Crafting the Quantum

Arnold Sommerfeld and the Practice of Theory, 1890–1926

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1 The Physics of Problems: Elements of the Sommerfeld Style, 1890–1910

In 1906, Sommerfeld was called to Munich to fill the chair in theoretical physics. The position had been vacant for a dozen years, ever since Ludwig Boltzmann had left it to return to Vienna. The high standards required by the Munich faculty and the paucity of practitioners in theoretical physics led to an almost comical situation in the intervening years, as the job was repeatedly offered to the Austrian in an attempt to lure him back. Failing both in this and a further attempt to win Hendrik Antoon Lorentz for the position, the search moved on to younger men. Of the three candidates (Emil Wiechert, Emil Cohn, and Sommerfeld), only Cohn held a position in physics. When Wiechert declined the position, the ministry offered it to Sommerfeld, who had come highly recommended by both Lorentz and Boltzmann.[1] Sommerfeld's work on Röntgen-ray (i.e., X-ray) diffraction and the electron theory had probably also attracted the attention of Wilhelm Röntgen, Munich's professor of experimental physics. Röntgen signaled his approval, and the 38-year-old Sommerfeld jumped at the opportunity to occupy a full professorship at the prestigious university.

The opportunity, however, brought with it a major challenge. "In Munich I had for the first time," wrote Sommerfeld in an autobiographical sketch, "to give lectures on the different areas of theoretical physics and special lectures about current questions. From the beginning I plugged away at—and wouldn't let any trouble divert me from—the founding through Seminar and Colloquia activities of a nursery [*Pflanzstätte*] for theoretical physics in Munich." These lectures,[2] written in Sommerfeld's hand and delivered during a critically formative period in the development of theoretical physics, provided a means for Sommerfeld to educate a new generation of students and researchers in his methods. They also offered a means for him to develop these methods and to master the relevant material himself. These early lectures (1906–1910), Sommerfeld's published writings, and his reports on his students' dissertations to the Munich philosophical faculty constitute the basis for this chapter, which is an attempt to describe the "Sommerfeld style" of theoretical physics. This is the first of three chapters devoted to an examination of the Munich school in the years before 1918. Chapter 2 takes up the question of the transference of this style to a generation of students and researchers via an examination of the day-to-day practices of pedagogy. In chapter 3, Sommerfeld's correspondence, particularly letters written to him from his students during the First World War, is used to delineate the subsequent applications of Sommerfeld's teachings during the years of the conflict.

Sommerfeld worked closely with his students and was known as an excellent teacher. He fused teaching and research, incorporating his most recent work into lectures. Concentrating on Sommerfeld's lectures thus provides a means of bridging the gap between pedagogy and research practice and a means of better understanding Sommerfeld's approach to the problems of physics and the state of the field as he saw it.[3] The casual prose of the lectures is in stark contrast to more guarded comments in Sommerfeld's published writings. For example, in his lectures on heat radiation, first delivered in 1907, one can clearly see an attempt to master Max Planck's *Vorlesungen über die Theorie der Wärmestrahlung*, published the year before.[4] At the same time, Sommerfeld was (as he was not in his publications) openly skeptical toward Planck's black-body work, preferring the theories of Lorentz and James Jeans.[5] The lectures thus provide insight into both the early years of one of the most important sites for theoretical physics in the early twentieth century and Sommerfeld's own approach to the problems of contemporary physics. Sommerfeld's case also, and more generally, provides a particularly telling example of one of the central arguments of this book: that theoretical physics at the turn of the twentieth century cannot be understood as a "distillation" of theory from physics, but rather must be seen as having been actively constructed from multiple and varied parts. Far from being merely a subset of an existing discipline, the subject that emerged in Munich was a blend of at least three components: mathematics, physics, and engineering. Drawing from his experience in each of these three fields, Sommerfeld selected and modified components that would make up the theoretical physics of the Sommerfeld School.[6]

After 1906, having previously held positions teaching first mathematics and then technical mechanics, Sommerfeld quite consciously "refashioned" himself into a theoretical physicist. Problems previously deemed mathematical were now reformulated to emphasize a new, more physical perspective; technical applications were blended with mathematical and physical methods that may well have seemed alien to Sommerfeld's former colleagues in engineering. Not merely an incorporation of fields distinct from physics, however, the process also involved the selection and emphasis of specific areas within physics itself—in particular, those parts of the field that were in accord with the electromagnetic view of nature, a worldview of which Sommerfeld was an ardent supporter.

Central, indeed perhaps essential, to Sommerfeld's work in these eclectic fields was his and his students' emphasis on the solution of specific problems in areas such as wireless telegraphy, the wearing on ball bearings, the turbulent flow of water in the channels of the Isar, and black-body radiation. It would be remarkably fitting that the

Festschrift prepared for him by his students on the occasion of his sixtieth birthday should be titled simply *Problems of Modern Physics*.[7] Eschewing an axiomatic and generalized approach, Sommerfeld sought out, both in his teaching and his research, issues of contemporary interest that he would then attempt to understand in theoretical detail. And it would be these problems—both in terms of topic and their forms of solution—that would provide coherence—"technical unity"—for the wide-ranging work of the Sommerfeld School.

Mathematics: The Kind of Notion We Call Heat

Der Verstand schöpft seine Gesetze (*a priori*) nicht aus der Natur, sondern schreibt sie dieser vor.
—Kant

The words above—meaning "The understanding draws its laws (*a priori*) not from Nature, but rather prescribes them to her"—are to be found at the beginning of a draft of what is probably Sommerfeld's first scientific work.[8] They should not, perhaps, be taken as evidence of any particular philosophical commitment. Sommerfeld was born and grew up in Königsberg, where his father was a practicing physician.[9] At the local university, he had walked the same halls as Germany's most famous philosopher. Yet, insofar as Kant was important to Sommerfeld's development in this period, it was probably only through his dictum that "in every specific natural theory only so much actual science can be found as there is mathematics within it."[10]

The seven-page paper never takes up philosophy again, but a parenthetical remark at the beginning lays out Sommerfeld's main aim: "The leading thought in my work is to simplify the problem of heat conduction by establishing a characteristic function."[11] While his approach here was principally mathematical, the inspiration for the work could be found in an existing physical problem, one set as a prize question, worth 300 Marks, by the *Physikalisch-Ökonomische Gesellschaft* (Königsberg Physical-Economical Society), of which Sommerfeld's father was a member.[12] In the local Botanic Garden, Franz Neumann, a co-founder of the mathematical-physical seminar at Königsberg, had established a meteorological station that measured the temperature below the surface. The task—set by three of Neumann's students and four other members of a commission established by the society in 1889—was to analyze the data the station produced in its measurements of temperature at different depths. "The Society," read the question, "would like as comprehensive as possible a theoretical evaluation of the geothermal measurements made at Königsberg, especially to understand the thermal conductivity of the earth and the causes of it. . . ."[13]

Sommerfeld set to work on solving the problem in a long essay, clearly intended to be presented to the society, and approached the institute for theoretical physics and Neumann's eventual successor there, Paul Volkmann (also a member of the prize commission), for aid in evaluating the terms that arose in his efforts to reduce an arbitrary curve to the sum of a trigonometric series.[14] Sommerfeld and Wiechert, Volkmann's assistant, built an integrating machine to deal with the calculations, although this met with limited success in its operation as a result of what Sommerfeld described only as "an insufficient practical understanding of the apparatus."[15] In the end, owing to a significant error made in assumptions about appropriate boundary conditions, Sommerfeld was forced to withdraw the solution. As it stood, however, the paper he had prepared was a good example of what Olesko has described as the theoretical physics of the Königsberg School.[16] Sommerfeld, who had attended Volkmann's lectures, had clearly noted his teacher's enthusiasm for the problem set forth by the Physical-Economic Society, and provided a solution that would conform to his expectations. In the first of two sections, Sommerfeld set out the mathematical theory for the ideal case of heat conduction in terms of Fourier series and then in terms of Fourier integrals. A short discussion of the operation of the integrating machine followed. In the second section, Sommerfeld dealt with the modifications to the theory that had to be considered in the "real world": corrections for nonperiodic temperature functions, for inhomogeneous surfaces, and for non-level ground (the station stood at the foot of a small hill). Finally, he considered the non-ideal character of the measuring instruments, offering a theoretical treatment of the air thermometer. The project shows a striking similarity to part of the Königsberg paradigm for theoretical physics: essentially the mathematical analysis of experiment. The only important element missing was a numerical error analysis of the results.

Yet, while he could do the problem "Königsberg-style," Sommerfeld did not revel in it. His dissertation on "arbitrary functions in mathematical physics," which he later claimed to have conceived and written out in a few weeks, made use of his earlier work on Fourier series and integrals, but largely without mention of the physics of heat conduction.[17] Consistently, over the course of his life, Sommerfeld would refer to himself in this period as a mathematician. His papers, even when dealing with possible topics in physics, would often emphasize that they were mathematical treatments, such as his 1894 work *Zur mathematischen Theorie der Beugungserscheinungen* (On the Mathematical Theory of Diffraction Phenomena) or his Habilitationsschrift, *Die Mathematische Theorie der Diffraction* (1896) (The Mathematical Theory of Diffraction), which he bragged would wake physicists up to the flaws in their analysis. In a letter to his mother in 1894, Sommerfeld referred to all that physicists had done for the mathematical theory of optics as "humbug and meaningless words."[18] The same year he refused the offer of an assistantship with the theoretical physicist Woldemar Voigt on the ground that he would have to work there on matters "which I do not wholeheartedly consider as my mission." That mission, especially given that Sommerfeld was happy to take up an assistantship with the Göttingen mathematician Felix Klein, was clearly mathematics.[19]

