The Practical Theorist: Sommerfeld at the Crossroads of Mathematics, Physics and Technology

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Résumé : Le développement de la physique théorique en Allemagne dans une discipline scientifique pendant les deux décades avant la guerre de 1914 est analysé avec le cas de Arnold Sommerfeld. L'étude se base sur sources primaires qui ont été ouvri à l' éxploitation dans un projet éditorial récemment.

Abstract: The emergence of theoretical physics in Germany as a discipline of its own right during the two decades before the First World War is analysed, using the case of Arnold Sommerfeld as a probe. The study is based on primary source materials and results from a recent editorial project.

Zusammenfassung: Die Entwicklung der theoretischen Physik in Deutschland zu einem eigenständigen Fach während der beiden Jahrzehnte vor dem Ersten Weltkrieg wird am Beispiel Arnold Sommerfelds untersucht. Die Studie basiert auf Primärquellen, die im Rahmen eines kürzlich beendeten Editionsprojekts erschlossen wurden.

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The emergence of theoretical physics—historiographic problems and new approaches

Much has been written on the origins of theoretical physics: I mention Rudolf Stichweh's sociological treatise (Stichweh 1984), Christa Jungnickel and Russell McCormmach's two-volume-account (Jungnickel/ McCormmach 1986) or Elizabeth Garber's recent monograph (Garber 1999); we could add the literature commemorating the past century of physics, such as a 1995 conference proceedings (Hoffmann et al. 1999), or a three-volume-treatise with reviews on the subfields of physics, written mostly by distinguished physicists (Brown et al. 1995).

Despite such a wealth of literature, understanding the emergence of theoretical physics remains a challenge. Although it is obvious that the formation of new scientific disciplines depends on the specific national cultures in which they are rooted, there has been no effort to analyse the emergence of theoretical physics in a comparative study for various countries. There is some agreement that the German case deserves particular interest, but I find it difficult to detect a common expert opinion on the most important factors which prompted the emergence of theoretical physics in Germany as a discipline of its own right, and when this process happened. In view of the underlying problem of defining the displinary identity of theoretical physics it is perhaps futile to expect unanimity. Nevertheless, even with the limited focus on the German situation, it is worth to strive for a deeper understanding of how and when theoretical physics acquired the stature which has lent it the accolade of a „Jahrhundertwissenschaft“ (Hermann 1993). Most historians of physics discern the advent of quantum and relativity theory as the pivotal developments which happened during the two decades before World War I–a period which is absent in Stichweh's book, and which is treated rather briefly at the end of Garber's treatise. Jungnickel and McCormmach provide a host of material, but here, too, the emphasis is on the nineteenth century.

In this paper I use the case of Arnold Sommerfeld as a probe to explore the beginnings of theoretical physics in Germany during the two decades before the First World War. But before I explain why I regard Sommerfeld a suitable candidate for such an inquiry a few remarks should be made about the character of theoretical physics around 1900. Scientific disciplines, from a social historical perspective, are institutions which claim academic territories, attribute priviledges and responsibilities, and justify demands for ressources (Meinel 1987). According to such criteria, by the 1890s theoretical physics may be called a discipline at least in Germany, where 12 out of 20 universities had professors for theoretical physics, offering special lectures on physical theories such as the theory of electricity and magnetism, or the theory of heat (Jungnickel/McCormmach 1986, vol. 2, table 1 and 2, pp. 161-165).

A closer look into the institutional settings of theoretical physics around 1900, however, reveals that its pillars were not always founded on solid ground: In Munich, for example, a new chair for theoretical physics was created in 1890 for Ludwig Boltzmann, but when Boltzmann left Munich four years later, the chair was abandoned and the financial ressources distributed among other disciplines. Theoretical physics in Munich was resuscitated only in 1906, when Sommerfeld became Boltzmann's successor (Eckert/Pricha 1984).

The new academic province also lacked an epistemic identity. With the boundaries between physics and mathematics blurred, mathematicians also claimed physical theorising as part of their own domain. In his famous Erlangen speech in 1872, the mathematician Felix Klein had coined the notion of "physical mathematics" to denote the specific use of concepts from physics in mathematics (Rowe 1985). Klein's idol in this regard was Bernhard Riemann, for whom physics had been an obvious starting point of mathematical work. Potential theory, with its mathematical analog in the theory of complex functions, served Klein as an example to illustrate this approach (Klein 1892).

The relation between physics and mathematics around 1900 has been explored in a number of recent studies, such as on Hermann Weyl (Sigurdsson 1991), Max Born (Staley 1992), David Hilbert (Corry 1997) and Hermann Minkowski (Walter 1999). Klein, Hilbert, Minkowski, Born and others spent most of their careers in Göttingen, apparently a place of budding mathematical and physical productivity. Arne Schirrmacher, in his forthcoming work on the Göttingen „milieu" during the two decades before the First World War, explores the „social space between discipline and scientists" (Schirrmacher 2003a). Another new effort to shed light on how theoretical physics in Germany became flourishing in this period is made by Suman Seth in his forthcoming thesis, with an emphasis on the problems with which the nascent discipline was concerned (Seth 2003).

