—were acutely aware that they were living through revolutionary times when the whole "civilized" world was seeking new leaders, structures, and answers. But Weyl alone, working in the quiet seclusion of Zurich, felt the need to promote a new intellectual reorientation that would encompass both mathematics and physics. For Weyl, Einstein's general theory represented nothing less than a revolution in human thought, a view shared by Hilbert. Yet Weyl went further; his pioneering work in differential geometry was motivated by the urge to explore the implications of space-time theories in order to reach a deeper conceptual understanding of the physical world. Moreover, he was convinced that this simultaneous revolution in mathematics and physics was already underway, having burst into the public eye well before the sudden collapse of Imperial Germany that ended the Great War.

Yet despite his sensitivity to these swirling intellectual currents, Weyl was not by temperament an iconoclastic or revolutionary thinker. Moreover, he lacked the hard-nosed, combative streak that made both Hilbert and Brouwer such forceful personalities. By announcing that analysis was undergoing a foundational crisis, Weyl sought to expose the hollowness of Hilbert's rhetoric and at the same time underscore his long-standing misgivings with Cantorian set theory, which Hilbert and Zermelo had sought to retain and rigorize by means of axiomatics. He was, however, more than happy to accomplish this by carrying Brouwer's banner forward rather than holding up one of his own making.

The political imagery in his "New Crisis" essay was all very apt, but the Oedipal dimensions of this conflict would seem even harder to ignore. For however strongly Weyl may have felt about Brouwer, he must have sooner or later realized that he had very little in common with him. In everything from their personalities and life styles to their philosophical views and mathematical tastes, he and Brouwer were opposites. Clearly, his feelings for Brouwer and his attachment to intuitionism were genuine, but just as clearly they were a means to gain some emotional distance from his tyrannical *Doktorvater*, a man intent on directing the course of his career. Intellectually, he knew how much he owed Hilbert, and he largely shared his ambitions for the "new mathematics" as well as his vision for a unified field physics of gravitational and quantum phenomena. As the heir-apparent of the Göttingen master, Weyl must have felt almost predestined to compete with him on these two major fronts. When seen from this vantage point, his brief flirtation with Brouwer's intuitionism appears less Olympian, more human, and in some sense more believable.



Weyl, far right, listening attentively to Edmund Landau, while Richard Courant stares at a book. Presumably taken in Göttingen around 1930.

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