Michael Eckert

Statement

and

Readings

Five decades of quantum physics: How Sommerfeld faced the quantum (r)evolution in the first half of the 20th century—a biographical perspective

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Abstract

Sommerfeld's attitude towards foundational questions—in particular with regard to the quantum—will be described with respect to the changing context and content of his research at significant stages from the 1910s to the 1940s. In the early period (before 1913) Sommerfeld focused on his so-called "h-hypothesis" as a general scheme to account for processes like the photoelectric effect, the emission of x-ray bremsstrahlung and other non-periodic elementary processes. In the subsequent period of the "Bohr–Sommerfeld model" he perceived foundational questions from the vantage point of atomic spectra. With the advent of quantum mechanics and new discoveries like the Compton effect and electron diffraction in the 1920s Sommerfeld made wave mechanics the basis from which he perceived quantum riddles. This became a major effort during the 1930s when he transformed the Wavemechanical Supplementary Volume of Atomic Structure and Spectral Lines from 1929 into a new edition in 1939. Sommerfeld kept a vivid interest in the progress of quantum research until his death in 1951.

I hope that this biographical approach illustrates how foundational questions in physics depend on time and on the broader context. Although I reach only a little beyond the formative period of quantum mechanics I hope the lessons drawn from this example are more generally of interest.

Michael Eckert
translated by Tom Artin

Arnold Sommerfeld

Science, Life and Turbulent Times 1868–1951

Springer

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Acknowledgment

In the case of such a source-dependent work, first thanks are due Sommerfeld's heirs for access to the private papers and for permission to make full use of their content. Sommerfeld's granddaughter, Monika Baier, deserves particular thanks for ordering her grandfather's private papers and for providing valuable information on the family background. The archivists of the various public institutions in which Sommerfeld correspondence resides, too, are gratefully acknowledged for their readiness to help. The bibliography reveals how much support the project has thereby received. Cited passages from letters and texts from other source materials have been adapted to modern notation for the sake of legibility. Numerous letters cited here only in excerpts are printed in full in editions of correspondence. In these cases, references are given indicating the respective volume of the published correspondence (e.g., ASWB I and ASWB II as abbreviations for the two volumes of *Arnold Sommerfeld – Wissenschaftlicher Briefwechsel, Band I und II*: since in those volumes the letters are printed in chronological order, there is no need to specify page or letter numbers). Thus the reader can study the respective letters in full and in the context of the other correspondence reproduced there. Since Sommerfeld's private letters are not publicly accessible, they are identified only by date, and no source is given.

It is conventional for the published work of a scientist to take precedence over unpublished papers at the beginning of a biographical endeavor. This approach reflects his life's work as a scientist as it has been perceived by his contemporaries. Usually this kind of biographical work begins with the mile-stone birthdays marking decades of a scientist's life.[1] Thus, festschrifts were assembled to celebrate Sommerfeld's 60th, 70th, and 80th birthdays. In 1968, on the occasion of his 100th birthday, the physicists of the University of Munich, organized an "Arnold Sommerfeld Centennial Memorial Meeting," and an "International Symposium on the Physics of the One- and Two-Electron Atoms." On this occasion, his successor at the Institute for Theoretical Physics (Fritz Bopp) was commissioned by the Bavarian Academy of Sciences to arrange for Sommerfeld's most important scientific papers to be published in the form of a four-volume edition.[2] The biographer is gratefully obliged to all those involved in this preliminary work. The editors of the scientific correspondence (the author and Karl Märker) had the support of the Munich Physics Department (especially Harald Fritzsch and Herbert Wagner), and the Bavarian Academy of Sciences (in the person of Past President Arnulf Schlüter). Special thanks are also due the Dean of the Faculty of Physics (Axel Schenzle) of Munich University for the financial support that has enabled the translation of the German original of this biography into English.

1 See chapter 14.

2 Sauter, *Sommerfeld*, 1969.

The biographer owes thanks and acknowledgment also to his colleagues in the history of science who have concerned themselves from a historical perspective with Sommerfeld and his fields of research. Atop the list are John L. Heilbron, and Paul Forman, who as participants in the *SHQP* project actually first set Sommerfeld research in motion. Subsequently, above all Armin Hermann, Ulrich Walter Benz, and Karl von Meyenn have helped cause this spark to jump over to Germany. In recent years, a new project on the history of quantum physics[3] at the Max Planck Institute for History of Science in Berlin brought further impetus to analyze Sommerfeld's work within the network of modern atomic and quantum theory. For this biography, it was a piece of good fortune to participate in this project. It would lead too far afield to list all the names of colleagues and friends who in this and in previous decades have researched the history of quantum physics—and thus also important aspects of the field of Sommerfeld biography.

Even if, in retrospect, quantum physics be deemed Sommerfeld's most significant area of research, the less spectacular mathematical, physical, and technical work to which he devoted himself in the course of his long scientific career deserves inclusion in his biography. The gratitude of the historian of science and biographer is therefore directed to all those who have devoted their scholarly attention to these aspects of Sommerfeld's work, and thus in diverse ways have advanced the work of this biography. Their names and contributions are to be found in the bibliography. Special thanks to our colleagues at the Research Institute of the Deutsches Museum, which constitutes a particularly sympathetic setting for the work of history of science. Last but not least, thanks to the German Research Foundation, without whose financial support this project would not have been possible.

3 http://quantum-history.mpiwg-berlin.mpg.de/main/(28 January 2013)

Contents

9 Wave Mechanics

In retrospect it seems as though following World War I quantum physics intensified critically and in a revolutionary act in the mid 1920s freed itself from the many contradictions of the old quantum theory. The breakthrough is identified with "matrix mechanics," sketched out in the summer of 1925 in a solitary stroke of genius by Werner Heisenberg, and then established on a solid foundation by the "triumvirate" of Heisenberg, Pascual Jordan (1902–1980), and Max Born. Early in 1926, the old quantum theory was independently revolutionized in an entirely different fashion by Erwin Schrödinger with "wave mechanics," and shortly, the equivalence of matrix and wave mechanics was recognized. Since then, physicists have referred to the core of this new physics succinctly as "quantum mechanics."

In the meantime, historians of physics have elaborated this rough sketch of the history of quantum mechanics into a quite complex picture,[1] without much having changed in the widespread conception of a revolutionary upheaval. With an eye to Sommerfeld, who together with his students made a lasting imprint on the quantum mechanical formation of atomic theory, reservations about this conception are pertinent. Even if there is no doubt about the radical upheaval in physical thinking itself, the transformation that accompanied quantum mechanics seemed to Sommerfeld and to a number of his contemporaries to be not so much a revolution, as a necessary process of adaptation to continuously changing realities. "The new development represents not an overthrow, but a felicitous advance in what already exists, with many fundamental clarifications and with increased precision," Sommerfeld wrote in 1928 in the Preface to his *Wave Mechanical Supplement* to *Atomic Structure and Spectral Lines*.[2] He replied to a colleague in physics who had sent him a book on the foundations of quantum mechanics: "You take the revolutionary position; I, the evolutionary."[3]

Even in a period of evolutionary transition, there can be critical developments and changing, apparently mutually exclusive conceptions and paradigm shifts, such as are characteristic of scientific revolutions.[4] By contrast to truly revolutionary crises, though, in evolutionary developments, old and new can exist side by side for a long time, even if the concepts associated with them cannot be brought into agreement with one another. There is considerable evidence that the development

1 Jammer, *Development*, 1966; Mehra/Rechenberg, *Development*, 1982; Darrigol, *c-Numbers*, 1992; Rechenberg, *Werner Heisenberg*, 2010; Meyenn, *Entdeckung*, 2011.

2 Sommerfeld, *Ergänzungsband*, 1929.

3 To Arthur March, 12. January 12, 1931. DMA, NL 89, 025. Also in ASWB II.

4 Kuhn, *Structure*, 1962. On the criticism of the concept of revolutionary crises, see Seth, *Crisis*, 2007.

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of quantum mechanics represented precisely such *evolutionary* critical processes. Physicists learned to live with contradictions, and they dealt with the situation in very different ways. Everyone who was actively involved in the developments that led to quantum mechanics lived through a period of processes of adaptation that in hardly any individual case were experienced as a collective revolutionary act. What is commonly labeled the "quantum revolution" was rather a process stretching over many years and experienced in quite different and individual ways at the quantum schools of Munich, Copenhagen, and Göttingen.

9.1 The Crisis of the Models

When Sommerfeld returned from the USA in April 1923, skepticism over model-related interpretations of quantum phenomena, which a year later he would express so clearly in the fourth edition of *Atomic Structure and Spectral Lines* in the chapter on complex structure of spectra, was already discernible. During his absence, Heisenberg and Born had calculated all possible orbits that one of the two helium electrons could describe when the atom was in an excited state. The paradigm for this lay in celestial mechanics with its methods of perturbation theory developed for planetary motion around a central star. But the hope of thereby calculating the energy states in the helium atom readable in the spectra in combination with quantum rules was not fulfilled. "The result of our calculation is negative," Born and Heisenberg wrote summarizing their model calculations.[5] Just a short time before, Born had been confident about the application of methods from celestial mechanics to quantum theoretical calculations of atomic models.[6] Heisenberg had even urged making this the sole topic of the seminar for the summer semester 1923 in Munich.[7]

But by the summer of 1923, all traces of this euphoria had vanished. Born and Heisenberg were not the only ones whose model calculations had proven to be failures. Pauli had struggled with similar calculations even earlier than Heisenberg. Considered an expert in the area of celestial-mechanical methods, he was asked by the publishers of an edition of the works of Karl Schwarzschild to contribute a paper on the applications of celestial-mechanical perturbation theory to atoms. "Since so much remains unclear about the theory for multi-electron atoms, however, this hardly fits together," he wrote Sommerfeld in June, 1923. "So there is really no justification for a physicist to undertake this work; an astronomer would be more appropriate." In light of this "breakdown of classical mechanics," it made no sense whatsoever to him to calculate the spectra of multi-electron atoms using methods of celestial mechanics. "This break-down is now hardly to be doubted, and it seems to me that one of the most important findings of recent years is that

5 Born/Heisenberg, *Elektronenbahnen*, 1923, p. 229.
6 From Born, January 5, 1923. DMA, HS 1977-28/A,34. Also in ASWB II.
7 From Heisenberg, January 15, 1923. DMA, HS 1977-28/A,136. Also in ASWB II.

the difficulties of the multi-body problem in atoms are of a physical, and not a mathematical nature." If the Born-Heisenberg helium calculations have failed, the cause is "certainly not that the approximation is insufficient," he commented on this most recent attempt at a model-related explanation of atomic spectra.[8]

Nonetheless, this did not represent a total abandonment of models. When Pauli sent his latest paper on the anomalous Zeeman effect to Munich a few weeks later, he conceded to Sommerfeld that although in his text he had "carefully avoided" any reference to models, he would not have gotten certain of his results "had I not been guided by model representations."[9] At this time, Sommerfeld was himself still not prepared to abandon model-related explanations. For example, to his former assistant Rubinowicz, he heartily recommended the helium model as a particular challenge to help lift him out of a depression.[10]

Model representations could not be entirely dispensed with, especially when spectroscopic findings were brought into relation with other physical phenomena. In 1920, Pauli had already pointed out that the spatial quantization introduced with respect to the Zeeman effect—that is, the quantization assumed in the Sommerfeld atomic model of 1916 of the inclination of the orbital plane in which an electron is in rotation around the atomic nucleus—could also elucidate the puzzle of the elementary magnets. According to experimental investigations carried out by Pierre Weiss (1865–1940), the smallest unit of magnetic moment of atoms or molecules was much smaller than the minimum magnetic moment an electron in its orbit around the atomic nucleus was supposed to have according to the Bohr atomic model. If, however, the orbital plane could adapt itself differently with respect to an external field, the "Bohr magneton" would amount to a multiple of the "Weiss magneton." When the spatial quantization of Otto Stern (1888–1969) and Walter Gerlach (1889–1979) was confirmed experimentally,[11] the search for the smallest possible magnetic moment moved to a new stage. In August 1923, Sommerfeld sent a brief notice to the *Physikalische Zeitschrift* in which he called attention to the fact that the latest spectroscopic findings on multiplets confirmed the magneton that had been expected according to the conception of the spatial quantization.[12] To be sure, the equation derived for the normal Zeeman effect had to be modified, because the multiplets of multi-electron atoms displayed an anomalous Zeeman effect in the presence of a magnetic field. In September 1923, Sommerfeld sent his theory of the magneton to the *Zeitschrift für Physik*.[13] In it, he was once more able to describe an "elegant regularity," which extended "beyond the

8 From Pauli, June 6, 1923. Geneva, CERN-Archive. Also in ASWB II.

9 From Pauli, July 19, 1923. DMA, HS 1977-28/A,254.

10 To Else Rubinowicz, August 18, 1923.

11 Friedrich/Herschbach, *Stern and Gerlach*, 2005; Schmidt-Böcking/Reich, *Otto Stern*, 2011, Chap. VII

12 Sommerfeld, *Magnetonenzahlen*, 1923.

13 Sommerfeld, *Theorie des Magnetons*, 1923.

area of the periodic table in question." But it presupposed the model representation that the electrons in the atom moved in different, variable orbital planes and thereby could adapt their angular momentum in an applied magnetic field variably. This did not please Pauli at all. "As you will see," he wrote Sommerfeld about his latest efforts on the theory of the anomalous Zeeman effect in July 1923, "I was so intimidated by the failure of all my model-related speculations that I have studiously avoided even the word impulse momentum [=angular momentum] in the paper."[14] For Heisenberg, too, model representations were necessary on the one hand, but on the other not really binding on physical understanding. He gave expression to this ambiguous conception, for example, when in December 1923, he reported to Sommerfeld about his efforts to deal with the Zeeman effect in the framework of his core model: "When one reflects in retrospect on what one has actually done, one sees clearly that none of the model representations really make sense. The orbits are real with respect neither to frequency nor to energy."[15]

Sommerfeld commented pointedly on this paradoxical situation half a year later when he wrote to Landé, "Recently, we have repeatedly had the experience that the arithmetic regularities go much further than would be expected from the model representations." Shortly before, Millikan had reported to him that he and his colleague Ira S. Bowen (1898–1973) had measured spectra in the range of ultraviolet light in "stripped atoms" (atoms from which all valence electrons had been blasted away in explosive spark discharges), which like X-ray spectra displayed a characteristic doublet nature, and also could be calculated with the same equation. Sommerfeld expressed his pleasure over the fact that "The relativity equation, far from being discarded or refuted, extends its validity to the optical domain."[16] This equation arose from fine-structure theory and explained the doublets as a relativistic effect, as opposed to the explanation of the optical doublets in alkali metals such as sodium, which were explained by the core model on the various orientations of orbits of valence electrons with respect to the atomic core. Thus, two different models of the doublet phenomenon stood in opposition to each other.[17] "The contradictions you and Millikan present are very serious," Paschen wrote Sommerfeld.[18] Sommerfeld, however, made a virtue of necessity. "This semester, I've lectured comprehensively on your and Bowen's work on the ultra-violet," he wrote Millikan towards the end of the winter semester 1924/1925.[19] He had "for the time being not [been able to] solve" the "serious contradiction," but he hoped for an elucidation soon from his assistant Gregor Wentzel, who was addressing this subject. Wentzel was unable to resolve the doublet puzzle definitively, however. "For me, the open

14 From Pauli, July 19, 1923. DMA, HS 1977-28/A,254.
15 From Heisenberg, December 8, 1923.DMA, NL 89, 009. Also in ASWB II.
16 To Landé, April 20, 1924. SBPK, Landé.
17 Forman, *Doublet Riddle*, 1968.
18 From Paschen, January 27, 1925. DMA, NL 89, 012.
19 To Millikan, February 9 1925. DMA, NL 89, 003. Also in ASWB II.

question of the 'relativistic' doublet is terribly unsatisfying," wrote Schrödinger too, "as you yourself keep stressing."[20] That Sommerfeld, without a certain model foundation, was nonetheless able to interpret the multiplets of the multi-electron atoms with the help of the inner quantum number seemed incomprehensible to Schrödinger. "How it was possible for you to infer these fundamentally so profoundly different regularities from really not a great wealth of evidence without an actual model, and based only on the sense of an analogy with classical theory, is still a mystery to me. I have slowly struggled to achieve clarity on the really quite complicated construction of these rules involving only integers, while you have incorporated these same rules into the observational data, so they now fit snug as a guard's uniform!"[21]

Pauli experienced the failure of model-related explanations as both crisis and incentive. He even praised Sommerfeld for the fact that the presentation of the complex structure of spectra in the fourth edition of *Atomic Structure and Spectral Lines* was "entirely free of model-related preconceptions":

> The model representations now find themselves in a difficult crisis of principle, which will, I believe, end in an even more radical intensification of the contradiction between classical and quantum theory. In particular, as it follows from the findings of Millikan and Landé concerning the representability of the optical alkali doublet by relativistic equations, the idea of specific, unique orbits of the electrons in the atom can scarcely be maintained. One now has the strong impression that with respect to all these models, we are speaking a language inadequate to describe the simplicity and beauty of the quantum world.[22]

Liberation from "model-related preconceptions" and the predilection for maximally simple, empirically based regularities led Pauli to formulate the eponymous "Pauli Exclusion Principle."[23] In a multi-electron atom, the quantum numbers used to characterize the energy level and makeup of the electron shells are ascribed to each individual electron—together with the rule that the quantum numbers of every electron must be different. In other words, every quantum state in the atom can be occupied by only one electron.

Viewed on its own, the passage quoted from Pauli's letter to Sommerfeld on the eve of the ground-breaking work on quantum mechanics would seem the revolutionary escalation of the model crisis. But in light of the reinterpretation of older concepts repeatedly necessitated by complex spectra and other phenomena, we see that this was merely one more process of adjustment in a far from concluded evolutionary development. Consciousness of "model-related preconceptions" did not carry with it renunciation of all model-based thinking. This was most clearly

20 From Schrödinger, March 7, 1925. DMA, HS 1977-28/A,314. Also in ASWB II.

21 From Schrödinger, July 21, 1925. DMA, HS 1977-28/A,314. Also in ASWB II.

22 From Pauli, December 6, 1924. DMA, HS 1977-28/A,254. Also in ASWB II.

23 Meyenn, *PaulisWeg I* and *II*, 1980 and 1981; Massimi, *Pauli's Exclusion Principle*, 2005.

exemplified in the work of two colleagues from the Ehrenfest Institute at Leiden, George Uhlenbeck (1900–1988) and Samuel Goudsmit (1902–1978), in which on the basis of Pauli's insights, they reinterpreted the vector model with which Landé, Heisenberg, and Sommerfeld had explained the magnetic splitting of spectral lines by the spatial orientations of different vectors of angular momentum. In the old vector model, a vector was attributed to the atomic core, which however entailed difficulties that continued to raise questions about this conception. Uhlenbeck and Goudsmit drew from this the conclusion that the atomic core had to be excluded from the otherwise very plausible vector framework model. Like Pauli in his formulation of the Exclusion Principle, they took up the burden of each individual electron which before had been borne by the core. To the electron, in addition to the three spatial degrees of freedom, they assigned a fourth, meant to represent "an individual rotation." The new degree of freedom was not understandable in classical terms (the rotation of a sphere on its own axis does not imply a new degree of freedom because it can be described classically by its three spatial coordinates), but now the core no longer presented problems. Thus, the electron took over "the still not understood property," argued the Leiden theoreticians, that before had been thought to belong to the atomic core.[24]

Hereby "spin" stepped onto the stage of atomic theory as an additional quantum phenomenon. Pauli stubbornly resisted the model-related interpretation of the new degree of freedom as intrinsic rotation, because for an object without spatial extension, this concept is actually meaningless. Nevertheless, the model took hold in the consciousness of physicists.

9.2 "We Believe in Heisenberg, but We Calculate with Schrödinger"

In the course of this evolution, Sommerfeld came to a position that might seem almost indifferent to the fundamental questions raised by quantum theory. "The difficulties in atomic physics that crop up ever more clearly these days seem to me to lie less in an excessive application of quantum theory, than in a somewhat excessive belief in the reality of the model representations," he commented on the state of research in the fall of 1924 to the Natural Scientists Congress convened that year at Innsbruck.[25]

The model crisis was not the only reason to undertake a critical review of classical conceptions such as the idea of the electron orbit in the atom. Another crisis arose from the wave-particle dualism. This crisis, too, had loomed for many years and compelled the physicists ever and again to adjust their theories to new empirical findings. "In light of this, the wave theory for X-rays would finally have to be

24 Uhlenbeck/Goudsmit, *Ersetzung*, 1925.
25 Sommerfeld, *Grundlagen*, 1924, p. 1049.

dropped," Sommerfeld had written Bohr in January 1923, after Compton informed him of the as yet unpublished results of his scattering experiments destined to enter history as the Compton effect.[26] "Whereas I formerly sought to uphold the wave theory for the pure propagation processes as long as possible, the Compton Effect forces me more and more to accept the extreme theory of light quanta," he wrote in the preface to the fourth edition of *Atomic Structure and Spectral Lines*, laying out his own process of reorientation on this question.[27] Up to now, it was still uncertain whether a theory developed in Copenhagen might succeed in interpreting the Compton effect also in terms of a wave concept.[28] When this was refuted experimentally in the spring of 1925, however, it could no longer be doubted that in the Compton effect, X-rays behave like particles. Since then, physicists have described the nature of light with a "this-as-well-as-that," even though wave and particle analogies are mutually incompatible.

