Chapter 11
The Fathers of Dimension

11.1 The Two Russians

For the next, and final, episode in Brouwer's topological career we have to go back to 1923. Now forty-two years old, Brouwer had acquired a reputation in mathematics that few could match. With his glorious past he had become in a way the wizard of topology, a man with an apparent direct access to the mysteries in which topology had been, and still was, veiled. That this man had given up his first place in topology, to save mathematics from the sterility that was threatening it, only gave greater intensity to his aureole of unselfish redeemer of mathematics. Like Moses he gave up his prominent role at the court of mathematics, in order to lead his people out of the affliction of Egypt into the land of intuitionism flowing with milk and honey.

That few were prepared to give up the 'flesh pots of Egypt', for a prolonged journey through the desert, did not matter. The reputation of radical reformers often ignores this realistic point.

Brouwer's critique of existing mathematics, his acknowledgement of fundamental uncertainty, fitted well in the mood of the times. The world—and in particular Germany—had lost its point of reference; the war had left behind a new awareness of social imperfections and of the inadequacies of the institutions of the older generation. In the wake of the war an exodus of royalty had taken place. The central powers were reduced in size, stability, and international status. Germany, the primary home of mathematical foundational activity, and the country where Brouwer's ideas could count on vivid interest, was in an exceptional state of confusion. The republic was struggling for recognition, for authority, and for survival. And the academic world was not being very sympathetic towards the new state. The consequences of a lost war, multiplied by the treaty of Versailles, were considerable; there was the trauma of the loss of national identity, and the all too apparent infringement of the national integrity. The French occupied the Ruhr in January. There was the galloping inflation—from day to day the mark sunk further; on July 25 the exchange rate had dropped to a terrifying $1 to 600,000 Mark. And the political instability was disturbing—a certain Hitler was putting his party organisation in order, and in November his Coupe in Munich just barely miscarried.

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In this uncertain world thousands of scientists worked, and tried to live under the notice 'business as usual'. This was not Brouwer's style, he was heavily involved in the struggle for the rehabilitation of German scientists and scientific organisation, and he quietly carried on his research for a rebuilding of mathematics under the laws of his intuitionism. The year 1923 saw the last of his efforts to finish off the Denjoy affair. He had started a new life after the war, and the rebuilding of mathematics along constructive lines occupied him almost exclusively. But in spite of Brouwer's farewell to topology, other than as a subject of editorial and general interest, the topic was not to leave him alone. The twenties brought him a certain degree of pleasure as it furnished the recognition that was due to him for his penetrating insights, but it also plunged him into one of the most bitter fights of his career. It all had to do with a minor slip of the pen (*Schreibfehler*) in 1913.

In 1923 the German Mathematics Society, *DMV*, held its annual meeting from 20 to 25 September 1923 in Marburg. The meetings of the *DMV* mirrored in a way the state of mathematics; new trends became visible in the list of topics. The 1923 meeting showed a pronounced presence of topology, represented by the speakers Alexandrov, Urysohn, Schoenflies, and Furch. There were furthermore many mathematicians among the participants with a first-hand expertise in topology, such as Bieberbach, Brouwer, Carathéodory, Dehn, Reidemeister, Schreier, Vietoris, and Wilson. The post-war development of topology had persisted, and finally the pioneering works of Brouwer found their worthy successors. Two young Russians, Alexandrov and Urysohn, had taken up the thread of Brouwerian style topology, and they shared a strong and natural talent for the subject. Both owed their topological education to Egorov and Lusin. In 1913 Alexandrov, the scion of an upper middle class family, enrolled at Moscow University at the age of seventeen. Having been taught by an outstanding mathematics teacher, he already knew most of the first year mathematics; he started to read the mathematics texts for himself, and good fortune brought him in contact with the writings of Cantor. From that moment, he was under the spell of set theory and topology. One of his fellow students was M. Suslin, who left his stamp on significant parts of set theory although he died of typhus, only 25 years old, in 1919. Alexandrov took Egorov as his guide in mathematics, who introduced him to such topics as measure theory, Hilbert spaces, and Baire's discontinuous functions. By 1915 Alexandrov had, in answer to a problem set by Lusin, determined the cardinality of Borel sets, and introduced the operation A (called thus after its inventor by Suslin). Lusin later called sets obtained by the A-operation analytic sets, claiming that 'A' stood for 'analytic'.[1]

At the time of Alexandrov's first results in descriptive set theory (comprising the theory of Borel sets, analytic sets, the Baire hierarchy, Borel measure, etc.) the young student Urysohn entered the stage. Urysohn was two years younger than Alexandrov, but he soon proved a match for him in topology.

The mathematical career of the two was interrupted for a few years by various kinds of activities (for example, Alexandrov spent a great deal of time in artistic

[1] Cf. Alexandrov (1979).

circles), and of course by the revolution. Fortunately both survived the period of civil war, and in 1920 they returned to the study of mathematics in Moscow. From then on things went fast.

In 1921 Urysohn finished his dissertation and was subsequently appointed as a lecturer. At the instigation of Egorov he turned towards topology. The problems that Egorov pointed out to him were right in the heartland of contemporary topology: what is, topologically speaking, a curve or a surface? The question may seem strange, in particular since 'everybody knew what a curve (surface) was'. But the usual definitions usually described curves by equations, and thus made use of notions which were not prima facie topological. What one needed was a definition that used only topological notions, something like 'a curve is a set such that ... ', where in the definition only notions from (basic) topology, such as point, neighbourhood, continuous, ..., are used.

In the course of his investigations Urysohn came upon the notion of dimension. In view of the topological nature of curves and surfaces, this was not so unexpected, already Euclid had stated that 'the extremities of surfaces are lines'.

In Alexandrov's memoirs there is a description of the birth of Urysohn's dimension theory:

> One morning in August [1921], Urysohn and I were both at the Burkovo dacha and went to swim in the Klyaz'ma. During our bath Urysohn told me about his definition of dimension at which he had just arrived and then began to expound at great length the basic propositions of dimension theory. I was thus present at the conception of one of the finest chapters in topology: Urysohn's dimension theory.[2]

Soon Alexandrov and Urysohn started to put their ideas on paper, the first result being their famous '*mémoire sur les espaces topologiques compacts*', published many years later by the Dutch Academy of Sciences.[3]

The two young mathematicians were sufficiently enterprising to see that beyond Moscow there was a whole world waiting to be discovered. And where better could gifted beginners go than to Göttingen, the capital of the mathematical world? One might object that beginning topologists would do well to go to Amsterdam and sit at the feet of the old wizard of the subject. But Brouwer had more or less said farewell to the subject; although he had published some 18 papers on topology after the war, this activity was more an aftershock of a big earthquake, than the beginning of a new life. Moreover, Brouwer was a loner, and in Göttingen Alexandrov and Urysohn would find a whole group of eager researchers. There might have been other options—Paris, Berlin, but in the mathematical culture of the time Göttingen was the perfect choice. It was a mathematical wonderland for them. During the summer semester of 1923 they met all the great mathematicians; Klein, Hilbert, Courant, Emmy Noether, ... Indeed, Göttingen went out of its way to welcome these first two Soviet mathematicians that had crossed the border.

[2] Alexandrov (1979), p. 296.

[3] Alexandroff and Urysohn (1929).

Alexandrov, in his memoirs, describes his experiences; he went to the lectures of Hilbert, Courant, Landau and—best of all—Emmy Noether. Hilbert was lecturing on intuitive geometry. His lectures were presented in 'an inspiring way, with a large number of individual remarks, always interesting, often witty, and sometimes profound'.[4] It is interesting to read that Alexandrov was not impressed by the presentation itself—'Hilbert spoke badly and could not even draw the simplest figure. Once he wanted to draw an ordinary rectangular parallelepiped. He tried to do so without success, and finally he turned angrily on his assistant (that was Bernays that summer). Bernays got up and (also without much sparkle) drew the ill-starred parallelepiped.' When Hilbert, with Cohn-Vossen, prepared the publication of these lectures, he asked Alexandrov to provide an appendix on topology. But Alexandrov did not oblige. Instead, he wrote a short monograph '*Einfachste Grundbegriffe der Topologie*' (The simplest basic notions of topology), which became as popular as the Hilbert–Cohn–Vossen book.

In Göttingen was Emmy Noether, the greatest influence on Alexandrov and Urysohn. She was already acknowledged as the mother of the new algebra, and she was lecturing on the theory of ideals. This term was the beginning of a lasting friendship.

Urysohn and Alexandrov gave a number of talks on topology in Göttingen, and Hilbert was sufficiently impressed to invite them to submit some papers to the *Mathematische Annalen*. They did so, but Ostrowski, who was asked to handle the papers, failed to take the appropriate steps to get them published. Apparently he considered them unsuitable for the *Annalen*, and so they just gathered dust for a year. When Emmy Noether, in 1924, discovered that the papers had been disregarded, she told Hilbert and insisted that the manuscripts should be sent right away to Brouwer for referee reports. Ostrowski was castigated by Hilbert and the papers were published without delay. It is not clear if Brouwer's help was called in after all.[5]

In August 1923 Urysohn and Alexandrov made a trip to Norway; it was the end of term, they went on a long walking tour along the coast and fiords. In order to save their shoes, they walked barefoot when possible.

11.2 The Definition of Dimension

They returned to Göttingen and ended their visit to German at the meeting of the *DMV* in Marburg. There Alexandrov and Urysohn lectured respectively on 'Investigations from the theory of point sets' and 'Theory of general Cantorian curves'.[6] Urysohn had in Moscow continued his research in dimension theory, and the results were presented by Maurice Fréchet to the French Academy for the publication in the

[4] Alexandrov (1979), p. 298.

[5] Alexandrov (1979), p. 300. Blumenthal apparently proposed that Brouwer should check the proof sheets after the papers were accepted, cf. p. 413.

[6] *Jahresber. d. Deutschen Math. Vereinigung* 1924, pp. 68, 69 (italics).

Comptes Rendus, where they appeared in 1922 and 1923.[7] During his stay in Göttingen someone must have told Urysohn about an earlier paper of Brouwer on the same subject. He duly looked up Brouwer's 1913 paper on natural dimension, read it, and discovered a mistake. He mentioned this in his lecture in Marburg, which was not attended by Brouwer; of course the news reached Brouwer, be it that Urysohn approached him privately, or that a participant at the meeting told him. Brouwer was shocked—a ten-year-old basic paper, and nobody so far had found anything wrong with it! Here Brouwer was confronted with the consequences of his decision to publish his dimension paper in '*Crelle*' and not in the *Mathematische Annalen*: the paper had been systematically overlooked. In fact Brouwer's results were not used for further research, or only discussed in passing in seminars.

Brouwer, who had not thought about the dimension notion since the publication in 1913, asked Urysohn to send him an exposition of the alleged mistake.

At the Marburg meeting Brouwer lectured in the 'foundations' section. He gave a talk on the negative consequences of his intuitionistic program, 'The role of the principle of the excluded middle in mathematics, in particular in the theory of functions'.[8] This talk had already been presented at the Flemish Congress for Science and Medicine in Antwerp, one month before, where for the first time Brouwer demonstrated his Brouwerian counterexamples in public.[9]

Brouwer's talk was wedged in between those of Behmann (Algebra of logic and decision problems) and Fraenkel (New ideas on the founding of analysis and set theory). The *Jahresbericht* of 1923 contained brief reports of the lectures at the Marburg meeting. At the end of the report on Behmann's lecture, an objection of Brouwer to the use of 'etc.' and 'finite number' is mentioned. The speaker and the chairman (Schoenflies), the report goes on, refuted these objections, as the notions played no role in the lecture. Brouwer was not altogether pleased with this detail. In the next volume he protested in a letter to the editor that his remarks had been misinterpreted.[10] One may guess that the remarks were provoked by Behmann's somewhat irrelevant statement that 'Thanks to the efforts of the symbolic logicians (Frege, Peano, Russell) we now know that, in the first place, the whole of mathematics is represented as a collection of purely logical facts, ...'. In fact, Brouwer only wanted to point out that the contents of Behmann's lecture had no foundational consequences, precisely because of the use of the above notions. And, so Brouwer concluded, his remarks were refuted neither in the lecture, nor by the speaker or the chairman. Was the matter important enough for a reaction, one wonders. Brouwer, in any case, thought so; he could not appreciate the implicit suggestion that his contribution to the discussion was silly and irrelevant.

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[7]Urysohn (1922, 1923).

