Max Dehn as Historian of Mathematics

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Personal Note
It gives me pleasure to dedicate this paper to Catriona Byrne for her many years of engagement on behalf of mathematics and its history.

1 Introduction: Biography and History

Compared with nearly any other field of knowledge, mathematics has an extraordinarily long and rich history. From time to time scholars have also avidly studied the mathematics of the past, and in some cases they took inspiration from it to invent something novel. A particularly striking example came about after 1588 when the eight books of the *Collection* of Pappus of Alexandria were published in Venice in the Latin edition prepared by Federigo Commandino. Pappus lived around 300 A.D., so some 500 years after the high water mark of ancient Greek mathematics and, as Thomas Little Heath remarked, his compendium was "obviously written with the object of reviving classical Greek geometry" [17, 2: 357]. Aside from the major works of Euclid, Archimedes, and Apollonius, the *Collection* is the most important mathematical text we possess from the ancient classical world. Indeed, without this text and its commentaries historians would never have been able to imagine the scope of the Greek tradition of geometrical problem solving. Pappus, to be sure, was not an inventive mathematician on the level of his predecessors; in fact, it would be more apt to think of him as an early historian of mathematics. Several of

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those who read him, on the other hand, were very formidable mathematicians, two of them being Descartes and Newton. To immediately appreciate the significance of Pappus's *Collection* for the flourishing of European mathematics in the seventeenth century, one needs only to read Henk Bos's insightful study of Descartes's *La Géométrie* [1].

During the early twentieth century, a resurgence of interest in history of mathematics came to fruition within the German mathematical community. Otto Neugebauer, who largely managed Richard Courant's Mathematics Institute in Göttingen, undertook pioneering research on Babylonian mathematics and astronomy. He and Otto Toeplitz, who taught in Kiel before moving to Bonn, founded the Springer journal *Quellen und Studien zur Geschichte der Mathematik Astronomie und Physik* in 1929. Meanwhile, in Frankfurt, Max Dehn was running a weekly seminar that studied ancient and early modern mathematical texts in their original languages. These sorts of studies took place during the short-lived era that saw the flowering of Weimar culture, which ended all too abruptly in 1933 when Hitler came to power. Neugebauer, Toeplitz, and Dehn all fled Nazi Germany, the latter two under threat to their lives. Historical research in mathematics continued in Germany, but much of it was thereafter colored by a nationalist or even explicitly racist agenda, led by the efforts of the Berlin mathematician Ludwig Bieberbach (Segal [39, pp. 334–417]). Although he lost all his positions after the fall of the NS-regime, Bieberbach maintained certain connections with influential figures who shared his interest in promoting historical studies of mathematics in Germany.

During the postwar era, the heyday of Bourbaki, a new wave of historical interest arose in the West. Axiomatization, rigor, purism, structuralist concepts—these watchwords of modern mathematics deeply affected the way mathematicians came to see but also to judge the mathematics of the past. The Bourbaki project itself had modest beginnings, but with time its goal was to canonize the fundamental structures in those theories which the group considered the established core theories of modern mathematics. In this sense, Bourbaki was only incidentally interested to look backward and identify when these ideas first arose. It would appear doubtful that the historical notes, which Bourbaki included in the volumes of *Éléments de mathématique* [2] and which were later gathered together in [3], generated great interest among mathematicians or historians of mathematics. Their intent, after all, was essentially just to provide a larger account of the intellectual context connected with the topics covered in the *Éléments*. For students of mathematics with a modicum of interest in the subject as intellectual history, many of these notes are still well worth reading today. That goes without saying, of course, for André Weil's history of number theory [47].

By the 1970s, though, a handful of scholars who pursued history of mathematics from other perspectives began to publish work that Weil, in particular, found distasteful or worse. Those partisan battles from long ago need not concern us here, but one aspect has real significance for the theme of this essay. Bourbaki represented a purist movement that hoped to canonize a certain body of mathematics, which contemporary mathematicians—or those who considered themselves to be well-rounded—would acknowledge as core knowledge. Jean Dieudonné described this objective very clearly in a lecture he delivered in 1968, later translated in [11]. This canonization naturally lent itself to a highly selective view of the past, a style of historiography that Ivor Grattan-Guinness called "the royal road to me." In that respect, it is worth noting that Dieudonné thought quite highly of Klein's lectures on nineteenth-century mathematics [24], as did Neugebauer and others (Rowe [34, pp. 32–33]). They generally lauded his account, in particular Klein's strikingly subjective remarks laced with autobiographical anecdotes. The contrast with the dry factual information in Bourbaki [3] is striking, but of course the name index in the back of that volume reveals a very clear Bourbakian image of the history of mathematics. To exaggerate only a little, this style of historiography judged the past almost exclusively from the standpoint of the present. Moreover, the names that appeared throughout were those credited with an important new idea or result. History of mathematics was thus reduced to a certain impressive chain of disembodied ideas. Who produced those ideas and why they put them into circulation were questions that went largely unasked, and these "mathematical people" never emerged from the shadows. Otto Neugebauer, I'm quite sure, thought that serious history of mathematics had nothing to do with the personal lives of mathematicians, but all that began to change in the 1970s.

It was also during that decade that three editors at Springer—Alice and Klaus Peters and Walter Kaufmann-Bühler—launched a newsletter they called "The Mathematical Intelligencer" (on its history, see Senechal [40]). The world of mathematics was still rather small in those days, but large enough that Springer's mailing list put "The Old Intelligencer" (as it was later called) into the hands of a few thousand mathematicians. The rest, as they say, is history, and today's glossy magazine bears practically no resemblance to those early issues. Throughout its nearly 50-years, *MI* has kept pace with new trends, emerging and older communities, subcultures, crossovers with the arts and sciences, etc., etc. History and biography played a large part as well, all part of a complex unfolding of varied interests in the realm of mathematical culture.

Here I'd like to offer some brief reflections on the life and work of Max Dehn (1878–1952), stressing, in particular, his interests in the history of mathematics. Dehn was remembered often in the pages of *The Mathematical Intelligencer*, beginning with an essay about the man and his work written by his former student, Wilhelm Magnus [27]. In the 1980s, John Stillwell translated Dehn's most important papers and published these with commentary in [10]. Since that time, Dehn's name and fame have only grown. This essay is adapted from parts of a forthcoming book, *Max Dehn: A Polyphonic Portrait* (Lorenat et al. [26]).

