CHAPTER 2

Max Dehn as Hilbert's First Star Pupil

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Making His Mark in Göttingen

After graduating from Hamburg's Wilhelm-Gymnasium in 1896, Max Dehn began his studies in Freiburg, an idyllic city in the southwestern state of Baden. Why he chose such a distant provincial university, rather than the much nearer one in Kiel, remains a mystery. It seems unlikely that his decision had anything to do with its program in mathematics, since Freiburg had only two professors, Jacob Lüroth and Ludwig Stickelberger, neither of whom was particularly prominent. Their careers are nevertheless interesting from the standpoint of academic networking, which would also play an important role in Dehn's career.[1]

When Dehn arrived in Freiburg, Lüroth was in his mid-50s and Stickelberger about ten years younger. Jacob Lüroth was, like his compatriot of the same age Max Noether, an algebraic geometer. Lüroth and Noether both grew up in Mannheim, where they became good friends, sharing a strong interest in astronomy. Later, they studied in nearby Heidelberg, only 20 km from Mannheim. Eventually, they gravitated to the school of Alfred Clebsch, who taught briefly in Giessen, before assuming Riemann's chair in Göttingen in 1868. The Clebsch-network continued to play a major role in German mathematics long after the master's sudden death at age 39 in November 1872. Clebsch had been a student of Otto Hesse, whose final academic station was the technical college in Munich, where he taught up until his death in 1874. Hesse's professorship was then transformed into two chairs that were offered to Felix Klein and Alexander von Brill, both former protégés of Clebsch. In 1880, Klein took a new professorship in Leipzig, and Lüroth then gained the vacant chair in Munich. A few years later, another former Clebsch pupil, Ferdinand Lindemann, left Freiburg for Königsberg, which opened the way for Lüroth to succeed him.

As these examples illustrate, personal connections played a major role in this small German world of higher mathematics. Had Alfred Clebsch lived to a ripe old age in Göttingen, Max Dehn would have found a very different atmosphere there. As it happened, though, by the 1890s Klein had emerged as the central figure for mathematics in Göttingen, supported by David Hilbert, the rising new

[1] Stickelberger was a Swiss mathematician, who had studied in Heidelberg and then Berlin, where he took his doctorate under Karl Weierstrass in 1874. When he first came to Germany in 1867, he crossed paths with Lüroth, who had just begun his career as a private lecturer in Heidelberg, so undoubtedly they got to know one another during the next two years.

star who arrived in 1895 from Königsberg.[2] Lacking any documentary sources from this time in Dehn's life, one can only guess why he chose to leave Freiburg after only one semester and then continued his studies in Göttingen. Dehn's daughter Maria recalled hearing that her grandfather, Dr. Maximilian Dehn, had wanted Max to follow in his footsteps by studying medicine. If so, Max senior may well have had some connections in Freiburg and, if so, perhaps he urged his son to study there.[3] Were that the case, then Max perhaps tested those waters and soon decided he wanted to change his course of study, presumably with his father's permission, assuming of course that this is what happened. All that can be said with certainty, however, is that Max decided to transfer to Göttingen shortly before his father died in April 1897. If he had gotten to know the two mathematics professors in Freiburg, he may well have spoken with Lüroth about his future ambitions. Assuming such a scenario, one can easily imagine that Dehn would have received a friendly tip, namely, that he should pack his bags and take up studies in Göttingen, where Klein had been teaching since 1886.

By the late 1890s, however, Klein's career as a creative mathematician was already behind him. Not even his lecture courses and seminars, which had long attracted students from both near and far to Göttingen, took on great importance next to his other activities. After acquiring Hilbert, a native of Königsberg who was Germany's most promising young talent, Klein became increasingly involved in organizational projects, in particular reform movements in technology and pedagogy [Tobies, 2021]. Students continued to flock to Göttingen, but the more gifted or ambitious among them were drawn to Hilbert rather than Klein. When Dehn arrived in Göttingen in the summer semester of 1897, Hilbert was 35 years old and on the cusp of new breakthroughs that would make him famous in the world of mathematics. Klein was offering an introductory course on differential equations. He and Hilbert were also teaching a joint seminar on function theory, and Hilbert was also giving a course on algebraic invariant theory.

This was a rare instance when Hilbert returned to the field that had dominated his research interests from the late 1880s right up until 1893. Whether Dehn attended remains unknown, but the contents of the course are known from an elaboration (*Ausarbeitung*) prepared by Sophus Marxsen, later published in English translation [Hilbert, 1993]. Marxsen went on to write his dissertation under Hilbert on a topic in invariant theory, graduating only a few months after Max Dehn. It was common practice in Göttingen for Klein and Hilbert to choose one or more students to prepare an "official" set of lecture notes, which other students could then consult in the *Lesezimmer*, the special library for mathematics that Klein installed when he arrived in 1886. To be so chosen was considered a high honor, as Max Born recalled in his autobiography [Born, 1975, 81] – he was given the task of writing up Hilbert's lectures from his 1904 course on the number concept and quadrature of the circle.[4] Although, Dehn never wrote about his student days – he was not inclined to attach much significance to the events in his own life – we do know that he was tapped to prepare the official elaboration for Klein's lectures on differential equations.

[2]Hilbert and Hermann Minkowski studied in Königsberg under Lindemann, who took his doctorate under Klein shortly after Clebsch's death.

[3]Maria Peters Dehn to Constance Reid, August 17, 1972, Texas University Archives.

[4]Born also prepared the *Ausarbeitung* for Hilbert's lecture course from 1905 on "Logische Prinzipien des mathematischen Denkens."

A much later source offers a hint as to what probably happened during Dehn's very first semester in Göttingen. One of the older students at this time was Otto Blumenthal, who would go on to become managing editor of the journal *Mathematishe Annalen* and a major figure within the web of academic power that Klein and Hilbert had begun to spin [Rowe, 2018b]. Blumenthal came from a Jewish family in Frankurt and was a close friend of the astronomer Karl Schwarzschild, whom Klein would bring to Göttingen in 1901. Klein and Hilbert always had their eyes out for new talent, but many times they tasked their assistants and protégés with finding bright and ambitious young men (and later women) among the students. In Blumenthal's case, though he came to be known as Hilbert's first doctoral student, he was actually discovered by Arnold Sommerfeld, who was Klein's assistant during the mid-1890s. Like Hilbert, Sommerfeld was a native of Königsberg, but his years as a *Privatdozent* in Göttingen were decisive for launching his spectacularly successful career. His first professorial appointment came in 1897, when he left Göttingen to join the faculty at the Mining Academy in Clausthal, located in the nearby Harz Mountains region. Hilbert then inherited Blumenthal, whose interests had been strongly formed by Sommerfeld, namely applications of mathematics to engineering problems.

Thus, when Dehn arrived in Göttingen in the summer of 1897, Sommerfeld was on his way to nearby Clausthal as Klein's leading disciple. Nevertheless, Sommerfeld continued to teach courses in Göttingen during that summer and the following winter semester; the topics were ordinary and partial differential equations and the calculus of variations. It appears, however, that Dehn never studied with Sommerfeld. Blumenthal, who spent the summer semester of 1896 in Munich studying under Alfred Pringsheim, returned to Göttingen and began assuming his new role as a "senior" student, and he enjoyed the growing recognition. He enrolled in Klein's course, alongside Dehn, whom he must have befriended from the start.

In any event, it was surely on Blumenthal's advice that Klein asked Dehn to prepare the official *Ausarbeitung* for his lecture course. How difficult this proved to be no one can say, but Blumenthal later wrote about the problems he had trying to work out the connections between Klein's geometric approach, based heavily on Sophus Lie's work on differential equations that admit infinitesimal transformations, and the formal analysis needed to represent these ideas [Rowe, 2018b, 308]. Shortly before Max Dehn's sixtieth birthday, Otto Blumenthal briefly recalled how their friendship began. He wrote on 11 November 1938, "I cannot claim to have much instinct, but back then I had a good one when I picked you out as my lead fox (*Leitfuchs*)" [Rowe/Felsch, 2019, 494]. Presumably Klein had asked Blumenthal to scout for an appropriate student in the course, and the latter chose Dehn as his "lead fox" at that time, more than forty years earlier.

Within the culture of the German universities, the *Fuchs* already had a long tradition, though this term eventually came to be identified with a novice among the older members of a fraternity. In an older guidebook to student life in Göttingen, one finds the following explanation:

> Fuchs is the name of a student in his first half year. The name is not at all inappropriate, because the young person who arrives with highly exalted ideas about a university and with the good teachings and rules of life of his concerned parents feels worried among the students; believes he sees a celebrity in everyone who

> meets him; feels noticed by all people; consequently expresses anxiety in posture, gait, and expression – in fact has many similarities with a fox. After a few weeks these anxious creatures disappeared, and towards the end of the six months usually behave in the opposite way; they often want to fly before they have wings and so fall into a different sort of ridiculousness. Regardless of this, one recognizes them for foxes. [Wallis, 1813/1981, 102].

Blumenthal surely had nothing at all like this in mind, but he knew from personal experience that a new arrival could profit immensely from the friendly help of older students. Unless Dehn came from Freiburg with personal recommendations from Lüroth and/or Stickelberger (which was not entirely unlikely), then meeting Otto Blumenthal was a tremendous stroke of luck.

Very few sources survive that offer clues like Blumenthal's "lead fox", although a post-doc from those early days, Ernst Zermelo, also befriended Dehn. Zermelo later visited Dehn and his family in Hamburg in 1900, and he afterward wrote an interesting letter expressing his thanks for that experience. Putting these pieces together, it seems that Dehn came to the attention of Klein through Blumenthal, who was about to come under Hilbert's wing. We have no detailed knowledge of the courses Dehn took with Hilbert and others, so piecing together a picture of his mathematical education involves a good deal of guesswork. What can be asserted with assurance is that Dehn's course work in Göttingen focused on mathematics, physics, and chemistry, the three fields he chose for his doctoral examination. In mathematics, he took courses offered by Hilbert, Klein, and Arthur Schoenflies, associate professor of geometry. His instructors in physics were Woldemar Voigt, Eduard Riecke, and Eugen Meyer, from whom he studied theoretical, experimental, and technical physics, respectively. The chemistry courses he took were taught by Otto Wallach and Wilhelm Kerp.

Regarding the general atmosphere in Göttingen during the five semesters Dehn studied there, we are fortunate that Otto Blumenthal recorded his own vivid recollections of that time [Rowe, 2018b, 307–310], parts of which surely applied to Dehn's situation. Blumenthal noted that back then the number of younger students, especially those who planned to become teachers, was relatively small, a factor that benefited the older ones, many of whom held doctoral degrees or came from foreign countries. In Blumenthal's opinion, "the lectures in Göttingen were ideally suited for highly motivated young people at the time. Those students without higher aspirations were less likely to get their money's worth, and they were also not well respected." Not surprisingly, it helped to be bright and ambitious, as the competition was ferocious. For some, this had an unhappy ending, because "whoever wanted to be recognized needed to reach higher than they would by their natural inclination," and Blumenthal recalled "quite a number of people who let themselves get carried away and later, in practice, fell down" [Rowe, 2018b, 309]. Blumenthal's professors were nearly identical with those under whom Dehn studied, with the exception of Blumenthal's first mentor, Arnold Sommerfeld. Both of them, for example, took courses with Göttingen's senior physicist, Woldemar Voigt, an experience Blumenthal recalled with real regret:

> It was characteristic of our generation that we only thought about the exams at the last moment; initially, we only wanted

> to learn as much mathematics as we possibly could. I believe that, compared with the lecture courses, the exercise sessions at that time were decisively inferior. This was especially the case for students who did not manage to grasp that practical experience was a necessary thing. This was particularly apparent in physics, which is the only point where I see an actual, and purely self-inflicted, shortcoming in my own training. We all took the physics tutorial taught by Voigt, but we treated this so marginally that nothing came of it. It was just seen as a counterpart to the exercises in descriptive geometry, even though Voigt took the matter very seriously and exerted considerable effort and patience with us. [Rowe, 2018b, 309]

Beginning students were offered problem solving sessions in the proseminars, which were generally taught by Schoenflies. Dehn probably took at least one proseminar, though this is uncertain. However, he definitely did some coursework with Schoenflies, who taught the lion's share of the courses in geometry. Thus, Dehn may well have gotten his first exposure to non-Euclidean geometry from a 2-hour course offered by Schoenflies during Dehn's very first semester in Göttingen. The semester following, he had the opportunity to take a course with him on space curves and curved surfaces, whereas on Saturday mornings Schoenflies conducted exercises in geometrical drawing. In the summer semester of 1898, Schoenflies offered a proseminar on set theory, a novelty at the German universities. During his many years in Halle, Georg Cantor never taught a lecture course on set theory. The first such course, taught by Dehn's friend Ernst Zermelo, was only offered in the winter semester of 1900/01. Zermelo was then a *Privatdozent* in Göttingen working closely with Hilbert.