For Heisenberg, too, quantum processes in 1925 were still to be understood by "model-related pictures of symbolic significance." He presented this view in a paper titled "On the Quantum Theory of the Multiplet Structure and the Anomalous Zeeman Effect," submitted in April 1925 to the *Zeitschrift für Physik*.[29] Two months later he authored the paper celebrated as the breakthrough to quantum mechanics, "On Quantum Theoretical Reinterpretation of Kinematic and Mechanical Relations."[30] In this paper the problem of the radiation of an electron in motion, in the simplest imaginable theoretical case in which the electron oscillates in only one direction, was formulated so that only experimentally observable quantities were taken into account and the familiar quantum laws were in force.[31]

Except at Max Born's Institute, where within a few months the new theory was further evolved to matrix mechanics and, at Cambridge, where a scientific loner by the name of Paul Dirac (1902–1984) was building out quantum mechanics in quite a different direction, Heisenberg's "reinterpretation" was at first met with reserve and skepticism. "Heisenberg has laid a big quantum egg," Einstein wrote in September 1925 to Ehrenfest at Leiden. "In Göttingen, they believe it; not I."[32] Even among Sommerfeld and his students, there was at first little enthusiasm. "Heisenberg's new quantum mechanics" first appeared only half a year after its publication as a colloquium topic at Munich.[33] Heisenberg had not exactly covered himself with glory at his doctoral exam following Sommerfeld's return from the USA in the summer of 1923 and had so annoyed his second reader (Willy Wien)

26 An Bohr, 21. Januar 1923. Kopenhagen, NBA, Bohr. Also in ASWB II.
27 Sommerfeld, *Atombau*, 1924.
28 Hendry, *Bohr-Kramers-Slater*, 1981.
29 Heisenberg, *Quantentheorie der Multiplettstruktur*, 1925.
30 Heisenberg, *Umdeutung*, 1925.
31 Rechenberg, *Werner Heisenberg*, 2010, Chap. 5.
32 Quoted in Fölsing, *Albert Einstein*, 1993, p. 644.
33 Lecture delivered February 19, 1926. Wednesday Physics Colloquium. DMA, 1997-5115.

that Sommerfeld was at pains to rescue his prize student from the disgrace of failure. Sommerfeld and Wien agreed that Heisenberg received an overall grade for his doctorate that just prevented the failure, being the average of the best grade from Sommerfeld and the lowest from Wien.[34]

Afterwards, Heisenberg had departed Munich as it were in flight, to continue his career with Born at Göttingen and Bohr at Copenhagen. Clearly, he—and doubtless Sommerfeld too—felt it as a token of ingratitude that he had sought refuge at competing quantum schools. In any case, when Sommerfeld sent him the fourth edition of *Atomic Structure and Spectral Lines*, Heisenberg expressed a measure of relief that Sommerfeld was apparently "not so terribly angry" with him.[35]

Presumably, the course Heisenberg had set out on with his quantum mechanics seemed to Sommerfeld a diversion from the recently so successful path of inductively deriving theoretical laws from the wealth of spectroscopic measurements. Almost all the work of Sommerfeld's students on quantum theoretical problems around 1925 dealt with such topics. Miguel Catalan, who had come to Munich in 1924 as guest researcher on a Rockefeller Fellowship, published papers together with Karl Bechert (1901–1981) on the structure of the cobalt and palladium spectra in the *Zeitschrift für Physik*. In May 1925, Bechert had completed his doctorate under Sommerfeld on the nickel spectrum.[36] Heinrich Ott (1894–1962), who had become assistant at the Sommerfeld institute after Wentzel, had addressed the "Problems of X-ray Spectroscopy."[37] Helmut Hönl (1903–1981), another doctoral candidate, focused on the problem of theoretically describing the intensity of spectral lines.[38] In the context of these papers, Heisenberg's "reinterpretation" seemed as though from another world. Limiting himself to one-dimensional electron motion made a comparison with experimental data impossible. On the other hand, Heisenberg's previous paper on multiplets more nearly fit the Munich tradition. And Sommerfeld had not let half a year go by before reacting to it. He considered it so important that he heartily recommended its closer study to the American spectroscopist and astrophysicist Henry Norris Russell (1877–1957).[39]

The "new quantum mechanics" first won adherents among Munich physicists when Pauli demonstrated how one could thereby treat the hydrogen atom.[40] "I too believe that one has to convert without reservation to Heisenberg's new mechanics," Sommerfeld now conceded after Wentzel, who was working with Pauli in

34 Rechenberg, *Werner Heisenberg*, 2010, p. 138.

35 From Heisenberg, November 18, 1924. DMA, HS 1977-28/A,136. Also in ASWB II.

36 Vote on the dissertation of Bechert to the Philosophical Faculty, 2. Section, May 18, 1925. Munich, UAM, OCI51p.

37 Vote on the dissertation of Ott to the Philosophical Faculty, 2. Section, July 12, 1924. Munich, UAM, OCI50p.

38 Vote on the dissertation of Hönl to the Philosophical Faculty, 2. Section, February 24, 1926. Munich, UAM, OCI 52p.

39 To Russell, July 3, 1925.Princeton, University Archive, C0045, box 63, folder 35.

40 Pauli, *Wasserstoffspektrum*, 1926.

Hamburg, had sent him Pauli's manuscript. To be sure, Sommerfeld still found missing the treatment of more difficult cases. Could Wentzel also derive "such from Pauli?"[41] He was already indicating what he hoped from the further development of quantum mechanics. It should explain what—from the fine structure of the spectra of hydrogen-like atoms to the complex spectra of multi-electron atoms—had heretofore been either derived from unrealistic models or merely sketched inductively.

A manuscript now burst onto the scene that Erwin Schrödinger had sent Willy Wien as editor of the *Annalen der Physik* with the request that he give it to Sommerfeld to referee. "An extraordinary mind, very well educated and critical," Sommerfeld had assessed Schrödinger in 1921, in recommending him for an appointment to the chair in theoretical physics at the University of Zürich (held previously by Einstein, Debye, and Laue).[42] Now, 5 years later, Schrödinger proved he was more than equal to the lofty standards that attached to the Zürich chair he occupied. He was well aware of the importance of the paper he had submitted to the *Annalen der Physik* under the title "Quantization as Eigenvalue Problem," for he wanted to know from Sommerfeld whether he "shared the very ambitious expectations" he himself had of it.[43]

Already in his initial reaction, Sommerfeld showed that Schrödinger's procedure appealed to him much more than Heisenberg's. "This is really terribly interesting," he wrote by return mail to Zürich. "I was just on the point of formulating a concept for lectures in London (this March) that was very much in the earlier key. Then your manuscript arrived like a thunder bolt. My impression is that your method is a replacement for the new quantum mechanics of Heisenberg, Born, Dirac." He conceded that it was still not clear to him how the one could be brought into harmony with the other, but he was "convinced that something entirely new will come of it that can set aside the contradictions that currently bedevil us."[44] To Pauli he wrote that same day that Schrödinger had obtained the same results from the hydrogen spectrum that Pauli had just calculated rather laboriously according to matrix mechanics, "but in a quite different, totally crazy way, no matrix algebra, rather as boundary value problems."[45]

Even before Schrödinger published his paper, the stage had been set for the competition between matrix and wave mechanics. "His way may not be so crazy," Pauli replied about Schrödinger's method, which he knew initially only through

41 To Wentzel, January 13, 1926. DMA, NL 89, 004.

42 To Edgar Meyer, 28. January 28, 1921. Zürich, University Archive, Papers of the Office of the Dean ALF. Also in ASWB II; Moore, *Schrödinger*, 1989, pp. 139ff.; Meyenn, *Entdeckung*, 2011, pp. 51ff.

43 From Schrödinger, January 29, 1926. DMA, NL 89, 013. Also in ASWB II; Meyenn, *Entdeckung*, 2011, pp. 170–172.

44 To Schrödinger, February 3, 1926. Carbon copy in DMA, NL 89, 004. Also in ASWB II; Meyenn, *Entdeckung*, 2011, pp. 173–175.

45 To Pauli, February 3, 1926. DMA, NL 89, 003. Also in ASWB II.

Sommerfeld's sketchy references.[46] Schrödinger published his paper in four "communications" in different issues of the *Annalen der Physik* of the year 1926. Subsequently, he gathered them together as a book with the title *Treatises on Wave Mechanics*, which appeared in 1927.[47] Schrödinger thought matrix mechanics "insupportable" and hoped it would soon "disappear. . . For I shudder at the very thought," he wrote to Wien, "of sometime down the road having to lecture to a young student on matrix calculus as the essential nature of the atom."[48] Pauli saw to it that among the matrix mechanicians at Göttingen, the rival theory from Zürich was thoroughly studied. "I believe that this paper numbers among the most important written in recent years," he wrote to Pascual Jordan on the appearance of Schrödinger's first "communication." "Please read it carefully and with reverence."[49] Although Schrödinger's wish to eradicate matrix mechanics entirely was not achieved, the physical equivalence of the two methods was soon demonstrated. Many of the problems in atomic physics studied heretofore could be solved by both methods. Preference thus fell to wave mechanics because its mathematical operations were simpler than those of matrix mechanics. Schrödinger's method was "far simpler and more convenient" than Heisenberg's, Sommerfeld wrote, praising wave mechanics on the occasion of his trip to England in March 1926. It employs "the language of the theory of vibrations."[50]

This language was familiar to every physicist. Opinions might differ radically as to the meaning of what Schrödinger's wave mechanics supposed was vibrating and propagating wave-fashion, but the mathematical formalism presented no fundamental difficulties. A vibrating string, a tuning fork, a vibrating membrane, the vibrating air in an organ pipe—every such system has, corresponding to its material properties, eigenmodes of vibration determined by magnitude and arrangement that can be found mathematically as the solution of an eigenvalue problem. When the underlying boundary conditions of the respective problem were given, the eigenfunctions (vibration forms) and eigenvalues (frequencies of the basic vibrations and the harmonics) could be calculated by means of a standardized process. The electron rotating around an atomic nucleus could, following Schrödinger, be represented according to the same formalism as a standing wave, whereby the eigenvalues corresponded to the energy terms that Bohr and Sommerfeld had calculated in a quite different manner 10 years earlier, and the quantum numbers revealed themselves as the indices of the eigenfunctions—in this case, spherical harmonics.

46 From Pauli, February 9, 1926. DMA, HS 1977-28/A,254. Also in ASWB II.

47 Meyenn, *Entdeckung*, 2011, pp. 176–208.

48 Schrödinger to W. Wien, February 22, 1926, quoted in Meyenn, *Entdeckung*, 2011, pp. 184–187.

49 Pauli to Jordan, April 12, 1926. quoted in WPWB I.

50 Sommerfeld, *Lectures*, 1926, p. 3. ("Schrödinger arrives at the same results as those obtained by the mechanics inaugurated by Heisenberg, but by a road that is presumably far simpler and more convenient [...] his treatment is expressed in the language of the theory of vibrations.")

Sommerfeld's lectures in England found a great reception. "I really believe everyone was very satisfied," he wrote home after his first week.[51] His host was the professor of physics at King's College, London, Owen W. Richardson (1879–1959), who 2 years later would be awarded the Nobel Prize for his work on thermionic emission of electrons in metals. But this visit was not exclusively about physics. Sommerfeld enjoyed the journey. "I have even tried playing golf (in Oxford), and once table-tennis (in London, at the Indian students' club, with Dasanacharya), often piano, e.g. in Edinburgh, accompanied song and cello," he reported to his wife 1 week later, when he rejoined his London hosts from lecture tours to other cities. "I have worked my way completely into the heart of the stout Mrs. Richardson."[52] Both Charles Galton Darwin (1887–1962), grandson of the famous biologist, and William Lawrence Bragg, who had invited him to Edinburgh and Manchester, respectively, appreciated his sociability. "Bragg is especially cordial with me, a good friend of Ewald," Sommerfeld wrote home from Manchester.[53]

"It is terribly kind of you to have promoted me in England already," Schrödinger wrote gratefully a few weeks later.[54] In his lectures in England, Sommerfeld had been content with outlines, but once back in Germany he immediately elucidated wave mechanics to the students and research colleagues of his school. "Here, we are closely studying Schrödinger's new Quantum theory, and estimate it very highly," he wrote to Richardson on the main topic of his seminar in the summer semester of 1926.[55] Towards the end of the semester, he invited Schrödinger to Munich so that he and his students could be introduced firsthand to the new theory.[56] In the process, a heated exchange arose with Heisenberg, who had traveled to Munich for this occasion, and was so critical of wave mechanics that even Sommerfeld began to waver again: "We've had Schrödinger here, together with Heisenberg," he wrote afterwards to Pauli, to whom he delegated the role of referee in this debate. "My general impression is that though 'wave mechanics' is an admirable micromechanics, the fundamental quantum puzzles are not in the least solved thereby."[57] These doubts were stirred up primarily by the lingering question what the concept of wave motion underlying Schrödinger's theory actually was. He corresponded with Einstein about this, too. "Of all the efforts to extract a deeper formulation of the quantum laws from the latest experiments, I like Schrödinger's best," Einstein

51 To Johanna, March 11, 1926.
52 To Johanna, March 19, 1926.
53 To Johanna, March 23, 1926.
54 From Schrödinger, April 28, 1926. DMA, HS 1977-28/A,314. Also in ASWB II.
55 To Richardson, June 12, 1926. Austin, Ransom, Richardson.
56 To Schrödinger, July 10, 1926. DMA, NL 89, 025. On July 23, Schrödinger spoke, on "Foundations of Wave Mechanics" and the following day on "New Results in Wave Mechanics." Physikalisches Mittwoch-Kolloquium. DMA, 1997-5115.
57 To Pauli, July 26, 1926. Geneva, CERN. Also in ASWB II.

wrote. "The Heisenberg-Dirac theories are admirable, certainly, but to me they don't exude the odor of reality."[58]

For advanced students like Hans Bethe, who had come to Munich from Frankfurt in the spring of 1926 to continue his studies under Sommerfeld's wing as a fifth semester student, this first encounter with wave mechanics remained an indelible memory. "We believe in Heisenberg, but we calculate with Schrödinger," as Sommerfeld put it, introducing his students to quantum mechanics.[59] Every participant in the seminar had to report on one subsection of the Schrödinger "communications" that had appeared by the summer of 1926. Thereafter, the seminar participants were prepared to write a doctoral dissertation on virtually any quantum mechanical topic.[60]

The first student to take up a doctoral dissertation at Munich using the Schrödinger method was Albrecht Unsöld (1905–1995). As Unsöld recalled years later, Sommerfeld initially proposed the wave mechanical treatment of the hydrogen ion, which had been the subject of Pauli's dissertation in the framework of atomic theory along the model of celestial mechanics 5 years earlier. "I soon saw that this was not going to work, and began to work with all sorts of more tractable spectroscopic topics," Unsöld recalled. Then, Sommerfeld became "really angry ... But when he then saw that I had found a number of new methods and theorems in the area of spherical harmonics, he graded the dissertation as summa cum laude."[61] In his commentary on the Unsöld dissertation, Sommerfeld stressed that it was "a characteristic of wave mechanics" that one could "put to good use" the mathematical methods of boundary value problems. Thus, Unsöld had "first derived the addition theorem of the spherical function" and demonstrated thereby "that the effect of the electron shells on external points exhibit simple spherical symmetry." From this, it had been possible to calculate the energy levels of the alkali and alkaline-earth atoms.[62] One year later, Unsöld's dissertation furnished [AU2]
the basis for an exhibit at the Deutsches Museum, where visitors could view "quantum mechanical atomic models."[63]

Around this time, at Schrödinger's request, Sommerfeld also arranged for Rockefeller Foundation grants for his former students Fritz London (1900–1954) and Walter Heitler (1904–1981) to allow them to pursue research on applications of

58 From Einstein, August 21, 1926. DMA, HS 1977-28/A,78. Also in ASWB II.

59 Bethe in Eckert/Pricha/Schubert/Torkar, *Geheimrat*, 1984, p. 8.

60 Bethe, *Sommerfeld's Seminar*, 2000.

61 Unsöld, personal communication, September 1, 1982.

62 Vote on the Dissertation of Unsöld to the Philosophical Faculty, 2nd section, December 11, 1926. Munich, UAM, OCI52p. Unsöld, *Beiträge*, 1927.

63 Eckert, *Atommodelle*, 2009.

Fig. 23: In 1927, Sommerfeld conceived this model of the gold atom for the Deutsches Museum. In place of electron orbits, there were, according to quantum mechanics, spatially distributed probabilities of an electron's being at that position, visualized in their ground state as spherical shells around the atomic core. The distance of each shell from the atomic core was calculated according to quantum mechanics and indicated an electron's positions of greatest probability; the thickness of the shells is proportional to the number of electrons in each state (Courtesy: Deutsches Museum, Munich, Archive).

wave mechanics.[64] They succeeded in elucidating the chemical relation between electrically neutral atoms by an interaction that had been classically inexplicable (the so-called exchange interaction), a quantum mechanical effect that illustrated forcefully the importance of the new theory for chemistry.[65]

Sommerfeld's institute in Munich also became a popular address for visiting researchers. American universities, primarily, used the study grants offered by the International Education Board of the Rockefeller Foundation and other support organizations to provide their students the opportunity of unrestricted research at one of the prestigious European scientific centers. The "traveling fellows" contributed significantly to the rapid spread of new scientific fields like quantum mechanics as widely throughout the USA as in Europe.[66] The first American grantees to

64 From Schrödinger, April 28 and May 11, 1926. DMA, HS 1977-28/A,314. Also in ASWB II; from Ewald, July 15, 1926. DMA, NL 89, 007; to Ewald, July 20, 1926. DMA, NL 89, 001; from Heitler, August 29,1926. DMA, NL 89, 009.

65 Heitler, Interview by John L. Heilbron, March 18, 1963. AHQP. http://www.aip.org/history/ohilist/4662_1.html (31 January 2013). Heitler/London, *Wechselwirkung*, 1927.

66 Sopka, *Quantum Physics*, 1988; Assmus, *Creation*, 1993.

come to Munich were the brothers Victor (1896–1985) and Ernst Guillemin (1898–1970). In 1923, Victor Guillemin had attended Sommerfeld's lectures at Madison and was so taken with them that he wished to delve deeper into atomic theory. During his stay in Munich, he witnessed the first debates about quantum mechanics. "Consequently quantum mechanics has been to me, not something I read about," he recalled years later; "I was 'there' when it was born."[67] In the summer of 1926, Linus Pauling also came as a visiting American student to Munich. Shortly before, he had completed his doctorate in physical chemistry at Cal Tech in Pasadena and had received a Guggenheim Foundation grant to study at Munich. "The exciting thing to me were the lectures Sommerfeld was giving on Schrödinger quantum mechanics and of course the seminars were devoted to it," he recalled years later.[68]

9.3 Electron Theory of Metals

According to the Pauli Exclusion Principle, every possible energy state in the atom can be occupied by at most one electron. If not only the electrons within an atom, but also the particles of a gas behaved according to this principle, the statistical distribution of particles across the different energy states of such a quantum theoretically "degenerate" gas was quite different from the determination reached by classical statistics for a normal gas. In 1926, on the basis of the Pauli Exclusion Principle, Enrico Fermi (1901–1954) and Paul Dirac established a new statistics—Pauli referred to it as the "Housing Authority" statistics.[69] In December 1926, he sent a manuscript, "On Gas Degeneration and Paramagnetism," to the *Zeitschrift für Physik*, in which he demonstrated in the theoretical case of "gas atoms with angular momentum" what the new statistics meant for the magnetic characteristics of such a gas: In an external magnetic field, according to "Housing Authority" statistics, all the particles could not line up the way, say, iron filings in proximity to a magnet would; only if, as a result of its reorientation, a particle acquired an energy state not already occupied by any other could it contribute to magnetization. "If the conduction electrons in the metal are regarded as an ideal degenerate gas," Pauli explained in transferring this conception to real circumstances, "we arrive on the basis of the developed statistics to at least a qualitative theoretical understanding of the fact that despite the presence of the electron's own magnetic momentum, many metals (especially the alkali metals) in their solid state show no or only very weak and roughly temperature-independent paramagnetism."[70]

In February 1927, Sommerfeld visited Pauli in Hamburg. When Pauli showed him the galleys of his paper "On Gas Degeneration and Paramagnetism,"

67 Victor Guillemin to KatherinSopka, 1972, quoted in Sopka, *Quantum Physics*, 1988, p. 2.41.

68 Pauling, Interview by John L. Heilbron, March 27, 1964. AHQP. http://www.aip.org/history/ohilist/3448.html (31 January 2013).

69 Pauli to Wentzel, December 5, 1926. In WPWB I.

70 Pauli, *Gasentartung*, 1927, p. 81.

Sommerfeld ventured the conjecture that other characteristics of metals could also be explained according to this paradigm. But Paul was less concerned with a theory of metals than with this as a test case of the new Fermi-Dirac statistics. Sommerfeld proposed that he should next apply this to the explanation of other characteristics of metals, Pauli recalled years later. "As I was not eager to do that, he made then this further application himself."[71]

Pauli's aversion to solid-state physics became legendary. Once, when an assistant planned to take up the theory of electrical resistance in metals, he reacted with the disparaging remark that this was "a filth-effect, and one shouldn't wallow in filth."[72] By contrast, electron theory of metals was quite to Sommerfeld's taste. The conception of an electron gas capable of moving freely between the atoms was a very old one. Unlike isolators, where electrons are bound to atoms, the electrical conductivity of metals appeared comprehensible only if the electrons were allowed freedom of motion. To be sure, this led to contradictions which brought the "free electron gas" into discredit. If the electrons could participate in the motion that showed up as an electrical current, this should be true for thermal motion as well, but the specific heat of metals is hardly distinct from the isolators, so that in this respect the electrons could not be assumed to be freely mobile.

Sommerfeld had long been familiar with this and other contradictions.[73] He found it attractive to investigate whether the "Housing Authority" statistics could also be made to account for the dilemma of specific heat and other characteristics of metals. Before he published anything on the matter, he familiarized himself and his advanced students with electron gas theory by way of a special course of lectures. He had always felt his way into new theories through this tried and true paradigm. For the summer semester of 1927, he therefore announced a special lecture course on "Structure of Matter." First, he explained the dilemma of specific heat: "Housing Authority" statistics provided that only a very few electrons could take part in thermal motion, so that the increase in specific heat was approximately only 100th that of isolators.[74] In the lecture hours that followed, he dealt with the ejection of electrons from metals (Richardson effect) and the phenomena accompanying contact between various metals (Volta effect). "During this semester" he had been "emphatically interested in the Fermi statistics and gas degeneration," he wrote Paschen towards the end of the semester. And there was "weighty evidence of the correctness of Fermi's degeneration formula."[75] Two weeks later, he sent a "presentation [of it] as broadly comprehensible as possible" to the editor of

71 Pauli to Rasetti, October, 1956. AHQP. ("[…] that one should make further application to other parts of metal theory like the Wiedemann–Franz law, thermoelectric effects, etc. As I was not eager to do that, he made then this further application himself.").