This study on Sommerfeld provides another case which illustrates the emergence of theoretical physics in Germany along heretofore unexplored trajectories. Sommerfeld's disciplinary roots grew in mathematics and mechanics, with ramifications into technology, before he became professor of theoretical physics at Munich University in 1906. Here, Sommerfeld became the respected teacher of many twentieth century theoretical physicists. Research schools have been recognized as units of analysis in the study of many new scientific disciplines (Geison/Holmes 1993). Founders of research schools, like Sommerfeld, therefore deserve our particular scrutiny. Although my focus in this study is limited to the roots of Sommerfeld's school before the First World War, its subsequent story amply confirms its role as a leading centre for theoretical physics in the first half of the twentieth century (Eckert 1993).

My account is based on primary source material. I will proceed chronologically and follow Sommerfeld's career from his first steps as a Privatdozent in Göttingen in the 1890s until he was a renowned theoretical phyicist about twenty years later. This is the period which we have covered in the first volume of Sommerfeld's scientific correspondence, from which I will draw most of the material in this article (ASWB, I). Although he became most famous for the elaboration of Bohr's model during and after World War I, Sommerfeld was regarded as a representative of theoretical physics well before the boom of atomic theory. How Sommerfeld's career during these crucial years changed from mathematics to theoretical physics, therefore, may illuminate important aspects of disciplinary formation, beyond the obvious biographical interest.

Mathematics

Arnold Sommerfeld was born in 1868 in the East-Prussian town Königsberg, where he went to school and spent his entire study period at the university. He studied mathematics, physics, and other natural sciences. In 1892 he accomplished his studies with a doctoral degree in mathematics and the state exam which qualified him to become a high school teacher—then almost the only profession which awaited a student of mathematics or physics in Germany. Sommerfeld was ambitious enough to hope for an academic career. He seized the first opportunity and became assistant of a mineralogist in Göttingen in 1893, then on the verge of becoming a Mecca of mathematics. Theoretical mineralogy seemed not too far away from mathematics, so that Sommerfeld hoped to pursue his true interests besides his duties in the mineralogy institute.

It took only weeks for the 25-year-old Sommerfeld to become aware that his hopes were frustrated. In his spare time he attended the advanced lectures of the great Göttingen mathematician, Felix Klein, but this only intensified his awareness of being in the wrong place in the mineralogical institute. He wrote to his mother that his time was spent with tedious business only, such as measuring crystals and proof-reading of his professor's new textbook on mineralogy. In June 1894, after a little more than half a year, he longed for the time when this "mineralogical killing of time,"[1] as he described it, would end. By this time he had already won Klein's interest, who offered him the prospect of becoming his assistant. Whenever Sommerfeld wrote home about the experiences in Klein's seminar he was enthusiastic:

> Yesterday I was with Klein; I am filled with ideas. He presented me with magnificent problems, unfortunately he asked me to present a seminar talk again next Tuesday. He is a great fellow, we have an excellent relationship with another. He really would like to make me a mathematical physicist. [2]

After one year, the "mineralogical killing of time" ended and Sommerfeld became Klein's assistant. He also had an offer by the theoretical physicist, Woldemar Voigt, but Sommerfeld regarded himself as a mathematician, and so he declined this offer—because, as he wrote to his parents, "I do not want to find myself again in an awkward position" and deal with things "which I do not regard completely as my task."[3]

As Klein's assistant, too, he was not completely free to do what he liked, but he soon adopted Klein's goals as his own. Klein had far-reaching interests, both in science and in science politics. Klein was eager to demonstrate that mathematics had a larger role to play in a modern society, and that Göttingen should become the Mecca of mathematics. [4] In 1893, he succeeded attracting Hilbert as professor of mathematics to Göttingen. By this time, he also began to reform mathematics instruction in high schools; for this purpose, he presented special lectures for mathematics teachers, such as on elementary geometry and on the theory of the top. When Sommerfeld became Klein's assistant, one of his tasks was to edit Klein's lecture on the top. In view of what finally resulted from this lecture it should be remarked that Klein had only a small booklet in mind when he charged Sommerfeld with this task. [5] Its mission was pedagogical, and the audience were high-school teachers. To Klein, the top was—in the tradition of British natural

1. Sommerfeld to his parents, 27 June 1894. Private Collection, Munich. (ASWB, I, 21).
2. Sommerfeld to his mother, 20 February 1894. Private Collection, Munich. (ASWB, I, 23).
3. Sommerfeld to his parents, 27 June 1894. Private Collection, Munich. (ASWB, I, 24).
4. There is a rich literature on this topic, e. g. (Rowe 1989; Rowe 2003), where references to earlier work by Herbert Mehrtens, Louis Pyenson, and others are given.
5. Neither Klein nor Sommerfeld had foreseen that this editing finally would result into a four-volume treatise—the preceeding lecture on elementary geometry was published as a booklet of about 30 pages.

philosophers—a “philosophical instrument”, with which he wanted to illustrate advanced mathematical concepts like quaternions and elliptical functions (Klein/Sommerfeld 1897-1910).