During Max Dehn's student days, Klein and Hilbert co-taught the seminars. Over the course of Klein's long career, his seminars served as a training ground for many aspiring young mathematicians. As was customary at the German universities, the students rather than the instructors did the lecturing, and Klein kept protocol books of all the presentations in his seminars going back to his first semester as a professor in Erlangen in 1872 (when he was 23 years old!). During Dehn's five semesters, Klein and Hilbert often held a seminar together. In the summer semester of 1897 the subject was function theory, whereas for the two semesters following the seminars dealt with topics meant to complement the material covered in Klein's year-long 4-hour lecture course on mechanics. Since testing and grades played no role at the German universities, the success of this system depended on its stringent selectivity – only a very small percentage of young males graduated from a classical Gymnasium – as well as a deeply ingrained work ethic. Seminars were, in general, quite open-ended, but Klein typically presented a set of topics at the beginning of the semester and assigned these to the participants at their first meeting (or even beforehand). Blumenthal remembered these seminar topics as extremely difficult, in fact far too difficult for the participants, as he wrote:

> For practical exercises in mathematics there was, besides the proseminar, only the seminar by Klein and Hilbert. I learned a tremendous amount from the lectures I presented there. Never did I have to work so hard and with such intensity as when preparing these lectures. I have the feeling that the topics were

> consistently too difficult. This was particularly evident when listening to the lectures of others. Only on rare occasions did I actually understand something. I don't think these seminars helped steer students' own work onto the right track. One had too much to do to learn about the topics, which were usually too difficult for someone to accomplish something on their own. [Rowe, 2018b, 309]

Blumenthal's mathematical training was heavily influenced by Sommerfeld, Klein, and to some extent by Schoenflies. After Sommerfeld's departure, he attended Hilbert's lecture course on number theory and joined in on Hilbert's memorable "number fields outings" (*Zahlkörperspaziergänge*). These took place soon after publication of his "Zahlbericht" [Hilbert, 1998]. Blumenthal's mathematical education was thus very stimulating and diverse, but hardly systematic, and he later realized some of the more glaring gaps in his knowledge of the fundamentals.

> I am very astonished that during the entire course of my studies I never developed a sense for mathematical rigor. That this elementary knowledge came to me so late was certainly due to an overemphasis on receptive learning. In other cases (irrational numbers) the difficulties never became clear to me psychologically; it took me a very long time to recognize the necessity of Dedekind cuts. Whether this was a personal weakness, or perhaps due to the overly deductive presentation of the elementary material, I can no longer say. [Rowe, 2018b, 309–310]

In reflecting back on what drove him to mathematics, Blumenthal dropped a number of important hints, starting with the sheer abundance of courses offered and the extraordinarily varied nature of the subject matter. Through Sommerfeld, he also gained a smooth transition to physics and the excitement of learning about all the things people everywhere were working on. Still, the biggest single factor, he thought, was "due to the reading room and the friendly atmosphere that prevailed there. There people got infected by what each of them was working on."

After completing his doctorate in 1898, Blumenthal took the customary precaution of preparing for and passing the state examination (*Staatsexamen*) for teachers, which qualified him to teach mathematics, physics, and chemistry at the *Gymnasien*. Many of the teachers at these demanding secondary schools held doctorates, as higher education was esteemed very highly in those days. Moreover, the number of university positions was so few and the normal waiting time to attain one so long that few families wanted to risk betting their sons would eventually be called "Herr Professor." Even Hilbert, who had to wait seven years before gaining his first appointment, took no chances and passed the *Staatsexamen* immediately after taking his Ph.D. in Königsberg. The next step for Otto Blumenthal was the *Habilitation*, which normally involved writing a second thesis and then submitting this to a receptive university faculty. Few took this step without the prior support of a faculty sponsor, in this case Hilbert, who encouraged his first doctoral student to spend the academic year 1899/1900 in Paris. He did not need to prod him, though, as Blumenthal spoke French nearly perfectly and was exceedingly fond of the French capital city. By the time he returned to habilitate in Göttingen, his friend Max Dehn had already moved on.

Dehn surely shared many similar experiences as a student in Göttingen, and yet his career took off on a very different track than those taken by Sommerfeld and Blumenthal, who began as fellow colleagues at the Institute of Technology in Aachen. Dehn would also later teach at a *Technische Hochschule* (TH); from 1913 to 1921 he held a full professorship (*Ordinariat*) at the TH Breslau, though he hardly felt at home there. Teaching mathematics to engineers was rarely a labor of love for someone like Dehn, whose life revolved around pure mathematics. Like others from this time, though, he had very broad training and scientific competences. Alongside mathematics, physics, and chemistry, he had also mastered several foreign languages. Over the course of his career, Dehn taught a wide range of mathematics courses, and some of his doctoral students wrote dissertations on topics far removed from their mentor's favorite research fields. This breadth of knowledge has to be kept in mind when considering Max Dehn's mathematical works, which reflect his burning interests as a young mathematician. Hilbert's own courses covered an incredibly wide range of topics, which accorded with Klein's vision that students should be exposed to the full gamut of mathematical knowledge [Tobies, 2021]. Before announcing their courses, the mathematicians would meet to discuss their plans for the coming semester, but the only real purpose was to achieve some balance in the content and level of the offerings. In later years, when the number of students was far greater, more attention was given to standard course offerings, though even these could vary a great deal depending on the whims and tastes of the instructor.

During the five semesters Dehn spent in Göttingen, Hilbert taught invariant theory and complex function theory (SS 1897); number theory, confocal curves and surfaces, and a special course on the number concept and quadrature of the circle (WS 1897/98); differential equations, Fourier series, and number theory (SS 1898); mechanics, theory of determinants, elements of Euclidean geometry (WS 1898/99); differential calculus, group theory, and calculus of variations (SS 1898). Although few documents have survived that shed any light on what Dehn learned from Hilbert, he arrived at the very time when his mentor was riding the cusp of major new developments in the foundations of geometry, a research field he transformed in a dramatic fashion after 1899. The events of the next few years would soon catapult Dehn into a role he surely never imagined for himself. He became Hilbert's star pupil, though in a field far removed from invariant theory and algebraic numbers, the areas in which Hilbert had first staked his reputation. How this came about was largely a matter of unforeseen circumstances, luck, and of course, talent.

Hilbert's "Grundlagen der Geometrie"

Before this time, Hilbert had firmly established his reputation as the era's leading authority in both invariant theory and the theory of algebraic number fields, two formerly distinct disciplines that had now been brought together through his work. But then came a most unexpected turn of events. Many years later, in [Blumenthal, 1935, 402], Otto Blumenthal recalled the buzzing chatter among the students when they read Hilbert's announcement for a 2-hour course on "Grundlagen der Euklidischen Geometrie" [Hilbert, 2004, 185–406], which he offered during the winter semester of 1898-99. Blumenthal and the older students, those who had been accompanying Hilbert on weekly walks, had never heard him talk about geometry, only number fields. Little did they realize that Hilbert had been contemplating the foundations of geometry ever since his years as a Privatdozent in Königsberg, as evidenced by recent historical studies.[5]

Hilbert's lecture course that semester surprised them even more, for in it he sought to lay out the fundamental structures underlying Euclidean geometry as few before had ever imagined. In his classic *La Géométrie*, Descartes had found a simple way to arithmetize geometry in the plane by introducing a unit of length. One can then add, subtract, multiply, and divide line segments by appealing to elementary properties of proportional lengths in similar triangles. Hilbert referred to Cartesian geometry as a segment arithmetic based on the real numbers (whereas Descartes had only a vague notion of number systems). Unlike Descartes' approach to arithmetization, Hilbert based his theory on a set of axioms for abstract number systems. In this way, he was able to derive segment arithmetics based on two central theorems of projective geometry, the theorems of Pappus and Desargues. Dehn very likely took not only this course but also the parallel 4-hour lecture course on projective and descriptive geometry taught by Schoenflies. If so, he surely would have encountered these theorems in their more familiar form. During the twilight of his career, Max Dehn took pleasure in teaching them to his students at Black Mountain College (see Chapter 12).

The following spring, acting on a request from Klein, Hilbert revised this material and presented it in the original version of his famous "Grundlagen der Geometrie" [Hilbert, 1899/2015], published in a two-part *Festschrift* commemorating the unveiling of the Gauss-Weber monument. In Göttingen the partnership between the mathematician Carl Friedrich Gauss and the physicist Wilhelm Weber was already legendary in June 1891, when Weber died at age 86. In fact, the collaboration between these two famous scientists had only lasted for six years, as Weber lost his position in 1837. He had the misfortune (or belated honor) of belonging to the "Göttingen Seven" – a group of professors who had the temerity to protest the annulment that year of the liberal constitution in Hanover. Soon after his death, plans began for erecting a monument commemorating this celebrated duo, the Gauss-Weber Denkmal, which was unveiled on 17 June 1899.

Two of those who came to Göttingen to attend the ceremony on that day were Georg Cantor and Hermann Minkowski. Cantor was curious to learn more about the status of the "arithmetical axioms" (his quotation marks) in Hilbert's *Festschrift* [Meschkowski and Nilson, 1991, 399]. What he may have heard about these no one can say, but Hilbert definitely spoke about this very topic with Minkowski, who alluded to it in a thank-you letter, written one week after the festivities in Göttingen:

> Dear friend,
>
> Now that I've returned to the reality of Zurich, the wonderful days in Göttingen seem today like a dream to me, and yet one can as little doubt their existence as that of your $18 = 17 + 1$ axioms of arithmetic. I felt especially comfortable in your warm home, and I've been reporting here repeatedly with pleasure about the exciting time I spent there with you. ...

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[5]See, in particular, [Toepell, 1986], [Hilbert, 2004], and the commentaries by Klaus Volkert in [Hilbert, 1899/2015].

> Anyone who experienced these days in Göttingen will hardly get over their astonishment as to the liveliness in the Göttingen mathematical circle, and at the moment this is entirely due to you. Spending time in such air gives a person higher ambitions and an impulse to more intensive creativity. [Minkowski, 1973, 116–117]

Hilbert's approach to Euclidean geometry was by no means an altogether original vision. Italian geometers had been promoting axiomatic methods well before him (he himself alluded to the work of Giuseppe Veronese), and the German geometers Hermann Wiener and Friedrich Schur sought to establish an approach to the foundations of geometry that did not depend on continuity principles [Wiener, 1891], [Wiener, 1893]. Max Dehn's first and most lasting contribution to this program was entirely in that same spirit, as were some of his later publications in topology. In their more systematic investigations, Hilbert and Dehn sought to exhaust the substantive results that could be derived from more elementary axioms before invoking continuity principles or properties of the full real number continuum. Richard Dedekind had famously derived the latter in *Stetigkeit und irrationale Zahlen* [Dedekind, 1872]. He did so by using so-called "Dedekind cuts" to complete the rational numbers by using a kind of transcendental construction for introducing irrational numbers on a rigorous basis. Hilbert sought to avoid such a construction, but the goal he announced in his public lecture "On the Number Concept" [Hilbert, 1900a] was essentially the same. By introducing his completeness axiom, he hoped to give a direct proof that the standard properties of the real numbers were free from contradictions.