2 Dehn in Frankfurt

In 1921, following complex negotiations, Max Dehn assumed the professorship formerly held by Ludwig Bieberbach in Frankfurt. Founded in 1914 as a privately endowed institution (*Stiftungsuniversität*), Frankfurt University hired many more scholars of Jewish background than nearly all the older German universities. In mathematics, Frankfurt's senior mathematician, Arthur Schoenflies, took full advantage of this situation during the first years of the Weimar Republic. When Dehn joined the faculty as its second full professor in mathematics, three others held positions as associate professors: Ernst Hellinger (appointed in 1914), Otto Szász, and Paul Epstein. These five Frankfurt mathematicians were all ethnic Jews, though after Schoenflies's retirement in 1922 his chair went to Carl Ludwig Siegel, the only non-Jew in this tightly-knit group.

In 1924, Dehn became head of the Frankfurt Mathematics Seminar, a post he held up until 1935 when his position was terminated, forcing him into early retirement. What unfolded under his leadership was a community of scholars who worked together in an atmosphere largely free from the competition and rivalries typical at other leading universities. In Frankfurt, the watchwords were cooperation and harmony. Those idyllic years were later memorialized by Carl Siegel, the community's last surviving member in [42], a lecture he delivered on 13 June 1964 in the Frankfurt Mathematics Seminar.

Soon after his arrival, Dehn decided to launch a private reading circle, a *Lesekränzchen*, that would long be remembered by all who attended. This group devoted its attention to the study of classical mathematical texts in their original languages, in particular Greek and Latin works written by, among others, Euclid, Bombelli, Cavalieri, Kepler, Roberval, Wallis, Huygens, Barrow, Newton, Leibniz, and Euler. The entire mathematics faculty took part in these gatherings as a truly communal undertaking, even though Dehn was its acknowledged *spiritus rector*.

Shortly before the Nazi Party came to power, Dehn gave a lecture to the German Mathematical Society, "Problems in Post-Secondary Teaching of Mathematics" [8]. This gave him the opportunity to speak about some of the unique features of the Frankfurt program, in particular its history of mathematics seminar. Among its several qualities, he laid stress on a humanistic virtue, namely, the sense of humility one gains through a deeper appreciation of the intellectual achievements of one's forebears. "Studying the development of mathematics, steadily, deeply, and without haste together with close colleagues," he wrote, "makes every mathematician more mature and fills him with a more human love of his science." At the same time, Dehn had no illusions about the effectiveness of this special seminar as a teaching tool. Most students lacked the necessary linguistic skills, but even more, the intellectual patience required to delve into difficult texts. He also noted very aptly that mathematical and historical thinking tend to run in opposite directions. Over the course of 10 years, he doubted whether more than a half-dozen students had gained anything of lasting value from the seminar. This telling remark clearly suggests that its true purpose was *Fortbildung*, i.e., cultural enrichment for the faculty and a few older teachers from the surrounding community.

Students were naturally encouraged to participate in the Frankfurt historical seminars as well, though not many possessed the requisite language skills to do so. Dehn thought only a rare few truly profited from the experience. A young astronomy student named Willy Hartner, who later founded Frankfurt's Institute for History of Science, recalled in 1981 how much he regretted never having participated regularly in Dehn's seminar. Hartner possessed the necessary prerequisites—that unusual mixture of philological and mathematical talents—but he admitted that in 1922, the year he first met Dehn, he had not yet discovered his interest in history (he was only 17 at the time). Nevertheless, he shared some vivid memories of the contrasting styles of Dehn and Hellinger as teachers:

> Anyone who, like me, ever heard Ernst Hellinger's differential and integral calculus and other lectures will have remembered well into old age his almost unequaled mastery. Today educational methods are very much in fashion, but I am sure Hellinger never bothered with such theories; with him it was as if a friendly fairy had put that in his cradle.
>
> Max Dehn embodied a completely different type of brilliance. In contrast to Hellinger, he loved to improvise and abandon himself to the overflow of thoughts storming through him. With all due acknowledgment of his mastery, this proved a bit difficult for us, his inexperienced listeners. Feeling very despondent, I asked him for a brief interview. It lasted a good two hours spent in the professors' cafeteria, where one drank miserable inflationary coffee at a price of about a billion marks a cup. I was pleasantly surprised that Dehn responded to my request without any sign of annoyance. The rest of the conversation was about very different things—art, music, languages, classical and modern, about history, and finally also about the political situation. It was the beginning of a lifelong friendship that we preserved in even more difficult times. (Burde, Schwarz and Wolfart [4, pp. 23–24])

Among the students who regularly attended this mathematics history seminar, one in particular stood out from all the rest—Adolf Prag, whose later career in some ways mirrored Dehn's. Not only did Prag's life crisscross with those of Max Dehn and his two daughters, Eva and Maria, but he also went on to play a singular role in historical studies devoted to the mathematics of the seventeenth century.

Prag was born in 1906 in a small village on the edge of the Black Forest, but soon thereafter his family moved to Frankfurt, where he attended the humanistic Goethe Gymnasium. There he acquired a solid grounding in classical languages that he would cultivate throughout his life. From 1925 to 1929 he studied mathematics at Frankfurt University, where he became a mainstay in Dehn's history of mathematics seminar. As Christoph Scriba later imagined the situation:

> Dehn, with his wide historical and philosophical interests, must have sparked a congenial vein in Prag. In addition, the outstanding linguistic abilities of this student, who was able to translate Latin and even Greek texts fluently into the German language, were a welcome asset for the discussions of this circle. [38, p. 410]

During this time, a lifelong friendship developed between Prag and two of Dehn's best students, Ruth Moufang and Wilhelm Magnus.

After completing his studies, Prag still needed to pass the state examination for teaching candidates and submit a thesis (*Staatsexamensarbeit*). For a topic he went to Dehn, who suggested that he write about the Oxford mathematician John Wallis, whose work Prag had studied in the seminar. The resulting thesis was so impressive that Dehn sent it to Otto Neugebauer, who published it in his new series *Quellen und Studien zur Geschichte der Mathematik* [31].[1] Years later, Christoph Scriba took up

[1] The published version, however, omitted a chapter on the Pell equation.

research on Wallis, a project that led to a close personal connection with Prag (see below).

As a Jew, Prag had no chance of gaining a position at a state-run school, so in 1931 he accepted a post at a private Jewish school in Herrlingen (Württemberg) run by Anna Essinger, a remarkable educator. Sensing early on that her undertaking had no future in Germany, she obtained support from Quaker organizations in 1933 to move the school to Kent, England. There at Bunce Court, a large house near Faversham, Prag continued teaching, and he later became deputy head of the school. The two daughters of Max and Toni Dehn attended this same school, where their father also taught from January to April 1938. In the spring of 1937, Frede Warburg, daughter of the well-known art historian Aby Warburg, joined the school staff, and the following year she and Adolf Prag wed. They survived the difficult times that lay ahead and died only months apart 65 years later in 2004. In the final section, I will briefly discuss Prag's singular role in the historiography of early modern mathematics.

