

## Chapter 14

# The Three Battles

If mathematics would for the sake of its safety seriously retire to this status of pure game, it would no longer be a determining factor in the history of mind.

H. Weyl

### 14.1 The *Grundlagenstreit*

We left the foundational debate in a fairly peaceful state. Hilbert's talk at the Weierstrass conference in 1925 was rather moderate in tone; there were a few references to dissident views, but more admonishing than belligerent in tone. Apart from a repeat of the Münster lecture in Copenhagen no further major activities took place that year. In November Hilbert was diagnosed with pernicious anaemia; he had not been well for some time, but the actual cause was not discovered earlier. At that time the disease was more often than not fatal. The immediate consequence was a serious decline in working power, and the disease took its toll on Hilbert's intellectual activities. Hilbert, nonetheless, tried to carry on his research as usual.

The years 1925–1926 were for Brouwer mainly filled with duties that had little to do with research. He spent time editing the Urysohn memoir for *Fundamenta*, and the necessary correspondence with Sierpinski and Alexandrov.

As an editor of the *Annalen* he was also very much occupied with the new influx of topological papers of, among others, Alexandrov, Urysohn, Menger, and Tumarkin. In between he found time to work on his contribution for the Riemann volume.

The foundational debate was watched with interest in mathematical circles, but only a handful of mathematicians actually took a position. If they did, it was mostly on the formalist side. The choice was, more often than not, determined by feelings of loyalty—any student or alumnus of the Göttingen university fostered 'right-or-wrong my teacher' feelings.<sup>1</sup> Another argument for siding with the formalists was

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<sup>1</sup>Cf. Lietzmann (1942).

of a pragmatic nature—life with PEM is easier than without it. It is, however, fair to say that the majority of the mathematical community deplored the hostilities of the last few years. A perfect occasion for a peaceful exchange of ideas arose when in the summer of 1926 Brouwer once more visited Göttingen.

Alexandrov, who was already in Göttingen, later told the charming story of how the two opponents came to lay down their arms.<sup>2</sup> Brouwer, he wrote, was immediately at home, as in the old days, in the circle of mathematicians. He was warmly welcomed in the intimate circle around Courant and Emmy Noether. The ill-feelings and suspicions having melted like snow for the sun, plans were made to get Brouwer and Hilbert together again. One evening a group of mathematicians, including Hilbert and Brouwer, was invited to Emmy Noether's apartment. And so Brouwer, Hilbert, Courant, Landau, Alexandrov, Hopf and a few other young mathematicians found themselves at the table under the roof of Emmy's friendly quarters (Landau used to question the validity of Euler's polyhedron theorem for this room). It fell to Alexandrov to start a conversation that would break the ice between Hilbert and Brouwer. Alexandrov's solution was as ingenious as simple: what better expedient was there to bring two persons together, than the mentioning of a third person both enjoyed criticising. Alexandrov cleverly introduced the acknowledged paragon of vanity Paul Koebe (in his paper he discretely referred to the well-known function theorist of Luckenwalde). The trick worked better than could be expected, it did not take long before Brouwer and Hilbert were outbidding each other in criticism of poor Koebe, 'at the same time they were nodding more and more friendly at each other, until finally they completely agreed in a mutual toasting'. The thus established peace lasted for the duration of Brouwer's visit. Even his lecture in the Göttingen mathematical society on 22 July 1926, 'On everywhere- and seemingly everywhere defined functions',<sup>3</sup> could not disrupt the peace. There are no notes of the lecture, but one may safely assume that it contained material from Brouwer's paper for the Riemann volume.

After his stay in Göttingen, Brouwer pilgrimaged in the company of Alexandrov to Batz to commemorate the lamented Urysohn. The topological fire in Brouwer had apparently not been extinguished completely, for Alexandrov wrote excitedly to Hopf that 'Brouwer has proved a topological theorem'.<sup>4</sup>

Less than a week later Brouwer was to be found at one of those meetings where he could indulge in the spiritual side of life. He attended a Sufi-meeting in France with a lady named 'Mies'. 'I am undergoing in humility the first impacts of the Sufi-order' he wrote to Bertha Adama van Scheltema, the sister of his deceased friend.<sup>5</sup> Brouwer was no stranger to spiritual intermezzo's of this kind. It is not easy to guess what interested him more, the spiritual experience and reflection offered, or the attraction of the colourful participants.

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<sup>2</sup>Alexandrov (1969).

<sup>3</sup>*Überall und scheinbar überall definierte Funktionen.*

<sup>4</sup>Alexandrov to Hopf, 12.VIII.1926.

<sup>5</sup>Brouwer to B. Adama van Scheltema, 22.VIII.1926.

Not much later Brouwer turned up at the Düsseldorf meeting of the DMV, where he delivered on 23 September his talk ‘On domains of functions’.<sup>6</sup>

Hilbert, in the meantime, was very poorly. He had his ups and downs, but generally speaking there was not much improvement.

In Amsterdam the faculty did something that certainly must have pleased Brouwer, and one is tempted to see his hand in it. In the meeting of October 24 the faculty decided no longer to admit philosophy as a major in the curriculum. Four years ago his colleague Kohnstamm had managed by clever manoeuvring to get philosophy accepted as a major, and Brouwer, in spite of his sharp intelligence, had lost that battle.<sup>7</sup>

The next significant event in the *Grundlagenstreit* was Hilbert’s second talk in Hamburg in July 1927. His health had somewhat improved, but it was a tired old man who appeared at the rostrum. Nonetheless, the event was a tremendous success. Courant, who accompanied Hilbert to Hamburg, reported to Springer, ‘The days in Hamburg with Hilbert were in every respect most satisfying. Hilbert’s condition was so good, that he could speak for an audience of over 100 with a soft and tired voice, it is true, but with intense temperamental outbursts and very impressive, and that he even took part in a big festive dinner in the evening of the second day. The whole occasion was a great triumph for him and did him psychologically exceptionally good, without harm to the body. Objectively, his blood picture has strongly improved and now our greatest hope is a liver cure. It will be, however, hardly possible to get Hilbert to eat liver in the required large quantities, but we have cabled to Harvard medical school, and we will probably soon get the liver preparation produced there, from which we expect much.’<sup>8</sup> The liver cure started not long after the lecture, followed indeed by the American preparation. The influence of American colleagues and students had certainly been helpful to introduce Hilbert to this newly discovered treatment.<sup>9</sup> This was by no means the end of the story; the right dose had to be found, and annoying and painful symptoms reared their heads. It was a surprising proof of the success of the treatment—and the determination of Hilbert—that he was again one of the main speakers at the annual meeting of the DMV in Leipzig. His topic was ‘The axiom of choice in mathematics’. The title is somewhat misleading, as ‘choice’ referred here to the  $\varepsilon$ -operator.

The Hamburg lecture of 1927 was, like its predecessor of 1921, published in the *Abhandlungen des Hamburgischen Seminar*, this time with the simple title *The foundations of mathematics*.<sup>10</sup>

The paper was in content a leisurely exposition of the Infinity-paper, a didactical introduction to the proof theory of arithmetic and analysis, laced with some popular foundational motivation, and the usual pot-shots at dissenters. Again, we must bear

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<sup>6</sup>Über Definitionsbereiche von Funktionen.

<sup>7</sup>Cf. p. 352 ff.

<sup>8</sup>Courant to Springer, 2.VIII.1927.

<sup>9</sup>See Reid (1970), Chap. 21.

<sup>10</sup>*Die Grundlagen der Mathematik*, Hilbert (1928).

in mind that the paper should be excused for its heavy handed propaganda, since it was doubtlessly the sort of language that could and would rouse an audience. One cannot read the paper without being touched by the belief of the author in his philosophical credo, and by his fighting spirit. Here was a man who had discovered the final answer to the foundational problems of mathematics. The wisdom of publishing all these belligerent remarks may be doubted. As a rule they reflect on the author, and moreover, they stand in the way of a proper scientific discussion. In particular, the formulations used by Hilbert tended to demonise the opponent. One might almost conclude that Hilbert did not want a *discussion*, he wanted to *win*.

As in earlier publications, Hilbert was determined not to give up the *Tertium non datur*. After pointing out that from a finitist point of view the negation of statements like  $\forall x A(x)$ , where each instance  $A(n)$  can be finitistically checked, is problematic, he continues:

But we cannot give up the use of the applications of the *Tertium non datur* or any of the other laws of Aristotelian logic which are expressed in our axioms, as the construction of analysis is impossible without them.

It may be remarked here that Hilbert was too pessimistic about a *Tertium non datur*-free mathematics. In due time work in the intuitionistic school and above all the results of the school of Erret Bishop were to give a powerful impetus to constructive mathematics by actually rebuilding large parts of analysis in a constructive manner. He showed that ‘constructive’ does not stand for ‘ugly and cumbersome’. He showed convincingly that constructive mathematics possesses the same elegance as classical mathematics.<sup>11</sup> We know now that (i) a substantial part of analysis can be developed in the intuitionistic frame work, (ii) the proofs are not as cumbersome as people in Hilbert’s day expected, and (iii) that the mathematical problems arising in the Brouwer–Bishop tradition are interesting and rewarding. Of course, it would be anachronistic to expect Hilbert to see this point. Hilbert’s solution was exemplary for the methods of the working mathematician. If you cannot solve the equation  $x^2 + 1 = 0$ , extend your system with  $i$  ( $= \sqrt{-1}$ ). So if you cannot negate and quantify at the same time, add the ideal statements (which allow negation and quantification), and add *Tertium non datur*. It is like the extension of an algebraic structure. And then, of course, show the consistency.

After a quick survey of some of the tools and methods of proof theory, Hilbert returned to answer the various objections that had been raised, and which he found ‘one and all as unjustified as possible’. He started by reconsidering Poincaré’s objections against his consistency proofs that intended to safeguard induction by applying induction. This time Hilbert elaborated his defence, as promised in 1922, cf. p. 440; Poincaré, he said, had not understood the distinction between conceptual induction and formalised induction. On this point Hilbert’s position was weak, to say the least. Unless he managed to pull off the finitist consistency proof that he had announced, he could not uphold his own view. Brouwer and Poincaré had a better intuition on

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<sup>11</sup>See Bishop (1967) and Bishop and Bridges (1985).

this point,<sup>12</sup> Hilbert saw Poincaré's negative comments as a consequence of his pronounced prejudice against Cantor's theory. The argument does not convince, why should a mistrust in set theory lead to a rejection of a combinatorial practice such as proof theory?

On the whole Hilbert was not impressed by what he read in the foundational literature. Most of it he considered as backward, as if belonging to the pre-Cantor period. Since Hilbert's yardstick was calibrated by the continuum hypothesis, Hilbert's dogma, 'consistency  $\Leftrightarrow$  existence', and the like, he was by definition right. But if one is willing to allow other yardsticks, no less significant, but based on alternative principles, then Brouwer's work could not be written off as obsolete nineteenth century stuff.

However, Hilbert clearly saw Brouwer as his prominent opponent, and thus he spent the rest of the paper giving him a thorough roasting. As he put it: 'By far the largest amount of space in the present literature on the foundations of mathematics is taken up by the doctrine that Brouwer has formulated and called intuitionism. Not out of an inclination for polemics, but to express my views clearly, and to prevent misconceptions about my own theory, I have to go further into certain statements of Brouwer.'<sup>13</sup>

There are actually two main points that Hilbert singled out for comment: the meaning of existence and the *Tertium non datur*. 'Brouwer declares existential statements by themselves to be one and all meaningless, insofar they do not at the same time contain the construction of object that is asserted to exist, for worthless scrip: it is through them that mathematics degenerates into a game.'<sup>14</sup> Hilbert's main defence against this was based on the  $\varepsilon$ -operator. What Hilbert exactly wished to achieve is not clear, in the following argument the  $\varepsilon$ -operator plays no role. A possible option might be the use of the  $\varepsilon$ -operator to obtain a term that can be seen as the solution promised by  $\exists x A(x)$ , namely  $\varepsilon_{x+2=7}$  is the solution of the equation  $x + 2 = 7$ .<sup>15</sup> What is left is a passionate plea for pure existence proofs on general grounds, they do away with lengthy computations, they provide real insight, . . . No mathematician will disagree, but the fact remains that Hilbert evades the issue: can the promise  $\exists x A(x)$  be fulfilled? Instead Hilbert stuck to his lofty views: 'Pure existence theorems have thus in fact been the most important landmarks in the historical development of our science'. 'But', he added, 'such considerations do not trouble the devout intuitionist'. As to the game, so deprecated by Brouwer, it is Hilbert's opinion that the formula game reflects exactly the *technique of our thinking*. And indeed, 'the basic idea of my proof theory is nothing else but to describe the activity of our in-

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<sup>12</sup>Cf. Brouwer (1913b), p. 88.

<sup>13</sup>Ibid, p. 77.

<sup>14</sup>The reference to 'worthless scrip' suggests that Hilbert has Weyl's New Crisis-paper in mind. But it covers Brouwer's views equally well.

<sup>15</sup>It is simple to prove  $\exists x(x + 2 = 7)$ .

tellec, to make a protocol about the rules according to which our thinking actually proceeds'.<sup>16</sup>

The defence of the *Tertium non datur* is, so to speak, left to the  $\varepsilon$ -axiom, from which it follows; the informal argument for the *Tertium non datur* comes down to the bottom-line: 'it has never yet caused the slightest error'. In the end Hilbert's arguments are pragmatic, and emotional, as the following quote shows:

Taking this *Tertium non datur* from the mathematician would be the same as, say, denying the astronomer his telescope, and the boxer the use of his fists. The ban of existence theorems and the *Tertium non datur* roughly boils down to renouncing the science of mathematics as a whole.

The last part reflects the then prevailing pessimism with respect to intuitionistic mathematics. One simply could not see how the essential parts of mathematics could be salvaged. Even those who were sympathetic to Brouwer's cause felt discouraged by the complications that had to be faced. Abraham Fraenkel saw the intuitionistic landscape as a bleak place; he feared that after the intuitionistic revolution, only ruins would remind the passing traveller of the former splendour of the architecture of mathematics. Perhaps some corners of the old buildings, e.g. arithmetic, might be saved, cf. p. 444.

Hermann Weyl was no less pessimistic:<sup>17</sup>

Mathematics attains with Brouwer the highest intuitive clarity. He succeeds in developing the beginnings of analysis in a natural way, retaining contact with intuition much closer than before. But one can not deny that, progressing to higher and more general theories, the fact that the simple principles of classical logic are not applicable finally results in a hardly bearable awkwardness. And with pain the mathematician sees that the larger part of his tower, which he thought to be joined from strong blocks, dissolves in smoke.

Understandably, Hilbert, who did not particularly love the competition, shared these bleak views:

For what can the wretched remains, the few incomplete and unrelated isolated results, that the intuitionists have obtained without the use of logical  $\varepsilon$ -axiom mean, compared to the immense expanse of modern mathematics!

There is little doubt that Hilbert's assessment was right, but what was his point? After all, any new discipline, say group theory, had to start from scratch. Around the turn of the last century abstract group theory was not everywhere received with open arms. How could this new subject of group theory ever hope to catch up with the enormous mass of techniques and results of, say, projective geometry? If one takes into account that intuitionism aimed at rebuilding mathematics, then it is evident that

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<sup>16</sup>A most interesting claim. Given a number of extra specifications, there are good arguments for Hilbert's claim when made for the system of Gentzen.

<sup>17</sup>Weyl (1928).

Hilbert was premature. Moreover, his own program would, under the above criteria, be disqualified as well.

Hilbert's ultimate disproof of Brouwer's foundational tenets is expressed in an observation following his own credo, 'The fact is that thinking proceeds parallel to speaking and writing, by forming and concatenating sentences.' From Brouwer's early writings, and from the Vienna lectures, we know that this belief is diametrically opposed to Brouwer's view on language and communication. Quite consistently, Hilbert continues, 'after all, it is the task of science to liberate us from arbitrariness, sentiment and habit, and to protect us from the subjectivism, which has made itself noticeable in Kronecker's views, and which, as it seems to me, finds its apex in intuitionism'. This is again one of those populist phrases that go well with almost any audience. One may be almost certain that Hilbert's statement was interpreted as 'Kronecker and Brouwer act without objective justification, they ban what they dislike'. It is more than likely that even Hilbert intended this interpretation, because otherwise he would not lump Kronecker and Brouwer together. Kronecker, so to speak, was the super objectivist, his rejection of such things as arbitrary sets and functions, which are ultimately a matter of belief, makes him a stricter rationalist-objectivist than even Hilbert. Brouwer, on the other hand, accepted such subjective objects as choice sequences, that by their nature escaped description and communication.<sup>18</sup> This could rightly be seen as subjectivism in mathematics, and Brouwer would insist on doing so. But here Hilbert's attitude seems somewhat unreasonable, for would it not be a matter of progress if such notions could be handled mathematically?

The critique of Brouwer and his intuitionism found an apotheosis in the following, undeniably subjective, expression of exasperation:

Under the circumstances, I am baffled that a mathematician doubts the strict validity of the *Tertium non datur*. I am stunned even more about the fact that, as it seems a whole community of mathematicians has nowadays constituted itself, that is doing the same. I am most astonished about the fact that also even in the circle of mathematicians, the power of suggestion of a single, high spirited and imaginative man, can exert the most improbable and eccentric influences.

Strong language, understandable in the euphoria of an exciting lecture, but out of place in a paper in a respectable scientific journal.

After this emotional intermezzo Hilbert returned to business, he sketched some progress made by Ackermann, and closed with the optimistic statement that the success of his program was just round the corner, 'the remaining problem is just to carry out a purely mathematical finiteness proof'. His conclusion was therefore, 'mathematics is a science without presuppositions. For its foundation I need neither God, as Kronecker does, nor a special faculty of our intellect attuned to the principle of

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<sup>18</sup>Cf. van Dalen (1999c).

complete induction, nor Brouwer's ur-intuition, nor, finally like Russell and Whitehead, axioms of infinity, reducibility, and completeness, which are in fact actual, contentual assumptions that cannot be compensated by consistency proofs.'

It is interesting to read these proclamations, in view of the lessons Gödel taught us about Hilbert's program. Nonetheless, the certainty and enthusiasm of Hilbert must have been infectious, as Courant's report indicates.

One can only speculate what made Hilbert so angry, after all in August 1926 he and Brouwer had made their peace, and since then not that much had happened. Or had Brouwer's spectacular success in Berlin upset Hilbert? Had he become convinced that Berlin was a hotbed of intuitionists—it is possible, where else would he have conjured up 'a whole community of mathematicians' abjuring the *Tertium non datur*? In fact, one may doubt if the number of practising intuitionists would surpass five. If anything, the cause of Hilbert's anger and anxiety must have been psychological. Here was a younger man, with an impeccable scientific past, who with great tenacity kept pointing out the Achilles heel of the formalist program, and who was not daunted by the displeasure of the reigning sovereign of mathematics. And worse, a man who had even inspired the defection of Hilbert's star student, Hermann Weyl. Hilbert was reputed to take a common sense position in political matters, to steer away from his conservative colleagues, and to accept the republic for what it was. He was, however, not able to practice the same common sense with respect to himself. He could not forget that he was the famous professor who was always right; indeed his students usually went out of their way to spare the great man embarrassments or inconvenience. So when this Dutchman offered him admiration for his mathematical achievements and for his role as a promoter of mathematics and mathematicians, but declined to drop his foundational program, he committed something comparable to high treason. Finally, Hilbert's mental-physical situation was put under pressure by the persistence of his pernicious anaemia. In all, there were enough factors present to worry even an experienced diplomat. A diplomat can, however, consult advisers, but Hilbert had isolated himself from potential advisers on foundational matters (and not only those). Advice was to be given, not to be taken. Under the circumstances, there was probably no one to question the wisdom of such a crude course. One might almost think that Hilbert was provoking an open conflict, which would allow him to sever all ties with Brouwer. Whether he was enough of a Bismarck to hatch such a plot is of course questionable. From Courant's report to Springer, we may believe that the Hamburg audience applauded the address of the Grand Master; the provocative and aggressive character could, however, not have escaped anyone in the audience. Whereas some saw Hilbert's talk as a triumph for mathematics, Hermann Weyl did not share that view. Following Hilbert's lecture he made a number of remarks, published in the same volume of the *Abhandlungen aus dem mathematischen Seminar der Hamburgischen Universität*.<sup>19</sup>

Weyl took his place at the lectern as if he were the counsel for the defence, who saw that his client had just been sentenced in absentia. 'Allow me to say a few words

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<sup>19</sup>Weyl (1928), van Heijenoort (1967).



in defence of intuitionism', he opened. Mathematics, he said, was considered a system of contentual, meaningful and evident truths. This was the generally accepted platform before the advent of Hilbert's proof theory. Poincaré took that position, and Brouwer seconded him. 'But he was the first to see exactly and in its full extent how it had in fact transgressed the limits of contentual thought. I think that we are all indebted to him for this recognition of the limits of contentual thought.' Hilbert too recognised these limits in his metamathematical considerations, and he accepted that there was nothing artificial about these limits. 'Thus', he went on, 'it does not seem strange to me that Brouwer's ideas found a resonance (*Gefolgschaft*); his point of view followed necessarily from a thesis that was shared by all mathematicians before the formulation of Hilbert's formal approach, and from a fundamental new, indubitable logical insight, that was recognised by Hilbert too'. The consequences of these insights led, however, to widely differing reactions. Brouwer rebuilt mathematics from the inside, putting up with the limitations; Hilbert opted, however, in Weyl's words, for a wholesale reinterpretation of the meaning of classical mathematics, that is the formalisation. At this point Weyl made a gesture that caused some sensation, 'Also in the epistemological appraisal of the thus created new situation, nothing separates me from Hilbert, as I am happy to acknowledge.' Did this mean the definite farewell to intuitionism? That seems too hasty a conclusion, for we have to keep in mind that it was the same Weyl who wrote in that year,

The ice field was broken into floes, and now the element of the flowing will soon completely be master over the rigid. L.E.J. Brouwer designs—and this is an accomplishment of the greatest epistemological importance—an exact mathematical theory of the continuum, which conceives it not as a rigid being, but as a medium of free becoming.<sup>20</sup>

The recognition of the epistemological basis of a system does not automatically lead to the rejection of another system which is compatible with it. In fact, one can very well be an intuitionist and practise proof theory. One could even, as Weyl seems to suggest, share the epistemology of the proof theorist. Let us consider one of the major points of difference, the interpretation of existential statements. Even the success of Hilbert's program did not provide an existential truth, like  $\exists x A(x)$ , with a construction of an  $a$  such that  $A(a)$ . The commitment of proof theory à la Hilbert is to finitary statements, not to all real statements. The latter would only be consistent.

It looks from our point of view more as if Weyl was granting Hilbert his project. For a discerning man like Weyl must have seen that the goal of the program was still not reached, and a few private calculations would have convinced him that this was a treacherous swamp to enter. So he might have, quite reasonably, accepted the philosophical part of Hilbert's program, without committing himself to either proof

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<sup>20</sup> 'Die Eisdecke war in Schollen zerborsten, und jetzt ward das Element des Fließenden bald vollendes Herr über das Feste. L.E.J. Brouwer entwirft—und das ist eine Leistung von der größten Erkenntnistheoretischen Tragweite—eine strenge mathematische Theorie des Kontinuums, die es nicht als starres Sein, sondern als Medium freien Werdens faszt.' Weyl (1928).

theory or intuitionism. Weyl's career, his work and personality show, I think, that he in the first place was a 'working mathematician' with an interest that could not be pinned down to one project or doctrine. There are enough statements to be found in his later writings that make it clear beyond doubt that basically Brouwer was right. For example, in his review of *The philosophy of Bertrand Russell*:<sup>21</sup>

Brouwer made it clear beyond a doubt that there is no evidence supporting the belief in the existential character of the totality of all natural numbers, and hence the principle of the excluded middle. . .