Sommerfeld’s own mathematical work, too, was influenced by Klein’s lectures. Sommerfeld in particular inherited the tradition of Klein’s „physical mathematics“, and he was still proud of it many years later. He praised Klein in a birthday address in 1919 for having revived “Riemann’s spirit” and demonstrated how powerful Riemann’s method has become as a consequence of “being imbued with approaches from mathematical physics” (Sommerfeld 1919). “Physical mathematics,” as performed by Klein and Sommerfeld in Göttingen, should not be confused with applied mathematics. Physics was meant to be the source of inspiration for mathematics, not the other way around. When Sommerfeld wrote to his parents that Klein wanted to make him a „mathematical physicist“, it was meant in this sense. The first example from physics which Klein asked Sommerfeld to elaborate was from thermodynamics: heat conduction. Although this problem had its origin in Sommerfeld’s efforts to solve a practical problem while he was still a student in Königsberg, it acquired a new meaning under Klein’s tutelage: It extended the “mirror method” from physics into a general mathematical procedure to find solutions of partial differential equations under certain boundary conditions.

A more sophisticated elaboration of this method became the topic of Sommerfeld’s habilitation work: the theory of diffraction of electromagnetic waves.[6] From a mathematical point of view it was an ingenious new approach (appendix 1), but it did not seem to provide novel results for optics because diffraction problems had been solved earlier and for more complicated configurations than the simple case of Sommerfeld: the diffraction on a semi-infinite plate. Sommerfeld could not even solve the next difficult problem, the diffraction on a slit, not to speak of more complicated devices such as gratings. The diffraction theory of the physicists, most advanced by Gustav Kirchhoff, was based on Huygens’ principle and not derived from Maxwell’s equations only. Sommerfeld, by contrast, attempted to solve Maxwell’s equations without additional assumptions other than given by the boundary values. Consequently, his approach was more general and applicable to cases which where not covered by the physicists’ method, such as in the close vicinity of the diffracting edge. Later, Sommerfeld’s approach became important phys-

6. Sommerfeld’s mathematical papers are reprinted in (ASGS, vol 1). Subsequently, Horatio Carslaw extended this method for application in diverse areas from acoustics to potential theory.

ically and technologically when it was regarded as a method to solve partial differential equations in physics under certain boundary conditions far beyond optics.

With the success in reach, even for such a simple problem as a diffracting edge, Sommerfeld was exhuberant. In August 1894, after he had delivered a talk in Klein's seminar, he wrote to his parents: "It was about the diffraction of light, treated as it should be, in a correct mathematical way. I gave the physicists with their flawed attempts to solve this problem a piece of my mind."[7] Two months later, when he had to express his criticism in written language instead of spoken words, he wrote to his mother that "Mr. Kirchhoff" caused him some worries, because he was convinced

> ...that it is all nonsense and talking-around which this mathematically most profound man among the physicists has done in optics. But that I cannot say in my work without further comment. In any case I have to read his work profoundly.[8]

This may suffice to illustrate how much Sommerfeld regarded himself as a mathematician, and his work as part of mathematics. Despite the frequent use of "mathematical physics" for such work, it was then closer to the heart and mind of the mathematicians than to the physicists. Klein suggested its publication in the renowned *Mathematische Annalen*. He was particularly pleased, as he wrote to the editor of this journal, "because this is probably the first time that one of our young people establishes a real progress in mathematical physics."[9] Poincaré, for example, in his work on diffraction theory, was full of praise for the "travail très important de M. Sommerfeld" and called his approach "une méthode extrêmement ingénieuse" (Poincaré 1897).

In 1897, two years after he had become Privatdozent in Klein's institute and started to lecture himself on special mathematical topics, Sommerfeld was called as professor of mathematics to the Bergakademie Clausthal. This was a small technical university, and once more Sommerfeld became aware that it was the wrong place to pursue his interests. He had to teach elementary mathematics and was soon frustrated by

7. Sommerfeld to his parents, 3 August 1894. Private Collection, Munich. (ASWB, I, 25).

8. Sommerfeld to his mother, 3 October 1894. Private Collection, Munich. (ASWB, I, 27).

9. Klein to Walther Dyck, 3 August 1895. Munich, Bavarian State Library, Manuscript Division, Dyckiana, box 5.

the lack of motivation and talent of his students. Fortunately, Göttingen was not far away from Clausthal so that he could maintain his ties to his former colleagues and friends, among them Emil Wiechert and David Hilbert, both friends from Königsberg with whom he could share common scientific interests. He also kept close contact with Klein, for whom he had to edit the theory of the top.

In 1898 Klein charged Sommerfeld with another far-reaching task: to edit the physics volumes of an encyclopedia of mathematical sciences. In 1899, Klein and Sommerfeld travelled together to England, where they visited British mathematicians and physicists, for whom Klein had much respect and sympathy because of their practical mathematical orientation. Some of them, like Augustus Edward Hough Love, subsequently wrote articles for the encyclopedia. The encyclopedia project brought Sommerfeld into contact with leading theoretical physicists: Ludwig Boltzmann, Hendrik Antoon Lorentz, Lord Kelvin, to name only the most prominent. If there was a major cause which should be singled out for changing Sommerfeld's career from a mathematician to a theoretical physicist, it was this encyclopedia project. It kept him busy for almost three decades (Sommerfeld 1904-1926).