Occasionally, visitors attended the Frankfurt historical seminar, one being André Weil, who vividly recalled the impression Dehn left on him:

> A humanistic mathematician who saw mathematics as one chapter—certainly not the least important—in the history of human thought, Dehn could not fail to make an original contribution to the historical study of mathematics, and to involve his colleagues and students in the project. This contribution, or rather this creation, was the historical seminar of the Frankfurt mathematics institute. Nothing could have seemed simpler or less pretentious. A text would be chosen and read in the original, with an effort to follow closely not only the superficial lines but also the thrust of the underlying ideas. . . . It was only later that I attended it, on subsequent visits to Frankfurt, a place I made a point of visiting as often as I could. I am not sure whether it was already in the summer semester of 1926 that, during a seminar session devoted to Cavalieri, Dehn showed how this text had to be read from the viewpoint of the author, taking into account both what was commonly accepted in his lifetime and the new ideas that Cavalieri was trying to the best of his ability to implement. Everyone participated in the discussion, contributing what he could to the group effort. [46, p. 52]

Weil was also very struck by the radically different atmosphere in Richard Courant's Göttingen [46, pp. 52–53]. He recalled, in particular, how he learned very little in conversations with those in Courant's own group. Nearly every time he got talking with one of them, the exchange would end rather abruptly with a remark like, "sorry, I have to go write a chapter for Courant's book" [46, p. 51]. There was a distinct awareness in Göttingen that Max Dehn and Carl Ludwig Siegel, both of whom thought of mathematics as an art form, were cultivating an approach to research in Frankfurt that stood in conscious opposition to the Göttingen model. Siegel's main hobby during these years was painting, especially impressionistic landscapes. After coming to Frankfurt, he lived at first with the painter Fritz Wucherer and his family in Kronberg, a wealthy town in the idyllic Taunus region northwest of Frankfurt. Wucherer owned an impressive villa and belonged to an artists' colony in Kronberg. He was well known for his landscape paintings, and for some time Siegel took lessons from him.

Otto Neugebauer, who served as Courant's "floor manager" at the Mathematics Institute in Göttingen, was certainly sensitive to the implicit criticism coming from

Frankfurt.[2] Neugebauer played a central role in designing the institute's new quarters, built with funding from the Rockefeller Foundation. When it opened in December 1929, Hermann Weyl delivered a lecture honoring Felix Klein, who had long dreamed of housing mathematics in such a building. Neugebauer, on the other hand, was eager to describe the physical arrangements as an inviting place for teaching staff and students to gather and meet. "We hope and believe," he wrote, "that the new mathematics institute will *not* provide new impetus for the "mechanization" of science, as so often prophesied, ... but rather will offer a workplace, where one can *enjoy* teaching and learning and, above all, the pursuit of pure science" [29, p. 4].

Courant's Göttingen was a multi-faceted enterprise, but at its heart flourished a "publish or perish" culture that stood as the antithesis of the one cultivated in Frankfurt. Indeed, one of the striking features of the latter was how little Dehn and Siegel chose to publish once they began working together. This hardly meant that they were unproductive, however; nor did they lack ambition. In fact, their decision to withdraw from this arena stemmed from a shared understanding that "more was not better"—real progress would take place outside the "mathematical factories," which were for producing and disseminating such an abundance of new results that contemporary mathematicians found themselves drowning in their own literature.

André Weil remembered Dehn invoking just this image when he visited Frankfurt around Christmas of 1926. Mathematics, Dehn told him,

> was in danger of drowning in the endless streams of publications; but this flood had its source in a small number of ideas, each of which could be exploited only up to a certain point. If the originators of such ideas stopped publishing them, the streams would run dry; then a fresh start could be made. To this purpose, Dehn and his colleagues refrained from publishing. (Weil [46, p. 53])

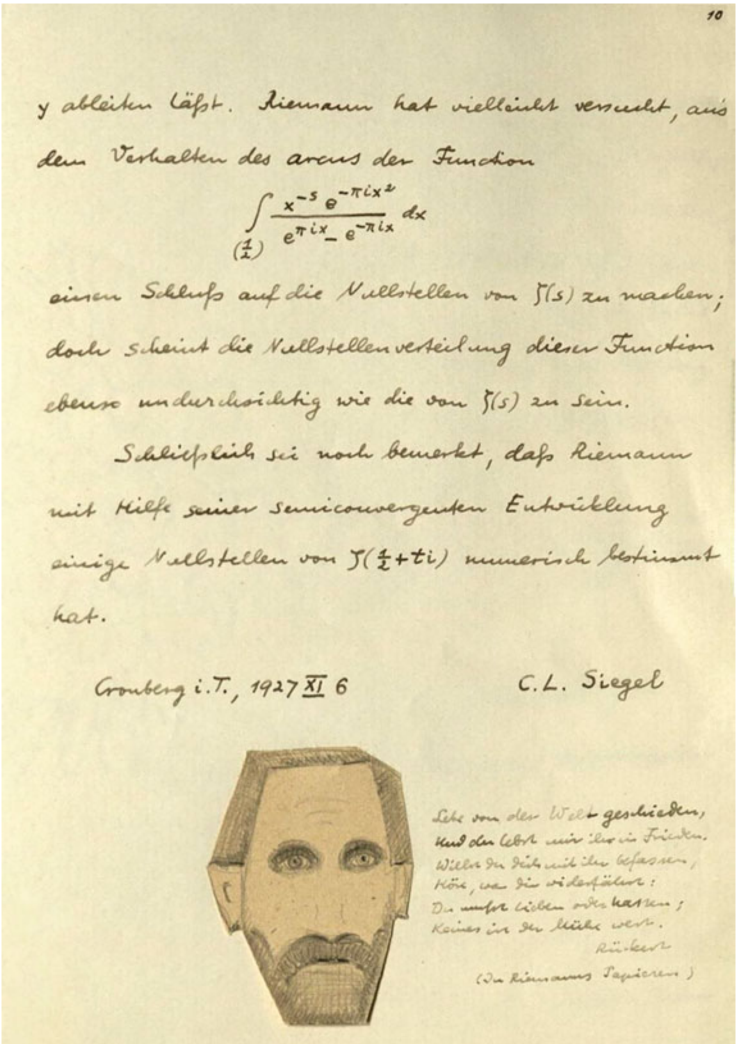
This view probably comes closer to Siegel's attitude than to Dehn's, if only because the latter was a born teacher and collaborator, famous for his generosity in sharing fresh ideas to help others.