And in *The Open World*:<sup>22</sup>

If mathematics would seriously retire to this status of pure game for the sake of its safety, it would no longer be a determining factor in the history of mind. (p. 77)

If mathematics is taken by itself, one should restrict oneself with Brouwer to the intuitively recognisable truths and consider the infinite only as an open field of possibilities; nothing compels us to go farther. But in the natural sciences we are in contact with a sphere which is impervious to intuitive evidence; here cognition necessarily becomes symbolic construction. (p. 82)

In 'David Hilbert and his mathematical work' he returned to the issue:<sup>23</sup>

L.E.J. Brouwer by his intuitionism had opened our eyes and made us see how far generally accepted mathematics goes beyond such statements as can claim real meaning and truth founded on evidence. I regret that in his opposition to Brouwer, Hilbert never openly acknowledged the profound debts which he, as well as all other mathematicians, owe Brouwer for this revelation.

Although Weyl was reluctantly prepared to live in a world where Hilbert's formalism prevailed, one in which a doctrinaire meaninglessness should be the ultimate scientific moral, he notes with sincere regret that 'If Hilbert's view prevails over intuitionism, as appears to be the case, *then I see in this a decisive defeat of the philosophical attitude of pure phenomenology*, which thus proves to be insufficient for the understanding of creative science even in the area of cognition that is most primal and most readily open to evidence-mathematics'.

Why was Weyl so defeatist? To be honest, there was no reason to give up hope so soon. After all, Hilbert's program had no palpable results to show. Most of the steps on the road to success had yet to come. There is a well-known quote of Weyl that may provide a hint: 'My work has always tried to unite the true with the beautiful and when I had to choose one or the other, I usually chose the beautiful.'

Hilbert was a master of sweeping presentations, but his technical evidence was not always solid. As Von Neumann remarked in a letter to Carnap, 'There are many

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<sup>21</sup>Weyl (1946), p. 275.

<sup>22</sup>Weyl (1932).

<sup>23</sup>Weyl (1944), p. 157.

programmatic publications of Hilbert, in which Hilbert states that something is being proven or almost proven, for which this is not even approximately the case.’<sup>24</sup> It could not have escaped Weyl that the grand old man of Göttingen had enough faith in himself to confuse wish and fact. And even if Hilbert would succeed, why give up ‘meaning’ for the evidence of the table of multiplication (Kreisel)? It is more plausible that Weyl’s motivations were of an altogether different sort. Let us not forget that here was his revered teacher, who was in a state which could very well be terminal. And circumstances like that tend to inspire a certain compliance. Moreover, if one shares the intuitionistic conviction that there is a real, sound mathematics, created by ourselves, then it is a matter of magnanimity to grant the pleasure of ‘playing the game’, which formalism professes. In short, Hilbert could argue, if Brouwer is right, he can prove the consistency I am looking for, so why worry? For Hermann Weyl this would have been too cheap a solution of a deep problem; but who knows, he may have had his reasons to prefer a compromising policy. We will probably never know. Whatever the meaning of Weyl’s epistemological move was, it was a serious blow for intuitionism. There are no comments known of Brouwer on the above. He never lost his esteem for Weyl, and kept his *New Crisis* paper on the reading lists until the fifties.

Brouwer’s reaction to the Hamburg lecture is not known, although it can easily be guessed. Apparently, Hilbert’s infinity paper was already as much as he could take, for he decided to write down his version of the foundational conflict. On December 17, 1927 he submitted his paper of ‘Intuitionistic reflections on formalism’, together with a paper on an embedding of spreads<sup>25</sup> to the KNAW. Two months later the same paper was submitted at the Prussian Academy in Berlin.

The paper lists all the relevant articles that had appeared so far. The last paper of Hilbert on this list is the infinity paper, so one may reasonably conclude that he had not yet seen the Hamburg paper when he submitted his manuscript. Compared to the rather excited paper of his adversary, Brouwer’s Reflections-paper is moderate in tone. It opens with what could be consider a peace proposal:

The disagreement of which is correct, the formalistic way of founding mathematics anew or the intuitionistic way of reconstructing it, will vanish, and the choice between the two activities be reduced to a matter of taste, as soon as the following insights, which pertain primarily to formalism but were first formulated in the intuitionistic literature, are generally accepted. The acceptance of these insights is only a question of time, since they are the results of pure reflection and hence contain no disputable element, so that anyone who has once understood them must accept them. Two of the four insights have so far been understood and accepted in the formalistic literature. When the same state of affairs has been reached with respect to the other two, it will mean the end of the controversy concerning the foundations of mathematics.

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<sup>24</sup>Von Neumann to Carnap, 7.VI.1931.

<sup>25</sup>Brouwer (1928b, 1928d), the translations below are Stefan Mengelberg’s, van Heijenoort (1967).

Brouwer shows himself optimistic, not to say certain. The acceptance of his points was just a matter of time, he said, as it is only a matter of reflection; the points contain no disputable elements, and everybody who has understood them must accept them. It is striking indeed that Brouwer had given up the radical condemnation of formalism of his early years. It is not unlikely that in the changing world of mathematics, he started to see that logic and formalisation at best offered certain benefits after all, and at worst, were harmless. After all, his own student had designed a logical codification for intuitionistic mathematics, and he, Brouwer, had approved the project.

After the brief introduction, which ended with a prediction of the end of the *Grundlagenstreit*, Brouwer formulated his four insights.

First insight. *The differentiation, among the formalistic endeavours, between a construction of the 'inventory of mathematical formulas' (formalistic view of mathematics) and an intuitive (contentual) theory of the laws of this construction, as well as the recognition of the fact that for the latter theory the intuitionistic mathematics of the set of natural numbers is indispensable.*

Second insight. *The rejection of the thoughtless use of the logical principle of excluded middle, as well as the recognition, first, of the fact that the investigation of the question why the principle mentioned is justified and to what extent is valid constitutes an essential object of research in the foundations of mathematics, and, second, of the fact that in intuitive (contentual) mathematics this principle is valid only for finite systems.*

Third insight. *The identification of the principle of excluded middle with the principle of the solvability of every mathematical problem.*

Fourth insight. *The recognition of the fact that the (contentual) justification of formalistic mathematics by means of the proof of its consistency contains a vicious circle, since this justification rests upon the (contentual) correctness of the proposition that from the consistency of a proposition the correctness of the proposition follows, that is, upon the (contentual) correctness of the principle of excluded middle.*

The list is followed by a critical discussion, in which Brouwer precisely checks the provenance of these insights.

The first insight is still lacking in Hilbert (1905) (see in particular Sect. V, pp. 184–185, which is in contradiction with it). After it had been strongly prepared by Poincaré, it first appears in the literature in Brouwer (1907), where on pp. 173–174 the parts of formalist mathematics mentioned above are distinguished by the terms *mathematical language* and *mathematics of the second order*, and where the intuitive character of the latter part is emphasised.<sup>2)</sup> In Hilbert (1922) (. . .), under the name *metamathematics*, mathematics of the second order broke through in the formalist literature. The claim of the formalist school to have reduced intuitionism to absurdity by means of this insight, borrowed from intuitionism, cannot very well be taken seriously.

<sup>2)</sup> [Brouwer's note] An oral discussion of the first insight took place in several conversations I had with Hilbert in the autumn of 1909.

We have seen that in the fall of 1909 Brouwer and Hilbert met in Scheveningen (cf. p. 125). On that occasion Brouwer discussed the various levels of language, logic and mathematics from his dissertation with Hilbert.

The precise distinction *formal, meaningless mathematics—contentual mathematics (metamathematics)* is only much later introduced in Hilbert's first Hamburg paper (1922). There is no reference in any of Hilbert's papers to Brouwer's role in clearing up the confusion about the two levels. Perhaps Hilbert had completely forgotten about the exchange in 1909. Courant confirmed what was common knowledge at the time, that Hilbert had great difficulty in keeping track of the information that he picked up. Courant mentions a few instances, which concern substantial research, they are by no means small, neglected corners of mathematics—'so indeed, Hilbert's theory of integral equations, one of his greatest achievements, was triggered by a bad memory'.<sup>26</sup> Whatever was behind Hilbert's unreliable memory, one should not forget that at the time of the *Grundlagenstreit* Hilbert had passed his sixtieth year, and he was at the height of his career. The success and admiration had left its trace in the form of a '*roi soleil*' mentality, it would not do to remind the king of such matters of credit. As usual, there is hardly any reason for a scientist to suppress reference to his sources, and certainly not for a successful mathematician like Hilbert. It is well known that some scholars only need the slightest hint, and they will build a beautiful theory. Others excel in originality, but have no urge to carry out the follow up. It is the co-operation of the two kinds that often produces striking results. We have seen that in the case of dimension theory, Brouwer was satisfied to work out the underlying idea so far that its significance was beyond doubt. It took a man like Menger (and had he lived, Urysohn) to work out all the interesting consequences. In the present case of the mathematics and language levels, one is hard pressed to find a reason for Hilbert to deny Brouwer his credit. Hilbert's grand design for proof theory was there for all the world to see; the most plausible explanation is that Hilbert simply forgot about Brouwer and his levels. That still leaves the question of why Hilbert did not acknowledge Brouwer's contribution after the subtle hint in the Reflections-paper. At worst, he could have said, 'Brouwer independently arrived at the idea of the mathematics levels'.

Brouwer definitely had not forgotten, and this time he decided to mention the fact; the above mentioned footnote serves the purpose. In the main text there is a hidden allusion to this matter, the two level distinction occurs in the dissertation where 'the mentioned parts of formalistic mathematics are distinguished as *mathematical language* and second-order mathematics, and where the intuitive character of the latter part is emphasised. It broke through in the formalist literature with the name *metamathematics* for second-order mathematics in the first Hamburg lecture.' In Brouwer's archive there is a small note to the effect that the 'Hilbert of 1900 did not feel the contradictory consequence of the lack of the intuiting of metamathematics, of which I freed him only in 1909 on a walk in the dunes, whereupon he changed my description "mathematical language of the second order" to "meta-

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<sup>26</sup>Courant (1981), p. 161.

mathematics”'.<sup>27</sup> Clearly, in the absence of written evidence or witnesses, it is hard to prove that, on this walk in the dunes, the topic of formalisation and levels was discussed. However, Brouwer was a scrupulous man when it came to historical facts; there is no reason to doubt the ‘discussion in the dunes’, and it is most plausible that, apart from Lie groups and topology, foundational matters also came up. And what is more plausible than that Brouwer turned to the criticism formulated in his dissertation. Hilbert never reacted to Brouwer’s statement. This may or may not be significant. One would guess, however, that a public reference to a poor credit policy would not have amused him. Hilbert had a reputation for a certain cavalier attitude with respect to credits. Courant, who belonged to Hilbert’s inner circle, lists in his *Reminiscences* a number of instances where Hilbert completely forgot where he picked up what.<sup>28</sup> Being a man of undisputed technical capacities, he could easily work out a topic for himself on information that reached him in conversation. Courant cites F. Schur and Fredholm as two fellow mathematicians who fell victim to Hilbert’s absent-mindedness; there were undoubtedly many more, but apparently nobody went as far as Brouwer to mention the matter of intellectual debt in public. Courant was acutely aware of Hilbert’s practice, and he observed the influence on his circle of students, ‘it did create in Hilbert’s students and assistants a feeling of neglect. A certain duty exists, after all, for a scientist to pay attention to others and to give them credit. The Göttingen group was famous for a lack of feeling of responsibility in this respect. We used to call this process—learning something, forgetting where you learned it, then perhaps doing it better yourself, and publishing without quoting correctly—the process of “nostrification”. This was a very important concept in Göttingen.’<sup>29</sup>

For the remaining three insights Brouwer also analysed Hilbert’s papers, and showed that in each case an acceptance of the insight by the formalists came *after* the intuitionist allegation. It is interesting to see that usually Hilbert, after some rearguard skirmishes, adopted Brouwer’s views. It is certainly strange that Hilbert never bothered to take Brouwer’s arguments seriously in public.

Number three on the list is a bit surprising, as Hilbert’s dogma is a rather marginal matter. One of those slogans that belong rather to popular lectures than to science proper. That Hilbert consistently missed the point is not all that surprising, as one must view the dogma, just as ‘PEM’, with an intuitionistic eye. Something that Hilbert could not, or would not, do, not even for the sake of argument.

Number four was, of course, the heart of the matter. Accepting it would mean for Hilbert to declare his program bankrupt. It took the soft spoken Gödel, and an equally withdrawn Gentzen, to present Hilbert with a *fait accompli*.

After analysing his four insights, Brouwer devoted the second and last section to the consistency of various forms of PEM, but not before answering politely, but firmly, Hilbert’s snubs:

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<sup>27</sup>The note is not dated, but it is almost certainly written in his later years. It also mentions Russell, Kant and Mannoury.

<sup>28</sup>Courant (1981), p. 160.

<sup>29</sup>Courant (1981).

According to what precedes, formalism has received nothing but benefactions from intuitionism and may expect further benefactions. The formalistic school should therefore accord some recognition to intuitionism, instead of polemicising against it in sneering tones while not even observing proper mention of authorship. Moreover, the formalistic school should ponder the fact that in the framework of formalism *nothing* of mathematics proper has been secured up to now (since, after all, the meta-mathematical proof of the consistency of the axiom system is lacking, now as before), where intuitionism, on the basis of its constructive definition of spread<sup>30</sup> and the fundamental property it has exhibited for finitary sets<sup>31</sup> has already erected anew several of the theories of mathematics proper in unshakable certainty. If, therefore, the formalistic school, according to its utterance in Hilbert (1926), p. 180, has detected modesty on the part of intuitionism, it should seize the occasion not to lag behind intuitionism with respect to this virtue.

One can see that Hilbert's qualification of the intuitionistic efforts as 'modest', or worse, 'wretched remnants', rankled.

We know that Brouwer was no stranger to emotional outbursts, but he always kept his public reactions under control. The above quotation is an illustration, there is no name-calling, just a summing up of some facts, and a mild admonition.

The paper contained a novelty, namely the strong refutation of the principle of the excluded middle. In fact this was an immediate corollary of his theorem that the continuum is indecomposable. Hence the statement 'very real is either rational or irrational' is contradictory. The indecomposability was proved directly in the Berlin lectures, but it is obviously an immediate corollary of the continuity theorem.

The *Grundlagenstreit* was warming up, yet nobody could foresee that it would soon be over. Brouwer used his Vienna lectures to lay a coherent philosophical picture before the world; two talks without antagonistic elements. Hilbert was preparing his grand lecture for the Bologna conference. A storm was brewing, and when it broke, its rage threatened to sweep away substantial parts of the castle of the German mathematical community.

## 14.2 The Bologna Conference

After the war the mathematicians had twice been called to an international meeting. One in 1920 in Strasbourg, and the next in 1924 in Toronto. The atmosphere in Strasbourg had been one of exhilaration, of victory. The vanquished, not allowed to attend, had met in Bad Nauheim licking their wounds. The Toronto meeting was still out of bounds for the Germans; certainly there had been an attempt to put an end to the boycott, but the motion for readmitting the Germans was not even tabled by the *Union internationale de Mathématique*, it was simply passed on to the *Conseil*.

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<sup>30</sup>Cf. p. 304 ff.

<sup>31</sup>Cf. p. 371 ff.

After the defeat of the Dutch-Danish-Norwegian proposal to drop the exclusion clause from the statutes of the *Conseil*,<sup>32</sup> a substantial part of the mathematical community wished to see that the next international mathematics conference truly deserved that name. The Union had already decided on the place: the international mathematics conference was to be held in 1928 in the venerable university of Bologna. And so the various mathematics societies duly advertised the conference. The German mathematics society, DMV, had inserted the flyer announcing the meeting in its journal, the *Jahresbericht der Deutschen Mathematischen Vereinigung*. The DMV traditionally informed their members of relevant events, and it clearly saw no objection to honour the old policy of free information. The more radical adversaries in Germany, however, viewed an announcement of the organisation that did not recognise them, in their own periodical, as a case of adding insult to injury. Brouwer, the implacable foe of the *Conseil* and its satellite, the Union, made himself the spokesman of the *Deutschnationale* opposition. In January, Brouwer wrote to Bieberbach about possible actions against the Union. Apparently, he had approached Bieberbach, as the editor in chief of the *Jahresbericht*. Bieberbach, in his reply of January 20, informed him that as the managing editor of the *Jahresbericht*, he could not possibly publish a comment of Brouwer on the invitation of the Union. 'It would create a novum if we wished to accept political arguments in the *Jahresbericht*, thus we have avoided, because of the political side of the matter, to speak of the planned conference in Bologna.' Brouwer, never to be caught out, agreed that his comments on the *Conseil* were only in form, but not in content, more political than the invitation for the Bologna conference.<sup>33</sup> His account, he said, would expose the hidden content of this invitation. Anyway, if the *Jahresbericht* would not publish his note, an enclosed leaflet would do. And indeed, a leaflet was produced, in which Brouwer reminded the readers of the motivation and formulations of the advocates of the boycott of German scientists.<sup>34</sup> He quoted the statements made by Painlevé in the *Académie des Sciences*.<sup>35</sup> These statements belonged to the emotional atmosphere of the war and its aftermath, and as such they were understandable, but they were not well-suited to promote peace and co-operation in the scientific world. Indeed, they were nothing less than a wholesale insult and condemnation of all German scholars. Painlevé's conciliatory moves in the affair of the Riemann volume of the *Annalen*, and his general role in the scientific community, make it clear that he was no longer the rabid anti-German of 1918.<sup>36</sup> But a public retraction of everything said and done in the early years after the war was probably asking too much. Nonetheless this was what the German nationalists among the mathematicians insisted upon.

Brouwer, in his leaflet, left it as a—none too subtle—suggestion: '... the readers of the *Jahresbericht* may contemplate in how participation in the planned congress

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<sup>32</sup>Cf. Sect. 9.1.

<sup>33</sup>Brouwer to Bieberbach, 23.I.1928.

<sup>34</sup>Dated March 1923 and addressed to the members of the DMV.

<sup>35</sup>Cf. p. 329.

<sup>36</sup>Cf. Schroeder-Gudehus (1966), pp. 248, 250.



is possible, without mocking the memory of Gausz and Riemann, the humanitarian character of the science of mathematics, and the independence of the human spirit.’

Not content with a passive resignation, Brouwer, possibly after consultation with like-minded Germans, decided to visit the Italian organisers in person, in the hope that the role of the Union could be reduced to naught. The physicist Sommerfeld wrote Brouwer in March that the new invitation for the Bologna congress did not mention the name of the *Conseil* or similar organisations. He expected that Brouwer would find an open ear in Italy.<sup>37</sup> Some time in late March or in April Brouwer visited Pincherle, the president of the organising committee, and Levi-Civita. The discussions were friendly, and for both sides most satisfactory. From the side of the organisers, it was agreed that the conference would not be a Union-conference, but an international congress under the aegis of the University of Bologna. Brouwer could thus return home with good news: the Union was sidetracked.

In the German mathematical community the neutral mathematicians and the anti-*Conseil* group had strong feelings about the conference. The neutrals advocated participation in Bologna, and the anti’s were, in spite of the Italian initiative, still wary. At the annual meeting in Bad Kissingen the DMV had decided not to send representatives to the Bologna conference, but it had at the same time sent out the parole to its members to attend in as large numbers as possible.<sup>38</sup> The Göttingers largely supported the Bologna conference, and the Berlin group contained some outspoken anti’s. It was no secret that a certain amount of friction existed between the Berlin group and the Göttingen group. Although there was no lack of co-operation between the mathematicians of both groups (and it should be kept in mind that Berlin and Göttingen were icons for many mathematicians all over Germany), there was a certain tendency to accuse the Göttingen group of smugness. Vice versa there was little feeling of antagonism, a Göttingen mathematician was usually well aware of the true or imagined superiority over sister universities.

Pincherle, in his role of president of the congress, had to face one unpleasant duty: to inform Picard, the president of the *Conseil* and the Union of the decisions of the Bologna committee. And he proved himself up to the occasion. On June 8 he sent a long letter to Picard—‘Monsieur et illustre Maître!’ He respectfully, but firmly, explained that one could no longer stick to the format of the Strasbourg and Toronto Conferences. If one wishes, he wrote, to re-establish between the scholars who practice the most pure of all sciences, the good relations which are so necessary for their progress, one should leave formal considerations behind. The organisers felt that they had to shape the congress in such a way that this meeting of all mathematicians would be possible. And so he informed Picard of the decision to make the congress an Italian matter, independent of the Union. Of course, he added, the Union will still have its business meeting during the conference, where they could fix their policy for the future. He hoped, nonetheless, that the great Emil Picard would be there to open the lecture series of the conference.

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<sup>37</sup>Sommerfeld to Brouwer, 24.III.1928.

<sup>38</sup>Perron to Bieberbach, 6.V.1928. Perron inquired if Bieberbach would go to Bologna and give a talk.

Picard, confronted with a *fait accompli*, replied that his quality of president of the *Conseil* did not permit him to attend the congress.

In view of the pleasing initiative of Pincherle, one would guess that the problem, at least in Germany, had been solved to everyone's satisfaction. Perhaps a number of French mathematicians would feel hurt, but the international character of the congress was at least saved. Yet, in Germany new problems and discussions arose. Bieberbach, in reply to a circular letter of Geheimrat Ziehen, the rector of the university at Halle, summed up the touchy issues that still rankled.<sup>39</sup> He pointed out that the information on the Bologna congress was far from consistent. Indeed, there must have been some mix-up affecting the invitations. Some mentioned the involvement of the Union, others did not; by way of illustration—Brouwer obtained a personal, albeit printed, invitation from Peano on July 8, in the usual flattering wording, full of misprints, and mentioning explicitly on the third line '*Sous les auspices de l'Union Mathématique Internationale*'.

Bieberbach's list of grievances contained more: the French invitation listed the Bologna congress as the eighth international mathematics congress, thus elevating the Strasbourg and Toronto congresses, from which the Germans were excluded, to a status of internationality they did, in his eyes, not deserve. As far as he knew the effects from a certain neutral side had resulted in the fobbing off in the form that in the latest circular the Union was no longer explicitly mentioned. 'A diplomatic feat, by which the real state of things was even more underlined.'

Furthermore, he took exception to the tactless plans for an excursion to the power station at the Ledrosse in South Tirol, which had changed hands after Versailles.

The call of the rector of the University at Halle to the universities and academies, to send representatives to Bologna, amazed Bieberbach. He could not see a justification in customs of the past. The Prussian Academy had left the decision to attend congresses of the *Conseil* to the individual scientists, but advised a large measure of restraint. 'Only if this congress, compromised beyond rescue, essentially suffers damage through the lack of German participation, can we expect with certainty that the next international mathematics congress will be really international, without involvement of the Union, either that this totally superfluous and ineffective organisation cleanses itself by extricating itself from the *Conseil*, or by a complete reorganisation satisfying the German wishes.' Here was a concrete goal; whereas Brouwer vented his moral objections against the *Conseil* and the Union, Bieberbach saw a solution to the present discontent in mathematical circles, and not only in Germany. The breaking up of the Union and the *Conseil* could start in Bologna, he thought. Ziehen must have informed the leading mathematicians of Bieberbach's letter, an unfortunate and rash action. For the letter was a personal reaction, and Bieberbach had not foreseen that Ziehen would circulate it.<sup>40</sup> Had he known the use Ziehen was going to make of his letter, he would probably have been more guarded in formulation and content. Bieberbach had not expected any such action from Ziehen, as

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<sup>39</sup>Bieberbach to Ziehen, 18.VI.1928.

<sup>40</sup>Cf. Bieberbach to Courant, 15.VII.1928.

the *Rektorenkonferenz* and the *Hochschulverband* had agreed that all foreign affairs would be handled by the latter.

The Bieberbach–Ziehen exchange was not entirely ignored: there was at least one sharp and aggressive reaction, it came from the grand-master of German mathematics. Hilbert wrote an angry letter to the German rectors and directors of mathematical seminars, resolutely repudiating Bieberbach's views: 'An die Herren Rektoren der deutschen Hochschulen und die Leiter der mathematischen Seminare. *Betrifft den Internationalen Mathematiker-Kongress in Bologna*'.<sup>41</sup>

Bieberbach, he said, was ill informed, and citing rumours. The congress had officially severed its ties with the Union; agitating against the Italian organisation and boycotting the congress would alienate friendly colleagues and organisations, and could only be seen as a slap in the face of the Italians.

The letter ended with an explicit call to the German colleagues, 'It would be in the interest of German science and German prestige to wish most urgently, that no university and no academy will desist from accepting the official invitation to participate in a friendly manner.'

The letter clearly outlined Hilbert's political position; he did not speak about the past insults, but stuck to the possibility offered by the Italian organisation. At the same time he put Bieberbach in his place, and this caused—as to be expected—some hard feelings.