One might perhaps wonder why Hilbert chose to avoid Dedekind's principle as a more intuitive way to establish continuity. Part of the answer seems to be that he strove for a simple *non-constructive* approach to this problem. In [Hilbert, 1900a] he referred to genetic methods for grounding arithmetical systems, which he set in contrast to the axiomatic method he found preferable. One should also mention Hilbert's longstanding interest in the Axiom of Archimedes, in particular in demonstrating its independence by way of non-Archimedean geometries.[6] It was precisely in this direction that Max Dehn made an important new contribution to clarifying the foundations of geometry. As Hilbert's tenth student,[7] Dehn belonged to a small group who wrote on a topic in geometry, though only Dehn's dissertation [Dehn, 1900a] had a lasting importance. In it he took a significant step beyond Hilbert by exploiting his model for a non-Archimedean geometry.[8]

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[6]As Volkert points out, had Hilbert decided to invoke Dedekind's principle as an axiom, he could have simply derived the Axiom of Archimedes from it [Hilbert, 1899/2015, 179–180]. In the seventh edition of *Foundations of Geometry* (*Grundlagen der Geometrie*, 1930), Paul Bernays introduced a weaker form of Axiom V.2, namely linear completeness. Based on this, one can then prove the old completeness axiom as a theorem. In Chapter 2, Bernays then proves the relative consistency of Cartesian geometry by means of the model for $\mathbb{R}$ based on Dedekind cuts [Hilbert, 1971, 31–32].

[7]According to MacTutor, Hilbert had 71 doctoral students over the course of his career in Göttingen, a number that easily dwarfs that of any other mathematician of the period, even Klein, who mentored some fifty.

[8]Dehn's axiom system was identical to the one in the original *Festschrift* [Hilbert, 1899/2015], where the congruence axioms appear in group 4 and the parallel postulate forms group 3; continuity (group 5) consists of the Archimedean axiom alone. Beginning with the second edition, [Hilbert, 1903], groups 3 and 4 appear inverted and the completeness axiom is added to group 5.

For Dehn's dissertation topic, Hilbert posed the question whether one could draw any conclusions regarding the sum of the angles in a triangle without invoking either the parallel postulate or the axiom of Archimedes. Dehn was able to derive two important new cases that differ sharply from the three classical ones: in elliptic, hyperbolic, and Euclidean geometries, the angle sum in triangles is, respectively, greater than, smaller than, or equal to two right angles. In what Dehn called a non-Legendrean geometry, there are infinitely many parallel lines through a given point to a given line, but unlike hyperbolic geometry, the sum of the angles in a triangle *exceeds* two right angles. Likewise, he uncovered the case of a semi-Euclidean geometry, where the angle sum is the same as in a Euclidean geometry. Thus, within the scope of the axioms in the original groups 1, 2, and 4 of Hilbert's *Festschrift*, Dehn was able to unveil two new types of plane geometries (for details, see Chapter 3).

Hilbert was elated by these new results. Three days before Dehn's oral exam, he wrote to his former mentor Adolf Hurwitz about Dehn's work: "Since you have some interest also in the foundations of geometry, I'd like to inform you of a dissertation by one of my best students Herr Dehn, whose results have delighted me" [Epple, 1999a, 230]. In 1902 a French translation of his *Grundlagen der Geometrie* came out, to which Hilbert appended a fairly lengthy account of Dehn's dissertation results.[9] When he later found time to modify his *Festschrift* for the second edition, Hilbert again used the opportunity to underscore the importance of Dehn's results [Hilbert, 1903, 23–24].

Max Dehn had not yet reached his twenty-first birthday when on November 8, 1899 he passed his doctoral examination in Göttingen. On the long list of those who wrote their dissertations under Hilbert, two others had already written on topics in foundations of geometry. These were Michael Feldblum from Warsaw and Anne Lucy Bosworth, a native of Rhode Island who held a master's degree from the University of Chicago; both passed their oral exams in July 1899. Doctoral candidates in mathematics typically chose their secondary fields from among astronomy, physics, or chemistry. In Dehn's case, he was examined by the theoretical physicist Woldemar Voigt and the chemist Otto Wallach.[10] Each probed his knowledge for 30 minutes, whereas Hilbert spent a full hour asking questions on a wide range of topics in geometry and foundations of mathematics. The protocol lists these areas: elements of descriptive geometry, algebraic surfaces and curves, line- and sphere-geometry, differential geometry: surface curvature, geodesic lines, then axioms of arithmetic, and finally elements of set theory. Hilbert's verdict read: "The knowledge of the candidate proved to be reliable and very versatile and is satisfactory in every respect." This may not sound like lofty praise, but in those days German professors rarely deviated from sober language. In fact, Hilbert's verdict was clear, as Dehn took his degree *summa cum laude*.

Hilbert's Paris Lecture

In late 1899 Hilbert received an invitation from the organizing committee to deliver a plenary address[11] at the forthcoming international congress in Paris. Soon

[9] Universitätsarchiv Göttingen (UAG) Phil.I.185.b.

[10] The protocol from Dehn's oral exam can be found in UAG Phil.I.185.b.

[11] In the final program, Hilbert was not among the plenary lecturers due to organizational confusion.

thereafter, he wrote to Minkowski asking for his advice about an appropriate theme for his talk. "Most alluring," his friend thought, "would be the attempt to look into the future, in other words, a characterization of the problems to which the mathematicians should turn in the future. With this, you might conceivably have people talking about your speech even decades from now. Of course, prophecy is indeed a difficult thing" [Minkowski, 1973, 119–120]. Minkowski's letters to Hilbert sparkle with witty remarks and telling observations, but at no time did he strike a more resounding chord than here.

Hilbert's first two problems were to become particularly famous, far more than the third, which had captured Max Dehn's attention even before the Paris ICM opened. The first concerned a conjecture raised by Georg Cantor, who claimed that the cardinality of the real number continuum was precisely the same as that of the smallest uncountable well-ordered set. Hilbert raised this as an open question, but his remarks made clear that he believed Cantor's continuum hypothesis was both true and also provable. His second Paris problem stemmed from his own quite different approach to the continuum, which he sought to characterize axiomatically. One year earlier, when he unveiled his new axiom system for the real numbers in [Hilbert, 1900a], he claimed one could prove its consistency directly. He now called for someone to provide that proof. Hilbert's third problem called for a proof that the cut and paste methods used to prove the equality of plane rectilinear figures do not suffice for handling polyhedra in space (see Chapter 3 for details).

Considering that Hilbert's speech would later become famous as one of the most influential ever delivered, it is fascinating to read an eyewitness account written around the time of the event itself. Charlotte Angas Scott from Bryn Mawr College was one of seven women who attended the Paris Congress. Still, she had an important role to play, namely to report on what transpired for the American Mathematical Society. Her detailed and yet colorful and opinionated account in [Scott, 1900] focused on more important talks and neglected those of lesser interest.[12] Charlotte Scott had attended the first ICM in Zurich, which she found far more successful than the second from an organizational standpoint. Under normal circumstances, Paris would have provided an excellent venue, but in 1900 this meant hosting a scientific meeting in the middle of a world's fair. The *Exposition Universelle*, held between April and November, attracted some 50 million visitors; little wonder that the mathematicians had difficulty finding one another. Scott had plenty of advice to impart when it came to making improvements for the future, and she was hopeful that the next ICM, tentatively planned for 1904 in Baden-Baden, would be more successful than the last. Indeed, she emphasized how important such gatherings could be just for the sake of someone's mental health during an era when mathematicians otherwise had so few opportunities to meet. In that connection, her general remarks about what such an event should aim to achieve are well worth quoting:

> The mathematician who is in any degree a specialist is in general rather solitary in the average college – he would have been better

[12]Scott had studied at Girton College, Cambridge from 1876 to 1880, and afterward taught there for four years while studying under Arthur Cayley. Since Cambridge did not grant doctoral degrees to women, Scott took hers from the University of London in 1885. That same year she came to the US, where she became one of the eight founding members of Bryn Mawr's faculty. By 1900 she had established herself as a prominent figure within the American mathematical community.

> off in Noah's Ark, for at the worst there would have been two of a kind. For his mind's health it is well that he should occasionally be thrown with those of kindred interests; it is well too that he should be made to feel the unity of mathematics. [Scott, 1900, 75–76]

Scott also noted that unlike Zurich, where about half the talks were delivered in either French or German (English and Italian were also permitted, but seldom spoken), in Paris nearly all the lectures were delivered in French; Hilbert was one of the few who spoke in German.

When she came to that lecture, Scott wrote more about Hilbert's general remarks than the individual problems he presented. The most striking among these was also the most significant, namely, his claim that in mathematics there is no *ignorabimus*. In Hilbert's opinion, every well-posed problem in mathematics was capable of being solved, whether in the positive or negative sense. This optimistic assertion marks the high point in his address, and one might well say he took this with him to his grave. Indeed, his gravestone carries the famous motto: "Wir müssen wissen, wir werden wissen" (We must know, we will know). In Paris, he recounted how mathematicians had recently resolved two of the fundamental problems bequeathed upon them by the ancient Greeks: the status of the parallel postulate in Euclidean geometry and the possibility of squaring the circle. These nicely illustrated what he considered to be characteristic of mathematical problems in general: each and every question was capable of being answered with finality. For Hilbert, this belief offered not only a strong psychological support; it even had the quality of a moral imperative: "the conviction in the solvability of every mathematical problem is a powerful incentive to the worker. We hear within us the perpetual call: there is the problem, seek its solution. You can find it by pure reason, for in mathematics there is no ignorabimus" [Hilbert, 1900, 298].

Judging from Scott's report as a whole – and she was an astute observer – we surely should not imagine that the audience to whom Hilbert spoke was on the edge of their seats. Nothing she wrote could be read as confirming Minkowski's prediction that Hilbert's address would turn out to be the event of the congress, though his text with its famous list of 23 problems would gain many avid readers in the years ahead. In 1902 the text of "Mathematische Probleme" became available in a French translation produced by Léonce Laugel.

Max Dehn did not attend the Paris ICM, but he may well have read Hilbert's text before it came out in print. In any event, his correspondence with Hilbert makes clear that they had discussed the third Paris problem, which Dehn set about to solve before the ICM convened. Little is known about the events in Dehn's life for many months after he took his doctorate in November 1899, but in all likelihood he lived at home in Hamburg and spent much of his time there working on Hilbert's third problem while preparing to complete the *Staatsexamen*. In the fall of 1900 he accepted an offer tendered by the geometer Friedrich Schur, who taught at the Karlruhe Institute of Technology, a *Technische Hochschule* (TH) in Baden. Dehn became his new assistant during the next academic year 1900/01.

One can gain a glimpse of Dehn's life after graduation from a letter written by his friend Ernst Zermelo, whose name is today synonymous with axiomatic set theory (the Zermelo-Fraenkel system, often abbreviated ZF). During the semester break from mid-March to mid-April 1900, Zermelo visited Dehn and his family in

Hamburg and he later wrote to thank Max for the delightful days he spent with them.[13] From his letter, dated 25 September 1900, we learn that Dehn spent some time traveling in Norway that summer; Zermelo was curious to know how far northward he had gone on his trip. Ernst Zermelo was seven years older than Max Dehn. A native Berliner, he took his doctorate under Hermann Amandus Schwarz with a dissertation that extended Weierstrass's approach to the calculus of variations.[14] After visiting Dehn in Hamburg, Zermelo began his teaching career in the summer semester with a 4-hour course on elliptic functions.[15] In his letter, he wrote very openly to Dehn (with whom he was on the familiar "Du" basis) about the loneliness he felt in Göttingen and how much he missed his own relatives. Those feelings came to him after experiencing the warm atmosphere in the Dehns' home in Hamburg, a city he got to know for the first time.