72 Pauli toPeierls, July 3, 1931. Quoted in WPWB II.

73 Eckert, *Elektronengas*, 1989.

74 Lecture notes of Unsöld, now DMA.

75 To Paschen, July 25, 1927. DMA, NL 89, 003.

Naturwissenschaften. "Using the new statistical methods of Fermi, my paper seeks to bring order to the age-old problem of the galvanic current, the Volta potential, thermal energy, etc."[76]

Following on this overture, electron theory of metals became, along with wave mechanics, a central focus of research at the Sommerfeld institute. Already in his first comprehensive publications on the subject, Sommerfeld referred to follow-up work being carried out by American fellowship recipients at his institute.[77] "I am very pleased with your two students Dr. Eckart and Dr. Houston," Sommerfeld wrote Millikan at Pasadena after these two had arrived at Munich in the fall of 1927 on Guggenheim Foundation grants. "I carry on the most interesting discussions with Eckart on fundamental questions of electron theory, and I admire the trenchancy and breadth of his observations. But Houston also proves himself admirably. He has taken up specific questions proceeding from my note on metal electrons energetically and with great success."[78] William V. Houston (1900–1968) had actually wanted to pursue research on spin, which became an increasingly challenging matter for quantum physics. But Sommerfeld had advised against this, he later recalled, and instead had given him the galleys of his application of quantum statistics to electron gas.[79] Sommerfeld had Houston work up a wave mechanical explanation of the mean free path lengths of the electrons in metal.[80] Shortly after the publication of Schrödinger's papers, Carl Eckart (1902–1973) had shown the equal validity of wave mechanics and matrix mechanics and had likewise had fundamental quantum mechanical problems in mind before coming to Munich. He too let himself be persuaded to pursue research on the electron theory of metals.[81] "Both gentlemen, Houston and Eckart, have been personally very agreeable, and have proven of direct utility to me," he wrote Millikan gratefully half a year later.[82]

The spark leapt across to other institutes as well. Sommerfeld never tired of proselytizing for the electron theory of metals as a promising area of future research.[83] This theory also offered the prospect of elucidating long inexplicable solid-state properties. Sommerfeld had at first merely replaced classical statistics with the Fermi-Dirac quantum statistics and otherwise had treated electrons as a free gas. But it was clear that this could actually be only a temporary solution. If the electrons in the atom obey the laws of quantum mechanics, this should also be true of their motion between the atoms in a crystal lattice. This quantum mechanical extension of electron theory was among the topics with which one could make a name as "a

76 To Berliner, August 6, 1927. DMA, NL 89, 001. Sommerfeld, *Elektronentheorie der Metalle*, 1927.

77 Sommerfeld, *Elektronentheorie der Metalle 1, 2*, 1928.

78 To Millikan, November 28, 1927. DMA, NL 89, 025.

79 Houston, Interview by G. Phillips and W. J. King, March 3, 1964. AIP.

80 Houston, *Leitfähigkeit*, 1928.

81 Eckart, *Elektronentheorie der Metalle*, 1928.

82 To Millikan, March 26, 1928. DMA, NL 89, 025.

83 Eckert, *Propaganda*, 1987.

modern physicist" at the end of the 1920s. To be "modern" was to be conversant with the new quantum mechanics, and the physics of solid-state phenomena offered a cornucopia of problems on which a contender for an academic career as a theoretical physicist could demonstrate his quantum mechanical expertise.[84]

The first of these new physics centers grew up at Leipzig, where in 1927, in the persons of Heisenberg and Debye, two Sommerfeld students were appointed to professorships in theoretical and experimental physics. Shortly thereafter, two other Sommerfeld students, Pauli and Wentzel, were appointed to professorships of theoretical physics at the ETH and the University of Zürich, respectively. The Munich "nursery" had thereby sprouted branches at Leipzig, Zürich, Stuttgart (Ewald), and Hamburg (Lenz), and as happens with subsidiaries of an enterprise, there was a lively exchange of knowledge and personnel among the branches of the Sommerfeld school. "So, you'd like to steal assistants? And naturally only the best!" The directors of the branches communicated in this tone when they needed to fill positions.[85] The founders of quantum mechanical solid-state theory—Hans Bethe, Felix Bloch (1905–1983), Rudolf Peierls (1907–1995), and others—began their careers in one of these branches and were occasionally transferred from one to another of them. The same was true for advanced students and recent doctorates in theoretical physics, who began their academic careers at the branches of the Sommerfeld school with a grant from the Rockefeller Foundation or some other granting institution. "I think it would be a nice idea," Heisenberg wrote Pauli on one occasion, "to establish a sort of physicists' exchange between Zürich and Leipzig."[86] In congratulating Sommerfeld on his 60th birthday, he coupled his best wishes with the hope that in Munich, Sommerfeld would "for a long time yet [sponsor] a nursery for physical babies as for Pauli and me at that time."[87]

9.4 The Planck Succession

Even though Sommerfeld's institute represented the "nursery" for the network of new quantum schools, the most prestigious chair to which a theoretical physicist could aspire was not Sommerfeld's, but Max Planck's at the University of Berlin. With this chair, Planck had assumed the legacy of Gustav Kirchhoff, who in 1875, as the first full professor of theoretical physics in Germany, had given this discipline the status of an independent field. As permanent secretary of the mathematical physics class of the Prussian Academy of Sciences, Planck exercised a significant representative function in addition to his university teaching activity. When the Solvay Congress of 1927 was being prepared in Belgium, to which for the first time

84 Hoddeson/Baym/Eckert, *Development*, 1987.
85 To Heisenberg, November 15, 1927. DMA, NL 89, 002.
86 Heisenberg to Pauli, August 1, 1929. In WPWB I.
87 From Heisenberg, February 6, 1929. DMA, HS 1977-28/A,136.

since the war physicists from Germany were to be invited, great value was put on Planck's participation. The topic was quantum mechanics. Aside from Planck, only Born, Heisenberg, and Pauli were invited from Germany—not Sommerfeld, which annoyed some of those invited. Planck judged himself, in contrast to Sommerfeld, "no longer among those on the leading edge of the development of quantum theory and in the front rank of those qualified to participate in the Congress."[88] Born also felt himself "quite taken aback" over the fact that Sommerfeld had not been invited, and thought "that your name should be at the top of the list of Germans invited to the new quantum congress."[89] In the event, the Belgians had not been prepared to invite more than four Germans: Born, Heisenberg, and Pauli represented unquestionably the front rank of the German quantum theorists, and Planck was invited because he—not Sommerfeld—would be recognized as the preeminent representative of German science.[90]

Nevertheless, Sommerfeld was hardly second to his 10-year senior Planck when it came to upholding the reputation of German science abroad. When Planck retired in 1926 at the age of 68, it was thus scarcely to be wondered at that Sommerfeld was at once thought of as his successor. Planck's chair was to be entrusted only to someone who, like Planck, could act as scientific spokesman. Already in the initial appointment deliberations, Sommerfeld ranked as the leading candidate; others named were Born, Hans Thirring, and Schrödinger; Einstein and Laue were also briefly considered, but withdrew their names from the list. Einstein did not wish to trade his position at the Academy, which offered him free pursuit of his own research, for a professorship that would burden him with teaching duties. And appointing Laue, who held the second full professorship in theoretical physics at the University of Berlin, would merely have evoked an additional succession debate. Thus, the first list of proposed candidates comprised "Sommerfeld, Born, Schrödinger."[91] With respect to Sommerfeld, there was no doubt even in ensuing deliberations of the Appointment Commission that he should be ranked in the top spot on the list. For the second and third spots, however, "after careful consideration" it was determined "that Schrödinger's physical achievements possessed an inherently more profound originality and a greater creative force" than those of Born. So Schrödinger was placed second and Born third. Heisenberg, as a representative of the younger generation, also came under consideration. He would "at some future date surely [number] among the first rank of researchers," but he was not yet to be entrusted with the role of scientific spokesman incumbent on the Planck successor.[92]

88 Planck to Lorentz, June 13, 1926. AHQP.

89 From Born, June 15, 1926. DMA, NL 89, 006. Also in ASWB II, S. 255.

90 Heilbron, *Dilemmas*, 1986, pp. 107–108.

91 Appointment Commission to the Philosophical Faculty of the Friedrich-Wilhelm-University, Berlin, June 18, 1926. UAB.

92 Minutes of the faculty meeting of November 18, 1926, and report of the Faculty to the Prussian Ministry of Culture of December 4, 1926. UAB.

Sommerfeld was informed of the imminent offer of appointment before receiving official notice of it from the Berlin Ministry of Culture. "The entire faculty has, as you are no doubt aware, decisively named you in the top position," Nernst wrote him in advance of the appointment.[93] At the time Sommerfeld received the announcement of appointment from the Prussian Ministry of Culture, he was on a trip to the Balkans. His wife, though, knew how she was to answer. "You have surely written to Berlin, as agreed," Sommerfeld wrote from Ragusa. "I will myself write first to Planck, and then after an appropriate interval to Berlin, that I will not come before the week after Easter."[94] He wished to negotiate with the Prussian Ministry of Culture the terms under which he would accept the appointment. Planck knew quite well how difficult it would be to pry Sommerfeld loose from Munich. He promised Sommerfeld he would do all he could to sweeten the appointment and closed his letter with the plea to Sommerfeld's wife "to exert her influence in favor of Berlin."[95]

What to Planck and the Berlin physicists was a hope appeared to Sommerfeld's Munich colleagues as a threat. The two mathematicians Oskar Perron (1880–1975) and Constantin Carathéodory (1873–1950) went at once to the Bavarian Ministry of Culture and on behalf of the faculty urgently requested the university overseer in the imminent negotiations to retain Sommerfeld in Munich. "I tried to make clear to him," Carathéodory reported to Sommerfeld later "what it would mean for the University if we were after all to lose you."[96] The President of the Bavarian Academy of Sciences wrote to Sommerfeld: "As pleased as I am that you have been accorded this recognition, I tremble in equal measure for Munich."[97] The Rector of the University, Karl Vossler, who was also a personal friend of Sommerfeld's, declared himself "prepared to take any step that would lead you to a favorable turn towards Munich. I am also convinced that the Senate would support me in whatever action would be appropriate to keep you in Munich." He invoked the cordial relationship between their families in averring that his wife and children would "suffer an irreparable personal loss by your departure. The farewell would be very heavy for us, and we have no intention of lightening the farewell for you and yours."[98]

Sommerfeld was probably determined from the outset to remain at Munich, but the Berlin offer presented him the opportunity of improving his position there. To achieve this, he had to negotiate with the Prussian Ministry of Culture an offer better than his current position, so that in subsequent negotiation with the Bavarian Ministry of Culture, he could in turn improve his Munich position.

93 From Nernst, March 19, 1927. DMA, NL 89, 019, folder 5,9.

94 From Windelband, March 24, 1927. DMA, NL 89, 019, folder 5,9. Also in ASWB II; to Johanna, March 30, 1927.

95 From Planck, April 7, 1927. DMA, NL 89, 019, folder 5,9. Also in ASWB II.

96 From Carathéodory, March 28, 1927. DMA, NL 89, 019, folder 5,9. Also in ASWB II.

97 From Gruber, March 28, 1927. DMA, NL 89, 019, folder 5,9.

98 From Vossler, March 30, 1927. DMA, NL 89, 019, folder 5,9. Also in ASWB I.

These negotiations extended over 2 months. Berlin was prepared to offer Sommerfeld a higher salary than Munich and to add a supplement for the rental of an apartment or the purchase of a house. In addition, he was given assurances of improved conditions at the institute, already considered insufficient by Planck.[99] At the negotiations over his remaining in Munich, Sommerfeld demanded above all the establishment of an associate professorship in theoretical physics. He was aware that, under prevailing financial circumstances, he could not count on fulfillment of this demand, but he wished on behalf of the University to put on record with the Ministry that such a professorship was to be instituted as soon as sufficient resources could be made available in Bavaria.[100] When he was assured of this, together with other improvements in his Munich position, he declined the Berlin offer. He gave as the principal reason for his decision "the much simpler working and living conditions, and the much better facilities of the Institute."[101] The Bavarian Ministry of Culture had "fulfilled entirely" his wishes, he wrote to Berlin, "not only with respect to my personal circumstances, but also with respect to the organization of the Institute and its pedagogy."[102]

Thereupon, the Planck succession was offered to the second candidate on the list, Schrödinger, who accepted after protracted negotiations over "virtually" the same conditions stipulated by Sommerfeld. "Differences: not quite the top salary, but 1,700 M less per annum," Schrödinger reported to his "advisor" Sommerfeld, who had fully briefed him beforehand on the inner workings at Berlin.[103]

9.5 "Not Sommerfeld, but Schüpfer"

At issue for Sommerfeld in his decision to remain at Munich was not just the increase in his salary and better equipment for his institute. Primarily, he did not wish to give up the successful pedagogical enterprise he had built up and been so personally involved in over the past two decades. "It seems to me doubtful that interaction with students in big and restless Berlin could be organized as intimately as at Munich," he wrote in an article for the *Süddeutsche Sonntagspost*. To be sure, it had not been easy for him, as an "old Prussian," to decline an appointment to Berlin, to "the city in which Helmholtz and Kirchhoff were active, where Planck and Einstein live, the center of German intellect and work." But he cherished the more informal Bavarian lifestyle, and the nearby mountains that offered him and

99 File notation, Prussian Ministry of Culture, April 29,1927. DMA, NL 89, 019. folder 5,9.

100 To the Dean, Philosophical Faculty, 2. Sektion, May 16. 1927. DMA, NL 89, 004. From the Bavarian Ministry of Culture, May 25, 1927 and June 20, 1927. DMA, NL 89, 019. folder 5,9.

101 To C. H. Becker, draft of an undated letter [between. May 31and June 11, 1927]. DMA, NL 89, 019. folder 5,9.

102 To the Prussian Ministry of Culture, June 11, 1927. DMA NL 89, 004. Also in ASWB II.

103 FromSchrödinger, July 14, 1927. DMA, NL 89, 019, folder 5,9.

his students opportunities for skiing, and lent his pedagogical enterprise a very personal character. Berlin "uses up its people quickly, whereas Munich, situated at the foot of the mountains, allows even the elderly to find refreshment and renewal."[104]

He already had very concrete plans for the associate professorship promised him in return for his declining the Planck chair. He had the outcome of his negotiations over remaining at Munich be given him in writing once more,[105] and wrote to Heisenberg, whom he had in mind for the associate professorship, describing his vision for the future of his "nursery." As a first step, he suggested "Please save yourself for the Munich associate professorship. You will thereby be entitled after a number of years to become my successor in the full professorship. Of course these are just my intentions, and are proposed without being binding on the faculty. But I see no obstacles in the way of their being carried out."[106]

Sommerfeld may have regarded the great esteem shown him during negotiations over the Planck succession as the expression of highest recognition on the part of Munich professors generally. In fact, many of his colleagues espoused radically different views. This became obvious when the next election for Rector was at hand in July 1927. Sommerfeld had put himself up for election as Vossler's successor, a move that stirred displeasure among anti-Semitic and right-wing circles. "The dissatisfaction with the current democratic-pacifistic and Jew-loving Rector, Dr. Vossler, is general," one might read in the *Völkischer Beobachter*, which sought at all costs to prevent "the Jew and Professor, Dr. Sommerfeld" from succeeding the hated Vossler.[107] That Sommerfeld, as the National Socialists later would concede, had no Jewish forebears down to his great-grandparents did not dampen the press campaign. He, like Vossler, was counted as Jew loving and liberal. Vossler had attracted the animosity of the right-wing circles when at the annual celebration of the establishment of the Reich, "standing on the grounds of the Constitution," as the *Vossische Zeitung* stressed, he had arranged for the black-red-and-gold national flag of the Republic to be raised alongside the black-white-red, which stood for the Kaiser's Reich. Unlike the *Völkische Beobachter*, the *Vossische Zeitung* maintained it would be "greatly" in the interests of the University of Munich "that the liberal era inaugurated by Vossler continue to prove its viability." To the liberal press, Sommerfeld was the guarantor of this tradition. The opposition candidate was a forestry expert by the name of Vinzenz Schüpfer, "whose scientific importance cannot in the least be compared to that of the famous physicist, Sommerfeld," as the *Vossische Zeitung* stressed.[108]

Sommerfeld lost with 50 votes to his opponent's 68. "Not Sommerfeld, but Schüpfer" ran the headline in the *Berliner Tageblatt*; the "scientifically insignificant,

104 Sommerfeld, *Berlin*, 1927.

105 From Goldenberger (Bavarian Minister of Culture), June 20, 1927. DMA, NL 89, 019, folder 5,9.

106 To Heisenberg, June 17, 1927. DMA, NL 89, 019, folder 5,9.

107 *Völkischer Beobachter*, July 8, 1927.

108 *Vossische Zeitung*, July 12, 1927.

Fig. 24: Procession of the Munich professors in 1926 on the occasion of the centennial celebration of the Ludwig Maximilian University (in 1826, the University was moved from Landshut to Munich). In 1927, Sommerfeld stood for election as Rector, but was defeated by "the right-wing sentiments of the professorial majority" (*Frankfurter Zeitung*) (Courtesy: Deutsches Museum, Munich, Archive).

but 'dependably nationalistic' forester Schüpfer" had defeated the "world-renowned physicist Arnold Sommerfeld."[109] "A victory for the party politicians," commented the *Frankfurter Zeitung* on the outcome of the election for Rector in Munich. Therein "the right-wing sentiments of the professorial majority [had] once more been documented." The election of Schüpfer was to be chalked up to the "Professors' table of German nationalists and like-minded adherents of the Bavarian People's Party."[110]

With Sommerfeld's defeat, the liberal era at the University of Munich represented by Vossler came to an end, even before it had properly begun. In an "address to Vossler," Sommerfeld paid tribute to his "extraordinary official service" and on behalf of his fellow signatories expressed the hope that it "would leave behind a lasting legacy in the history of the University." However, the notation "not sent" at the bottom of the hand-written draft implies that due to a lack of signatories, this declaration was stillborn.[111] Ultimately, Sommerfeld had to have been beset after all by regrets over his refusal of the Planck chair. "Now and then I am sorry not to have come to Berlin," he wrote to Einstein; "my dear Munich colleagues have certainly greatly annoyed me in the interim."[112]

109 *Berliner Tageblatt*, July 22, 1927.
110 *Frankfurter Zeitung*, July 21, 1927.
111 To Vossler, undated [ca. July, 1927].DMA, NL 89, 019, folder 5,9.
112 To Einstein, November 1, 1927. AEA, Einstein. Also in ASWB II.

9.6 The Volta Congress

Annoyance over politics at his university, however, was soon eclipsed by his enthusiasm for physics. "On Quantum Mechanics" and "Selected Questions in Wave Mechanics" were the titles of Sommerfeld's special lecture courses in the winter semester of 1927/1928 and the summer semester of 1928. Aside from the first efforts at a quantum mechanical solid-state theory, to which he himself had given the impetus with his electron theory of metals, quantum mechanics accounted in many other areas for a sense of breakthrough among theoretical physicists. In 1926, Born had for the first time applied quantum mechanics to collision processes and in this connection had given a new interpretation to Schrödinger's wave mechanics. It was not the electrons described by the Schrödinger equations that were spatially "smeared" like a wave, but the probability of finding them at this or that location.[113] In December 1926, Dirac and Jordan lifted quantum mechanics with a "transformation theory" to new abstract heights.[114] In March 1927, Heisenberg added fuel to the fire with his "uncertainty principle."[115] In less than 2 years, quantum mechanics had, as no theory heretofore, turned physics inside out with respect to both foundations and applications.

In September 1927, on the occasion of the hundredth anniversary of the death of Alessandro Volta (1745–1827), an international congress of physicists took place in Como, at which differing conceptions of quantum mechanics were exchanged for the first time in a larger context. At first, Sommerfeld suspected that politically motivated propaganda was behind the event. "I have been invited to a small conference of big shots in Como in 1927 in observation of the Volta centennial," he wrote James Franck. "I have serious reservations about attending because I assume the Italians will not forego the opportunity of making it political and trotting out Mussolini."[116] As in the microcosm of the Munich Rector's election, in the larger picture, too, science was not isolated from politics. An international conference in Italy, where the fascists had just taken power, seemed to Sommerfeld a chess move by Mussolini to make his politics internationally presentable. Before he accepted the invitation, therefore, he wanted to know whether he was alone in his reservations. "Have you also been invited, and what do you plan to do?" he inquired of Laue. "Is Planck going? It would be a good thing if we could agree on a common course of action."[117] Franck, Laue, and Planck, who like Sommerfeld had received invitations to Como, advised in favor of attending. "If the Italians do something tactless, it only reflects back on them," Laue replied.[118]

113 Beller, *Quantum Dialogue*, 1999, pp. 39–49.
114 Mehra/Rechenberg, *Development*, 2000, Part 1, Chap. I.5; Janssen/Duncan, *Transformations*, 2009.
115 Rechenberg, *Werner Heisenberg*, 2010, Chap. 8.
116 To Franck, July 20, 1926. DMA, NL 89, 001. Also in ASWB II.
117 To Laue, July 22, 1926. DMA, NL 89, 002.
118 From Laue, July 27, 1926. DMA, NL 89, 010.

Accordingly, Sommerfeld put his reservations aside and accepted the invitation. The Volta Congress brought the leading physicists from the recently adversarial states together for the first time since World War I. What Sommerfeld had thought would be "a small conference of big shots" turned out to be a congress of more than 60 participants from over a dozen countries (Denmark, Germany, England, France, India, Italy, Canada, Holland, Austria, Switzerland, Spain, the Soviet Union, the USA)—among them prominent physicists such as Niels Bohr, Arthur H. Compton, Hendrik A. Lorentz, Ernest Rutherford, and Robert A. Millikan. The lectures delivered at this congress covered physics in its broadest scope. At Como, Sommerfeld presented his current results in electron theory of metals.[119] A number of other lectures also dealt with solid-state physics. But what made the congress an unforgettable event for its participants were the discussions of the interpretations of quantum mechanics. Bohr's concept of the "principle of complementarity" was the subject of vigorous debate that continued well beyond the conclusion of the congress.[120]

As with his lecture tour in England in March 1926, Sommerfeld used the Volta Congress as an opportunity to socialize with colleagues over and above purely professional substance. He befriended the Russian physicist Jakow Frenkel (1894–1952) and after the congress traveled with him through southern Italy. Frenkel shared Sommerfeld's interest in the electron theory of metals and later made important contributions to solid-state physics. "My traveling companion here and for Sicily is Frenkel, a physicist from St. Petersburg," Sommerfeld explained to his wife.[121] They must have embarked on their joint trip through southern Italy quite spontaneously, for at one point they found themselves temporarily in financial straits such that Sommerfeld was compelled to ask his "illustrissime amice," Tullio Levi-Cività (1873–1941) in Padua, to help them out of their difficulty by sending money.[122] "October 1, and I am still in Naples! How is it going to work out for me to get home?!" Sommerfeld reported the impromptu extension of his trip to his wife. "First Pompeii, and along with it, Vesuvius, on horseback (!)," he enthused concerning his latest travel experiences. "I may go broke again today. But it was a great experience. The trip down into the crater (which entailed a supplement of 15 Lire), fabulous: every other minute a thunderous eruption of water and sulfur vapor. A terrific fumarole [. . .] Next morning, to Capri, Blue Grotto in a small bark, instead of the steam-boat company with the herd."[123]

119 Sommerfeld, *Elektronentheorie der Metalle und des Voltaeffektes*, 1927.

120 Beller, *Quantum Dialogue*, 1999, Chap. 6. See also http://www.science20.com/don_howard/revisiting_einsteinbohr_dialogue (31 January 2013).