For many German mathematicians Hilbert's action was the ardently awaited signal. Thus Perron wrote to Landau 'Finally the circular of Hilbert, for which I had been waiting passionately, has arrived.'<sup>42</sup>

Hilbert, in a manner of speaking, had viewed the discussion and the arguments concerning the Bologna congress from the height of his Olympus, blurring the finer details. He was probably quite right to do so, because an overly discerning gaze is no asset if one wants to reach a compromise. But it should be noted that in this way he did not do justice to Bieberbach's observations. Small wonder that Bieberbach replied publicly to Hilbert's attack. Since his '*hochverehrter Lehrer, Herr Geheimrat Hilbert*' had severely reprimanded him, he felt obliged to defend himself. The letter is a clever balancing act, between not insulting Hilbert and sticking to his guns. On the whole, one must say that, viewed as an academic exercise Bieberbach performed most credibly. E.g. in countering Hilbert's argument, that Picard had felt offended after Pincherle's letter, he remarked that this was beside the point, 'for our aim is only the satisfaction of our own value, not the degree of irritation of Mr. Picard'. He deplored that Hilbert, by using the word 'denunciated', had introduced a false note into the discussion.<sup>43</sup> His veneration for his teacher and the level of courtesy in a discussion would forbid him to follow suit.

<sup>41</sup>Hilbert to rectors of German universities, 29.VI.1928.

<sup>42</sup>Perron to Landau, 2.VII.1928.

<sup>43</sup>...*Dieses Schreiben nimmt in schroffster Weise Stellung gegen den diesen Herbst in Bologna stattfindenden Internationalen Mathematiker-Kongress, denunziert den Kongress als eine Veranstaltung des conseil internationale de recherches und rät daher dringend an zu einer Ablehnung der an die Universitäten usw. ergangener Einladung.*

There must have been an enormous amount of correspondence concerning the Bologna congress, most of it will have found its way to the wastepaper basket, but some of the letters of the influential mathematicians have been preserved. There are basically two kinds of views, one, like Hilbert's, shows itself satisfied with the Italian results, another one, like that of Bieberbach and Brouwer, considers the concessions promising but not conclusive. Von Mises, for example, in his letter to Courant,<sup>44</sup> agrees with the latter that 'we should have nothing to do with the Union'. But he disagreed with Courant (and the Göttingen mathematicians)<sup>45</sup> about the real content of the Italian communications. The role of the Union is left completely vague, there is no evidence that the Union is put out of action. For one thing, Pincherle, who became president of the organising committee in his function of chairman of the Union, should have abdicated in the latter function.

From von Mises' letter we learn that Brouwer had not aimed, in his discussion with Pincherle, at a complete separation between congress and Union, but rather at a more modest goal: the appointment of an independent international committee that would after the conference decide on the future conferences, so that there would be a guarantee that these would no longer be congresses of the Union. That much was promised to him. The main thing was, he argued, to prevent that the Union, 'which at this moment is fighting its final battle', could present the Bologna conference as 'sufficiently international', for then 'the Union, this pronounced *political organisation*, would still exist in four years time, and all the present conflicts would have to be repeated'. He concluded that the only definite solution could be reached through a wholesale ignoring of the conference by the German mathematicians.

By now, in the early days of July, all positions had been taken up. It was unlikely that any German mathematician would change his views. The remaining correspondence can be viewed as damage control. Courant, who had become Hilbert's main lieutenant, tried to pacify Bieberbach in a letter of July 10. Hilbert had acted in such a radical manner, as he had promised to give a talk in Bologna, and believed that the political issues had been straightened out. To his surprise, the letter from Halle, followed by Bieberbach's letter, had given substance to the idea that going to Bologna was un-German. Hence his sharp reaction, which was in no way intended personally. As Courant put it, 'Hilbert did not want to react once more to your reply, he had, however, expressly assured me how little from his side a personal insult was intended.' One might wonder why Hilbert did not send a short note to that effect to Bieberbach, but one has to keep in mind that he still suffered from his pernicious anaemia. He probably tried to avoid all action that was not strictly necessary. Of course, there is also the possibility that the king of mathematics does not apologise to a former student, who happened to be a Berlin professor.

Courant went on to explain that Hilbert had been seriously annoyed by the Riemann affair: 'Hilbert had felt deeply insulted when, as a consequence of Brouwer's

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<sup>44</sup>Von Mises to Courant, 7.VII.1928.

<sup>45</sup>He referred tongue in cheek to the Hilbert–Bieberbach discussion, noting that 'it is a good illustration of your remark that inside the mathematics society no controversies exist'.

exhortation and, as Hilbert thinks, with your co-operation the collaboration, desired by Hilbert, of French mathematicians to the Riemann volume, was blocked.’ Courant’s, no doubt kindly intended, attempt at reconciliation met with doubt and scorn. Bieberbach replied five days later that he assumed that Courant did not agree with the use of the term ‘denunciate’ to describe his action. ‘I presume that this is solely on Hilbert’s conto.’<sup>46</sup> Courant was completely wrong, Bieberbach added, in his view of Brouwer’s role in the Riemann affair. Bieberbach’s letter shows some of the discontent one would find in Germany over the high-handed actions of Hilbert.

As far as this matter of the Riemann-volume is concerned, there was, as you know, a discussion about it in Innsbruck.<sup>47</sup> And I have made use of my right to ventilate my opinion. I have never committed myself to voice only opinions that pleased Hilbert; and I would find it foolish if such a demand would seriously be made to me. Also in this matter I have not unfolded any agitation, as I have better things to do. I have, only when my opinion was asked, stuck to my point of view.

Bieberbach also felt that Hilbert had not done his homework, but nonetheless accused Bieberbach of ‘using secondary information’. Here Bieberbach had a point, right from the beginning he had been involved in the discussions concerning the Bologna congress. He was well-informed about all the negotiations. Thus it is not surprising that he was upset, if not offended, by the amateurish diplomacy of the Göttingers, ‘Rather one could reproach the Hilbert clique, that they attach decisive importance (*Bedeutung*) to ad hoc formulations, e.g. like the letter of Bohr, without taking into account the previous history and the laborious correspondence over many years.’

Bieberbach’s letter also provides some useful information on Brouwer’s role. From the point of view of Göttingen it seemed as if there was a huge conspiracy with Bieberbach, Brouwer and Von Mises as the main characters. But actually, Brouwer’s actions in Italy were his own, Bieberbach only learned about them from Von Mises. The only significant contact about the Bologna matter concerned Brouwer’s letter to the editor of the *Jahresbericht* (see p. 542). As to Brouwer’s involvement in the Bologna affair, Courant had confirmed that Hilbert’s thoughts went beyond just suspicions: ‘The idea that Brouwer’s disposition was actually normative for the colleagues in Berlin was moreover confirmed by reports of Von Mises; and I can only say that Hilbert rejected most passionately this interference and this playing the judge.’

In his reply Bieberbach soberly asked ‘Why, by the way, is Brouwer’s interest in the matter an ‘outside interference’, but in contrast, that of Bohr and Hardy not?’ As Bieberbach wrote, Hilbert had already at the time of the Bad Kissingen meeting, thus before the conflict arose, put himself forward as a lecturer at the conference. This certainly was an extremely visible sign from the German side of the wish for

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<sup>46</sup>Bieberbach to Courant, 15.VII.1928.

<sup>47</sup>Apparently a meeting of the editors of the M.A. editors at the time of the annual meeting of the DMV.

**Fig. 14.1** Ludwig Bieberbach. [Courtesy U. Bieberbach]



appeasement, in particular since it came from the greatest mathematician, at a time when the congress was still a full-blooded Union congress. He, Bieberbach, had acquiesced, as one might assume that few Germans would attend. Hilbert's role as a symbol of German good will was part of Bieberbach's master plan. He had not dreamed that 'Hilbert was essentially so little independent, that he demanded a general participation, and considered the alternative as an insult for his person.' In short, Bieberbach was not subdued by the carrot-and-stick letter of Courant.

In another letter, to the applied mathematician Walther, Bieberbach again analysed the key statements concerning the separation of Union and congress, pointing out the various readings and glaring inconsistencies (e.g. in June Bortolotti had told Bieberbach that Picard's objection to the procedure was that the invitations for the conference were not offered for inspection to the *Conseil*, and that the rector of the University of Bologna had sent them out). He had seen too many diplomatic formulations not to be sceptical. 'But', he wrote, 'mundus vult decipi and who wants to go to Bologna, and thinks that he has to let his glory shine in the beams of the international sun, to him such formulations are *Butter aufs Brot*'. The correspondence between Courant and Bieberbach went on for another round. Since only Bieberbach's letter has been preserved, one has to guess the content of Courant's letter. Bieberbach, in his vehement protest against Courant's claims, maintained that Brouwer's disposition had not been of decisive influence on him. It had not exercised any influence at all, he wrote. Hilbert's conciliatory move towards Bieberbach must again have been brought up by Courant. But Bieberbach remained adamant. Hilbert had publicly insulted Bieberbach—even after Bieberbach's reaction of July 3, Hilbert's circular had been distributed further; for example to the Prussian Academy. Instead of authorising Courant to make friendly noises, he should have stated his apology

before the same Forum that had witnessed the insult. But, he went on, ‘in view of the circumstance, that Hilbert is substantially older than I am, and that he is my teacher, I will be satisfied if you will get yourself an authorisation from Hilbert, to state to me that he regrets and retracts the expression ‘denunciate’.

Since the negotiations concerning the Bologna congress were now all water under the bridge, he advocated a composed conduct of the German delegation in Bologna: ‘It is more important to prevent that in Bologna the Germans present there sneer altogether too much at those who stayed away to paint a picture of their own excellence at the expense of German prestige and unity. Perhaps you should just think about it if you could not contemplate this idea. So far, everything that comes from Göttingen has a ring of such disdain for dissenting Germans, that I am really worried how it will be in Bologna, if this mood comes to an eruption, just as it is said to have burst out in Göttingen before foreigners.’

Brouwer’s role had become marginal, after his negotiations in Rome and Bologna, he carried on his correspondence, but no longer played a role. He once more sent out his leaflet with the unfortunate statements of Painlevé (August).

The unity of the foreign advocates of ending the boycott of Germans had begun to crumble. Harald Bohr had, after an exchange of letters with Pincherle, come to the conclusion that the Italians were offering a really international meeting where all nationalities participated with equal rights. He saw the Union as definitely defeated—‘we, the internationally minded, (i.e. people like you, Hardy etc.) have in fact won so completely, that from my point of view it would be neither natural, nor advantageous, if now the congress would be blown up by the opponents of the Union. The relatively minor questions and formalities should not create a rift between us.’<sup>48</sup>

Hardy also wrote to Brouwer about the conference boycott; he had, like Brouwer, been a staunch enemy of the *Conseil*—‘I detest it, and have never had anything to do with it.’ But—contrary to Brouwer—he hoped ‘that the Germans would, on the whole, adopt the policy of being more magnanimous than they could reasonably be expected to be’. In short, he shared Bohr’s view. Having fought the *Conseil* tooth and nail, with Brouwer, Bohr and others, he could not share Brouwer’s adamant views, as appears from the closing hints of his letter: ‘Finally, if you are to demand that everybody should formally retract all the imbecilities which have been uttered during the war, then assuredly there will never be any Congress of any kind until everybody born before 1914 is dead. And I should hope that, whatever happens at Bologna, it will at any rate be enough to make everybody realise that the era of imbecility is passed.’

So this was the end of the road, a small group of die-hards refused to go to Bologna, so close to Canossa, as Bieberbach had remarked. A sizeable contingent of the German mathematical community did accept the assurances of the organisers, and participated. There was also a number of mathematicians that did not go, but for other than political reasons (e.g. Hardy and Menger).

The conference became a huge success. The mathematician Härten reported to Brouwer that Pincherle had opened the congress by declaring that the Italians were

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<sup>48</sup>Bohr to Brouwer, 3.VII.1928.

the only organisers of the congress, and on many occasions it was proclaimed ‘that this was the first truly international congress after the war’.<sup>49</sup>

Three weeks later Härten sent a more complete report to Brouwer. The German aspects were quite satisfactorily reckoned with, he wrote, there were for instance a large number of German posters instructing the participants, more than English or French ones. The congress organisation clearly had done its best not to offend the Germans. In particular there were quite a number of apologetic remarks about the recent past. Pincherle was elected with general approval as president of the meeting. The choice of the vice presidents was judicious: Hadamard, Hilbert (‘exceptionally strongly applauded, very striking’), Fehr (Switzerland), Young (UK), Birkhoff (USA), Bohr (Scandinavia), Rey Pastor (Spain & South America), Sierpinski (Poland), and Lusin (USSR).

The series of lectures was opened by Hilbert. Constance Reid described the occasion in her biography of Hilbert: ‘At the opening session, as the Germans came into an international meeting, for the first time since the war, the delegates saw a familiar figure, more frail than they remembered, marching at their head. For a few minutes there was not a sound in the hall. Then spontaneously, every person present rose and applauded.’<sup>50</sup> Härten also described this event:

... the first lecture of Hilbert, who was greeted with a storm of applause. Numerous repetitions; power of concentration clearly much hampered by physical suffering. Content familiar by latest publications. Strong applause—Also a strong applause greeted Hadamard, whose lecture was also very good in presentation much more effective than Hilbert’s. At Hadamard’s lecture the applause much stronger than before. In Hilbert’s case the applause was meant for the person, in Hadamard’s case also for the lecture.

It should probably not surprise us that the Italian government made the most of this meeting. At the opening the Podesta greeted the participants in the name of ‘the fascist city, that was happy to show the foreign visitors the accomplishment of the fascists’. The participants were given tags with bands in the Italian colours and at breakfast there were little Italian flags for the participants to wear. This unnecessary and somewhat childish nationalism did not exactly please all participants.

The Union was active after all, it distributed flyers with invitations to join its business meeting. At this meeting Pincherle announced that Switzerland had offered to host the next congress. In his closing address the Union did not figure at all. In preceding private sessions invitations from Prague and the Netherlands had reached the meeting. Switzerland had declined, in spite of pressure from all sides. The Dutch professor Schouten had telegraphed after the meeting to the Dutch government, and he had obtained a positive reaction. The next day in informal discussions objections against the Netherlands were raised on the grounds of Brouwer’s actions. After some more pressure Switzerland came back on its refusal, and the Netherlands and Prague withdrew their offers.

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<sup>49</sup>Härten to Brouwer, 6.IX.1928.

<sup>50</sup>Reid (1970), p. 188.



After the congress there were some retrospective discussions, but on the whole the hopes were high that from now on the boycott would be a thing of the past.

Brouwer may not have been successful in discouraging the Germans to participate, but he could well be satisfied with the final outcome. If one tries to give a reason for the uncompromising attitude of the anti-Unionists, one should look at the past history of the *Conseil* and the Union. All attempts to drop the exclusion clauses had been rudely blocked by Picard and his followers. In certain quarters there was absolutely no faith in the will or capability to accept the enemy of the past as the colleague of today. Hence the uncompromising stance of people like Bieberbach, Brouwer and Von Mises. On the other side there was a more optimistically minded group, led by the Göttingers, who believed that a compromise at the Bologna meeting would pave the way for a restoration of the old free co-operation. History has proved the Göttingers right, but in 1928 the future still offered two options.

Looking back at Brouwer's role in the Bologna affair, one is almost surprised by his innocence. For him it was a fight between good and evil, between a closed shop of *Conseil*-connected scholars and institutions, and the free world of science. His attacks were conspicuous for the quotations of his adversaries, not for his own strong language. The most effective action he undertook was his personal visit to Rome and Bologna, and the negotiations with his Italian colleagues. It probably was the turning point in the action for detaching the congress from the Union. His pamphlet might be considered a clever attempt to keep the German mathematicians away from Bologna, but it is doubtful if it changed the view of those who had not made up their minds to stay home. For a successful action of that sort, one needs an organisation and strongly motivated activist-supporters. None of this was within Brouwer's province. He had always been a loner, and even the sympathy of a number of German colleagues was not more than the support of kind well-wishers. Against a network like that of the Göttingen mathematicians, an unworldly Amsterdam professor stood no chance. Brouwer, like the legendary Roland, was famous for his lost battles.

Brouwer's actions and pamphlets were personal in the sense that he referred to certain mathematicians in the higher ranks of the *Conseil*, but no attacks at German colleagues were launched. Hilbert on the other hand personally attacked Bieberbach, and there is a private note of his, probably of this period, in which he poured his wrath over Brouwer:

*Erpressertum.*

In Germany a political blackmail of the worst kind has come up. You are not a German, not worthy a German birth, if you don't speak and act as I tell you now. It is very easy to get rid of these blackmailers. One has but to ask, how long they have been in the German trenches. Unfortunately German mathematicians have fallen victim of this blackmail, for example Bieberbach. Brouwer has known to make use of this state of the Germans, and without having been active himself in the German trenches, all the more to work towards inciting and to cause discord among the Germans, in order to pose as the master of German mathematics. With complete success. He will not succeed in this for the second time.

One should of course keep in mind that Hilbert's health was very poor at the time, and this may well have influenced his judgement and emotional balance. A far more fateful manifestation of Hilbert's illness would be in the aftermath of the Bologna congress bring German mathematics to the brink of 'civil war'.

### 14.3 The War of the Frogs and the Mice

Who will rid me of this meddlesome priest

Henry II

The dreaded and perilous pernicious anaemia had not left Hilbert alone. After a modest improvement in January 1928, Hilbert was plagued again by its various symptoms. In this condition he had a heart attack in the weeks before he set off for Bologna.<sup>51</sup> So when he appeared in Bologna, he was physically in a poor state. His triumphant reception and the success of his lecture must have given him the strength to make it through the congress, but after the congress he completely broke down. He was taken to a sanatorium in Luzern where he spent a full five weeks. His situation was for a longer period so desperate that he saw himself at death's doorstep. It was this unfortunate and sad combination of events that lay at the root of the following tragic history of a conflict in the German mathematical world.

On 27 October 1928, a curious telegram was delivered to Brouwer, a telegram that was to plunge him into a conflict that for some months threatened to split the German mathematical community. This telegram set into motion a train of events that was to lead to the end of Brouwer's involvement in the affairs of German mathematicians and indirectly to the conclusion of the *Grundlagenstreit*. The telegram was dispatched in Berlin, and it read:

Professor Brouwer, Laren N.H. Please do not undertake anything before you have talked to Carathéodory who must inform you of an unknown fact of the greatest consequence. The matter is totally different from what you might believe on the grounds of the letters received. Carathéodory is coming to Amsterdam on Monday.

Erhard Schmidt

Following Schmidt's instructions, Brouwer, puzzled as he was, went about his daily routines. Two registered letters that had arrived at the post office added to the mystery. Brouwer collected these letters from Göttingen and waited for the arrival of Constantin Carathéodory. The letters were still unopened when Carathéodory arrived in Laren on the thirtieth of October. Carathéodory's visit figures prominently in the history that is to follow.

In order to appreciate the full dramatic magnitude of the following history, one must keep in mind that Brouwer was on friendly terms with all the actors in this

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<sup>51</sup>Courant to Springer, 3.X.1928.

**Fig. 14.2** Constantin Carathéodory. [Courtesy M. Georgiadou]



small drama, with the possible exception of David Hilbert. Some of them were even intimate friends of his, for example Carathéodory and Otto Blumenthal.<sup>52</sup>

Carathéodory found himself in the embarrassing position of being the messenger of offensive news, in which he was involved against his will. The first letter, he explained to Brouwer, should have carried more signatures, or at least Blumenthal's signature. Carathéodory's name was used in a manner not in accordance with the facts, although he would not disown the letter should Brouwer open it. Finally, the sender of the letter would probably seriously regret his action within a couple of weeks. The second letter was written by Carathéodory himself, although Blumenthal's name was on the envelope. He, Carathéodory, regretted the contents of the letter.

Thereupon Brouwer handed the second letter over to Carathéodory, who proceeded to relate the theme of the letters. The contents of the second can only be guessed, but the first letter can be quoted verbatim. It was written by Hilbert, and copies were sent to the other dramatis personae in the tragedy that was to follow.

Hilbert's letter was brief:

Dear Colleague,

Because it is not possible for me to co-operate with you, given the incompatibility of our views on fundamental matters, I have asked the members of the board of managing editors of the *Mathematische Annalen* for the authorisation, which was given to me by Blumenthal and Carathéodory, to inform you that henceforth we will forgo your co-operation in the editing of the *Annalen* and thus delete your name from the title page. And at the same time I thank you in the name of the editors of the *Annalen* for your past activities in the interest of our journal.

Respectfully yours,  
D. Hilbert.

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<sup>52</sup>A wealth of information on Carathéodory can be found in the recent biography (Georgiadou 2004).

The meeting of the two old friends was painful and stormy; it broke up in confusion. Carathéodory left in despondency and Brouwer was dealt one of the roughest blows of his career.

Although Brouwer had kept himself completely under control during the visit, he suffered under a strong reaction. After the visit he was ill for a few days and ran a temperature.<sup>53</sup>

The *Mathematische Annalen* was the most prestigious mathematics journal at that time. It was founded in 1868 by A. Clebsch and C. Neumann. In 1920 it was taken over from the first publisher, Teubner, by Springer.

For a long period the names of Felix Klein and the *Mathematische Annalen* were inseparable. The authority of the journal was largely, if not exclusively, based on the mathematical fame and the management abilities of Klein. The success of Klein in building up the reputation of the *Annalen* was to no small degree the result of his choice of editors. The journal was run, on Klein's instigation, on a rather unusual basis; the editors formed a small exclusive society with a remarkably democratic practice. The board of editors met regularly to discuss the affairs of the journal and to talk mathematics. Klein did not use his immense status to give orders, but the editors implicitly recognised his authority.

Being an editor of the *Mathematische Annalen* was considered a token of recognition and an honour. Through the close connection of Klein—and after his resignation, of Hilbert—with the *Annalen*, the journal was considered, sometimes fondly, sometimes less than fondly, to be 'owned' by the Göttingen mathematicians.

Brouwer's association with the *Annalen* went back to 1915 and before, and was based on his expertise in geometry and topology. In 1915 his name appeared under the heading 'With the co-operation of'. Brouwer was an active editor indeed; he spent a great deal of time refereeing papers in a most meticulous way.

The status of the editorial board, in the sense of by-laws, was vague. The front page of the *Annalen* listed two groups of editors, one under the head *Unter Mitwirkung von* (with the co-operation of) and one under the head *Gegenwärtig herausgegeben von* (at present published by). I will refer to the members of those groups as associate editors and chief editors. The contract (25 February 1920) that was concluded between the publisher, Springer, and the editors (*Herausgeber*) Felix Klein, David Hilbert, Albert Einstein, and Otto Blumenthal speaks of *Redakteure*, but does not specify any details except that Blumenthal is designated as managing editor.

The loose formulation of the contract would prove to be a stumbling block in settling the conflict that was triggered by Hilbert's letter. At the time of Hilbert's letter the journal was published by David Hilbert, Albert Einstein, Otto Blumenthal and Constantin Carathéodory, with the co-operation of L. Bieberbach, H. Bohr, L.E.J. Brouwer, R. Courant, W. von Dyck, O. Hölder, T. von Kármán, and A. Sommerfeld. The daily affairs of the *Annalen* were managed by Blumenthal, but the chief authority, undeniably, was Hilbert.

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<sup>53</sup>Gawehn to Von Mises, 9.XI.1928.

Given the status of the *Annalen*, any mathematician would be more than happy to join the editorial board, and once on the board, expected to remain there until a ripe old age. Two of the most prominent editors, Klein and Hilbert, served resp. from 1876 till 1924, and from 1901 till 1939. So Klein retired at 75, one year before his death, and Hilbert at 77, four years before he died. Under the circumstances, Brouwer, who was an active associate editor, could expect eventually to become an editor, and to remain so for years to come. Hilbert dashed these expectations with one stroke of the pen. Even if the procedure remained a secret, which was to be doubted, Brouwer would be the laughing stock of the mathematical world. The readers of the *Mathematische Annalen* would note that Brouwer's name had disappeared from the list, and they would draw their own conclusion. Had Hilbert contemplated these consequences when he signed the dismissal in an emotional impulse? And did he realise the enormity of the insult he was committing?