Yet even more than a preference for mathematics over other fields, the young Sommerfeld seems to have had a distaste for the physics of Volkmann and others on the prize commission. Upon hearing of the death of Heinrich Hertz, for example, he wrote to his mother, asking her whether she had read about it yet: “It is Awful! The man began his brilliant experimental investigations five years ago. Half of all physicists at the moment are following in his footsteps and are working on Hertzian oscillations. There are few discoveries that can stand next to his electromagnetic light waves. If it had to be a physicist that died, why couldn’t it have been one of the useless Papes, Volkmanns etc.”[20] In 1908, however, when he took up the problem of heat conduction again, Sommerfeld’s approach to physics (if not to particular physicists) had changed completely. The title of his summer lectures—“Heat Conduction, Diffusion and Conduction of Electricity, together with their Molecular and Electron-Theoretical Connections”—already attested to an involvement with matters of profound interest to physicists at the time. The electron theory in particular was one of the main areas of research of Lorentz, one of the most respected members of the physical community, and the faculty at Munich had, in their choice of Boltzmann’s successor, made clear their desire for someone expert in what Sommerfeld would term “the burning questions of electrons.”[21] In fact, if any one topic could be said to have been the center of path-breaking theoretical research it was this one, and Sommerfeld himself had already contributed three significant articles to the field, published in the reports of the Göttingen Science Society between 1904 and 1905.[22]

If the topic was explicitly physical, the approach was even more so. Commenting on the topic of his Königsberg lecture, heat conduction, Sommerfeld noted that it was “the source of the methods of mathematical physics”: “[T]he book by Fourier is the original Organon of these methods. At the beginning of the 19th century the problems of heat conduction were the order of the day. Apart from Fourier: Poisson, Lamé, Kelvin. Nowadays it’s totally out of fashion, because its physical result [is] not great.”[23]

Yet these mathematical methods would not be the sum total of the course. “Because the mathematical approach [*Gesicht*] is too one-sided,” Sommerfeld wrote “we will give the lecture in addition a more physical orientation. This then has a more current interest.”[24] Thus, Sommerfeld took up the topic in a way that Paul Ewald, one of his students, described as characteristic. He “penetrated quickly through the classical parts of the subject and, after having laid this foundation, dwelt on the topical problems requiring research—ship waves and turbulence in Hydrodynamics, theory of relativity in Electrodynamics, radiation theory, specific heat and energy quanta in Thermodynamics.”[25] Recent research topics discussed in the lectures included not only electron theory in general but also Nernst’s osmotic theory and the Wiedemann-Franz law, which postulated a proportionality between coefficients of heat and electrical conduction.

The most fundamental equations of early-nineteenth-century mathematical physics were consciously redirected toward a more physical—albeit *theoretical* physical—end. Under the German academic principle of *Lehrfreiheit* (academic freedom), Sommerfeld had no requirements other than to teach something called "theoretical physics." No particular subjects were prescribed, and no specific curriculum had to be worked through. His mode of structuring his lectures can thus be seen as representative of his own vision of the shape of the field. It was, clearly, a vision that made use of the methods of mathematicians, but did not necessarily accept their mindset. From his origins as a mathematician in Königsberg, working on Fourier series and arbitrary functions, Sommerfeld had begun, after the turn of the century and certainly after the move to Munich, to refashion himself.[26]

Perhaps most telling of Sommerfeld's explicit attempts to make such a change in persona was his insistence on being provided with experimental facilities. Experimental ability was still considered a fundamental prerequisite for a fully trained physicist, and Sommerfeld clearly felt that he lacked such ability. His previous chair had been in "technical mechanics" (a fusion of mathematics and engineering), and it seems reasonable to read his desire for an adequate laboratory as part of an attempt to shift fields, and, just as importantly, to be seen as doing so.[27] This latter aspect comes to seem even more important in light of the fact that very little experimental work was actually done at the institute. Sommerfeld himself did none, and while he began at Munich emphasizing the importance of laboratory work to his students, comparatively few of them actually followed that path. When Ewald arrived, in 1908, only one student was to be found in the fourth room of the building, that devoted to experiment: Ludwig Hopf was attempting, without a great deal of success, to observe and measure the onset of turbulence in an open trough as he varied the velocity of flow and the viscosity.[28] If a detailed engagement with experimental and observational data would remain a lasting characteristic of the Sommerfeld School, a dual experimental-theoretical strategy would slowly drop away.[29] By 1910, Sommerfeld had given more than a dozen courses and had published a series of well-received papers in physics. One might also imagine that his anxiety—the worry expressed to his mother in 1894 that he knew nothing about experiment and feared making a fool of himself in physics—had receded.[30] Sommerfeld's shift from a mathematician to a theoretical physicist was underway.

Technical Mechanics: Gyroscopes and Ship Waves

The year after completing his thesis in 1891, Sommerfeld sat for the state exam that would qualify him as a teacher. After satisfying the requirements of military service, he moved in 1893 to Göttingen, the site of (as he put it) "mathematical high culture." Family connections initially won him a position as assistant to Theodor Liebisch in the mineralogical institute, but his interests continued to lie in mathematics. In 1894 he became an assistant to Felix Klein, with whom he completed his Habilitation thesis two years later. It was during this period that Sommerfeld came to an appreciation of the value of connections between mathematics, physics, and engineering. He later credited Klein with "giving to my mathematical outlook that sense which is best suited to applications."[31]

In 1897 Sommerfeld became professor of mathematics at the mining academy in Clausthal, a position that seems to have involved teaching basic mathematics to largely uninterested students. Klein's maneuverings eventually resulted in a better offer, and in 1900 he took up the professorship for technical mechanics at the technische Hochschule at Aachen.[32] As a student of Klein's, however, he was initially greeted with suspicion: "In 1900 I was called as a professor of technical mechanics to the Aachen Hochschule. As a result, I was compelled for several years to apply the main focus of my works to engineering problems. I had there the satisfaction, that my Aachen colleagues and students, who first regarded the 'pure mathematician' with mistrust, soon recognized me as a useful member, not only in education, but also in practical matters of engineering, so that I was consulted for expert reports, for collaboration for the engineering society etc."[33]

That Sommerfeld was not exaggerating the degree of hostility which commonly met non-engineers when hired into a technical college may be seen from the welcome that greeted Otto Krigar-Menzel, a student of Helmholtz's, when he was hired to the professorship of theoretical physics at the technische Hochschule at Berlin in 1904.[34] Alois Riedler, who had previously held a chair at Aachen and who numbered among the most prominent of the engineering professors on the faculty in Berlin, serving as Rector in 1899, reacted rapidly and with aggression. There had been an "urgent need," he wrote to the Kultusminister (minister of education and arts) in July, for a teacher "who knows our needs and who gives to physics, which at the moment has proven to be a scarcely fruitful activity, a new style and content."[35] That need, Riedler made clear, had not been satisfied through the hire of Krigar-Menzel. "The teacher and the teaching task stand in the most glaring opposition to the urgent needs of our Hochschule." The Hochschule, he continued, "is no playground for areas of science that belong exclusively in universities, and can find with us as many listeners as engineering lectures among theologians."[36]

A tension, apparent and even acknowledged, thus existed between Sommerfeld's early training as a mathematician and his need to fit into his new role in an engineering school. One can begin to see that there would be two reasons that Sommerfeld's Aachen years would be such a formative element of his "physics of problems." Not only did he arrive at the school at an early, impressionable stage of his career, but also—and more importantly—he was compelled while there to adapt himself to his (at times antagonistic) environment.

Things had changed dramatically since 1870, when the Rheinisch-Westphälisch Polytechnical School in Aachen celebrated a long-awaited opening as Prussian and French forces faced one another across battlefields not far away. Then technical colleges had sought to imitate universities as closely as possible, and a mathematician like Sommerfeld would have been welcomed as one who could be counted on to provide the requisite *Bildung* (cultivation) for engineering students. Indeed, it was in this period that Klein himself took up a position at the technische Hochschule in Munich, his classes on analytic geometry drawing over two hundred students.[37] By the turn of the century, however, an increasingly muscular and praxis-minded engineering community could set its own terms, consciously contrasting the aims of technische Hochschulen with the classical university.

Around the middle of the nineteenth century, when the Verein Deutscher Ingenieure (Society of German Engineers) was founded, an enormous distinction still divided the “artisanal” engineer from the Hochschule engineers who led the society. Without the social status that in England, for example, had followed from an obvious economic utility, and in the absence of an effective German bourgeoisie, the society’s leadership had sought a means of raising the public status of engineers by allying them with an intellectual aristocracy, the so-called German Mandarins.[38] The espoused ideals of engineering became those of the mandarinate: pure over applied science, general cultivation over specific training. Engineers would pursue the abstract ethics of German Kultur in an institution for higher learning rather than grub after money in a workshop or factory.

In the 1880s, however, “entrepreneurial” visions of engineering replaced “professorial” ones. Under the leadership of Theodor Peters, who became the business manager of the Verein Deutscher Ingenieure in 1882, the society emphasized far more “practical” issues, attracting to its Zeitschrift authors who “wrote about concrete matters and emphasized criteria such as application, cost effectiveness, fuel efficiency, material strength and ease of construction.”[39] In terms of education, this change in outlook meant radical alteration. At the level of secondary schooling, moving away from its former support for the classical Gymnasium, the association began a vehement push for the modern Realschulen.[40] At the tertiary level, new community leaders argued for a vision of technische Hochschulen as institutions equal to universities, but nonetheless completely independent of, and fundamentally different from them (see figures 1.1–1.3).[41] This effort to keep the two institutions separate but equal would constitute one of the principal differences between these engineers and Klein, who sought to bring them ever closer.