Dehn's seminar proved to be deeply inspirational for Siegel, whose singular ability to attack truly formidable problems in number theory was becoming legendary (Yandell [51, p. 208]). He was surely long intrigued with the mysterious results Riemann had communicated in his 8-page paper on the zeta-function, which no one had been able to unravel. With the assistance of his friend, Erich Bessel-Hagen, he set to work studying Riemann's unpublished notes related to the distribution of primes, a question that Riemann's teachers, Gauss and Dirichlet, had studied before him. Siegel worked on this topic, off and on, for several years.

On 6 November 1927, he composed a 10-page manuscript that dealt with Riemann's ideas, though he clearly never intended this text for publication. Instead, he gave it to Max Dehn, no doubt as a birthday present, as Dehn turned 49 on 13 November of that year. At the end of the manuscript, he even added a humanistic touch to fit the occasion. Figure 1 shows Siegel's portrait of Riemann along with

[2] On Neugebauer's early career, see Rowe [34].

some lines from a famous poem, “Friede mit der Welt” (Peace with the World) by Friedrich Rückert,[3] which he found among Riemann’s notes:



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y ableiten läßt. Riemann hat vielleicht versucht, aus dem Verhalten des arcus der Function

$$\int_{(\frac{1}{2})} \frac{x^{-s}\, e^{-\pi i x^2}}{e^{\pi i x} - e^{-\pi i x}}\, dx$$

einen Schluß auf die Nullstellen von $\zeta(s)$ zu machen; doch scheint die Nullstellenverteilung dieser Function ebenso undurchsichtig wie die von $\zeta(s)$ zu sein.

Schließlich sei noch bemerkt, daß Riemann mit Hilfe seiner semiconvergenten Entwicklung einige Nullstellen von $\zeta(\frac{1}{2}+ti)$ numerisch bestimmt hat.

Cronberg i.T., 1927 XI 6 C.L. Siegel

Lebe von der Welt geschieden,
Und der Welt mit ihr in Frieden.
Willst du dich mit ihr befassen,
Höre, was dir widerfährt:
Du mußt lieben oder hassen;
Keines ist der Mühe wert.
Rückert
(In Riemanns Papieren)

Fig. 1 The final page from Siegel’s manuscript on Riemann’s unpublished work on the zeta-function. Dehn Papers, Dolph Briscoe Center for American History, University of Texas at Austin

[3] Rückert’s poetry was set to music by numerous famous composers; best known among these works are the “Kindertotenlieder” in the composition by Gustav Mahler.

Lebe von der Welt geschieden,
Und du lebst mit ihr in Frieden.
Willst du dich mit ihr befassen,
Höre, was dir widerfährt!
Du musst lieben oder hassen;
Keines ist der Mühe wert.

(Live apart from the world,
And you live with her in peace.
Should you want to engage with her,
Hear, what shall befall you!
You must love or hate;
Neither is worth the effort.)

Siegel's research project eventually led to his reconstruction of the Riemann-Siegel Formula, published in *Quellen und Studien* [41]. H.M. Edwards summed up his accomplishment with these words:

> The difficulty of Siegel's undertaking could scarcely be exaggerated. Several first-rate mathematicians before him had tried to decipher Riemann's disconnected jottings, but all had been discouraged either by the complete lack of explanation for any of the formulas, or by the apparent chaos in their arrangement, or by the analytical skill needed to understand them. One wonders whether anyone else would ever have unearthed this treasure if Siegel had not. [12, p. 136]

In January 1928, Max Dehn addressed a large audience at Frankfurt University when he spoke about "The Mentality of the Mathematician" [6], a speech Abe Shenitzer later translated for readers of *Mathematical Intelligencer* [9]. Dehn spoke on a ceremonial occasion, namely the annual celebration of the founding of the modern German nation in January 18, 1871. Since he had to approach this topic from some higher plane, though, he chose to illustrate what he hoped to convey by appealing to history, even going back to ancient times.

Certainly the views Dehn expressed in "The Mentality of the Mathematician" cast considerable light on the speaker's own quite unique way of thinking. His first and most immediate task was to assure his listeners that mathematicians were engaged in a creative activity. For "the layman often thinks that mathematics is by now a closed science, and gives little thought to the origin of the discipline he is familiar with from school." Dehn spoke of the sense of divine inspiration that ancient Greek mathematicians felt after making a profound discovery, and how "Eratosthenes and Perseus, in the manner of winners in an Olympic competition, made votive offerings out of joy at attaining their goals." Turning to early modern times, he talked about Cardano's wild urge to work out all the various types of solutions of cubic equations in his *Ars Magna*, but he also made clear that mathematical knowledge had to be clarified and communicated to have a decisive impact. This was particularly evident in the case of Descartes, who fashioned himself as having made a great new discovery—a method for systematically solving geometrical problems by reducing them to algebraic equations—when, in fact, he had mainly brought forth a known method with exceptional clarity.

Dehn's admiration for Descartes' accomplishments did not extend to his person, however, as this great French thinker was extremely impressed by his own sense of superiority and gloated over what he had accomplished. For Max Dehn, Gerolamo Cardano was a far more sympathetic figure, as can be seen from this passage:

> Cardano, who died in 1576 at the age of 75, was a typical man of the Renaissance. In view of our present topic—the creative power of the mathematician—Cardano is of special interest to us. His productivity was unbelievably extensive. Ninety years after his death, ten large folios of his work appeared, and the publisher assured readers that this was only half of what Cardano had written. There is no area between heaven and earth that he left untreated. He wrote about all the natural sciences, medicine, astrology, theology, philosophy and history. His autobiography—which Goethe compared to Benvenuto Cellini's—has great charm. In it he describes with touching ingenuousness a life afflicted with manifold misfortunes. At times we are strongly reminded of Rousseau's *Confessions*. Goethe writes at length about Cardano in his history of the science of color—about his talent, his passion, his wild and confused state that always comes to the fore .... [9, p. 20]

Turning to Dehn's seminar, one can easily see that the choice of texts was largely confined to classical antiquity and the period in early modern Europe leading up to the emergence of the calculus in the works of Newton and Leibniz.[4] Siegel thus recalled spending a number of semesters studying works by Euclid and Archimedes. Another block of texts dealt with developments in algebra and geometry from Leonardo of Pisa and Cardano to Viète, Descartes, and Desargues. Finally, the seminar looked carefully at texts documenting the emergence of infinitesimal calculus over the course of the seventeenth century, especially key authors associated with the British tradition: Wallis, Gregory, Barrow, and Newton. This overall plan was thus entirely conventional; yet even so, knowing in advance what one expected to find in an older mathematical text was usually of little help when it came to reading and *actually understanding* such works *in detail*.