It is hard to imagine what Hilbert had expected; he could not have counted on a calm resigned acquiescence from the high-strung, emotional Brouwer. In Brouwer's eyes (and quite a few colleagues would have taken the same view) a dismissal from the *Annalen* board was a gross insult.

Carathéodory, in trying to win Brouwer's acquiescence, had apparently revealed only part of the underlying motive, as appears from Brouwer's letter of November 2 to Blumenthal,

Furthermore Carathéodory informed me that the *Hauptredaktion* of the *Mathematische Annalen* intended (and felt legally in the position) to remove me from the editorial board of the *Annalen*. And only for the reason that Hilbert wished to remove me, and that the state of his health required to give in to him. Carathéodory begged me, out of compassion with Hilbert, who was in such a state that one could not hold him responsible for his behaviour, to accept this shocking injury in resignation and without resistance.

Hilbert himself made no secret of his motivation; in a letter of 15 October he asked Einstein for his permission (as a *Mitherausgeber*) to send a letter of dismissal (the draft to the chief editors did not contain any explanation) and added

Just to forestall misunderstandings and further ado, which are totally superfluous under the present circumstances, I would like to point out that my decision—to belong under no circumstances to the same board of editors as Brouwer—is firm and unalterable. To explain my request I would like to put forward, briefly, the following:

1. Brouwer has, in particular by means of his final circular letter to German mathematicians before Bologna, insulted me and, as I believe, the majority of German mathematicians.
2. In particular because of his strikingly hostile position vis-a-vis sympathetic foreign mathematicians, he is, in particular in the present time, unsuitable to participate in the editing of the *Mathematische Annalen*.
3. I would like to keep, in the spirit of the founders of the *Mathematische Annalen*, Göttingen as the chief base of the *Mathematische Annalen*—Klein,

who earlier than any of us realised the overall detrimental activity of Brouwer, would also agree with me.<sup>54</sup>

In a postscript he added: ‘I myself have for three years been afflicted by a grave illness (pernicious anaemia); even though this disease has been taken its deadly sting by an American invention, I have been suffering badly from its symptoms.’

Clearly, Hilbert’s position was that the chief editors could appoint or dismiss the associate editors. As such he needed the approval of Blumenthal, Carathéodory and Einstein. Blumenthal had complied with Hilbert’s wishes, but for Carathéodory, the consent was problematic; apparently he did not wish to upset Hilbert by contradicting him, but neither did he want to authorise him to dismiss Brouwer. Hilbert may easily have mistaken Carathéodory’s evasive attitude for an implicit approval. Carathéodory had landed in an awkward conflict between loyalty and fairness. He obviously tried hard to reach a compromise. In view of Hilbert’s firmly fixed conviction, he accepted the unavoidable conclusion that Brouwer had to go; but at least Brouwer should go with honour.

Being caught in the middle, Carathéodory sought Einstein’s advice. In a letter of 16 October he wrote ‘It is my opinion that a letter, as conceived by Hilbert, cannot possibly be sent off.’ He proposed, instead, to send a letter to Brouwer, explaining the situation and suggesting that Brouwer should voluntarily hand in his resignation. Thus a conflict would be avoided and one could do Brouwer’s work justice: ‘Brouwer is one of the foremost mathematicians of our time and of all the editors he has done most for the *Mathematische Annalen*.’ The second letter we mentioned above must have been the realisation of Carathéodory’s plan. Einstein answered, ‘It would be best to ignore this Brouwer affair. I would not have thought that Hilbert was capable of such emotional outbursts.’<sup>55</sup> The managing editor, Blumenthal, must have experienced an even greater conflict of loyalties, being a close personal friend of Brouwer and the first Ph.D. student (1898) of Hilbert, whom he revered. Einstein did not give in to Hilbert’s request. In his answer to Hilbert (19 October 1928) he wrote:<sup>56</sup>

I consider him [Brouwer], with all due respect for his mind, a psychopath and it is my opinion that it is neither objectively justified nor appropriate to undertake anything against him. I would say: ‘Sire, give him the liberty of a jester (*Narrenfreiheit*)!’ If you cannot bring yourself to this, because his behaviour gets too much on your nerves, for God’s sake do what you have to do. I, myself, cannot sign, for the above reasons such a letter.

Carathéodory, however, did not possess Einstein’s strength to cut the knot, once the moral issue was decided. He was seriously troubled and could not let the matter rest. He again turned to Einstein:<sup>57</sup>

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<sup>54</sup>Carathéodory had also received a copy, and probably Blumenthal as well.

<sup>55</sup>Einstein to Carathéodory, 19.X.1928.

<sup>56</sup>Einstein to Hilbert, 19.X.1928.

<sup>57</sup>Carathéodory to Einstein, 20.X.1928.

... your opinion would be the most sensible, if the situation would not be so hopelessly muddled. The fight over Bologna... seems to me a pretext for Hilbert's action. The true grounds are deeper—in part they go back for almost ten years.<sup>58</sup> Hilbert is of the opinion that after his death Brouwer will constitute a danger for the continued existence of the *Mathematische Annalen*. The worst thing is that while Hilbert imagines that he does not have much longer to live (...) he concentrates all his energy on this one matter, (...). This stubbornness, which is connected with his illness, is confronted by Brouwer's unpredictability... If Hilbert were in good health, one could find ways and means, but what should one do if one knows that every excitement is harmful and dangerous? Until now I got along very well with Brouwer; the picture you sketch of him seems me a bit distorted, but it would lead too far to discuss this here.

This letter made Einstein, who in all public matters practised a high standard of moral behaviour, realise that these were deep waters indeed.<sup>59</sup>

I thought it was a matter of mutual quirk, not a planned action. Now I fear to become an accomplice to a proceeding that I cannot approve of, nor justify, because my name—by the way, totally unjustifiably—has found its way to the title page of the *Annalen*... My opinion, that Brouwer has a weakness, which is wholly reminiscent of the *Prozessbauern*,<sup>60</sup> is based on many isolated incidents. For the rest I not only respect him as a man with an extra-ordinarily sharp eye, but also as an honest man, and a man of character.

While I beg you not to blame me for my stubbornness and while I assure you that I will never make use of the fact that it was *you* who informed me about this, I remain with warmest greetings.

From these letters, even before the real fight had started, it clearly appears that Einstein was firmly resolved to reserve his neutrality. Einstein had called Brouwer 'an involuntary champion of Lombroso's theory of the close relation between genius and insanity', but Einstein was well aware of Brouwer's greatness, and did not wish him to be victimised. It is not clear whether Einstein's opinion was based on personal observation or on hearsay; the two knew each other well enough, they had met in 1920 when Einstein visited the Academy in Amsterdam, and Einstein had stayed with Brouwer in Blaricum.<sup>61</sup> Furthermore they had probably met during some of the visits of Einstein to Leiden, and at meetings of the editorial board of the *Annalen*.

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<sup>58</sup>An enigmatic remark. It is far from clear what Cara (as Carathéodory was called among friends) had in mind. The only possible point of friction around 1918 could have been the offer of a chair in Göttingen to Brouwer, who subsequently turned it down. But is hard to see how that could have bothered Hilbert.

<sup>59</sup>Einstein to Carathéodory, 23.X.1928.

<sup>60</sup>This probably refers to the troubles in Schleswig-Holstein during roughly the same period when farmers resisted the tax policies of the government. Hans Fallada has sketched the episode in his *Bauern, Bonzen und Bomben*.

<sup>61</sup>Oral communication, Mrs. F. Heyting-van Anrooy.

Einstein's somewhat crude characterisation of Brouwer in the letter of 19 October may also have been prompted by a wish to pacify the unstable Hilbert. There is no better remedy to calm a person down, than by outdoing him.

It did not take Brouwer long to react. Brouwer was a man of great sensitivity, and when emotionally excited he was frequently subject to nervous fits. In the days following Carathéodory's visit, Brouwer was actually physically incapacitated (see p. 554).

On 2 November Brouwer sent letters to Blumenthal and Carathéodory, from which only the copy of the first one is in the Brouwer archive—it contained a report of Carathéodory's visit. The letter stated that 'in calm deliberation a decision on Carathéodory's request was reached'.

The answer to Carathéodory, as reproduced in the letter to Blumenthal, was brief:

Dear Colleague,

After close consideration and extensive consultation I have to take the position that the request from you to me, to behave with respect to Hilbert as to one of unsound mind, qualifies for compliance only if it should reach me in writing from Mrs. Hilbert and Hilbert's physician.

Yours

L.E.J. Brouwer

This solution, although perhaps a clever move in a political game of chess or in a court of law, was of course totally unacceptable—even worse, it was a misjudgement of the situation. The prevailing view is that a gentleman rather suffers the accusations of an unaccountable person, than to mention this unaccountability. Cara was thoroughly upset; he reported the outcome of his trip to Blaricum in a letter to Courant,<sup>62</sup> the idea had been that by explaining that Hilbert had been acting under the pressure of his illness, the pill could be sugared, thus making it possible for Brouwer to withdraw voluntarily. Erhard Schmidt had thought that this would satisfy Brouwer. At first Brouwer was quite sensible, Cara wrote, and he promised to do nothing before he had talked to Schmidt. 'Unfortunately I have today received a totally absurd letter from Brouwer, so that my whole action seems to have fallen through.' If there was any bright spot at all, he said, it was that now the Berlin mathematicians would no longer unconditionally support Brouwer, and that would reduce the risk that the whole matter got into the open. Being a man of honour, Carathéodory added, 'However, after my mediation has so sadly failed, I must resign as soon as possible from the board of the *Annalen*.'

Blumenthal's first reaction was guarded, 'With Brouwer it's complete chaos, you will hear soon enough. Hilbert must not hear about Cara's trip.'<sup>63</sup>

Hilbert's brief note had triggered a development that would have greatly surprised him. He had dismissed an associate editor, and that was it. The idea that he

<sup>62</sup>Carathéodory to Courant, 3.XI.1928.

<sup>63</sup>Blumenthal to Courant, 4.XI.1928.



had to justify his decision would not have crossed his mind. For Brouwer, on the other hand, it was unthinkable that one could fire an editor just because of an ‘incompatibility of fundamental views’. In particular, since the formulation allowed only one interpretation: ‘no intuitionist on my board’, Brouwer had every right to be upset. And so would every well meaning editor. For Brouwer, giving in meant swallowing a grave insult; for the Hilbert side, Brouwer’s demand as formulated above, was equally unacceptable.

Hilbert, like one of his ideal statements, had in the mean time been eliminated from the discussion. For his protection, his friends and students had decided to avoid any excitement that could harm his precarious health. And so the defence of the old master was taken up by the younger Hilbertians. There must have been quite a bit of hurried consultation, which could not have been all that easy, as Hilbert’s extremely negative view of Brouwer was certainly not universally shared. Most editors were of course aware of the skirmishes in the *Grundlagenstreit*, but they would not dream of considering Brouwer a poor editor or a political risk for the *Annalen*. In fact, Brouwer was on friendly terms with most German mathematicians, be it from Göttingen, or elsewhere. There were of course obvious exceptions, such as Koebe, but on the whole he was a welcome guest at any university.

The conflict had presented itself so suddenly and so totally unexpectedly to Brouwer that he failed to realise to what extent Hilbert saw him as a deadly danger for mathematics, and as the bane of the *Mathematische Annalen*. His belief that the announced dismissal was the whim of a sick and temporarily deranged man emerges from a letter he dispatched to Mrs. Hilbert three days later:

I beg you, use your influence on your husband, so that he does not pursue what he has undertaken against me. Not because it is going to hurt him and me, but in the first place because it is wrong, and because in his heart he is too good for this. For the time being I have, of course, to defend myself, but I hope that it will be restricted to an incident within the board of editors of the *Annalen*, and that the outer world will not notice anything.

A copy of this letter went to Courant with a friendly note, asking him (among other things) to keep an eye on the matter: ‘As a matter of course, I count especially on you to bring Hilbert to reason, and to make sure that a scandal will be avoided.’<sup>64</sup> Courant, after visiting Mrs. Hilbert, replied to Brouwer that Hilbert was in this matter under nobody’s influence, and that it was impossible to exert any influence on him.<sup>65</sup> The reader should realise that there was no animosity between Courant and Brouwer. As the *Annalen* affair left in the end deep scars and lasting aversions, it is well to keep in mind that there were no hidden or open personal conflicts between Brouwer and the other members of the editorial board. It was not as if a bone had to be picked. In particular there was no bad blood between Brouwer and Courant,

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<sup>64</sup>Brouwer to Courant, 6.XI.1928.

<sup>65</sup>Courant to Brouwer, 10.XI.1928.

the favoured assistant of Hilbert. Indeed sometime in the past Brouwer had warmly recommended Courant for a mathematics chair in Münster.<sup>66</sup>

Apart from Einstein, who kept a strict neutrality, all the editors (mostly reluctantly) did take sides—the majority with Hilbert. Hilbert himself, however, no longer took part in the conflict. His position was fixed once and for all, and in view of his illness the developments were as far as possible kept from him (e.g. Blumenthal stressed in a letter to Courant that Cara's attempted intervention with Brouwer should be kept secret from Hilbert).<sup>67</sup> One might wonder whether Brouwer, as a relative outsider (one of the three non-Germans among the editors), stood a chance from the beginning; his letter of November 2 to Carathéodory doubtlessly lost him a good deal of sympathy and proved a weapon to his opponents.

In a circular letter of 5 November 1928 Brouwer appealed directly to the publisher and the editors, thus widening the circle of persons involved. The letter was clearly addressed to all editors, both chief and associate. This, of course, widened the circle of the informed, and it would make it more difficult for the Hilbert side to sweep the matter under the carpet.

*To the publisher and the editors of the Mathematische Annalen.*

From information communicated to me by one of the chief editors of the *Mathematische Annalen* at the occasion of a visit on 30-10-1928 I gather the following:

1. That during the last years, as a consequence of differences of opinion between me and Hilbert, which had nothing to do with the editing of the *Mathematische Annalen* (my turning down of the offer of a chair in Göttingen, conflict between formalism and intuitionism, difference in opinion concerning the moral position of the Bologna congress), Hilbert had developed a continuously increasing anger against me.

2. That lately Hilbert had repeatedly announced his intention to remove me from the board of editors of the *Mathematische Annalen*, and this with the argument that he could no longer 'co-operate' (*zusammenarbeiten*) with me.

3. That this argument was only a pretext, because in the editorial board of the *Mathematische Annalen* there has never been a co-operation between Hilbert and me (just as there has been no co-operation between me and various other editors). I have not even exchanged any letters with Hilbert since many years and that I have only superficially talked to him (the last time in July 1926).<sup>68</sup>

4. That the real grounds lie in the wish, dictated by Hilbert's anger, to harm and damage me in some way.

5. That the equal rights among the editors (repeatedly stressed by the editorial board within and outside the board)<sup>\*)</sup> allow a fulfilment of Hilbert's will

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<sup>66</sup>Cf. Freudenthal to Hopf, 22.I.1932. Freudenthal wrote that in Brouwer's opinion Hilbert was not pleased at all, feeling that Brouwer was trying to do him out of a valuable collaborator.

<sup>67</sup>Cf. p. 558.

<sup>68</sup>Cf. p. 528. The last preserved written communication was dated 20.VIII.1919.

only in so far that from the total board a majority should vote for my expulsion. That such a majority is scarcely to be thought of, since I belong to the most active members of the editorial board of the *Mathematische Annalen*, since no editor ever had the slightest objection against the manner in which I fulfil my editorial activities, and since my departure from the board, both for the future contents and for the future status of the *Annalen*, would mean a definite loss.

6. That, however, the often proclaimed equal rights, from the point of view of the chief editors, was only a mask, now to be thrown off. That as a matter of fact the chief editors wanted (and considered themselves legally competent) to take it upon themselves to remove me from the editorial board.

7. That Carathéodory and Blumenthal explain their co-operation in this undertaking by the fact that they estimate the advantages of it for Hilbert's state of health higher than my rights and honour and freedom of action (*Wirkungsmöglichkeiten*), and than the moral prestige and scientific contents of the *Mathematische Annalen* that are to be sacrificed.

I now appeal to your sense of chivalry and most of all to your respect for Felix Klein's memory, and I beg you to act in such a way that either the chief editors abandon this undertaking, or that the remaining editors split off and carry on the tradition of Klein in the management of the journal by themselves.

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\*) [Brouwer's note] (From the editorial obituary of Felix Klein, written by Carathéodory) 'He (Klein) has taken care that the various schools of mathematics were represented in the editorial board and that the editors operated with equal rights alongside of himself—He has (...) never heeded his own person, always had kept in view the goal to be achieved.' (From a letter from Blumenthal to me, 13-9-1927). 'I believe that you overestimate the meaning of the distinction between editors in large and small print. It seems to me that we all have equal rights. In particular we can speak for the *Annalenredaktion* if and only if we have made sure of the approval of the editors interested in the matter under consideration.—Although I too take the distinction between the two kinds of editors more to be typographical than factual (I make an exception for myself as managing editor), I understand your wish for a better typographical make up very well. You know that I personally warmly support it. However, we can for the time being, as long as Hilbert's health is in such a shaky state as it is now, change nothing in the editorial board. I thus cordially beg you to leave your wish for later. At the right moment I will certainly and gladly bring it out.'

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Laren, 5 November 1928

L.E.J. Brouwer

The above circular letter was dispatched at the same day as Brouwer's plea to Mrs. Hilbert; the two letters are in striking contrast. One letter is written on a con-

ciliatory note, the other is a determined defence and closes with an unmistakable incitement to mutiny.

Blumenthal immediately took the matter in hand. He wrote to the publisher and the editors<sup>69</sup> to ignore the letter until he had prepared a rejoinder. The draft of the rejoinder was sent off to Courant on November 12, with instructions to wait for Carathéodory's approval and to send subsequently copies to Bieberbach, Hölder, von Dyck, Einstein and Springer. It appears from the accompanying letter that Carathéodory had already handed in his resignation, although he had given Blumenthal permission to postpone its announcement, so that it would not give food to the rumour that Carathéodory had turned against Hilbert.

Blumenthal, being the acting managing editor, had more or less assumed responsibility for the defence of Hilbert. It is hard to understand that he, who had been a close friend of Brouwer's, could from one day to the next turn against his former friend, and organise a campaign against him. It is not unlikely, however, that the Riemann affair, and the subsequent Bologna affair, had already introduced a measure of estrangement. It was of course well-known that Hilbert ran a tight ship, but it still comes as a bit of a shock to see that Blumenthal actually feared that Carathéodory would be banned from Hilbert's circle: 'Poor Cara has, in spite of his best intentions, got himself into a tight corner, and I don't know yet if Hilbert will break off relations with him.'<sup>70</sup>

In the meantime Brouwer had travelled to Berlin to talk the matter over with Erhard Schmidt, and to explain his position to the publisher Ferdinand Springer. Brouwer, accompanied by Bieberbach, called at the Berlin office of Springer. The visit is described in a memorandum *Aktennotiz* 'Unannounced and unexpected visit of Professor Bieberbach and Professor Brouwer' (13 November 1928). As Springer wrote, his first idea was to refuse to receive the gentlemen, but he then realised that a refusal would provide propaganda material for the opposition. Springer opened the discussion with the remark that he was firmly resolved not to get involved in the skirmishes and that he did not consider the *Annalen* the sole property of the Company (like other journals), but that the proper *Herausgeber*, Klein and Hilbert, had in a sense entrusted it to the publisher. Moreover he would choose Hilbert's side, out of friendship and admiration, if he would be forced to choose sides.

The unwelcome visitors then proceeded to inquire into the legal position of Hilbert, a topic that Springer was not prepared to discuss without the advice of his friends and which he could not enter into without consulting the contract.<sup>71</sup> Thence the two gentlemen proceeded to 'threaten to damage the *Annalen* and my business interests. Attacks on the publishing house, which could get the reputation of lack of national feelings among German mathematicians, could be expected.'

Springer took this stoically, and assured that he would know how to react to such statements, but that he would accept any negative effects without complaint.

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<sup>69</sup>Blumenthal to the editors, 16.XI.1928.

<sup>70</sup>Blumenthal to Courant, 28.XI.1928.

<sup>71</sup>The contract had indeed a clause to the effect that changes in the editorial board required the assent of the publisher. The problem was of course how to interpret 'editor'.

The implicit threat was definitely in bad taste, not in the last place because the Springer family had Jewish ancestry. Bieberbach's later political views have gained a good measure of notoriety;<sup>72</sup> it certainly is true that already before the take-over of the Third Reich his views had grown more and more nationalistic. Brouwer's position on these matters was neither political, nor nationalistic, it was dictated by his extreme aversion of the scientific boycott of Germany. Nonetheless, the above lines show that, wittingly or not, he ran the risk of being associated with right wing Germans.

Thus rejected, Bieberbach and Brouwer asked if Springer could suggest a mediator, to which Springer answered that he was not sufficiently familiar with the personal information involved, but that two *deutschfreundliche* foreigners like Harald Bohr and G.H. Hardy might do.<sup>73</sup> Before leaving, Brouwer threatened to found a new journal with De Gruyter, and Bieberbach declared that he would resign from the board of editors if it definitively came to the exclusion of Brouwer. In a letter to Courant (13 November 1928) Springer dryly commented 'On the whole the founding of a new journal, wholly under Brouwer's supervision, would be the best solution to all difficulties.'<sup>74</sup> He also conveyed his impression of the visit: 'I would like to add that Brouwer, as a matter of fact, does make a scarcely pleasant (*unerfreulich*) impression. It seems, moreover, that he will carry the fight to the bitter end (*der Kampf bis aufs Messer führen wird*).'

In Aachen, Blumenthal was preparing his defence of the announced dismissal of Brouwer and, following an old strategic tradition, he took to the attack. After consulting Courant, Carathéodory and Bohr he drew up a kind of indictment. From a letter from Bohr and Courant to Blumenthal,<sup>75</sup> one may infer that the draft of 12 November was harder in tone and more comprehensive than the final version. There is mention of a detailed criticism of Brouwer's editorial activities and of matters of formulation ('... leave out capriciousness (*Schrullenhaftigkeit*)...').

Carathéodory remained an uncertain factor in the coming power play; Bohr and Courant realised this, and they therefore would prefer him to do his bit: 'We feel that Cara should make a stand himself because of the misuse Brouwer made of his kindness, respectively that he should explicitly authorise you to use defensive words.' A clever suggestion, but not exactly considerate. Bohr and Courant explicitly warned Blumenthal:

To what extent Brouwer exploits without consideration every tactical advantage that is offered to him, and how dangerous his personal influence is

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<sup>72</sup>Cf. Mehrrens (1987).

<sup>73</sup>This suggestion of the publisher encouraged the impression that the conflict had a political origin. Blumenthal complained to Courant (letter of 18 November 1928) '... the bad thing is, that Brouwer managed to move everything on to the political plane, just what Carathéodory thought he had prevented'. The idea of mediation was not pursued.

<sup>74</sup>Brouwer indeed founded a new journal, the *Compositio Mathematica*, with the Dutch publisher Noordhoff.

<sup>75</sup>Bohr and Courant to Blumenthal, 14.XI.1928.

(Bieberbach), can be seen from the enclosed notice which Springer has just sent us.<sup>76</sup>

The correspondence of Blumenthal, Bohr, and Courant shows an unlimited loyalty to Hilbert, which it would be unjust to ascribe to Hilbert's state of health alone. There is no doubt that Hilbert as a man and a scientist inspired a great deal of loyalty in others, let alone in his students. Sentences like 'We don't particularly have to stress that we are, like you, wholly on Hilbert's side, and also, when necessary, prepared for action',<sup>77</sup> illustrate the feeling among Hilbert's students.

A revised version of Blumenthal's letter is dated November 16, and it is this version that was in Brouwer's possession. It incorporated remarks of Bohr and Courant, but not yet those (at least not all of them) of Carathéodory. It contained a concise *resumé* of the affair so far, and proceeds to answer Brouwer's points (from the letter of 5 November 1928).

Blumenthal partly based his handling of the matter on correspondence, partly on conversations with Hilbert in Bologna. The contents of the latter conversation remain a matter of conjecture, but it may be guessed that in August at the conference Hilbert had made clear his objections to Brouwer—in particular after Brouwer's opposition to the German participation in the conference.