Zermelo remarked that the Hilberts were quite delighted with their sojourn in Paris, no doubt in large part because they stayed in the same hotel with the Minkowskis. This was the conveniently located Hôtel St. Pétersbourg on the rue Caumartin, where Minkowski's oldest brother Max often stayed. As a scientific event, though, Hilbert found the Paris ICM to be very disappointing. Zermelo also reported on various other encounters with mutual Göttingen friends, including Otto Blumenthal. Several were returning from their summer vacations, preparing for the new semester, which would commence in mid-October. Zermelo complained about the time he needed to prepare for his lectures, though without mentioning that he had decided to teach a 2-hour course on set theory. Since he had gravitated into Hilbert's circle, one can easily imagine how Zermelo now felt inspired to tackle the first Paris problem, Cantor's continuum hypothesis. In the meantime, Dehn was hard at work on Hilbert's third problem.

Dehn Cracks Hilbert's Third Paris Problem

One month after the Paris ICM ended, the DMV held its annual meeting in Aachen with Hilbert as chair. He reported on German participation in Paris and noted that the DMV should begin work on preparations for the next ICM, which would take place somewhere in Germany in 1904. This Aachen conference was otherwise fairly uneventful with only sixteen lectures on the program. Max Dehn attended, but he was not on the program of speakers. Having missed the chance to speak with Hilbert in Aachen, Dehn wrote to him from Hamburg on 24 September to ask whether Hilbert had any news about potential positions for the coming academic year. In all likelihood, Hilbert had promised to discuss this with Sommerfeld in Aachen, as Dehn referred to the latter in his letter. The matter was now urgent, as he had only one week to decide whether to accept an offer from the TH Karlsruhe. This was a post-doctoral position offered by the professor of geometry Friedrich Schur, which meant teaching the tutorial that supplemented the lecture course in descriptive geometry. Schur, who had been a protégé of Felix Klein in Leipzig

[13] Dehn Papers, Briscoe Center for American History, University of Texas at Austin.

[14] In 1894 Zermelo became Max Planck's assistant and began pursuing a career in mathematical physics. So he was heavily immersed in physics when he came to Göttingen in 1897, thus around the time Dehn took up studies there. Two years later he submitted his habilitation thesis on vortex motions on the surface of a sphere.

[15] Zermelo also expressed disappointment that his work was going so slowly, in particular his efforts to finalize his paper on vortex motion, which was eventually published in [Zermelo, 1902], [Zermelo, 2010/2013, II: 300-463].

during the 1880s, was very impressed with Dehn's dissertation (he mentioned this in a letter to his friend Friedrich Engel).[16]

Apart from this more immediate concern, Dehn's letter to Hilbert also contained a very noteworthy piece of information. In it, he briefly described his partial solution to the third Paris problem, which he almost surely knew about from the time of Hilbert's lecture course. Moreover, Dehn wrote about this as if it were a progress report, referring to "my tetrahedron work." This letter would thus appear to confirm that he and Hilbert had already spoken about this, either before or during the time Hilbert was composing the final text for his Paris lecture. Hilbert's third problem aimed to show that, unlike areas of plane rectilinear figures, the theory of volume for polyhedra requires continuity assumptions, such as the so-called principle of exhaustion employed in Euclid's *Elements* (for details, see Chapter 3). In his formulation of this challenge, Hilbert referred directly to an impossibility proof based on properties of tetrahedra. The concluding passage notes that the problem would be solved if "we succeeded in specifying two tetrahedra of equal bases and equal altitudes which can in no way be split up into congruent tetrahedra, and which cannot be combined with congruent tetrahedra to form two polyhedra which themselves could be split up into congruent tetrahedra" [Hilbert, 1900, 302].

Dehn's initial result concerned decomposition alone, and in his letter to Hilbert he mentioned two examples of tetrahedra that cannot be decomposed into a finite number of congruent pieces and then used to construct a prism of equal volume. This, he stated, was the case both for a regular tetrahedron as well as one with a dihedral angle formed by three perpendicular planes. One month later, on 27 October, Hilbert submitted this paper for publication in the *Nachrichten* of the Göttingen Scientific Society [Dehn, 1900b]. No sooner had Dehn returned the corrected proofs in early November than he realized he could modify his argument to prove the general result allowing for the addition of congruent pieces as well (he called this *egalité par soustraction*, though why he preferred this French term seems unclear). Writing from Karlsruhe on 12 November 1900, he felt somewhat annoyed with himself that he had already sent off the proofs: "I'm sorry that I didn't finish this matter two weeks earlier." He then asked Hilbert whether he might write this up as a supplement to his first paper or simply include it in the projected paper he planned for *Mathematische Annalen* [Dehn, 1900b]. Hilbert evidently preferred the second alternative, but a note appears at the end of [Dehn, 1900b, 354] indicating that the impossibility of *egalité par soustraction* would appear in the forthcoming paper in the *Annalen*.

Dehn was probably hoping to habilitate at a German university, rather than starting his career as a geometer at a *Technische Hochschule*, even one as esteemed as the Karlsruhe TH. Although one of the most prestigious in Germany, the Karlsruhe Technical College followed the general developmental pattern of many others. Founded in 1825 by the Grand Duke of Baden as a polytechnical school modeled in some ways on the École Polytechnqiue in Paris, it only gained the full structure and status of a *Technische Hochschule* in 1885. Although the institution had already since 1865 gained the right to habilitate new faculty members, this was seldom exercised in mathematics before 1900. In 1899, Kaiser Wilhelm II, acting as King of Prussia, decreed that henceforth all *Technische Hochschulen* in Prussia would have the right to confer doctoral degrees, a major step in elevating their status to

[16]Friedrich Schur to Friedrich Engel, 27 October 1900, Engel Nachlass, Giessen.

a level comparable with the universities. Baden then followed suit the very same year.

Geometry had a particularly strong tradition in Karlsruhe, owing to the long tenure of Christian Wiener, who was professor of geometry from 1852 to 1896, succeeded thereafter by Friedrich Schur. Many positions in geometry were established at the polytechnical institutes, where descriptive geometry was taught to large classes of engineers and architects. Schur's previous assistant, Martin Distelli, was particularly adept in this field, having studied under Wilhelm Fiedler at the ETH in Zurich. Fiedler's drawing courses were famous, and Distelli became known as a leading expert on kinematic geometry, including gearing mechanisms. This hands-on approach to geometry had great appeal for Felix Klein, whereas Hilbert was far more interested in abstract studies based on axiom systems. Both aspects had a strong appeal for Max Dehn, however. In any event, no suitable alternative position was available, and so Dehn accepted the offer from Karlsruhe, where he spent just one year before transferring to Münster. Since he was looking for the opportunity to habilitate, an unlikely possibility in Karlsruhe, this first appointment led to a dead end.

Dehn's First Years in Münster

Dehn's decision to transfer to the Philosophical and Theological Academy in Münster was without doubt due to the support of its senior mathematician Wilhelm Killing. Although Killing, like Dehn, was a leading expert on foundations of geometry, today he is remembered above all for his pioneering work on Lie algebras [Hawkins, 2000]. Killing studied in Berlin under Ernst Eduard Kummer, Karl Weierstrass, and Hermann von Helmholtz. He was a deeply religious Catholic and, like Weierstrass (who recommended him for the post), he taught for a good decade at the seminary Collegium Hosianum in Braunsberg (today Braniewo).[17]

Killing's decade in Braunsberg was followed in 1892 by his appointment as professor of Mathematics in Münster, a city famous for a singular event in European history. It was in Münster's Old City Hall (*Rathaus*) in 1648 that delegations met to sign the Peace of Westphalia ending the Thirty Years War.[18] The traditionally Catholic city of Münster was also the site of the last university founded within the Holy Roman Empire, which was already in its death throes when Napoleon dissolved it in 1806. A decade later, after the Congress of Vienna redrew the map of Europe, the Catholics living in the Rhineland became citizens of Prussia, whose rulers were Lutherans. The Prussian government founded a new university in Bonn in 1818, while transforming the university in Münster to a Philosophical-Theological Academy with just ten professors. This institution gradually grew, however, and during Dehn's years there it regained its former status as the Westfälische Wilhelms-Universität, its present-day name.

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[17] During this time, Killing and his wife entered the Third Order of Franciscans. The Collegium Hosianum was originally founded in 1565 as a Jesuit college by Polish Cardinal Stanislaus Hosius and soon thereafter became one of the most important centers of the Counter-Reformation in Europe. After the partition of Poland in 1772, Braunsberg became a part of the Protestant Kingdom of Prussia, which suppressed the Society of Jesus. The Prussian government closed the Collegium and converted it into a Gymnasium Academicum, later called Lyceum Hosianum.

[18] The adversaries once again recognized a form of religious tolerance enshrined in the earlier Peace of Augsburg of 1555, according to which each prince had the right to determine the religion of his own state, whether it be Catholicism, Lutheranism, or now some form of Calvinism.

Max Dehn was the first mathematician to habilitate in Münster. During his first years there, the Prussian ministry elevated the academy to a university. The normal procedure when applying for habilitation was for candidates to submit a post-doctoral thesis to the faculty along with other documents attesting to their scientific attainments and potential. One or more faculty members would then report on the candidate's qualifications, and if these were judged positively the formal procedure could commence. This usually required submitting three topics for the colloquium, which began with a lecture on the topic chosen by the faculty, followed by discussion, during which candidates might be questioned on quite wide-ranging mathematical matters. In Dehn's case, he submitted his forthcoming paper on the third Paris problem [Dehn, 1902] as his post-doctoral thesis, which was evaluated along with his earlier work by Wilhelm Killing.

As a true expert on foundations of geometry, Killing clearly appreciated the significance of Dehn's accomplishments like few others among his contemporaries. In his report, he briefly alluded to the classical theories of Euclid for treating the sizes of plane rectilinear figures and polyhedra, highlighting how the latter required the so-called method of exhaustion based on continuity properties. Killing further noted that a number of geometers had discussed the need to prove that the theory of volume for stereometric figures required infinitesimal methods, though no significant progress had been made on the problem, despite recent efforts. This state of affairs had then led Hilbert to call attention to this outstanding problem in his noteworthy speech at the ICM in Paris, about which Killing commented:

> Thus the mathematical world was very much astonished when only a few months later a young mathematician, Dr. Max Dehn, who had recently graduated summa cum laude in Göttingen with an outstanding dissertation, solved this problem. His fundamental ideas for proving this were communicated to the Göttingen [Scientific] Society by Hilbert himself, along with a number of examples of polyhedra having equal volume but which cannot be decomposed into congruent pieces.
>
> This work [Dehn, 1900b] also shows, however, that the proof is by no means simple and that the author did not come upon it merely by a lucky accident. On the contrary, one must recognize that it took true geometrical talent, along with goal-oriented discipline and energetic determination to put this problem to rest.