[8]*Die Rolle des Satzes vom ausgeschlossenen Dritten in der Mathematik, insbesondere in der Funktionentheorie.* Brouwer (1923a).

[9]See p. 442. In a sense the Unreliability paper of 1908 contained Brouwerian counterexamples. The application of Brouwer's technique to realistic mathematical problems followed in 1923.

[10]Brouwer (1925c).

In order to put the topic of this section in the proper perspective we now have to retrace our steps to the beginning of the twentieth century.[11]

The grand master of mathematics, Poincaré, had in the nineteenth century dominated the development of mathematics, his innovations in many areas have largely determined the shape of mathematics as we know it. But not only was he a great and deep scholar, he also had an admirable gift for popularisation, a very rare talent indeed. He published a large number of essays, mostly in the *Revue de Métaphysique et de Morale*, that were subsequently collected into small paperbacks. These books were an immense success. What made these books so extra-ordinary was that Poincaré explained things that could be understood by high school students, but that were equally well of use to professional mathematicians and physicists, who would draw inspiration from Poincaré's ideas and views.

Brouwer, for one, had been a devoted reader of Poincaré's philosophical essays, as is testified by his dissertation, where Poincaré is quoted ten times. This influence of Poincaré is best seen in the section on mathematics and the world (i.e. physics) and that on mathematics and logic.[12]

The topic of the present chapter, dimension, was also presented by Poincaré in one of his semi-popular expositions. He had already considered the question of the dimension of our (physical) space in 1895; that particular approach was based on the group theoretical approach of Sophus Lie, in the tradition of the *Riemann–Helmholtz Raumproblem.* For our account of the dimension theory this approach is not really relevant; much more so is a renewed attack of Poincaré on the question 'why has space three dimensions?' From 1903 onwards the idea of 'cut' appeared in his considerations. Some similar ideas had already been expressed by Euclid, who stated that a point is the end of a line, a line the boundary of a plane, and a plane the boundary of a solid body. Roughly speaking, we assign the dimension 0 to points and the dimension 1 to a line because if we cut it in a point we get two pieces. Similarly, if you cut a plane along a line, you get two pieces, etc. In 1903 Poincaré basically restricts these arguments to physical space, but in 1912 in the paper 'Why has space 3 dimensions?'[13] he considers mathematical space and its objects, and states his famous definition:

> A continuum has n dimensions when it is possible to divide it into several parts by means of one or more cuts which are themselves continua of $n-1$ dimensions. The continuum of n dimensions is thus defined by the continuum of $n-1$ dimensions; this is a definition by recursion.

The 'one or more cuts' are necessary, a circle for example, which one would definitely wish to be 1-dimensional, needs two cuts before two pieces are obtained.

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[11] My account of the dimension episode makes use of original documents, of Freudenthal's comments in the Collected Works of Brouwer, and of the outstanding papers of Dale Johnson, Johnson (1979, 1981). The reader who wishes to learn more about the topic is urged to consult these publications.

[12] Cf. p. 77.

[13] *Dernière Pensées*, Poincaré (1912), p. 488.

Poincaré's paper is a beauty, full of sound insight into geometry and physics. As he died that same year, it is idle speculation to guess what Poincaré would have done with this new purely mathematical definition. It is worth noting that at the time that Poincaré submitted his paper to the *Revue de Métaphysique et de Morale*, Brouwer's proof of the invariance of dimension had already been published, and that Lebesgue's second paper on that topic had already been presented to the *Académie des Sciences*.[14]

In Poincaré's paper there is no mention of this fundamental fact, as Dale Johnson puts it: 'The attitudes of Poincaré and Brouwer, the two greatest topologists at the beginning of our century, towards the problem of proving dimensional invariance were very different. While the former apparently thought that this problem was not very important, the latter thought that it was highly important, urgently requiring solution.'[15] The importance of the invariance problem is indeed a consequence of one's faith in the correct choice of notions. If one believes that dimension is essentially a topological notion, i.e. independent of our knowledge of the real line, plane, space, etc., and that topology is the science that studies properties invariant under topological transformations (i.e. continuous transformation without cutting or pasting), then Brouwer's invariance theorem is the supreme test for the notion of dimension. On the other hand, if one thinks that it is too early to say whether we have the right notions, and a certain amount of adjustment is still to be expected, then the invariance of dimension is nice, but not the last word. Perhaps this was roughly what distinguished Poincaré and Brouwer.

Before Poincaré's last paper, Frederich Riesz and René Baire had already investigated the topic of dimension, however, without success.[16] As their work is not relevant for our story, we will move on to Brouwer.[17]

The birth of modern dimension theory took place in 1913 in one of Brouwer's last papers before the First World War. This paper, with the cryptic title 'On the natural notion of dimension', was published in the *Journal für die reine und angewandte Mathematik*, also known as *Crelle's journal*, or simply '*Crelle*' (after its founder). The choice of this journal remains a matter of conjecture; although it was one of the better journals, it certainly did not cater to the specialists in topology. And, as a matter of fact, Brouwer's paper was totally overlooked by all and sundry. Why, is hard to say. If only Brouwer would have sent his paper to the *Mathematische Annalen* the history of dimension theory might have taken a totally different course. It is difficult to ascertain why Brouwer sent it to *Crelle* instead of the *Mathematische Annalen*; one reason might be that Brouwer wanted to avoid a leak that would give away the result, and might cause more priority problems. The reader may recall that

[14] 27.III.1911.

[15] Johnson (1981), p. 105.

[16] Cf. Johnson (1981).

[17] There are two more interesting definitions, Fréchet's *dimension types* and Hausdorff's dimension. They are interesting, and up to a point, fruitful notions, but they fall short of being 'natural'. Hence we will not consider them (cf. Johnson 1981). Note, however, that Hausdorff's dimension has suddenly become important in the theory of fractals.

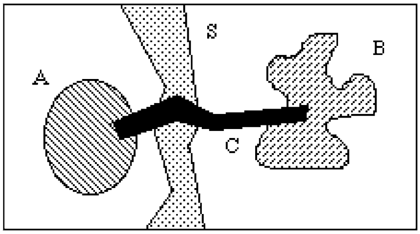

Fig. 11.1 Separation

in 1913 Lebesgue had not yet produced a proof of his paving principle, although he had informed Brouwer by mail of a new proof, to be published in the *Bulletin de la Société Mathématique de France*.[18] So if Brouwer's proof of the principle appeared in 1913, one could say in all fairness that Lebesgue had been given ample time to make good his claims.

A letter from Hellinger, editor of Crelle, has been preserved, in which Brouwer is promised immediate publication.[19] So, maybe Brouwer was in a hurry to get the paper published before others would catch on to Poincaré's ideas.

Brouwer's paper started with a discussion of Poincaré's proposal. The most obvious shortcoming was that some objects were assigned a dimension that one clearly would not want them to have, e.g. consider a double cone (like a diabolo), one cut of just one point separates it into two parts (to be precise, the removal of one point—a 0-dimensional set—produces two parts), so it should have a dimension 1. *Quod non.*

Furthermore, Poincaré had used the word 'continuum' in his definition. In order to develop the notion of dimension 'out of nothing' Brouwer therefore replaced the word 'continuum' by 'normal set in the sense of Fréchet'; which is a set with the property that any two of its points are contained in a closed connected subset.

Brouwer also objected to the use of 'one or more' in Poincaré's definition, pointing out that a finite number of m-dimensional manifolds can be joined to form an $(m + p)$-dimensional manifold.[20]

He then proceeded by giving a definition of separation: a set S separates two sets A and B if every closed connected set[21] C meeting A and B intersects S. In the definition S, A and B are closed subsets of a set P.

Next he went on to define 'n-dimensional':

> The expression: 'P has the general dimension degree n', in which n denotes an arbitrary natural number, now will express that for every choice of

[18] Footnote in Brouwer (1913a), p. 151. Lebesgue's proof appeared in 1921.

[19] Hellinger to Brouwer, 21.XI.1912.

[20] The simplest example is: the sets of the rationals and of the irrationals are 0-dimensional, but their union—the real line—is 1-dimensional.

[21] Brouwer was using the modern notion of 'connected'. This was not an obvious thing in 1913, and questions about the use of the notion have been raised by Urysohn and Menger. Whenever in the sequel 'connected' is used in the text without additional specifications, the modern notion is meant.

> [closed sets] A and B there exists a separating set S, which has the general dimension degree $n-1$, but not for every choice of A and B there exists a separating set which has a general dimension degree less than $n-1$. Furthermore the expression: 'P has general dimension degree zero, resp. infinite' means that P has no part that is a continuum, resp. that for P neither zero, nor any natural number can be found as its general dimension degree.[22]

Brouwer's definition is, like Poincaré's one, inductive ('recurrent' as he called it), it avoids the 'double cone'-difficulty because all points (subsets A and B) are treated on an equal footing.

It is not too far fetched to guess that Poincaré, could he have returned to the topic, would have hit on the same or a similar improvement. Speculations of this sort are always tricky; it is nonetheless pertinent to point out that Brouwer's geometrical intuition was better adapted to the infinite variety of possible pathologies than Poincaré's intuition, which felt more comfortable with the natural aspects of geometry. So Brouwer may have been in a better position to handle the intricacies of dimension.

Brouwer, of course, realised that it is one thing to introduce a new general notion, but that the proof of the pudding is in the fact that the notion extended the old one, so he went on to put this definition to the test. That is to say, he set out to prove that the traditional dimension of our space is indeed 3 according to the new definition (or rather that $\mathbb{R}^n$ has general dimension degree n).

An interesting detail, more of a methodological than of a topological nature, is that Brouwer rephrased the definition in the form of a two-person game. Let the players be called $\forall$ and $\exists$.[23] The players move alternately, and we will denote the moves by $i_\forall$, $i_\exists$.

Here are the moves of both players:

$1_\forall$—$\forall$ chooses two disjoint closed subsets A_1 and B_1.
$1_\exists$—$\exists$ chooses a closed separating set S_1.
$2_\forall$—$\forall$ chooses two disjoint closed subsets A_2 and B_2 of S_1.
$2_\exists$—$\exists$ chooses a closed separating set S_2 in S_1. Etc.

$\exists$ wins if a separating set S_h is reached which does not contain a continuum (we would say, is totally disconnected); otherwise $\forall$ wins. We say that $\exists$ has a winning strategy if, no matter what the moves of $\forall$ are, $\exists$ wins. If $\exists$ has a winning strategy such that each game (sequence of moves) ends after at most n moves (of each) then the original set has dimension at most n. If there is no winning strategy for $\exists$ to win in at most $n-1$ moves, then the dimension is exactly n (i.e. $\forall$ can choose his moves in such a way that n moves are required for $\exists$ to win). Brouwer's reformulation of

[22] We have replaced Brouwer's π, ρ, ρ', π_1 by P, A, B, S. Note that 0 is not considered a natural number. The problematic and controversial aspects of this definition will be considered in detail in the sequel (see p. 408 ff.).

[23] *Abélard* and *Éloise* in Wilfrid Hodges' terminology. The presentation of the game is somewhat updated.

the definition of dimension in terms of a game is remarkable because it is (to my knowledge) the first such reformulation of an inductive definition.[24]

Here is an example of how the game works for the dumb-bell:

Two solid balls are connected by a line. If ∀ chooses closed sets in the left and right hand balls, then ∃ can separate them by a point, however, ∀ can play a better game—if he chooses two points in the right hand ball, then ∃ has to separate them by, for example, a plane section, then ∀ can choose two points in this disc, which ∃ will separate by a line, and finally ∀ may choose two points in the line, which ∃ separates by a point. This game takes 3 steps, and indeed, ∃ has a winning strategy for a 3-step game, but not for a 2-step game, so the general dimension degree is 3.

Far from considering the game definition an exotic toy, Brouwer used it in the following soundness proof.

Brouwer had already observed that the new notion was no good unless one could prove the *dimension theorem*,[25] which states that the n-dimensional Euclidean space has general dimension degree n. He had immediately realised that here Lebesgue's tiling principle was the thing to use, and so he wrote down a proof, which was—by the way—short and elegant. He used the occasion to indicate in a footnote the gap in Lebesgue's 1911 proof, and to give a slick proof of the tiling principle by means of his mapping degree. In a footnote he pointed out that, since the dimension (dimension degree, as he called it) was a topological invariant, the invariance of dimension now became an immediate corollary.