From Blumenthal's circular letter, the editors—and also Brouwer—learned the contents of Hilbert's letter of October 25.

In view of the importance of the letter, it is worthwhile to reproduce Blumenthal's letter here.

To the publisher and the editors of the *Mathematische Annalen*.

As manager of the board of the *Annalen*, I feel obliged to reply to Brouwer's circular letter to the publisher and the editors of the *Mathematische Annalen*. My exposition relies in part on letters of Hilbert, Carathéodory, and Brouwer, in part on an extensive discussion I had with Hilbert in Bologna.

I would like to point out in advance that the formulation of Brouwer's letter is misleading: one can get from it the impression that the editor who visited Brouwer on October 30 (Carathéodory) formulated the statements 1–7. This is of course not the case for any of them, these are rather opinions that Brouwer formed himself.

In the following I give a brief report of the developments, and react to the relevant points of Brouwer's letter.

Blumenthal proceeded to quote Hilbert's letter of 25 October to Brouwer in full (cf. p. 555) and continued

Brouwer has not opened this letter, as I should note already here, and as I explain later. He is, however, informed by Carathéodory about its content, in

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<sup>76</sup>The above mentioned *Aktennotiz*.

<sup>77</sup>Ibid.

particular also of the motivation for Hilbert's action given in the first sentence. Brouwer's points 2 and 3 refer to this. About this I have to say the following:

*On points 2 and 3.* Brouwer interprets the notion of co-operation in a literal sense (point 3). This is complete misapprehension of the true meaning. It is rather the case that Hilbert had become convinced that Brouwer's activity was detrimental for the *Annalen*, and that he could therefore no longer take the responsibility to act as a chief editor in an editorial board, to which Brouwer belonged. By no means does this concern a pretext.

*On points 1 and 4.* The motivations for Hilbert's way of operating, indicated by Brouwer, in these points is not correct. The motivation in point 4 is spiteful and therefore requires no refutation. Also the scientific differences with respect to the foundations, of which one could think, play no role. In particular it is not true what Brouwer seems to suggest in point 5, that the mathematical direction, represented by him, will in future get less opportunity to speak. Also Brouwer's circular letter before the Bologna congress, the expression of which Hilbert found insulting, has only in conjunction with other, perhaps more important, factors acted as a catalyst on his decision. The causes lie much deeper. I will give them in my formulation, but I am certain to get Hilbert's meaning precisely.

Felix Klein had, until his resignation from the editorial board, formed among us a kind of supervising body, that in difficult cases could be called in, or that acted on its own initiative, to support important decisions (for example the transfer of the *Annalen* to Springer Verlag), or to smooth disagreements inside the editorial board. It is good and necessary that in a numerous board like ours, there is such a supreme body available, that, relieved from the details of the management, keeps an eye on the general relations and feels responsible for these. After Klein's death, Hilbert had felt obliged to fulfil this position, and has already acted in this sense, and I for one have also personally always instinctively recognised him as such.

Hilbert saw in Brouwer a headstrong, unpredictable and domineering character. He had feared that, when he at some time should have left the board, Brouwer would bend it to his will, and he has judged this such a great danger for the *Annalen*, that he wanted to stand in his way as long as he still could do so. Probably under the influence of his renewed illness, he felt obliged in the interest of the *Annalen* to order Brouwer's exit from the board, and to tackle this measure immediately and with all energy.

Cara and I, who were associated with Brouwer in a long-standing friendship, had objectively to recognise Hilbert's objections to Brouwer's editorial activity.

True, Brouwer was a very conscientious and active editor, but he was quite difficult in his dealings with the managing editor and he subjected the authors to hardships that were hard to bear.

For example, manuscripts that were submitted for refereeing to him lay around for months, while in principle he had prepared a copy of each submitted paper (I recently had an example of this practice). Above all there is

no doubt that Klein's premature resignation from the editorial board is to be traced back to Brouwer's rude behaviour (in a matter in which Brouwer was formally right). The further course of events has shown that Hilbert was even far more right than we thought at the time.

Since we could not reject the objective justification of Hilbert's point of view, and were confronted by his immutable will, we have given our permission for the removal of Brouwer from the editorial board.<sup>78</sup> We only wished—unjustifiedly, as I now realise—a milder form, in the sense that Brouwer should be prevailed upon to resign. Hilbert could not be induced to this procedure, so we finally, though reluctantly, have decided to give in to him (*den Weg freigegeben*). Einstein did not comply with the argument that one should not take Brouwer's peculiarities seriously.

*Point 5 and 6.* In how far it was justified that the other editors were not first informed of Hilbert's plan, I don't want to go into here. Formally speaking the justification seems to be given by the distinction between 'Mitwirkenden' and 'Herausgeber' on the cover.

*The events after the dispatch of Hilbert's letter.*

On October 26 and 27 Cara and I were in Göttingen to discuss the situation. Subsequently Cara travelled in the interest of the matter to Berlin. Although he saw objectively that Brouwer's eviction was unavoidable, he decided in Berlin to make a last attempt, to settle the matter in an amicable sense, by weakening the categorical form of the expulsion. For that reason he came on the thirtieth of October to Laren, after Brouwer had been asked in advance, by telegram, not to take any steps until Cara's arrival. Since Brouwer had not opened Hilbert's letter, Cara informed him of the content (but not the formulation), and proposed him to resign of his own free will from the editorial board and to leave the letter unopened. He wanted to prevent that Brouwer would feel insulted by the form, and he felt justified to do so, as the rudeness seemed partially determined by Hilbert's ailing health.

He did not make it clear to Brouwer that in our opinion he had to leave the board, and bade him, out of consideration with Hilbert and his illness of that moment, to withdraw by himself. Brouwer reserved a decision until further calm deliberation. He had left Hilbert's letter unopened, and written to Cara on November 2 the following letter:

At this place Blumenthal inserted the text of letter of Brouwer of 2 November, see p. 558.

For this frightful and repulsive letter, which Brouwer also sent to me in copy, I can only offer the explanation that Brouwer (on purpose or inadver-

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<sup>78</sup>Blumenthal is here less than truthful. Cara had not authorised Hilbert. He had probably tried to pacify Hilbert, who took this for the desired approval. Cara wrote, referring to Blumenthal's draft, 'In fact I have not given an authorisation in the legal sense in my letter to Hilbert.' (Carathéodory to Courant, 14.XI.1928.)



tently) had put together from Cara's statements and entreats precisely the ugliest part. I must confess, and Cara has written me likewise, that I have been thoroughly deceived in Brouwer's character, and that Hilbert has known and judged him better than we did. I too am no longer in a position to co-operate further with the writer of this letter in the board, and I now side actively with Hilbert. I cannot understand that Brouwer after this letter can appeal to the chivalry of the editors and to the memory of Felix Klein.

I beg the gentlemen either for a speedy reaction, or for their tacit consent, that from the next issue Brouwer's name is no longer on the cover, and that he no longer gets my *Annalen* information.

So far the 'case for the prosecution'. As an indictment the above letter did not make a convincing impression. It was written to refute Brouwer's points, and to justify Hilbert's decision. In neither was Blumenthal very successful. Admittedly he was in a difficult position, he had read Hilbert's letter to his fellow '*Herausgeber*', and that letter listed some concrete complaints. But he could not very well use these, as the letter was for the chief editors only, he could not ask Hilbert's permission to use the letter, because Hilbert was not supposed to know what was going on, and finally, Hilbert's complaints were very subjective, another editor would probably have seen the mentioned facts in a completely different light. Worse, these facts would have given ammunition to Brouwer.

Blumenthal indeed acted as if he had direct access to Hilbert's thoughts. For example, in the case of the refutation of Brouwer's points 1 and 4, he gave no evidence, he just denied Brouwer's statements. Looking at the evidence, cf. p. 551, one cannot but conclude that the *Grundlagenstreit*, the Bologna affair, and the Riemann affair brought out some uncontrolled emotions on Hilbert's side. Hilbert's references to intuitionism and Brouwer went beyond scholarly comment. The matter of the Göttingen chair is not as clear. Position bargaining was a normal thing in Germany, and that Hilbert's offer was used by Brouwer to improve his situation in Amsterdam would not have surprised anybody. And yet—might not Hilbert have taken Brouwer's decision ill? After all, he was planning to get the leading topologist for Göttingen, and perhaps, in a corner of his heart, he saw Brouwer as a useful addition to his foundational team—the discussion in 1909 might have left some memories. And there is Carathéodory's statement that Hilbert's objections go back ten years.

Considering all evidence, one would be inclined to side with Brouwer rather than Blumenthal. Point 4 is a debatable one. When not under the influence of a fatal disease, would Hilbert have had no wish to hurt Brouwer? From the height of his Olympus he might have acted objectively and impersonally: 'I cannot get along with this man, so he'd better go.' But perhaps Hilbert, when not on his Olympus, was enough of a man of flesh and blood to be susceptible to the 'I'll get you' mood. Whatever was the real state of affairs, the point is subjective, but precisely for that reason Blumenthal should not have dismissed the point so perfunctorily.

Hilbert, in the letter to his fellow chief editors, mentioned three points: (1) the insult implicit in Brouwer's Bologna circular letter; (2) Brouwer's anti-*Conseil* and anti-Union feelings as standing in the way of his editorial work; and (3) the *Annalen* should remain in Göttingen. The heart of Hilbert's argument lies in (2). For, one

could hardly accept (1) as a ground for dismissal.<sup>79</sup> If Hilbert was so sensitive about insults, had he forgotten his words in the second Hamburg lecture? And (3) was rather irrelevant. Journals have no fixed abodes (not counting the proceedings of academies, and the like). Of course, it is a pleasure and an honour for a department to be almost a synonym for a prestigious journal, but all these things would pass one day. Whatever Hilbert's assessment may have been, there is not the slightest indication that Brouwer would have tried to move the *Annalen* elsewhere. Knowing Brouwer's attachment to Göttingen, he would have insisted on keeping the journal where it was.

Yet, even a man like Carathéodory, to whom the term 'the milk of human kindness' could justifiably be applied, had his reservations on this point. It tells something about the internal frictions in German mathematics, that this man, who obtained a doctorate and habilitation in Göttingen, and who was for some time Felix Klein's successor, was critical of the Göttingen imperialism. It should be added that he knew the mathematical world better than most, having studied in Berlin, where he briefly held a chair, and at the time of the *Annalen* conflict he was professor in Munich. Nonetheless, it might have come as a surprise to Courant when Carathéodory told him that Hilbert's claim was for him one of the grounds to resign from the *Mathematische Annalen*.

No, Hilbert saw problems with, in particular, French authors. Brouwer, if he were a chief editor, might, in his opinion, block a paper of Painlevé, or of Picard. Here he was mistaken; Brouwer could very well distinguish between an individual and an organisation. We may recall that Brouwer had lectured Denjoy at length for not observing this distinction.<sup>80</sup> His objections against an invitation of Painlevé to participate in the memorial volume of Riemann was based on the invitation and on the special occasion. As it was, Brouwer was only an associate editor with no influence on papers that were not refereed by him. No matter how one looks at this point, one cannot but conclude that Hilbert's fears were mostly the product of his unfortunate health situation.

Anybody with a cool mind could see that the Bologna conference had restored the international character of mathematics, and that the nationalistic differences would no longer play a role. This clearly had escaped Hilbert when he was in Bologna. Either he was fixed on the past, or he had an axe to grind.

A few editors responded to Blumenthal's letter in writing, but the majority remained silent. Only von Dyck, Hölder and Bieberbach<sup>81</sup> sent their comments. Von Dyck could 'neither justify Brouwer's views nor Hilbert's action' and he hoped that a peaceful solution could be found. Hölder was of the opinion that he could not approve of a removal of Brouwer by force (November 27).

Bieberbach's letter showed a thorough appreciation of the situation. And he at least, was willing to take up the case of the underdog. In view of his later political

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<sup>79</sup>If anything, Brouwer's circular letter could be condemned rather as divisive, than as insulting.

<sup>80</sup>Cf. Sect. 9.3.

<sup>81</sup>Bieberbach to Blumenthal, 24.XI.1928.

extremism, one might be inclined to question the purity of his motives; however in the present letter there is no reason not to take his arguments at their face value. Like Brouwer, and probably the majority if not the whole of the editorial board, he contested the right of the *Herausgeber* to decide matters without the support of the majority of all the editors, let alone without consultation. Indeed this seems to be a shaky point in the whole procedure. Bieberbach referred to the ‘Innsbruck resolution’, which laid down that the chief editor had no claim to the dismissal of editors. ‘And now’, he said, ‘the right to dismiss editors should follow from typographical characteristics. That would reduce the members of the editorial board to subalterns, who can be dismissed any day by the chief editors.’

As a matter of fact, the contract between Springer and the *Herausgeber*<sup>82</sup> is not very concrete in this particular point. It states: ‘Changes in the membership of the editorial board require the approval of the publisher.’ The correspondence makes it clear that Hilbert did not observe this rule. Bieberbach observed that a delay in handling papers cannot be taken seriously as grounds for dismissal: such things ought to be discussed in the annual meeting of the board.

He devoted a few lines to the procedure proposed by Blumenthal. Blumenthal had not asked for a vote on the expulsion of Brouwer, his plan was to decide the matter on the basis of individual reactions plus the ‘silence lends consent’-principle, without even mentioning a time limit. Such a procedure contradicted the most elementary principles of justice, Bieberbach observed. The exclusion of an editor should be handled with extreme care. ‘I would consider it correct’, he continued, ‘that in our circle the question of expulsion should only be made a point of discussion if such a sensational failure of one editor is the case, that one count on unanimous agreement, not, however, if beforehand a prominent member of the board of chief editors, such as Einstein, opposes the exclusion, . . .’

As one of the main dissidents in the matter of the Bologna affair, Bieberbach added some ‘scholastic’ comments to Blumenthal’s report of Hilbert’s feelings.

Brouwer’s pamphlet was directed at those visiting the Union-congress in Bologna. As Hilbert himself claimed, the Bologna conference was *not* a Union-congress, how could he be insulted?

Bieberbach found no difficulty in dissecting Blumenthal’s case against Brouwer. He concluded that ‘A dismissal without any notice of an editor, who is moreover a scientist of world fame, after thirteen years of diligent activity could only be justified by defamatory actions or so, not by incidents that only hold inconveniences for the editor in chief (*geschäftsführender Redakteur*)’.

Blumenthal had been making the most of Brouwer’s, admittedly tactless, letter to Mrs. Hilbert. Bieberbach correctly spotted a serious flaw in Blumenthal’s charge involving Brouwer’s ‘terrifying and repulsive’ letter.

Finally I hold it totally unjustified to concoct material against Brouwer from letters that he wrote after learning about the action that was mounted against himself. For it is morally impossible to use actions, to which a person

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<sup>82</sup>25.II.1920.

is driven in a fully understandable emotion over an injustice that is inflicted on him, afterwards as a justification of this injustice itself.

The point is well taken. It does not exonerate Brouwer, but it at least makes clear that to use it against Brouwer is in poor taste. Bieberbach explicitly stated that he would not support Brouwer's dismissal; on the contrary, he strongly sided with Brouwer, without, however, attacking Hilbert.

There is a certain tendency to dismiss Bieberbach's statements and opinions on the basis of his later political views and actions. Needless to say that this does not go well with rational reflection. Be that as it may, Bieberbach's letter painfully exposed the weaknesses in the case against Brouwer.

The publisher reacted in a cautious way. Springer thought that Brouwer was 'an embittered and malicious adversary', and that he should not receive a copy of the circular letter without the permission of the lawyer of the firm. Springer also concluded that the publisher should not state in writing that he officially agreed to Brouwer's dismissal, because it would imply a recognition of Brouwer's membership of the board of editors in the sense of the contract. In short, Springer abstained from voting on Blumenthal's proposal.

At this point the whole action against Brouwer seemed to have reached an anti-climax. One may surmise a good deal of activity in the camp of the Göttingers. The matter now began to take on national proportions. In view of the barely veiled animosity between Berlin and Göttingen, as the ultimate bastions of mathematics, and Brouwer's close connections with the Berlin mathematicians, the temporary leaders of the Göttingen group started to worry that the Brouwer dismissal might lead to an open rift between the two groups. This might even lead to further unpleasantness, as the main loyalties among German mathematicians were with Berlin or Göttingen; once a Göttinger, always a Göttinger, and the same for Berlin. It thus became a matter of some urgency to settle the Brouwer matter. Had Hilbert, much like the ghost of Hamlet's father, not continually hovered in the background, the editor's would have been able to find a compromise, so that Brouwer could stay on. Unfortunately the outcome was fixed in advance. Nonetheless the Hilbert party had its worries.

If only Einstein could be persuaded to give his assent...! Although it seemed doubtful that anyone could succeed where even Hilbert had failed, Max Born tried to convince Einstein to remain at least neutral. In a lengthy letter he summed up the arguments, sketched the mood, outlined the consequences. Hilbert, he said, was so ill that he wouldn't live much longer. Every emotion could do serious harm, and would shorten the time left to finish his work. 'What is more, he is full of a strong will to live, and he sees it as his task to carry out his new founding of mathematics, to which he has to devote himself with his last strength. His mind is clearer than ever, and the rumour, spread by Brouwer, that Hilbert were not completely *compos mentis* is an extraordinary heartlessness.' Born had spoken with Hilbert on the topic 'Brouwer', and Hilbert had declared that he saw Brouwer as an eccentric and unbalanced man. According to Born, Hilbert considered Brouwer's ultra-German behaviour in the Bologna matter a folly, 'but the dreadful thing was that the Berlin mathematicians fell for Brouwer's nonsense'. Born mentioned Bieberbach,

Von Mises and Schmidt as dupes of Brouwer, a claim that was not borne out by evidence, at least Bieberbach and Von Mises were anti-Bologna-Union on their own accord. Born had travelled with Von Mises in the USSR, and at one occasion Von Mises opened the conversation with, ‘the Göttinger simply ran after Hilbert, who was no longer quite responsible for his actions’ (*unzurechnungsfähig*). Born immediately broke off the conversation, since in his opinion Von Mises was too insignificant to have an opinion on Hilbert. As he pointed out, this was before the Bologna conference, thus before Hilbert’s collapse. In every community there is invariably an amount of gossip floating around, in mathematics no less than in other subjects. It is more than likely that the rumours of Hilbert’s disease, combined with his strange emotional behaviour at the Hamburg and Münster lectures, had encouraged speculation about his mental health. Thus von Mises’ view may have displeased, but not surprised, Born.

In view of a meeting to be held at Springer’s in Berlin, Born begged Einstein not to do anything that might harm Hilbert’s interest. It was important that the chief editors should speak with one voice.

The pressure on Einstein was, from a strategic point of view, understandable. His immense scientific and moral prestige made him a key figure in any debate. If he could be persuaded to side with Hilbert the battle would be half won. In spite of personal pressure from Born (20 November 1928) on behalf of Hilbert, Einstein remained stubbornly neutral. In his letters to Born and to Brouwer and Blumenthal one may sense a measure of disgust behind a facade of raillery. In the letter to Born (November 27) the apt characterisation of ‘*Frosch-Mäusekrieg*’ (war of the frogs and the mice) was introduced.<sup>83</sup> After declaring his strict neutrality he went on:

If Hilbert’s illness did not lend a tragic aspect, this ink war would for me be one of the most funny and successful farces performed by that sort of people who take themselves deadly seriously.

Objectively, I might briefly point out that in my opinion there would have been more painless remedies against an overly large influence on the managing of the *Annalen* by the somewhat mad (*verrückt*) Brouwer, than eviction from the editorial board.

This, however, I only say to you in private, and I do not intend to plunge as a champion into this frog-mice battle with another paper lance.

Einstein’s letter to Brouwer and Blumenthal of November 25 is even more cutting and reproving.

I am sorry that I got into this mathematical wolf-pack (*Wolfsherde*) like an innocent lamb. The sight of the scientific deeds of the men under consideration here impresses me with such cunning of the mind, that I cannot hope also in this extra-scientific matter to reach a somewhat correct judgement of them. Please allow me therefore to persist in my ‘booh-nor-bah’ (*Muh-noch-Mäh*)

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<sup>83</sup>War of the frogs and the mice—a Greek play of unknown author; a late medieval German version, *Froschmeuseler*, is from the hand of Rollenhagen.

position and allow me to stick to my role of astounded contemporary. With best wishes for an ample continuation of this equally noble and important battle, I remain

Yours truly,  
A. Einstein

The whole affair now rapidly reached a deadlock. A week before, Springer had, after seeking legal advice at Blumenthal's urging, written optimistically to Courant<sup>84</sup> that the legal adviser of the firm, E. Kalisher, was of the opinion that it would suffice that those of the four chief editors who did not want to advocate Brouwer's dismissal actively would abstain from voting, thus giving the remaining chief editors a free hand. Apparently Springer did not realise that since two editors with a high reputation had already decided not to support Hilbert, the solution, even if it was legally valid, would lack moral support. If this solution should turn out to raise difficulties within the editorial board, the publisher could still fire the whole editorial board and reappoint Hilbert and his supporters, so the advice ran. In the opinion of the legal adviser the publishing house was contractually bound to the chief editors (*Herausgeber*) only; there was no contract with the remaining editors.

Bieberbach's letter, mentioned above, apparently worried Carathéodory to the extent that he decided to ask a Munich colleague from the law faculty for advice. This advice from Müller-Erbach plainly contradicted the advice from the Springer lawyer. It made clear that

- (1) Brouwer and Springer-Verlag were contractually bound since Brouwer had obtained a fee.
- (2) Hilbert's letter was not legally binding.

Müller-Erbach sketched three solutions to the problem:

- (a) Springer dismisses Brouwer. A letter of dismissal should, however, contain appropriate grounds.
- (b) The four chief editors and the publisher form a company (*Gesellschaft*) and dismiss Brouwer.
- (c) A court of law could count the *Mitarbeiter* as editors. In that case the only way out would be to dissolve the total editorial board and to form a new one.

Carathéodory considered the first two suggestions inappropriate because it would not be fair to saddle Springer with the internal problems of the editors. Hence he recommended the third solution.<sup>85</sup> Here, for the first time, appeared the suggestion that was to be the basis of the eventual outcome of the dispute.

Hilbert, the main contestant in the *Annalen* affair, had quite sensibly withdrawn from the stage. The developments, had he known them, would certainly have done

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<sup>84</sup>Springer to Courant, 17.I.1928.

<sup>85</sup>Cara to Blumenthal, 27.XI.1928.

his health no good. He had authorised Harald Bohr and Richard Courant to represent him legally in matters concerning the *Mathematische Annalen*. Thus the whole matter became more and more a shadow fight between Brouwer and an absentee.

At this point the dispute had reached an impasse. Although Springer upheld in a letter to Bieberbach the principle that the chief editors could dismiss any of the other editors, the impetus of the attack on Brouwer seemed to ebb away. A meeting between Carathéodory, Courant, Blumenthal and Springer had repeatedly to be postponed and finally had been cancelled.

Courant agreed with Carathéodory that the dissolution of the complete board would be a convenient way out;<sup>86</sup> however, it would require a voluntary action from the editors and the ultimate organisation of the editorial board should not have the character of a legal trick with the sole purpose of rendering Brouwer's opposition illusory.

Carathéodory, who, on the basis of Müller-Erbach's information, had come to the conclusion that the original plan of Hilbert, even in a modified form, would not stand up in a court of law, expressed his willingness to assist 'out of devotion to Hilbert' in the liquidation of the affair, but quite firmly refused to be involved in the future organisation of the *Annalen*.

The reluctance of Carathéodory to be involved in the matter beyond the bare minimal efforts to satisfy Hilbert and spare Brouwer (his friend) is throughout understandable. As far as we can judge from the correspondence, only Blumenthal exhibited an unbroken fighting spirit. He realised, however, that his circular had not furthered an acceptable solution,<sup>87</sup> and he leaned towards alternative solutions. In particular, Blumenthal wrote, the time was favourable to Carathéodory's plan. The *Annalen* were completing their hundredth volume, and it would present a perfect occasion to open with volume 101 a 'new series' or 'second series' with a different organisation of the editorial board. But at the present time he was facing a dilemma. Because Hilbert's letter clearly had no legal status, Brouwer was still a *Mitarbeiter* and his name should appear on the cover of the issue that was to appear—this, however, conflicted with Hilbert's wishes. Could Bohr and Courant, as proxies of Hilbert, authorise him to print Brouwer's name on the cover? Otherwise the publication would have to be postponed. The authorisation probably was given.