Killing then went on to discuss how Dehn's post-doctoral thesis (*Habilitationsshrift*) [Dehn, 1902], which was about to be published, went beyond [Dehn, 1900b], in particular by demonstrating that one cannot obtain an elementary theory of volume even by extending two given polyhedra of equal volume, i.e., by attaching equi-decomposable polyhedra to them. This rules out a theory of volume analogous with the theory of area developed by Hilbert in [Hilbert, 1899/2015]. The concluding passage in Killing's report then gave this assessment of the new work:

> One will rarely be in the position to commend the work of a young mathematician to the same degree as the present treatise

> of Dr. Dehn, not only by virtue of the importance of the topic but also the excellence of the solution.[19]

A similar enthusiasm for the candidate can be found in Killing's report on the colloquium, which took place on 31 October 1901. The topic for Dehn's lecture was set theory, a field of research that only a handful of specialists would have felt competent to speak about. As mentioned above, Ernst Zermelo had only begun to study Cantor's theory of sets around this time. The main purpose of such a colloquium lecture was to gauge whether the candidate could present an understandable account of a research field somewhat outside their own special interests. In this case, the chosen topic was even far beyond the ken of most mathematicians, whereas Dehn had to address a general academic audience. Nevertheless, Killing came away most impressed by the way Dr. Dehn

> knew how to pick out the most important results from the multilayered material on set theory as found in numerous publications that have been strewn about and which lack all systematic order, ordering these by way of a transparent unifying principle, and presented in a clear and elegant spoken form. Accordingly, the lecture demonstrated complete command of the subject matter in this restricted, but important branch of mathematics, thereby making evident an outstanding talent for teaching, as was unanimously acknowledged afterward in the private meeting among the faculty.

Killing opened the public discussion following Dehn's lecture by asking him to describe the historical background that led Cantor to formulate the fundamental ideas of set theory. In the course of answering, Dehn sketched the proof for one of Cantor's fundamental results. After this, he was asked to compare the pros and cons of the two main approaches to complex function theory due to Riemann and Weierstrass, as well as to comment in this connection on the theorem of Mittag-Leffler. Killing then asked Dehn about the justification for complex numbers as well as other hypercomplex systems, before inquiring about the different methods for proving the fundamental theorem of algebra. Finally, he wanted Dehn to discuss different approaches to synthetic geometry. The candidate then characterized Poncelet's methods before giving a detailed comparison of the methodological differences between Steiner's works and those of Staudt. In all of these areas of pure mathematics, Killing attested that Dehn possessed "not only broad and firm knowledge but rather, for his age, an altogether astonishing penetration of the subject matter." Still, the colloquium was not yet over: another colleague asked about the significance of Euclidean geometry, and two physicists wanted to know about the significance of quaternions for their discipline. These questions, too, Dehn answered to the satisfaction of all concerned.

One should add that a candidate for habilitation who got as far as the colloquium stage could virtually count on gaining the *venia legendi*, the official title allowing one to teach at a German university. It almost never happened that a

[19] Copy of Killing's report from the petition of 5 November 1901 submitted by the Dean of the Philosophical Faculty to the Minister of Education, in Dehn's personnel record, Westfälische Wilhelms-Universität Münster.

colloquium proved so disastrous that this right was denied.[20] Nevertheless, the performance of a candidate could seriously affect their standing in the faculty. In this respect, Max Dehn clearly got off to a remarkably good start. Probably few would have imagined, least of all Wilhelm Killing, that a decade later he would still be a lowly *Privatdozent* in Münster. It would be a long and difficult waiting game.

As mentioned above, Hilbert appended a fairly lengthy account of Dehn's dissertation results to the French translation of his *Grundlagen der Geometrie*, which in the meantime had elicited several different reactions (see the commentary by Klaus Volkert in Chapter 5 of [Hilbert, 1899/2015]). By early 1903, Hilbert was preparing the second German edition, which would be the first to appear as an independent publication. Not surprisingly, he enlisted Dehn's support for the proofreading, as he would do in the future.

Like the original *Festschrift*, this book was published by Teubner in Leipzig, which employed a large staff. Proofreading, typesetting, and printing were all closely linked, and it was not uncommon for papers and books to go through several rounds of proof correction before publication. Thus Dehn received the galley proofs one sheet (*Bogen*) at a time, each corresponding to 16 printed pages. These technical details help explain what had happened when Dehn wrote from Münster to Hilbert on 5 February 1903 about a problem in the text concerning the completeness axiom, which first appeared in the French edition. Dehn explained that he had already sent back the first *Korrekturbogen*, so he no longer had the precise wording in front of him. He thus paraphrased it (correctly) from memory as asserting that one could not add new elements to the system without contradicting one or more of the prior axioms. Dehn then wrote:

> I now believe that this formulation could lead to misunderstandings. It should evidently mean that one cannot add anything to the system without invalidating one of the earlier axioms, *insofar as one does not alter the existing order or congruence relations*: a point lying between two others before the extension remains so after the extension, segments and angles that were earlier congruent to others, remain congruent. If this stipulation is not made, then it is easy to see that no geometry satisfies the completeness axiom.[21]

Dehn went on to describe how one can easily extend a given geometrical system by changing the order and congruence relations. He then continued:

> I do not know if this condition is immediately clear or whether it is perhaps on account of this open to formal attack. If one were to introduce it explicitly, then the axiom loses its philosophical character, in that it now requires that completely definite geometrical properties (congruence and order) must be fulfilled. Beyond that, the condition is too stringent: one only needs to require that order and parallelism are not disturbed. Then one

[20] During the Nazi era, on the other hand, political factors loomed very large. Max Deuring, a favorite pupil of Emmy Noether, was not allowed to teach in Göttingen, and the same fate befell Dehn's former student, Ruth Moufang, in Frankfurt. In such cases, the candidates were allowed to habilitate but were nevertheless denied the *venia legendi* as they were deemed unfit to teach German students.

[21] Dehn to Hilbert, 5 February 1903, Nachlass Hilbert 67, SUB Göttingen.

> sees very clearly the close relationship between this axiom and that of Dedekind.

Hilbert afterward added an explicit remark explaining that the extensions ruled out by the completeness axiom were those that maintained order and congruence; the passage is a nearly verbatim rendering of the quotation above from Dehn's letter [Hilbert, 1903, 17]. Regarding Dedekind's method of achieving continuity by means of cuts in the set of rational numbers, Hilbert made no mention of this in the text, though he did emphasize that the Axiom of Archimedes, V.1 in his system, was a linear axiom, like Dedekind's, whereas VI.2, Hilbert's completeness axiom, was not. Paul Bernays would later prove, however, that only a linear version of completeness is needed in order to derive the full completeness axiom as a theorem. His proof was published in the seventh edition [Hilbert, 1930, 31-32]. This new version of the theory focused on linear properties, which led back to Dedekind, and in fact Hilbert explicitly introduced Dedekind cuts in order to prove that his system of axioms with linear completeness was relatively consistent by appealing to Cartesian analytic geometry.

Dehn would continue to advise Hilbert on matters concerning foundations of geometry, but in the years ahead he took his first steps into a related, but far larger realm, namely the geometry of space. The path he followed is described in greater detail in Chapter 4, whereas here we take up key threads that go back to Hilbert's first two Paris problems, both of which surfaced in dramatic fashion at the International Congress that convened in Heidelberg in 1904. There, Max Dehn had a front-row seat as a witness to a major turning point in the history of set theory and foundations.

ICM in Heidelberg 1904

Hilbert's lecture at the Heidelberg ICM was on the foundations of logic and arithmetic. Although nowhere near as grandiose as the address he delivered in Paris four years earlier, he nevertheless adopted the same self-assured tone. Speaking not only for himself but for all experts in the field, he picked up where he had left off with his second Paris problem:

> While we are essentially in agreement today as to the paths to be taken and the goals to be sought when we are engaged in research on the foundations of geometry, the situation is quite different with regard to the inquiry into the foundations of arithmetic; here investigators still hold a wide variety of sharply conflicting opinions. [Hilbert, 1904, 174]

This remark barely hinted at the more recent controversies that had arisen in connection with certain paradoxes in naive set theory. Hilbert and Zermelo were well aware of these, though both were confident that the problems they raised could be overcome. In the meantime, Hilbert proposed to establish the consistency of the axioms for arithmetic by simultaneously developing the laws of logic and arithmetic. This approach has often been regarded as the initial step toward Hilbert's proof theory based on finitist principles, which he and Paul Bernays developed during the 1920s and 1930s.

Reading Hilbert's text, which he republished along with [Hilbert, 1900a] in numerous editions of *Grundlagen der Geometrie*, one might imagine that his lecture in Heidelberg must have stirred up a good deal of excitement and discussion. Hilbert was surely counting on such a reaction, so he must have been disappointed that his audience was preoccupied by what they had just heard. For on this occasion, Hilbert was upstaged by the speaker who preceded him, the the Hungarian mathematician Julius König. His topic, in essence, was Hilbert's first Paris problem, though he took dead aim at Cantor's continuum hypothesis (CH), rather than speaking vaguely about it, somewhat as Hilbert had done four years earlier. Instead, König caused a sensation by answering the problem in the negative: he presented what seemed to be an airtight "proof" that CH was false, indeed, that the cardinality of the continuum was *greater than* any aleph [Moore, 1982, 86–88].[22] This proof was based on technical results that included a formula from Felix Bernstein's recent dissertation. Had König's argument been correct, this would have had a devastating impact on Cantor's whole theory of transfinite numbers, which was predicated on the belief that all (consistent) sets could be well ordered. What Cantor, Hilbert, Felix Hausdorff, and the other experts in the audience witnessed on this occasion was without doubt a dramatic event.

Max Dehn may well have been sitting next to his friend Ernst Zermelo when they heard König speak. Following the Heidelberg Congress, a storm of controversy broke loose over Cantor's theory, partly caused by logical paradoxes as well as problems arising from ill-defined sets. Even more controversial, however, was the argument Zermelo used to prove Cantor's claim that one could always establish a well ordering for any set whatsoever, however large it might be.

Arthur Schoenflies was not only present when König spoke, he was also part of a small group of mathematicians who left for vacation in Switzerland immediately after the Congress ended [Schoenflies, 1922].[23] By chance, this group – Hilbert, Hausdorff, Schoenflies, and Kurt Hensel – ended up staying together in the same hotel in Wengen, a mountain village in the Bernese Oberland. Cantor was also vacationing with two of his daughters in Wengen as well, but at a different hotel. Yet, since the other mathematicans' main topic of breakfast conversation was König's "proof," he was more than eager to come over and join them. Schoenflies mainly recalled how Cantor was entirely convinced that König's proof was somehow faulty, and it seems likely that he and others suspected there was a problem with a result König took from Bernstein's dissertation. Cantor supposedly even joked that he had more faith in the king (König) than in his ministers, presumably meaning Cantor's former student, Felix Bernstein. If that anecdote is true, then he must have already surmised that Bernstein's dissertation – written in Göttingen under Hilbert's supervision – contained an erroneous result.

What happened next is not altogether clear, but it seems that David and Käthe Hilbert returned to Göttingen, whereas Cantor stayed on with his daughters in Wengen. Zermelo went back to Göttingen, as well, though in late September he was staying south of the city in the village of Münden. By the end of the month, Hausdorff had returned to Leipzig, and in a letter to Hilbert from 29 September 1904 he broke the news about the error in Bernstein's dissertation.

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[22]It should thus be noted that this argument did not pertain to the original form of Cantor's conjecture – as Hilbert had presented it in Paris – but rather to Cantor's claim that $2^{\aleph_0} = \aleph_1$.

[23]Schoenflies appears to have had little sympathy for abstract set theory, whereas Zermelo poured scorn on Schoenflies's competence when it came to such matters [Ebbinghaus, 2007a, 60,76].