At this point the matter rested for some 10 years, until, in 1923, Brouwer to his surprise met a young man who had independently discovered the definition of dimension, and Urysohn was equally surprised to find out that some one (and not just any old topologist) had long ago given a definition of dimension. There must have been some measure of consolation in his quick discovery of an 'irreparable mistake', as he called it.

Brouwer might have switched from topology to intuitionism, but that did not mean that he was willing to acquiesce to the criticism of Urysohn. Being very meticulous in his work, Brouwer was not prone to serious errors, so he felt that he was basically right. It could not be denied that Urysohn's remarks seemed to imply that Brouwer's claim to the right notion of dimension, including the 'soundness proof', was forfeited. So a reaction was in order. As soon as he returned from the Marburg meeting, he consulted his notes, and almost immediately sent a card to Urysohn from Zandvoort, where he spent a short holiday at the seaside. Only Brouwer's draft remains,—Urysohn never received the card.[26] It ran as follows:

> Saturday 29.IX.23 from Zandvoort, pension John Bückmann, written to Dr. P. Urysohn, Mathematisches Seminar der Universität, Moskau, concerning the

[24]Cf. Aczel (1977).

[25]Brouwer (1913a), p. 148.

[26]He replied (27.XII.1923) that he blamed the loss of this card on the university rather than on the Russian mail. Letters addressed to his house had always reached him, but recently three letters, addressed to the university, had gone astray.

> note in pencil (at the separation definition) in the margin of my personal copy (*hand exemplaar*) of *Über den natürlichen Dimensionsbegriff*. This pencilled note, that clarifies everything, must date back many years; it is very well possible, that it has been made as a result of a remark by a colleague (in that case probably Weyl, Gross or Rosenthal). I shall try to ascertain this and also check if the note has not already been added as an Erratum to a later publication.
>
> (signed) L.E.J. Brouwer.

Notwithstanding, his conviction that—in view of the correction—his dimension notion was impeccable, he was somewhat worried about Urysohn's handling of the matter. After all, a beginning mathematician does not every day catch a famous mathematician nodding. Brouwer was himself no stranger to this vice. A letter to Bieberbach, one of the editors of the *Jahresberichte*,[27] which routinely published reports of the annual meeting of the German mathematicians, sheds light on Brouwer's worries:

> ......Furthermore I would like to request the following (which I beg you to pass on to Gutzmer, in case it concerns not you but Gutzmer): In Marburg a Russian, Mr. Urysohn from Moscow, gave a talk (which I did not attend), in which he presented my general dimension theory as being untenable on principle. This certainly was unjustified, for my paper in question has, in the course of the years, not only been studied and checked in numerous seminars, but also by very critical and penetrating mathematicians like Weyl, Rosenthal, Birkhoff, Veblen and Alexander,[28] without yielding anything but a few omissions, such as occur in any paper, that don't damage the body in the least and the correction of which occurs automatically to the reader: and so the paper has so far always been cited without objections, and now Urysohn doubtlessly has misunderstood it.—Now, if the mentioned Marburg talk or a report of it should be submitted to the *Jahresberichte*, I would like to ask and to advise you to submit the manuscript in question to my inspection (taking into account the above mentioned, and to avoid unpleasant polemics) before sending it to the printer.

It is likely that the letter to Bieberbach predates the card to Urysohn, otherwise Brouwer would probably have mentioned the 'minor correction in the margin' referred to in the card to Urysohn. Brouwer always maintained that the mistake discovered by Urysohn was a minor slip of the pen, one of those little details that any serious reader would automatically correct. In view of the fact that—in spite of all foundational preoccupations—topology had become second nature to him, it is quite plausible that Brouwer was somewhat amazed at the fuss made about such a little gap in the definition. The above letter also shows that in Brouwer's view

[27] Only Brouwer's handwritten and undated copy survives, but it must have been written between the meeting at Marburg and Urysohn's letter with details.

[28] Even though the paper was apparently discussed in certain circles, it found no follow up in the mathematical literature.

the natural dimension paper was not an overlooked item. He had of course sent out reprints to his brethren in topology, and the above cited mathematicians may very well have read the paper, but grosso modo it remained relatively unnoticed until Urysohn brought it to the attention of qualified mathematicians. From a letter to Schoenflies it appears that Brouwer had inserted the corrections in handwriting in the reprints that he had sent out, but which he seemed unable to recover when they would have been helpful.

Anyway, it is evident that right after the Marburg conference Brouwer could not quite recollect the precise details as to what happened with the 1913 paper and its corrections. As we will see, he soon found enough evidence to convince himself that he had not overlooked the point raised by Urysohn. Bieberbach apparently complied with Brouwer's request and sent him Urysohn's manuscript and Brouwer did not find any objectionable remarks in it (the published account of the talk, anyway, contained no reference to Brouwer's oversight)[29] for he wrote to Bieberbach:

> Enclosed, Urysohn's report is returned, with warm thanks. My name is not mentioned in the citations. I hope to have a discussion with Mr. Urysohn (who was in Marburg, probably under the fresh influence of the smear campaign (*Hetze*) led by Hilbert against me), for the time being I would have only one request to you to inform me, should Mr. Urysohn in a possible correction of his report insert my name afterwards, ...

Clearly Brouwer did not wish to take risks; as we have already seen, he was somewhat over-sensitive to real or imagined slurs on his professional status, and he did not want to be ridiculed by a newcomer on account of an ordinary lapse in exactness. He wanted to be prepared if it should come to a public debate, and so without qualms he used his status to get first hand information on a possibly looming battle.

It must have been a relief for him to find out that Urysohn was not after a cheap victory; to some it might have been tempting to correct the high and mighty Brouwer, but Urysohn had no inclinations of the sort, nor did he need the doubtful publicity involved in a bit of one-upmanship.

Brouwer may have been right about Urysohn being somewhat prejudiced against him; the remark on Hilbert's campaign against himself is evidence that is not easy to overlook. Brouwer was all too familiar with the goings on at the major universities in Germany (in particular at Göttingen), to remain unaware of an unkind reception of his foundational views. It is unlikely that it was 'all in the mind'.

Urysohn, who had not received Brouwer's card of 29 September, reacted to Brouwer's request for details, which Brouwer had made in Marburg. In a letter of October 24, in which he clearly and precisely pointed out where Brouwer's proof of the 'dimension theorem' failed: 'In Marburg you have called upon me to impart to you the objections which I have made in my lecture to your proof in Crelle's journal.' Urysohn's letter gives a perspicuous and to the point exposition of his objections. It contained a clever counter example to

[29] JDMV 1923, p. 69 italics.

some claims in Brouwer's proof, Brouwer quoted it in his note in the Proceedings of the Amsterdam Academy.[30] Urysohn's letter would have alarmed anybody less well-versed in topology than Brouwer. Criticising Brouwer's definition of 'connectedness', Urysohn warned, 'I will show below that your proof remains defective under *any* definition of these notions.' The conclusion being that (i) the notion of dimension was not properly defined, (ii) whatever the corrections were, the proof of 'dimension degree of $\mathbb{R}^n$ is n' could not be saved.

Brouwer must have been in some confusion how to react. He finally replied in December, apologising for the delay and explaining that he had hoped to get a reaction to his card of September, so that an exchange of views might be more fruitful. He inquired if Urysohn had received the card at all, and he closed the letter on an unusually complimentary note:

> I have the impression that the method sketched there[31] leads to an extremely valuable insight into the structure of continua, after trivialities have been published by the dozen in this field during the last years.

For those readers who think that Brouwer is laying it on a bit thick, it must be pointed out that Urysohn was indeed an exceptionally talented mathematician; during the short period of his mathematical activity (only three years) he produced a wealth of results and insights. To this day his name is connected with important notions and results in topology. Brouwer simply recognised a young genius when he met one (or read his work).

In the ensuing correspondence Brouwer and Urysohn promised to exchange reprints; it is characteristic of the hardships of the time that Urysohn could not afford to pay the price for the usual number of reprints of the *Comptes Rendus* notes, so he promised Brouwer to copy them for him by hand.[32]

In a letter of 22 January 1924,[33] Brouwer set himself to discuss the dimension paper in some detail. He began by repeating the content of the lost card:

> After returning home from Marburg, your objection raised there became immediately clear to me by consulting my personal copy of the paper *Ueber den natürlichen Dimensionsbegriff*, where I have an old note in the margin of p. 147, lines 17–20, according to which this passage is 'to be brought into conformity with page 150 at *)'.[34] It was this note in the margin that my card, mailed from Zandvoort, referred to.

He then entered into Urysohn's objections; in the first place he remarked that the notion of domain (Gebiet)[35] in his older papers had been used in a way that fixed

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[30]Brouwer (1924e).

[31]i.e. in the *Comptes Rendus* notes of 1922.

[32]The copies are still in the Brouwer Archive.

[33]Brouwer's copy (handwritten).

[34]'*mit S. 150 bei *) in Uebereinstimmung zu bringen*'.

[35]A domain is an open, connected set, see e.g. Brouwer (1910a), p. 169.

the meaning in the natural dimension paper of 1913, in the second place, he agreed that the key to the gap in the proof was the separation definition. Here Brouwer recounted the history of the paper and its sequel:

> As to the origin of the oversight (*Versehen*) of p. 147,[36] my records of that time make it probable that in the manuscript of the paper there had originally not been an explicit definition of separation, as for example in my paper 'Proof of the invariance of dimension', published in Math. Ann. 71, and that such a definition was inserted rather thoughtlessly, after a co-reader of the proofs had pointed out the absence to me. When, not long after the publication of the paper, the oversight became clear, a quick correction must have remained forthcoming, because I expected soon the publication of the paper, on the same topic, promised by Lebesgue and mentioned on p. 151, and I was convinced that this paper would require a rejoinder, which would accommodate in a natural way the required correction. When subsequently the paper promised by Lebesgue kept us waiting year after year, the matter disappeared in the course of the years gradually from the realm of my thoughts, and without your interpellation I would perhaps never have thought of it again. I have now also, as a result of your remarks, studied the statement of Lebesgue, published with a delay of 10 years (and not, as agreed, in the Bull. de la Soc. Math., but in the second volume of Fundamenta Mathematicae, and seen that it, exactly as I already expected 10 years ago, calls for a rejoinder on my part, because the proof of Lebesgue of the auxiliary theorem of p. 150 of 'On the natural notion of dimension' only offers a botched up form of my proof of the same theorem. I hope that this rejoinder will appear soon. It will contain (mentioning your priority) at the same time the correction of my old slip. [. . .]
>
> To be sure, my own investigations are oriented since some years in another direction, but my interest in topology has remained and I consider you as one of the few who could here open up new perspectives.

Within a month Brouwer mailed a manuscript containing the correction to Urysohn.[37] What, in fact, was this mysterious 'oversight'? This is spelled out in Brouwer's revised paper,[38] which provided an argument based on a seemingly trivial correction. The cause of all problems and confusion was the erroneous insertion of the adjective 'closed' in the separation definition.

Consider three disjoint closed sets A, B and S. S is said to separate A and B if every closed connected set meeting A and B also meets S. By deleting the last occurrence of 'closed' the standard separation notion is obtained. What Brouwer claimed in the above letter was that the separation definition was added at the proof reading stage, and that the insertion of 'closed' was a slip made in haste. We will

[36] Brouwer (1923e).

[37] Brouwer to Urysohn, 19.II.1924.

[38] Brouwer (1923e).

return to this slip later. In the published corrections Brouwer used a somewhat different version of separation: S separates A and B if S determines a domain containing A, but not B.

Both corrections were noted by Urysohn in his letter of 24 October. In fact, the modern reader will find Urysohn's formulation of the separation definition more congenial.

The old definition stated that any closed connected set 'running from A to B' must intersect S (A and B disjoint). By dropping the 'closed' from the definition S has to block more 'connecting sets', so the notion of separation becomes stronger. In other words, it becomes easier for $\exists$ to win the dimension game. We therefore speak of a weaker dimension.