121 To Johanna, September 29, 1927.

122 To Levi-Cività, September 28, 1927. Rom, BANL, Levi-Cività.

123 To Johanna, October 1, 1927.

9.7 A *Wave Mechanics Supplement*

Two months after the Volta Congress, in Brussels at the fifth Solvay Congress, there were lively discussions between Bohr and Einstein on how quantum mechanics, which could not be understood in the terms of classical physics, should be interpreted. But it was not just questions of interpretation that made the Volta and Solvay Congresses extraordinary events in the history of physics of that time. From solid state, nuclear, and astrophysics to chemistry, new applications of quantum mechanics opened up, giving the field ever more the appearance of a huge construction site for theoretical physicists, but one lacking a comprehensive plan underlying the whole and with no indication what new buildings were going up. In this situation it was no wonder that soon a demand for overarching surveys arose. The interest was "so great that a report concerning the current state of research would perhaps be in order," Rudolf Seeliger wrote in November 1927 to Sommerfeld. Seeliger had completed his doctorate under Sommerfeld's direction in 1910 and now taught theoretical physics at the University of Greifswald. As coeditor of the *Physikalische Zeitschrift*, Seeliger was familiar with the current publication focus of his colleagues and the scientific publishing houses. He had been requested from many different directions "to present a comprehensive report about wave mechanics," so he passed these interests along to his former teacher. In addition, a colleague planning to write a book about X-ray spectra had recently written him to inquire whether "in light of the rapid developments" Sommerfeld's exposition of this subject in the latest edition of *Atomic Structure and Spectral Lines* "was still valid and supported by you, or should be revised."[124]

Three years after the appearance of the fourth edition of *Atomic Structure and Spectral Lines*, the idea of a new edition must often have occurred to Sommerfeld. Even if quantum mechanics in his opinion was not a revolution, but only a further step in the evolution of atomic theory, this step was nonetheless so important that it had to be properly presented in a new edition. But since Sommerfeld had kept the previous edition "entirely free of all model-related preconceptions" (at least insofar as its most important part, the complex structure of spectra, was concerned), it was initially less a matter of correcting the pre-quantum-mechanical presentation, than of amending fundamental theoretical principles. The pre-quantum-mechanical conceptual system was perfectly adequate to embrace the laws of the spectra themselves, as he had described them in the first four editions. Although according to the Pauli Exclusion Principle and the introduction of spin, that which before had been intended for the atomic core with the inner quantum number had to be transferred to each individual electron. But this changed little in the empirically established laws formulated in terms of the concepts of the old Bohr-Sommerfeld atomic model. For example, in a 1928 book on spectra, the astrophysicist Walter Grotrian expressed the view that "even with the current state of theory, there need be no

124 From Seeliger, November 20, 1927.D MA, NL 89, 013.

reservations" against these conceptions that have actually been superseded by quantum mechanics, "so long as one maintains clarity that the Bohr electron orbits are to be regarded merely as illustrative aids to conceptualization, and not as reality."[125]

Given this background, Sommerfeld abstained from a revised presentation of the largely empirically based spectral laws he had described so comprehensively in the fourth edition and concentrated fully on quantum mechanics. There was, in other words, to be no fifth edition of *Atomic Structure and Spectral Lines* (for now, at least), rather just a "wave mechanical supplement" to the fourth edition. He quite intentionally employed the term "wave mechanical," and not "quantum mechanical," increasingly the general usage, "because in practical application, Schrödinger's methods are clearly superior to the specifically 'quantum mechanical' methods." In addition, he wished "as much as possible to restrict [himself] to concrete questions." He would discuss the "principal questions of uncertainty and observability" only peripherally.[126]

Like the various editions of *Atomic Structure and Spectral Lines*, the *Wave Mechanical Supplement* was not the product of a lone act of writing in an ivory tower, but rather mirrored the research enterprise of the Sommerfeld school. Anyone who had taken a doctorate under Sommerfeld or worked as an assistant or lecturer at his institute between 1925 and 1928 could find his work in one or another subchapter or could bask in the knowledge of having sown critical seeds of various passages. In this way, for example, the "crystal interferences of electron waves," which had only shortly before been discovered experimentally and in 1928 formed the subject of Bethe's doctoral dissertation, became the contents of one chapter.[127] Unsöld, the "latest Wunderkind" of the Sommerfeld school, found himself immortalized in a chapter on the "Spherical Symmetry of the S-Terms."[128] Sommerfeld registered special thanks to his assistant, Karl Bechert, without whose "devoted assistance" he could hardly have produced this work.[129]

Unlike when 10 years before Sommerfeld had conceived the outline of *Atomic Structure and Spectral Lines* in the course of giving popular lectures on atomic models, the *Wave Mechanical Supplement* did not emerge from an effort to popularize wave mechanics. Mathematical expositions of spherical harmonics and Bessel functions, complex integration, and other such matters more likely to scare off theoretically less knowledgeable readers were not relegated to "Addenda and Supplements," but were on

125 Grotrian, *Darstellung*, 1928, p. VII.

126 Sommerfeld, *Ergänzungsband*, 1929, Vorwort.

127 Vote on the Dissertation of Bethe to the Philosophical Faculty, 2. Section, July 24, 1928. UAM, OC Np1928. Sommerfeld, *Ergänzungsband*, 1929, pp. 241–250.

128 To Finlay Freundlich, January 14, 1927. DMA, NL 89, 001. Vote on the Dissertation of Unsöld to the Philosophical Faculty, 2. Sektion, December 11, 1926. UAM, OCI52p. Sommerfeld, *Ergänzungsband*, 1929, pp. 101–105.

129 Sommerfeld, *Ergänzungsband*, 1929, Foreword.

the contrary an integral part of the presentation. Although Sommerfeld was primarily addressing theoretical physicists, he also wished to disseminate the theory beyond his own discipline. The first opportunity to do so came in May 1928, when he was asked to lecture to the German Bunsen Society for Applied Physical Chemistry, which met that year in Munich. The Bunsen Society was an association steeped in tradition, which prided itself on "exchanging the gold of new foundational ideas and results in the area of physics into currency for the use of chemical science and chemical engineering," as their president emphasized in his welcoming speech.[130] Sommerfeld did not wish to represent quantum mechanics to the Bunsen Society as a complete overturning of the atomic concepts with which chemists had just become familiar. "I will of course stress," he wrote to Friedrich Hund (1896–1997), who also had been invited to give a talk, "that much from the original models remains intact, namely quantum numbers, spectra, the periodic table, the Pauli Principle." Hund also might wish to dilute his scientific wine with "a little popular water," Sommerfeld counseled; he suggested a quantum mechanical interpretation of the chemist's formulas of valence bond.[131]

In his own lecture, Sommerfeld drew quite a clear picture of the actually rather unclear new atomic theory. Just as wave optics replaces geometrical optics when one transitions from coarse optical instruments to finer ones such as the microscope, it is necessary to supplant the "ordinary macro-mechanics" of our everyday experience with wave mechanics when dealing with things of atomic dimensions. But wave mechanics is also a statistical theory. It describes the behavior of "swarms of electrons" with laws like those we recognize for waves. These are not waves in space, however, but rather an "abstract something" that describes the probability of an electron's location. Sommerfeld explained this "something" with the example of a hydrogen atom, in which the square of the Schrödinger wave function shows the density of the charge cloud of the electron around the atomic core. "What does 'density of the charge cloud' mean?" he went on to ask, to preclude the false image of an electron with spatial extension. "We believe that the electron is a virtually point-form structure, and that its entire charge is concentrated in the smallest space." The density of the charge cloud indicates the probability that a given point-form electron is to be met with here or elsewhere. The old atomic model with its planetary orbits displayed a "disc symmetry"; the new theory offered a far more plausible explanation of how the electrical charge filled the space in the atom. The formation of molecules and the forces among ions in a crystal could also be explained satisfactorily. "In general, I have the impression," he concluded his lecture, "that the new theory addresses the needs of chemists in an especially felicitous, and actually a better way than the earlier conception of individual electron orbits."[132]

130 *Zeitschrift für Elektrochemie und angewandte physikalische Chemie* 34 (1928), pp. 425–426.

131 ToHund, February 29, 1928. DMA, NL 89, 002.

132 Sommerfeld, *Bedeutung*, 1928.

10 Cultural Ambassador

"When I travel abroad I feel I am not merely a private individual and globetrotter, but an ambassador of German culture in the realm of science." Thus Sommerfeld began his lecture on December 8, 1928, in Tokyo.[1] Tokyo was one of many stops on an 8-month world tour on which Sommerfeld carried out this self-imposed cultural mission. The idea of a world tour occurred to him after the Volta Congress in September 1927 when Millikan proposed a split guest professorship that would take him to the University of Chicago and the California Institute of Technology for the winter semester of 1928/1929.[2] Though that plan fell through, Millikan wanted at least to bring Sommerfeld to Cal Tech: "pasadena [sic] wants you definitely winter quarter twenty-nine," he telegraphed to Munich.[3] After brief deliberation, Sommerfeld accepted the invitation and announced that this time he would travel to America from the east, across the Pacific.[4] He may have been giving himself an unusual present on the occasion of his 60th birthday, which he would celebrate somewhere in Japan on December 5, 1928. Or, was he perhaps dodging festivities threatening him at home on this day? Bethe believed that "A major motivation for this trip was that he did not want to be in Munich on his 60th birthday."[5]

In any case, Sommerfeld's travel plans quickly made the rounds, assuring that invitations to lecture flooded in from many countries. The first invitations came by telegram from India.[6] Sommerfeld asked Chandrasekhara Venkata Raman (1888–1970), who invited him to lecture at the University of Calcutta, and Meghnad Saha (1893–1956), whom he had met at the Volta Congress in Como, to arrange a 4-week lecture and sightseeing tour through India for him.[7] In Japan, his former student Otto Laporte, now a professor at the University of Michigan in Ann Arbor, just then on a guest professorship at the University of Kyoto, provided the first contacts. Toshio Takamine (1885–1959) wrote Sommerfeld in March, 1928 that he had just from Laporte received "the glad tiding . . . that in the coming winter, there may be a chance for us to have the pleasure + honour of being visited by you, + if possible, to hear your lectures a few times in Japan."[8] To leave no doubt that he regarded his guest lectures as a cultural mission, Sommerfeld consulted the Cultural Division

1 Sommerfeld, *Entwicklung*, 1929b.
2 From Millikan, October 22, 1927. DMA, HS 1977-28/A,232.
3 From Millikan, November 25, 1927. DMA, HS 1977-28/A,232.
4 From Millikan, February 6, 1928. DMA, NL 89, 011. Also in ASWB II.
5 Preface to Eckert/Pricha/Schubert/Torkar, *Geheimrat*, 1984, p. 9.
6 From Raman, February 11, 1928. DMA, NL 89, 024, folder India.
7 To Raman, February 28, 1928. DMA, NL 89, 024, folder India.
8 From Takamine, March 19, 1928. DMA, NL 89, 019, folder 4,3. Also in ASWB II.

M. Eckert, *Arnold Sommerfeld: Science, Life and Turbulent Times 1868-1951*,
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of the Foreign Office regarding his travel plans. He was advised to apply for a subsidy from the Emergency Organization of German Science (*Notgemeinschaft der Deutschen Wissenschaft*).[9] In addition, he would have to arrange for his replacement at the University of Munich for the winter semester of 1928/1929. His lecturer Heinrich Ott was to give his main lecture course. Karl Bechert, his second assistant, would take over the exercise classes. Advanced lectures would be given by Laporte, who wished to spend several months in Germany on his return from Japan in the summer of 1928 to visit his parents in Munich, before returning to the USA in January 1929.[10]

10.1 German Science on the International Stage

Shortly before setting forth on his great journey around the world, Sommerfeld was once more put in mind of the peculiar situation of German science in the decade following World War I: His Spanish colleague Blas Cabrera (1878–1945) informed him of plans to open the *Conseil International de Recherches* (International Research Council) to membership by German scientists. The *Conseil* had been established after World War I as a replacement for the International Association of Academies, which was dominated by the Central Powers.[11] To Sommerfeld, however, this international research council was a relic of the boycott against German science following World War I. Although Germany should certainly not remain excluded from this international scientific organization, the manner in which the exclusion of German science was lifted only occasioned fresh embitterment. Fritz Haber had for months tried vainly to negotiate a solution acceptable to both sides. Ultimately, only the discriminatory paragraph of exclusion was stricken, without ceding to Germany's wish for acknowledgement of its role as one of the leading scientific nations. The treatment accorded Germany was no different from countries such as Siam, Haber noted critically, while resigning himself to the fact that the time was not yet ripe for an equitable international organization of science.[12]

Sommerfeld shared his colleagues' embitterment. His opinion of the international research council was "not exactly flattering," he replied to Cabrera, for "the *Conseil*, born of the political hatred, costs a great deal of money, and has, so far as I am aware, no accomplishments to show for it." Sommerfeld thought the whole organization of this research council was misconceived. "Morocco, Egypt, Tunisia with independent representations! This is called democracy, but in reality it serves

9 To the Notgemeinschaft, May 1, 1928. DMA, NL 89, 020, folder 6,6.

10 To the University of Munich, March 2, 1928. UAM, E-II-N.

11 Von Cabrera, 7. August 1928. DMA, NL 89, 006. Also in ASWB II.

12 Szöllösi-Janze, *Fritz Haber*, 1998, pp. 588–590; generally on this topic, see Forman, *Internationalism*, 1973; Schröder-Gudehus, *Wissenschaftsbeziehungen*, 1990.

no other purpose than to support mad French claims." He thought the best course would be revival of the International Association of Academies, although it was clear to him that this was not to be realized. "It may be that for the sake of international courtesy, Germany will feel obligated to join the *Conseil*," he conceded, but on precondition that "the inane insults to German science" cease. "I write this in a great hurry shortly before my departure, and in my own name only," he wrote, to avert the possibility Cabrera would construe his statement as an official German position on this much discussed and hotly debated situation among diplomatic circles of the Foreign Office and the negotiators from the various academies. But he also made it clear that he was not alone in this opinion. "I believe, however, that many of my colleagues feel similarly. That the current status must be altered is clear to everyone. I myself hope that this alteration may come about in a spirit of friendship and mutual trust, but—in light of its whole pre-history— the *Conseil de Recherches*—does not seem to me to offer a suitable means to this end."[13]

The *Conseil International de Recherches* was converted into the International Council of Scientific Unions in 1931 and gradually shed its character of an Entente organization in opposition to the Axis powers.[14] But in 1928, the boycott imposed on German science at the instigation of the *Conseil International de Recherches* following World War I remained in place in general consciousness. To be reminded of this just a few days before his world tour gave further impetus for Sommerfeld to make this trip a cultural mission with the aim of restoring the reputation of German science.

10.2 Impressions of India

Outfitted with clothing for the tropics and accompanied by his assistant Bechert, Sommerfeld began his world tour on August 21, 1928, in Genoa aboard a steamer bound for the Suez Canal. Atypically, on this journey he kept a journal. "August 23, early, the Aeolian Islands, Stromboli, very picturesque," he recorded as the ship passed Sicily. "27th, the Canal, fabulously interesting," he wrote on entering the Suez Canal. He was fascinated by the waves generated by the ship which, once free of the ship, moved on as though on their own. He was reminded of the story of the "horse at the Scottish canal by Reynolds or Kelvin," he wrote under the heading "waves in the Suez Canal" in the "scientific portion" of his journal. He was referring to a particular wave phenomenon that had fascinated British physicists of the nineteenth century, and that he intended to analyze theoretically in the future.[15] "Red

13 To Cabrera, August 11, 1928. DMA, NL 89, 001. Also in ASWB II.

14 Greenaway, *Science* International, 1996.

15 Journal of the world tour.

Fig. 25: Karl Bechert and Sommerfeld aboard ship en route to India (Courtesy: Deutsches Museum, Munich, Archive).

Sea, weathered well; will not dock at Aden; heat now bearable; humidity reduced," he telegraphed several days later to his wife.[16]

Bechert accompanied him as far as India and made final corrections to the *Wave Mechanical Supplement*, galleys of which had been taken along on the great journey.

The first stage led through the Gulf of Aden and the Arabian Sea to Ceylon (now Sri Lanka). It was so sultry on the Red Sea that the passengers could not endure their cabins through the night. "Silent, sleep-walking figures in bath-robes on deck," Sommerfeld wrote describing the scene. "I lay several hours on a deck-chair, managing quite well, naturally completely soaked with sweat. Amazing one doesn't come down with rheumatism."[17] This stretch of the trip was not inspiring of pleasant memories. The Red Sea seemed to him a "God forsaken corner" that cost one of the ship's cooks his life. "Heat stroke at 40° [C.] below-decks. Bechert sat with him last night because the medical aide was himself totally exhausted. Burial at sea

16 To Johanna, September 1, 1928.

17 To Johanna, September 5, 1928.

with chorale, speech by a young missionary, and prayers by the Captain under the German flag—very moving."[18]

Once disembarked at Colombo, they journeyed through northern Ceylon. On September 9, they embarked on a steamer for the short crossing to the Indian mainland, where they continued their journey by train. "During the journey, Bechert has calculated the essentials of the Zeeman Effect. The countryside is well built-up and irrigated," Sommerfeld noted about the trip through southern India.[19] The goal of this stage was Madras (now, Chennai), where he was to lecture at the university. He lodged "in the fine English house of the Principal of the College," he wrote home. Although he was "eaten up at the moment by mosquitos," he felt both "personally and professionally" very well.[20] Bechert added to the report of this first stop on their cultural mission with impressions common to all travelers in southern India: "What we have seen: enormous temple complexes, great dark halls, excessively decorated towers, priests, monks, dark- and light-colored, black-haired people, great poverty, friendliness and hospitality, palms, palms, red sand and blue-green fields, brightly colored birds, blue and gold-brown mountains, bananas, rice, deep dark blue sea, and flowering gardens." The professor is doing well, he reassured Sommerfeld's wife, though up to now, with the heat, they had not been terribly diligent. "When just lazing about is exhausting in this great heat, it's surely impossible to work, don't you agree?"[21] Two days later, Bechert began his return trip to Munich to fill in for Sommerfeld in one portion of the course-work for the coming winter semester.

From Madras, Sommerfeld traveled on by train into the interior of the country to Bangalore, the capital of Mysore State (now Karnataka). Here, his cultural mission included a lecture to the South Indian Science Association on German universities and students, among other topics. The Maharaja's representative invited him to a tea party and chatted with him about Goethe. His host was an English physicist with whom he immediately felt at home. "In the evening, I played with my host—an Englishman—some Beethoven violin sonatas, and sang Brahms's 'Feldeinsamkeit' with his wife. All with windows wide-open and lively participation of mosquitos." Thus he described these manifestations of German culture in far-off India. The next day, he awoke with a fever. "I felt pretty awful, as hot as on the Red Sea, and asked for the doctor, an English military physician. He admitted me to his hospital this morning, where I lie in a pleasant pavilion, open on 4 sides, and am given all sorts of medicines to swallow."[22]

The fever came and went repeatedly, so that he remained in the hospital for 10 days. Malaria was suspected but not confirmed. "Pretty weak, and in need of sleep," Sommerfeld wrote in his journal when finally he was able to return to the home of

18 Ibid.

19 Journal of the world tour.

20 To Johanna, September 12, 1928.

21 Bechert to Johanna Sommerfeld, September 13, 1928.

22 To Johanna, September 18, 1928.

his host.[23] He had been touchingly cared for, he reassured his wife in his next letter home. Nonetheless, the lost time was annoying. “I’ll have to cut my whole India program short.” In addition, there was now anxiety about developments in the Physics Department at Munich, for he had learned from letters from home that Willy Wien had died after a gallstone operation. Sommerfeld feared that Johannes Stark, with whom he had long been at odds, would be appointed Wien’s successor. “Call Schmauss and tell him that illness has delayed my letter, but that I will send it before departing here,” he wrote his wife. He asked her to convey to the Dean of the faculty, August Schmauß (1877–1954), his distress that decisions might be reached in Munich before receiving his recommendations.[24] But Heinrich Wieland (1877–1957), his colleague in chemistry at Munich, assured him that they would await his opinion. The names of James Franck, Walter Gerlach, Gustav Hertz (1887–1975), and Robert Wichard Pohl (1884–1976) had been placed on an initial, provisional appointment list. “Differing opinions regarding the ranking of the top two candidates could be discussed per telegram.” Stark was not being considered as a candidate by a single member of the faculty. “But we will bear in mind the danger that attaches to this name.”[25]

Because of his stay in the hospital, Sommerfeld set out only after a 2-week’s delay for the next stop along the way of his India trip. To arm himself for the long journey and the exertions attending it, he had “now also taken a boy,” he wrote his wife, who was worried about his welfare. “He is unbelievably attentive and proper, knows exactly where every article of my clothing is, sews on buttons for me, steers me to the dining car, and waits in my compartment until I’m there again.” Because of this, the long journey became for him the “pinnacle of comfort.” It was very hot, to be sure, but bearable “if one is motionless.” His “boy” was, incidentally, “a married man of 30-something.” He was paid “about 40 M for 4 weeks. Of course I pay for his III class ticket, but nothing for his board.”[26]

In Calcutta, Sommerfeld was received like a statesman. Raman, who had invited him for 3 weeks of guest lectures, greeted him by placing a floral-chain around his neck. The German Vice Consul also made an appearance at the railway station on Sommerfeld’s arrival. Sommerfeld was put up “extremely comfortably” at the German Consulate. “A huge hibiscus tree is blooming in my bed-room. At night, large glow-worms come flying in. Continuous medium-hot greenhouse air,” he described his new surroundings to his wife. “This afternoon another reception at the Residency College. In the evening, dinner at the German Embassy for 8 guests.” The University of Calcutta bestowed an honorary doctorate on him, and three Indian scientific organizations, the Mathematical Society Calcutta, the Indian Association for the Cultivation of Sciences, and the Indian Academy of Science, inducted him as an honorary member.[27]

23 Journal of the world tour.

24 To Johanna, September 27, 1928.

25 From Wieland, September 10, 1928. DMA, NL 89, 019, folder 5,10. Also in ASWB II.

26 To Johanna, October 3, 1928.

27 To Johanna, October 10, 1928.

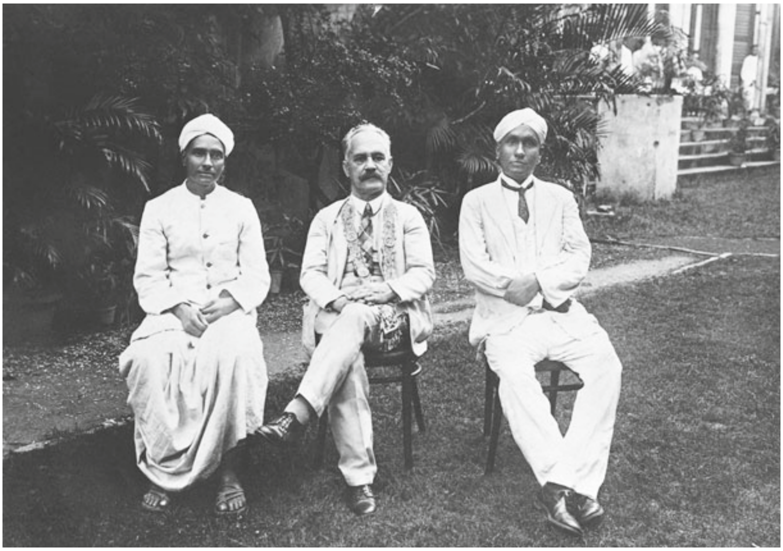

Fig. 26: In Calcutta, Sommerfeld was the guest of the discoverers of the "Raman Effect," K. S. Krishnan (*left*) and C. V. Raman (*right*) (Courtesy: Deutsches Museum, Munich, Archive).