Some three decades later, when Carl Siegel returned to Frankfurt to speak about the times he shared with his former colleagues there, he had this to say about their history of mathematics seminar:

> As I look back now, those communal hours in the seminar are some of the happiest memories of my life. Even then I enjoyed the activity which brought us together each Thursday afternoon from four to six. And later, when we had been scattered over the globe, I learned through disillusioning experiences elsewhere what rare good fortune it is to have academic colleagues working unselfishly together without thought to personal ambition, instead of just issuing directives from their lofty positions. [42, p. 226]

[4] Protocol books from Dehn's seminar are in the possession of the Frankfurt University Archives.

3 Dehn on the History of Geometry

Max Dehn's contributions to the literature on history of mathematics came mainly in the form of essays and occasional articles. His single most impressive piece was a six-part appendix to the third edition of Moritz Pasch's classic monograph *Vorlesungen über die neuere Geometrie* [30]. The second edition of Pasch's book, published by Teubner in 1912, had long been out of print. During the postwar era, after Springer assumed Teubner's former role as the leading German publisher of mathematical texts, Courant's "yellow series" often published older standard works in an updated form. Pasch was already approaching 80, so he was in no position to produce a substantially new edition, but Courant was surely more than pleased when Dehn agreed to write an appropriate supplementary appendix.

Some 5 years later, Courant turned once again to Dehn to request a supplement for a new edition of Arthur Schoenflies' textbook on analytic geometry [35]. Dehn's six appendices to [7, pp. 298–411] not only offered an overview of foundations and a modern treatment of linear algebra, it also contained a brief historical overview as well as a section on still unsolved problems in analytic geometry. In short, this material made the book far more than simply an elementary textbook. Here, as well as in the case of Pasch's book, Dehn drew on material he had developed for his courses in Frankfurt. This circumstance is reflected in his preface to Pasch [30], where he wrote: "The appendix corresponds approximately to a two-hour, one-semester lecture course, in which the instructor reports on what he considers to be all the more important questions, discussing the most important problems in detail, and above all seeking to stimulate independent study and the reading of classical works" [5, p. viii].

Among the classics in the history of geometry that Dehn had in mind, two were preeminent: Euclid's *Elements* and Hilbert's *Grundlagen der Geometrie* [18], which in 1922 was published in a 5th edition containing several new supplements. As for the significance of Pasch's original text from 1882, Dehn described this as marking the end of a quest to derive projective geometry from purely elementary principles, formulated in a complete system of axioms that avoids appealing to congruence properties or notions of continuity, such as the Axiom of Archimedes [5, p. 188]. Hilbert's axiom system, on the other hand, stood closer to the original system of Euclid, which made it possible to analyze which parts of geometry were susceptible to an elementary treatment and which were not.

Dehn's approach in this survey was largely systematic, though he added footnotes containing brief historical remarks coupled with references. The first question he raises is the role of the parallel postulate in ancient Greek geometry, a problem compounded by philological difficulties. In most of the extant manuscripts this postulate appears under the "common notions," which textbook authors usually referred to as axioms to distinguish these from the strictly geometrical postulates. The "parallel postulate" was then given as Axiom 11 in these texts (in the English tradition, following Robert Simson, it was Axiom 12). The Danish historian of mathematics Johann Heiberg argued that this was due to the editorial intervention of Theon of Alexandria who, according to Heiberg, had removed the fifth postulate from its original position and placed it under the "common notions." In preparing the modern Greek/Latin edition, Heiberg restored the postulate to what he believed was its original place. He found it listed as the fifth postulate in an older non-Theonine manuscript housed in the Vatican Library, which he took as his principal *Urtext* in preparing the modern edition. The English translation published afterward by T.L. Heath [17] follows Heiberg's edition almost without exception. In Dehn's day, these were very recent events, though today few realize that the parallel postulate has only been called Euclid's fifth for little more than a century. In several places, when discussing Greek mathematics, Dehn made similar comments about difficulties arising from a dearth of historical source material.

The logical or mathematical status of the parallel postulate long remained one of the most famous of all geometrical mysteries. Pasch's work put the last touches on projective geometry, a theory in which the properties of parallel lines play no role. Alongside those developments, however, more subversive thinkers—Lobachevsky and Bolyai—staked out arguments for a new theory of geometry in which parallel lines no longer satisfy Euclid's fifth postulate. Although it took several decades for mathematicians to embrace non-Euclidean geometry, once they did so, the contingent status of the parallel postulate became clear: Euclidean geometry was only a special case. Indeed, among the infinitely many possible spaces of constant curvature, Euclidean geometry was the one in which that constant was zero. Dehn's discussion took up the connection between non-Euclidean geometries and projective geometry, an insight Felix Klein recognized once he learned about the possibility of obtaining a general projective metric, a technique Arthur Cayley used to derive Euclidean geometry. Dehn also briefly noted how Riemann's notion of a manifold with local curvature properties led to the natural question of the various possible global extensions, a problem that led to Clifford–Klein space forms.

Dehn sketched these various topics quite rapidly before turning to problems underlying the foundations of projective geometry. Here he focused on the difficulty of providing a logically sound and complete construction of coordinate systems in projective geometry. Dehn distinguished between an older, more intuitive approach that depended on the Archimedean axiom and the purely projective methods developed by Pasch. From Desargues theorem—which follows immediately from the incidence axioms for points, lines, and planes in space—one can easily generate a network of rational points in the plane by iterating the construction of a fourth harmonic point for every triple. Pasch then found a way to extend this construction to irrational points by invoking a projective substitute for the Archimedean axiom.