Indeed, Urysohn's example (reproduced in Brouwer 1924e) exhibits a separating set that meets all closed connected 'intersecting sets', but not all 'connecting sets' which are merely connected.[39]

The alternative separation definition does not need this barrier between A and B: A and B are separated if there is an open connected set S such that $A \subset S$ and $B \cap S = \emptyset$.[40]

This letter is followed by a long silence on the part of Urysohn. So long indeed that Brouwer started to worry, did Urysohn get the letter, was Urysohn not convinced? One should keep in mind that Brouwer's reputation was at stake. He had become the paragon of exactness, as one may infer from a letter from Carathéodory to Hilbert in 1912: 'You know how often, if you make an exception for Brouwer, in this part of mathematics people are sinning.'[41] With his high standards of exactness, he did not hesitate to publish his own corrections. But it was more than he could bear to seeing the crowning paper of his topological days reduced to a 'nice try'. The more so, as he was certain that the criticised point was no more than a slip of the pen, a detail that every well-informed reader would correct in passing. When Urysohn's reaction remained forthcoming, he took a minor preventive action. At the Marburg meeting Urysohn had made it known that a comprehensive treatment of the notion of dimension (and much more) was to appear in Fundamenta, so any criticism that had been communicated privately to Brouwer could be expected to be made public in this paper. And so Brouwer wrote to the editor of *Fundamenta Mathematicae*, Sierpinski, that the substantial paper, submitted by Urysohn might contain a criticism of Brouwer's dimension paper that was based on a 'small isolated error' in the definition of a technical notion, and that a correction of this definition answered all objections of Urysohn.[42] He conjectured that the unreliable

[39] Cf. Johnson (1981), p. 173.

[40] In Brouwer's formulation 'S separates A and B if S determines an open connected set S' such that $A \subseteq S'$ and $B \cap S' = \emptyset$'. The modern formulation runs as follows: 'S separates A and B if the complement of S is the disjoint union of two open sets A' and B', such that $A \subseteq A'$ and $B \subseteq B'$'. Cf. Hurewicz and Wallman (1948).

[41] Carathéodory was discussing topological papers of his for the *Annalen*. 5.V.1912.

[42] In the letter to Sierpinski (25.III.1924), Brouwer cited a specific place in his paper on the Jordan curve theorem (Brouwer 1910a, p. 170, line 9), where one of the basic terms is introduced. Its role becomes clear in his correction, e.g. Brouwer (1924h).

Russian postal system might be the reason for the breakdown of the communication between Urysohn and him,

> Being convinced that the editorial board of the Fundamenta Mathematicae, just like me, wants to prevent avoidable polemics, I am informing you for the present that, except for the above mentioned correction, my memoir 'Ueber den natürlichen Dimensionsbegriff' is perfectly in order. I hope therefore that you will be so kind to see that the work of Urysohn, which you must publish, contains no unfounded criticism.

It turned out, however, that Brouwer's intervention with Sierpinski was not necessary. Urysohn's reply showed that he took Brouwer's point:[43]

> I admit that your new definition[44] is the only one that is connected with the remaining contents.[45]

He added that he failed to see why Brouwer called this notion of separation 'the usual one',[46] for in publications before the natural dimension paper separation was only used for manifolds—'where the two existing notions are identical'.

The matter may appear somewhat confusing, but one can visualise a separation of (say) two points x and y by a circle, and more generally by a set that is connected. Now 'the more connected' this set is, the harder it is to get from x to y. So if you cannot get from x to y past a weakly connected set (a thin wall) you can certainly not get from x to y past a strongly connected set (a thick wall) and so if two sets (or points) are already separated by a weakly connected set, they are strongly separated.

Without following Urysohn's criticism in detail let us remark that the crucial point was the notion of *separation* and hence also that of *connected set*. As the episode of Urysohn's critique (and the later controversy with Menger) rests on the question 'was Brouwer aware of the correct notion of connectedness?', we will take a somewhat closer look at the matter.[47]

The first idea that comes to mind when considering connected sets in the plane is that of 'arcwise' connectedness, i.e. a set is (arcwise) connected if any two of its points can be connected by an arc or a path (i.e. continuous image of the closed interval [0, 1]). This idea goes back to Weierstrass, who used connection by a polygonal line. The next step towards a more abstract notion was taken by Jordan, who defined a connected closed set as one that could not be split into two non-empty disjoint closed sets.[48]

Schoenflies adopted the same definition in his *Bericht* of 1908 (without the 'non-empty' clause), and he gave separate definitions for open and closed connected sets.

[43] Brouwer to Sierpinski, 28.III.1924. 'After waiting for more than three months I finally received a sign of life from Urysohn, which makes my letter of 25 March superfluous.'

[44] Of separation.

[45] Urysohn to Brouwer, 20.III.1924.

[46] Brouwer (1923e), footnote 11.

[47] We follow Freudenthal's exposition in Brouwer (1976).

[48] Jordan (1893), p. 25.

As late as in 1918 Carathéodory followed this example in his lectures on real functions.

The present notion, which says that a set A in a topological space is connected if it cannot be written as the disjoint union of two open (or closed) sets in the relative topology of A, was formulated by Lennes,[49] and (almost certainly) independently by Brouwer in the same year.[50] There is also an elegant completely general definition of connectedness in Brouwer's letter to Engel.[51]

Hausdorff's book,[52] the bible for whole generations of topologists, spelled out the modern definition, and so the notion is generally ascribed to him, but Brouwer would consistently refer to Lennes.[53] In a private note (in the handwriting of Cor Jongejan) he remarked:

> I only started to quote Lennes after I observed that the definition of connectedness which was in the old days considered obvious, was blown up into a discovery by a later generation.

Anyway, there is no doubt that Brouwer knew the general notion of 'connectedness'. When Karl Menger suggested in the late twenties that Brouwer in 1913 did not know Lennes' paper (and hence the above notion), Brouwer even produced letters showing that he had refereed the paper of Lennes that was a sequel to the 1911 paper.[54]

Actually, virtually unknown to the topologists of the day, Frigyes (Friederich) Riesz had already given the correct definition in a long paper, which was unfortunately published in an inaccessible place (1906).[55] So, as far as one can judge, he should also be credited with the modern notion.

The notion of connectedness was really important for the definition of dimension, since both the notions of 'separation' and 'domain' were based on it, as Urysohn had seen at once.

In his manuscript of the revised dimension paper Brouwer had explained to Urysohn that he had indeed meant by 'connected' and 'domain' the correct notions all the time, and that the fateful word *abgeschlossen* (closed) was a slip of the pen, and that he always had the intended concept of separation in mind, cf. p. 409.

For a punctilious man like Brouwer the matter of the oversight presented an awkward situation, to say the least. He must have felt that outsiders would and could

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[49] Lennes (1911). We know that Brouwer had seen Lennes' definition in the subsequent publication of Lennes in 1912.

[50] Brouwer (1911d), p. 308. 'Eine innerhalb κ abgeschlossene Punktmenge [π] soll *zusammenhängend* heißen, wenn sie nicht in zwei innerhalb κ abgeschlossene [(oder relative abgeschlossene)] Teilmengen zerlegen lässt.' The insertions are Brouwer's, made in the margin of his private copy.

[51] A set X is connected if for any assignment of neighbourhoods U_p to points p in X and for any two points p and q there is a finite sequence $p = p_0 = \cdots = p_n = q$, with $p_i \in U_{p_{i+1}}$. Brouwer to Engel, 9.III.1912, CW II, p. 149.

[52] *Grundzüge der Mengenlehre*, 1914.

[53] Cf. Freudenthal in Brouwer (1976), p. 487.

[54] Blumenthal to Brouwer 3.II.1912, 12.II.1912, cf. Brouwer (1976), p. 487.

[55] Riesz (1960).

consider his claims as 'fiddling the books of history'. Freudenthal, in his edition of Brouwer's topological work, has carefully sorted out the evidence, and although Brouwer's case would probably have a hard time in a court of law (for after all, a devious person could fake the historic evidence), it looks strong enough to be adopted as the correct one.

In the first place, Brouwer had corrected his reprints in handwriting by inserting 'connected in the sense of Lennes' in the margin of the definition in question. In his private copy the word *abgeschlossen* is struck out with pencil, and commented 'to be deleted conform footnote *) p. 150' and finally, Brouwer had added a note in the proofs of Schoenflies' new edition of the 'Bericht' (1913) on p. 382: 'similarly the investigations of Brouwer in Math. Ann. 70, p. 161–165 and Journ. f. Math. 142, 146–152 (in the latter one the word 'closed' on p. 147, line 18 has to be deleted, according to a communication of Brouwer)'. That particular part of the proof sheets of Schoenflies' Bericht is still in the Brouwer Archive. It shows a handwriting that fits his style of writing in the early teens of the century. The new edition of Schoenflies' *Bericht* did not contain this footnote. The reason for this is unclear, perhaps Schoenflies thought the note irrelevant (and he might have a point), perhaps the printer missed it. It had escaped Brouwer's eye, for only after Urysohn's intervention did he find out that it had not been adopted.

Urysohn, evidently, was sufficiently satisfied to drop the matter and to accept Brouwer's views and hence his priority for the definition of dimension. It should be pointed out that Brouwer's and Urysohn's definitions are in general not equivalent. There is also a technical distinction: Brouwer gave a *global* definition of dimension, i.e. for a space in its totality, whereas Urysohn gave a global and a *local* one (where dimension is defined in a point x, i.e. by applying the separation procedure in arbitrarily small neighbourhoods of x).[56]

The, for Brouwer so desirable, peace was suddenly disrupted when on June 21 a registered letter from Göttingen was forwarded to Brouwer, who was at that moment conducting gymnasium examinations in the country (the traditional, time consuming but useful, voluntary task of Dutch professors). It contained an urgent message from Urysohn, and a proof sheet of a paper that Urysohn had submitted to Hilbert for the *Annalen.* It was entitled '*Über den natürlichen Dimensionsbegriff*', and contained an exposition of the mistake in Brouwer's eleven year old paper with the same title. The note leaves the impression that Brouwer had utterly failed the goal he had set himself. Hilbert must certainly have felt a certain quiet amusement, seeing his (by now) arch-enemy stumble, especially where Brouwer had always shown little patience with the weaknesses of others, such as Lebesgue, Engel and Schoenflies.

But it is always easier to attack the weaknesses of a person you don't know personally, than those of a man you have come to appreciate and like, so Urysohn, somewhat embarrassed, wrote Brouwer that he had already submitted this small note to the *Annalen* in July (thus before the Marburg meeting), and since then had completely forgotten about it. So when he suddenly received the proofs he was at a loss,

[56]Cf. Johnson (1981), p. 178.

> It is completely unclear to me what I should do with it, perhaps the 'supplement added in the proof' will satisfy you.

The supplement ran as follows:

> In the preceding claims I have obviously based myself on the assumption that one sticks to the definition of dimension of Vol.142 of Crelle's Journal. Now, Mr. Brouwer has since then published a correction,[57] where he indeed changes the notion of separation on which the notion of dimension is based. *In that way* the proof has been corrected completely, and I would like to stress particularly that, as I have been informed, the necessity of such a modification of the definition of separation had been known already for a long time to Mr. Brouwer, and that its publication has not been carried out due to a lapse. Nonetheless, I believe that the above lines may be useful, since Mr. Brouwer, in his Correction, has not indicated *why* the old definition has to be rejected.
>
> Göttingen, 21 June 1924.

Brouwer acted immediately, he elaborately explained to Urysohn that publication would be unwise,[58]

> Many thanks for sending me the proofs of your forgotten small note for the Annalen and for seeking my advice on it. It is my opinion, that in both our interests, the publication of this note should absolutely be refrained from. For the publication of an oversight, which had escaped the notice of scholar B by scholar A, is only then compatible with the dignity of scholars, if either the oversight can only be grasped by means of an extensive exposition of new discoveries of A, or if all consultation between both parties involved has become impossible (e.g. on political grounds or because of the death of B). In each other case such a publication creates a suspicion that either A allows himself to be carried away by imprudent ambition, resp. deliberately wants to offend B, or that B did not want to acknowledge his oversight to A, resp. has refused public acknowledgement, at least to fullest extent. Fortunately, none of the above circumstances exists in the present case, rather the contrary, in every respect.

'It would be useful', he continued, 'to publish your counter example, but that would find a natural place in a note in which I am going to exhibit the evidence of my early correction'. He added that he hoped that Urysohn would agree with the retraction of the note, saying that the matter with the Editorial board and the publisher of the *Mathematische Annalen* could easily be arranged by himself, being after all an editor.

Urysohn's letter and the proof sheet may possibly not have caught Brouwer unawares. It is not unlikely that Alexandrov and Urysohn's manuscripts were, after all, passed on to Brouwer for advice.

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[57] Brouwer (1923e, 1924h).

[58] Letter of 24.VI.1924.