Mechanics

However, in 1899 Sommerfeld was not yet prepared to give up mathematics for physics. Frustrated with his Clausthal position, he hoped for an opportunity to be called to a chair of mathematics where he could make better use of his talents. Such an opportunity seemed within his reach when a chair for geometry at the Göttingen University had to be replaced, but there were other candidates which Klein regarded as better qualified for this position.[10] Sommerfeld must have been very disappointed, but he did not reproach Klein for being overlooked and patiently awaited the next opportunity. He had not to wait very long. In 1900 he was called to a chair at the Technische Hochschule Aachen as professor for mechanics.

It was not unusual for mathematicians to teach mechanics at technical universities. Around 1900, however, mathematicians from universities who occupied chairs in technical universities often were regarded

10. Friedrich Schilling, another pupil of Klein, was called to this chair. In 1904, when Schilling accepted an offer at the Technical University Danzig, Klein transformed it into a chair for applied mathematics, the first chair for this specialty in Germany. It was offered to Carl Runge.

with some suspicion by their engineering colleagues: The technical professors argued that the university mathematicians only used this as an additional job opportunity, without a true interest in technological problems. For Sommerfeld, the ambitious pupil of Felix Klein, this was even a greater challenge because Klein was eager to establish closer ties between mathematics and technology—much to the dislike of representatives of the technical universities who struggled for the same rights as the universities and regarded Klein's initiatives as an effort to invade a territory which they claimed as their own property (Manegold 1970; Hensel et al. 1989).

Mechanics as a discipline with a long tradition both in mathematics and in technical applications was an ideal field to demonstrate Klein's tendencies. It was not accidental that Klein reserved the task of editing the mechanics volumes of the encyclopedia for himself. At his university in Göttingen, with the support of influential industrialists and politicians, Klein established new institutes for applied sciences, one of them for applied mechanics. With his pupil Sommerfeld as a professor of mechanics in a technical university, Klein had high expectations, and Sommerfeld was more than alert and duly reported to his former teacher how he was received by his engineering colleagues. In a letter he wrote, for example:

> One of my colleagues here, one of the most intelligent men, said with regard to you: Timeo Danaos et dona ferentes. [Be afraid of the Danaos even when they offer gifts]. This seems to be the basic mood...In a recent general meeting, when the discussion dealt with your tendencies and Riedler's remarks against them, I spoke up vigorously against the distrust of your tendencies. [11]

Sommerfeld did his best to counter such distrust by reorienting his own work towards technological problems. In July 1901, for example, he lectured on resonance phenomena connected with unbalanced motors or steam engines (Eckert 1996a; Eckert 1996b). Another area where Sommerfeld hoped he could bridge the gap between theory and practice was fluid mechanics—a traditional test-case for the use of mathematics in technology since the advent of calculus. Sommerfeld was not entirely unprepared for this challenge. He had corresponded with David Hilbert earlier about the mathematical foundations of hydrodynamics. [12] Now he attempted to calculate the resistance of a fluid in a pipe. Generations of

11. Sommerfeld to Klein, 13 June 1900. Reproduced in (ASWB, I, 166-167).

12. Sommerfeld to Hilbert, 13 December 1897; Hilbert to Sommerfeld, 16 December 1897. (ASWB, I, 80-85).

19^{th}-century practitioners on the one side and mathematically-minded theorists on the other could not solve this problem. It had become a symbol for how far theoretical hydrodynamics and practical hydraulics diverged (Sommerfeld 1900). It comes as no surprise that Sommerfeld, too, could not solve it; but it became the starting point for related investigations which resulted in a theory of lubrication.[13]

Among Sommerfeld's publications from the Aachen period, that is between 1900 and 1906, about a dozen are related to technolgy. Not all of these papers contain new research results, some articles reveal more rhetorical effort to bridge the gap between theory and practice than actual progress. Nevertheless, the wealth of topics with which Sommerfeld dealt is astonishing, comprising for example the theory of railway brakes or the bending of vertical plates in rolling mills. Furthermore, not all of his efforts resulted in publications. For example, Sommerfeld corresponded with and advised a ship-builder about the use of tops as a means of stabilizing ships against uncontrolled motion.[14] In 1904, he was asked to co-author a textbook on locomotives. Sommerfeld reported to Klein that he agreed with this project and would start writing as soon as the final part of the theory of the top was finished.[15] Klein could not have had a more ambitious missionary for his goals.

Physics

We have to recall Sommerfeld's disparaging remark about physics when he criticized Kirchhoff in 1894, and his hopes to be considered for a chair in mathematics in 1899, before analysing how he became a theoretical physicist and what it meant to be a theoretical physicist around 1900. Although much of his work, retrospectively, does not appear far away from theoretical physics, Sommerfeld regarded himself as a mathematician during the first decade of his career, and he was regarded as such by his colleagues. This was most obvious, for example, in 1902, when Sommerfeld's name was first mentioned on a list of candidates for a chair of theoretical physics at the university of Leipzig—on the fourth place behind Carl Runge, Emil Wiechert, and Theodor Des Coudres—and then cancelled again. The chair had been newly established in order to attract

13. (Sommerfeld 1904). See also the related correspondence in (ASWB, I, 135 and 223-225).