> After the continuum problem had tormented me in Wengen nearly like a monomania, my first glance here of course turned to Bernstein's dissertation. The worm is sitting there at exactly the place I suspected, on p. 50: ... Bernstein's consideration leads to a recursion from $\aleph_{\mu+1}$ to $\aleph_\mu$, but it fails for those $\aleph_\mu$ which do not have a predecessor, that is for precisely those alephs which Herr J. König necessarily requires. I had already written in this sense to Herr König on my trip back, so far as I could do so without having Bernstein's work, but I have received no answer. [Purkert, 2015, 16]

Hausdorff soon thereafter clarified the error in a short note published in the DMV's *Jahresbericht* [Hausdorff, 1904] at the end of which he offered these remarks on König's lecture:

> The formula
>
> $$\aleph_\mu^{\aleph_\alpha} = \aleph_\mu \cdot 2^{\aleph_\alpha}$$
>
> obtained by Mr. F. Bernstein ("Untersuchungen aus der Mengenlehre", Diss. Halle 1901, p. 50) by means of unrestricted recursion is thus to be regarded provisionally as unproven. Its correctness appears all the more problematic, as from it, as Mr. J. König has shown, would follow the paradoxical result *that the power of the continuum is not an aleph and that there exist cardinal numbers that are greater than any aleph.* [Hausdorff, 1904, 571]

Zermelo had also been skeptical of König's reasoning, but instead of trying to refute his argument directly, he countered it by coming up with a proof for one of the cornerstones of Cantor's theory: the assertion that *every* consistent set can be well ordered. In conversations with Hilbert's newest star pupil, Erhard Schmidt, Zermelo quickly saw that a simple non-constructive proof for well ordering followed immediately from what some called the choice principle, later known as the Axiom of Choice (AC). This simply asserts that if given a collection of nonempty subsets of a set M, then we can freely pick out one element from each of these subsets. As Zermelo later emphasized, however, this terminology can be misleading because if one has infinitely many subsets, then AC implies the existence of a choice function that "chooses the elements all at once," i.e., independent of time [Ebbinghaus, 2007a, 54–55]. In [Zermelo, 1904], he used the notion of *Belegungen* or coverings, a term Cantor also used in his "Beiträge" [Cantor, 1895].

Retreating to the village of Münden just south of Göttingen, Zermelo found the peace and quiet he needed to write up his argument. One week later, on 24 September, he sent off his proof to Hilbert in the form of a letter. The latter immediately inserted this into the next issue of *Mathematische Annalen*, since the text of [Zermelo, 1904] took up only three pages. Zermelo's proof opened the flood gates of controversy regarding Cantorian set theory, leading to various efforts to secure, modify, or dismiss its main results [Moore, 1982]. Recognizing its profound importance, he sent advance copies of his manuscript to a number of experts and other interested parties. One of those who read it was Max Dehn, who responded positively, evidently unperturbed by Zermelo's use of the choice principle.[24] Dehn's response led Zermelo to send him a postcard on 27 October 1904, which was transcribed and translated in [Ebbinghaus, 2007b].

After thanking him for his friendly assent, Zermelo added these remarks:

> I also received the same day-before-yesterday from the "younger Berlin school," i.e. from [Edmund] Landau and [Isaai] Schur, in fact. But if you only knew how I am bombarded in the meantime by letter with objections from the experts of set theory, from Bernstein, Schoenflies, and König! Some fine polemic will still unfold in the Annalen; but I am not worried about that.

Zermelo's postcard thus reveals that he had been engaged in a flurry of correspondence during the past month, but also that he predicted these private communications would eventually lead to an open debate. This did, indeed, ensue (see [Moore, 1982]), but Zermelo only answered his critics four years later [Zermelo, 1908]. Since Dehn had received no further updates regarding the status of König's claims, Zermelo gave him a short synopsis. This made clear that both he and Julius König had independently found the error in the latter's proof, but could only confirm this after checking Bernstein's dissertation, which was not at hand in Heidelberg. He thus went on to write:

> So you still do not know the fate of König's talk? Innocent soul! Nothing else was really discussed during the entire vacation. K[önig] solemnly informed both Hilbert and me that he revoked the Heidelberg proof. B[ernstein] did the same with his theorem on powers. K[önig] can be happy that the library in Heidelberg had closed so early; otherwise he might have made a fool of himself. Thus I had to wait for checking until my return to G[öttingen]. Then it was instantly obvious. In the meantime, however, K[önig] himself had found that out when elaborating his proof. ...[He points to page 50 of Bernstein's dissertation.] You will see the mistake at once which is the only result K[önig] uses. Blument[h]al says that this is a real "Bernstein ruin." But K[önig] wants to save what can be saved and fantasizes about the set W of *all* order types which *could* be contained in many a set so that my ... [argument] ... would be wrong. Strangely enough, he is backed up in this matter by Bernstein and Schoenflies, by the latter of course in quite a confused and misunderstood manner. The W-believers already deny the def[inition] of subset, perhaps even the principle of the excluded middle. [Ebbinghaus, 2007b, 431]

As noted earlier, Dehn had already learned some of the fundamental ideas of Cantorian set theory as a student. He may well have attended the proseminar Schoenflies offered on this topic, but in any case Hilbert chose this as one of the subjects for his final oral exam. Moreover, Dehn's knowledge of set theory was by no means merely passive. In [Dehn, 1904] he applied point set topology to show the true scope of the Dehn invariant for polyhedra. In his earlier studies related to the third Paris problem, he had merely given examples of pairs of polyhedra with

[24]Hilbert had never explicitly appealed to such a general principle in his existence proofs, but Hausdorff later became a strong advocate of AC [Dreben and Kanamori, 1997, 80–81].

the same volume, but which were inequivalent because the values of this invariant differed. Dehn's new results showed that, in fact, there are uncountably many different pairs of such inequivalent polyhedra, and that this is true not only in the Euclidean case but also in non-Euclidean geometries (Chapter 3). As we shall later see, however, Max Dehn never felt drawn to axiomatic set theory, as practiced by Zermelo and Fraenkel, nor did he revel in the abstractions promoted by Cantor and Hausdorff in their works on transfinite arithmetic.

It was probably during the ICM in Heidelberg that Dehn first met the Danish geometer Poul Heegaard, although the latter's name does not appear on the list of participants. Wilhelm Magnus claimed that they got to know one another then and that they left the city on the same train, Dehn going to Hamburg and Heegaard returning to Copenhagen. On the train ride together, according to this story, they supposedly decided to co-author the article on topology for the German Encyclopedia of Mathematical Sciences [Magnus, 1978/79, 134]. In any event, it was around this time that Dehn and Heegaard began their collaboration by adopting a clear division of labor: the older Dane wrote up summaries of the literature, leaving the younger Dehn the task of outlining the foundations of the discipline [Dehn and Heegaard, 1907]. Magnus considered this pioneering effort to systematize combinatorial topology a characteristic example of Dehn's entire mathematical outlook. Although this program failed to provide a lasting framework for research on higher-dimensional manifolds, it nevertheless served to throw new light on deep problems that challenged several of the world's leading mathematicians for decades to come (for further discussion of [Dehn and Heegaard, 1907], see Chapter 4).

Academic Opportunities, 1905–1910

Hilbert was always eager to promote the careers of young mathematicians who studied under him, and he later expressed frustration because of the fact that faculties many times failed to act on his advice. In Dehn's case, Hilbert had already recommended him for an associate professorship in Würzburg in 1903, alongside two others, Felix Bernstein and Gustav Herglotz, though none of the three was chosen. It should be noted, on the other hand, that diplomacy was not Hilbert's strong suit. Unlike Klein, who normally dominated either by persuasion or sheer perseverance, Hilbert was impulsive, opinionated, and arrogant. He did not go out of his way to make enemies, but his authoritative manner often rubbed his fellow mathematicians the wrong way. During the period 1905 to 1910, when Dehn was in Münster, patiently awaiting his first call to a professorship somewhere in Germany, he became entangled in a controversy with Theodor Vahlen, a mathematician whom Hilbert rightfully saw as an usurper in the field of foundations of geometry (see below).

One of the mysteries Hilbert struggled with while writing *Foundations of Geometry* concerned the relationship between the two classical theorems of Pappus and Desargues. For some time it seems he conjectured that the latter was stronger, but it took another five years before Gerhard Hessenberg could clarify this situation. He showed, in fact, that just the opposite is true: Pappus's theorem implies Desargues' theorem, but not conversely [Hessenberg, 1905]. Dehn was vacationing with his family in Hamburg when he received the manuscript of this paper from Hilbert, who was eager to learn his opinion. On returning the paper, Dehn wrote on 12 April 1905 that he had been too busy to study it thoroughly, but expressed confidence in what he had read. He was especially impressed with Hessenberg's elegant proof of Pappus's theorem, and wrote furthermore: "he appears to use in these things essentially the usual tools, your segment arithmetic and the pseudo-geometry from my dissertation. I'm very happy to see this matter now clarified and that the sphere-beings (*Kugelbewohner*) can be blessed without having to believe in continuity."[25]

Furthermore, Dehn reported on his recent results on Euler's formula in $\mathbb{R}^5$ in relation to non-Euclidean spherical excess in $\mathbb{R}^4$ ([Dehn, 1905a] and [Dehn, 1905b] discussed in the chapter following). He also mentioned that he had been asked to fill in for the *Ordinarius* in Kiel during the upcoming summer semester. This was a reference to Paul Stäckel, who had accepted a professorship at the TH Hanover. Hilbert wrote back the very next day, expressing his delight over this turn of events:

> Above all, my hearty congratulations for your [temporary] appointment in Kiel. That makes me happy in the first instance for your sake, but also because it means that finally there are prospects for a Göttingen product to move ahead. But do be very careful around the colleagues in Kiel, as confidentially speaking, faculties can be difficult; one must be ready – I'm thinking of Blumenthal's case – for anything.[26]

Otto Blumenthal, mentioned earlier as the first mathematician in Göttingen to discover Dehn's budding talent before graduating as Hilbert's first doctoral student, was a bit more than two years older than Max Dehn. During the academic year 1904/05 he held a similar temporary professorship in Marburg. Later that same year, Sommerfeld arranged for his appointment at the TH Aachen, where the faculty elevated him to the chair formerly occupied by Lothar Heffter. Although he longed for an appointment at a university, Blumenthal never had the chance to leave Aachen, and in one particularly flagrant case this was due to antisemitism [Rowe, 2018b, 144–150].

Max Dehn was opinionated, too, especially when it came to areas of mathematics in which he had already established his reputation as a leading authority. He was still only 27 years old and patiently waiting for his chance to climb up the academic ladder. Although Hilbert cautioned him to be careful when speaking with the professors in Kiel, he wrote nothing in the very same letter about showing a similar reserve when criticizing the work of a colleague who wrote a mediocre book, one that Hilbert clearly found personally offensive. This colleague was the Greifswald mathematician Theodor Vahlen, whose career trajectory would soon collide with Dehn's in an unfortunate way.

As a backdrop to the section following, it should be mentioned that, only shortly before Dehn wrote his review, he nearly gained his first associate professorship in Greifswald. Although he may not have been aware of this, the Greifswald faculty had placed his name first on their list of candidates for the vacancy created when Gerhard Kowalewski departed for Bonn. Kowalewski's appointment in Bonn was engineered by Eduard Study, who had only recently left Greifswald. Study's successor in Greifswald was his good friend Friedrich Engel, nothing unusual, as

[25] Dehn to Hilbert, 12 April 1905, Nachlass Hilbert 67, SUB Göttingen.

[26] Hilbert to Dehn, 13 April 1905, Dehn Papers, Dolph Briscoe Center for American History, University of Texas at Austin.

personal favoritism and compatibility (*Verträglichkeit*) played a large role in academic appointments. In preparing the list of candidates for the empty post of associate professor, both Engel and Greifswald's senior mathematician, Wilhelm Thomé, surely had considerable influence. They noted that Dehn was the youngest of the three nominated. The others named were Vahlen and Felix Hausdorff, an unbaptized Jew, unlike Dehn.[27]

Vahlen was described in the report as a former student of Leopold Kronecker in Berlin, who since 1896 was teaching as a *Privatdozent* in Königsberg. No comparative remarks were made, and the faculty only commented that any of the three would be completely suitable [Brieskorn and Purkert, 2018, 712–713]. Vahlen was chosen by the ministry in 1904 and he remained in Greifswald until 1924. In that year he was forced to resign without pension rights because of political acts directed against the Weimar Republic. These later events as well as Vahlen's singular career as a prominent Nazi figure within the German mathematical community are recounted in [Siegmund-Schultze, 1984]. In 1905, no one had any inkling, of course, of what was to come in the aftermath of the war. Still, in later years the parties involved surely never forgot the acrimonious public exchanges that took place that year.