Blumenthal had written on June 14 that he was sending Brouwer proof sheets of a paper of Urysohn and Alexander plus three more papers that were already in print. Which papers were involved is not known. The small note mentioned above should also have passed through Brouwer's hands, since Brouwer contacted Blumenthal, before he had received Urysohn's letter, asking permission to deal with Alexandrov and Urysohn directly in the name of the editorial board. Blumenthal replied on 14 June that the decision was not his; Hilbert had to be consulted about the matter. On the same day, in a letter, he had suggested that Hilbert authorise Brouwer to discuss the matter of the dimension note directly with Urysohn, and that on account of the alleged error the paper might after all have to be rejected. He also suggested that it would be wise to send Brouwer the already accepted manuscripts of Alexandrov and Urysohn so that he could if necessary make corrections—'for this single example makes me apprehensive'. In his letter to Brouwer he carefully added that Brouwer could only correspond with Urysohn in his own name; since he was personally involved in the matter of the definition of dimension, it would not be fitting to act in an editorial capacity. Hilbert's reaction has not been preserved, but it is likely that he gave Blumenthal his fiat. So when Brouwer wrote to Urysohn, he was not violating any editorial guidelines. He must have felt confident—on account of Urysohn's letter—that an agreement would be forthcoming; indeed he informed Blumenthal that Urysohn was about to agree that his paper was not suitable for publication. Should some more persuasion be necessary, he wrote, then Blumenthal could tell Urysohn that Brouwer would pay the bill for the now useless proofs, and that the editorial board 'would obviously follow in this matter the judgement of the only involved and only qualified member'. We see that Brouwer was not totally adverse to some muscle-flexing, the cause justified this, as Brouwer had already refuted Urysohn's claim that Brouwer's mistake was beyond repair; moreover Urysohn had accepted Brouwer's explanations. In Urysohn's words, 'I have received your card of the twenty second and your letter of the twenty fourth of this month; I am very grateful to you that you have given me so kindly and extensively your opinion. I completely agree with everything that you have written.' It would certainly have been a serious blot on Brouwer's topological record if Urysohn had been right. So this episode in the history of dimension ended well. It remains a bit of a mystery why Hilbert did not assign the papers of Alexandrov and Urysohn to Brouwer right away. One would expect that in a well-run editorial board manuscripts would be handled by the recognised experts, and certainly in a tricky subject like topology.

In the Brouwer–Urysohn correspondence there is a particular interesting letter, which plays no role in the discussion, but which sheds more light on the confusion of notions. On 14 June Brouwer wrote:

> Perhaps the enclosed variant to the passage in Crelle's journal 42, between p. 149, l.2 bottom and p. 150, line 10 bottom, by means of which the proof is adapted to the separation definition found on p. 147, (thus without the deletion of the word 'closed') will interest you. [...]
>
> This variant, which I recently found among my papers from the years 1912 and 1914, is most probably communicated in the correspondence that I pur-

> sued at the time with Schoenflies, Gross and others about, among other things, dimension. I will see if perhaps the other parties have filed this correspondence better than I have. My own interest has been diverted for a full nine years from these topics, and as an archivist I have always been a failure. I consider, by the way, as before, the separation definition without the word 'closed' as the proper and more fruitful one for dimension theory.

The enclosed part of a proof was submitted two days later to the *Mathematische Zeitschrift*, and published that same year.

The upshot of this message was that Brouwer had at an earlier period investigated another notion of separation (weak separation), which led to another (strong) concept of dimension. Freudenthal, who had no access to the Brouwer–Urysohn correspondence, noted in his comments on the dimension episode, 'The fine distinction between weak and strong connexion may appear as an a posteriori implantation—such subtleties in the stone age of topology! Yet, Brouwer was subtle.'[59] The above letter proved Freudenthal right. From the argument of the paper in Crelle and Brouwer's use of the correct notion of connectedness, one may well conclude that he indeed had the correct (weak) dimension notion in mind, and the 'slip' was really a slip. The fact that Brouwer discussed the above strong dimension is probably a partial explanation of the confusion, and also a demonstration of his undiminished topological powers. Both dimension notions are mentioned again in Brouwer (1924e) and Brouwer (1928f).

Urysohn did not react to Brouwer's alternative notion (at least no written evidence is extant).[60]

When Urysohn received Brouwer's letter concerning the note for the *Mathematische Annalen*, he suddenly recalled that in the manuscript of his paper '*Mémoire sur les multiplicités Cantoriennes*', submitted to the Fundamenta Mathematicae, a similar reference to Brouwer occurred, and so he hurried to ask Sierpinski to replace the reference by a revised one.[61] The original reference to Brouwer was stern in tone:

> I lately learned about a paper of M. Brouwer *(Über den natürlichen Dimensionsbegriff), …* where he proposes to solve this question by a method which seems (at first sight, at least) very close to mine. Now, the proof of M. Brouwer contains an error which, it seems to me, cannot be corrected, and which undermines all his results. I refer for the details to a supplement to be found at the end of the first part of the present mémoire.[62]

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[59] Brouwer (1976), p. 551.

[60] For a thorough analysis of the various notions the reader is referred to Freudenthal's commentary in the Collected Works II, p. 548 ff. and Johnson (1981), p. 171 ff.

[61] This request was made in a letter mailed from Göttingen, so it was not written under the supervision of Brouwer. Urysohn to Sierpinski, 27.VI.1924.

[62] Copy in the Menger Archive.

Urysohn pointed out to Sierpinski[63] that although the remark was justified with respect to the original paper, it would be out of place after Brouwer's correction.[64]

> Analyzing my 'Brouillon', I have seen that in the introduction to my 'Mémoire sur les multiplicités Cantoriennes' I have written about Brouwer's 'Natural notion of dimension' an observation, in which I wrote roughly the following: 'Now the proof of this theorem contains an error which, it seems to me, cannot be corrected.' Although this observation is justified with respect to the old formulation of Brouwer's paper, in his correction[65] he changes the *definition of the notion of dimension*, it seems to me that the publication of my observation would not be appropriate. Therefore I permit myself to beg you to modify this observation: if possible to replace it by the observation below, if it is already impossible on technical grounds, to delete it at least altogether. Here is the text of the desired observation:
>
> "the memoir was already finished when I got to know the paper '*Über den natürlichen Dimensionsbegriff*', published by Brouwer in 1913 in the Journal für [die reine und angewandte] Mathematik (v.142, p.146). I hope to return to the definition of Brouwer and mine."

The letter must have reached Sierpinski, since Menger at some later date acquired a handwritten copy. The first proofs of Urysohn's paper do not contain the desired correction, so either Sierpinski thought the matter of marginal importance, or the proofs were ready before he could inform the printer.

The correspondence between Brouwer and Urysohn (often in combination with Alexandrov) continued right up to the death of Urysohn. The two Russians left Göttingen first for Bonn to visit Hausdorff. After repeated fruitless visits to the Dutch consulate in Cologne, they finally got a visa for an extended trip to Holland and continued their journey. The two stayed in Blaricum with Brouwer. From this visit there is a pictorial witness: a photograph of Brouwer sitting between his two young friends in the garden.[66]

Not surprisingly, a great deal of time was spent on discussing the work of Brouwer and of Urysohn on dimension theory. One of the topics, of course, was the above mentioned mémoire.

Brouwer was so impressed by the two young visitors that he insisted that they should come back in October and spend the year in Holland.

Unfortunately Brouwer had a previous engagement in Göttingen,[67] so that he had to leave the two Russians behind. Urysohn and Alexandrov tried to make the best of it, and they went as proper tourists with Cor Jongejan and a lady friend of

[63] Urysohn to Sierpinski, 27.VI.1924 (Menger Archive).

[64] Brouwer (1923e).

[65] Brouwer (1924e).

[66] See Collected Works II, p. 453.

[67] He was invited for talk on the foundations of mathematics; it was one of the exchanges in the Brouwer–Hilbert debate, commonly known as the *Grundlagenstreit.*

Fig. 11.2 Alexandrov, Brouwer, and Urysohn in the garden at Brouwer's hut (1924) [Brouwer archive]

hers to Amsterdam. From Amsterdam they sent a postcard to Brouwer, lamenting his absence. The Russian must have made a somewhat curious impression on the Dutch, as appears from the fact that they were kicked out of the Rijksmuseum on the grounds that they did not wear a jacket.

From Amsterdam they moved on to Paris where they met Fréchet. Their next stop was in Brittany, in Batz, a small fishing village at the end of the world. Urysohn reported their experiences in a letter:[68]

> *Hochgeehrter und lieber Herr Professor*,
>
> Only now we finally got down to writing a letter. In Paris we walked around every day from 9 in the morning until 10 at night[69]—for apart from the city and the museums there was still the police headquarters, which gave us troubles, and the German consulate, where we asked for a transit visa for the return journey etc. After four days we got so tired[70] that we made the decision to postpone the continuation of Paris until the return journey (Urysohn), resp. until eternity (Alexandrov). We have come here the day before yester-

[68] Urysohn and Alexandrov to Brouwer, 29.VII.1924.

[69] With the greatest pains: Paris is even more horrible than I could ever have thought [Urysohn's note].

[70] And Alexandrov had cursed so much and has become so unbearable [Urysohn's note].

day, and it took us a whole day before we could find a quiet place at the coast. [.........] With our best wishes for you and the two ladies

Paul Urysohn Paul Alexandrov
Le Batz (Loire Inférieure), Pension de famille 'Le Val Renaud'.

Alexandrov and Urysohn had in the meantime contemplated Brouwer's invitation to spend a year in Amsterdam. Being exceptionally bright, they could have spent a year almost anywhere, in particular Göttingen must have been on their mind. For any young mathematician that place offered glimpses of mathematical heaven. It says something for Brouwer that they decided to come to Amsterdam. Their curricula vitae and a formal letter of acceptance were enclosed with their letter.

Brouwer answered by return post, reporting about the reception of his talk,[71] and asking Urysohn to add his signature to the preceding letter.

The letter was immediately answered by Urysohn and Alexandrov, and the missing signature was provided.[72] The letter is written in a jocular tone, it closes with the promise to tell Brouwer all about France at the meeting of the German Mathematics Society in Innsbruck.

A week later they sent a picture postcard from the Pointe du Raz,—'Many warm greetings from the place here at the picture, which we climbed and swam around in all directions.'

Returning from Cap Finisterre to Batz they moved into a small cottage at the sea shore, where they alternatingly pursued their research and went swimming. On August 15 and 16 the sea started to get rougher and 'our swims became more and more interesting', as Alexandrov put it. On the 17th Urysohn had thought out the proof of his famous metrisation theorem and he managed to finish to write down the first page of the paper '*Zum Metrizationsproblem*'. At 5 o'clock the two went down for their customary swim.

The dramatic consequences were recorded in Alexandrov's 'Pages from an autobiography':[73]

> When we got into the water, a kind of uneasiness rose up within us; I not only felt it myself, but I also saw it clearly in Pavel. If only I had said, 'maybe we shouldn't swim today?' But I said nothing ... After a moment's hesitation, we plunged into a not very large shore wave and swam some distance into the open sea. However, the very next sensation that reached my consciousness was one of something indescribably huge, which suddenly grabbed me, and this sensation was accompanied by the rather absurd but quite precisely formulated thought: had this wave come to me all the way from Venezuela to no useful purpose here? A moment later I came to myself on the shore, which was covered with small stones—it was the shore of a bay, separated from the open sea by two rocks between which we had had to swim as we made our

[71] See p. 446.

[72] Alexandrov and Urysohn to Brouwer, 4.VIII.1924.

[73] Alexandrov (1980).

way to the open sea. I had been thrown over by a wave, right across these rocks and the bay. When I was on my feet, I looked out to the sea and saw Pavel at those same rocks already in the bay, in a half-sitting position. I immediately swam up to him. At that time I saw a large group of people on the shore. (It was a Sunday, and many people from various places had come to Batz to admire the sea.) After swimming to Pavel, I put my right arm around him above his waist, and with my left arm and my legs I began to paddle to shore with all my might. This was difficult, but no one came to my assistance. Finally, when I was already quite near the shore, someone threw me a rope, but within a few moments I reached land. Then eye-witnesses told me that the same great wave that had thrown me across the bay had struck Uryson's head against one of the two rocks and after that he had begun to roll helplessly on the waves in the bay.

When I pulled Pavel to the shore and felt the warmth of his body in my hand, I was in no doubt that he was alive. Some people then ran up to him, and began to do something to him, obviously artificial respiration. Among these people, there happened to be, as I was later told, a doctor, who apparently directed the attempts at life-saving. I do not know and did not know then how long they continued, it seemed like quite a long time. In any case, after some time I asked the doctor what the condition of the victim was and what further measures he proposed undertaking. To this the doctor replied 'What do you want that I should do with a corpse?'.[74]

As I now remember, the only thought that entered my mind when I heard these words was that the word 'fasse' is the 'présent de subjonctif' form of the verb 'faire' and that our French teacher at my school had often asked us for this form and for the subjunctive in general.