After the late 1870s, the emphasis was on increased shop and laboratory training for engineering students at the technische Hochschulen. In terms of detailed curricular change, the 1890s and later years saw the “de-emphasis of calculus in favor of less precise but much more pragmatic graphic methods.”[42] More generally, and

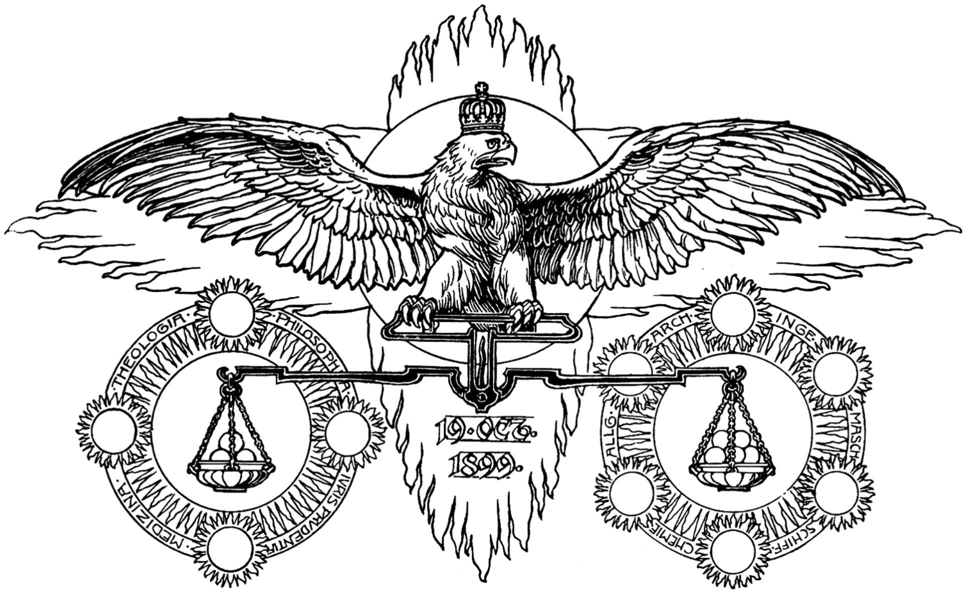

Figure 1.1
The four faculties of the university (theology, philosophy, medicine, law), represented by four spheres, balanced by the six sections of the technische Hochschule (architecture, civil engineering, mechanical engineering, shipbuilding, chemistry and mining, general). An imperial eagle perches above the scale. Source: *Die Hundertjahrfeier der kgl. T. H. zu Berlin, 1899* (1900).

controversially, prominent engineers, including Alois Riedler, argued against what they portrayed as an excessive reliance on theory over practice—on *Wissen* (knowledge) over *Können* (Capability)—and began to agitate for a "demotion" of mathematics and other theoretical subjects to the status of "auxiliary sciences" [*Hilfswissenschaften*].[43] Just how vehement this agitation was can be seen from the lines below, from a tract written in 1895. Having acknowledged that engineering requires the aid of sciences to climb the "heights" of the knowledge of reality, Riedler asserted that

> the ruling "Theory" remains below in the comfortable valley: The terrible preparatory education forces it to do so. Down there in the valley, it pursues all kinds of Gymnastics, [yet] knows not itself the efforts and dangers of the mountains and also deceives its disciples about them. The disciples probably storm the heights sometimes, far away from the creating world, with neither aim nor purpose. But the learned, unfruitful theory, when it raises itself to bold flight, then flies from the sight of the real world, up over the clouds to Abel and Riemann, where the Theta functions disappear, where the "special" concept "Dimension" is replaced by the general concept "manifold" and then can be gymnastically performed [*geturnt*] in a world of four and more manifolds.[44]

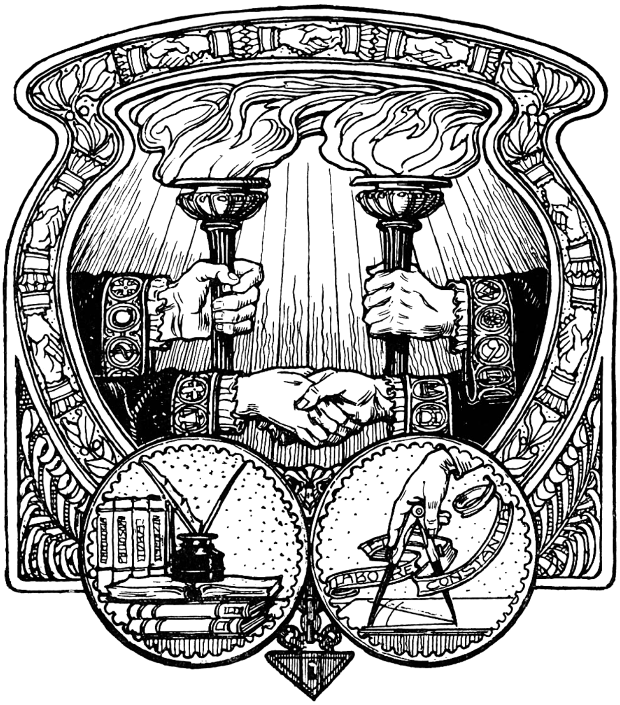

Figure 1.2
The university (represented by four texts, labeled according to the four faculties) clasps hands with the technische Hochschule (represented by a hand using a compass). Note, further, that the V shape subtended by the quills in the ink-pot on the left opens to the sky, whereas the V formed by the compass offers an inverse image, opening to the Earth. Source: *Die Hundertjahrfeier der kgl. T. H. zu Berlin, 1899* (1900).

In view of how closely this mocked mathematics resembled some of the work of the Göttingen school, it should be taken as a mark of Klein's power that he was able to place Sommerfeld in a technische Hochschule at all.

A great deal was at stake for young theoreticians in the debate over the importance of the mathematical sciences to engineers. Riedler and others were arguing for a substantial reduction—in schools that had long been seen as stepping stones to better jobs in universities—in both the numbers and significance of such young scholars. In this environment, Sommerfeld's 1903 speech "The Scientific Aims and Results of Modern Technical Mechanics" takes on a particular significance. One can read it both as a defense of the technical sciences, now that "they have developed from their own innate power a confident and self-sufficient position," and as a claim for the relevance of theory to such sciences.[45] Sommerfeld, in other words—as befitted a man in his position—was seeking to play two roles, both that of theoretician and of engineer.

Figure 1.3
The six sections of the *technische Hochschule*. Note that the cherub representing the “general” section is the only one not using his hands. Instead, he stares (rather perplexed) at an integral. Source: *Die Hundertjahrfeier der kgl. T. H. zu Berlin, 1899* (1900).

Sommerfeld and Klein

Sommerfeld presented his paper on modern technical mechanics as part of a panel organized by Klein, and there are strong similarities between the positions of the two men. Klein's program sought to bring Hochschulen and universities closer together. This program, however, did not advocate equality for engineers; rather, it sought to subordinate all fields to the central control of a mathematics that would function as "a specific power pervading the whole."[46] Thus, his plan for a Göttingen institute for physical technical research—posited as an educational counterpart to the physikalisch-technische Reichsanstalt—would also have a complementary relationship to the technische Hochschulen. The latter would provide an engineering education for the "Front-Line Officers," while Klein's smaller, more exclusive institute would supply a specific education in the exact sciences for future "General Staff Officers."[47] It was not difficult to work out the implied power relations between the two, and one can see why engineers might fight so strongly to keep the two institutions separate while guaranteeing their equal status.[48]

It is perhaps not surprising that Sommerfeld, Klein's student and supporter, was met with distrust when he took up his position at Aachen. And although Sommerfeld claimed to win over many of his doubters, there is a strong sense in which he remained far more of a member of the general staff than a front-line officer. He certainly worked on engineering topics, examining the problems involved in railway braking, studying the operation of dynamos, and, in what he saw as his most important project, working on the hydrodynamic theory of lubrication.[49] Yet even in the latter the aim of the task was less that of providing an effective solution to a practical problem—Sommerfeld admitted that his theory "doesn't correctly treat in all parts the real occurrences of bearing friction"—and more that of demonstrating the general applicability of the mathematics. His purpose, he claimed, was merely "to show how far one can come with the pure hydrodynamic theory."[50] The enjoyment of the project, he would write later, came from "helping the power of mathematical-physical thought to a victory in its exact treatment of an apparently inapproachable subject."[51] The papers on this and other "technical" topics should be seen as displays of mathematical virtuosity and proofs that engineering was part of the whole that mathematics pervaded.

And yet it is also necessary to distinguish between Klein and Sommerfeld. Klein, it has been argued, was driven to his attempts to bring universities and technische Hochschulen together by the fear that an increasingly powerful engineering movement would sideline university mathematics.[52] He thus pushed for a mathematics that would be more useful for the technical sciences, irritating both engineers (who were increasingly independent) and neohumanist mathematicians (who resented the importation of "Americanismus" and the loss of their pure ideals).[53] But, as the comments about the Göttingen institute imply, Klein's position was not completely centered between these two poles. While his rhetoric was as much about mathematicians learning from engineers and vice versa, this was not the way that things actually played out. Thus, in spite of his support for the efforts of those who worked on technical mechanics, it was not such a compromise between engineering and mathematics that emerged in 1898 as a result of Klein's maneuverings over teaching reform, but the far less industrial "applied mathematics." An alliance with the physical sciences was pursued largely in order to strengthen the position of mathematicians, and a separation between mathematicians and natural scientists could still occur, Klein noted to Paul Gordon in 1890, if "we are strong enough or feel overly restricted."[54] The ultimate aim was the salvation of mathematics, and Klein's own neohumanist predilections meant that he leaned more strongly toward a solution that would keep mathematics strong and independent. Quite simply, he advocated mathematical engineering rather than engineering mathematics.[55]

For Sommerfeld, on the other hand, with his chair in technical mechanics, the applications of mathematics were avowedly industrial. In his 1903 paper, Sommerfeld spoke on his own field, discussing problems of machine construction and of gyroscopic compasses on ships. The audience, of course, was different here, and Klein had organized the session, so one can assume he was in full accord with the idea that mathematicians should be told to wake up to the need to enter the industrial age. Yet Sommerfeld's "active contact with the problems of technology" was, as the topics of his papers demonstrate, more than an occasional tactic. His aim may have been, like Klein's, to sell mathematics to the world, but Sommerfeld also clearly believed that mathematicians and physicists had something equivalent to buy. Since Sommerfeld was the outsider at Aachen, it was largely a one-way trade. But once he had (much to Klein's chagrin) given up that post in favor of Munich, the situation changed. Like his interest in mathematical physics, Sommerfeld's enthusiasm for technical mechanics became an integral part of his theoretical physics.