14. See (ASWB, I, 140-142, 201-206).

15. Sommerfeld to Klein, 8 November 1904. (ASWB, I, 238-239). The textbook of locomotives did not materialize because its first author, August von Borries, died in 1906; the final volume of the theory of the top appeared only in 1910.

Boltzmann to Leipzig, just as the Munich chair for theoretical physics had been created for Boltzmann ten years ago. But Boltzmann stayed even shorter in Leipzig than in Munich; he returned to Austria after two years. Des Coudres became his successor. Both Runge's and Sommerfeld's names were cancelled from the list because they were regarded more as mathematicians than as physicists. [16]

In 1904 there were rumors that Sommerfeld was regarded as a candidate to succeed Runge at the Technische Hochschule Hannover, because Runge was then called to Göttingen. But this opportunity did not materialize because, as Sommerfeld was informed confidentially, the Prussian ministry regarded him too successful in mechanics so that he should not vacate his Aachen chair (which was also under Prussian administration). [17]

Why, then, and when did Sommerfeld consider himself and was considered by others as a theoretical physicist? If we look at his publications during the first decade of his career, from 1892 to 1902, the majority of his work was in mathematics, although—as I have mentioned— this was often close to physics, such as his mathematical theory of diffraction. A sequel of this work dealt with the diffraction of x-rays; Sommerfeld published four papers on this problem between 1899 and 1901, comparing results from his earlier mathematical theory of diffraction with results from the physicists' diffraction theory. Another research theme, with which he was busy in 1898 and 1899, addressed the problem of electromagnetic wave propagation along wires. From the perspective of physics, both themes belonged to quite different areas, but from Sommerfeld's perspective both offered opportunities to display his mathematical virtuosity with complex integrals and special functions. [18]

Although these research themes involved some contact with contemporary research in physics, for example the recent experiments by the Dutch experimenters Hermanus Haga and Cornelis Wind about the passage of x-rays through narrow slits, Sommerfeld's emphasis was on mathematics in these early papers. This orientation began to change when Sommerfeld got into closer contact with physicists—prompted by his editing of the physics volumes of the encyclopedia for mathematical sciences. Among his most important authors for the encyclopedia was Hendrik Antoon Lorentz, who wrote two fundamental articles on Maxwellian electrodynamics and on the novel electron theory (Lorentz

16. (ASWB, I, 156-157).
17. (ASWB, I, 158).
18. Reprinted in (ASGS, II) (electromagnetic waves) and (ASGS, IV) (x-ray-diffraction).

1904). Sommerfeld edited these articles with the same zeal with which he entered new territory in technology in an attempt to win the trust of the engineers in Aachen. This effort is witnessed by an intensifying exchange of letters which often contained long elaborations of original research, and personal visits resulting in a cordial relation between the families of Lorentz and Sommerfeld.[19] Other close relations developed with Wilhelm Wien, then another authority for theoretical physics in general and the electron theory in particular,[20] and with the spectroscopist Friedrich Paschen.[21]

It is obvious from Sommerfeld's correspondence with these physicists how he re-oriented his own research more and more towards physics, to the extent that he started to publish in 1904 on the most advanced topic of contemporary theoretical physics, the theory of electrons.[22] By this time he started to regard himself more a physicist than a mathematician. When he received a call to the Berlin mining academy as professor of mathematics and mechanics, Sommerfeld declined and stayed in Aachen under the condition that he obtain an assistant. This assistant was Peter Debye, a dutch engineering student, with whom Sommerfeld now began to conquer new physical territory despite their official chores as representatives of mechanics in a technical university. Debye also helped Sommerfeld translate his electron theory into Dutch so that Lorentz could present it to the Amsterdam Academy.[23]

Two years later, when Sommerfeld received a call to the chair of theoretical physics in Munich, a former Aachen student recalled that Sommerfeld had told him once: "I am not really a professor of technology, I am a physicist."[24] I do not go into the details of Sommerfeld's call to Munich in 1906 as Boltzmann's successor (Eckert/Pricha 1984), it may suffice to mention that he was recommended by such outstanding physicists as Lorentz, Boltzmann, and Wien, and that Röntgen, the experimental physicist in Munich, found Sommerfeld's electron theory particularly attractive because he hoped that this would contribute to solve the ten-year-old riddle about the nature of x-rays.

We should remember that in 1902 Sommerfeld was not yet considered as a suitable candidate for the Leipzig chair because of his mathematical orientation. Now, only four years later, his recent work in electron theory

19. For an overview see: http://www.lrz-muenchen.de/∼Sommerfeld/, and for some typical examples, (ASWB, I, 211-221).
20. (ASWB, I, 225-229, 242-244, 250-253).
21. (ASWB, I, 232-234, 236-240, 245-246).
22. Reprinted in (ASGS, II, 39-182).
23. (ASGS, II, 148-149, 158).
24. Kurt Rummel to Sommerfeld, 3 August 1906, (ASWB, I, 254-255).

was taken as a valid entry into physics. Ironically, the only voice that was raised against his call to Munich came from a mathematician, Ferdinand Lindemann, who erroneously believed that Sommerfeld's electron theory was sloppy from a mathematical point of view (Eckert 1997).