Some more time passed, and I went into my room and finally dressed. (Until then I had remained in my swimming clothes.) Pavel Urysohn lay on his bed covered by a sheet. There were flowers at the head of the bed. It was here that I thought for the first time about what had happened. All my experiences, all my consciousness, with such distinctness and clarity. All this merged into a single awareness of how good, how exceptionally good, things had been for each of us, only about an hour ago.

And the sea raged. Its roaring, its crashing, its bubbling, seemed to fill everything. The next day, I sent telegrams to Brouwer, and to my brother Mikhail Sergeevich, in Moscow, whom I asked to tell the Urysohn family about what had happened. That same evening I received in reply a telegram from Brouwer with the words 'Appelez-moi où vous voulez'. I asked Brouwer to come to Göttingen, where I planned to stop for a few days on my way to Moscow.

The funeral was on 19 August. In the belief that it would accord with the wishes of Urysohn's father, I asked a rabbi to perform the funeral rites. As far as the funeral itself is concerned, I remember the huge number of people who

[74] 'Que voulez vous que je fasse avec un cadavre?'

> came to it, the pile of living flowers on the new grave, and the noise of the sea, which could be heard even in the cemetery. On 20 August I left Batz and after stopping in Paris for a day I arrived in Göttingen on the 22nd, where Brouwer, Courant and Emmy Noether awaited me. Hilbert and Klein asked me to come and see them. This was my last meeting with Klein. He died in the summer of the following year.

The death of Urysohn was a shock for everybody who had known him. Many mathematicians who had met him during his short presence on the European scene of mathematics sent their condolences to Alexandrov.

Brouwer, in particular, suffered from the terrible blow. He had in their all too brief acquaintance come to see Urysohn as a precious gift from heaven, as the predestined person to carry on topology from where he had left it.

The little time Alexandrov, Urysohn and Brouwer were together was spent with intense discussions on Urysohn's recent work (and undoubtedly Alexandrov's work as well). They discussed in particular Urysohn's dimension theory and his big memoir for the Fundamenta. Knowing Brouwer's principles where historical credit and detail were concerned, it was no surprise that considerable attention was paid to these aspects of the paper. The conversations confirmed Brouwer's impression, created by the correspondence; here was the spiritual son he had not even dared to hope for. Indeed, all reports of Urysohn's personality bear witness to a genuine geniality and to an exceptional, sympathetic character.

Long after Urysohn's death, Brouwer expressed his feeling in a moving letter to Paul's father.[75]

> Dear Mr. Urysohn,
>
> I thank you for your kind and trusting letter. I believe I can understand your feelings, precisely because of the deep and almost mystical impression Paul left in my mind. He must have united in a rare way the best from you and from his mother's nature, and indeed in such a mature way, that already during his lifetime his soul almost seemed to dream and to float over the earth. This particular impression put itself, while we were together, alongside my admiration for his considerable mathematical achievements, which I felt for a long time. In particular for the powerful and surprising new life that he infused into the scientific subject topology, cultivated in the past by me.
>
> It almost has the appearance as if there were a transcendental causal connection between his superterrestial state of mind and his short meteor-like corporal existence, and as if death had been for him more a waking up than a falling asleep.
>
> May you, heavily tried father, in awareness and in certainty of the epic beauty of his short earthly course of life, find the solace that I wish you so much with all my heart.

[75] Brouwer to Urysohn Sr., 14.II.1925.

> In case you should want to go this summer to the resting-place of your dear deceased, and I can be of any assistance in making this journey possible, please dispose yourself of me without any reservation.
>
> I remain in warmest sympathy
> Yours sincerely
> L.E.J. Brouwer

Although the paths of Urysohn and Brouwer met only briefly, Urysohn made a lasting impression and Brouwer with his characteristic impetuosity set himself the task to protect the memory and reputation of Urysohn.

11.3 The Viennese Connection

At the time that Urysohn was working out his ideas on dimension, another young man was turning his mind to the same problems. Karl Menger, an Austrian student and son of the famed economist Carl Menger, had the good fortune to be in his first year at the University of Vienna when Hans Hahn joined the faculty.[76] Hahn, born in 1879 in Vienna, studied mathematics in Strasbourg, München and Vienna, where he got his Ph.D. in 1902. After a sequence of short appointments at and visits to Göttingen, Vienna, Innsbruck and Czernowitz he was drafted into the army, where he sustained serious injuries and was subsequently decorated and discharged in 1916.

After a five year term as a professor in Bonn, he returned to Vienna where he stayed until his death in 1934.

Hahn was a versatile mathematician with wide ranging interests. He published in a great number of areas, among others variational calculus, function theory, set theory and topology, real functions, Fourier series and foundations of mathematics. His book '*Reelle Funktionen*' became the standard work for a generation of mathematicians. Hahn's philosophical interests led him to take part in some of the sessions of the Vienna Circle. One of his first actions in Vienna was the introduction of a mathematics seminar for students. The first topic was the theory of curves. Menger fell immediately for this fascinating subject. He started to work out his ideas and within a week presented Hahn with a proposed solution. Encouraged by Hahn, he pursued his ideas further and in June 1921 he handed in a short note with the basic notions of curves, including the notion of one-dimensionality.

Overtaxing himself in a situation where the consequences of the war were still noticeable in the form of lack of heating and proper food, he fell ill and had to spend more than three semesters in a sanatorium. Nonetheless he finished his first paper at the end of 1921. In the preceding fall of 1921 Menger felt so worried about the state of his health and the prospects for future research and publications that he decided to deposit his main results with the Austrian Academy, remembering that his father

[76] Mayrhofer (1934).

once told him that one of the roles of academies was to act as trustees of people's ideas by accepting sealed envelopes and testifying later to their contents and the date of their deposition.[77] Menger's document contained the discussion of a number of basic concepts of topology, their definitions and properties.[78]

A copy, authenticated by a notary, is in the Brouwer Archive. The document contained under the heading I. *On the dimensionality of Continua (Draft)*[79] the following items:

§1. The notion of curve
§2. Theorems about curves
§3. The notion of surface
§4. A continuum is called n-dimensional if D is $(n-1)$-dimensional[80]

Menger's definition of dimension, although equivalent to the one of Urysohn had a very appealing form. It did not make use of any form of separation, but directly made the induction step by reducing a neighbourhood to its boundary. Here is Menger's definition: A set is at most n-dimensional if any of its points has arbitrary small neighbourhoods with at most $n-1$-dimensional boundary. It is a routine matter to define 'has dimension n'.

The note ends with the remark that the definition of curve, surface and dimension were already given by him in April 1921, as could be confirmed by Hans Hahn and Menger's fellow student, Otto Schreier.

The paper 'On the dimensionality of point sets', that was to become the first published account of Menger's theory of curves and dimension, was submitted to the *Monatshefte für Mathematik und Physik*, the Austrian journal for mathematics and physics, and it was read by Hans Hahn, one of the editors and definitely the most competent referee one could wish for. Hahn discovered a mistake which Menger could not repair (for good reasons: Sierpinski had already provided a counter-example), and although Menger in a letter to Hahn[81] suggested a solution by changing the definition of 0-dimensional, the paper was not published in its present form.

At the time of the writing of his paper, Menger was still unaware of Brouwer's dimension paper of 1913. This explains why Menger erroneously ascribed to Brouwer the viewpoint that 'dimensionality is a property belonging to certain point sets derived from the possibility to map them one-one and continuous onto certain other ones'. In the days before the general notion of dimension was made explicit, geometric dimension was considered only in the traditional setting of Euclidean spaces,

[77] Menger (1979), p. 251.

[78] Sealed document No. 778 (1921). Subsequently published in Menger 1929, part 1.

[79] *Über die Dimensionalität von Kontinuen* ('*Zur Theorie der Punktmengen*' in Menger 1929, the change of title is not explained).

[80] D is assumed to be the boundary of the intersection of a sufficiently small neighbourhood with the continuum.

[81] Menger to Hahn, 15.II.1922.

and Brouwer's invariance of dimension must be viewed in this light. Menger's above characterisation would rather apply to Baire's work.

Menger's poor health kept him in the sanatorium until April 1923; roughly at that time he became aware of Brouwer's 1913 paper.[82] Later in the year he found out that Urysohn (who was virtually unknown at the time) had presented a talk at the Marburg conference in September. He hastened to get hold of the Comptes Rendus notes and was just in time to insert a reference to Urysohn in the proofs of his first published paper.

Upon publication of this paper 'On the dimensionality of point sets, part I', he mailed reprints to various mathematicians, and got in exchange a number of reprints from Fréchet, Brouwer, and the Warsaw school. Brouwer obviously was interested because of the close similarity with his own and Urysohn's work.

The first contact between Brouwer and Menger must have been in late February early March 1924, for there is a letter of March 12 in which Menger thanks Brouwer for sending him a reprint of the 1913 paper, adding that:

> In 1921, when I tried to define the notion of curve and of dimension, I was in the first year of my study at the university and I was not aware at all of your paper, dear Professor, in the *Journal f. d. reine und angew. Math.* 142, in which the definition is essentially anticipated.

In June 1924 Brouwer had communicated his own paper 'Remarks on the notion of natural dimension' to the Amsterdam Academy, in which he took the Menger–Urysohn definition into account and showed that it was equivalent to the natural dimension for locally compact metric spaces, a result that was already mentioned in Urysohn's letter of 24.X.1923, as Brouwer pointed out in a footnote. Brouwer sent a copy to Menger, who replied:[83]

> I cannot, dear Professor, thank you enough for your attention that you paid to my little note, and also for your kind letter and the mailing of your paper which I had already read with great interest in the Proceedings of 28.6.1924.

He went on to point out that for wider classes of spaces the MU- and N-dimension were not equivalent.[84]

Menger's interests were by no means restricted to topology, and he shared an interest with his teacher in foundational matters. He told Brouwer that:

> Recently I had to present a talk on research on the foundation of mathematics in a privatissimum of the epistomologist Prof. Schlick. It may well have been the first time here in Vienna that an extensive presentation of intuitionism was offered. The report was followed by a long discussion. It would be very

[82] In my description of the role of Menger in the development of dimension theory, I am relying on Menger's reminiscences, Menger (1979), the surviving correspondence and printed material.

[83] Menger to Brouwer, 13.XI.1924.

[84] MU-dimension = Menger–Urysohn dimension, N-dimension = natural (i.e. Brouwer's) dimension.

fortunate for me to obtain in a few months time instruction from your lectures on these fundamental questions which touch me in my innermost being.

Fate had plunged Brouwer once more into topology; it definitely was not by his wish that he had to divide his attention between his mission in life—intuitionism—and his one-time love—topology. But the sudden appearance, first of Urysohn and Alexandrov and then of Menger left him no choice but to return to the fields of his first glory and to attend to some unfinished business.

11.4 The Scientific Legacy of Urysohn

During his life, cut short by the tragic accident, Urysohn had been incredibly creative and productive. The papers and manuscripts of the topological genius allowed Alexandrov to publish two volumes of collected works of almost one thousand pages.[85] At the time of his death only a small part had appeared in print. We have already come across Urysohn's *Comptes Rendus* notes, and the manuscripts that he had submitted to the *Mathematische Annalen* (p. 399). In addition, at the time of Urysohn's death there was a substantial manuscript in the hands of Sierpinski, the editor in chief of *Fundamenta Mathematicae*.

The personal and scientific impact of Urysohn on Brouwer had been tremendous—a brilliant scientist and a soul mate who had miraculously appeared. Terrible as the shock of Urysohn's death must have been for Brouwer, it was nothing compared with its impact on Alexandrov. He had lost his closest friend, a fellow mathematician who shared his soul with him. Reading the surviving correspondence one gets the impression that Alexandrov had lost the will to live. In one of Brouwer's characteristic, deeply empathetic letters he encouraged and comforted the despairing young man,[86]

> I have received both letters, and I am in my thoughts continually with you. Yet I would not pray, in accordance with your statement, that you will not have a long life. In the first place, as it is not because of objective events, but only for the illumination of our sense of duty and for the sake of strength to bear the trials that are imposed on us, that we may pray. In the second place, because our earthly existence was given us only for the purification of our soul from the original sin of fear and desire, and that it is only in accordance with the time required for the satisfaction of *this* purpose, that the span of life of the righteous is measured.
>
> Just because of that the death of the righteous person has for himself always the characteristic of fulfilment, a release and a salvation, and we should after his death offer him further our love, but not our compassion, in particular not when his transition to death was a light one.