One need not portray Klein as a Machiavellian figure, politicking in support of his own field, in order to acknowledge the local and contextual differences between him and Sommerfeld. Those differences are clear in a book they wrote together: the four-volume, 966-page *Über die Theorie des Kreisels* (On the Theory of the Gyroscope).[56] The idea of working on such a topic came to Klein during the annual meeting of the Verein Deutscher Ingenieure in 1895. "Here the thought came to me," he wrote later, "of connecting theoretical reflections that were familiar to me with the needs of physical and technical understanding via a detailed lecture over a specific mechanical problem, like gyroscopic theory."[57] A two-hour lecture followed, after which Klein approached Sommerfeld about writing up and publishing a more detailed but still small treatment of the subject, much as had been done with the lectures that same semester on number theory.[58]

The timing of the book, with the first volumes appearing at the height of Klein's battles, is crucial for understanding its purpose. Whereas the second volume was a more collaborative effort, the first volume was almost entirely based on Klein's ideas, and we can use it as a means of better understanding his position.[59] Klein had long argued for a more intuitive means of teaching mathematics, one that would have an appeal beyond the ranks of pure mathematicians and would "establish not only a knowledge of mechanics, but so to say, a feeling for it."[60] At the same time, he wanted to push the idea that mathematics was useful beyond its own boundaries and that it had a role to play in practice as well as in theory. *Über die Theorie des Kreisels* would thus have two aims—one for each of its audiences. Gyroscopic motion was a well-known but rather poorly understood problem in mechanics—one, moreover, that had applications in many neighboring fields, including astronomy and physics. The gyroscope thus offered a bridge to the natural sciences. At the same time, an appropriate analysis of gyroscopic motion could provide an example of a general approach to mechanics for pure mathematicians. Klein deplored what he referred to as the too abstract and formal direction that mechanics had taken in Germany, a tack that hindered a direct understanding: "The student who probably learns general mechanical principles analytically by heart, for that reason never grasps their actual mechanical meaning in a lively enough way and appears, when positioned before a specific problem, clumsy in its solution."[61]

The solutions the text offered, which it drew largely from the pattern of English textbooks, would be manifold. First, it eschewed the approach of Lagrange and those who followed him, and emphasized the importance of geometrical representation. Second, it emphasized the importance of understanding the *causes* of motion and made these comprehensible through the introduction of vector notation. Finally, emphasis was placed not on understanding the mechanics of the problem through the equations, but rather on allowing "the analytical formulation, as a final consequence, to appear of its own accord from a fundamental understanding of the mechanical relationships."[62]

The book that Klein had planned was thus principally one that would elucidate complicated mathematical concepts by applying them to a more easily grasped, more intuitive mechanical object. Through a work originally intended to span at most two volumes, the reader would be introduced to some of the mathematics of Euler, Lagrange, and Jacobi, and to elliptical functions and integrals. The place of these topics in areas outside of mathematics would be emphasized to appeal to a wider audience. And yet, although Klein was to talk in 1922 of "the needs of physical and technical understanding" that the gyroscope book was supposed to satisfy, the book that he planned in 1895 did not deal with any technical problems. The "applications" mentioned were to astronomy and physics, and even these were largely dealt with in the volume that Sommerfeld completed while in Aachen. The technical applications of *Über die Theorie des Kreisels* were all undertaken by Sommerfeld.[63]

The opening words of Klein and Sommerfeld's joint introduction to the fourth volume (1910) laid out the division of labor that had developed between the inception and the completion of their work:

> When Felix Klein held a two-hour lecture in the winter semester of 1895/96 "On the Gyroscope" he intended it in the first instance to emphasize the immediate grasp of mechanical problems that was extended particularly in England in opposition to the more abstract coloring [*Färbung*] of the German schools and, on the other hand, to render fruitful the methods of Riemannian function theory that were principally built up in Germany. The consideration of applications and physical reality was at the time certainly indicated and heartily supported, but was not yet carried out to its full extent.
>
> In the extensive publication stemming from the pen of A. Sommerfeld the interest in applications took over more and more, particularly after his taking up of chairs in technical mechanics and later in physics.[64]

The result of the increasing emphasis on astronomical, geophysical, and technical applications, the authors continued, was a forced reworking of the kind of mathematics being utilized within the text. Thus, although the original aim of the text under Klein had been to produce intuitive instruction methods for particular mathematical techniques, Sommerfeld had introduced a change in mathematical content to suit an emphasis on scientific and engineering applications.[65]

Hydrodynamics: Theory in Practice

One can track, through the volumes of *Über die Theorie des Kreisels*, the changes in Sommerfeld's intellectual interests: mathematics when he arrived in Göttingen, applied mathematics after his time as assistant to Klein, increasingly industrial problems while a professor for technical mechanics at Aachen. In the same period, however, Sommerfeld had been engaged on another extensive project: the editorship of the physics section of *Die Enzyklopädie der mathematischen Wissenschaften mit Einschluss ihrer Anwendungen*.[66] This latter, after his failed attempt to pass the job on to his colleagues, became Sommerfeld's letter of introduction to leading members of the physics community, including, in particular, Boltzmann and Lorentz. Between these and the contacts made during the work on gyroscopic theory (including Carl Cranz, the "Pope of Ballistics"), Sommerfeld came to be connected to a majority of mathematics-minded professors in the German-speaking world.[67]

This multitude of interests came together after Sommerfeld's call to Munich in 1906, as can be seen in his work on hydrodynamics. A subject Sommerfeld had studied as a mathematician in 1900, and had called upon in his examination of the theory of lubrication while at Aachen, now became the topic of a lecture series in the summer semester of 1908.[68] In 1909, after a five-year hiatus, he returned to the question of hydrodynamics in his published work. His paper "A Contribution to the Hydrodynamical Explanation of Turbulent Fluid Motion" displayed the multiple resources upon which its author was able to draw: published as part of the proceedings of the fourth international mathematical congress in Rome, its second reference was to a book on theoretical physics, and its first section offered a literature review of works that dealt with the relationship between theory and praxis.[69] The paper began as follows: "The cleft between technical hydraulics on the one side and theoretical hydrodynamics on the other, between the investigations of large-scale properties on the one hand and laboratory investigations of capillarity on the other, has often been noted and bemoaned."[70] The gap was first bridged, Sommerfeld claimed, by Osborne Reynolds, who introduced a theoretical means of examining the phenomenon of turbulence. He did not, however, supply an exact method of calculating the value of the parameter that bears his name (the Reynolds number). This was the role that Lorentz played, simplifying the case of Couette's flow (that between two cylinders) to the geometrically easier case of the flow between two parallel plates, one moving at a fixed velocity with the velocity gradient approximated as linear.[71]

Sommerfeld's work, which took up the problem at this point, restricted itself to examining the mathematically easier case of the *onset* of turbulence—the border between turbulent and non-turbulent flow—and did not consider the character of the flow above the critical value of Reynolds's parameter. This problem, as well as a complete discussion of the equation he arrived at "which appears to me to form the actual content of the problem of turbulence," he reserved for another time.[72] The problem chosen, that of turbulence, was portrayed as standing on the border between technical and theoretical studies, one foot on either side. Technical hydraulics and theoretical hydrodynamics come together in a subject that would form a part of the lectures on theoretical physics. The fusion would pervade the work of the Sommerfeld School more generally. Sommerfeld's research topics often became dissertation projects for his students; thus he passed on the problem of turbulence to Ludwig Hopf in an attempt yet again to unite the worlds of theory and engineering.

Hopf's partially experimental work was touched upon above, but little mention was made there of the importance of technical applications within it. At first glance, studies of sugar water in troughs may seem obscure, but the point was to extend Sommerfeld's own studies. The open channel in which the water flowed would be a different case—and one more often seen in the real-world systems, such as rivers or canals—from that usually studied, where the fluid was enclosed on all sides. Sommerfeld hoped that the different set-up would allow the observation of different phenomena, such as the effect of capillarity, as the water shifted from laminar to turbulent flow.[73] Hopf's project would thus, quite literally, combine what Sommerfeld saw as "large-scale properties," and phenomena studied in the laboratory, the sets of observations that Sommerfeld associated, respectively, with technical hydraulics and theoretical hydrodynamics.

In almost perfect mimicry of his supervisor's paper, Hopf's dissertation began with the placement of hydrodynamics within physics, and continued with a discussion of the theory/praxis divide:

In perhaps no other branch of physics do theory and praxis remain as foreign to each other as in Hydrodynamics; and it is particularly peculiar and regrettable, given that both have grown into very extensive and important areas. Alone, the hydrodynamic theory on the one hand faced insurmountable mathematical difficulties in its treatment of phenomena in real fluids; on the other hand it opened up an easily accessible and praiseworthy [*dankbar*] area by going around these difficulties via idealized frictionless and eddyless fluids. Engineering [*Technik*] naturally developed completely independently of these theories and created its own mathematical fundamentals, with all the pros and cons of empirical formulas. Right at the moment the need for a metamorphosis of these conditions has become many times more keen, and a great deal of attention has been turned to real fluid [dynamical] problems.[74]

Of these latter, that of fluid flow, Hopf claimed, "is in fact the most urgent and most interesting of hydrodynamical problems, precisely because it can be completely and satisfactorily solved."[75] The laboratory examination of these "real-world" flows would constitute half of Hopf's dissertation.

Sommerfeld described the second half, "On Ship Waves," as "purely theoretical" when he reported on the project to the philosophical faculty.[76] That was certainly true in the sense that there were no *experimental* observations in that section. Yet even here, Hopf made clear the dual role that his investigations served. "The shape of waves which accompany a ship moving on tranquil water, and the corresponding resistance, against which the ship exerts continual effort, are not only of great significance practically, but also offer the theoretician several interesting problems."[77] As Sommerfeld had advised, the theoretician was coming into active contact with the problems of technology—so active, in fact, that Hopf's investigation later found outside financial support from the Gesellschaft Mittlere Isar, a group that wanted to regulate the flow of Munich's largest river.[78] "Purely theoretical" it may have been in one sense, but Hopf's dissertation found a significant role to play in practice.

Physics: The Electromagnetic Worldview[79]

It should by now be clear that the theoretical physics espoused by Sommerfeld was considerably more than simply a subset of physics. Sommerfeld drew heavily from his knowledge of both mathematics and technical mechanics in constructing his own brand of theory. Theoretical physics in Munich was an interdisciplinary creation. Yet it should also be clear that Sommerfeld's was a selective borrowing. Although he collaborated on two texts with the mathematician Felix Klein, one on number theory and the other on gyroscopes, it was only the latter that became a resource for dissertation topics in Munich. Although he published papers during his Aachen period on both railroad brakes and lubrication (in fact related topics), it was the hydrodynamic theory utilized in the latter that passed into his lectures on theoretical physics. Along with this selectivity came a process of modification. If the (traditionally mathematical) problem of the diffusion of heat was to become a topic in theoretical physics, then it was to be altered to fit into its new surrounding. If technical hydraulics was to be part of the Munich curriculum then this would be in combination with more overtly theoretical considerations—the analysis of wear on ball bearings would be replaced by the still "practical" but less entirely industrial problem of turbulence in open channels.