Together with Debye, who accompanied Sommerfeld from Aachen to Munich as his assistant, he was ambitious to demonstrate that he was successful as a true physicist, rather than as a mathematician who uses physics as a resource of interesting topics. For that purpose, Sommerfeld insisted on having experimental facilities in his institute. This seems strange from our modern notion of theoretical physics, but it was not unusual by the standards of 1900. It should be mentioned that Sommerfeld's name had been dropped from the Leipzig list in 1902 not only because he was considered as a mathematician rather than as a theoretical physicist; furthermore, as was argued in a faculty meeting, he had "never performed an experimental investigation" and lacked "any experience for directing experimental work so that the new institute for theoretical physics would be in danger of being left unused."[25] In the view of Leipzig's faculty and probably most physicists around 1900, a true physicist, experimental or theoretical, was expected to perform or to supervise experiments. Sommerfeld tried hard to comply with this expectation during the first years in Munich, as I have described elsewhere (Eckert 1999).

Many years later, in an autobiographical sketch, Sommerfeld wrote that he had intended from the very beginning to establish a "nursery of theoretical physics" in Munich (ASGS, IV, 672-682, here p. 675). With the hindsight of how well he succeeded with this goal it is easy to forget the difficulties he faced. In this situation Sommerfeld's mathematical heritage was better suited to attract pupils than a more fundamental orientation, such as Planck's or Einstein's. The range of Sommerfeld's early work in physics comprised a diversity of topics. One of his first Munich doctoral students, Peter Paul Ewald, recalled how he was offered a theme for his dissertation:

> Sommerfeld took a foolscap sheet of paper out of the drawer and I saw a list of some ten or twelve research problems written out in his large clear handwriting. He discussed and explained them to me one by one. Calculation of self-inductances of solenoids for alternating currents; propagation of radio waves over a surface of finite conductivity; an unsolved problem of gyroscopic theory; a new attempt at explaining the instability of Poiseuille

25. Minutes of faculty meetings, 6 December 1902. Leipzig, University Archive, PA 410, Bl. 34-42.

> flow, and further subjects. Each subject had its own merit and its own type of mathematical technique, and Sommerfeld pointed them out.[26]

Ewald, however, had already made up his mind; after attending a lecture on optics in which Sommerfeld had criticised conventional dispersion theory because it did not account for the phenomena of crystal optics, Ewald wanted to attack this problem. Sommerfeld agreed with this choice too.

This not only illustrates the flexibility in the choice of problems, it also marked the beginning of what Sommerfeld later called the most important event in the history of his institute: In 1912, a discussion between Ewald and Sommerfeld's Privatdozent Max von Laue about problems in crystal optics from Ewald's doctoral dissertation prompted Laue to suggest the now famous experiment of x-ray-diffraction on crystals, performed in Sommerfeld's institute. Without the peculiar circumstances during the beginnings of theoretical physics in general and Sommerfeld's career in particular, we would not understand why this experimental discovery was made in an institute of theoretical physics, and why it was regarded as its most important accomplishment in spite of other pathbreaking theoretical discoveries in atomic physics.

Sommerfeld's institute, by this time, had already become known as an attractive centre among those physicists who intended to specialize in theory. Einstein wrote to Sommerfeld in 1908 from Bern in Switzerland, where he worked in the patent office: "I assure you that, if I were in Munich and time would permit it, I would attend your lectures in order to perfect my mathematical-physical knowledge."[27] Paul Ehrenfest wrote in 1911 to Sommerfeld how much he wished to go to Munich "in order to learn—among many other things—particularly this under your personal supervision: how one performs a research work which requires a true effort of calculation."[28] Neither Einstein nor Ehrenfest became Sommerfeld's students, but their letters are a clear evidence that the attractivity of Munich as a center of theoretical physics started well before Sommerfeld made atomic theory his favourite topic of research.

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26. Peter Paul Ewald: The Setting for the Discovery of X-Ray Diffraction By Crystals. Unpublished manuscript of a speech at the First General Assembly of the International Union of Crystallography at Harvard University, 2 August 1948, pp 21-22. Munich, Deutsches Museum, Archive, NL 89, 027.
27. Einstein to Sommerfeld, 14 January 1908. (ASWB, I, 321-323).
28. Ehrenfest to Sommerfeld, 17 September 1911. (ASWB, I, 402-404).

Conclusion

What are the lessons from this study regarding our introductory questions? I have discerned three avenues—mathematics, mechanics, physics—and placed Sommerfeld at the crossroads of those avenues. I also suggest to approach the broader topic of the emergence of theoretical physics, intellectually and institutionally, from a vantage point where mathematics, mechanics, and physics cross or become aligned for part of their routes, so to speak.