[85] Urysohn (1951).

[86] Brouwer to Alexandrov, 31.VIII.1924.

> And for the mourning surviving friends and relatives the following holds: each sorrow has for the heart that feels it, its ennobling meaning and in the days of sorrow it is often easier than in the days of joy, to become aware of God's presence, because sorrow, to be born in peace, forces dematerialisation. May this also be so for you!

Almost immediately after the heartbreaking message from Alexandrov, Brouwer had decided to see that justice was done to Urysohn's unpublished work. For a man with his meticulous editing habits, this was not just a matter of sending manuscripts to the printer after a cursory inspection of the text. He was known for his punctilious treatment of papers, his own as well as those of others. The page proofs of his own papers that have been preserved show a rich variety of corrections, concerning both the formulation and the mathematical content. Mostly these corrections were of a cosmetic nature, improving the text. It was only natural that he and Alexandrov should act together in this enterprise. Both were more than competent where the content of the papers of Urysohn was concerned; Alexandrov wholeheartedly shared Brouwer's sentiment, for him this work of love for his deceased friend was a matter of course. As guardians of Urysohn's scientific estate there was much to do, sorting out the manuscripts and representing the scientific interests of the deceased. Their first challenge was the monograph-sized paper for the *Fundamenta Mathematica*, the so-called '*memoir*'. In a way this was a touchy project, mainly because the two first-rate topologists who had taken it upon themselves to supervise the editing of this fascinating manuscript were in their own research so close to the subject matter that extra care had to be exercised to keep the necessary distance. For Brouwer the matter was particularly sensitive, as Urysohn's original text contained a rather unflattering characterisation of Brouwer's dimension paper. Brouwer was, however, not the man to adopt the safe procedure, and to back out. He had no doubt at all that he was the right man for the task.

Urysohn's criticism of Brouwer's dimension paper was concentrated in a particular footnote which explicitly claimed that Brouwer in 1913 had failed to establish the right notion of dimension (see p. 415). Urysohn had written the memoir unaware of Brouwer's earlier definition of dimension. He discovered Brouwer's paper just in time to insert a last minute footnote, as appears from a letter to Sierpinski.[87]

The first page proofs of the memoir still contained the old footnote, see p. 415, but the next proof took Urysohn's wishes into account. The formulation, however, is different from the one in Urysohn's letter; a reference to Menger is appended,

> I mention moreover the work of Menger (Monatshefte für Math. u. Phys. 23 (1923)), where the point of departure is more or less the same as the one of my notes cited in the present memoir, and which has only come to my notice in the spring of 1924.

Indeed Urysohn learned about Menger's work, as appears from a letter written in March 1924,[88]

[87] Urysohn to Sierpinski, 27.VI.1924.

[88] Urysohn to Menger, 22.III.1924.

> As it appears, we have almost simultaneously found the definition of dimension: mine I have found in July 1921, presented to the Moscow Mathematical Society in October 1921. Two announcements have appeared in the Paris Comptes Rendus (1922, Vol. 175, pp. 440, 481); the first part of my major publication is at present in print at the Fundamenta Mathematicae.

This, apparently, was the only contact between the two new masters of dimension theory.

As we have seen, p. 409, Brouwer had already contacted Sierpinski about the memoir. In March, uncertain of Urysohn's views, Brouwer had approached the editor in chief of the *Fundamenta Mathematicae*, asking him to keep an eye on possibly unjustified comments on Brouwer's definition of dimension—an unmistakable reference to the footnote. A possible argument between Brouwer and Urysohn had been smoothly averted, when Urysohn had accepted Brouwer's explanation. Unfortunately Urysohn had failed to correct his comments in the memoir, and he had informed Sierpinski in haste, scarcely a month before his visit to Brouwer, that a correction of the footnote was in order.

Once Alexandrov and Brouwer had agreed to edit Urysohn's scientific estate, interested parties had to be informed. In the case of the *Mathematische Annalen* there was no real problem. Brouwer was the editor who handled the papers of Alexandrov and Urysohn, and it was completely natural that the matter could be settled by Brouwer and Alexandrov. As for the *Fundamenta Mathematicae*, the editor had to be informed of the new arrangement concerning the corrections of the proofs. On 11 September Alexandrov wrote to Sierpinski that Brouwer was so favourably impressed with Urysohn that all correspondence on the subject of Urysohn's papers would be handled jointly by the two of them.[89]

It seems that Sierpinski was pleased to accept the arrangement, for Brouwer referred in a letter to information from Sierpinski:[90] the memoir was scheduled to appear before the autumn of 1925 in two volumes of *Fundamenta Mathematicae*, the introduction and the first two chapters in Volume 7, and the rest in Volume 8. He advised Alexandrov to leave the matter alone for the moment. But he also foresaw the possibility of difficulties,

> In the meantime it seems that we should, alas, take into account the possibility that Kuratowski will have on his own authority declared the introduction of Paul's memoir ready for printing (although such an act without the authority of Paul's heirs should appear incomprehensible to me), that thus about note 3, which criticises me, nothing could be done. Finally, the proofs that were sent to me do not look as if they are ready for printing at all; they still contain many annoying printing errors.

Almost all of the following correspondence is concerned with the proofs and corrections. There seemed to have been one long struggle to get the proof sheets,

[89] Alexandrov to Sierpinski, 11.IX.1924.

[90] Brouwer to Alexandrov, 24.X.1924.

to convince Sierpinski to grant more time to insert corrections, both of a material character and of a purely typesetting nature. Sierpinski was probably not happy at all to have two excellent topologists go over the proofs with a fine-tooth comb, and so we see a running battle over access to proofs, the right to insert corrections and the like.

A week after Brouwer's letter Alexandrov tried to get Sierpinski's co-operation,[91]

> Shortly before his death Paul Urysohn has talked at length with Brouwer about his own papers and in particular his *memoir*. In the course of the exchange various questions concerning the final formulation were discussed and joint decisions were taken accordingly. Therefore it is very important that the proof sheets of the memoir, until the moment that they are marked with the decision 'imprimatur!', are read by Professor Brouwer, so that, if it were only in appendices, we can enter such changes as Paul Urysohn would have entered himself, had he been alive.

Without speculating on the possible state of mind of a deceased person, one can say that, if anybody, Alexandrov knew Urysohn's mind; he had worked with him over the past years, and he was in an excellent position to understand Urysohn's work, not just the polished results but also the thoughts and deliberations that had gone before. So it is not unreasonable to trust him in this matter. As for Brouwer, he had a singular gift to fathom a kindred spirit. So there was no pair better equipped for this task than Alexandrov and Brouwer.

The task of supervising the editing of the *memoir* was not an easy one. Printing was in an advanced stage, and apparently the proof reading was done by local staff in Warsaw, so it seemed almost hopeless to expect that the publisher would welcome more proofreading, and hence more corrections.

A week later Alexandrov, who had returned to Moscow, where he was living with Urysohn's family (and actually was thrown out of the apartment together with them), dropped a harmless looking bombshell. He wrote Brouwer that he was about to approach Sierpinski, and he included the text of the letter.[92] In this letter he confessed a painful oversight: although he and Urysohn had together gone over the final version of the text, they had forgotten to change the expression 'se propose a résoudre' to 'résout', see p. 415. As the old formulation contained 'a certain offensive meaning' for Brouwer, a meaning which had escaped them at the time, he had begged Sierpinski to change the formulation 'as required by scientific truth and by the wishes of the author as expressed in his last days'.

In his letter to Brouwer, Alexandrov cautiously expressed his hope that things would work out, 'I believe that the revision of the flawed footnote 7) in the introduction is not totally hopeless.' Brouwer could not draw much comfort from this kind but rather lame reassurance. Some two weeks later he himself wrote to Sierpinski; thanking him for his co-operation, he went on to say that then were a num-

[91] Alexandrov to Sierpinski, 20.X.1924.

[92] Alexandrov to Brouwer, 20.X.1924.

ber of corrections and emendations, made necessary by the exchange of ideas between Urysohn, Alexandrov and Brouwer, shortly before the death of Urysohn—'in a form that only the author could have recognised [...], a form which Alexandrov and I have determined in accordance with the last notes and the last verbal statements of the deceased, for which we accept full responsibility'. He added that one of the corrections concerned himself, and that Alexandrov had written about it.

A complicating factor on Brouwer's side was that he was still recovering from a serious illness. In October he mentioned his health problems, and Alexandrov was worried accordingly.

Brouwer's health was again in a poor state, in letters to Alexandrov cod liver oil is mentioned, and a recommendation of the family doctor to spend the Christmas vacation in the Engadin (Switzerland). Needless to say that Brouwer refused to go, there were too many things he had to do. His ill health no doubt was real, but keeping Brouwer's medical history in mind, one can hardly doubt that Urysohn's death and the subsequent pressure of Urysohn's posthumous publications made things worse. It looked as if his not uncommon nervous breakdowns had surfaced again.

On the thirteenth of October Brouwer replied to an earlier letter of Alexandrov saying that he was still in bed. 'My recuperation progresses smoothly, but not very fast, and my doctor declares moreover that I have to be careful for a long time and that I will have to spare myself. I have not been able to extract a diagnosis with a scientific medical name: he spoke of "flu with complications".'

In a touching letter Alexandrov replied, expressing his worries that the mysterious illness might have been a flu combined with pneumonia, which in his opinion could easily develop into tuberculosis. 'I am very much worried about your weak lungs .... You should therefore not only take care, but also make your food as intensive as possible, to put the whole organism in order, otherwise it will not work. You should take great quantities of butter, eggs, milk, cream etc., and *cocoa*, I think that now meat is indispensable to you. .... Please keep me continuously informed of the news of your health.'[93] The last part of the advice was not self-evident for a vegetarian like Brouwer! Alexandrov's advice to take cod-liver oil ('oleum jecoris Aselli') was, however, dead on target, Brouwer adored any medication that came straight from Mother Nature's drugstore.

Alexandrov's letter also contained a German translation of the poem 'Worüber singt der Wind?' of Alexander Block, a poem that summed up the sad emotions of Alexandrov.

Warte, mein alter Freund, und dulde, dulde,
Das Dulden wird nicht lange dauern, nur schlafe fester;
Es wird ja alles doch vergehn,
Es wird ja niemand was verstehn,
Weder dich verstehn, noch mich,
Noch das, was von dem Winde
Uns vorgesungen ist...

[93] Alexandrov to Brouwer, 20.X.1924.

> So much was sung us by the wind. It was really as if between the lines of life a divine song was sung, intertwined. I recall how we landed at 4 o'clock in the morning for the first time at the Norwegian coast at Christiaansand. The sun had not yet risen, we stood in this cool morning air and we gazed at the rocks of the shore, at the motionless, totally quiet sea, at the blazing red sky. So we stood for a long time, silent, until the sun had come up, and still longer. We did not speak a word, but we both felt in the same way the eternal infinity of nature. But even in daily life, in the most ordinary aspects, this other thing was present, that what we felt and understood, and what separated us. This haze of eternity ... it surrounded our joy, thus perhaps it lasted so briefly ...

It was actually neither the first nor the last time that 'tuberculosis' was mentioned, nor the first time that Brouwer's vegetarian diet was deplored. On 16 February 1921 Van Eeden wrote in his diary: '—I went home with Brouwer. He said that he had been ill, and I understood that it was the beginning of tuberculosis. I loved him so much, he was friendly and so warm-hearted. If only he would eat meat.' Van Eeden's fears may have been somewhat exaggerated, but he was a medical man and he definitely knew how a tuberculosis patient looked! The conclusion one may safely draw is that Brouwer's ascetic way of life and the nervous tensions were taking a heavy toll.

On 20 October Brouwer wrote Alexandrov that his health was improving and that he hoped to resume his lecturing the next Friday. 'I feel as if I have come slowly to the light from a dark abyss', and on 3 December he sent a card to Alexandrov with a reassuring message, 'My health is now really satisfactory; I live indeed most carefully (the cod liver oil is faithfully taken).' That Christmas Brouwer stayed home, his family doctor had wanted to send him to Engadin, but Brouwer argued successfully that the backlog of tasks he would find on returning from Switzerland was a greater danger for his health than a Christmas in Laren. Brouwer was grateful for the attention of his young friend. There was also good news concerning the grant. The Rockefeller Foundation had approved the arrangements for Alexandrov's stay in Amsterdam, where he would receive $180 per month from the grant.