Sommerfeld's papers and lectures dealt, not with general principles, but with specific problems. Mathematical resources and theoretical underpinning were marshaled to get to a point where these problems could be dealt with by Sommerfeld in his research and by his students in their dissertations. The aim was not, that is, to create a unified, general physics and a community that corresponded to it, but to study—and to create students who would study—particular problems, drawing topics from a wide range of sources. The wideness of this range contrasts with historians' concentrations on what have come to be called the "twin pillars" of modern theoretical physics—the quantum and relativity theories. Yet Sommerfeld studied these subjects too. With the nature of theoretical physics still under construction, it was not merely the "outside" disciplines that were selected from and modified in the building up of the Sommerfeld style of theoretical physics. Sommerfeld's approach to the third strand—physics—was also one of partial inclusion.

By 1911, the year he presented a paper on the "quantum of action" at the Solvay Conference, Sommerfeld vocally espoused the necessity of some form of a quantum hypothesis. In his earlier lectures, however, his reservations concerning the validity of Planck's position were far more apparent. While Sommerfeld's resistance to the theory has been noted before, usually in passing, there has been no detailed analysis of why this should have been so. What his lectures make clear is that Sommerfeld's attachment to the electromagnetic worldview led him to favor the so-called Lorentz-Jeans formula for black-body radiation, despite its known failure to accord with experiment as well as Planck's did. This conclusion, as well as illustrating elements of the Sommerfeld style, has deep implications for our understanding of the "conversion" of several leading physicists to the quantum theory. In *Black-Body Theory and the Quantum Discontinuity*, Thomas Kuhn claimed that a lecture that Lorentz gave in Rome in 1908 marked a turning point in the history of the early quantum theory and led to a growing acceptance of the idea of a quantum discontinuity. While I grant the importance of the Rome lecture, it is argued here that the acceptance of discontinuity followed what was, in fact, a more profound realization. In 1906, Sommerfeld still assumed that electromagnetic theory was untroubled by the problems that plagued mechanics. It was to be expected, he implied, that a mechanical description of the electromagnetic ether should produce inconsistency in the form of the incorrect blackbody curve. Lorentz's lecture of 1908 was the first statement by one of its leading proponents that the electromagnetic worldview must fail for the case of radiation. What was at stake for a significant part of the theoretical physics community was, far more critically than the question of discontinuity, the question of whether the electromagnetic worldview could incorporate Planck's results, or whether the universalizing dream of the electromagneticists had to be abandoned. As Sommerfeld's Solvay paper would show, it would be the latter view that prevailed.

Relativity and the Electromagnetic Worldview

In the winter of his first year at Munich, Sommerfeld began a series of lectures on "Maxwell's Theory and Electron Theory," a topic described in a letter that December to Lorentz as the "burning questions of electrons."[80] Sommerfeld introduced his students almost immediately to the current problems plaguing the subject area at hand. After a short historical overview of the topic, he noted:

> Of course all this is only valid for the negative electron and the apparent mass bound to it. About the positive electron and the matter apparently inseparably bound to it, we know nothing. Also there are still serious difficulties to overcome regarding electro-optical phenomena, which should, according to the electron theory, show the influence of the earth's movement. Lorentz recently said, in reply to my question as to how the electrons were doing: badly. The difficulties are not overcome in the face of Kaufmann's newest experiments. Therefore Planck was also pessimistic.[81]

The reference was to the experiments of Walter Kaufmann, who had attempted to distinguish between the two most prominent electron theories at the time: that of Max Abraham, a former student of Max Planck's, who assumed a rigid, spherical electron, and the so-called Lorentz-Einstein theory, which assumed a deformable electron.[82] Planck, in his report on Kaufmann's results at the 78th meeting of the Gesellschaft Deutscher Naturforscher und Ärzte (GDNA, Society of German scientists and physicians) in September 1906, called the two possibilities respectively the "sphere" and the "relative" theories and concluded that one still couldn't work out which of them was right. "Therefore," he wrote,

> no option remains but to assume that some essential gap is still contained in the theoretical interpretation of the measured values, which first has to be filled before the measurements can be utilized for a definitive decision between the sphere theory and the relative theory. One could think here of various possibilities, but I don't want to discuss these further, because to me the physical foundations [*Grundlagen*] of each theory appear too uncertain.[83]

Sommerfeld commented on Planck's "pessimism" in the discussion that followed. A strong supporter of Abraham's theory, Sommerfeld disliked in equal measure, as he wrote in a letter to Lorentz, both the latter's deformable electron and Einstein's "deformed" time.[84] The 38-year-old Sommerfeld was clearly one of the younger contributors to the conversation in Stuttgart, and his suggested explanation for the difference in opinions caused some merriment:

> Sommerfeld (München): I would not, for the time being, like to ally myself with the pessimistic standpoint of Mr. Planck. In the extraordinary difficulties of measurement the deviations could perhaps yet have their ground in unknown sources of error. In the question of principles formulated by Mr. Planck, I would suspect that the men under forty years of age will prefer the electrodynamic postulate, those over forty the mechanical-relativistic postulate. I give preference to the electrodynamic. (laughter)[85]

In an article written in 1970, the historian Russell McCormmach explained Sommerfeld's hostility to the "mechanical-relativistic postulate" as deriving from the younger physicist's devotion to an "electromagnetic view of nature."[86] This worldview encompassed three related positions: a distaste for, and mistrust of, mechanical modeling, especially as applied to microscopic phenomena; a belief that the only physical realities were electromagnetic in nature; and a programmatic commitment toward a "concentration of effort on problems whose solution promised to secure a universal physics based solely on electromagnetic laws and concepts." [87] This last notion of an electromagnetic *program* is crucial. Many, if not most physicists at the turn of the century made some use of electromagnetic concepts in their work. Yet, as Sommerfeld's comments make clear, even when working on the subject of the electron itself, not all methods and approaches were equally palatable. Preference, from the proponent of the electromagnetic worldview, would go to those theories that used only electromagnetic properties (or those assumed electromagnetic in nature, such as the electron's mass),[88] eschewing mechanical concepts like deformability.[89] It was thus not merely the problems chosen, but also the modes of solution deemed acceptable, that marked Sommerfeld out from his older colleagues.

In common with many physicists of the generation that completed their university studies in the last two decades of the nineteenth century, Sommerfeld had seen what was portrayed as the gradual failure of the mechanical worldview, which held that all physical phenomena could be explained in terms of the equations and concepts of mechanics. His generation had also witnessed both Hertz's discovery of electromagnetic waves and the later successes of Lorentz's electron theory—crowned with the discovery of the electron in the last years of the century. In 1901, Wilhelm Wien (37 years old at the time) discussed the possibility of reducing all of mechanics to electromagnetic theory. A return to mechanical explanation thus seemed to be a "throwback." "For the younger physicists," wrote McCormmach, "the electromagnetic concepts clearly pointed to the future of physics."[90]

One should not, however, confuse support of the electromagnetic program with a form of theoretical determinism. Many of the problems studied in Munich can certainly be understood as part of the furtherance of the program of the electromagnetic worldview, understood not merely as support for microphysical studies, such as those of the electron theory, but also macrophysical ones, like those of wireless telegraphy. Nonetheless, the aim of Sommerfeld's lectures does not seem to have been to establish the electromagnetic worldview as the sole philosophy of the Munich school, a "hegemony" like that supposedly put forward by Niels Bohr in Copenhagen in the 1920s with respect to the correspondence and complementarity principles.[91] This would have been almost impossible in any case, for as a program, the electromagnetic worldview was as much a promise for the future as a claim about the present. As Sommerfeld noted in his lectures: "Certainly, the electromagn[etic] foundation of mechanics is the music of the future [*Zukunftsmusik*]. But I am convinced that matters will proceed here just as with the music that received the name *Zukunftsmusik* thirty years ago."[92]

The worldview functioned as one standpoint from which Sommerfeld and his students could understand and critique the work of others, and the lectures are not (even where it would have been possible) written entirely from a single perspective. The electromagnetic worldview provided the glasses through which Sommerfeld examined the world at this time, but one does not get the sense that he insisted that everyone else use the same prescription. He was selective, but neither exclusive nor dogmatic. With this proviso regarding the flexibility of the electromagnetic program in mind—at least in Sommerfeld's hands—the next section explores the program in action, for it was not merely the relativity theory that was viewed through the lenses of the electromagnetic worldview. The quantum theory—more particularly, the theory of black-body radiation—was judged according to its fit with the requirements of the electromagnetic program as well.

Teaching Planck's *Lectures*

In 1906 Planck published his now-famous book *Vorlesungen über die Theorie der Wärmestrahlung*, both a summary of his earlier work and a continuation and reexamination of it.[93] Sommerfeld appears to have gone over the text with a fine-toothed comb, adopting both Planck's summary of previous approaches to the problem of radiation and, for the most part, his units.[94] After beginning with the rapidly written claim that "radiation is a focus of modern research," Sommerfeld divided previous approaches, much as Planck had, into three types: thermodynamic, electrodynamic, and statistical methods.[95] In the thermodynamic category he placed the work of Kirchhoff, Stefan and Boltzmann, and Wien; in the electrodynamic, that of Helmholtz, Maxwell, Rayleigh and Jeans, and Lorentz; whereas the statistical principally dealt with the methods of Boltzmann, Gibbs, and Planck. An outline of the structure of Sommerfeld's lectures is reproduced here as table 1.[96] The column on the left represents the proposed structure of the course, as laid out in Sommerfeld's first lecture. The column on the right provides the topic headings for the course actually delivered. The last three sections of the proposed course were compressed into a single one.