It seems that Bohr had also put forward a solution to the affair. From the correspondence of Carathéodory and Bohr with Blumenthal, one gets the impression that Bohr's proposal was a slight variant of Carathéodory's suggestion. The main difference was that Bohr advocated a total reorganisation of the editorial board. In his proposal there would only remain *Herausgeber*, and no *Mitarbeiter*. So the solution would look like a fundamental change of policy, and hence it would no longer be recognisable as an act levelled against Brouwer.

Apparently Bohr envisaged Hilbert, Blumenthal, Hecke and Weyl as the members of a new board. And should Weyl decline, one might invite Toeplitz. Blumenthal questioned the wisdom of reinstating himself as an editor; it could easily be

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<sup>86</sup>Courant to Carathéodory, 30.XI.1928.

<sup>87</sup>Blumenthal to Courant and Bohr, 4.XII.1928.

viewed as the old board of chief editors in disguise.<sup>88</sup> In his letter to Courant, the next day, he considered the dissolution of the editorial board at large as necessary, and he fully agreed that Hilbert should choose the new editors.

From then on things moved smoothly; Springer accepted the dissolution of the editorial board and agreed to enter into a contract with Hilbert on the subject of the reorganised *Annalen*. By and large only matters of formulation and legal points remained to be solved.

One might wonder where Brouwer was in all this—he was completely ignored. In a letter of November 30 to the editors and the publisher he confirmed the receipt of Blumenthal's indictment which had only just reached him. In a surprisingly mild reaction he merely asked the editors to reserve their judgement—blissfully unaware that nobody was going to cast a vote—for the composition of a defence would take some days.

Because the dissolution of the editorial board had to be a voluntary act, it was a matter of importance to get Einstein's concurrence. The contract of 1920 presented an elegant loophole that would allow both parties to settle the matter without breaking the rules. In Sect. 5 the clauses for termination of the contract were listed, and one of them stipulated that if the editors (*Redaktion*) renounced the contract, without a violation from the side of the publisher, the latter could continue the *Mathematische Annalen* at will.

Possibly Einstein's agreement could be dispensed with, but it is likely that a decision to ignore Einstein's vote would influence general opinion adversely; moreover, it would be wise to opt for a watertight procedure, as Brouwer would not hesitate to test the outcome in court.

So pressure was brought to bear on Einstein. James Franck, a physicist and a friend of Born, begged him to listen to the new plan. He stressed the political side of the issue, 'At this time, . . . , whether the mathematicians split into factions or whether the affair is arranged smoothly, depends on your decision. It would almost be an inappropriate joke (*ein nicht all zu guter Witz*) if in this case you would be claimed for the nationalistic side' (undated). Franck was not the only person to discover a (real or imaginary) political aspect in the controversy at hand. Blumenthal had already complained to Courant (November 18) that Brouwer had managed to introduce the political element into the matter. Born also, in his letter to Einstein of November 11, tied the conflict to the political issue of the German nationalists and the animosity of Berlin vs. Göttingen.

The successful conclusion of the undertaking was conveyed to Springer by Courant. In his letter of December 15 he announced the co-operation of Einstein, Carathéodory, Blumenthal and Hilbert in the transition. At the same time he proposed that a new contract be made between Hilbert and the publisher, and that Hilbert get *carte blanche* for organising the editorial board. Blumenthal should be invited to continue his activity as managing editor and, according to Courant, he would probably accept. Also—and this is a surprising misjudgement of Einstein's

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<sup>88</sup>Blumenthal to Bohr, 5.XII.1928.



mood—Courant thought that there was a 50% chance that Einstein would join the new board. As far as he himself was concerned, Courant thought it wiser to postpone his own introduction as an editor until the dust had settled (the matter apparently had been discussed earlier).

Courant had to work hard to prepare the various documents, to solicit comments, make changes, etc. The *Annalen*-new style would have one *Herausgeber*, Hilbert, and a variable (but small) number of *Mitarbeiter*. Hecke and Blumenthal were eventually chosen for the latter function.

The new arrangement promised a satisfactory end to the *Annalen* affair, but not everyone was happy. Blumenthal, for example, cautiously pointed out that Hilbert would become the only chief editor. If he intended any criticism, he was careful to leave it to Courant to read it between the lines.<sup>89</sup> Carathéodory on the other hand openly expressed his disappointment. He, too, deplored the end of the old regime. When confronted with Hecke's comments on the practice of the past (letter from Courant to Carathéodory, December 17): '... that Hecke, when he learned about the organisation of the editorial board and the competence of the *Beirat* [the advisory editors] grasped his head and judged a revision and a more strict organisation absolutely necessary.' Carathéodory heartily disagreed: 'For, Klein had organised the board of editors of the *Mathematische Annalen* in such a way that it formed really a kind of Academy, in which each member had the same rights as the others. That was in my opinion the main reason why the *Annalen* could claim to be the first mathematics journal in the world. Now it will become a journal like all other ones.'<sup>90</sup> It did not take Blumenthal long to recognise the negative sides of the new set up. On 2 February 1929 he sent out a note, 'On the future organisation of the *Annalen*', in which he drew the attention to the decline of the journal compared to other journals. Since the associated editors (*Nebenredaktion*) had been eliminated, one simply needed a larger staff: 'the increasing necessity of scientific advisers follows inevitably from the increasing specialisation'. In short, Blumenthal proposed to reinstate something like the old associate editors under a different name. In the same letter he broached the question of the successor of Hilbert, should he step down. One finds it difficult to reconcile this letter with the arguments that were put forward in favour of the solution to the conflict.

Finally Courant suggested that the publisher alone should inform all present editors of the collective resignation. With respect to Brouwer, he advised Springer to write a personal letter explaining the solution to the conflict, and to stress that he [Springer] would regret it if Brouwer were left with the impression that the whole affair would restrict his freedom of action, and that the publishing house would be at his disposal should he wish to report on his foundational views. It is not known whether this letter was ever written, but Courant's attitude certainly was statesman-like and conciliatory.

The fact that the whole board was going to be dismissed, and that only Hilbert and Blumenthal were going to be reappointed, was an unpleasant message for most,

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<sup>89</sup>Blumenthal to Courant, 16.XII.1928.

<sup>90</sup>Carathéodory to Courant, 19.XII.1928.

if not all, of the sitting editors, but in particular for the senior members Von Dyck and Hölder. Van Dyck had even at one time been a chief editor. It is greatly to Blumenthal's credit that he asked Courant to intervene with Hilbert, so that the latter would write a few nice words to the two; the pill needed a strong dose of sugaring, he said.

Like a good statesman, Courant realised that the past events carried the potential for a long period of friction. Of course he was aware of the reputation of arrogance of the Göttingen group, but he was sensible enough to see that there was life outside Göttingen. As he wrote to Carathéodory,<sup>91</sup> 'We should also think of the future relations between the German mathematicians. If a part of the colleagues does not learn to understand what really motivated Hilbert, then the vexation will not yield and can burst out here and there. If such a latent tension—that will not come from Hilbert's circle—is to be avoided for the future, then one must make use of the present moment to rob the matter of any unjustified ugly appearance and enter into a basis of mutual understanding and trust. It would be gratifying and comforting if you would help us to make all persons involved, in particular our Berlin colleagues, to adopt this position.'

Once the decision was taken, no time was wasted; after the routine legal consultations the publisher carried out the reorganisation and the editors were informed of the outcome (December 27). In spite of Courant's considerations mentioned above, the letter was signed by Hilbert and Springer. Brouwer, like everybody else, was thanked for his work and was given the right to a free copy of the future *Annalen* issues. The matter would have been over, were it not for some rumblings among the former editors and for a desperate but hopeless rearguard action of Brouwer.

Carathéodory had been considerably distressed during the whole affair; from the beginning he had been torn between his loyalty to Hilbert and his abhorrence of the injustice of Brouwer's dismissal. His efforts to mediate had only worsened the matter and the final solution was an immense relief to him. In a fit of despondency he wrote to Courant:<sup>92</sup> 'You cannot imagine how deeply worried I was during the last weeks. I envisioned the possibility that, after I had parted with Brouwer, the same thing would happen with all my other friends.' He had even considered accepting a chair at Stanford that was offered to him. In his answer Courant tried to set Carathéodory's mind at ease:<sup>93</sup> he believed that he had succeeded in convincing Hilbert that Carathéodory, in his position, could not have acted differently; the matter was settled now 'without fears of a residue of resentment on Hilbert's part'. Two days later he wrote that the night before he had discussed the whole matter with Hilbert, who had asked Courant to tell Carathéodory that 'he thinks that you would have done everything for him, as far as possible'. Hilbert was completely satisfied with the result of the undertaking, and in his opinion the *Annalen* were even better protected now than through his original dismissal of Brouwer '... and by and by it

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<sup>91</sup>Courant to Carathéodory, 23.XII.1928.

<sup>92</sup>Carathéodory to Courant, 12.XII.1928.

<sup>93</sup>Courant to Carathéodory, 15.XII.1928.

has become completely clear to me that in fact no personal motives have inspired Hilbert's first step, ...' Carathéodory expressed his pleasure with Hilbert's views but he was not satisfied with Courant's evaluation of the motives behind Hilbert's move.<sup>94</sup> 'Now, he himself has given as the exclusive motive for his decision that he felt insulted by Brouwer; I would find it unworthy of him, to construe after the fact, that only impersonal motives had guided him.' This last remark could hardly be left unanswered by Courant. He had worked hard to pacify the participants in the affair, and here one of the former chief editors was lending support to the rumour that Hilbert was not completely devoid of some personal feelings of revenge. In an attempt to quench this source of dissent he and Bohr admonished Carathéodory. Courant calmly repeated his view and referred to Hilbert's personal statements that he 'fostered no personal feelings of hate, anger or insult against Brouwer'.<sup>95</sup> Even a bit of subtle pressure was brought to bear on Carathéodory: 'Our responsibility to Hilbert at this point is even greater, as he is not yet filled in on the development of the conflict; in particular he does not surmise your visit to Laren and the disconcerting report of it by Brouwer.'

Bohr was less subtle in his approach (same letter); if Carathéodory were not convinced of Hilbert's impersonal motives, he should ask Hilbert himself. 'For, that Hilbert—without being aware of it and without being able to defend himself—should first be considered 'of unsound mind' and then 'not to the point' (*unzurechnungsfähig*. . . *unsachlich*), that is a situation, that I, as a representative of Hilbert, cannot in the long run witness without action.' In spite of Bohr's sabre rattling Carathéodory stuck to his guns: 'To judge Hilbert's motives is a very complicated matter; I believe that I see through his motives because I have known his way of thinking for more than 25 years. It is true that the motivations that you indicate, and which H. also expounded in Bologna in discussion with Blumenthal, were there. The total complex of thoughts, that caused the explosion of feeling of October 15,<sup>96</sup> was much more complicated.'

Who was right, Courant and Bohr, or Carathéodory? The matter will probably never be completely settled. There is no doubt that the question of 'how to safeguard the *Annalen* from Brouwer's negative influence (real or imagined)' was uppermost in Hilbert's mind. But who is to say that no personal motives were involved? There are Hilbert's own statements to the effect that no personal grudge led to his action, e.g. to Blumenthal and Courant, but how much weight can be attached to them? In any case they contradict the letter of October 15.

Finally, there was the 'blackmail' note (see p. 551), which indicates a strong emotion and vexation, if not more. And if Hilbert had personal motives, so what? Would we think less of a person if he were not the cardboard saint that some would prefer him to be?

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<sup>94</sup>Carathéodory to Courant, 19.XII.1928.

<sup>95</sup>Courant to Carathéodory, 23.XII.1928.

<sup>96</sup>Cf. letter to Einstein, October 15.

Were Courant and Bohr themselves all that certain about Hilbert's motives? This question will probably never be answered. The available correspondence is not really informative, the lack of personal motives is systematically given credence by quoting Hilbert. They may have realised this, when they wrote 'Thus it is nothing less than a reconstruction after the fact, if one stresses now at the liquidation these objective motives, although the first step of Hilbert, made under such singular circumstances, could perhaps create another impression.'

The whole problem seemed to have been settled satisfactorily. Hilbert, who was only partially informed of the goings on, wrote to Blumenthal 'a triumphant letter, that everything was glorious'.<sup>97</sup> Courant had written a conciliatory letter to Brouwer in which he expressed the hope that the solution to the matter satisfied Brouwer. He also wished to convince Brouwer that no personal motives had played a role in Hilbert's action, and definitely no motives 'whose existence were in conflict with the respect for your scientific or moral personality'.<sup>98</sup> Little did he know Brouwer!

As a matter of fact Brouwer launched another appeal to the publisher and the editors the same day Courant was offering Brouwer the 'forgive—and—forget' advice. Brouwer insisted that in the interest of mathematics the total editorial board of the *Mathematische Annalen* should remain in function. As he realised that a written defence from his hand would inevitably wreck the unity of the editors, he was willing to postpone such a letter; moreover, Carathéodory, in a letter of December 3, had promised him to do his utmost to find an acceptable solution, and had begged him to be patient for a couple of more weeks. Sommerfeld had also pressed Brouwer to wait for Carathéodory's intervention. The final solution, as formulated in the Hilbert–Springer letter, did *not* satisfy Brouwer. He recognised that the re-organisation of the *Annalen* was mostly, if not wholly, designed to get rid of him. Also, Brouwer had explicit views on the ideal organisation of the *Annalen*. In a circular letter (23 January 1929) to the editors, Blumenthal and Hilbert excluded, Brouwer rejected the final solution. According to him, the *Mathematische Annalen* were a spiritual heritage, a collective property of the total editorial board. The chief editors were, so to speak, appointed by free election and they were merely representatives *vis a vis* the mathematical world. Thus, Brouwer argued, the contractual rights of the chief editors were not a personal but an endowed good. Hilbert and Blumenthal, in his view, had abstracted this good from their principals, and hence were guilty of embezzlement, even if this could by sheer accident not be dealt with by law (the reader may hear a faint echo of Brouwer's objections to the consistency program, see p. 442 of Brouwer 1923c). Brouwer then proceeded to attack Blumenthal's role in the *Annalen*. He repeated Blumenthal's earlier views on the equal rights of all editors and referred to certain irregularities in the management of the *Annalen* in 1925, when Blumenthal had committed an exceptionally strong infringement.<sup>99</sup> Brouwer had only given up his plan to call a meeting of the collective

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<sup>97</sup>Blumenthal to Courant, 31.XII.1928.

<sup>98</sup>Courant to Brouwer, 23.XII.1928.

<sup>99</sup>The German term is 'Übergriff', which could also be translated as 'impertinence'. In view of Blumenthal's reputation, that version seems implausible.

board to discuss Blumenthal's lapse, when Blumenthal made it clear that he planned to give up the position of managing editor no later than at the publication of volume 100. There is no information on this alleged intervention. Since 1925 was the year of the Riemann affair, it might have played a role.

There is concrete evidence that Blumenthal wanted to resign from the *Annalen* board. A stern letter from Hilbert to Blumenthal of November 18, 1925, opens with words that quelled all opposition: '*NEIN*, I do not agree at all with your plan.' Since the letter provides useful background information for the *Annalen* conflict, it is worthwhile to reproduce it here in part.

Already by itself, I would at the moment indeed follow the tendency to reduce the number of members of the editorial board, instead of increase it—including the chief editors, where, by the way, Springer should also have his say. When Cara once wanted to resign, I have—but only for the case that you would insist on an immediate replacement—mentioned who is by far the most important and most generous mathematician of his generation, Hecke as his successor; you turned him down with the argument that you, as a substitute for me, needed a real worker and manager for yourself, and at that point you were right. And now Weyl should be such a person? Weyl, who has a purely academic and luxury professorship, and for the rest lives for his scientific and literary activity, and for his health! And such a man should from Zürich conduct the business of the *Annalen*! His name on the title page were nothing but an honour from us for his person. [...] Thus I come to my main motive for rejecting your plan: if you lay down the managing of the *Annalen*, I would like to get it to Göttingen.

Hilbert wanted in fact to strengthen in Göttingen the connections with physics. But, he continued:

Fortunately you are for the time being prepared to conduct the management, and it is my sincerest and warmest wish that you will do this for a long time to come. But I recommend you—in particular in your own interest not to let yourself be distracted from the trusted practice of your management by external influences, and I recommend a smooth development *without* editorial meetings, *without* changing editors, and *without* formal innovations.

And that was the end, Blumenthal had to stay on, whether he liked it or not. So, maybe Brouwer should not blame him for hanging on. The letter sheds light on a few details that bear on the previous pages. Much of what went on in Göttingen and Berlin was of course circulating in mathematicians' gossip. The problem is that this is hard to trace, and that it is usually coloured one way or another.

The *Annalen* were settling down under the new regime and, due to a tactful handling of all publicity, the excitement in Germany was dying out, even as Courant wrote to Hecke, among the colleagues in Berlin—and Brouwer was completely ignored. After waiting for months—and probably realising that the battle was over and that everybody had gone home—he fired his parting shot, the letter of defence against Blumenthal's indictment of 16 November 1928. The letter is three and a

half folio sheets long and contains a report of the events mentioned above, as experienced by Brouwer. The tone of the letter is bitter, Brouwer felt that he was let down by his supporters, ‘To my astonishment and disappointment up to now no correction has followed from the other side, in spite of my challenge, of the false representations in Blumenthal’s circular letter of 16.11.1928.’ This applied most of all to Carathéodory, who had failed to straighten out Blumenthal’s distorted interpretation of the facts about Carathéodory’s visit. So there was no choice but to speak himself.

In the first place he denied Blumenthal’s claim that Brouwer had substituted his own interpretation for Carathéodory’s version of the developments leading to, and including, Hilbert’s action. The views, he wrote, were not mine, but ‘views that, during the aforementioned visit, came up between Carathéodory and me in mutual agreement, i.e. that were successively uttered by one of us and accepted by the other’. He also elaborated the grounds for not acquiescing in the dismissal. He had told Carathéodory

I would consider a possible dismissal from the editorial board not only a revolting injustice, but also a serious damage to my possibility to function and, in the face of public opinion as an offending insult; that, if it really came to this unbelievable event, my honour and freedom of action could only be restored by the most extensive flight into public opinion.

At the end of the otherwise friendly visit the shadow returned, and the two parted shattered and in grief.

Reading Brouwer’s report, one gets a gloomy impression of a meeting of two friends, confronted with a human tragedy they know they cannot prevent. It was an almost paradoxical situation, both knew that the request was an injustice, yet both knew that a decision had to be taken. In view of the role of Carathéodory’s visit in the history of the conflict, Brouwer’s report follows here:

At his visit on 30.10.1928 Carathéodory informed me first of all, while the two letters lay unopened, that the ‘fact of the greatest consequence, which was unknown to me’ consisted of the following: recently the taking of a wrong medicine had brought out in Hilbert a situation of such a serious nature, that on the one hand ‘he could no longer be taken seriously at all’ (Carathéodory’s words)<sup>100</sup> and that on the other hand the slightest opposition to his will could be fatal for him. In this situation Hilbert got the idea to remove me from the board of the *Annalen*, and wished to realise this idea by all means. It should be evident that the realisation of Hilbert’s plans would mean a scandalous

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<sup>100</sup>[Brouwer’s footnote] One might think for a moment that the communication of such utterances carried something incorrect, because one assumes with respect to these naturally an atmosphere of confidentiality. This assumption of confidence, however, and the solidarity which it presumes, in as far as it is not invalidated by the end of the conversation, is certainly not compatible with Carathéodory’s later silence in the face of Blumenthal’s later false representations. Moreover, even justifiable scruples must take second place in the present case, where a discussion of wicked defamation and isolation of status is concerned—just as in the case of the question of a witness in a court of law.

injustice. In order not to jeopardise Hilbert's life, he (Carathéodory) begged me to take no action against it, for the time being. Hopefully Hilbert would soon return to the right medicine, and as a consequence of the improvement of his condition, regain better views, before anything definite could happen.

One of the two unopened letters was Hilbert's. The message it contained, that Hilbert dismissed me as an editor, 'authorised by Blumenthal and Carathéodory', were not justified; for when he (Carathéodory), after his return from America, received a letter from Hilbert, asking for his authorisation, he had answered that he would on principle not oppose Hilbert, but that he would come to Göttingen to discuss the matter. Arriving in Göttingen he had learned from Blumenthal that Hilbert had dispatched the letter of dismissal, mentioning the above authorisation.<sup>101</sup> In the following conversation of half an hour with Hilbert, the matter was mentioned between them, as little as it was until today. As far as the second letter was concerned (with Blumenthal's name on the cover as sender), this was written by him (Carathéodory), in which he begged me to withdraw of my free will from the editorial board, in consideration of the situation of Hilbert's health. He now regretted, however, to have written this letter.

Subsequently I have returned the second letter unopened to Carathéodory, and declared that I would consider my possible removal from the board not only a scandalous injustice, but also a serious harming of my professional prospects, and in its public aspect, a despicable insult; that should it really come to this unheard of event, my honour and professional prospects could only be regained by means of the most far-reaching flight into publicity; as a consequence the crime practised against me would cause a public scandal—Carathéodory replied that he had been prepared for such a position of mine, that in his expectation the *Annalen* would meet its doom over the realisation of the plan hatched against me. And that he had already made the decision to resign from the editorial board, in which decision could for the time being—again out of consideration for Hilbert's health—not be carried out.

The further course of our conversation then came to the seven points in my circular letter of 5.11.1928.

Concerning the consideration with Hilbert's state of health, demanded by Carathéodory, I gave as my opinion that if there were an immediate mortal danger for Hilbert, it would be a crime to assist him in ending his life with a crime; on the other hand that unreasonable tolerance would possibly only increase his sensibility and lust for power in a manner that would endanger his happiness in life. I promised, however, to consult on these latter psychological questions a suitable acquaintance. In case after further reflection my position would not change, the acceptability of Carathéodory's request, to undertake for the time being nothing against Hilbert's plans, would for me be equivalent to the probability of a cancellation of these plans even without my active

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<sup>101</sup>Only a thoroughly confused person would thus run the risk of a court case for falsification. If true, this supports Carathéodory's and Brouwer's view.

interference. The conversation closed with Carathéodory's repeated reference to Hilbert's terrible state, and the words that he (Carathéodory), under these circumstances, 'appealed to my mercy'.

During this conversation of two hours in the morning of October 30 Carathéodory's position was indeed that of a mediator, friend and ally, who counselled me on the possibilities and means to prevent a calamity. The discussion seemed concluded in full agreement, in spite of the temporary differences in our evaluation of details of the situation. Carathéodory stayed accordingly some more hours at my house with some guests that I had invited at the occasion of his visit; all guests had the impression of a perfect mood. Only at the parting, when I was again alone with Carathéodory, I mentioned a thought that came up at that moment, that, as Hilbert had survived Einstein's objections against his plans, he could also suffer without danger a repudiation of the authorisation, mentioned without justification in his letter to me. Only when I did not get from Carathéodory a reply to the question, but only exclamations such as 'What can one do' and 'I don't want to kill a person' (perhaps to be ascribed to the uneasiness of parting), amazement, uncertainty and irritation came up in me, which expressed themselves, under a complete change of mood at my side, in phrases like 'I cannot follow you any more', 'I consider this visit a farewell', and 'I am sorry for you'.

A fortnight later Brouwer visited Schmidt in Berlin. Schmidt's account roughly agreed with Carathéodory's, but he added one piece of information: Hilbert's anger was to a great extent caused by Brouwer's actions in the Riemann affair.

The second part of Brouwer's circular letter concerns Blumenthal and his indictment. He saw Blumenthal's hand in the action against him, for only Blumenthal had insight in the record of the individual editors. The objections listed by Blumenthal 'could only degenerate into anecdotes, if one would ascribe them to Hilbert. He counts already for years so little as editor, that for a regular handling of the business, it had even been proved to be dangerous to submit manuscripts to him. Accordingly Hilbert does not try to mention these grounds in his well-known letter of dismissal.' As a motive for Blumenthal's alleged action, Brouwer mentioned the promise to resign at the completion of Volume 100, and the admonishments he repeatedly directed to Blumenthal in relation with arbitrariness and damaging actions in general.