As a physicist, too, Sommerfeld enjoyed his stay in Calcutta. "My book—the English edition that is—is known in the remotest corners of the country," he wrote, delighted over the familiarity of the Indian physicists with his work.[28] Over and above this, in Calcutta, he witnessed history-making experiments. "At the Institute, saw scattering, blue-green, in a block of ice," he inscribed in his journal following a visit to Raman's laboratory. The "Raman Effect," as it was soon to be named, denotes the scattering of light onto atoms and molecules, whereby the incident light is scattered back at a lower frequency specific to the scattering material. It had been discovered only a few months earlier. "Promise indirectly to propose Raman for the Nobel Prize," Sommerfeld noted to himself.[29] He told a reporter from the Indian newspaper *The Statesman* he felt privileged to be present at these latest experiments of Raman's, and he hoped to be able to make some contribution to the theoretical elucidation of this scattering effect. He characterized the effect as one of the most interesting discoveries of recent years.[30] Two years later, Raman was awarded the Nobel Prize for this work.[31]

From Calcutta, Sommerfeld visited other cities in northern India. On October 15, he was in Benares (now Varanasi), the religious center of Hinduism on the Ganges, to give a lecture at the Hindu University. The Chancellor of the University,

28 To Johanna, October 3, 1928.

29 Journal of the world tour.

30 Cited in Singh, *Arnold Sommerfeld*, 2001, p. 1491; Torkar, *Meeting*, 1986.

31 Singh/Riess, *Seventy Years*, 1998.

"a friend of Gandhi, strict Brahman," invited him on a river cruise on the Ganges and conversed with him "on Goethe, Haeckel, Spinoza, matter, and spirit."[32] The following day he inspected Sarnath, 10 km to the north, a historic city of early Buddhism. "Countless monastic cells, each with an image of Buddha and a small stupa," Sommerfeld wrote in his journal. It brought to mind Pompeii.[33]

He took the occasion of his lectures and talks at the various Indian universities and colleges to discuss the political situation with professors and students. "Everywhere, much sympathy for Germany. Admiration for our speedy reconstruction. All would like to study in Germany, but only if they have been to Cambridge can they find academic positions," he wrote, in criticism of the colonial dependency on England. "Indians unanimous in condemnation of the current system and in the demand for a position of respect within the British Empire."[34]

He experienced a particular insight into Indian-Bengal culture in his encounter with Rabindranath Tagore (1861–1941), resident in Santi-Niketan (now Shantiniketan) as spiritual head of a small scholarly and artistic community. Tagore had met Sommerfeld previously on a visit to Munich and was pleased now to be able to offer the professor from Germany the experience of "an Indian autumn's tranquility."[35] "Here, total stillness prevails around the 'poet,' as he is generally known," Sommerfeld enthused over his visit to Santi-Niketan. "Tagore is incredibly diligent in all aspects, as poet, musician, philosopher, and organizer of Indian education." His role in the cultural development of India could scarcely be overestimated. Tagore had "thrown their 'Sir' back in the faces of the English" and was striving for a "restoration of the decaying village life," though not like Mahatma Gandhi (1869–1948), whose politics of "non-cooperation" he rejected. Sommerfeld compared Tagore to Goethe, primarily because of his influence on the intellectual upper strata of Indian society.

Sommerfeld had actually wanted to visit Delhi, too, but abstained from the trip to the Indian capital which Raman had characterized for him as follows: "You will find there the monuments of many big empires now destroyed and the monuments of one more big empire not yet destroyed." Sommerfeld quoted this sarcastic description in a letter to his wife to illustrate the anti-British sentiment he constantly encountered. "Condemnation of the current governing methods of the British is universal among Indian professors."[36]

In light of this sentiment on the part of his host, it was not surprising that Sommerfeld was "under surveillance by the secret police," as the German Vice Consul warned him. He had noticed no sign of this, however, he noted in his journal under the heading "Political Items from Calcutta." From his many political

32 To Johanna, October 18, 1928.

33 Journal of the world tour.

34 Ibid.

35 From Tagore, October 15, 1928. DMA, NL 89, 024, folder Indien.

36 To Johanna, October 22, 1928.

discussions with his hosts, he concluded that most Indians desired independence—not through separation from England, however, but in the sense of self-governance, as had earlier been granted the "dominions" of the British Empire. Currently, India was obliged to import everything from England, "from matches to locomotives." There was only one technical research institution (in Bangalore). The criticism was widespread that not enough was spent on education. "Everything else seems peripheral. Great respect for the guru (teacher)."[37]

Such journal entries make it clear that in his cultural mission, Sommerfeld was no ivory-tower scholar, blithely singing the praises of German science, but oblivious to the sociopolitical situation in his host country. He registered very precisely the wants and the needs of his hosts and was open to instruction wherever the opportunity presented itself.

10.3 German Science at Chinese Outposts

On October 26 in Calcutta, after a 6-week stay in India, Sommerfeld once more boarded a steamer, bound this time for Rangoon (now Yangon) in Burma (now Myanmar). After a tranquil, 3-day ship's passage, a similar round of lectures and sightseeing awaited him. "Today, tea-party with various addresses, to which I naturally have to answer," he wrote to his wife after his arrival. "Early tomorrow, excursion to Pegu; in the evening, popular lecture: German and Indian universities; day after tomorrow lecture on spectral lines. In between, visits to institutes, hospitals, etc."[38] Actually, he would gladly have lodged aboard ship during the 3 days of his Rangoon stay, going ashore only to fulfill his lecturing obligations. But his English hosts would not forego putting him up in their home and spoiling him with all the comforts they were privileged with as colonial masters. In contrast, the overwhelming majority of the Burmese population lived in extreme poverty. A rickshaw driver earned "a few miserable rupees," Sommerfeld wrote, describing his impressions of Rangoon. "These drivers trot quite fast in the heat of the sun, and naturally die around the age of 30." He reported to his wife also that he had met a Buddhist monk, "born an Irishman, and previously a British officer! It is not unusual for the English to convert to Buddhism or Hinduism. It seems to be something in the air here."[39] From Rangoon, the journey proceeded to Penang and Singapore (now in Malaysia). Here, freed for a few days from lecture obligations, Sommerfeld could enjoy being a tourist, although this visit was not entirely private, either. He had been "often together with the German Consul General," he wrote home. He also met the American and French Consuls for dinner and lunch.[40]

37 Journal of the world tour.

38 To Johanna, October 29, 1928.

39 To Johanna, November 3, 1928.

40 To Johanna, November 12, 1928; Journal of the world tour.

His next destination was the Philippines. "One day out from Manila. I've been in bed the last two days; from lying on the top deck, I've picked up a disagreeable rheumatism and a bit of fever."[41] He wrote this to his wife during the passage across the South China Sea aboard the German steamship "Coblenz," 3 days after departing Singapore. By the time he reached Manila, however, he was fever-free again. He described the hotel in which he lodged as "very elegant, very expensive, very loud." From 1898 to 1941, the Philippines was a US colony, and Sommerfeld registered the contrast with the British colonies in his journal. Unlike India and Burma, one traveled through Manila "in a one-horse carriage . . . The Americans apparently do not tolerate the rickshaws drawn by humans, and have replaced them with nice pony-drawn vehicles."[42]

From Manila, the voyage continued across the South China Sea to Hong Kong, and from there along the Chinese coast north to Shanghai. "We have arrived: no more heat. We wear woolens. I'm also free of the fever, and slowly regain my appetite," Sommerfeld wrote by way of diagnosing his recovery from the trials of the tropics.[43] In Shanghai, lecturing duties awaited him once more. The first invitation came from the "Quest Society," a club for popular science enthusiasts who had asked to hear a lecture by Sommerfeld "on atomistics."[44] Another request had come from the Germanophone Tung-Chi University in Woosung near Shanghai. "In local German circles, your visit to Shanghai is eagerly anticipated," the German Consul General had written Sommerfeld. The Director of the Tung-Chi University had expressed the "wish for contact with you," and he conveyed this request "all the more since from the appearance of a prominent German scholar I anticipate a particularly lasting impression on the Chinese students, and may hope that thereby the German cultural influence on the Tung-Chi University will be valuably reinforced." This technical university, consisting of a medical and an engineering school, was "one of the most valuable German cultural efforts in China."[45] It was established in order "to assure Germany, the Germans, and the German spirit a commensurate role in influencing Chinese reform," as a German Consul General in Shanghai had formulated it following the Boxer Rebellion early in the twentieth century. Principally, the engineering school, opened in June 1914 under German direction, was intended to secure Germany a preferential position among competing European powers in the exploitation of the huge Chinese market. But the outcome of World War I had shattered these hopes. The Tung-Chi University passed to Chinese ownership, and the main thrust of German-Chinese relations was perforce relegated to the cultural realm. The University retained its German faculty and enjoyed the uninterrupted support of its—now Chinese—owners.[46]

41 An Johanna, November 15, 1928.
42 Journal of the world tour.
43 To Johanna, November 22, 1928.
44 From Herbert Chatley, August 16, 1928. DMA, NL 89, 019, folder 4,3.
45 From Fritz August Thiel, November 13, 1928. DMA, NL 89, 021, folder 9,6.
46 Bieg-Brentzel, *Tongji-Universität*, 1984; Steen, *Beziehungen*, 2006.

"First evening, lecture to the Quest Society; next evening, lecture at the Paulun Hospital; third day, visit to Tung-Chi University with address to the students beneath a picture of Sun Yat-sen, the current national hero, both latter addresses in German since the students of this university take their classes conducted in the German language."[47] Sommerfeld gave his wife this summary of his 3-day sojourn in Shanghai. In his journal, he registered once more what he had said in his address in "conclusion to the students": They were "privileged over millions of others in that they were being taught the best science by German instructors" and were thus "duty-bound to idealism."[48] By printing his address in both German and Chinese, the *Tung-Chi Medizinische Monatsschrift* (Tung-Chi Medical Monthly) was responsible for extending the effect of his mission to this "furthest outpost of German science and culture" well beyond the term of his visit.[49]

While Sommerfeld was carrying out his cultural mission in China, the pending appointment of a successor to Wien took a turn that caused him some concern. Sommerfeld wanted to see Debye, Franck, and Gerlach placed equally in the top spot, Gustav Hertz in the second, and Ernst Back in the third spot. The candidate list drawn up by the appointment committee, however, ranked only Debye and Franck equally in the top spot; Gerlach and Hertz ranked second and third.[50] "If the Ministry gets a refusal from Debye and Franck, then it will be easier for an offer to be made to Stark, than if—as I wished—we had clearly placed a man in the first spot whom we would get, namely Gerlach," as he explained his fear to his wife. Johanna Sommerfeld acted the role of intermediary between her husband and the faculty in the matter of this appointment. Even Johannes Stark was aware that Sommerfeld's wife could exert some influence. But in his attempt to ease his strained relation to Sommerfeld through his wife, he suffered shipwreck. That Stark should exploit his absence "to wear down" his wife outraged Sommerfeld. He was all the more pleased to see his arch enemy sent packing. "I would really love to have seen you, coolly, politely, and oh so innocently, telling Giovanni Robusto to get lost."[51]

10.4 Birthday in Japan

Sommerfeld departed Shanghai on November 29, 1928, aboard the S.S. Nagasaki-Maru bound for Japan. After a tranquil passage across the East China Sea, the steamer arrived the next day in Nagasaki, where Sommerfeld was welcomed by a delegation of Japanese physics professors. Following a brief stay, his journey

47 To Johanna, December 1, 1928.

48 Journal of the world tour.

49 Sommerfeld, *Entwicklung*, 1929a.

50 From Wilkens, October 31, 1928, and November 15, 1928, with the draft of a reply from Sommerfeld, DMA, NL 89, 019, folder 5,10.

51 To Johanna, December 1, 1928.

continued to Kobe. From here, he traveled by train to Tokyo. "The Japanese really know how to make one's life comfortable," he wrote a few days later from Tokyo.[52] Here too, the guest from far-off Germany was treated with extraordinary attentiveness. Yoshikatsu Sugiura (1895–1960), an employee of the respected Physical-Chemical Research Institute (Rikagaku Kenkyujo, RIKEN), who had been a guest researcher from 1925 to 1927 at Copenhagen, accompanied him everywhere and paid his expenses "on orders from above." "I dubbed him my finance minister," Sommerfeld wrote his wife.[53] Sugiura and an "adjutant" anticipated his every wish. "That I should celebrate my birthday in Japan was seen as a token of special favor on my part towards Japan. They have, however, declared the 6th my birthday, and made it almost a national holiday."[54]

On account of this misunderstanding, December 5, 1928, the actual date of Sommerfeld's 60th birthday, ran its course relatively uneventfully. Apart from the congratulatory telegrams that arrived at his Tokyo hotel from Europe, this day was for him nearly a normal workday. To spare his hosts the embarrassment of last minute rescheduling for the fifth all the festivities planned for December 6, he did not correct the misunderstanding and delivered the first of several 2-h lectures at the Empirical University of Tokyo on "Fundamental Questions of Wave Mechanics" according to plan.[55]

The next day, accordingly, Sommerfeld was "surprised" with a wide-ranging birthday celebration. Following his lecture, invited guests, including the German ambassador in Tokyo, Wilhelm Heinrich Solf (1862–1936), adjourned to a reception at the Sanjo Palace of the University. The birthday dinner was served in traditional Japanese style, presided over by Count Masatoshi Okochi (1878–1952), Director of the RIKEN, who coincidentally on this day was celebrating his own 50th birthday. "At dinner, Germans and Japanese guests were seated in alternation," Sommerfeld described the event to his wife. "Shoes off, of course, cushions in place of chairs, straw mats on the floor, chopsticks in place of knife and fork. I had already practiced with these, and proudly declined knife and fork. I only asked to have my cushion raised a bit, because I can no longer fold my legs under me comfortably. In front of us, cute little Japanese serving girls sit (or rather crouch) on the straw mat, chatter superficially with the guests, and bring the innumerable dishes, all of them served individually in lacquered bowls. High point of the affair: dance of two geishas, high art, extremely graceful, dance or theater, as you will. Of course speeches by Okochi and me."[56]

52 To Johanna, December 4, 1928; Ozawa, *Aufenthalt*, 2005.

53 To Johanna, December 24, 1928.

54 To Johanna, December 4, 1928.

55 Ozawa, *Aufenthalt*, 2005, p. 51. I gratefully acknowledge Michiyo Nakane for her transmission of Sugiura's Japanese translation of the texts of these lectures.

56 To Johanna, December 24, 1928.

After weathering the birthday festivities and one more lecture at the University of Tokyo, on December 8, 1928, Sommerfeld was guest at an event of the Japanese-German Cultural Society. He again delivered the popularizing lecture "On the Development of Atomic Physics in the Last Two Decades," which he had already given at Tung-Chi University, and here too met with great interest.[57] He had used "Ernst's observation that I went abroad as a German cultural ambassador" as an introduction, he wrote home.[58] This lecture remained a pleasant memory also because of an observation of his translator, who on this occasion had compared Bohr with Copernicus, and Sommerfeld with Kepler.[59]

It may be that in light of this comparison, Sommerfeld was reminded that he, in contrast to Bohr, had not been honored with the Nobel Prize. Three days before, on his birthday, he had confided to his journal: "Read letters and verses from home, sadly also notice about Nobel Prize."[60] Several days later he wrote his Munich colleague Heinrich Wieland, who that year had received the Nobel Prize in chemistry: "Hail and conquer! I congratulate your dear wife also on her famous husband. According to everything I know about you, I am persuaded the choice was well deserved. But to dispel all suspicion of false modesty, I must simultaneously note that it is gradually becoming a public scandal that I have still not received the Prize." In India, he had heard rumors that Bohr, "out of rivalry," was blocking the award of the Prize. He knew nothing about any such machinations, but he had already several times been on the short list. "Once, the Stockholm press had actually asked for my picture. In any case, it would have been the only right and proper thing, after Bohr received the Prize in 1922, for it to be given to me in 1923. The Royal Society, for example, made Bohr and me Fellows at the same time, as was fitting. So much for unburdening my heart, and for the sake of truth."[61]

But Sommerfeld had no time to sink into depression over the withheld Nobel Prize. His Japanese lectures were being eagerly awaited, and Sommerfeld took great pains not to disappoint these high expectations. At Kyoto, among his audience were the future Nobel laureates Shin-ichiro Tomonaga (1906–1979) and Hideki Yukawa (1907–1981), third year physics students, who preserved these lectures in memory as "unforgettable and superb."[62] In Kyoto, Sommerfeld also repeated his popularizing lecture "On the Development of Atomic Physics in the Last Two Decades." Tomonaga recalled that Sommerfeld spoke on this occasion also about the energy levels in hydrogen that could take up an electron. "Then the following happened: as he explained this, he ran around the podium. But because he was going backwards, he did not see the edge, and fell off. My teacher, Professor Tamaki,

57 Sommerfeld, Entwicklung, 1929b.
58 To Johanna, December 21, 1928.
59 Ozawa, *Aufenthalt*, 2005, p. 52.
60 Journal of the world tour.
61 To Wieland, December 13, 1928. DMA, NL 57.
62 Cited in Ozawa, *Aufenthalt*, 2005, p. 55.

Fig. 27: On an excursion to Hakone, Hantaro Nagaoka (1865–1950), the patriarch of Japanese physics, introduced Sommerfeld to the natural beauty of the surroundings of Tokyo. In this nature preserve, volcanic activity is everywhere in evidence (Courtesy: Deutsches Museum, Munich, Archive).

who was sitting in the first row, quickly picked him up. Then Sommerfeld, without much ado, went right on: 'Exactly as I just now fell down, the electrons, too, fall down from here to there.' I remember that he got the people laughing in the audience on his side."[63] Sommerfeld recorded in his journal simply, "Lecture to a big audience. English. Very good and very popular."[64] In Kyoto, he went to see the temples and the Imperial grounds, so that his stay in this city was a special experience of Japanese culture.

63 Ibid.

64 Journal of the world tour.

On December 17, 1928, Sommerfeld returned to Tokyo to observe experiments at the RIKEN several days before embarking on the ship's long passage across the Pacific to America, and especially to meet Nagaoka, patriarch of Japanese physics, who had returned early from a trip to Europe in order to greet Sommerfeld in his homeland. On the last day of his 3-week sojourn in Japan, Nagaoka accompanied him to Hakone, a locale in the foothills of Mt. Fuji, where he was able to get a final, lasting impression of the volcanic nature of Japan ("sulfurous air through the gorge of fumaroles, witches' kitchen, with hot-springs").[65] On parting, Nagaoka gave him the gift of an artfully decorated, bamboo walking-stick, "carved with a rat's tooth (!), depicting 100 Japanese faces, truly a work of art, signed by the artist," as Sommerfeld wrote his wife in his last letter from Japan. He felt "great reverence for the ancient history and culture of the country."[66]

10.5 Visiting Professor in Pasadena

Sommerfeld passed the Christmas Season and the New Year aboard a Japanese steamship from Yokohama bound via Honolulu for the U.S. west coast. "Now there is a Christmas tree (with electric lights) in the dining room, and a maple tree (artificial leaves with cotton snow), decorated with cherry blossoms, Japanese paints, etc." he wrote home about the unusual circumstances of his Christmas observance. The passage was stormy, "the entire sea grey and white with spray; it's barely possible to write. Many are seasick; not I."[67] He recorded in his journal that he spent most of his time in letter-writing ("20 letters and numerous postcards").[68] Aside from his correspondence, he penned a longer article on his impressions of India for a Munich art journal.[69] Regarding his stay in Honolulu, where he went ashore for a few hours, he had little to report: "Hawaiian girls dancing, to the accompaniment of fatsoes."[70]

Although a 3-month stay in the USA still lay ahead, he experienced the crossing of the Pacific as the first leg of his return home. "The Japanese haven't trisected the master," he wrote musing on the previous weeks. He reviewed with amusement several situations that had befallen him among the many honors bestowed on him in Asia. At his being named an honorary member, an Indian mathematician had, "in grim earnest," analyzed his mathematical papers so conscientiously that he had said in rejoinder, "I can't know how a frog feels during its own vivisection.

65 Ibid.
66 To Johanna, December 24, 1928.
67 To Johanna, December 25, 1928.
68 Journal of the world tour.
69 Sommerfeld, *Reiseeindrücke*, 1929.
70 Journal of the world tour.