These brief remarks then led over to Dehn's main topic, which begins with a modernized account of Hilbert's approach to segment arithmetic based on the two lines theorem of Pascal (Pappus's theorem) and the theorem of Desargues. His treatment of these, however, draws on elementary group theory for geometric transformations, leading to a proof that the fundamental theorem of projective geometry entails both theorems, Pascal as well as Desargues. Dehn also gave a proof of Hessenberg's theorem, namely that the planar theorem of Desargues follows from Pascal's. The individual achievements of others (Staudt, Wiener, Hilbert) are only mentioned in a footnote, and Dehn caps off this section with a schematic chart providing an overview of the relative dependence of the various axioms and fundamental theorems. All of this reflects Hilbertian interests, except for the appeal to group theory, where for details he points to Schwan [37]. The author of this study was a Gymnasium teacher in Düsseldorf, who went on to write his dissertation under Max Dehn.[5]

Following this overview, Dehn presents a section containing proofs of the key theorems. He emphasizes that one must first prove Pascal's theorem without recourse to continuity, and he begins with a synopsis of the original proof given by Friedrich Schur in [36]. This proof made essential use of a beautiful idea first discovered by Germinal Pierre Dandelin in connection with conics that lie on a hyperboloid of one sheet, thus a quadric surface generated by two systems of lines. Dandelin showed that a spatial hexagon obtained by connecting 6 points along corresponding generators, as these alternate between the two families, leads to a so-called Brianchon point, the common intersection point of the 3 diagonals.[6] The dual incidence relation follows as well, and taking a plane section of the quadric then leads to a conic with an inscribed hexagon that satisfies Pascal's theorem. Dehn not only credited Hermann Wiener with having brought out the significance of the theorems of Desargues and Pascal for foundations of geometry, he also emphasized how Schur's proof of Pascal's theorem was inspired by Dandelin's older ideas. These enabled Schur to prove the two-line version of Pascal's theorem, the case required for a commutative segment arithmetic (Dehn [5, pp. 228–232]).

Turning back to his earlier discussion of Euclid's *Elements*, Dehn underscored what Schur had achieved, namely the very first purely synthetic introduction of a segment arithmetic without any appeal to continuity or the parallel postulate. He thought this work, and not Saccheri's, could more fittingly have borne the title "Euclidis ab omni naevo vindicatus" (Euclid freed of every flaw). A century earlier, the English mathematician Henry Saville had pointed out two major flaws in the classical presentation: the opaque use of the parallel postulate in Book I and the glaring break in Book V, where Euclid inserted a general theory of ratio and proportion before applying it to develop the theory of similar rectilinear figures in Book VI. The cornerstone concept in Book V was the famous Definition V.5 that provides a theoretical criterion for determining when two ratios will be equal. Euclid merely needed to invoke that definition once, in the first proposition of Book VI, after which everything fell easily into place.

Dehn seemed to be saying that this historical development—from Saville to Saccheri and Lambert, passing through the discovery of non-Euclidean geometry and Pasch's grounding of projective geometry, and then the rigorous coordinatization of elementary synthetic geometry with Schur's work—represents a story that was already essentially closed when Hilbert stepped onto the scene. What

[5] Wilhelm Schwan, "Extensive Größe, Raum und Zahl," Diss. Frankfurt University, 1923.

[6] Pictures illustrating this argument for this case of Brianchon's theorem can be found in Hilbert and Cohn-Vossen [19, pp. 92–93].

he wrote immediately afterward, though, fully clarifies why Hilbert's *Grundlagen der Geometrie* occupies such a significant place in this chain of developments. Indeed, in surveying what had transpired up until 1899, Dehn described the series of highways and byways that led to important stations, but in such a complicated fashion that one could hardly view these as more than a collection of significant results that fell well short of constituting a unified theory. Hilbert, on the other hand, was the first to recognize the validity of "exotic geometries," as for example, plane geometries in which the theorem of Desargues fails to hold. This finding went hand in hand with one of his central insights: *The validity of the plane theorem of Desargues is the necessary and sufficient condition for deciding whether the plane can be embedded in space.* Schur's proof of the Pascal theorem made essential use of spatial geometry, whereas Hilbert sought to reveal the possibilities for building a theory of geometry in the plane by exploiting the power of the parallel postulate. After spelling out this motivation, Dehn proceeded to give Hilbert's planar proof of Pascal's theorem.

In the closing section on projective geometry, Dehn describes some of the simple consequences of arithmetization, illustrating the theorems of Desargues and Pascal by means of incidence configurations for points and lines in the plane. Hilbert sometimes called these closure theorems, since they lead to closed figures that lie in special position in the plane. The Desargues theorem leads to a $(10, 3)$ configuration, whereas Pascal is a $(9, 3)$ (thus 9 points and 9 lines that are incident in triples). In the first case, one has 30 linear equations, three for each of the 10 lines whose equations are satisfied by substituting the coordinates of the 3 points that lie on them. But since these linear relations are not independent, translating the theorem into algebra leads to the result that one can deduce the final relation from 29 of them. Similarly for the Pascal theorem, as both are examples of *Schnittpunktsätze*, as Hilbert described in *Grundlagen der Geometrie*. In this setting, duality follows immediately from the fact that point and line coordinates enter symmetrically in systems of linear equations.

In the remaining parts of his survey, Dehn took up several topics closely related to Hilbert's researches as well as his own. He addressed here the problem of proving the absolute consistency of an axiomatic system, as Hilbert long claimed must be possible. Like Henri Poincaré before him, Dehn doubted that the principle of complete induction could ever be reduced to a consistency argument (Dehn [5, pp. 260–262]). The focal point of the Hilbert–Bernays program to formalize mathematics was their effort to prove the consistency of the axioms for arithmetic. Hilbert regarded this as the first step toward solving his second Paris problem, which required doing the same for the real numbers. Thus, by the mid-1920s, Dehn publicly doubted the feasibility of Hilbert's formalist program. At the time he spoke in Frankfurt, L.E.J. Brouwer was trying to topple formalism, while pressing for a new approach to foundations based on his philosophy of intuitionism. Only 2 years later, Kurt Gödel would demonstrate the power of Hilbert's proof theory by using it to demonstrate that the consistency of arithmetic was a formally *unprovable* proposition.

Pasch had no time to study Dehn's essay in any detail when he received the page proofs; his failing eyesight likely hindered him from doing more that glancing through the text. Still, he sent his congratulations to Dehn, while expressing his delight over the sheer volume of material his survey contained as well as the careful handling of it.[7] He only added his wish that Dehn somewhere mention the term "Pasch's Axiom" in his text, since several writers had used this terminology. Hilbert himself had acknowledged in a footnote that Axiom II.4, the last among his axioms of order, was first introduced by Pasch in 1882.

Dehn's survey was intended as an overview of historical developments from antiquity to modern times, not, of course, as a detailed historical study. In all likelihood, he wrote this text without having to undertake any substantial amount of research. After all, this topic had long been an integral part of his own teaching and research. As noted earlier, he viewed the Frankfurt reading circle as a vehicle for intellectual enrichment, not as a training ground for future historians of mathematics. One of his star students, though, continued in that direction, a largely unknown story with some surprising wrinkles.