Dehn's Critique of Vahlen's *Abstrakte Geometrie*

In his letter from April 12 to Hilbert, cited above, Dehn mentioned that he had been asked to review Vahlen's new book on the foundations of geometry. This, too, was welcome news for Hilbert, who had already looked at this work and found it worse than disappointing. As Klaus Volkert has pointed out, he had every reason to feel slighted: Vahlen's *Abstrakte Geometrie* [Vahlen, 1905a] made numerous references to Hilbert's *Grundlagen der Geometrie*, but mainly to criticize small points in it.

Vahlen's book was the first monograph on this subject published in Germany since the appearance of Hilbert's booklet. As we shall see, Vahlen was not without supporters within the German mathematical community, even though he left no lasting mark as a mathematician. Instead, he is solely remembered as a political figure, probably the only professor of mathematics who joined the Nazi Party already in 1924, soon after Hitler's failed *Putsch* in November 1923. Vahlen had the party number 3,961, and after 1933 this early support of the Nazi Party enabled him to become a prominent figure in the regime. In fact, he may have played a role in pushing through Dehn's mandatory retirement in 1935.

In view of these circumstances and the events that would follow, Hilbert's letter is well worth quoting:

> Do indeed take on the review of Vahlen's book. It is altogether important that it receive a decent critique. I received the book yesterday from the book dealer. After paging through it I gained the impression that the whole thing lives from the ideas in my *Grundlagen*, but without saying so. For example, the theorem

[27] At this time, Hausdorff had only begun his research on point sets and transfinite arithmetic, which then culminated in 1914 with the publication of his *Grundzüge der Mengenlehre* [Hausdorff, 2002]. Study later brought him to Bonn as Kowalewski's successor in 1910, after which Hausdorff's career swung like a pendulum between Bonn and Greiswald, where he succeeded Engel in 1913, before returning to Bonn in 1921.

> stating that the commutative law is not a consequence of the remaining axioms, which cost me a lot of trouble to prove, is simply taken over, and when he does name me, then only to point out some oversight in the first edition. ... Also with respect to other authors, I believe Vahlen acts as though everything stems from him.[28]

Hilbert encouraged Dehn to write a lengthy review for the *Göttingischen Gelehrten Anzeigen*, which he noted paid its reviewers well. Had Max Dehn published his review there, word would have eventually spread, but probably the scandal that ensued would not have occurred. Instead it appeared in the highly visible *Jahresbericht* of the German Mathematical Society (DMV), whose editor, Jena's August Gutzmer, found himself in a difficult position.

Whether Dehn felt prodded by Hilbert's encouragement or not would be hard to say, but he certainly studied Vahlen's book carefully before taking it apart. The scope of this monograph was far broader than Hilbert's, but Dehn hinted that it lacked originality: "In the present work, the author attempts to give a comprehensive and detailed exposition of the foundations of geometry based on methods that have recently been created, especially in Italy and Germany" [Dehn, 1905c, 535]. Aside from this general remark, one finds nothing in the review suggesting that Vahlen dealt with any particular individuals unfairly, as Hilbert insinuated. After a brief paragraph outlining its contents, Dehn simply took up a series of statements, relentlessly exposing them as either obscure, false, or just outright mathematical nonsense. The tone throughout was dispassionate, and of course most of Dehn's criticisms were only intelligible to experts in the field. So, in effect, he was writing for them; others could only draw the obvious conclusion, namely, that this was a worthless book written by an incompetent author.

Vahlen had to respond, of course, and he wasted no time in doing so in [Vahlen, 1905b]. This reply went through all of Dehn's criticisms, point by point, conceding certain shortcomings, but then describing them as the kind of harmless blemishes one finds in any mathematics book. He ended by charging Dehn with unfairness, underscoring that his review failed to mention any of the many positive accomplishments Vahlen took pains to record. Vahlen's long rebuttal closed by accusing Dehn of having merely *thumbed through* his book looking for mistakes instead of actually *reading* it.

The editors of the *Jahresbericht* evidently sent Vahlen's response to Dehn, who submitted a brief reply. This appeared immediately afterward and in it Dehn only bothered to remark that Vahlen's final sentence was totally unfounded: he had, of course, read the book carefully, and since he had expressed what he had to say about it clearly enough, he saw no need to comment further. A number of readers, however, had been puzzled as to why he had not given an overall assessment in his review, and so he offered that here: "Consequently [*Somit*] this work can only be recognized as having limited use: everything of value contained in it, except for some details, can be found presented more clearly and perfectly explained in other works ..." [Dehn, 1905d, 595]. He then named four German authors, one of course being Hilbert. Dehn's mentor had been concerned that Vahlen had appropriated his ideas, whereas this simple remark was far more devastating, since Dehn flatly asserted

[28]Hilbert to Dehn, 13 April 1905, Dehn Papers, Dolph Briscoe Center for American History, University of Texas at Austin.

that Vahlen's work was inferior to the ones he had drawn upon, while adding nothing of substance to those works. Vahlen could not very well leave such a charge unanswered, and so he proceeded to spell out eight *new* and *valuable* contributions found in his *Abstrakte Geometrie* and nowhere else in the mathematical literature [Vahlen, 1906a].

The spat between Vahlen and Dehn was observed from afar by Oswald Veblen, whose review of Vahlen's *Abstrakte Geometrie* in the *Bulletin of the American Mathematical Society* surely gave Max Dehn pleasure to read. Much like Dehn, Veblen took brief note of the book's five chapters: I Foundations of arithmetic; II Projective geometry; III Projective geometry; IV Affine geometry; and V Metric geometry. A book on these topics, he wrote, "*would* be very useful for giving a general view of the recent studies on foundations of mathematics," if only it were written with

> that precision of language which is indispensable in any discussion of such a subject. The reader is constantly confronted with statements which are incorrect if taken literally and which, if not taken literally, are open to more than one interpretation. Many of the author's postulates are labeled by him as definitions. Moreover, there are places where it is very difficult to determine which of the previously stated hypotheses are being used and which are not. As a consequence, the reviewer is able to state hardly a single new *result* which is surely established by this book. [Veblen, 1906, 505]

As if these comments were not devastating enough, Veblen let his American audience know that Max Dehn had already taken the time to skewer Vahlen's book for the German Mathematical Society:

> It seems to the reviewer not to be worth while to lengthen this notice with criticisms of details, especially as many of the points that would be mentioned have already been adverted to by Dehn in a review published in the Jahresbericht der Deutschen Mathematiker-Vereinigung, volume 14, page 535. The reader who is interested in such things will find a rejoinder to Dehn by Vahlen on page 591 of the same volume, a retort by Dehn on page 595, and a second "Erwiderung" by Vahlen in volume 15, page 73. With these he may compare a footnote by Schoenflies on page 31, volume 15. [Veblen, 1906]

Turning to that page in Arthur Schoenflies's article, one finds a reference to Dehn's critique of Vahlen's definition of continuity. Echoing Dehn's opinion that this was an illusory concept, Schoenflies claimed it was either nonsense or reduced to Dedekind's notion of continuity. Vahlen denied this, while asserting that his definition was a variant of Veronese's concept. In the decisive footnote mentioned by Veblen, Schoenflies called this an erroneous claim, and then added this stinging remark: "It is very regrettable that a book that strives to treat its subject from all sides comprehensively should suffer from such a fundamental deficiency, which disturbs to a great extent the consistency of the presentation in the arithmetical as well as the geometrical part" [Schoenflies, 1906, 31]. This attack, too, was answered in the *Jahresbericht* in [Vahlen, 1906b].

As noted earlier, Schoenflies had been one of Dehn's teachers when he first came to Göttingen. During the 1890s, he developed a strong interest in early point set topology, about which he wrote reports both for the *Encyklopädie der mathematischen Wissenschaften* (1898) and for the DMV [Schoenflies, 1900]. A protégé of Felix Klein and a personal friend of Hilbert, Schoenflies was a geometer with broad interests. He left Göttingen in 1899 to assume a full professorship in Königsberg, where Vahlen had already been teaching as a *Privatdozent* for the past three years. These circumstances suggest that there was no love lost between Schoenflies and Vahlen, and the former may well have harbored suspicions that his colleague hoped to make a name for himself in geometry at the expense of Hilbert. As for Vahlen and his sympathisers, they were quite astounded by the ferocious tenacity of the attacks on Vahlen's book.

Vahlen himself undoubtedly took note of the fact that his two assailants were both Jews from Göttingen, later characterized as the citadel of "Jewish mathematics" by Nazis in academia. Friedrich Engel shared no such feelings, but he also felt that the attacks against Vahlen had gone too far, and so he complained to August Gutzmer, editor of the *Jahresbericht*. Gutzmer responded on 14 January 1906:

> The unmixed pleasures of life will never be granted to an editor. I have often experienced the truth of that saying, and the case of Vahlen-Dehn is a glowing testimonial as such. But please do not believe that you, with your friendly words, are guilty of contributing to these mixed joys – quite the contrary, I am very grateful to you for speaking out forthrightly! It gives me the opportunity to respond in a similar manner, something I have often done with friends and colleagues whose opinions I value and rely upon.
>
> As for the present matter, I believe that this dispute has not yet come to a close, as I expect to receive a final reply from Vahlen as the party under attack. Once that response has reached my hands, I intend to intervene as editor and declare this to be the last word. If I had wanted to take sides against Vahlen, I would have certainly stated so clearly. But the matter will now take a new turn because the next issue will contain an article by Schoenflies, in which he deals with Veronese's continuity, an issue of decisive importance for this entire dispute.
>
> It was precisely my neutrality that prevented me from writing to Dehn in the manner you suggested. The fact that Dehn's response was extremely sharp could not disqualify it from publication, as it contains no personal attacks. Every reader can decide for themselves whether or not this undermined (*entkräftet*) Vahlen's presentation. Were I in Vahlen's position, I would simply submit a short statement saying that Dehn's review and response are so unusually sharp that I must pass on to others to form their own opinion of the value of the book – one word more than that would be too much. . . . [29]

Gutzmer further explained that Dehn's response to Vahlen's defense had arrived very late, which is why Gutzmer had been unable to send it to Vahlen before it

[29] Gutzmer to Engel, 14 January 1906, Engel Nachlass, Giessen.

went to press. In fact, Teubner had prepared the whole issue and was ready to send it out, so the publisher had to remove another notice in order to place Dehn's reply immediately after Vahlen's rebuttal. Engel's role here as Vahlen's advocate gave Gutzmer the chance to persuade Vahlen that, as editor, he needed to assume a neutral role, while making clear that he did not condone Dehn's actions. He thus ended by writing:

> I hope very much, dear Engel, that after reading these explanations you will see this matter with somewhat different eyes; presumably Vahlen will see it differently, too, and you are welcome to share these lines with him if you are so inclined. I trust that you know me far too well to imagine that in my capacity as editor of the Jahresbericht I would intervene in a tone like that of Dehn's and take sides in such a difficult problem. Precisely the opposite was my intention.

Engel was evidently satisfied with this response; he sent Gutzmer's letter to Vahlen the following day, adding a note of agreement and urging Vahlen to avoid any sharp formulation in his reply. As planned, Gutzmer then added a footnote stating that this would be the final contribution in this controversy [Vahlen, 1906a, 74]. Schoenflies's critique in [Schoenflies, 1906] was printed in the same issue, however, and so Vahlen had to send yet another response to dispute its claims [Vahlen, 1906a, 74].