But I must say I felt quite alright during this friendly vivisection."[71] He was constantly asked for autographs with maxims such as "what is most important for research." In such situations he had delivered himself of bits of wisdom like "Onward and upward" or "Integral p dq = n h."[72]

With his arrival in San Francisco, that portion of Sommerfeld's world tour during which he felt his role was as a scientific missionary came to a close. Six years earlier on his first visit, the American physics profession had already rendered him great respect. The California Institute of Technology in Pasadena, where he would spend the next 2 months as visiting professor and meet his former students Epstein, Pauling, and Houston as colleagues, was on the way to becoming a center of modern physics that had no cause to shy away from comparison with European universities. More than anywhere else in America, Sommerfeld felt at home here, and he was immediately reinforced in this feeling by his hosts. "Today at noon I will be with the Millikans, in the evening, with the Paulings," he wrote home shortly after his arrival in Pasadena. "Have also already been with the Houstons; last evening to the theater on invitation from Epstein." In addition, he was staying in an idyllic apartment at the Faculty Club, "with a view of palms and fruit-bearing orange trees, and in the back, a view of the blue mountains."[73]

Also, his duties as visiting professor had more in common with his familiar teaching regime in Munich than with his function as cultural ambassador in Asia. His teaching load comprised four 1-h lectures weekly and participation in the colloquia. The subject of his lectures corresponded broadly to what he had written in the just published *Wave Mechanical Supplement*, so that little preparation was required. "Here in California, life is made easy for one in every respect," he wrote Rubinowicz. "To be sure, I have not only my lecture courses, but have also to speak at all sorts of meetings, in English of course."[74] On the social level too, Pasadena had something to offer. "Yesterday there was a faculty dance. Quite nice and easy," he wrote home 2 weeks after his arrival. "I even danced, in spite of the jazz music. Last week, I heard very good music, string quintet, at the home of a friend of Epstein's, a professional violinist; I'll go again next week. A week ago I played with Pauling's trio. I had to speak at a society lunch about India and Japan, ½ h. Also, a colloquium lecture in addition to the usual lectures. Next week I have to speak to a similar society in Los Angeles. But it is good that each day I have several quiet hours to myself to gather my thoughts –not as it was in India and Japan, and will be to a greater extent in America after March 15." This latter reference was to the numerous lecture invitations for the last weeks of his U.S. visit that he had received in Pasadena, and that would require careful travel planning for the period following his visiting professorship. He also had to devote not a few of his quiet hours to

71 To Johanna, December 25, 1928.

72 To Margarethe, undated [around December 27, 1928].

73 To Johanna, January 13, 1929.

74 To Rubinowicz, January 15, 1929.

composing letters of thanks for the numerous birthday greetings and especially for the festschrift for which thirty of his students had in his honor written articles on "Problems of Modern Atomic Physics."[75]

In the weeks of his Pasadena stay, the question who would be his new experimental physics colleague on his return to Munich was also resolved. Debye, placed first on the appointment list together with Franck, had withdrawn his candidacy, since he had only shortly before been appointed at Leipzig. An appointment at Munich so soon after taking up his post at Leipzig would have clashed with the understanding among the Ministries of Culture of the various states according to which there was to be no recruitment within 2 years of an academic chair's being taken up.[76] Thereupon, the offer had fallen to Franck, who placed conditions regarding the improvement of the Munich Institute's outfitting that the Bavarian Ministry of Culture would not accept.[77] "The offer has gone to Franck, as I hear," Sommerfeld wrote his wife in Munich at the end of January. "I greatly value him as a colleague. But Gerlach would have been better."[78] Sommerfeld feared that in the end, Stark would after all come under consideration and asked Franck to accept the appointment. But Franck did not feel he could do that, as he explained to Sommerfeld in a long letter, since the Ministry was unprepared to meet his demands.[79]

At the same time, Stark complained that Sommerfeld had been blocking his appointment as Wien's successor. He had been informed by a person he declined to name, "that you are the ultimate and decisive author of the candidate list for appointment to the Wien chair. Since this list excludes me, it is tantamount to an official discrediting of my person and my scientific achievements. You must understand that I will defend myself against this discrediting, and intend to make public my viewpoint on the scientific grounds you have adduced."[80]

Sommerfeld replied coolly to Stark that he could not respond to anonymous innuendoes, and that the appointment list had not at all been drawn up on the basis of his recommendations.[81] "I don't foresee any good ending here," he wrote his wife. "Ultimately, we'll have to go to Berlin or to America after all."[82] His mood soon improved again, however, when the Dean informed him per telegram that the appointment had now gone to Gerlach. He wrote his wife that he had immediately telegraphed Gerlach: "Accept unconditionally." And Stark could "Go jump in the lake with his polemical threats."[83] Gerlach did not accept the appointment right away, however, but went first to the Ministry to negotiate further. This was "pure

75 To Johanna, January 20, 1929. Debye, *Probleme*, 1929.
76 From Debye, 21. December 21, 1928. DMA, HS 1977-28/A,61. Also in ASWB II.
77 From Wieland, January 19, 1929. DMA, NL 89, 019, folder 5,10.
78 To Johanna, January 27, 1929.
79 From Franck, February 5, 1929. DMA, NL 89, 019, folder 5,10.
80 Form Stark, January 30, 1929. DMA, NL 89, 019, folder 5,10. Also in ASWB II.
81 To Stark, February 18, 1929. DMA, NL 89, 019, folder 5,10. Also in ASWB II.
82 To Johanna, February 17, 1929.
83 To Johanna, March 3, 1929.

Fig. 28: Together with Pauling and his family, Sommerfeld visited the "Painted Canyon" (in the picture, Pauling's wife and "little Linus") (Courtesy: Deutsches Museum, Munich, Archive).

theater," Johanna wrote her husband in Pasadena after Gerlach informed her of it. All the same, Gerlach had given her the impression that he would accept the appointment in the end. "Then you can exhale. But his appointment is to begin only in October."[84]

But the back and forth of the Munich appointment question was not enough to seriously dampen Sommerfeld's sense of well-being in Pasadena. On one weekend, the Pauling family took him along on an excursion to the desert. They slept under the stars in the "Painted Canyon."

"It was warm in the sleeping bag, in spite of the night's being quite cool. In the morning, we climbed around a bit, little Linus mostly on big Linus's back. Long car trip back through endless orange and lemon groves, blue mountains, snow-capped in part, up to 3,600 m. high, well-tended villages, wonderful roads. All of it very pleasant."[85]

84 From Johanna, March 21, 1929.

85 To Johanna, February 10, 1929.

To the "cordiality of Pasadena life" belonged also his regular weekly meetings following dinner at the Faculty Club for a game of bridge with the Swiss astrophysicist Fritz Zwicky (1898–1974), who had come to Cal Tech in 1925. These evenings were reminiscent of his childhood days in Königsberg when he had played whist at home "with father, Aunt Minchen, and Ochen, who always played incorrectly, which invariably annoyed father." It also became a pleasant custom to be picked up by a violin virtuoso for musical evenings. "In the course of my time here, we have played through all the Beethoven and Schubert violin sonatas. I have also often played piano at the social gatherings I'm frequently invited to."[86]

Scientifically, too, he felt thoroughly at home. His lectures on wave mechanics met with great approval. "The students here (mostly older, and very sensible) are already beginning to turn to me with their troubles and their discoveries," he wrote his wife after the first two weeks of lectures.[87] He had Vieweg Publishers send 20 copies of the *Wave Mechanical Supplement* to Pasadena, a number, however, that was insufficient to meet the demand of his audiences.[88] He was most pleased by Schrödinger's reaction to his book, which reached him at Pasadena: "What you have done here is once more—like the main volume—something only you could accomplish. You are the master builder, creating a whole for which the rest of us merely supply the building blocks, often enough so crudely hewn that you must chisel them skillfully, when you don't actually prefer setting a stone of your own in the place where the one supplied won't fit."[89] This reaction from the architect of wave mechanics quickly took the sting out the letter he had received the same day from Stark threatening a continuing and fruitless argument. "My joy over this was greater than my annoyance over Stark," he wrote home.[90]

The great demand for the *Wave Mechanical Supplement* also quickly engendered the desire for a translation into English. At Cal Tech alone, students ordered 60 copies of the German edition.[91] "My Wave Mechanics is already supposed to be translated into English," Sommerfeld wrote with pleasure. Additionally, he had "been calculating diligently."[92] He was referring to the problem of explaining quantum mechanically the generation of X-rays at the braking of electrons. He had first taken up this question on the passage from Japan to America and therefore referred to it as his "Pacific problem."[93]

86 To Johanna, February 24, 1929.
87 To Johanna, January 20, 1929.
88 To Johanna, February 3, 1929.
89 From Schrödinger, January 29, 1929. DMA, HS 1977-28/A,314. Also in Meyenn, *Entdeckung*, pp. 462–464.
90 To Johanna, February 17, 1929.
91 To Johanna, February 3 and March 10, 1929.
92 To Johanna, February 10, 1929.
93 To Johanna, January 27 and February 3, 1929.

10.6 The Second American Tour

In the last 6 weeks of his U.S. visit, Sommerfeld was much in mind of his experiences in 1923 when at the conclusion of his Karl-Schurz Professorship at Madison, he had crossed this enormous country from coast to coast, delivering lectures here and there along the way. "Yesterday, the Grand Canyon during the day, for the second time in my life," he wrote his wife on March 15 en route of his 4-day train trip on the "California Limited," which brought him to Chicago at the conclusion of his visiting professorship in California. Taking leave of Pasadena, as at the end of his visit 6 years earlier, was not easy for him. "The boys," as he called the students in his lecture courses, had arranged a big party in his honor at which they bade farewell with an original theater-piece, two Beethoven trios, a Grieg sonata, Chinese music, and all sorts of culinary delicacies. "Everyone was royally entertained in the broadly informal atmosphere."[94] One day later, "near the Missouri," he wrote to a colleague, "You can just imagine how interesting India and Japan were, and how attentively everyone has provided me the best and most comfortable their countries have to offer. But this last impression is the greatest: southern California with its natural beauty and amazing progress is remarkable, and the people there are marked with an unusual measure of optimism, cheerfulness, sociability. The competence of the kind of people who have settled there preclude their degenerating into hedonism."[95]

In Chicago, Arthur Holly Compton and Carl Eckart prepared a cordial welcome for him. "The Comptons were charming to me, took me to hear a good quartet, and also arranged for music at home," he wrote his wife.[96] Sommerfeld did not mention the "German hater" Michelson, who had disinvited him on his first visit to the USA, and this time too had acknowledged him only with a brief perfunctory greeting appended to a letter from Compton.[97] During his visit of only 4 days, Sommerfeld also nearly missed seeing Heisenberg, who delivered guest lectures shortly after his at the University of Chicago. Although he traveled in the opposite direction, Heisenberg, like Sommerfeld, used the occasion of this invitation to make a world tour, whose overture were the Chicago lectures that began in April, 1929. Scientifically, too, Heisenberg stressed different things. While Sommerfeld touted the advantages of Schrödinger's method in his Pasadena lectures, Heisenberg sought to spread the "spirit of Copenhagen."[98]

From Chicago, Sommerfeld's journey continued to Ann Arbor, where Laporte, newly minted as a professor in the physics department, welcomed him, then on to

94 To Johanna, March 15, 1929.

95 To Grimm, March 16, 1929. DMA, HS 1978-12B/172.

96 To Johanna, March 23, 1929.

97 From A. H. Compton, May 4, 1928. DMA, HS 1977-28/A,54.

98 Rechenberg, *Werner Heisenberg*, 2010, p. 631; Heisenberg, *Prinzipien*, 1930, pp. V–VI; from Heisenberg, March 28, 1929. DMA, HS 1977-28/A,136.

Madison, where the many invitations from old friends and acquaintances precluded much rest.[99] After a brief stop in Columbus, Ohio and Pittsburgh, Pennsylvania, the next destination was Philadelphia. "From the 9th floor of a fine hotel in the large and fine city of Philadelphia," he began his next weekly letter to his wife on the stationary of the "Bellevue Stratford." "This is actually the preferable form of hospitality. One is put up in a hotel, has no obligations, signs the check at breakfast and other meals, and leaves everything else to one's hosts. It was the same at the Athletic Club in Pittsburgh. In Columbus, Ohio, I was actually on the 14th floor, and the spring wind whistled around the windows at night as at Sudelfeld." His host in Philadelphia was the Director of the Bartol Research Foundation of the Franklin Institute, who also enjoyed some renown as a cellist, and invited Sommerfeld to musical evenings in his home. An evening spent at a concert by the Philadelphia Orchestra, world famous at that time under the direction of Leopold Stokowski (1882–1977), made for an air of relaxation. "Philadelphia is almost peaceful."[100]

The last portion of the trip was hectic again. In New York, he had to settle the taxes due on honoraria he had received for the lectures delivered in the USA, book passage for his return to Europe at the offices of North German Lloyd, and deliver a lecture at Bell Laboratories. Then he attended a conference in Tuxedo Park, NY, at the invitation of the legendary American physicist, banker, and patron of science Alfred Lee Loomis (1887–1975). "Mr. Loomis, who has hosted 30 overnight guests and 110 others who came today for the lectures, is the American Anschütz," Sommerfeld wrote, comparing Loomis to the German inventor of the gyroscopic compass, and patron of the arts and sciences, Hermann Anschütz-Kämpfe (1872–1931). The laboratory of this "American Anschütz" in Tuxedo Park achieved legendary status for the development there of microwave radar during World War II. Already in the 1920s, though, a particular aura surrounded Loomis. "He is a man of Wall Street and a physicist, by preference," Sommerfeld thought.[101]

Both his visit to Tuxedo Park and his subsequent stay in Washington, where his lecture to the National Academy of Sciences formed as it were his official scientific farewell performance in the USA, were very pleasant experiences for him. He was not dealing with a lay audience here but with the elite of American physics. He had been occupied with his "Pacific problem" just at the time these invitations to Tuxedo Park and Washington had reached him in California. Thus, he had proposed "production of X-rays according to wave mechanics" as his lecture topic, thereby putting the pressure on himself to work up his provisional calculations of the X-ray bremsstrahlung into a demonstrable theory.[102] In Washington,

99 To Johanna, March 29, 1929.

100 To Johanna, April 7, 1929.

101 To Johanna, April 15, 1929. Conant, *Tuxedo Park*, 2002; on Anschütz-Kämpfe, see Broelmann, *Intuition*, Chap. 4.3.

102 From Loomis, with the draft of a reply from Sommerfeld, February 11, 1929. DMA, NL 89, 019, folder 4,4.

this was also his lecture topic. He regarded it "as the fruits of my Pacific and Californian muse."[103]

Therewith, Sommerfeld's "Pacific problem" garnered some notice even before he had published anything about it. Helmuth Kulenkampff (1885–1971), an experimental physicist at the Technical University of Munich and regular participant in the Sommerfeld colloquium, had provided the initial impetus. Shortly before the beginning of Sommerfeld's world tour, Kulenkampff had carried out a string of precise measurements of the distribution of directions of X-ray bremsstrahlung, in which he had used extremely thin aluminum foil as anticathodes, in order to eliminate the secondary effects (diffusion and multiple scattering) that arise with normal anticathodes. With these measurements, he had confirmed that Sommerfeld's 1909 derivation for the classical radiation of a straight-line braked electron was in its essence correct.[104] Now, the challenge that presented itself to Sommerfeld was to elaborate the theory such that it confirmed the earlier results according to quantum mechanics as well.[105]

At first glance, it might not seem that Kulenkampff's experiments and Sommerfeld's efforts at explanation constituted a very great challenge. If the classical theory was already capable of describing the bremsstrahlung in these experiments well, what need was there for a quantum mechanical explanation? In truth, though, more was at stake than just a newer derivation of a classical theory. Treating the process of absorption, emission, and scattering of electromagnetic radiation quantum theoretically presented great difficulties. Already before quantum mechanics, much effort had gone into reinterpreting X-ray bremsstrahlung quantum theoretically.[106] "Beginning work on bremsstrahlung, in light of discussions with Sugiura," Sommerfeld had written about his "Pacific problem" in his journal one morning after a hot bath, shortly before the ship's docking at Honolulu. "Looks promising but complicated."[107]

In the earlier quantum theory, the problem had been to calculate the energy loss of an electron that was first approaching an atom on an energy-rich hyperbolic course and then moving away from it on an energy-poorer one. The energy difference corresponded to the energy emitted by the X-ray bremsstrahlung. Wave mechanically, the incident electrons could be pictured as a smooth wave that is scattered onto the atom. The decisive magnitude of the electromagnetic wave radiated in this scattering process, Sommerfeld argued, was that of the "matrix element" corresponding to the electrical dipole moment that has to be calculated from the product of the amplitudes of the incident and reflected electron waves,

103 To Johanna, March 10, 1929.

104 Kulenkampff, *Untersuchungen*, 1928, p. 629.

105 Sommerfeld, *Production*, 1929a and 1929b.

106 Kramers, *Theory of X-ray Absorption*, 1923; Wentzel, *Quantentheorie des Röntgenbremsspektrums*, 1924.

107 Journal of the world tour.

multiplied by the distance from the scattering center and integrated over the whole space. That, at any rate, was how he presented his "Pacific problem" to the National Academy of Sciences. Although he could reinforce his basic idea with several calculations, he left out the complete mathematical implementation. "I must leave that for a fuller paper to be published later in the *Annalen der Physik*."[108] Only in this paper—which, be it said, did not appear for another 2 years—did the whole complexity of the "Pacific problem" manifest itself.[109]

Sommerfeld was already onboard the steamer in New York for the return passage to Germany when a telegram reached him from Washington announcing his election as a nonresident member of the National Academy of Sciences.[110] His US colleagues, chief among them Millikan as incumbent Secretary of the Academy, could not have made Sommerfeld a more wonderful parting gift.[111] From Tokyo he also received a token of highest esteem. "Your visit to Japan marks an event in the history of the development of mathematical physics in Japan," Nagaoka wrote him in flowery language. Sommerfeld's lectures had "had no doubt an effect of balmy dew falling on the tender leaves beginning to sprout."[112]

In the light of so many tributes, the exertions of his world tour receded into the background. At home once more, Sommerfeld set himself the task of accepting every opportunity that arose to report on his travel experiences. Above all, he showed himself to have been deeply impressed with India.[113] At a meeting of the Bavarian District Association of the German Physical Society, he praised "the great scientific activity prevailing in India, particularly in the school of C. V. Raman in Calcutta." In Japan also he had seen impressive examples of physical research. He showed his assembled colleagues photographs with "Kikuchi lines," a diffraction pattern obtained through multiple scattering of electrons in crystals, which Seishi Kikuchi (1902–1974) had discovered shortly before his visit at the RIKEN in Tokyo.[114] He reported on his world tour also to the Bavarian Academy of Sciences, and to the "Casual Ones," a tradition-rich Munich society of scholars and artists which had admitted Sommerfeld to its ranks 3 years earlier.[115] "What was most edifying to those of us present, and made us truly proud," the historian of the

108 Sommerfeld, Production, 1929b.

109 Sommerfeld, *Beugung*, 1931.

110 From Millikan, April 24, 1929. DMA, NL 89, 020, folder 6,3.

111 To Millikan, April 25, 1929. Millikan Papers, Pasadena, Archives of the California Institute of Technology, 42.17.

112 From Nagaoka, May 3, 1929. DMA, NL 89, 019, folder 4,3. Also in ASWB II. The letter continues, "We are ever anxious to reap rich harvest of science in the Far East by tightening the band of connexion between the scientific circles of Germany and Japan in course of time. I must call for your help in fulfilling this ardent desire."

113 Sommerfeld, *Reiseeindrücke*, 1929.

114 Sommerfeld, *Physik in Japan*, 1929.

115 Sommerfeld, Bericht, 1929; Rohmer, *Zwanglose Gesellschaft*, 1937.

"Casual Ones" noted, "was the highly distinguished reception accorded Counselor Sommerfeld as a representative of German research, which is certainly not accessible to any random number of people in that far-flung region of the globe, which all too readily we had imagined as scientifically backward."[116]

10.7 Critique of Positivism

Sooner or later, the "Globetrotter," as the Casual Ones had dubbed him, needed nevertheless to reacclimate himself to daily life at home. His greatest concern was over the still orphaned Wien Institute. Gerlach's appointment appeared a done deal, but so long as this position remained unoccupied and Gerlach was still in Tübingen, Sommerfeld feared that ultimately Johannes Stark would after all be made the offer. Only when in June 1929 he received an inquiry from Tübingen whether he could recommend Stark as successor to Gerlach there was the situation clear. "The light and dark sides of Johannes Stark are generally known," he wrote to Tübingen. "Since I fought strenuously against his candidacy for Munich, just as strenuously as he sought to push it through, it would be inappropriate for me to recommend him to Tübingen."[117] Debye was pleased along with Sommerfeld "over having averted the great danger."[118]

Thereafter, Sommerfeld was once more at peace and able to concentrate on his subject. With a talk at the German Physicists' Day in Prague he showed that, for all his predilection for concrete problems, he was not indifferent to questions of fundamental principles raised by quantum mechanics. The trend towards the fundamental was evoked by the physicists of the "Vienna Circle," primarily by the theoretical physicist Philipp Frank (1884–1966), teacher at the German University at Prague, who opened the meeting with a programmatic lecture on the meaning of the "current physical theories" for epistemology. Richard von Mises (1883–1953) spoke on the causality principle and its statistical interpretation; the causality principle had been called into question by quantum physics. Frank and von Mises hoped for broad acceptance among the assembled physicists for the "scientific philosophy" of the Vienna Circle, which they sought to develop further with reference to the new discoveries of modern physics as the legacy of the positivism represented by Ernst Mach (1838–1916).[119]

116 Chroniken der Gesellschaft der Zwanglosen, 1924–1931, here, p. 152. BSB, manuscripts, Cgm. 8026(13a.

117 To the University of Tübingen, June 11, 1929. DMA, NL 89, 030, folder Gutachten.

118 From Debye, June 21, 1929. DMA, NL 89, 007. ". . .über die Abwendung der starken Gefahr" involves an untranslatable pun on Stark's name. "Stark" means "strong" in English.