Table 1: Structure of Sommerfeld's Lectures

Sommerfeld's Lecture First Outline	**Sommerfeld's Lectures on *Theorie der Strahlung* Sommer 1907**
§1. Kirchoff 1859 §2. Stefan-Boltzmann 1879 u. 1884 §3. W. Wien. 1893 Das Wien'sche Verschiebungsgesetz	Einleitung u. Übersicht (Introduction and Overview)
Elektrodyn. Methoden	§1. Kirchhoff §2. Stefan-Boltzmann'sches Gesetz (Stefan-Boltzmann Law) §3. Wien'sches Verschiebungsgesetz (Wien's Law of Displacement)
§4. Helmholtz 1856 Reciprocitätssatz. Umkehrbarkeit des Strahlenganges. §5. Maxwell 1873 Strahlungsdruck §6. Rayleigh-Jeans 1905 §7. Lorentz 1903	Elektrodynamisches Teil
Statistische Methoden	§4. Maxwell'sches Strahlungsdruck (Maxwell's Radiation Pressure) §5. Bewegter Spiegel (Moving Mirror) §6. Jeans Ableitung eines Grenzfalles des Strahlungsgesetzes (Jeans Derivation of a Limiting-Case of the Radiation Law) §7. Lorentz Ableitung derselben Grenzformel aus der Elektronenth (Lorentz's Derivation of the Same Limit-Formula from the Electron Theory)
§8. Verteilungssatz der Energie §9. Wahrscheinlichkeit und Entropie, nach Boltzmann §10. Der Planck'sche Oscillator §11. Das Planck'sche Strahlungsgesetz §12. Das Planck'sche Elementarquantum h der Energie. Folgerungen von Einstein	Dritter Abschnitt. Statistisches.
	§8. Beispiel aus der Gastheorie §9. Planck'sche Theorie

The difference between Sommerfeld's discussion of previous treatments and his analysis of Planck's own contribution jumps to the eye. Whereas his summary of earlier research (sections 1–8) was in some cases as detailed as Planck's, his discussion of Planck's theory in section 9 is remarkable concise. Sommerfeld achieved this by excising almost entirely the discussion of the production of radiation by Hertzian resonators, a topic that took up nearly one-third of Planck's text. Instead, within half a page of expressing the energy of a Hertzian dipole in terms of its total energy U, the electromagnetic moment f, and constants K and L,

$$U = Kf^2 + \tfrac{1}{2}L\dot{f}^2, \tag{1}$$

Sommerfeld merely restated the relation Planck derived between the total energy of a resonator and its average energy $\bar{u}$ at frequency ν:

$$U = \frac{c^3}{8\pi\nu^2}\,\bar{u}\,. \tag{2}$$

A parenthetical note after the equation promised that a proof would follow, perhaps as an exercise, since no such proof appeared in the lecture notes themselves. Following Planck closely from this point on, Sommerfeld eventually arrived at Planck's equation, relating energy to frequency for a black body at a particular temperature T:

$$U = \frac{h\nu}{(e^{h\nu/kT} - 1)}. \tag{3}$$

Having obtained Planck's formula, Sommerfeld immediately launched into a section labeled "critical remarks." Sommerfeld appears to have paid close attention to comments made by Paul Ehrenfest in the *Physikalische Zeitschrift,* the year Planck's book appeared.[97] Planck had introduced the resonators into radiation theory in part in order to obtain a parallel to the Maxwell-Boltzmann distribution in kinetic theory. Just as interaction between molecules brought about the Maxwell-Boltzmann distribution as an equilibrium distribution of velocities, the interaction of resonators was supposed to ensure that an initially arbitrary distribution of energies in a black body would result in an equilibrated radiation. Ehrenfest quashed that possibility by showing that the resonators could not do what was required. Since they emitted and absorbed energy at characteristic frequencies, only resonators at the same frequency interacted, producing an equilibrium distribution of intensity and polarization for each color. For resonators at different frequencies, however, no interaction was possible, so any arbitrary frequency distribution would persist. Ehrenfest wrote:

1) The frequency distribution of the radiation introduced into the model [described by Planck] will not be influenced by the presence of arbitrarily many Planck resonators, but will be permanently preserved.

2) A stationary radiation state will [nevertheless] result from emission and absorption by the oscillators in that the intensity and polarization of all rays of each color will be simultaneously equilibrated in magnitude and direction.

In short: radiation enclosed in Planck's model may in the course of time become arbitrarily disordered, but it certainly does not become blacker.—For the discussion to come the following formulation is especially suitable: Resonators within the reflecting cavity produce the same effect as an empty reflecting cavity with a single diffusely reflecting spot on its wall.[98]

Planck had made similar remarks at the end of his lectures, realizing, in his book's conclusion, that much of his analysis to this end had been fruitless.[99]

It is clear that Sommerfeld drew his inspiration from Ehrenfest's critique. His first objection, under the heading "The Role of the Resonators," reads:

The resonators only operate like a Reagent, strips of blotting paper, not like a catalyst [*Ferment*], coal dust. The non-black radiation remains non-black. The resonators can only increase the disorder of directions, not the color distribution. Because the resonator only works in the region (ν, $d\nu$) to which it is allotted [*abgestimmt*]. The resonator does nothing more than a diffusing mirror. (Cf. § 6 Jeans).[100]

Another comment referred to the dissimilarity between the methods of Boltzmann and Planck. Whereas Boltzmann had proven that the entropy, *S*, was a maximum for the Maxwell-Boltzmann distribution (that is to say, that the equilibrium distribution was the most probable one), Planck had skipped this step. Sommerfeld noted, apparently again following Ehrenfest, that the "substitution for this unfortunately missing consideration" was the "auxiliary assumption" [*Hilfsannahme*] that we now know as Planck's hypothesis, $\varepsilon = h\nu$. It was only with this hypothesis that Planck was able to get to a result that provided the requisite dependence of the total energy on both temperature and frequency.[101]

Yet, while Ehrenfest was prepared to take the close accordance of Planck's formula with experimental data as proof that there was some validity to his analysis, Sommerfeld was less enthusiastic. In fact, although Ehrenfest rejected the resonator approach, he did not reject the recourse to combinatorics. Rather, he explained the fundamentally different assumptions that led to the different results of Boltzmann (his former teacher) and Planck. For Ehrenfest, Planck's hypothesis was an additional (if peculiar) constraint that led to an experimentally verifiable result. Ehrenfest was willing to accept a version of Planck's thermodynamical/statistical approach as long as the appeal to resonators was abandoned. For Sommerfeld, on the other hand, the failure of Planck's resonators seems to have appeared emblematic of the problem with Planck's method in general, and Sommerfeld treated the "auxiliary assumption" as little more than something that allowed Planck to get to the desired result. "I think it is very possible," he wrote in the lecture, "that Planck's formula is only a good approximation."[102]

As an approximation, Planck's equation was not alone. Sommerfeld described the result of the Englishman James Jeans as an "approximation" as well. Jeans had assumed that energy could be distributed equally among the eigenvalues of vibrations within a cube of side length L. Doing so, however, resulted in a curve that was not in accordance with the experimental data of researchers like Planck's friend at the Berlin technische Hochschule, Heinrich Rubens.[103] Sommerfeld explicitly compared the assumptions implicit in Jeans's derivation to those of Planck in section 6 of his lectures (the section to which he pointed at the end of his first "critical remark"):

> The most interesting question is now this: Why do we only obtain an approximate formula?
>
> 1. The assumption of the equipartition of energy is not generally valid for the Aether, *it is derived mechanically*. It is, so to speak, [mere] chance that it is still valid for long waves. Long thereby means nothing: Size depends on L, L drops out.
>
> 2. Standp. of Planck. The quantity h is the quantum of action of energy. The energy can not be divided arbitrarily. If the smallest amount of energy were $h = 0$, then Planck's formula would also reduce to that of Jeans.[104]

And yet, although both approaches were seen as only partially successful, and although Planck's formula seemed to fit the data better, Sommerfeld did not accept Planck's derivation. In deciding which theory to reject, the impotence of Planck's resonators outweighed the failure of the Rayleigh-Jeans equation to match available experimental results. In effect, Sommerfeld bracketed off the question of which expression was more correct in terms of its relation to experimental data. The choice between Planck's and Jeans's formulas was, rather, reframed as a choice between two distinct *methods*. In fact, Jeans's result, as Jeans himself had derived it, did not receive Sommerfeld's support. The italicized lines in the quotation above suggest that Sommerfeld was skeptical of the very basis of Jeans's derivation: the Englishman had assigned a *mechanical* property (the equipartition of energy) to what was—for a proponent of the electromagnetic worldview—a fundamentally non-mechanical ether. Section 7 of Sommerfeld's lectures, however, was titled "Lorentz's derivation of the same [i.e. Jeans's] limit formula from the electron theory." Methodologically , then, Jeans's expression was (or could be shown to be) a result following from the electron theory, and that spoke strongly in its favor.

Lorentz's derivation thus provided, in Sommerfeld's eyes, a positive endorsement of Jeans's formula. On the other hand, Sommerfeld saw significant difficulties in accepting Planck's approach to the theory of radiation. He laid these out in a series of "General Comments" toward the beginning of the lectures, offering a critique of the three possible methods for approaching the problem of radiation. Of the first, thermodynamics, he noted that it was at once "the most secure but the least satisfying" possibility. "In opposition to Energetics," he wrote, "one demands an understanding of Mechanism or Electrodynamism." The kinetic theory, Sommerfeld claimed, had eliminated thermodynamics by explaining its laws in terms of statistical mechanics. Along similar lines, "The program ~~offered by Planck~~ of radiation th[eory] should offer: to explain thermod[ynamics] electro-statistically."[105]

Planck, however, while utilizing the statistical techniques of the kinetic theory, had come out firmly on the side of thermodynamics in his analysis of heat radiation. Discussing the calculation of radiation intensity in his lectures, he noted that it was "in no way determined," so that "in a case where according to the laws of thermodynamics and according to all experience a single valued result is to be expected, pure electrodynamics leaves [one] completely in the lurch," and one in fact ends up with infinitely many solutions. Mechanics served no better: "The temporal course of a thermodynamic process cannot be calculated on the mechanical heat theory or the electrodynamic theory of heat radiation under the [same] initial and boundary conditions that completely suffice in thermodynamics for the single-valued determination of the process."[106]

For Sommerfeld, the fact that Planck did not seek to explain radiation solely in electro-statistical terms spoke against his methods: “Planck’s theory is therefore not ideal; the theories of Jeans and Lorentz are better in principle.”[107] Here, then, was the programmatic aim of the electromagnetic view of nature in operation—programmatic because, as noted earlier, Sommerfeld had specific objections to Lorentz’s particular version of the electron theory, preferring Abraham’s. Nonetheless, he clearly deemed either better than one that did not seek to reduce all other explanatory means to electrodynamics. Jeans’s result, as derived through the electron theory, was to be preferred over any result following from a system of thought that might seek to deny the unificatory capacities of electromagnetism. No doubt, like Lorentz himself, Sommerfeld hoped that a more complete electromagnetic theory would result in an expression in better accordance with experience and experiment. Until then, however, an “approximation” derived along correct programmatic lines trumped one derived in a manner deemed “not ideal.”