For Dehn, however, the consequences of this controversy were yet to come. In 1910, Greifswald's senior mathematician, Wilhelm Thomé, died, and Engel turned to his friend in Bonn, Eduard Study, for advice as to an appropriate successor. Study recommended Dehn, evidently having forgotten the highly visible debate from five years earlier. Engel then reminded him of the "compatibility problem," since Vahlen's presence made Dehn's candidacy unthinkable. Study then replied on 2 October 1910: "When I wrote you, I didn't even think that Dehn and Vahlen don't get along. You will probably have no alternative than to have Vahlen fill the missing *Ordinariat*, and he is certainly not so *much* worse than most of the others." This, indeed, is what happened: Vahlen was thus promoted to a full professorship in Greiswald, whereas Dehn remained in Münster, where he was now in his ninth year as a *Privatdozent*. Finally in 1911, Dehn got his first break when Lothar Heffter left Kiel to assume the professorship in Freiburg previously held by Jacob Lüroth, who died in 1910. This enabled Georg Landsberg, who held an associate professorship in Kiel, to be promoted to full professor, whereupon Dehn was appointed to Landsberg's former position. This appointment became effective on 1 April 1911 and paid Dehn a yearly salary of 2,600 Marks with a supplement of 1,620 Marks for living costs.

Call to Kiel, 1911

Kiel remained a provincial university throughout the nineteenth century with only one professorship in mathematics up until 1874. In that year, Leo Pochhammer, formerly a *Privatdozent* in Berlin, joined the faculty. In 1906, Georg Landsberg from Breslau was appointed as the first occupant of a new *Extraordinariat*, the position Max Dehn would obtain in 1911, when Landsberg succeeded Lothar Heffter as full professor (*Ordinarius*). An expert on algebraic functions, Landsberg is still remembered today as co-author with Kurt Hensel of the monograph *Theorie der* *algebraischen Funktionen einer Variablen* (1902). He also guided the doctoral work of Jakob Nielsen, who first matriculated at Kiel in 1908. Nielsen eventually took his doctorate under Dehn in 1913, following Landsberg's death a year earlier (see Chapter 6).

Kiel lies only 100 kilometers north of Hamburg, so Max had many opportunities to visit his family. Still, he did not forgo some of the local pleasures provided by living in a port city on the Baltic Sea, one of which was sailing. He surely found this a romantic activity. On a trip back home, he got to meet a young art student from Berlin named Antonie (Toni) Landau, who happened to be boarding with one of Max Dehn's family members. Toni was from Berlin, where her father Isidor was a newspaper editor and theater critic, but also the author of travel books. Her mother Louise worked as a translator, rendering novels by John Galsworthy as well as writings of Theodore Roosevelt into German. Both were originally from Poland. Max invited Toni to go sailing with him, which marked the beginning of a whirlwind romance. She was just nineteen when they married on August 23, 1912.

Only one month later, Georg Landsberg unexpectedly died, a circumstance that seemed to open the way for Dehn to become an *Ordinarius* in Kiel. The discrepancy in salary between associate and full professors was extreme, so that only the independently wealthy could be satisfied with the lower position. His long period of waiting seemed to be over, but in the end this was not to be. We gain a glimpse of what transpired from a letter Dehn wrote to Hilbert on 9 March 1913:

> I owe you my deep gratitude for your efforts on behalf of my promotion. Pochhammer informed me that he read your letter in the faculty meeting. Unfortunately, the success of this proved impossible due to a minority opinion forwarded by two gentlemen, who characterized me as a poor lecturer. Probably the motivation behind this was of a private nature, as objectively this description could only apply to a single course taught two years ago. Since I could easily refute this claim through the success of my present teaching activity, Pochhammer wrote to Elster[30] giving a detailed minority report. This will be of no use at the moment, but it will hopefully help to nullify the unfavorable report in the future.[31]

A few months later, Dehn finally got the call he had long waited to receive – he would succeed Constantin Carathéodory as *Ordinarius* at the Technical College in Breslau. Hilbert may well have had a hand in this, as he had helped orchestrate the negotiations that brought Carathéodory to Göttingen as Klein's successor. In early July, he sent his congratulations to Dehn, who wrote back to say that he had been very angered by the way Hilbert's input had been dismissed by his colleagues in Kiel.[32] He maintained his ties there, however, as he was succeeded by his good friend Otto Toeplitz. Dehn was no doubt greatly relieved to gain a full professorship soon thereafter, though at the newly founded Institute of Technology in Breslau rather than at a university, which would have suited him much better.

[30] Ludwig Elster was a ministerial official in Berlin.

[31] Dehn to Hilbert, 9 March 1913, Nachlass Hilbert 67, SUB Göttingen.

[32] Heinrich Wilhelm Jung, who taught at a Gymnasium in Hamburg, was chosen as Landsberg's successor.

Call to Breslau, 1913

The Silesian city of Breslau had a university dating back to 1702. This was an initiative of Jesuits, who were granted this privilege by the Austrian Emperor Leopold I. After Silesia came under Prussian control, this Jesuit university with its philosophical and theological faculties was retained, but it was later combined with the Lutheran university in Frankfurt on the Oder, which was displaced to Breslau in 1811. This gave the Prussian University of Breslau a quite distinctive character. In mathematics, it was strongly represented by two synthetic geometers, Heinrich Schröter and his successor Rudolf Sturm, as well as the invariant theorist Jacob Rosanes. Two of Max Dehn's closest colleagues, Ernst Steinitz and Otto Toeplitz, were students of Rosanes. All three – Steinitz, Toeplitz, and Rosanes – were of Jewish descent. In 1905, Adolf Kneser joined the faculty. He and Dehn shared common mathematical interests, as did Kneser's son, Hellmuth, who profited from their discussions during and after the war (see Chapter 4).

The *Technische Hochschule* in Breslau, located about three miles from the university, was only founded in 1910 with three professorships in mathematics. These were first held by Constantin Carathéodory, Ernst Steinitz, and the geometer Gerhard Hessenberg. When Dehn succeeded Carathéodory in 1913, he found the two remaining colleagues much to his liking; indeed, all three had strong interests in various facets of geometry. As discussed above, in 1905 Hessenberg solved a delicate foundational problem that had baffled Hilbert by proving that the Theorem of Desargues follows from Pascal's Theorem for two lines. Dehn surely already knew several of his earlier papers as well when Hilbert sent him that manuscript.

Hessenberg held the chair in geometry, which meant he taught the standard courses in descriptive geometry. His interests, though, stretched from technology and differential geometry to Cantor's set theory and critical philosophy. Intellectually, he had much in common with Max Dehn, though he exceeded the latter when it came to sarcastic humor. During the first years of the war, he was elected to the office of rector at the TH Breslau, a sign of the high respect his colleagues had for his administrative abilities. As a geometer, Steinitz studied problems involving finite polyhedra, configurations, and groups. These topics were very close to Dehn's interests during the period he taught in Breslau. Indeed, as we will see in Chapter 4, both he and Steinitz stood at the forefront of a movement to promote combinatorial methods in the study of topological problems.

Abraham Fraenkel was studying at the university when Dehn arrived at the TH Breslau. He later recalled how Dehn, Steinitz, and Hessenberg all had to adapt their teaching to the needs of the engineering students, which meant they had little to offer him. Fraenkel nevertheless remembered an incident from that time:

> In the mathematics colloquium I presented my as yet unpublished doctoral dissertation, even though it fell on the Sabbath. It was a complicated matter, but I could not avoid it, and had reasonable success. Only Professor Dehn, whose baptism failed to accelerate his career as he had hoped and expected, commented that young mathematicians should not concern themselves with such abstract subjects, but should instead stick to concrete problems. [Fraenkel, 1967, 101]

Leaving aside the remark about Dehn's baptism – which reflects Fraenkel's opinions as a Zionist – this anecdote surely accords with Dehn's general pedagogical views. As a true disciple of Hilbert, he believed that abstract concepts arose from concrete problems and that no mathematician should neglect the latter in favor of the former when, in fact, they are inextricably connected.

It was during these years in Breslau, interrupted by the Great War, that the Dehns' children were born: Helmut in 1914, his two sisters Maria and Eva in 1915 and 1919, respectively. Their father was conscripted into the army in 1915, working mainly as a coder/decoder near battle sites on the Western front.[33] Helmut confirmed that Max Dehn was less than enthused about military life. In one instance, he was standing by the road when an entourage escorting the Kaiser passed. He failed to notice the other men standing at attention and saluting, but others noticed him with his hands still in his pockets [Yandell, 2002, 122]. Nevertheless, he completed his war service and was discharged without incident as a corporal, in sharp contrast to the experiences of his future colleague Carl Ludwig Siegel (as briefly discussed in Chapter 5).

Unfortunately, Dehn's three years of war service interrupted his work with colleagues in Breslau. Soon afterward, in 1919, Hessenberg left for Tübingen, one of several changes in the mathematical landscape during the immediate postwar period. In some instances, Jewish candidates were among the first to be considered for promotion. In 1920 Toeplitz was elevated to a full professorship in Kiel, and he afterward turned to Klein and Hilbert for advice regarding a vacancy there. There was widespread awareness that many talented Jewish mathematicians had been passed over during the past decade, a circumstance that placed Toeplitz in an awkward position in conducting this search, since he, too, was of Jewish background.[34] Toeplitz also wrote to Eduard Study in Bonn, who advised him to consult the "Old Testament," after which he named several qualified individuals, all of whom happened to be Jewish. The two he mentioned first were Emmy Noether and Arthur Rosenthal, neither of whom were named by Klein or Hilbert.[35] The position in Kiel eventually went to Ernst Steinitz, Dehn's colleague in Breslau, who was high on Hilbert's list.

Since Dehn and Toeplitz were both protégés of Hilbert, they were very likely acquainted with Emmy Noether. She wrote to Dehn on 8 January 1920, answering a no longer extant letter from him. Dehn surely anticipated that Steinitz would soon be leaving Breslau for Kiel, so he wrote to Noether asking her opinion of Werner Schmeidler's abilities. After receiving her positive report, he informed her that it was very helpful, though he did not plan to include Schmeidler's name on the short list of candidates. Noether then replied, in turn:

> I had already heard that Schmeidler will not be on the Breslau list, but I'm pleased that you have a favorable impression of him based on my report. Meanwhile, Toeplitz has offered to let him re-habilitate in Kiel and to take on a large teaching contract

[33]In an account of his wartime service at the time he applied for his pension, Dehn listed four different battle sites where he had been stationed: La Bassée, Noyon-Roué, St. Quentin, and Oise.

[34]See the discussion in [Bergmann/Epple/Ungar, 2012, 206–207] and in [Rowe, 2018a, 349–350].

[35]Study to Toeplitz, 1 July 1920, quoted in [Koreuber, 2015, 40]. Eight years later, Noether was also briefly considered for this very same position in Kiel, see [Siegmund-Schultze, 2018].

> (with an inflation allowance of around 9,000 Marks). He will no doubt accept, if certain conditions (per diem expenses until he has found a place to live and similar things) are met. You surely know that Toeplitz had originally turned to Nielsen.[36]

Emmy Noether's own earlier attempt to habilitate in Göttingen had led to a famous clash within the philosophical faculty in 1915. It was not until June 1919 that the Berlin Ministry of Education agreed to exempt her from the decree that restricted habilitation to male candidates [Rowe, 2021, 44–61]. Considering that this had transpired only a half-year earlier, her letter to Dehn shows that Noether was not only well connected but her opinions carried real weight as well. Schmeidler and Steinitz both accepted the offers from Kiel, and Dehn then brought Nielsen to Breslau, though he would leave for Copenhagen after just one year (for more on Nielsen, see Chapter 6).

Dehn also decided to leave Breslau in 1921 when he was offered a prestigious chair in Frankfurt. Before departing, he may have remembered Emmy Noether's report when he decided to choose Werner Schmeidler as his successor.[37] Dehn's appointment in Frankfurt was partly due to the support of Arthur Schoenflies, the senior mathematician on the faculty. How these complicated negotiations played out is described in detail in Chapter 5.

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[36]Noether to Dehn, 8 January 1920, Dehn Papers, Dolph Briscoe Center for American History, University of Texas at Austin.

[37]When another position became vacant in Breslau just one year later, it went to Fritz Noether, whose sister afterward often visited him there during holidays.

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