119 Stöltzner/Uebel, *Wiener Kreis*, 2006, pp. X–XV.

But in the latest discoveries of physics, Sommerfeld saw no reason to revive the Mach positivism. Although in introducing quantum mechanics Heisenberg had directed his attention to observable magnitudes, this was not the essential distinction to the pre-quantum theories. "The error of the older quantum theory," Sommerfeld argued in his lecture, "was not the introduction of unobservable magnitudes, but excessive faith in classical mechanics. Wave mechanics, which so splendidly corrected this error, introduces unobservable magnitudes on a far greater scale than the old quantum theory." Nor did he regard the causality principle as being called into question by quantum mechanics; it needed only to be freed from the confining corset into which eighteenth century mechanics had forced it, and elaborated with respect to the principle of "finality." "The causality of the 20th century must not limit itself to the initial state, but must take the end-state into consideration as an equally determinative moment." This had already been made evident before quantum mechanics in the form of the spectroscopic combination principle, whereby the frequency of a spectral line is, after all, determined simultaneously by the difference of the respective energy values of initial and end states. The indeterminism presumably evoked by quantum mechanics was an inevitable consequence of the wave-particle dualism. Herein, Sommerfeld saw the true, philosophically meaningful discovery of the newer physics—and wave mechanics had found the appropriate formulation of it. To be sure, this dualism had not yet been reconciled. Sommerfeld did not believe that "this would be possible in the physical arena. . . More likely, perhaps, through some sort of philosophical synthesis." Perhaps, he concluded his lecture, it would 1 day be possible with a dualistic worldview to grasp "the infinitely more difficult, infinitely more delicate, but never to be evaded question of the collaboration of mind and body. Fortunately for our generation, much still remains to be done on the firmer ground of real physics."[120]

A few months later, speaking in Vienna, though Sommerfeld sounded a note of sympathy with the Mach positivism, he nonetheless carried wave-particle dualism into the field as the decisive argument against it: "According to the positivist conception, the dual nature of the electron, juxtaposed to the dual nature of light, means nothing less than the assignment of two different ways of describing related empirical facts. Is that all there is to be said, then? Is no remainder left over? Does not the conjecture suggest itself that this dualism in the area of physics is in some way related to the dualism that runs through our entire lives, the dualism of mind and matter, of I and not-I, of body and soul? . . . The scientific worldview of the Vienna Circle may be inclined to brush aside such questions as insubstantial. I do not believe the human spirit will be content with so dismissive a solution."[121]

120 Sommerfeld, *Bemerkungen*, 1929.
121 Sommerfeld, *Elektronentheorie*, 1930.

In a lecture in April 1930 in Würzburg, "On Clarity in Modern Physics," Sommerfeld again stressed the wave-particle dualism as the true challenge for the modern understanding of the natural world. Quantum theory did not permit precise predictions about the magnitudes coupled in the Heisenberg uncertainty relation; it has demonstrated an "insurmountable limit, beyond which the exact space-time description becomes illusory." This uncertainty concerns only our mental images, not the physical facts that can be determined experimentally. "Philosophy," he said, alluding to recent discussions with the Vienna Circle, "will cautiously follow after, and will ultimately, having overcome temporary difficulties, only gain thereby." The Würzburg lecture was published in the *Unterrichtsblätter für Mathematik und Naturwissenschaften*, the organ of high school teachers in Germany, and gained wide international readership as well through a reprint in *Scientia*.[122]

In this lecture, Sommerfeld had not identified the philosophy of the Vienna Circle by name. But it was clear that, with his reference to "mental images," and the call for a finalistically elaborated causality principle, he was distancing himself from Mach's positivism and its adherents. When Moritz Schlick (1882–1936), active since 1922 as successor to Ernst Mach at the University of Vienna, entered into this discussion, Sommerfeld expressed his pleasure "over the tolerant attitude and willingness to understand" with which the representatives of the Vienna Circle had received his critique. Although ultimately the Vienna Circle also saw the necessity of an accommodation of philosophy to the discoveries of modern physics, Sommerfeld believed the facts of physics tended to reinforce his conception. "I am not a dogmatist in the religious sense, but I am a dogmatist when it comes to the laws of nature. I cannot abide the Mach 'principle of the untidy laws of nature,' the Uncertainty Principle notwithstanding. Einstein rejects it, too. He once said to me: 'all physics is metaphysics.'"[123]

10.8 Quarrel with Stark

Sommerfeld's debate with representatives of the Vienna Circle related to the philosophical conclusions that had to be drawn from the discoveries of modern physics. His lectures at Prague, Vienna, and Würzburg, however, also provided the substance of a quarrel that only superficially concerned the questions thus raised. Johannes Stark seized the opportunity to give the veneer of scientific dispute to his resentment against Sommerfeld after the dashing of his Munich hopes. Stark had long led a campaign against modern atomic theory. He conceived of the atom as a structure rotating around an axis, out of which electromagnetic energy was ejected in form of "quantum eddies" and transformed into "light eddies." Initially, his

122 Sommerfeld, *Anschaulichkeit*, 1930.

123 To Schlick, October 17, 1932. DMA, NL 89, 025. Friedl/Rutte, *Moritz Schlick*, 2007, pp. 317–319.

polemic had been leveled against the Bohr-Sommerfeld atomic model. Now he set his sights on quantum mechanics. His attacks peaked in 1930 in a series of essays in the *Annalen der Physik*.[124] Caricaturing the statistical interpretation of the Schrödinger wave function Ψ, he wrote that one could "intellectually, of course, construct such a swarming about of electrons according to such a law," but this construction would be in conflict with experience. "Frenzied motion of the sort characterized above" had never been observed. "In order to establish the space-time behavior of the electron in its atomic field causally according to the Sommerfeld interpretation of the Ψ-function, one would have to depart the realm of physics, and postulate the electron's consciousness of the Schrödinger equation and its capacity to behave accordingly."[125]

Here, Stark had arrived at a point that seemed to him the cardinal sin of the whole quantum enterprise since Bohr: the violation of the causality principle. In an entire printed page, he quoted what Sommerfeld had argued in his Prague lecture on the necessity of elaborating the understanding of causality in order to set out clearly the irrationality of including both initial *and* end states in the quantum theoretical calculation of a spectral line: "The Sommerfeld construction of a new causality means not only the dissolution of the concept of causality as we have known it, but also the blurring of our concept of time. It arises from the effort to explain the dependence of the frequency on the end state, based on the conception that the radiation of the frequency during the transition from an initial state occurs after an end state. It has not been experimentally demonstrated, however, that this conception comports with physical reality. Nor has it been demonstrated that this is the only possible physical conception."[126]

Sommerfeld was accustomed to annoyance with Stark. In 1909, during the quarrel over interpretation of X-ray bremsstrahlung, the scientific aspect of the controversy was still uppermost. By 1921, when Sommerfeld defended the Bohr atomic model against Stark's attacks, hardly anyone took the experimental physicist seriously—his recent Nobel Prize in experimental physics notwithstanding—when he presumed to express an opinion in the area of theoretical physics. To a colleague, who also felt pilloried by Stark's latest attacks, Sommerfeld expressed his "sense of comradeship at having been jointly insulted" and made it clear that Stark's article "had been written more on personal than on substantive grounds." Thus, he did not intend "seriously to reply" to the article; he had, however, recently reacted to "several of Stark's objections" in a lecture given at Würzburg.[127] He was alluding to the lecture "On Clarity in Modern Physics," which actually had nothing to do with his quarrel with Stark and as Sommerfeld explained in a footnote to the printed version of the lecture also in the spoken version contained nothing of it. But in the

124 Kleinert, *Axialität*, 2002.
125 Stark, *Axialität*, 1930, p. 717.
126 Ibid., pp. 718–721.
127 To Ronald Fraser, May 13, 1930. DMA, NL 89, 001.

printed version, he could not forbear pointing "to obvious misconceptions in Stark's exposition."[128]

This incited Stark to a further attack. In the *Unterrichtsblätter für Mathematik und Naturwissenschaften*, he warned high school teachers against the "dogmatism" Sommerfeld was spreading with his theories.[129] Even in the *Annalen der Physik*, Stark's writing was now unmistakably polemical. Sommerfeld had spread the "theory of the swarming electron" even at the Deutsches Museum. There, "presumably with his collaboration," models had been displayed "which were supposed to represent the spherical-symmetrical form of the cloud of swarming electrons around an atomic center."[130] After the publication of this article, Gerlach, newly minted colleague of Sommerfeld, brought to the attention of Eduard Grüneisen (1877–1949), the responsible editor at the *Annalen der Physik*, that Stark was here less concerned with the substance of the matter than with a personal quarrel with Sommerfeld. "I see now," Grüneisen wrote Sommerfeld apologetically, "that I would have done better to have at least sent you the proofs to give you the opportunity of marking the passages you wished to see changed." He admitted that he did not judge Stark's views as negatively as Gerlach. "Not because I am sympathetic to them—quite the contrary—but because, despite his curious ideas and gross lack of tact (whose victim I myself once was) he is an important researcher, whose opinion in scientific matters readers of the *Annalen* are interested in hearing." That Stark often struck the wrong note was one thing; but that his expositions "sprang from un-objective motives, and were insulting" was not something he was prepared to grant. Stark had just "misunderstood a great deal." Therefore, he proposed that Sommerfeld "set forth to the readership of the *Annalen* from your viewpoint the problems around which Stark's argument revolves."[131]

"Why would you assume that Stark does anything on substantive grounds?" Sommerfeld replied to the editor of the *Annalen*. "His rage against me stems from the fact that the faculty rejected him as successor to Wien. First, he took the occasion of my 60th birthday to try to get chummy with me, and went so far as to trouble my wife in my absence with such an attempt. Then, when he became aware that he was not on the list, he wrote me a crude letter. Now he dumps his whole opposition to the development on my quite innocent head. Incidentally, his knowledge of this development is based solely on a single lecture of mine, not from the original sources of Heisenberg, etc." Sommerfeld did not blame Grüneisen for publishing Stark's article, although passages such as the "references to the Deutsches Museum" had no business being there. "But let's not be at swords' points over that! I know that an editor is a much harassed man." At all events, he was still indecisive whether he should take up Grüneisen's invitation to counter Stark's accusations

128 Sommerfeld, *Anschaulichkeit*, 1930a, p. 165.

129 Stark, *Dogmatismus*, 1930.

130 Stark, *Axialität*, 1930, p. 677.

131 From Grüneisen, October 4, 1930. DMA, NL 89, 024, folder, Starkiana.

with an article in the *Annalen der Physik*. "In any case, I will do it less than thoroughly; otherwise you will receive another dozen responses from Stark, and as a member of the board, I must seek to avoid that."[132]

In the end, though, Sommerfeld did send an article that filled three printed pages to the Annalen der Physik, "Rebuttal to the Attacks of Prof. J. Stark." He declined to dispute causality in modern physics, or wave-particle dualism, with Stark because "Prof. Stark, in his attacks, is ignorant not only of the general evolution of theory since 1926, but also of the experimental facts of electron diffraction, that were discovered in tandem with the theory." On the other hand, he made it perfectly clear where he stood with regard to Stark's exposition directed to chemists of the "axial structure" of the atom. "There is no doubt about the spherical symmetry of the charge distribution of filled shells or the ground states of hydrogen, the alkalis, the noble metals, etc., unless one is prepared to abandon entirely wave mechanics with its innumerable consequences, which are indispensable to experimentation." In the case of chemical bonding, the matter was very complicated, but what had heretofore been known about it was not in conflict with quantum mechanics. Quite the contrary: Here, for the explanation of polar bonding, for example, one required the quantum mechanical exchange interaction. For nonpolar bonding, the spin-concept was indispensable. No physical theory could presume to explain the wealth of chemical facts; it could "treat only simple, typical cases." Stark had on several occasions raised the chemistry of carbon bonding as an example. But Sommerfeld would not allow the still unresolved problems in this area to serve as an argument against modern physics, for the structure of such complex molecules belonged properly in the realm of chemistry. "It is not the role of physics to seek to replace or improve upon this work. It can, however, contribute to the basic elucidation of the valence concept, just as it was able to shed light on questions of atomic structure and the periodic table. Anyone denying this has simply been uninvolved in the modern development."[133]

Any settlement of this dispute was out of the question, and after this rebuttal Sommerfeld put out of his mind the "Starkiana," as he had labeled the folder containing the unpleasant evidence of this quarrel. Although his colleague Georg Joos (1894–1959) from the University of Jena thought some example of "Stark's nonsense" should be exposed "with relentless severity" so that it would be obvious even to "people at a greater distance" what to think of the statements of the Nobel laureate on modern physics,[134] Sommerfeld declined his advice. It was clear to the majority of his colleagues that in this quarrel, Stark had once more put himself in the wrong. "Heitler and I have read and discussed your 'Rebuttal,'" Born wrote Sommerfeld. "We both thought it splendid in both tone and substance: factually sharp and yet polite. It is very good that you publicly take Stark to task for not

132 To Grüneisen, October 9, 1930. DMA, NL 89, 024, folder, Starkiana.

133 Sommerfeld, *Erwiderung*, 1930.

134 From Joos, November 20, 1930. DMA, NL 89, 024, folder, Starkiana.

having read the original sources."[135] A letter arrived from Zürich from Aurel Stodola (1859–1942), a retired professor of engineering who had a lively interest in modern developments in physics even while mourning those bygone days when it was still possible to understand natural phenomena with classical mechanics. Stodola saw no profit in the "attacks of the hothead Stark." Obviously Stark was "barking up the wrong tree, and, with his 'light eddy,' which supposedly collides with the atomic ion, is merely yearning for an explanation according to the old mechanics of force and impact, whose time (sadly) is now past."[136]

10.9 On the Road Again

While the quarrel with Stark in the *Annalen der Physik*, in the *Unterrichtsblätter für Mathematik und Naturwissenschaften*, and in correspondence with numerous colleagues was still stirring passions, Sommerfeld went traveling once again. "I intend to go to Odessa to the Russian Physics Day," he notified Rubinowicz of a visit.[137] Lemberg (Lwów), now in Poland, where Rubinowicz was a Professor of Theoretical Physics at the Technical University, was on Sommerfeld's travel route. To be sure, this trip did not go off quite so comfortably as his recent world tour. The day he spent with Rubinowicz in Lemberg had been "by far the most pleasant of the whole trip," he wrote after his return from Odessa, "for in Soviet Russia, every comfort is gone. Nonetheless, the trip to the Black Sea was interesting and sunny, almost tropical."[138]

In October, 1930, Sommerfeld traveled to Brussels, where he had been invited to the sixth Solvay Congress. Perhaps in compensation for his not having been invited to the Congress in 1927, he was now accorded the fitting honor of delivering the opening lecture on the theme of the Congress: "magnetism."

Sommerfeld used the opportunity to address the old question of the "magneton" from the perspective of spectroscopy in light of the latest findings.[139] By way of preparation, he had chosen the topic of his special lecture course for the preceding summer semester, 1930 accordingly. "I haven't yet properly begun working on the Solvay report, but I am lecturing on the topic," he wrote Pauli, who was likewise to read a major paper at the upcoming Congress and was an important consultant especially on the subject of magnetism. In the same letter he mentioned almost in passing that he was just in the process of "building a small house."[140] The Sommerfelds completed the move from Leopoldstraße to number 6 Dunantstraße, their future address bordering the English Garden, just shortly before the Solvay Congress.[141]

135 From Born, November 13, 1930. DMA, NL 89, 024, folder Starkiana.
136 From Stodola, December 14, 1930. DMA, HS 1977-28/A,331.
137 To Rubinowicz, August 14, 1930.
138 To Rubinowicz, September 18, 1930.
139 Sommerfeld, *Magnetismus*, 1932.
140 To Pauli, June 24, 1930. DMA, NL 89, 003. Also in ASWB II.
141 To Vieweg, October 17, 1930. Wiesbaden, Vieweg-Archive, Sommerfeld.

Fig. 29: Sommerfeld and Auguste Piccard (1884–1962) at the Solvay Congress, 1930, in Brussels (Courtesy: Deutsches Museum, Munich, Archive).

Not long after the Solvay Congress, Sommerfeld went abroad once more. The *Institut Henri Poincaré* in Paris invited him to give talks in April 1931 on wave mechanics.[142] Unlike 1922, when on his trip to Spain he had experienced France still as enemy territory and had taken no pleasure in a stop in Paris along the way, this time he refused to let himself be governed by resentment. “I must revise my judgment that Paris is not a beautiful city,” he wrote home following an initial stroll through the city. Nor did he feel it an imposition to deliver his lectures in French. “I have apparently not committed any linguistic errors, and scarcely needed to glance at my lecture notes.”[143] Langevin, who at the Solvay Congresses before World War I had been very friendly to him, now too was particularly attentive. “Most elegant luncheon at his house,” he wrote about an invitation to Langevin’s. “Afterwards, climb up the Eiffel Tower accompanied by his son and son-in-law, boat-ride along the Seine, sight seeing in old sections of the city.” One evening, he was taken to the opera. To be sure, on this visit to Paris he felt there was not as much interest in his lectures as he had hoped. Not even Langevin attended his lectures. “That is typical. No one has the time.”[144]

142 Sommerfeld, *Problèmes*, 1931.
143 To Johanna, April 21, 1931.
144 To Johanna, April 25, 1931.

On his return from Paris, a 2-month stay in the USA was on Sommerfeld's travel schedule. "I would like very much to come to Ann Arbor in 1931," he had written Herzfeld in Baltimore already in July, 1929. Together with Epstein in Pasadena and Laporte in Ann Arbor, Herzfeld represented the Sommerfeld school in America.[145] Annually since 1923, summer courses had been offered in which in an informal atmosphere, advances in theoretical physics were discussed. The ambitious level of the lectures and discussions soon brought these courses into high repute and made Ann Arbor, even for European experts in theoretical physics, a desirable destination.[146] In the summer of 1931, Pauli and Kramers were, with Sommerfeld, among the distinguished guests from abroad. "I am convinced we are going to have a very nice time together," Sommerfeld wrote, delighted.[147] Since the event in Ann Arbor began as early as June, he had to absent himself from half of the Munich summer semester.[148] Millikan also sent him an invitation to a congress of the American Association for the Advancement of Science, scheduled for a week earlier in Los Angeles and Pasadena, but Sommerfeld passed up this detour to far-off California because for lack of time.[149]

When Sommerfeld boarded the "Columbus" in mid-June 1931 to make the voyage across the Atlantic, he gladly eased once again into the daily life of a comfortable ocean steamer. "Wake up at 7:15. First, ½ hour exercise, strenuous but necessary. Then a hot bath in seawater (in the tub)," he wrote his wife describing the daily routine on the crossing. "Then breakfast with a lot of good coffee, grapefruit, and emphatically declining all meat dishes. In good weather, shuffle-board on the sundeck, a very nice way to exercise, with an elegant young American woman and two American gentlemen. Lunch around 1:00, often with caviar as a starter." In the afternoons, he retired to his cabin. As reading matter for the journey he had brought a biography of Napoleon by Emil Ludwig (1881–1948) and *The Apple Cart* by George Bernard Shaw (1856–1950), "A Political Extravaganza," as the subtitle declared. "I did not enjoy the latter as much as I usually do Shaw. The former is exceptionally well written, interesting, and—so far as I can tell—credible." In the evenings, etiquette was in force. "Dinner at 7:30 at the Captain's table; naturally I have to get myself up in a tuxedo for that."[150]

Once arrived in Ann Arbor, the terrific heat put him in mind of his world tour. But the physicists of the University of Michigan went to great efforts to make their guests' life as comfortable as possible. In the company of Laporte, Pauli, and Walter Colby (1880–1970), he was taken "swimming in an isolated lake. Water, lukewarm.

145 To Herzfeld, July 25, 1929. DMA, NL 89, 002. Also in ASWB II.

146 Schweber, *Empiricist Temper*, 1986, pp. 78–79.

147 To Pauli, June 24, 1930. DMA, NL 89, 003. Also in ASWB II.

148 To the Philosophical Faculty of the University of Munich, January 14, 1931. UAM, E-II-N.

149 From Millikan, February 7, 1931. DMA, NL 89, 011; to Millikan, February 25, 1931. DMA, NL 89, 025.

150 To Johanna, June 19, 1931.

We swam until dark," he wrote, describing his first impressions. "Many fire-flies, larger than our homegrown and secretive glow-worms; they fly as high as the tops of the trees." He lodged in a fraternity house and was taken at every opportunity on excursions by car. He was driven about even close by the university. And even Pauli, whose social graces often left something to be desired, behaved "very nicely." The following day, Sommerfeld added a few lines to the letter. It was becoming "ever hotter," but in the evening they had enjoyed a "lovely swim in a lake." And of course he was again requested to play the piano. "Last evening I played two Beethoven sonatas with a (rather mediocre) violinist at the fraternity house."[151]

On this visit, too, he found the American lifestyle very much to his taste. "Everything is organized splendidly in this country: in instruction, financially, socially (dress is unbelievably casual for the men, so that I almost constantly run around in sandals and an Indian shirt). Everything geared towards having as good a time as possible, and accomplishing things with minimal effort and trouble—exactly the opposite of us!"[152] But as in the years 1923 and 1929, "this time, too, the pleasure of America [was] soured by politics," as he wrote home on July 12, 1931. He was again confronted with "once more alarming news about Germany" in the American newspapers: "extreme financial emergency, great French outrage, threat of resignation by Brüning and Hindenburg, continuing crisis despite Hoover plan." Three weeks earlier, the American president had proposed that German reparations payments to France and German war debts to the United States be suspended for 1 year. Sommerfeld thought such a moratorium was "tremendous," even if Hoover "had not acted out of friendship with Germany, but rather to rescue American capital in Germany and overseas business."[153] Added to his annoyance over politics in the larger picture came his concern over things at the University of Munich, where National Socialism was spreading like an epidemic. He read in the newspaper that two universities had been closed on account of National Socialist student unrest. "Was Munich one of them? What has happened in the election of the Rector?" he wanted to know from his wife. "I am really quite anxious about what is happening at home!"[154]

There had in fact been riots a few days earlier at the University of Munich. The trigger had been the lectures of the liberal constitutional law scholar Hans Nawiasky (1880–1961). On June 26, 1931, National Socialist students at the University of Munich had mounted an initial protest demonstration which gave the *Völkischer Beobachter* the pretext of further inciting the students over the "Nawiasky scandal." On June 30, the leader of the National Socialist German Student Alliance publicly attacked Nawiasky in a speech delivered in the atrium of the University of Munich. In the venue where normally academic speeches were delivered, the Horst-Wessel Song now rang out. The agitated students screamed "Heil Hitler," "Death to Jews,"

151 To Johanna, June 30, and July 1, 1931.
152 To Johanna, July 8, 1931.
153 To Johanna, July 12, 1931.
154 To Johanna, July 8, 1931.

and "Death to Nawiasky," in response to which the administration thought its only recourse was to order the police to clear the university and close it down for a week.[155]

But the American newspapers reported less about the situation at the University of Munich than about the foreign and economic political situation in Germany. "What is happening in Germany? That is the fearsome question I can't escape in conversation and in my solitary hours," Sommerfeld wrote his wife on July 24, 1931. "The economic devastation is clearly bad." As on his first trip to the USA, when growing inflation in Germany had worried him, now, too, he sent dollars and checks home in order to stave off economic distress at least domestically. The new house the Sommerfelds had occupied for the past year accounted for additional financial worries. "The question is: should I use my Ann Arbor earnings, which by the way will be paid only at the end, to pay house-related expenses, or should I leave a part of them in the U.S.?"[156] Mixed in with his private financial worries was anger over French politics which Sommerfeld blamed for the looming failure of the Hoover moratorium. "Bitterness over the French outrages is general, especially strong in the London papers, as an Englishman told me today, nor do the American papers offer any excuses for the French tactic of extortion, and the malicious stalling of the Hoover plan. Shame on *la grande nation*!"[157]

Because of worries over money and politics, his experiences at the summer school in Ann Arbor receded into the background—at least in personal letters to his wife. What he did report related to musical evenings with his hosts or the farewell party he and Pauli hosted for the professors and students "with their girls" towards the end of the summer school semester. For this occasion, they organized a "colloquium on hyperphysics," at which Sommerfeld "presided ceremonially," and "many comic lectures" were delivered. "Music followed, provided by a professional pianist (German-American), then dance. Served: ice cream and punch (nonalcoholic). A great success, general satisfaction."[158] This was surely not the only party Sommerfeld and Pauli threw in Ann Arbor, and no doubt not all these convivial events were quite so nonalcoholic. This is illustrated by a mishap that occurred just at the start of the summer school. Pauli, probably not entirely sober, sustained a complex shoulder break. Because of prohibition, the consumption of alcohol could not be openly acknowledged, but from Pauli's correspondence we learn that they did not have to suffer under excessive abstinence. The official version of Pauli's accident that was given out was that he had slipped and fallen at a swimming pool, but at the place in the correspondence relating this event, we find an exclamation mark. However it was that Pauli had sustained his injury, on top of it he had to endure the derision of his colleagues. He ran around with his arm extended in a cast "like a traffic cop signaling," one participant in the course wrote. Pauli himself

155 Behrendt, *Hans Nawiasky*, 2006.
156 To Johanna, July 24, 1931.
157 To Johanna, July 30, 1931.
158 To Johanna, August 19,1931.

seemed to take pleasure in the general amusement at his appearance and later added with a dose of self-irony that this was the only time he ever extended his arm in a Hitler salute.[159]

For all the informality, Sommerfeld's lectures and the discussions about the current problems of theoretical physics did sap his energy somewhat, so that ultimately he longed for "the tranquility of the ocean voyage" on his return.[160] As on his earlier trips, he had devoted his courses at Ann Arbor to his favorite topics of the theoretical physics of those years, electron theory of metals and wave mechanics.[161] Such rapid strides were being made in these areas that he could not rely on the lectures he had worked out earlier. Walter Brattain (1902–1987), at that time still at the dawn of his career, later to be awarded the Nobel Prize in physics as co-inventor of the transistor, recalled many years later how impressed he had been by Sommerfeld's lectures, which dealt directly with the area (thermionic emission) in which he was working at the time at Bell Laboratories. "Several of us had interesting discussions with him on some of the current problems of thermionic and field emissions, of which the theoretical interpretation was still in doubt."[162]

10.10 Consolidation of the New Theories

The thermionic and field emission of electrons, to which the inventor of the transistor referred, belongs to the electron theory of metals, which Sommerfeld, using the Fermi-Dirac statistics on the free electron gas, had established in 1927 as a promising subsection of theoretical solid state physics. In the early 1930s, too, electron theory of metals was still a focus of research at Sommerfeld's institute. Sommerfeld himself left the working out of details mostly to his students and confined himself to publishing the results in the role of coauthor. For example, he gave the work on the thermoelectric and magnetic properties of metals over to Nathaniel Frank (1903–1984), a physicist from the Massachusetts Institute of Technology who had come to Munich in 1929 on a grant from the National Research Council (NRC) in order to bring his theory into line with the latest results of research in this area.[163] To William Allis (1901–1999) and Philip Morse (1903–1999), who in 1930 had likewise come as NRC Fellows to Munich, he gave over the working out of a wave mechanical theory of scattering of slow electrons on

159 Cited in WPWB 2, p. 84.

160 To Johanna, August 19, 1931.

161 Symposium on Theoretical Physics and Courses in Physics. Summer Session, 1931, June 29 to August 21. University of Michigan Official Publication, Vol. XXXII, Nr. 54, April 4, 1931, p. 9.