The continuation of Sommerfeld’s “General Remarks” shows him waxing lyrical over the total explanatory possibilities of electrodynamics, which “creates here as well the highest unity”:

> Heat (radiated) is light, therefore electr[icity?]; but heat is, on the other hand, molecular motion. ~~How it should~~ It must convert electr[ical] action into inertial action; as it does so, the theory shows the apparent degree to which kinetic energy actually should be electromagn[etic] energy of the charged matter. Therefore in short: From the ident[ity] of light Leslie Prevost Rumford 18th Cent. and heat, the id[entity] of light and electr[icity] Maxwell Hertz end of the 19th Cent. and the id[entity] of heat and molecular mechanics Clausius Maxwell Boltzmann 19th Cent follows necessarily the id[entity] of molecular mech[anics] and electrodynamics (20th Century).[108]

If Boltzmann had shown that thermodynamics reduced to mechanics, this last identity showed that both thermodynamics and mechanics could be reduced to electrodynamics. This conclusion, in turn, suggests a pointwise refutation of Planck’s “introductory theses” on the basis of the electrodynamic worldview. The responsive sentences below indicate Sommerfeld’s position:

> 1) Heat diffuses [fortpflanzt sich] in two different ways, conduction and radiation.
>
> 1a) Heat diffuses in only one way, electrod[ynamic], in conduction the electr[ic] fields of the charges are bound to the molecule, in radiation they spread out freely in the Aether.
>
> 2) Heat rad[iation] is much more compl[icated] than heat conduction, because in that case the state cannot be characterized through a vector.
>
> 2a) Heat rad[iation] is much easier than heat conduction, because the particularities of the charge distribution (matter) don’t play a part. In the Aether only the direction and intensity of the radiation, in heat conduction the directions of movement of the molecule as well.[109]

One can read these responses as the principle (and the "in principle") reasons that Sommerfeld approached *Wärmestrahlung* with skepticism. Although Planck's approach allowed a simpler calculation of certain fundamental constants (and here Sommerfeld was thinking much more of *k*, Boltzmann's constant, than *h*), it went against the worldview that Sommerfeld had adopted. Certainly, Planck's resonator approach had its own, intrinsic difficulties, but, more generally, it suffered from its adherence to a viewpoint that Sommerfeld and others were seeking to supersede with the physics of the future. As with his response to relativity theory, Sommerfeld considered quantum theory a step backward, presumably also the domain of men over 40, not the young-bloods in whose camp he placed himself.

Black Bodies in an Electromagnetic World

Kuhn's argument that Lorentz's lecture in Rome in 1908 marked the beginning of the acceptance of the "quantum discontinuity" runs as follows:

> During 1908 Lorentz produced a new and especially convincing derivation of the Rayleigh-Jeans law. Shortly thereafter he was persuaded that his results required him embracing Planck's theory, including discontinuity or some equivalent departure from tradition. Wien and Planck quickly adopted similar positions, the former probably and the latter surely under Lorentz's influence. By 1910 even Jeans's position on the subject had been shaken, and he publicly prepared the way for retreat. These are the central events through which the energy quantum and discontinuity came to challenge the physics profession.[110]

In the Rome paper Lorentz proved that the electron theory *must* lead to Jeans's result. That is to say, there could no longer be any suggestion of his electromagnetic approach avoiding the problems that followed from the equipartition theorem. Lorentz stated that such had been his hope, after reading Jeans's papers. Now that hope was officially dashed.[111] Without at this point making a choice between them, Lorentz then stated the difference between the Rayleigh-Jeans and the Planck case as baldly as possible. Accepting Planck would bring theory in line with experiment, but "we can adopt it only by altering profoundly our fundamental conceptions of electromagnetic phenomena." Accepting Jeans on the other hand, would "oblige us to attribute to chance the presently inexplicable agreement between observation and the laws of Boltzmann and Wien."[112] For experimentalists, the issue was now clear: Jeans's equation did not work at all. If the choice was between it and Planck's, then the latter had to be accepted. In a paper published a few months after the Rome lecture, Lorentz acknowledged that he had been convinced in the interim by the arguments of experimentalists (including Wilhelm Wien, Otto Lummer, and Ernst Pringsheim) and had abandoned any support for Jeans's equation.[113] For the final step, Kuhn claimed, Lorentz's "great personal authority" was responsible, to a great extent, for spreading the gospel to the rest of the physics community.[114]

But exactly what gospel was being spread? Participants in the discussion referred, variously, to the "Rayleigh-Jeans," the "Jeans," and the "Jeans-Lorentz" formula. While the two former do not necessarily carry with them the association of Jeans's result with the electron theory, the latter definitely does. Kuhn's inconsistent attention to this fact elides the difference.[115] Proponents of the electromagnetic worldview (including Sommerfeld, Wien, and to a lesser extent Lorentz) may not have regarded the choice between continuity and discontinuity as the central issue. Rather, the question that "came to challenge" them, the question over which they struggled, was whether the electron theory could produce a Planck-like formula. Once it was accepted that this was impossible, discontinuity was adopted quite readily by this group.

Lorentz wrote to Wien early in June 1908, noting that he had been "ceaselessly racking his brains over the last few years" over the question of deriving Planck's formula (or something similar) from the electron theory. Contrasted to this language of constant struggle, Lorentz's description of Planck's alternative solution, the introduction of elementary quanta of energy, seems almost casual: "In and of itself, I have nothing against it; I concede at once that much speaks in its favor and that it is precisely with such novel views that one makes progress. I would, therefore, be prepared to adopt the hypothesis without reservation if I had not encountered a difficulty."[116] Kuhn highlighted this difficulty to explain Lorentz's hesitancy in accepting discontinuity, but the problem Lorentz outlined was not that of discontinuity *per se* but rather that of an asymmetry between the (continuous) absorption and emission of energy by resonators in interaction with the ether, and discontinuous emission and absorption otherwise. This specific question would continue to bother those who had accepted the idea of a quantum discontinuity for some time, and would eventually lead Planck to his so-called second and third theories, each of which posited different mechanisms (one continuous, one discontinuous) for resonator emission and absorption. Lorentz did not have a difficulty with discontinuity "in and of itself." What counted was whether the electromagnetic worldview could include it. "I can only conclude," he wrote in the *Physikalische Zeitschrift*, "that a derivation of the radiation law from electron theory is scarcely possible without profound changes in its foundation. I must therefore regard Planck's theory as the only tenable one."[117]

The first radical move for proponents of the electromagnetic worldview was not the adoption of a new theoretical position—that would come later—but the forced abandonment of their old one. For Wien, it was not immediately obvious after the Rome lecture that such an abandonment was even being posited, and his route toward Planck's theory can be understood as the inverse of Lorentz's. If Lorentz tried to obtain Planck's result by beginning with the electron theory, Wien—after dismissing Jeans's result on experimental grounds early on—began with Planck's energy elements and then sought to understand them in electromagnetic terms. His original reaction to the Rome lecture evinced a certain irritation with what he saw as Lorentz's rather "poor" rederivation of Jeans's result in Rome. "The lecture which Lorentz gave in Rome," he wrote to Sommerfeld:, "has disappointed me greatly. That he presented nothing more than the old Jeans theory without bringing in any sort of new viewpoint I find a little poor. Besides, the question of whether one should regard the Jeans theory as discussable lies in the region of experiment. His opinion is not discussable here because observations show enormous deviations from the Jeans formula in a range in which one can easily control how far the radiation source deviates from a black body. What's the point in presenting these questions to the mathematicians, who can make no judgment on precisely this point? It seems, in addition, a little peculiar to seek the advantage of the Jeans formula, in spite of the fact that it corresponds with nothing, in the fact that it can preserve the whole unlimited multiplicity of electron oscillations. And the spectral lines? Lorentz has not shown himself to be a leader of science this time."[118]

Ditching Jeans's result on experimental grounds, however, was relatively unproblematic compared with doing so for methodological reasons. It was not until he read Lorentz's second paper that Wien realized, with some dismay, what giving up Jeans's result meant in relation to electromagnetic theory. He wrote to Sommerfeld: "Lorentz has recognized his error over radiation theory and that Jeans's hypothesis is untenable. Now, however, the situation is not so simple, since in fact it appears as if Maxwell's theory must be abandoned for the atom. Hence I have a problem to pose you again. Namely, to check how far Lorentz's statistical mechanics and proof is founded on the fact that a system obeying Maxwell's equations (including electron theory) must also obey the supposition of the 'equipartition of energy,' from which Jeans's law is deduced. Namely a restriction of the degrees of freedom, as required by Planck's energy element, must also require an electromagnetic interpretation. Now it seems to me almost as if such [an interpretation] would be impossible, as if precisely this restriction requires additional forces (fixed connections and the like) that don't fit in with a Maxwellian system. If that's really the case, one doesn't need to rack one's brains any more about an interpretation of the energy element and a representation of spectral series on an electromagnetic basis, but rather must seek to find an extension of Maxwell's equations within the atom."[119]

Standing almost as bookends, outlining first the problem and then the proposed solution, are the statements "it appears as if Maxwell's theory must be abandoned for the atom" and "we rather must seek to find an extension of Maxwell's equations within the atom." Between the two is an interpretation of *both* Jeans's and Planck's derivations in electromagnetic terms. That is, Wien translated the question of equipartition and the question of the meaning of Planck's energy elements into the language of electromagnetic theory. The contradictions that arose in so doing led him to both echo and reject a comment written to him less than two weeks earlier by Lorentz: "One doesn't need to rack one's brains any more." The effort to save the electron theory and the electromagnetic worldview in its entirety now seemed fruitless, and

Wien pointed quite calmly to the need for an intra-atomic extension of Maxwell's equations.