The exchange of letters must have been quite frequent in the fall, although only a few have survived. There is a note of Brouwer's in which a few points of a letter of 7 November to Alexandrov are written down; one of these says 'vision in my dream of Urysohn: "Yes, yes, yes, yes,—yes, of course" about my plan to have a new typesetting of the imperfect text'. Far from being a cheap trick to impress Alexandrov, it shows that the memoir occupied a prominent place in his thoughts.

A few day earlier he had written to Sierpinski, stressing the importance of allowing more time for a proper proof reading.[94] After thanking him for the proofs of the introduction, he expressed his hope that

> there would possibly be time to make changes and corrections in the introduction, that had become necessary by the exchange of ideas and that had

[94] Brouwer to Sierpinski, 4.XI.1924.

> taken place in the last weeks of the life of the author, between him, his friend Alexandrov and me. In this hope I will send you shortly the corrected proofs of the introduction in the form that only the author could recognise at the day of his death, a form which Alexandrov and I have established from the last notes and the last verbal statements of the deceased, and for which we accept the full responsibility.
>
> Among the changes there is one which concerns me personally, and about which Alexandrov has written you a letter, which he will send through me as an intermediary and which I will get registered myself to-day.
>
> In the proofs which I will return to you I have done my best to correct typographic errors which were left in fairly large numbers and some of which are not all that innocent.

On 17 November Brouwer again turned to Sierpinski, explaining that certain corrections could only be made in the final proof.[95] There were for example, he wrote, 'passages [. . .] thoughtlessly written at the time, and recognised as absurd by the author shortly before his death—which, if they were printed in their actual form, would gravely damage his scientific memory . . .'. For this reason he begged Sierpinski to suspend all printing, to send all proof sheets to him, and to have new proof sheets made after his corrections, including those pages which had gone through their final printing. Any necessary costs would be gladly paid by him. One would not have to be extremely sensitive to resent a letter of this sort. No editor likes to be told in a patronising way what to do. The next day Brouwer reported to Alexandrov that he had written in unmistakable terms to Sierpinski about the poor editing and correcting. He urged Alexandrov to support him. Thus Alexandrov duly informed Sierpinski of his surprise and displeasure, to find out that the introduction and the first chapter had been printed *without enabling him to read* the text. The fact that Kuratowski had read the text did not mean that there was therefore no reason for Brouwer and himself to read the proofs. He forcefully appealed to Sierpinski to redress the situation.[96]

Sierpinski reacted swiftly and firmly.[97] Urysohn, he wrote, had asked for one correction (see p. 416) *if possible*, and he clearly did not expect to see any proofs. 'Therefore the editor of the Fundamenta Mathematicae considered himself entitled to start the printing of the memoir of Paul Urysohn, without sending any proofs, except for that single correction. Paul Urysohn never proposed any more modifications of his memoir.'

What had started as a simple duty to honour Urysohn's memory, and a service to a well-known mathematical journal, thus started to take the form of another conflict. That was exactly how Brouwer saw it.[98]

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[95] Brouwer to Sierpinski, 17.XI.1924.

[96] Alexandrov to Sierpinski, 24.XI.1924.

[97] Sierpinski to Brouwer, 25.XI.1924.

[98] Brouwer to Alexandrov, 21.XI.1924.

> Now it also occurs to me, that for the fortifying of our position (it indeed is like a real war; our position assumes the role of a fortified camp, in the wall of which, as a consequence of your fateful letter to Sierpinski, a breach was shot, which we have to close with all our might) it were very important to add the following passage …

In view of the position taken by Sierpinski, Brouwer thought it best that Alexandrov would make it clear to Sierpinski that Brouwer and he would take no important decision without mutual consultation, and that hence it was obvious that Alexandrov had to see the proofs. Moreover, Sierpinski's claim that Urysohn had not asked for proofs seemed in conflict with the fact that the first galley proof of the memoir had reached Urysohn in Le Batz. Anyway, would any author of a memoir of more than three hundred pages voluntarily forego the proof reading?

Should it come to the worst, then Brouwer could, as an 'ultimate medicamentum heroicum, withdraw in name of the family, the paper from the Fundamenta'. That would probably teach the editors a lesson.

In December another complicating factor arose—Brouwer, with his refined instinct for possibly hidden insults, had discovered that the uncorrected version of the manuscript implied a reading with drastic personal consequences. Namely, Urysohn had characterised Brouwer's notion of separation as 'the old notion of cut' (*coupure*). This, he was certain, would immediately tell every reader that Brouwer (i) missed the essential point in his definition of dimension, and (ii) tried to cover up his lapse. To what extent Brouwer's fears were justified is hard to say. Urysohn's formulation was unfortunate, and went back to the period before his exchange with Brouwer, and indeed an acute reader might doubt Brouwer's explanation. So, Brouwer foresaw a disaster, unless he could repair the formulation. In view of the fact that Urysohn had fully accepted Brouwer's view, and in view of the discussions in Blaricum (where, one may be certain, Brouwer once more pointed out the true history of his oversight), there is little doubt that the unfortunate passage was overlooked in July 1924. But for Brouwer the dimension matter began to border on an obsession. In his letter to Alexandrov he forcefully argued his case.[99] He spoke of 'Sierpinski and Kuratowski tainting Paul and the two of us'. Somehow the matter was settled, for on 15 January 1925 he wrote 'The peace with the Warsaw people, also towards me, has been completely re-established.'

Peace or no peace, the end of the problems had not been reached. On February 17 Brouwer informed Alexandrov that a new disaster had taken place. Brouwer had returned the galleys 8 and 9 to Fundamenta Mathematicae, but the printing shop had in the meantime, 'on account of the small stock of type (just enough for a few galley sheets)' destroyed the type for galley 8, 'as Sierpinski informed me in a further most polite and apologetic letter'. So galley 8 had to be set again, and—whereas galley 9 was impeccably set—'contained, of course, again the normal quantity of Kuratowski errors'.

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[99] Brouwer to Alexandrov, 24.XII.1924.

The last surviving letter to Sierpinski on the matter of editing Urysohn's memoir is the one of May 24, 1925. In it Brouwer conveyed the wish of the family of Urysohn to preserve the original manuscript of chapters 4 through 6, which they considered 'the culminating part of the scientific activity of the deceased', as 'a relic'. So he begged Sierpinski, in the name of the Urysohn family, to cede that part of the manuscript. A copy 'carefully made under my direction (in which moreover some posthumous wishes of the deceased were taken into account)' was offered in exchange. The correspondence does not say if Sierpinski honoured the request of the family.[100]

Urysohn's memoir appeared without further problems in the Fundamenta Mathematicae. It was a substantial piece of topology, all together 236 pages; the paper dealt with a rich variety of topics, including dimension theory and indecomposable continua. The modern reader, accustomed to Bourbaki-style presentation would find the paper a bit long winded here and there, but this opus of the twenty-five-year-old topologist contains the fundamental theorems of dimension theory.

The editing of Urysohn's memoir took a heavy toll on Brouwer, both emotionally and clerically. In addition to this, Brouwer was also handling the newer papers of Alexandrov and Urysohn for the *Mathematische Annalen*, complete with correcting formulations, streamlining proofs, etc. All of this took place at the time that he was occupied with the preparation and publication of his finest intuitionistic work: the bar theorem, fan theorem and the continuity theorem.

Did Brouwer actually 'falsify the text', as Menger later suggested? It is highly unlikely; after all he did not act without Alexandrov's approval, and Alexandrov had been present and taken part in the discussion between Brouwer and Urysohn in July 1924. Those who have known Alexandrov know that he would not be participant in shady practices of this sort, and he was certainly not one to be pressured into such an act.

It is an altogether different question whether it was wise to take part in the editing. Although his motives were totally unselfish, and based on his admiration and love for this sudden topological prodigy, less kind tongues could easily spin a tale of text manipulation. It is a pity that Brouwer and Alexandrov did not add an editorial comment to the Memoir.

Brouwer's factual influence on the text is hard to assess. With his editorial routine, he probably did some polishing of the formulations, and added an occasional useful reference. The most damaging accusation would of course be that of systematic self-promotion. Without access to the original manuscript there is little one can say. Brouwer is mentioned a number of times in footnotes (re references), but these are all harmless 'useful information for the reader'. There is no reference beyond generally recognised facts. Yet, the editing of the memoir would become the subject of some most unpleasant altercations.

The correspondence with Alexandrov contains regular references to a planned stay in Amsterdam. In the letter of February 17 arrangements for Alexandrov's

[100] So far no copies of the manuscript in whatever form have been found.

stay in Amsterdam are mentioned. The Rockefeller Foundation had put a grant at Alexandrov's disposal and it had taken some ingenuity to get permission for this stay from the Soviet authorities. Brouwer left no stone unturned, wrote letters and asked his friend and colleague Mannoury to write a letter to the Soviet authorities. Mannoury duly did so, he asked the Volkscommissariat for education to grant Alexandrov permission to come to Holland to collaborate with Brouwer. He signed his letter as 'Professor of Mathematics at the University of Amsterdam' and 'Member of the K.P.H.' (Communist Party of Holland).[101]

Brouwer had also enlisted the help of his colleague Pannekoek, the astronomer, who had an international reputation as a Marxist theoretician. Pannekoek, in turn had asked a Dutch engineer, Rutgers, to put pressure on the authorities.[102] The organisation of Alexandrov's stay in Amsterdam began to take shape. For Brouwer it was important to arrange the details, as he wanted to make the most of not only Alexandrov's presence, but also that of a number of other visitors, to wit Menger, Vietoris, Kerékjártó and Wilson.[103] A special set theoretical lecture series would only be worthwhile if all the visitors could attend. Brouwer strongly advised his visitors to come and live in Laren or Blaricum, he preferred to have them around, so that he could organise meetings and seminars at home, without travelling to Amsterdam. In his letter of March 15, 1925 he discussed the housing arrangements with Alexandrov; it had apparently been agreed that Alexandrov was going to live in Laren. Was it not a good idea, he asked, if Menger also found himself rooms in Laren, or the neighbouring towns of Hilversum or Bussum. 'I hesitate to make this suggestion myself to him, because I would accept thus certain imponderable obligations, from which I want to safeguard myself as long as I don't know Menger personally.' Alexandrov would not be bound by any obligations if he arranged temporary housing for Menger. Brouwer would, of course, if necessary look for suitable rooms for Menger. One should not read too much into these lines, Brouwer had always been extremely sensitive in his contacts, being completely in the dark with respect to Menger, he felt he could not commit himself as he did towards Alexandrov.

Menger, in fact, had already contacted Alexandrov; in an undated letter from Alexandrov to Menger an earlier letter of Menger is mentioned. Alexandrov's letter must have been written in the first months of 1925 (perhaps even earlier). It is an enthusiastic welcoming of a fellow topologist, expressing admiration for the parallel efforts of Menger and his friend Urysohn in the area of dimension theory—'of course you will be cited in the 'Mémoire sur les multiplicités Cantoriennes', and the relevant footnote has already been sent to the editors; Brouwer and I are completely of the same opinion on this matter'. He looked forward to work with Menger in Brouwer's seminar, believing that this would be useful to both of them. In fact, he bade Menger to come to Holland a bit later in order to make their stays overlap for a

[101] Mannoury to Lunatscharsky, 4.XI.1924.

[102] Rutgers had offered his expert services to the Soviet government, and in time he became an appreciated courier of Lenin, with quite significant assignments for the promotion of the world revolution.

[103] Wilfrid Wilson was a British topologist who took his doctorate with Brouwer in 1928.

longer period. He looked forward, he wrote, to make the acquaintance with Menger from whom he expected many interesting things.

Menger had entered Brouwer's life through correspondence, and Brouwer had immediately recognised Menger's contribution to dimension theory. Indeed, in his sequel to the dimension paper of 1913[104] he referred to Menger (1923) and spoke of the *Menger–Urysohn dimension* (*MU*-dimension).

In 1925 things were brightening up for Brouwer and his Urysohn project. Alexandrov had already informed him in January that the peace with the Warsaw mathematicians had been completely restored. Of course, Brouwer had enough to grumble about, but he was no longer hampered by editorial interference. In the spring his topological admirers started to arrive, and the stage was set for a period of intensive study and research in modern topology.

[104]Brouwer (1924e). Brouwer (1924g) is an abridged version in which Menger is not mentioned.