4 Historical Studies on Leibniz and Newton

Adolf Prag never lost his passion for history of mathematics, and in some symbolic sense one could say that Prag played an important role in resolving one of the most contentious issues in the history of mathematics. This concerned the famous priority dispute over the invention of the calculus between the followers of Newton and Leibniz.[8] In fact, this was only one of an entire series of conflicting issues that divided Newton and Leibniz, who held starkly opposing views regarding God's place in the world He created. Leaving all else aside, it remains difficult to say whether Prag took great interest in the calculus controversy, which was both prolonged and vicious. Like Max Dehn, he took a deep interest in the British tradition, but there seems to be no evidence that the Frankfurt seminar paid close attention to the latest iteration of the Newton vs. Leibniz squabble in contemporary revisionist literature. Prag only entered this story through a back door decades after World War II. To appreciate the context, though, requires glancing further backward to the years after the First World War.

First, though, a few words about the circumstances that led to this controversy. Newton was a secretive and mistrustful personality, so very few knew anything about his early mathematical work from the mid-1660s, including those parts related to the calculus. This was still the case when Leibniz visited London in

[7] Moritz Pasch to Max Dehn, 7 July 1926, Dehn Papers, Dolph Briscoe Center for American History, University of Texas at Austin.

[8] To be sure, historians have continued to grapple with the issues at stake in this conflict up to the present day. A particularly thoughtful analysis can be found in Guicciardini [15, pp. 329–384]. See also Westfall's account in [48].

the mid-1670s. After he heard something about this work, Leibniz made inquires, which Newton answered in two letters. These passed through the hands of Henry Oldenburg, secretary of the Royal Society, and they would later be used as evidence against Leibniz. He and Newton never personally met, so this incidental exchange evidently went unnoticed at the time.

A decade later in 1684, when Leibniz published the basic rules for the differential calculus, he made no mention of Newton's mathematical work, which still remained unpublished. Three years later, Newton published his *Principia*, which required mathematical methods rooted in calculus. Newton could have derived some of the main results using his theory of fluxions, but if he did so, he left no trace of this in the text. Instead, he dressed up all his results in a geometrical garb, which avoided infinitesimals by invoking the method of first and last ratios. However, hints that Newton might have anticipated Leibniz's invention began to surface in the late 1690s. Around that time, insiders gradually learned that Sir Isaac claimed ownership of the essential methods Leibniz had put into print. These rumors eventually turned into charges of foul play, and in 1712 a committee of the Royal Society, under Newton's presidency, undertook an investigation of the matter. Its report (prepared essentially by Newton himself) concluded, not surprisingly, that Leibniz stole the calculus from Newton and later pretended that he alone had invented it. Since the events surrounding this whole story have been dealt with many times and are far too complicated to describe here, let me skip over them entirely in order to describe a later chapter in this controversy, one which has only rarely been discussed in the historical literature.[9]

Interest in the conflict between Newton and Leibniz had much to do with the fact that partisans for the two sides drenched the matter in blatantly nationalistic rhetoric. This aspect was hardly forgotten when the old debate broke forth in a new form soon after the Great War ended. In 1920, the English mathematician J.M. Child leaped into the fray after undertaking a careful study of the work of Isaac Barrow, who preceded Newton as the first Lucasian Professor of Mathematics at Trinity College, Cambridge. Since most of the relevant texts from the period were in Latin, Child published English translations in [25], together with critical remarks directed at the commentaries written by Carl Immanuel Gerhardt, the nineteenth-century editor of Leibniz's mathematical writings. Gerhardt discovered Leibniz's own account of his path to the calculus, "Historia et origo calculi differentialis," which he published already back in 1846. This marks the beginning of Gerhardt's efforts to reclaim Leibniz's place in the history of mathematics, work that Child sharply criticized. Whereas Newton and his acolytes had charged Leibniz with appropriating Newtonian methods, Child disagreed with this claim, arguing that the brilliant German had instead obtained his main ideas from Isaac Barrow.[10] Child's

9 For a detailed account of the original controversy, see Hall [16].

10 An attempt to support the case of Barrow was made in Feingold [13]. For an analysis of more recent scholarship relating to the possibility that Leibniz was influenced by Barrow's work, see Probst [32].

translations of the "Historia et origo" and other texts relating to Leibniz's early works received praise in the English-speaking world, although David Eugene Smith made a point of condemning the polemical manner in which Child defended his claims in support of Barrow [43].

In Germany, on the other hand, leading scholars associated with the ongoing Leibniz Edition in Hannover sought to refute Child's claims, some of which were based on speculation. To a large extent, Child was forced to argue in the dark, owing to the fact that he had no knowledge of Leibniz's writings beyond those contained in Gerhardt's publications. The latter had alluded to notes Leibniz had made in his copy of Barrow's *Lectiones Geometricae*, but Child had no access to this book when he published [25]. Heinrich Wieleitner recovered Leibniz's copy of Barrow's *Lectiones* along with a number of other manuscripts that Gerhardt had overlooked. Wieleiter urged Dietrich Mahnke, then a young philosopher who had studied under Husserl and Hilbert in Göttingen, to investigate these sources. This led to Mahnke's *Habilitationsschrift*, published as [28], which contained a lengthy rebuttal of Child's claims. One year later, Wieleitner habilitated in history of mathematics at Munich University. One of the theses he proposed to defend at that time read: "It is totally unjustified to accuse our Leibniz of untruthfulness (or even only forgetfulness) in regard to the reporting about the course of his invention of the differential and integral calculus" (Hofmann [20, p. 211]).

At his death in 1716, Leibniz left behind a vast collection of writings and correspondence. As part of an effort to organize these documents for publication, the Berlin Academy established a Leibniz Commission with leading figures from a variety of fields, including the physicist Max Planck, the mathematician Ludwig Bieberbach, and the philosopher Nicolai Hartmann. Mahnke and Conrad Müller were appointed as mathematical editors. Shortly before the outbreak of the war, however, the Academy aligned itself with the government by appointing Theodor Vahlen, a high-level Nazi mathematician, as its president (Thiel [44]). Vahlen was a natural choice, owing to his close alliance with Ludwig Bieberbach, secretary of the Academy's Mathematics and Natural Sciences Division.

Three years earlier, Bieberbach had founded the journal *Deutsche Mathematik* with the support of government funds. Both he and Vahlen took considerable interest in promoting Leibniz's mathematical reputation. After Mahnke was killed in a car accident in 1939, Bieberbach invited Joseph Ehrenfried Hofmann to take Mahnke's place in the project. Vahlen even appointed him head of the entire work group in Berlin, thereby signaling that publication of Leibniz's mathematical work and correspondence held highest priority. Soon thereafter, Hofmann published an essay in *Deutsche Mathematik* setting forth his approach to history of mathematics [21].