Sommerfeld's reply, dated 20 June, was less pessimistic. He claimed that he did not find Lorentz's electrostatistical derivation of Jeans's result conclusive.[120] He promised Wien that he would communicate his objections to Lorentz, and indeed he did so the same day. Rather than accept Lorentz's calculations as a proof that the electromagnetic worldview must fail in the face of Planck's result, Sommerfeld merely used the opportunity to emphasize what was at stake in such a question. "At one time," he wrote, "when I lectured on the theory of radiation, I believed Jeans's paradox could be overcome by saying that electrodynamics is not subject to mechanical laws. Your present remarks seem to me to be an excellent foundation for the resolution of this question."[121]

Fixing a date for Sommerfeld's acceptance of the necessity of discontinuity is not easy.[122] In November 1908 he wrote to Lorentz urging him to ignore his earlier criticisms, but did not explicitly retract his objections to Lorentz's theory in general.[123] It was, however, in the latter part of 1908 that Sommerfeld attended Minkowski's lectures on relativity and was "converted" by them.[124] This is critical, since Abraham's rigid spherical electron theory, which Sommerfeld had originally favored over Lorentz's, was not relativistically invariant. If Sommerfeld applied the relativity theory consistently to the choice between competing electron theories, that is, he would have been induced to accept Lorentz's some time after 1908. By late 1909, Sommerfeld would make this point explicitly, in lectures that mark the first classes taught anywhere in the world on relativity theory. In introductory comments, Sommerfeld noted that the hypothesis of the rigid electron "was dropped because it includes the hypothesis of absolute space" and that "the deformable electron follows from the concept of relative space-time, which experience demands."[125] This, in conjunction with the removal of his specific reservations about Lorentz's derivation, would imply that Sommerfeld accepted Lorentz's conclusion that the electron theory and the electromagnetic worldview were incapable of dealing alone with the theory of radiation.

Three quite different responses to the questions raised by Planck's black-body theorem are sketched above. Yet it should also be clear that Lorentz, Sommerfeld, and Wien held much in common.[126] All three men conceived the problem of radiation as one to be cast at first solely in electromagnetic terms. If, after repeated efforts, that should prove impossible, the answer was not to abandon electrodynamics in favor of some other extant approach, but to find a new way of extending it. That is, electrodynamics provided the only standpoint from which one could begin to construct the future steps required to come to a comprehension of the puzzles introduced by black-body theory. And the question at hand was not "the problem of the quantum"—such a problem did not yet exist in such terms for the majority of physicists. For electromagneticists, Planck's result was a problem for and of the electromagnetic worldview in general and Lorentz's electron theory in particular. Only after they had acknowledged the reality and insurmountability of the problem within present electromagnetic theory—after June 1908—did they focus on discontinuity.

On the other hand, for those not committed to the electromagnetic worldview, the issue of discontinuity was an important means of understanding Planck's result. Einstein and Ehrenfest, who approached the issue from the perspective of Boltzmannian statistical mechanics, were the first, Kuhn argues, to "discover" the quantum discontinuity, some years before the Rome lecture. Jeans, on the other hand, initially denied the force of experimentalists' arguments, not conceding their validity until 1910. His description of the choice on offer at the time does not include discussion of electron theory, but does place the issue of discontinuity—expressed in terms of differential equations—front and center:

> Planck's treatment of the radiation problem, introducing as it does the conception of an indivisible atom of energy, and consequent discontinuity of motion, has led to the consideration of types of physical processes which were until recently unthought of, and are to many still unthinkable. The theory put forward by Planck would probably become acceptable to many if it could be stated physically in terms of continuous motion, or mathematically in terms of differential equations.[127]

For proponents of the electromagnetic worldview, the most important issue introduced by black-body theory was the apparent failure of electron theory to incorporate or duplicate Planck's more experimentally verified result. The acceptance of discontinuity followed with comparatively little struggle after that blow to their shared worldview had been assimilated. For those who were not wedded to the electromagnetic picture, however, discontinuity became the most troubling thing about Planck's energy elements. Thus, perhaps one should, if one is to adapt Kuhn's religious language, speak not only of "converts" to discontinuity, but also of "lapsed" or at least disillusioned electromagneticists.

Conclusion

All those who have written on Arnold Sommerfeld in any detail have noted the number and eclecticism of both the problems he studied and the methods of their solution. This emphasis on specific questions and their specific solutions, the search for a mechanism or a process rather than a generalizing postulate is what distinguishes Sommerfeld's "physics of problems" from Planck's "physics of principles." Thermodynamics, which provided the model for Planck's unifying methodology, was to Sommerfeld "the most secure, but the least satisfying" approach to physics, for it failed to provide the specificities of mechanism. Historians have, perhaps naturally, tended to fragment Sommerfeld's various projects, attributing some to theoretical physics, others to mathematics or technical mechanics. Doing so is, in some ways, an obvious way of understanding a "physics of problems," for the specificity of problem solving can suggest a lack a coherence, an inability to be unified. Heretofore the discussion in this work, has also considered—separately—the three elements that went into making up theoretical physics in Munich: mathematics, technical mechanics, and physics. It remains to be considered how these three elements formed a recognizable single style. What, to phrase the question in its starkest form, was Sommerfeld's theoretical physics other than a single name given to a collection of disparate interests?

Perhaps not surprisingly, the problems themselves provide the answer. The problems themselves would often cross and hence blur the disciplinary boundaries that composed theoretical physics in Munich, producing what Andrew Warwick, in his discussion of mathematics training in nineteenth-century Cambridge, has termed a "technical unity."[128] The problems accorded with Sommerfeld's physical worldview and thus dealt on a majority of occasions—in the early years—with electromagnetic theory. At the same time, they were genuine problems of current technological interest, solved with mathematical prowess turned to physical ends. It was this quality of interdisciplinary fusion within the problems studied in the Sommerfeld School that brought a commonality of approach.

One may clearly discern this emphasis on both interdisciplinarity and technical unity in the selection and solution of problems in Sommerfeld's reports to the Munich Philosophical Faculty on his students' dissertation and habilitation projects. As noted earlier, most of these topics flowed from his own research, and the range of titles provides a good insight into the problems that Sommerfeld deemed significant. In addition, in the short commentaries describing the work, he would pick out those elements he deemed most important, so that even within the context of a given problem, one can discern those aspects representative of Sommerfeld's own interests.

The extent of Sommerfeld's pursuit of the electromagnetic view of nature appears in the number of his students' projects that deal with problems related to electromagnetic theory. Of the ten theses supervised or co-supervised by Sommerfeld in Munich between 1908 and 1911, eight discussed some aspect of electromagnetism, such as wireless telegraphy, electrical conduction in gases, measurements of capacitance, or the calculation of light pressure on spheres of arbitrary material. In many of the theses the question was not merely one of sheer theoretical analysis, but was derived from a practical problem. Hence, the project conducted by Hermann von Hoerschelmann took up the (very topical) question of the "Mode of Operation of the Bent Marconi Sender in Wireless Telegraphy." The problem, which Sommerfeld called "rather mysterious," lay in the discrepancy between the theoretical and the actual operation of a Marconi radio station. A bent sender should provide a signal in a preferred direction, that given by the antenna wire. Some of those who used such senders, however, had failed to detect such a preferred direction. Sommerfeld's discussion reveals his close interaction with those at the forefront of technological use and production, as he cites Count Arco (one of the doyens of German telegraphy) and an unnamed marine officer:

> Even though this [theoretical] effect is called into doubt by several practical men—Count Arco told me that in Marconi's opinion the antenna only conducted horizontally in this way because otherwise he couldn't accommodate the large length of the wire, and a marine officer in the radio commandos [*Funkencommandos*] wrote to me that he had experienced no directional effect in the vicinity of such a Marconi station—nonetheless Marconi's data has, given all previous experiences, the greatest right to attention. Therefore, because Marconi at his distance station [*Fernstation*] now uses the bent sender throughout and has invested significant capital in it, a clarification of its mode of operation is an important theoretical task.[129]

Rather than merely a question of electromagnetic theory, it is the practical issue of the operation of an existing radio station—an issue considered by those who made use of such stations—that provides the impetus for a theoretical investigation. Armin Hermann has noted that Sommerfeld would often pursue "physical questions that he examined up to their technical application." Here the situation is reversed, as *Praxis* provides the problem for *Theorie*. In a similar fashion, commenting on a project that dealt with the spreading out of wireless telegraphic waves on the Earth's surface, Sommerfeld effectively chided previous, more mathematically inclined researchers (Poincaré and Nicholson) for their failure to explain clearly the success of practitioners. Wireless telegraphers had succeeded in overcoming the problem of the curvature of the Earth in their attempts to send long-distance signals, and the project of Hermann March was devoted to explaining this practical success theoretically.[130] At the same time, this problem-focused fusion of practical technology and electromagnetic theory required the development of a sophisticated mathematical apparatus, and Sommerfeld lauded his student's work in developing the means of representing the electromagnetic fields in terms of the integrals of spherical functions. This, he claimed, was "important for several problems of mathematical physics and also appears noteworthy to me from a pure mathematical standpoint."[131] The one problem, in other words, would fuse all three elements of the Sommerfeld Style.

Specific problems could, on occasion, not merely require all three aspects of Sommerfeld's theoretical physics, they could recur in areas that corresponded to different disciplinary contexts. In Hopf's project, for example, Sommerfeld noted the similarity of one part of the solution with another well-known phenomenon. For ship waves in water of finite depth, the angle subtended by the wake is a constant, a result similar to that arrived at by Ernst Mach through his studies of the shock waves that were produced by an object moving at supersonic speeds (e.g., a bullet) that underwent a rapid deceleration (by hitting a wall, for example). Both of these, of course, were problems in mechanics (with technical applications), but Sommerfeld, following Stokes, Lenard, and Wiechert, had already used such a model in the case of X-ray production through the braking radiation of an electron. "According to this theory," Paul Ewald wrote, "X-rays are the electrical analogue to the sound cracks which travel forth in air from a target hit by shot."[132] Yet later the model would be used as Sommerfeld's point of entry into studies on quantum theory.

The problems that characterized Sommerfeld's theoretical physics were thus among the agents that provided a form of unity for his eclecticism, both through the fact that multiple elements were mobilized toward their solution and through the recurrence of particular problems and modes of modeling and solution in different disciplinary contexts. Technology, mathematics, and physics were planted together in Sommerfeld's nursery for theoretical physics. In spite of the diversity of its subject matter, the eclectic physics that emerged there shared common roots, and grew to bind its tendrils together.