In defence of Blumenthal, it must be acknowledged that in his unshakable loyalty to Hilbert he would probably go very far, but he would never seek to protect himself. Indeed, there is convincing evidence that Blumenthal almost immediately regretted his actions against Brouwer. Blumenthal was a man of high integrity, but in this case his *Doctovater* overruled his conscience. That Blumenthal mismanaged the editorial procedure from time to time was a recognised fact; the Lebesgue note of 1911 is an example. But that would not have been a reason for ousting Brouwer.

Brouwer's refutation of Blumenthal's four points is more interesting, as it provides relevant information:

Ad 1. There could very well be a reality corresponding to the word 'rude' (*schroff*), if the meaning of the word is fixed as follows: will for integrity



(the duty of every human being), with in addition the will for clarity (destiny of the mathematicians).—These wills have manifested themselves with me if the honour and prestige of the *Annalen* was at stake. (There were, by the way, cases, where Blumenthal himself had called upon me.) Then neither the vanity of the authors, nor Blumenthal's wish to please, could be spared—When I have occasionally carried my will through against that of the managing editor [Blumenthal], the latter has indeed found no support with his colleagues in the board, or has had reasons not to look for it.

There is not much evidence in this case. There is hardly any correspondence between Brouwer and authors left, but there are notes that show that Blumenthal was quite happy to use Brouwer as a trouble-shooter. Furthermore, a certain natural wish to ingratiate in Blumenthal cannot be denied. This may not be a recommendation for a managing editor.

Ad 2. The occasion Blumenthal hints at in his report on Klein's resignation can hardly be anything but the following: I had a discussion with Klein about a paper that I had already handled, the author of which had appealed to Klein, as chief editor, concerning the changes I had demanded, and he had made him his views plausible in a personal discussion. In the conversation with me, Klein then saw that the author was wrong (not formally, as Blumenthal suggests, but contentual), and that he could, as a consequence, not stick to the promise given to the author.<sup>102</sup> In the further course of his discussion Klein offered his view that the manner in which the chief editors were mentioned at the cover apparently gave the public a misleading impression, and he for himself, as far as it concerned his person, could not very well bear the responsibility for this impression.—Some time later he resigned as a chief editor.—Such a conduct does as much credit to Klein as little as it does for Hilbert that, with on his part, a much smaller contribution to the editorial activity than Klein made at the time of his resignation, the opportunity existed to exploit the internal weakness of his position for the external strengthening of it.

Ad 3. As I devoted yearly some thousand hours, it is almost obvious that manuscripts that had come in were usually for months in my possession. Only the word 'lay around' (*Lagern*) is misleading, for never papers were temporarily forgotten, or even missing (as happened with Hilbert), but they were always the subject of the most intensive editorial activity, by which their content was as a rule considerably influenced. As I moreover kept only in extremely exceptional cases, in which great deficiencies were found, manuscripts beyond the normal printing time limit, the papers were much better stored with me, than that they would have 'laid around' at Blumenthal's.—Blumenthal was, by the way, until shortly of the opinion that my procedure was normal and scrupulous, otherwise he would not so often have appealed to me for refereeing, even of papers, for which the content of I could not in the least be considered an expert.

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<sup>102</sup>The available evidence points at Mohrmann.

# MATHEMATISCHE ANNALEN

BEGRÜNDET 1868 DURCH  
ALFRED CLEBSCH UND CARL NEUMANN

FORTGEFÜHRT DURCH  
FELIX KLEIN

UNTER MITWIRKUNG  
VON  
LUDWIG BIEBERBACH, HARALD BOHR, L. E. J. BROUWER,  
RICHARD COURANT, WALTER V. DYCK, OTTO HÖLDER,  
THEODOR V. KÁRMÁN, ARNOLD SOMMERFELD

GEGENWÄRTIG HERAUSGEGEBEN  
VON  
DAVID HILBERT                      ALBERT EINSTEIN  
IN GÖTTINGEN                      IN BERLIN  
OTTO BLUMENTHAL                CONSTANTIN CARATHÉODORY  
IN AACHEN                      IN MÜNCHEN

100. BAND



BERLIN  
VERLAG VON JULIUS SPRINGER  
1928

# MATHEMATISCHE ANNALEN

BEGRÜNDET 1868 DURCH  
ALFRED CLEBSCH UND CARL NEUMANN

FORTGEFÜHRT DURCH  
FELIX KLEIN

GEGENWÄRTIG HERAUSGEGEBEN  
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BERLIN  
VERLAG VON JULIUS SPRINGER  
1929

Fig. 14.3 Mathematische Annalen, Volume 100, Issue 1, 1928 © and Mathematische Annalen, Volume 101, Issue 1, 1929 ©

Brouwer's reply is, at least to us, completely convincing. A modern editor would be happy indeed if his referees were as punctual as Brouwer. Brouwer in fact had a reputation of being a most scrupulous editor, he would—in cases where it made sense—rethink the content of the paper, which resulted in considerable, and sometimes essential improvements. He mentioned the copying procedure below, this may have been the expression of a certain caution, adopted over the years. This meant a considerable extra work load, borne by Cor Jongejan, who was paid an assistant's salary for these and similar jobs.

Ad 4. Although Blumenthal knows to give an example of my 'principle' to obtain a copy of every manuscript coming in, and although I think that this act is by itself the elementary right of the refereeing editor, I have since many years done so only then, when a paper seemed indeed acceptable, but fit for publication only after rewriting or considerable emendations. Then I considered it a duty in the face mathematical history, and indeed because one must take into account the possibility of an unjustified reference to the date of submission.

The circular ended with:

I challenge Blumenthal to make public the *Annalen* correspondence, in particular with the complete correspondence between him and me. I claim that these documents will really refute those accusations of him against me.

In an open discussion in a free meeting of editors, Brouwer would undoubtedly have scored, but the days of Klein's 'academy-concept' were over. The *Annalen* had become a one-man enterprise; this one man happened to be a man of great prestige, and an outstanding mathematician (although already then some of his colleagues would prefer the past tense), but a man with absolute power is not so easily corrected, as history has taught.

Whatever one may think of the crisis-management of Hilbert's lieutenants, Bohr and Courant, they had managed to outwit Brouwer. The whole matter was decided behind closed doors and the books were closed on Brouwer.

And so the curtain fell over one of the most tragic and completely unnecessary conflicts of twentieth century mathematics. It seems desirable to sum up the affair, but the motives of the invisible main actor were so confused that it is hard to come to a satisfactory conclusion. The available evidence tends to show that Hilbert's primary motivation was Brouwer's political activity and the Riemann affair. Although he nowhere explicitly says so, he felt it as a loss for the *Annalen* that French authors would keep their distance. The fear that was formulated in correspondence, that Brouwer would take over the *Annalen* after Hilbert's death, was rather a figment of Hilbert's imagination. It is not unlikely that Brouwer would have been pleased to move up to the chief editors, but that his influence would go any further than what a chief editor normally does, i.e. accept or reject papers, is not plausible. Even in his own faculty, his own mathematical association, his own academy, he had no excessive influence. So why would the *Annalen* be different? Some people have mentioned the danger that Brouwer would turn the *Annalen* into an intuitionistic bastion. The record shows that even his own journal, *Compositio Mathematica*, did not function as such.

There remains the matter of the *Grundlagenstreit*. Here we have to admit that Hilbert's emotions ran high. His aversion to this odious doctrine, with its by far too clever proponent—as witnessed, for example, by his refusal to quote Brouwer for any foundational contribution—certainly went beyond rational evaluation.

There is one more point that might have added to Hilbert's emotional outburst. David Rowe has pointed out that Brouwer's reminder of a forgotten credit, see p. 538, was the first public manifestation of this sort. There had been grumbling before, but no one had the courage to put it in print. Here we may very well have an instance of the old-time German professor, who could not forget such a slur.

Reactions on the *Annalen* affair are scarce. Of course the German mathematicians must have known that something was going on. A healthy dose of gossip is part of academic life. But it may be doubted that the facts were also known beyond the borders of Germany. There is for example a letter from Ehrenfest to Van der Waerden from that period:<sup>103</sup>

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<sup>103</sup>Ehrenfest to Van der Waerden, 8.X.1928.

It was by itself a great pleasure to be again in Göttingen, together with the wonderful Harald Bohr! In spite of all the sadness I actually had to laugh about this scandal bomb that exploded around the editors of the *Mathematische Annalen*. It looks almost as if the physicists are a more tolerant and humorous people than the mathematics-only people. They have such a terribly refined brain that it can tip over at any moment.

Alexandrov, when informed about the *Annalen* affair by Cor Jongejan, thought that ‘such a dismissal is a terrible insult, and also an act that is hard to justify to the international mathematics’. But knowing both Brouwer and the Göttingers, he hesitated to take sides. ‘Be that as it may, the whole matter is and remains most unpleasant, and it will hardly contribute to the moral prestige of the mathematicians in central Europe. But what I find most distressing, is that the painful, and for the collective mathematics in Germany damaging, antagonism Göttingen–Berlin will be further intensified.’<sup>104</sup>

A later letter of Alexandrov to Hopf suggests that there were rumours about a boycott of the *Annalen* in protest against Brouwer’s dismissal. That seems, however, never to have been a serious plan.

Looking back at the *Mathematische Annalen* affair and the surviving correspondence, and keeping in mind that before the final blow-up all the persons concerned, with the possible exception of Hilbert, had been living in the general atmosphere of friendship that was so characteristic for the international brotherhood of mathematicians, one gets a strong impression of regret and reluctance at the side of the managing and associated editors. Leaving aside Hilbert and Blumenthal, nobody was out to hurt Brouwer. The majority of the editors had at one time or another more or less close connections with Brouwer, and all of them were aware that he was a strict, perhaps difficult, but not unfair, colleague. Bohr and Courant, Hilbert’s two lieutenants, for example, were doing their utmost to reach a clean, objective solution; they did not muster the zeal of Blumenthal, but looked for a minimal damage compromise, in the hope that Brouwer would play along and be so magnanimous as to swallow the insult that no cosmetic procedure could obscure for him.

Blumenthal was an exception; there had apparently been frictions between him and Brouwer before 1928, but the main source of his vehement denouncement of Brouwer—so uncharacteristic for the man of peace he was—was his unerring loyalty to Hilbert. Later in life, according to his daughter, he sincerely regretted the consequences of his actions.

Hilbert, the leading actor in this almost Shakespearean drama of the ‘Frogs and the Mice’, only played a role at the beginning and the end of the conflict. After his initial appearance in the role of defender of the realm, he was whisked back stage, only to reappear in the finale for the ceremony of signing the letters that sealed the conclusion of the conflict. There are no known reports of comments or reminiscences of Hilbert that shed more light on the episode or on his motivations. It was as if for him the book on Brouwer was closed once and for all. As we have seen

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<sup>104</sup>Alexandrov to Hopf, 6.XII.1928.

above, Hilbert told his closer associates at the time of the conflict that there was no personal element in his actions and statements, and most of his followers were happy to take his word for it. Nonetheless, one would probably do well to entertain a certain measure of doubt where motives and self-knowledge are concerned. A person who is the subject of unlimited admiration and trust is mostly not in a good position to do serious soul searching.

Brouwer himself refused to publish further in the *Mathematische Annalen* and he convinced Heyting to follow his example. Heyting's paper was the outcome of a prize problem set by the *Wiskundig Genootschap* in 1927; it was formulated by Manoury, cf. p. 500, and obviously endorsed by Brouwer. It may come as a surprise, but Brouwer was highly pleased with Heyting's formalisation of intuitionistic logic, so much indeed that when Heyting had rewritten the material as a full blown paper, Brouwer expressed his appreciation in no uncertain terms, 'Your manuscript has interested me extraordinarily, and I am sorry that I have now to hasten to return it. In future I would appreciate it if you made a copy of your manuscripts before sending them to me, if at least you value a more than superficial reading. In the meantime I have already got such a high appreciation for your work, that I ask you to edit it in German for the *Mathematische Annalen*.'<sup>105</sup> At the time of writing Brouwer could not have foreseen what Hilbert had in store for him, but after the battle of the frogs and mice had run its course, he asked Heyting to withdraw his manuscript from the *Mathematische Annalen*. Two months later he wrote to Heyting that had hoped that Blumenthal and Hilbert would show repentance and mend their ways before the summer was out, but that he had now given up hope.<sup>106</sup> Anticipating Heyting's understanding, he had decided 'not to leave the manuscript in the hands of those who call themselves falsely editors of the *Mathematische Annalen*'. He proposed to submit the paper to the Prussian Academy, where Bieberbach could handle it. As a consequence Heyting's historic contribution to logic appeared in the reports of the academy, which did not have the distribution and visibility of the *Annalen* by a long chalk.

That Brouwer was seriously impressed by the advances made by his student may further be illustrated by his letter to Weyl. Brouwer was proposing Heyting for a chair in Utrecht, and he asked Weyl to support his proposal.

In Utrecht there is an important vacancy list of the mathematics faculty: Barrau, H.J.E. Beth, Schaake (all three insignificant). My (alphabetical) list: Heyting, Hurewicz, Van der Waerden (respectively intuitionist, topologist, algebraist). Heyting and Van der Waerden are Dutch, Hurewicz (my assistant) though a Polish citizen, and educated in Moscow and Vienna, is already for a long time in Holland. In order to document my list with the minister, I need foreign testimonials. For Heyting (so far my only really gifted intuitionistic student) you are the only eligible author of such a testimonial. Such a testimonial should on the one hand stress the general importance of intuitionistic

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<sup>105</sup>Brouwer to Heyting, 17.VII.1928.

<sup>106</sup>Brouwer to Heyting, 28.IX.1929.

research in the present stage of development of mathematics (in Holland nobody outside Amsterdam believes that), on the other hand it should qualify Heyting's papers (which are enclosed) as epoch-making.<sup>107</sup>

The letter also shows that Brouwer had few illusions about the acceptance of his program in his own country; it must be said that he did little to convert his colleagues to his views. Brouwer was quite prepared to explain his views in lectures and papers, but he would not go round to practice empire building.

In 1929 there was another publication in the intuitionistic tradition: an intuitionistic analysis of the game of chess by Max Euwe.<sup>108</sup> It was a paper in which the game was viewed as a spread (i.e. a tree with the various positions as nodes). Euwe carried out precise constructive estimates of various classes of games, and considered the influence of the rules for draws. When he wrote his paper he was not aware of the earlier literature of Zermelo and Dénès König. Von Neumann called his attention to these papers, and in a letter to Brouwer Von Neumann sketched a classical approach to the mathematics of chess, pointing out that it could easily be constructivised.

#### 14.4 The Endings of the *Grundlagenstreit*

The issues of the *Grundlagenstreit* were far from settled. In military terms, Brouwer had built an impregnable citadel, and Hilbert produced blue prints for an immense fortress.

Brouwer's intuitionism could only be defeated on philosophical grounds, his mathematical intuitionism consisted of honest mathematics, and there was no grand claim for which an account could be asked. Strictly speaking there was a master plan: the rebuilding of mathematics along intuitionistic lines. But that was an open-ended claim. One could not reject intuitionism, because it had, say, not yet proved the fixed point theorem for the unit square. In the first place, because the list of such challenges is endless, and none of them would be crucial. In the second place, the reply is flexible. In the case of the fixed point theorem, the intuitionist would say, I cannot give you the fixed-point theorem, because it is false in my mathematics, but I can give you our version, which is an  $\varepsilon$ -theorem, cf. p. 503.

In Hilbert's approach there was a crucial test: the consistency proof for, say, arithmetic by finitary means. In 1928 at the Bologna congress Hilbert 'saw the light at the end of the tunnel', that is to say, he had set up his machinery, and considered it just a matter of time before the remaining technicalities were carried out.

The *Annalen* affair rudely interrupted the conflict; Brouwer felt deeply insulted, and retired from the field. He did not give up his mathematics, but he simply became invisible. He no longer appeared at meetings of the DMV to report on intuitionistic mathematics. Even worse, he gave up publishing for a decade, there is evidence

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<sup>107</sup>Brouwer to Weyl, 16.II.1928.

<sup>108</sup>Euwe (1929).

that he carried on his more reflective research, but the results remained restricted to such things as classroom notes, communications to Heyting and Freudenthal, etc. His withdrawal from the debate did not mean a capitulation, on the contrary, he was firmly convinced of the soundness and correctness of his approach.

Looking back, one has to come to the conclusion that there never had been a discussion between Brouwer and Hilbert on the essential points. One doubts if the two ever had a private conversation on the foundations, with the exception of the Scheveningen walks. It is very likely that Hilbert never read Brouwer's basic papers, such as the 'domains-of-functions'-paper (although he probably attended his lecture in Göttingen in 1926, and the paper was a contribution to the Riemann volume). Indeed, referring to Brouwer's publications, Bernays asserted in an interview, 'he has not read these things at all'.<sup>109</sup> All of Hilbert's attacks consisted of rather superficial comments on hearsay bits of Brouwer's repertoire. Brouwer, on the other hand, repeatedly put his finger on the crucial spots of Hilbert's program: (1) consistency of induction requires induction (siding with Poincaré); and (2) consistency does not prove existence. Hilbert was in fact encouraging work on (1), and he did not see the point of (2).

The historic facts were in fact all speaking for Hilbert; he had every right to be optimistic about the success of his program. There was considerable activity in his proof theory, there was significant work of Ackermann (1925, 1928), Herbrand (1928–1931), and von Neumann (1927).

Yet Hilbert's program foundered when in 1930 a young man with his soft voice announced that formalised classical mathematics was incomplete. This stunning event took place at the *Tagung für Erkenntnislehre der exakten Wissenschaften* in Königsberg. At this meeting (which was embedded in the Meeting of the Natural Sciences and Medicine)<sup>110</sup> an exchange of ideas between the 'big three' of the foundations of mathematics was arranged. Rudolph Carnap spoke for the logicians, Arend Heyting for the intuitionists, and Johann von Neumann for the formalists.

Reidemeister, who was in charge of this meeting, wrote to Heyting that neither Brouwer, Hilbert, nor Russell<sup>111</sup> were invited as speakers; this was a symposium where the younger researchers presented their work, so it seemed more appropriate not to invite the big names. Apparently Heyting was worried that Brouwer might feel passed over. Reidemeister also begged Heyting not to be upset that Hilbert was going to be present, he was invited by the *Naturforscher*. In view of Hilbert's recent lecture in Bologna, von Neumann was definitely a better choice. It was a meeting of the second generation, and as a consequence the tone was friendly and objective.

The surprise of the meeting was Gödel's contribution to the discussion at the end. He had already presented a talk which dealt with his completeness theorem. This solved one of Hilbert's 1928 problems. His contribution to the discussion at

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<sup>109</sup>*Er hat die Sachen überhaupt nicht gelesen.* Interview, Bernays 18.VII.1977.

<sup>110</sup>To be precise, the 6th *Deutsche Physiker- und Mathematikertagung*, and the 91st *Versammlung der Gesellschaft Deutscher Naturforscher und Ärzte*. September 5–7, 1930 Königsberg. Proceedings published in *Erkenntnis* (23), 1931.

<sup>111</sup>Weyl (1946), p. 275.

the closing session, however, spelled disaster for Hilbert's program. It said, roughly, that there were simple statements, comparable to Goldbach's conjecture, that were neither provable nor refutable in classical mathematics. In symbols, statements  $A$  such that  $T \not\vdash A$  and  $T \not\vdash \neg A$ , where  $T$  is a suitable mathematical theory, for example arithmetic.<sup>112</sup>

This was a dramatic moment—the end of a program, but what a magnificent end! Hilbert, however, missed it, as he was whisked off in a taxi to the radio studio, where he was expected for a broadcast of his '*Logik und Naturerkenntnis*' (Logic and the understanding of nature). According to contemporary sources, Hilbert learned about this stroke of fate only months later; no one had the courage to disappoint the old master. When he learnt about Gödel's work, he was angry.

Bernays had heard about Gödel's result through the grapevine. He wrote to Gödel, asking for advance information in the form of proof sheets.<sup>113</sup> One may thus take it that Hilbert was not aware of the results either. This would be consistent with the paper that Hilbert published after the Königsberg meeting, namely 'The Founding of elementary number theory' (submitted 12.12.1930).<sup>114</sup> In this paper Hilbert upheld his program as if nothing had happened. He did, however, introduce in his paper a novelty, the  $\omega$ -rule:

$$\frac{A(0), A(1), A(2), \dots, A(n) \dots}{\forall x A(x)}$$

In words: if you have derived  $A(0), A(1), A(2), \dots, A(n) \dots$ , then you have also derived  $\forall x A(x)$ . This could have been an answer to Gödel's theorem, but as he considered the  $\omega$ -rule to be a finitary rule (p. 456), the system would be finitary and fall under Gödel's theorem. There is no reference to Gödel in this paper, but that does not say much in view of Hilbert's reference practice. The paper was the report of a talk before the philosophical society in Hamburg, in December 1930.

Hilbert's next, and last, paper, 'Proof of the Tertium non datur' (submitted 17.7.1934), does not mention Gödel either, but as it is more modest in scope, one may guess that Hilbert revised his perspective.

Gödel published his epochal paper in the Austrian mathematics journal, the *Monatshefte für Mathematik und Physik*, in 1931. In the same year the *Erkenntnis* issue with the lectures of the Königsberg meeting appeared with a brief synopsis of his incompleteness results. This time it included the second incompleteness theorem, 'For a system in which all finitary forms of proof are formalised, a finitary consistency proof is not possible.'

Immediately after Gödel's talk John von Neumann had collared Gödel. Von Neumann, who was reputedly the fastest thinker of his generation, had right away seen what was going on, and he had realised that Gödel's argument could be 'internal-

<sup>112</sup>*Erkenntnis*, 1931, pp. 147, 148.

<sup>113</sup>Bernays to Gödel, 10.XII.1930, cf. Dawson (1997), p. 282.

<sup>114</sup>Hilbert (1930).



ized'. This then would yield the unprovability of the consistency of (say) arithmetic in arithmetic.<sup>115</sup>

Gödel's incompleteness theorems brought the second ending of the *Grundlagenstreit*. Where Hilbert had won the conflict in the social sense, he had lost it in the scientific sense.

What could be learned from the history of the *Grundlagenstreit*? In fact little, the whole matter was in the real sense a struggle of Titans. Both Brouwer and Hilbert were exceptionally gifted, be it that their characters were almost completely opposite. The older man was totally engrossed in mathematics—the paradigm of the German professor as the icon of traditional *Bürgertum*, the younger man equally infatuated with mathematics, but without the compulsion, who always was glad to change the three piece suit of the professor for the bohemian outfit that belonged to his free and artistic way of life, complete with numerous love affairs. The fact that their philosophies of mathematics were so different undoubtedly played a role, but should not be overestimated. Those who had followed Hilbert's evolution from the first steps in 1904 to the middle twenties must have seen that he sought safety for mathematics in the realm of the finitary, and that whatever intuitionism might be, it certainly encompassed Hilbert's finitary mathematics. Abraham Fraenkel, who had observed the developments closely, was quoted in the *Vossische Zeitung*, 'With charming wit Fraenkel called Hilbert the second intuitionist',<sup>116</sup> and in his 'Ten Lectures on Set Theory' Fraenkel noted that 'one could even call him an intuitionist'.<sup>117</sup>

Brouwer had come to the same conclusion in his 'Intuitionistic Reflections on Formalism'. If anything, Hilbert could rather accuse Brouwer of surpassing the limits of finitary mathematics. It is doubtful whether he was aware of this, although Bernays wrote him in 1925 that 'he had discovered a certain difference between the finitary position and that of Brouwer'.<sup>118</sup> In 1977 Bernays said that he had grasped the difference between 'finitistic' and 'intuitionistic' through the Gödel translation. Taking into account the cited letter, the plausible reading seems to be 'the Gödel translation confirmed my earlier view'.

As Brouwer had put it, after acknowledging a few foundational insights, the whole matter 'intuitionism—formalism' became a matter of taste. So why the incessant harping on Brouwer? Even in his last paper but one, Hilbert repeated his complaints: 'Nonetheless there are even today followers of Kronecker, who do not believe in the *Tertium non datur*: it is well by far one of the crassest disbeliefs that we find in the history of mankind'.<sup>119</sup> Since the paper has the character (and hence

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<sup>115</sup>Gödel called this the Von Neumann conjecture: 'If there is a finitary consistency proof at all, then it can be formalised. Then Gödel's theorem yields the impossibility of a consistency proof at all' (15.1.1931). See also Dawson (1997), p. 68 ff.