Mathematics, our first avenue, deserves a closer analysis with respect to its claims in physics. As we have seen with Sommerfeld's attitude as a mathematician towards the „mathematical optics" of Kirchhoff, the physicist, there was no common understanding in both disciplines about their subject matter even if it dealt with the same problem. Another example for the different conceptual understanding of physical theorising is provided by the efforts of physicists and mathematicians to proof Kirchhoff's Law in radiation theory (Schirrmacher 2003b). Although the various cases of „mathematical physics" abound of subtleties so that we often can't see the wood for the trees, one conclusion seems obvious: theoretical physics around 1900 emerged from a territory where mathematics had its own claims and research interests. In order to define more precisely the nascent disciplinary identity of theoretical physics as distinct from the mathematicians' excursions in physics, we need more such examples.

Mechanics, the second avenue, had a theoretical and a technical lane. Because of its close historical ties to mathematics, in particular calculus, theoretical mechanics could be regarded almost as a mathematics specialty. By the end of the 19^{th} century, however, it also had developed into an engineering science, technical mechanics —with an ever growing gap opening up between both lanes. In view of Sommerfeld's numerous contributions to technical mechanics it was argued that he "indeed contributed considerably to bridge the gap which had opened by the end of the 19^{th} century between mathematics and technology" (Hermann 1967). Such a statement tends to exaggerate the convergence of theory and practice as accomplished by Sommerfeld's efforts.[29] My emphasis here, however, is not on the theory-practice dispute. In the context of this study I conclude that the wealth of practical problems with which

29. Even if we consider the theory of the top, with which Sommerfeld was probably concerned longer than with other mechanical specialties, there is a remarkable difference in how engineers dealt with this topic and how this was done in the Klein-Sommerfeld-treatise (Broelmann 2002).

Sommerfeld was confronted during his Aachen years, together with the preceeding mathematical orientation as Klein's assistant, also shaped his broader research approach. Sommerfeld was pleased when he could demonstrate the tractability of a practical problem in principle; working out detailed engineering applications was beyond his interests.

Physics, our third avenue, became Sommerfeld's main avenue where this approach came to bearing. It may be characterized as an eagerness to display the mathematical tractability of the problems across the entire spectrum of physical specialties, from mechanics to atomic theory. This breadth enabled Sommerfeld to have so many doctoral students, and it was mirrored in their research themes already during the first years of his Munich „nursery" for theoretical physics (appendix 2). By comparison, theorists like Planck who focused on problems because of their bearing on fundamental physical or metaphysical questions, were less prone to found a „school"—even if the institutional means were available. Sommerfeld's case may be unique in its specific combination of aspects, institutional and intellectual, from all three avenues; but it lends itself for comparison with other examples along these avenues to see what aspects are recurrent and should be singled out as responsible for the emergence of theoretical physics.

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Appendix 1: Sommerfeld's diffraction theory (1895)

Sommerfeld's mathematical diffraction theory differs from Kirchhoff's mathematical optics [30] fundamentally by a different conception: It aimed at a rigorous solution of a partial differential equation under certain boundary conditions. In contrast to Kirchhoff's theory, Sommerfeld made no use of Huygens' principle. He had a precursor in this effort: In 1893, Poincaré solved the wave equation for a diffraction problem by series expansion—without resorting to Huygens' principle—in order to explain the polarisation of the diffracted light (Poincaré 1893). Sommerfeld referred to Poincaré's effort as one which also breaks with the older theory (Sommerfeld 1896, 317). However, Poincaré's motivation was different: „It is not at all Poincaré's intent to analyse mathematically the functions of diffraction, but to derive formulae for the physicists," Sommerfeld argued in an unpublished manuscript of his habilitation work about Poincaré's goals, whereas he claimed for himself to arrive at results „which beyond their physical application are entitled to a certain mathematical interest." [31]

Sommerfeld considered the following configuration: An infinitely thin semi-infinite screen is arranged in the xz-plane of a Cartesian coordinate system with $x > 0$, so that the z-axis coincides with the diffracting edge. The light source is assumed in the form of a line parallel to the z-axis at $Q = (r_0, \varphi_0)$ (using polar coordinates). The problem is to find at $P = (r, \varphi)$ solutions $u(Q, P)$ (u represents a component of the electrical or magnetic field) of the wave equation

$$\frac{\partial^2 u}{\partial r^2} + \frac{1}{r}\frac{\partial u}{\partial r} + \frac{1}{r^2}\frac{\partial^2 u}{\partial \varphi^2} + k^2 u = 0$$

which satisfy certain boundary conditions for $\varphi = 0$ and $\varphi = 2\pi$. According to the mirror-method, the boundary condition at the surface of the diffracting sheet (i. e. $u = 0$ or $\frac{\partial u}{\partial \varphi} = 0$ for $\varphi = 0$, the two cases refer to parallel or vertical polarization of the electric field with respect to

30. In contrast to Sommerfeld, Kirchhoff, in his „Lectures on mathematical optics" (Kirchhoff 1891) had mathematized Huygens' principle (whereby each point hit by a light wave may be regarded as the source of a secondary spherical wave) so that the excitation at a given point in space can be expressed as an integral over the secondary waves. This approach violated the boundary values, although it accounted for the practical diffraction phenomena in optics quite well.