Together with his wife Josepha, Hofmann worked on the Leibniz papers up until 1943, when their house in Berlin was destroyed in a bombing raid. Thereafter, they moved to the small town of Ichenhausen in the Swabian region of Bavaria. Hofmann lost most of his private library, but not the manuscript material in his possession. After the war, the Berlin Academy terminated Hofmann's position with the Leibniz Edition, though he refused to accept this decision or to cooperate with the new staff charged with editing Leibniz's mathematical papers and correspondence. With exclusive access to this material, Hofmann began writing a monograph that gave the first detailed account of Leibniz's early mathematical journey during the years 1672–1676. Although completed in 1946, this text was first published 3 years later in [22]. Seen against the backdrop of earlier events, this book represents the (almost) final response of German historians of mathematics to charges that Leibniz owed a major intellectual debt either to Newton or to Barrow. It takes little imagination to realize, however, that practically no one living in Germany in the year 1949 would have been interested to read about such arcane matters. Later, though, following the resurgence of interest in history of mathematics in the 1960s and 70s, Hofmann's work found many readers, thanks in large part to the efforts of Adolf Prag. Through his translation of [22] into the updated English edition [23], Prag played a major role in making this story available to a wider audience.

After the war ended, Hofmann maintained his ties with Bieberbach, who lost his professorship in Berlin. As a notorious spokesman for Nazi principles, Bieberbach's fate was sealed the moment his case came up during the denazification procedures. Others, on the other hand, came away unscathed. Freiburg's Wilhelm Süss, who had assumed a leadership role in the German mathematical community during the Nazi era, was quickly reinstalled in his former professorship. During the period under French occupation, he converted his former center for war-related research in the Black Forest into a conference center, today the internationally renowned Mathematics Research Institute in Oberwolfach. Hofmann enjoyed good relations with Süss, who in the wake of the war invited him to spend several months in Oberwolfach writing his book on Leibniz's mathematical development [22]. He also supported Süss's main project at the time, namely preparation of a volume on pure mathematics for the series FIAT Reviews of German Science (Remmert [33, pp. 142–145]).

Beginning in 1954, Hofmann organized yearly workshops on the history of mathematics in Oberwolfach. Many who attended these were senior mathematicians or teachers at secondary schools, but they also attracted young historians. One of these was Christoph J. Scriba, who eventually became Hofmann's collaborator and later his successor as a workshop organizer. Scriba also served as a key figure for building bridges to England, where he spent 2 years as a post-doc in Oxford during the early 1960s. During his stay, he struck up a warm friendship with Adolf Prag. Since Scriba's research project was devoted to studying the papers and correspondence of John Wallis, he clearly had good reason to make this personal connection, having read Prag's paper on Wallis [31]. In 1965, he invited Prag to attend a workshop on history of mathematics in Oberwolfach, and in subsequent years Adolf Prag was often among those who participated at these events. During one of these meetings in the 1960s, he and Hofmann discussed a plan to bring out an English translation of [22].

After his retirement from teaching in 1966, Prag lived in Oxford, which gave him the chance to work in the Bodleian Library. He had already struck up a friendly cooperation with Tom Whiteside, who was beginning work on his voluminous edition of Newton's mathematical papers. Prag had served as an external examiner for Whiteside's Cambridge doctoral exam, which took place in 1959. The latter's thesis on "Patterns of Mathematical Thought in the Later Seventeenth Century" [49] fell directly into the field of studies that Prag first took up more than three decades before in Frankfurt under Max Dehn. He now began to play an important supporting role in Whiteside's work, which led to the publication of *The Mathematical Papers of Isaac Newton* in eight volumes [50].[11]

When the first volume appeared in 1967, Prag brought a copy with him to Oberwolfach, just as he did in 1981 when he spoke there about the eighth. In the preface to that final volume, Whiteside wrote:

> It is wholly just that my old friend and colleague Adolf Prag endures to share the title-page of this final volume of Newton's mathematical papers with me. In his seventies he remains the ever-willing, near omniscient helper that he has always been, and without his furnishing and correction of a wide spectrum of matters literary, technical and historical this edition would have been much the poorer in its detail. (Scriba [38, pp. 409–410])

Whiteside's former mentor, Michael Hoskin, played a major part in lining up funding for this Newton project, but also in persuading Cambridge University Press to publish it. Once it was underway, he perhaps also had a hand in the delicate negotiations with CUP over the translation of Hofmann's biography of Leibniz. This was only completed the year after Hofmann died in 1973. Thus, Adolf Prag not only served as a kind of ambassador for Whiteside's Newton during his trips to Germany, his translation [23] gave the English-speaking world a full account of what German scholarship had to say about Leibniz's early mathematical career.

André Weil, one of those who read it very carefully, had also attended Max Dehn's Frankfurt seminar in the 1920s. When he reviewed the book for the American Mathematical Society, however, his words of praise were mixed with a general sense of disappointment. For Weil, the charges leveled against Leibniz had been refuted long ago, which left him puzzled why Hofmann wrote at such length about the priority debate:

> Perhaps the reader of this volume would have been spared a great deal of dull material if the author, at the outset, had made up his mind whether to write the "grand synthesis" he seemed to promise us or to appear as the lawyer for the defense in the absurd prosecution for plagiarism launched against Leibniz in the early years of the eighteenth century by Sir Isaac's sycophants and eventually by Sir Isaac himself. Even if there could ever have been a case against Leibniz, C.I. Gerhardt's excellent publications seemed to have closed it long ago. But we find Hofmann constantly on the defensive . . . [45, p. 680]

Naturally, Weil took no interest in the nationalistic motives on both sides of this controversy, but how else can one explain all the ink various writers have spilled over a peculiar priority dispute? In the book's preface, Hofmann indicated how he realized, after Prag and Whiteside had approached him with idea of preparing a translation, "that the original text would require thorough revision" [23, p. ix]. Yet, as Weil rightly pointed out in his review, the text itself is virtually identical to [22],

[11] In his obituary for D.T. Whiteside, Niccolò Guicciardini duly noted Prag's importance for the success of this momentous undertaking [14, p. 5].

the original German version. André Weil clearly preferred Max Dehn's approach to history, which took a loftier view of earlier mathematical accomplishments rather than dwelling on petty squabbles over intellectual property rights.

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