<sup>116</sup>8.X.1929.

<sup>117</sup>Fraenkel (1927), p. 154.

<sup>118</sup>Bernays to Hilbert, 25.X.1925.

<sup>119</sup>Hilbert (1930).

the precision) of a popular lecture, one should not worry too much about the details, but here are a few noteworthy statements:

- The a priori is nothing more nor less than a basic disposition, which I would like to call the finitary disposition.
- Kronecker has clearly enunciated the view, and illustrated it by means of numerous examples, which nowadays coincides essentially with our finitary disposition.
- The problem of the foundations of mathematics is, as I believe, definitely dispelled by proof theory.

So we note that Hilbert's a priori is even more restricted than Brouwer's of 1907. Furthermore, Hilbert made after all these years his peace with Kronecker. In Hilbert's hindsight Kronecker was, foundationally speaking, a forerunner of himself, who did not go all the way. One guesses that for a similar rehabilitation of Brouwer it was too early. Moreover, Brouwer was still alive, so, in contrast to Kronecker, he could possibly protest.

All great men, with great ideas, attract criticism, one may think of Cantor, Einstein, Gödel. Sometimes (and perhaps often) the criticism is offered by persons halfway between crank and amateur, sometimes by colleagues, who lack the imagination or intuition to appreciate novel circumstances or principles.<sup>120</sup> Reading Hilbert's foundational papers, one gets a strong impression that he did not suffer the criticism (of fools?) gladly. In the majority of these papers he complained about all those people who held obscure views and could not see the point of his program. His final reaction was, 'The critic of my theory should point to me exactly the place where my alleged error is to be found. Otherwise I decline to check his line of thought.'<sup>121</sup> A definite exasperation speaks from these lines, but was it reasonable? Assume someone would claim to have proved that after  $10^{10^{10}}$  there are no more primes. Would he not rather say, go and study your Euclid, than check the long and intricate proof. The ultimate consequence of the above viewpoint would be that Hilbert had to reject Gödel's criticism of his program, for Gödel did not carry out a proof check of Hilbert's (non-existent!) proof. Apparently, in his emotions, he allowed himself to say things that he could not possibly mean.

The reader may have got a somewhat negative impression of Hilbert's proof theory. This should be corrected at once, it is a fact that Hilbert gave the world a new discipline of great finesse; after Gentzen added beauty and structure to it, which it was so sadly lacking, it became an elegant and powerful mathematical tool. In modern logic it occupies an important place, being the tool *par excellence* to bring out the finer details of complexity and structure. It also happens to be the part of logic that is of considerable interest to computer science. That Hilbert saw it primarily as a prop for settling all fundamental questions about the foundations of mathematics was simply a matter of a lack of experience in topics of this complexity and intricacy. It was a gift from Hilbert to mathematics, comparable to the rich gifts

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<sup>120</sup>For an instructive discussion of a particular instance of perpetual criticism, see Hodges (1998).

<sup>121</sup>Hilbert (1931).

he had already made. From our modern point of view it is surprising that a man like Hilbert, with a gift for sweeping methods, should not have been the first model theorist instead of the first proof theorist. In the light of the exacting demands that model theory made where intuition was concerned, proof theory was indeed more accessible to traditional mathematical techniques.

So far it seems as if the *Grundlagenstreit* was some sort of personal quarrel between Hilbert and Brouwer. What were the reactions from the mathematical community? From the isolated comments in correspondence or in print, one might easily get the impression that the foundational issues, including the surrounding gossip, went largely unnoticed. It would be a mistake, however, to conclude that the conflict was a private affair between a few prominent mathematicians. Hilbert's lectures and publications always attracted a good deal of attention, and although most readers would not be in a position to judge the merits of Hilbert's program, they would certainly read Hilbert's diatribes. And on the other side Brouwer in his soft-voiced lectures could capture large audiences. His Berlin lectures had exerted a strong fascination on the audience. In contrast to Hilbert's more aggressive approach, Brouwer's lectures made their impression through a mixture of persuasive argument, wit, vigorous proof and cynical metaphor. The reports of these lectures leave no doubt that Brouwer could recruit and inspire a not inconsiderable following. Nonetheless there are few explicit references to the foundational crisis. The man who did most to enlighten his contemporaries in the confused matter of the foundations was Fraenkel, his books and papers covered the basic issues of the period. His expertise in foundational matters was widely recognised by his colleagues. Hausdorff, after reading Fraenkel's *Ten Lectures on set theory*,<sup>122</sup> spoke his mind without reservation, 'I still nurse hopes that you, as the best expert in this literature, will at some time launch a vigorous and witty attack at intuitionism—although it might be more advisable to let this castrates' mathematics suffocate in its own complicate obtuseness. It is indeed stupid to stick into every mathematical theorem, like the egg of an ichneumon wasp, an unknown number fabricated out of the decimal expansion of  $\pi \dots$ '<sup>123</sup> Study's reaction to Fraenkel's book was one of relief, 'I cannot find the outcry of Brouwer and Weyl, and also that of Hilbert, as remarkable as you and others do.'<sup>124</sup> The function theorist Konrad Knopp shared this sentiment; after the publication of Fraenkel's *Introduction to set theory*<sup>125</sup> of 1924, he confided, 'I am glad to draw from it the confirmation of my feeling, that the 'shake-up of the foundations' by Brouwer was by no means as disastrous as it seemed to be.'

Hausdorff was an implacable enemy of intuitionism, he was not prepared to relent, as appears from another letter to Fraenkel, in reply to the receipt of a copy of Fraenkel's 1928 edition of the *Introduction to set theory*, 'I hope that it will leave my heartfelt aversion to intuitionism untouched.'<sup>126</sup>

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<sup>122</sup>*Zehn Vorlesungen über die Grundlegung der Mengenlehre*, 1927.

<sup>123</sup>Hausdorff to Fraenkel, 20.II.1927.

<sup>124</sup>Study to Fraenkel 5.III.1927.

<sup>125</sup>*Einleitung in die Mengenlehre*.

<sup>126</sup>Hausdorff to Fraenkel, 19.XI.1928.

The geometer Finsler had in his inaugural address in 1922, *Are there contradictions in mathematics?*, noted that Brouwer and Weyl rejected the principle of the excluded middle. He generously granted that ‘such assumptions may in themselves lead to very interesting research’, but, he continued, ‘an exact science cannot be based on these, not mentioning the great complications that would thus arise; also many of the most certain results must be given up’.<sup>127</sup> A positive appreciation of intuitionism is to be found with Ludwig Bieberbach. He had earlier deplored the decline of the intuitive approach to mathematics as advocated and practised by Felix Klein, but the propagation of formalism by Hilbert and his school had made him aware that there was a real danger that formalist tendencies could seriously harm mathematics, in the sense that intuitive and applied mathematics could become a neglected, if not extinct, part of mathematics. He sensed in Brouwer’s intuitionism a healthy antidote against the formalist epidemic. In an address for an audience of mathematics teachers he stressed the importance of Brouwer’s intuitionism—including choice sequences.<sup>128</sup> ‘There is a fresh breath of air thanks to intuitionism’, he called out. In how far Bieberbach was right, is debatable. After all in Hilbert’s Göttingen applied mathematics flourished, e.g. in the hands of Courant. And Hilbert occupied himself intensely with theoretical physics (albeit with formalisation in mind). But undeniably, there certainly was strong influence of Hilbert’s formalist doctrines on theoretical and methodological levels.

Spectators, who observed the foundational conflict with more detachment from some distance, often saw clearer that this middle European muddle of loyalty, common sense and foundational ingenuity, was a serious matter, and not just a clash of personalities. As the American mathematician Pierpont observed

And yet when one hears one of the greatest living mathematicians calmly telling the world that a considerable part of our analysis is devoid of proof, if not nonsense, and when one beholds the mighty efforts which the champions of Weierstrass are making to repel these attacks, it is reasonable, in view of such facts, to ask ourselves, ‘Is all well?’<sup>129</sup>

The German philosopher Grelling described the *Grundlagenstreit* pointedly:

If intuitionism has been characterised with a certain propriety as revolutionists who overturned the ancient régime, Hilbert might be compared with Napoleon who, without regard for considerations of legitimacy, established, through a brilliant political stroke, a new order whose success is the substitute for legitimacy.<sup>130</sup>

The working mathematician, more concerned with practising mathematics than with reflecting on its soundness, could not stop wondering why Brouwer, the wizard of

<sup>127</sup>Finsler (1925).

<sup>128</sup>About the scientific ordeal of mathematicians (*Vom Wissenschafts ideal der Mathematiker*), 1926.

<sup>129</sup>Pierpont (1928), p. 37.

<sup>130</sup>Grelling (1928).

topology, had given up the riches of traditional mathematics for a life in the arid desert of the foundations. André Weil, who attended some of Brouwer's Lectures in Berlin,<sup>131</sup> wrote to Fréchet:

people here are very excited at the moment, because Brouwer has just arrived and he has started a series of lectures, not on topology, but on intuitionistic mathematics; this is very particular, as you know. I have no pretensions to understand it, but Brouwer is a very interesting man. He has declared in his first lecture that the principle of the excluded third is a superstition which is about to disappear. It is a pity that such a remarkable man devotes himself exclusively to such bizarre things.<sup>132</sup>

Weil's view was shared, and is still shared, by many mathematicians all over the world. The often somewhat esoteric issues of the foundations of mathematics have traditionally met with tolerance at best, and distrust at worst.

## 14.5 The Menger Conflict

The third battle was partly a clash of personalities, and partly a scholastic argument concerning dimension theory. The arguments, subterfuge and confusion resulted in a case so complicated that it is difficult to keep track of the finer details. We will stick here to a simplified account that will give a rough idea of the matter. The interested reader may find a detailed exposition in van Dalen (2005).

When the dust of the Bologna Conference had settled, Brouwer was confronted with the next conflict. In 1928 Menger dropped a bombshell that shook the usually quiet community of topologists; he had been writing a book on dimension theory<sup>133</sup> that was announced in a flyer of the publisher with the words 'These and numerous other questions are answered by the dimension theory, founded by Menger. . . and Urysohn.' This formulation was more than enough to evoke Brouwer's curiosity and wrath. He had more or less closed the books on his dimension involvement after the settlement of a misunderstanding with Urysohn, and the subsequent publishing of Urysohn's *Mémoire*, and here was something that could not pass unchallenged. So far he had every reason to believe that the matter had been settled with mutual consent of the persons involved. Urysohn had fully acknowledged Brouwer's place in the genesis of the dimension notion, and Menger might have grumbled in private, but in his publications he had not questioned Brouwer's priority. An all out attack was certainly not foreseen. And here, suddenly out of the blue, a book had appeared that boldly informed the reader that the author had submitted a paper on dimension theory in February 1922 (published in 1926 in the Amsterdam proceedings) and, a page later, that Urysohn had published his *Comptes Rendus* note in September

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<sup>131</sup>Cf. p. 499.

<sup>132</sup>Weil to Fréchet, 31.I.1927.

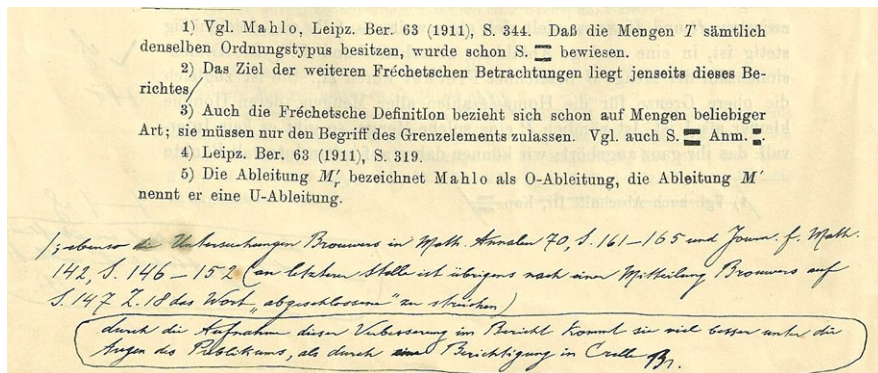
<sup>133</sup>Menger (1928b).

**Fig. 14.4** Karl Menger.  
[Courtesy Eve Menger]



1922. The order of the events subtly suggested that Menger had a justified claim to priority, leaving it up to the reader to wonder why there was such a discrepancy between the submission of Menger's paper and the publication (elsewhere). The story of Urysohn's entrance on the stage of dimension theory has been dealt with in Chap. 12, cf. p. 397 ff., and Menger's discovery of the notion of dimension can be found in Sect. 12.3. Urysohn had discovered a mistake in Brouwer's 1913 dimension paper, but in the ensuing exchange following the first letter, he accepted Brouwer's explanation of the 'slip of the pen' (pp. 408, 415). Indeed, Brouwer's account was convincing enough, and there was even written evidence to support his point—the proof sheet of Schoenflies' *Bericht* containing the footnote with the correction to Brouwer's separation definition. Brouwer had inserted the correction in writing, which was for whatever reason not adopted by Schoenflies, unfortunately without informing Brouwer of his decision.

The initial contact between Brouwer and Menger was pleasant enough, but it appears that gradually Menger started to feel unappreciated and even aggrieved in Amsterdam (cf. Menger 1979, p. 237), and the dimension book may have been the outcome of his growing discontent—were people, and in particular Brouwer out to steal his priority of dimension theory? (cf. Menger 1928b, 1930). The statements and their formulations in the book leave little to the imagination of the readers; for example, after discussing the definition of dimension, he describes Brouwer's position as 'This notion of general degree of dimensionality already comes quite close to



**Fig. 14.5** The footnote mentioning the slip of the pen, inserted by Brouwer in the proofs of Schoenflies' *Bericht*. [Brouwer archive]

the notion of dimension that underlies the theory of dimension.' Brouwer was allocated a place in dimension theory among the predecessors: Euclid, Cantor, Poincaré and Brouwer. If Menger had wanted to insult Brouwer, this passage would do very well; the patronising tone, usually reserved to pat a harmless colleague who did not make it on the shoulder, would be enough to make the blood boil of the person at the receiving end of Menger's wit. On the next page Menger characterised Brouwer's paper 'Remarks on the notion of natural dimension'<sup>134</sup> as a second attempt to provide a definition of dimension—this time with the right result. Even Menger's friend and student Hurewicz thought that Menger had gone too far:

What you write about Brouwer, about his character and his 'morals', is nothing new for me. Nonetheless I find that your conduct with respect to him is wrong and inappropriate. As to the matter itself, in my opinion the historical survey in your book is not completely free from a subjective representation. [...]

'Why didn't you send the proofs to Brouwer, so that he could have reacted', Hurewicz continued; 'the matter would now only cost time and energy, and the satisfaction of the beautiful book would be spoilt'. 'A "life and death" struggle because of these foolishnesses! But that you have not for a minute given a thought to the impossible situation into which you have brought me, I very much hold against you.'<sup>135</sup>

Menger went, so to speak, out of his way to repaint the picture of dimension. He relegated, for example, Brouwer's theorem of the invariance of dimension to geometry or analysis, outside dimension theory. The fact that dimension is invariant under homeomorphisms is, in a way, a pleasant phenomenon, but if it were not the case, the notion of dimension would not suffer. At most it would show that

<sup>134</sup>Brouwer (1924e).

<sup>135</sup>Hurewicz to Menger, 10.X.1928.

homeomorphisms had no place in it (Menger 1928b, p. 243). Thus, one might say, Brouwer had just stumbled upon a marginal fact about dimension.

Brouwer could indeed not have found the right definition of dimension, Menger claimed, since he did not know the right notion of connectedness (Menger 1928b, p. 86). This was exactly the point that Brouwer had cleared up with Urysohn, see p. 410. One can hardly assume that Brouwer had not explained the facts to Menger during his stay in Amsterdam in 1925.

Brouwer reacted almost immediately; in the paper ‘On the historiography of dimension theory’<sup>136</sup> he discussed his own work and the relation with the work of Urysohn, Menger and Alexandrov. He left no doubt: ‘I have founded the theory of dimension in my paper ‘On the natural dimension’.’ Furthermore he had shown the correctness of his definition for  $n$ -dimensional Euclidean spaces, which was a necessary and non-trivial corroboration of the definition. Here one might remark that a definition plus a correctness proof does not constitute a whole theory, but it certainly is the starting point for a theory. Brouwer was exceptional in the sense that he had no compulsion to exploit his ideas; usually he laid down the basics of some subject and left the further development to his fellow mathematicians. This was the case with dimension theory, and even with his intuitionism. In the latter case he usually pursued the fundamental problems and left the mathematical elaboration to his students Belinfante and Heyting. In a way his new topology was an exception; that he spent in that case more time pursuing the consequences of his innovations was mainly the result of the competitive behaviour of Lebesgue and Koebe.

The bone of contention with the dimension definition was the fateful ‘slip of the pen’. Brouwer maintained that in 1913 he had the right notion of separation in mind, but that somehow in the proof reading stage a restrictive term had been introduced, cf. p. 408. He was convinced that any competent reader would have noticed the slip immediately. This may seem a somewhat bold claim, but one should keep in mind that for a person like Brouwer, with a miraculous topological intuition and insight, it would be natural to assume that fellow topologists shared his acuity. Finally, one may ask why Brouwer did not produce the evidence of the proof sheet of Schoenflies’ *Bericht*; it would have settled the matter once and for all. There are two reasons for not doing so, in the first place Menger could simply have refused to acknowledge the evidence, in which case Brouwer would look like a schoolboy trying to mislead the teacher, in the second place Brouwer had set strict norms for what evidence in general would be acceptable. In a letter to Hahn,<sup>137</sup> discussing Menger’s reference to his early documents, he explained

... with respect to scientific historiography only such documents can be considered pure, thus can be printed by an author, which either have been written and dated by, if possible uninvolved, scholars, or if they come from the author himself, have been in the custody of other, still living, if possible uninvolved, scholars, who declare that they have been uninterruptedly from the time of

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<sup>136</sup>Brouwer (1928f).

<sup>137</sup>Brouwer to Hahn, 4.VIII.1929.



their originating to the time of publication in their possession. One's own papers, that have been, if only for a short while, in one's own possession, can never, not even a little bit, be counted as a proof, as long as there is not at the same time an official report, in which on the basis of an analysis of the ink, the age of every page and every change has been established (assuming that this is possible).

It would have been highly inconsistent to relax the standards of evidence in the case of his own documents.

Freudenthal, who knew Brouwer's handwriting better than anybody else, considered the footnote correction in the Schoenflies text genuine, and there seems to be little doubt that this is the case. Brouwer was a difficult man with a gift for clever arguments, but his integrity was beyond doubt.<sup>138</sup>

On another point Brouwer did score: he could supply written evidence that he had checked the relevant papers of Lennes on Blumenthal's request. But even here Menger put forward a twisted argument to the effect that Brouwer had indeed known the correct notion of connectedness, but that the Lennes paper was so little known that the reader could not be assumed to be aware of it. By now the discussion had reached a level of advanced nitpicking. Even Hahn could not get the two contestants to agree. Hahn and Brouwer had come to agree that the proper thing to do for Menger was write a rejoinder to Brouwer's historiography note that Brouwer could accept, and that would have a conciliatory statement of Brouwer attached to it. Menger did write a note, Brouwer had it type set for the Amsterdam proceedings, and Menger retracted it. In the following months Hahn tried to keep the negotiations going. He had contact with both parties, and there seemed to be a solution in sight. Menger, in the meantime, started his investigations into the editorial activities of Brouwer and Alexandrov with respect to Urysohn's *mémoire* for reasons that can only be guessed. On the main issue hardly any progress was made. Brouwer got ill, Hahn kept prodding him and when finally an agreement was in sight he made an unfortunate remark in a letter to Brouwer, 'I have exerted myself, since I got to know your role, to convince Menger that a calm, objective settlement were possible, and the only thing to be desired.' Brouwer, with his previous experience with Menger, thought this line an obvious consequence of an 'evidently preceding *accusation of Menger* that a calm and objective discussion with him were impossible'. This led to a hardly flattering outburst on his side, describing the behaviour of Menger in Holland.

Menger, when pressed by Hahn to produce the necessary evidence of claims made in the preceding note, resorted to a lame 'that has slipped from my memory'. Altogether Brouwer was not convinced that Menger would produce a balanced historical presentation of the facts involving Brouwer's slip of the pen, the right notion of 'connected', . . . . Under these circumstances he saw no possibility to deliver his conciliatory epilogue. In spite of Hahn's frantic diplomatic activity no agreement

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<sup>138</sup>Moreover, a recent preliminary analysis by the Dutch forensic institute showed no deviation among the corresponding handwritten corrections.

was reached, and Menger cut without consultation the knot by submitting his text to the *Monatshefte*. When Brouwer heard this he was outraged, and even more so when the *Monatshefte* refused to accept a rejoinder from him. Here the affair essentially ended. Brouwer protested, he wrote to the editors of the *Monatshefte*, considering Hahn guilty of treason. But nothing could be done. Menger's Reply could hardly be considered a balanced account of dimension history. One point irked Brouwer in particular—and no wonder—Menger openly accused Brouwer of fiddling the published version of Urysohn's mémoire. This was indeed a serious accusation, based on (inconclusive) evidence of material made available to Menger by Sierpinski. Menger had conducted the investigation of Brouwer's activity as if he were a police officer on a criminal case, and he published the indictment without the intervention of a court, in this case the editorial board. The whole affair left Brouwer with bitter feelings. The mathematical community did not react in any way to the conflict, nonetheless Brouwer suffered intensely. An attack of this sort was beyond his imagination.

In the end the damage was not very serious. Those who knew the inside story of Brouwer–Menger and dimension did recognise the significance of Menger's topological work and his contributions to dimension theory, but in the matter of priority for the notion of dimension they sided with Brouwer and Urysohn. Alexandrov remarked 'In my view Brouwer is objectively right, and if I were asked for an opinion, I can only—in accordance with my information, and my conscience—do as I have done, that is, agree with Brouwer and Urysohn, and not with Menger.' Hopf, in his reply of 3 March cautiously agreed, 'Brouwer seems to be right'. The smoothing effect of time has taken the sharp edge from the matter. In the book of Hurewicz and Wallman (1948), the standard text for dimension theory for a long time, the issue is not mentioned at all, and Brouwer is simply given credit for his definition of dimension.

One particular event took place in the middle of this dimension dispute, it was mentioned in Brouwer's letter of August 9 to Hahn on his vacation address in Bellagio. It was the nightmare of any scientist; the letter opened with a dramatic statement written in a remarkable mood of resignation:

Four days ago my briefcase (*Brieftasche*),<sup>139</sup> which also contained my scientific diary, was stolen from me on the front platform of a Brussels tram, by a pickpocket, and both the police and the detectives consider the case as hopeless. Since in this diary my collective scientific thoughts and ideas of the last three years, which have largely disappeared from my memory, and of which only a few have already found a registration elsewhere, had been recorded, this event means for my scientific personality a serious personal mutilation (*Verstümmelung*), in a way that is like the 'decapitation' (elimination of the central process) for a pine tree. To my amazement, I remain so far, fairly calm under this blow of fate; I believe, however, from certain phenomena, that I have nonetheless suffered a nervous collapse, the consequences of

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<sup>139</sup>Brouwer uses *Brieftasche*; it is more likely that he was carrying a small type of briefcase that was very common at the time, than a wallet.

which will perhaps only later become visible, together with a disorganisation of my scientific thoughts.<sup>140</sup>

After the exchange of some more letters and cards, Brouwer reported that there was no progress in the matter of the stolen notebook.

For the recovery of the lost papers I mobilise everything possible—advertisements in the newspapers, police, detectives, clairvoyants—but so far no success.<sup>141</sup>

The loss of this notebook may shed more light on Brouwer's unexpected withdrawal from research for a considerable period. A young man would go ahead, reconstructing and remembering his ideas, but Brouwer was approaching 50, and the mental scars of his battles were visible to the discerning eye.

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<sup>140</sup>Brouwer to Hahn, 9.VIII.1929.

<sup>141</sup>Brouwer to Hahn, 17.VIII.1929.