31. A. Sommerfeld: Undated Manuscript (probably 1894 or 1895). Microfilm 23, section 3 and 4. Archive for History of Quantum Physics. Here p. 75. On Poincaré's approach towards physics see (Walter 2002).

the z-axis) is satisfied by a suitable superposition of waves from the light source at Q and its mirror Q'. To simplify the procedure it is assumed that the light source Q is moved to infinity, i. e. $r_0 = \infty$, so that the incident light forms a plane wave which hits the screen under an angle φ_0. But, obviously, mirroring the incident plane wave on the opposite side of the screen could not solve the problem. Sommerfeld, therefore, extended the physical space around the diffracting edge ($0 < \varphi < 2\pi$) by a mathematical space ($-2\pi < \varphi < 0$)–and placed the mirror image Q' there. (In mathematical terms this meant solving the wave equation on a two-sheet Riemann surface with the origin as branch-point. The details of Sommerfeld's theory are beyond the scope of this appendix, so that we have to content ourselves with the results.) The solution is obtained as a superposition

$$u(r, \varphi, \varphi_0) = U(r, \varphi, \varphi_0) \pm U(r, \varphi, -\varphi_0)$$

with

$$U(r, \varphi, \varphi_0) = e^{ikr\cos(\varphi-\varphi_0)} \frac{e^{\frac{i\pi}{4}}}{\sqrt{\pi}} \int_{-\infty}^{\sqrt{2kr}\cos\frac{\varphi-\varphi_0}{2}} e^{-i\tau^2} d\tau$$

From this result Sommerfeld derived asymptotic formulae for short wavelengths (i. e. for large kr with $k = \frac{2\pi}{\lambda}$) and for three distinct areas: the geometrical shadow, (A), the illuminated area behind the screen, (B), and in front of the screen, (C):

$$\begin{array}{lcl} (A) \quad u & = & Z \\ (B) \quad u & = & Z + \cos[kr\cos(\varphi - \varphi_0)] \\ (C) \quad u & = & Z + \cos[kr\cos(\varphi - \varphi_0)] \pm \cos[kr\cos(\varphi + \varphi_0)] \end{array}$$

with

$$Z = \frac{1}{4\pi}\sqrt{\frac{2\pi}{kr}} \cos(kr + \frac{\pi}{4}) [\frac{\pm 1}{\cos\frac{\varphi+\varphi_0}{2}} - \frac{1}{\cos\frac{\varphi-\varphi_0}{2}}]$$

The radial dependence proportional to $\frac{1}{\sqrt{r}}$ shows that Z is a cylindrical wave. In other words: the result in the shaded area (A) may be regarded as if the edge acted as an illuminated line from which a cylindrical wave originates; in the illuminated regions (B) and (C) the result could be interpreted as a superposition of plane waves with this cylindrical wave.

Although such an interpretation could be reconciled with Kirchhoff's theory, Sommerfeld's „exact" theory added an important new feature which was of more than mere mathematical interest: It made evident where the range of validity of Kirchhoff's theory ended and that of Poincaré's theory began. Both, Kirchhoff and Poincaré aimed at a theory suitable for application to optics (Poincaré referred to specific experiments by Louis Georges Gouy). Kirchhoff's theory was valid for small diffraction angles only; Poincaré's series expansions, on the other hand, were valid for large diffraction angles. „The range of Kirchhoff's formulae, therefore, is rather small," Sommerfeld concluded at the end of his paper; „beyond this range they become noticibly false. Here Poincaré's formulae become validated" (Sommerfeld 1896, 374). In a sequel to his earlier paper, Poincaré confirmed Sommerfeld's result in 1897 and praised his approach as „an extremely ingenious method" (Poincaré 1897, 313). From a modern perspective, it is easy to categorize Sommerfeld's method among the arsenal of methods of theoretical physics. It became the model for a host of further efforts to solve partial differential equations in physics. [32]

32. See, for example, the chapter „Geschichte des Sommerfeldschen Beugungsproblems" in (Rubinowicz 1966, 153-161). However, it is not often treated in textbooks on optics. An exception is (Born 1933), but in view of Born's own mathematical heritage this is not surprising.

Appendix 2: Sommerfeld's first doctoral students (1908-1914)

1908

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1909

Hondros, Demetrius: *Über elektromagnetische Drahtwellen.*

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1911

Hoerschelmann, Harald v.: *Über die Wirkungsweise des geknickten Marconischen Senders in der drahtlosen Telegraphie.*

Lenz, Wilhelm: *Über das elektromagnetische Wechselfeld der Spulen und deren Wechselstrom-Widerstand, Selbstinduktion und Kapazität.*

Scheidel, Valentin: *Spezielle Bewegungsformen des schweren symmetrischen Kreisels.*

Hüter, Wilhelm: *Kapazitätsmessungen an Spulen.*

1912

Ewald, Peter Paul: *Dispersion und Doppelbrechung von Elektronengittern (Kristallen).*

1914

Landé, Alfred: *Zur Methode der Eigenschwingungen in der Quantentheorie.*

Epstein, Paul S.: *Über die Beugung an einem ebenen Schirm unter Berücksichtigung des Materialeinflusses.*

Dehlinger, Walter: *Über spezifische Wärme zweiatomiger Kristalle.*

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