162 Brattain to Goudsmit, December 15, 1955. Quoted in Schweber, *Empiricist Temper*, 1986, p. 78.

163 From Frank, January 28, 1929; to Frank, February 8, 1929. DMA, NL 89, 022, folder 9,35; to Frank, November 27, 1930. DMA, NL 89, 001; from Frank, December 15, 1930. DMA, HS 1977-28/A,101. Sommerfeld/Frank, *Statistical Theory*, 1931.

gas atoms and declined immortalizing himself as coauthor in the publication of the theory. Here was a case of explaining a phenomenon entirely incomprehensible in the absence of wave mechanics: the "Ramsauer Effect," discovered 10 years earlier. Experiments had shown that the weakening of an electron beam passing through a gas was not to be brought into line with the classical conception of particle collisions. At very low energies, the cross-section drops below the value that would have been obtained according to the gas kinetic theory, as though the gas atoms became more permeable for slow electrons than for high-velocity electrons. "The fundamental idea and impetus for this work," Allis and Morse wrote at the end of their theory published in the *Zeitschrift für Physik* "comes from Professor Sommerfeld."[164] He was content to make the results worked out at his institute public in a lecture to the Berlin Physical Society.[165]

In these years, the need for a consolidation of quantum mechanics, particularly in its significance for the solid state theory, was discernible in manifold ways. Editors of compilations and handbooks kept turning to Sommerfeld in hopes of persuading him to undertake a survey of the newer physical theories. In the fall of 1929, for example, the editor of the *Handbuch der Radiologie* wrote Sommerfeld from the Leipziger Akademische Verlagsgesellschaft, asking for "a concise presentation of the conductivity of metals from the viewpoint of quantum theory." Sommerfeld declined since at the time he was occupied with reworking *Atomic Structure and Spectral Lines* for the fifth edition, which appeared in 1931. But he proposed that the editor turn to one of his students. "Mr. Peierls is in Zürich. Perhaps he would not be disinclined to undertake the conductivity of metals for you. Bethe is with me in Munich. I would not personally encourage him to take on this work since he already has enough to do. You are of course free to make him the offer."[166]

Rudolf Peierls had begun his studies with Sommerfeld at Munich, completing them with Heisenberg at Leipzig. Thereafter, he became Pauli's assistant at Zürich. "Dr. Peierls works on the theory of heat conduction in solid bodies," as Pauli characterized his research area in 1929.[167] Following his doctorate under Sommerfeld, Bethe was also something of an academic vagabond, and quantum mechanical solid state theory was the research area in which he, too, made his name. As long as they occupied no secure professorships, they had to support their candidacies for openings with publications that were as innovative as possible. Comprehensive surveys, such as the editor of the *Handbuch der Radiologie* sought, were time-consuming and, as a rule, offered little space for the presentation of original research. Thus, they represented rather an obstacle to the pursuit of their personal careers. Sommerfeld counseled his protégés therefore against taking up such offers from the scientific publishers, though they could certainly be lucrative.

164 Allis/Morse, *Theorie der Streuung*, 1931.
165 Sommerfeld, *Theorie des Ramsauer-Effektes*, 1931.
166 To Marx, October 19, 1929. DMA, NL 89, 003.
167 From Pauli, May 16, 1929. DMA, HS 1977-28/A,254. Also in ASWB II.

In 1931, when Springer Verlag instituted a search for authors for two constituent volumes of the *Handbuch der Physik* on quantum theory and solid state physics, Sommerfeld was once more the first person they consulted. Bethe had already published several papers 3 years after his dissertation that qualified him for a pending professorship, so that Sommerfeld had no qualms about arranging for him to prepare an article for the quantum theory Handbook volume. Adolf Smekal (1895–1959) wrote gratefully to Sommerfeld in April 1931 that he was most pleased by this arrangement with Bethe, who had already accepted. Smekal, who had taken over the editorship of these volumes of the Handbook for Springer Verlag, added a request for one further article. In the volume planned for solid state physics, electron theory of metals and the theory of ferromagnetism were to be consolidated into a "quantum theory of the metallic state." Smekal courted Sommerfeld as the actual founder of this area: "There could be no greater contribution to the profession and to the *Handbuch der Physik* than if you could see your way clear to making a definitive presentation of your work and the research related to it."[168] Sommerfeld, however, did not want to take on the burden of this work alone. Bethe appeared to him to be the most suitable author for this task, although he had already passed on to him the article for the quantum volume. He would—he stipulated in forwarding Smekal's letter to Bethe—"accept [the offer] only if you take on 90% of both the work and the honorarium. Article to be signed . . . by A. Sommerfeld and H. Bethe." He "absolutely did not" wish to persuade him to take this on, and even cautioned him against "too much scribbling."[169]

Bethe was on a Rockefeller grant in Rome working with Fermi when this offer reached him. "In and of itself, this would of course be very attractive to me," he wrote thanking Sommerfeld, "but like you, I am afraid I am loading myself up with too much 'scribbling.'" He wanted to devote himself entirely to research during his stay in Rome. On the other hand, both the subject "and the quite substantial honorarium" seemed thoroughly attractive. He could only take on the assignment—he decided, after weighing the pros and cons—if he were permitted to deliver the article, not as Smekal had wished by January 1, 1932, but by, "say, April,'32."[170] Smekal accepted this condition "so entirely" that Bethe—as he wrote Sommerfeld several weeks later from Capri—saw himself "honor-bound, as it were," to take on this Handbook article, too.[171] The expectation of completing two Handbook articles in a year proved illusory, however. On the agreed upon date of submission, "only one chapter of the 1st Handbook article [was] finished," as Bethe confessed to Sommerfeld in April, 1932. Smekal granted Bethe an extension until August 1, 1932, but even this period proved insufficient.[172] It often became apparent only in

168 From Smekal, April 17, 1931. DMA, NL 89, 013. Also in ASWB II.
169 To Bethe, April 18, 1931. DMA, NL 89, 013. Also in ASWB II.
170 From Bethe, April 25, 1931. DMA, HS 1977-28/A,19. Also in ASWB II.
171 From Bethe, May 30, 1931. DMA, HS 1977-28/A,19. Also in ASWB II.
172 From Bethe, April 20, 1932. DMA, HS 1977-28/A,19. Also in ASWB II.

the act of writing that one or another aspect needed to be more closely researched before it could be cast in the form of definitive textbook knowledge for the Handbook. The "scribbling" demanded many months more of intensive and not merely authorial work. In the end, though, all concerned could be satisfied with the result. Bethe's article on the "quantum mechanics of the one- and two-electron problems" in the first part of volume 24 of the *Handbuch der Physik*, published in 1933, had the scope of an entire book and became a classic of modern physics.[173] It served as a model for many subsequent textbooks on quantum mechanics. The same is true of the article on the "electron theory of metals" in the second volume. The agreed upon listing order of the authors ("A. Sommerfeld and H. Bethe") was retained, although Sommerfeld contributed only the 36 page introductory chapter with his semiclassical electron gas theory, while Bethe in 254 pages presented the quantum mechanical theory of the behavior of electrons in rigid bodies.[174] The theory had thereby acquired a "definitive presentation," a grateful Smekal wrote Sommerfeld in November, 1933. "It is a quite signal honor for the other contributors to this volume of the Handbook to appear in your company."[175]

The *Handbuch der Physik* was the most celebrated, but not the only medium driving the consolidation of quantum mechanics, and the burgeoning theoretical atomic, molecular, and solid state physics forward. The *Handbuch der Radiologie* of the Akademische Verlagsanstalt, the *Müller-Pouillet* textbook series of the Vieweg Verlag, and others kept this trend in view. As in Bethe's case, one or another adherent of the Sommerfeld school was recruited for such survey articles.[176] Sommerfeld also often assigned his doctoral students of those years topics intended to underscore the importance of modern theory for a broad range of physical phenomena through the application of quantum mechanics to problems of solid state physics. Herbert Fröhlich (1905–1991) was, for example, to handle the photo effect on metals. In the case of a single atom, the emission of an electron resulting from the irradiation of light could be "very naturally and easily described" with wave mechanics—thus Sommerfeld began his report on Fröhlich's dissertation—but in the case of electrons of metal, a corresponding treatment presented "quite substantial difficulties."[177] Fröhlich remained committed to theoretical solid state physics and contributed to its dissemination and consolidation.[178]

173 Bethe, Quantenmechanik, 1933.

174 Sommerfeld/Bethe, *Elektronentheorie der Metalle*, 1933.

175 From Smekal, November 28, 1933. DMA, NL 89, 013.

176 Peierls, *Elektronentheorie*, 1932; Nordheim, *Statistische und kinetische Theorie*, 1934; Nordheim, *Quantentheorie*, 1934.

177 Vote on the dissertation of Herbert Fröhlich to the Philosophical Faculty, 2nd section, July 22, 1930. UAM, OC-Np-1930. Fröhlich, *Photoeffekt*, 1930.

178 Fröhlich, *Elektronentheorie*, 1936.

Two other dissertations completed in 1931 at Sommerfeld's institute dealt in addition to the consolidation of wave mechanics with questions in the area of the "Pacific problem."[179] Otto Scherzer (1909–1982) was to treat the scattering of protons pursuing the method Sommerfeld had developed for the braking of electrons and to explain why in the experiments conducted heretofore no proton bremsstrahlung had been observed.[180] August Wilhelm Maue (1908–1970) was to work out how the earlier solution for the X-ray bremsstrahlung found by Kramers according to the correspondence principle differed from the wave mechanical. Since Kramer's theory had been adduced for astrophysical problems, Sommerfeld hoped that with the theory employed by Maue heretofore inexplicable inconsistencies between theory and observation in astrophysics could be cleared up.[181]

Sommerfeld demonstrated with these papers that his Institute remained a very productive "nursery" of modern theoretical physics even more than 30 years after its founding. Theoretical solid state physics got its decisive boost in the early 1930s, and many of the pioneering publications originated in the Sommerfeld school.[182] Following the discovery of the neutron in 1932, nuclear physics also blossomed into a new subfield of physics, and here, too, quantum mechanics was the key to theoretical understanding. The university physics institutes in Germany could not keep pace with the explosion of knowledge in theoretical physics, so that even prominent theoreticians like Bethe faced a bottleneck in openings for professorships. Nonetheless, Sommerfeld's Institute and its "branches" in Stuttgart (Ewald), Hamburg (Lenz), Leipzig (Heisenberg), and Zürich (Pauli, Wentzel) remained for a few years still productive venues of the new physics.[183] "Still"—because in 1933 with the "seizure of power" of the National Socialists came decisive changes, which brought about a slow and painful end to the Munich "nursery," and had as a consequence the decline of modern theoretical physics in Germany altogether.

179 Sommerfeld, *Beugung*, 1931.

180 Vote on the dissertation of Otto Scherzer to the Philosophical Faculty, 2. Sektion, November 27, 1931. UAM, OC-Np-1931/32. Scherzer, *Ausstrahlung*, 1932.

181 Vote on the dissertation of August Wilhelm Maue to the Philosophical Faculty, 2. Sektion, November 27, 1931. UAM, OC-Np-1931/32. Maue, *Röntgenspektrum*, 1932.

182 Hoddeson/Baym/Eckert, *Development*, 1987; Eckert, *Sommerfeld*, 1990.

183 Eckert, *Atomphysiker*, 1993, Chaps. 6 and 7.

14 Legacy

The posthumous recognition of scholars has a long history. The Paris Academy of Sciences traditionally honored its deceased members with eulogies, establishing thereby a ritual adopted subsequently by virtually every scientific society. Although both the hero worship of antiquity and the hagiography of the Middle Ages live on in this tradition, these eulogies, biographical memorials, commemorative speeches, and obituaries of academics are valuable sources for the history of science, for they reveal what the immediately following generation of an esteemed scientist regards as his most significant contributions. When the work of a scholar continues to be influential years after his death, hagiographical presentation makes way for another form of recognition. His textbooks are revised by his students and published in ever newer editions. Publication of his collected writings facilitates access to his work by new generations of scientists. Congresses at 10-year anniversaries serve to illuminate one or another pioneering accomplishment in light of developments that followed it. In few cases does interest in a scholar spread past this phase and reach circles outside his own sphere of influence. This does happen in the case of exceptional figures such as Einstein and Bohr, who achieved world renown even during their lifetimes, as well as of those whose influence spreads beyond their particular fields to other disciplines. Finally, there is yet another sort of scientific afterlife, when a law, a formula, or a natural phenomenon has been named for the scientist who postulated or discovered it.

Sommerfeld's continuing presence offers striking examples in each of these categories. From the obituaries of the year 1951 to contemporary concepts labeled with Sommerfeld's name, we see reflected quite various facets of the manner in which theoretical physicists deal with the history of their field.

14.1 Obituaries

Personal reminiscences dominated the first responses to the news of Sommerfeld's death. "I still remember so well how, when he stayed with us back then, Professor Sommerfeld began working on his book very early in the morning," wrote a physicist from the Philips research laboratory in Holland to Sommerfeld's widow. "As grateful as you must be to your late husband for having shared such a long life, we students are equally grateful to him for his textbooks and leadership and for the great pleasure reading his publications has always given."[1] A member of "The Casual

1 From Niessen, May 7, 1951. DMA, NL 89, 017, folder 2.6.

M. Eckert, *Arnold Sommerfeld: Science, Life and Turbulent Times 1868-1951*,
DOI 10.1007/978-1-4614-7461-6_14, © Springer Science+Business Media New York 2013

Ones" recalled in his condolence letter to Johanna that Sommerfeld had been a "true Casual One. . . He always had a large audience, and much that later found its way in print to publication was probably first uttered aloud to us." He had been "ever ready to lend a hand," and "quite a number of us have been the recipients of his good counsel, and many of his assistance, too. The loyalty of this man was a support, that bound the society closer together, all the more so as Arnold Sommerfeld also gladly gave himself up to high spirits and humor."[2]

Not everyone held Sommerfeld's memory in high esteem, however. "With his death, the terroristic tyranny of dissonant physics may well have lost the most powerful exponent it still possessed," Hugo Dingler wrote in a letter to the like-minded Bruno Thüring on the news of Sommerfeld's death.[3] Even 6 years after the fall of the "Third Reich," the war against modern theoretical physics had still not come to an end for these fanatics.

In the Sommerfeld obituaries, there was no hint of such enmity. Rather, they enumerated the lasting contributions he had made to his science, and they revealed in addition something of the individual relationship of the obituary writer to Sommerfeld. Pauli admired about Sommerfeld that in one person, he had "felicitously" embodied "the epitome of the scholar and the teacher."[4] For Heisenberg, Sommerfeld was the fatherly teacher who always showed sympathy for the problems and needs of his students and "considered the personal lives of the students with friendly interest, with the cheerful calm of the Munich professor who gladly eases tensions with a joking word, or readily overlooks inadequacies."[5]

The same tenor ran through the obituaries written by other Sommerfeld students. Each according to his personal experience adduced still other aspects. "Sommerfeld's success as a teacher was due to the clear and concrete expression of his ideas," Ewald explained to the readers of *Nature*. Even as a beginning student, one understood in Sommerfeld's lectures "that behind the domain of established theory lies a field of unsolved problems."[6] For Bechert, too, the secret of Sommerfeld's success lay "in his manner of teaching, of setting assignments, and in his willingness to support those working around him in their individuality." As longtime assistant to Sommerfeld, Bechert could also contribute a few items with respect to daily life in the Sommerfeld pedagogical enterprise. He cited Sommerfeld's concept of the large course that "Lectures should not be constructed and so smoothly delivered that the listener thinks he has understood everything. There must always be something left over which he needs to ponder." For advanced students and doctoral candidates the special lectures and the seminar had been most important. "Often, he gave the small lectures on a subject he wanted to get to know

2 From Anton Weiher, August 3, 1951. DMA, NL 89, 017, folder 2.7.

3 Dingler to Thüring, May 8, 1951. Aschaffenburg, Hofbibliothek, Dinglerestate.

4 Pauli, *Arnold Sommerfeld*, 1951.

5 Heisenberg, *Arnold Sommerfeld*, 1951.

6 Ewald, *Arnold Sommerfeld*, 1951.

himself, and not a few of his own papers grew out of such lectures." In the seminar, Sommerfeld had often interrupted the speakers and posed a question when something was not clear to him. "In discussions, he exhibited model generosity, and was open to any opinions opposing his; in scientific matters, only factual correctness interested him. In discussions, whether he had been right or wrong was not worth a second thought."[7]

In his obituary, Laue laid greatest stress on Sommerfeld's research but also honored Sommerfeld as teacher. As a student of Felix Klein, Sommerfeld had received superb preparation for his pedagogy. Klein's art of lecturing, "the art of representation in general," as well as "the art of knowing human beings and of knowing how to treat human beings" had been passed on "by the great teacher" to Sommerfeld. Laue also compared Sommerfeld to Planck and Einstein, whom he had known well for many years as Berlin colleagues. They had both concentrated "particularly on the fundamental principles of physics"; by contrast, "the model, or at least the concrete instance," had been Sommerfeld's focus. "In Planck, Einstein, and Sommerfeld we have representatives of two generally appearing, quite different scholar types."[8] Herewith, Laue was the first to formulate that dichotomy between orientation towards principle and orientation towards problem, in terms of which Sommerfeld's work later was often characterized (see Chap. 15).

Most obituaries filled only a few pages. With his ten printed pages, Laue had offered what was already a very detailed representation. Max Born, however, composed the most comprehensive tribute. As a member of the London Royal Society, Sommerfeld merited an entry in the *Obituary Notices* of this learned society, and Born, as no other scientist in Great Britain, was equipped to pay him this honor. It must have cost him some effort to meet the membership's high expectations, though, for the long tradition of obituary writing for the Royal Society set a lofty standard. It required a scrupulous listing of all scientific publications and a more comprehensive presentation of the scientific importance of the deceased than was usual in such obituaries.[9]

14.2 Leading Figure for the History of Physics

Ten years after Sommerfeld's death, the second phase of his legacy commenced. American physicists and historians of physics planned a trail-blazing project for the history of recent science: "Sources for History of Quantum Physics (SHQP)." The terrain was first to be explored with some "sample biographies." "The first, that of Arnold Sommerfeld, indicates what can be learned about a prominent physicist for whom numerous obituary notices have been written," the collaborators on the

7 Bechert, *Arnold Sommerfeld*, 1951.

8 Laue, *Sommerfelds Lebenswerk*, 1951, pp. 514 and 518.

9 Born, *Sommerfeld*, 1952.

project wrote in justifying the choice of Sommerfeld as the leading figure. The comprehensive obituary Born had published in *Obituary Notices* of the Royal Society furnished valuable grounds for the process going forward. The project's central focus was the collection of sources, and here, too, Sommerfeld proved a leading figure. Fritz Bopp put Sommerfeld's papers and other effects still in the possession of the institute for theoretical physics of the University of Munich at the disposal of the project. Ernst Sommerfeld contributed a portion of the correspondence he had found in his father's house. The papers thus conveyed were recorded on microfilm and, thereafter, returned to Germany. Together with materials from other estates, an extensive microfilm archive was thus assembled forming the basis of research into the history of modern quantum physics.[10]

With the SHQP Project, the modern history of physics was on the way to becoming a discipline within the history of science, committed to historical methodology and oriented towards primary sources. The works created during the 1960s within this project, under the leadership of Thomas Kuhn (1922–1996), in which Sommerfeld's part in the emergence of modern atomic and quantum physics was explored, belong to the milestones of the modern history of physics.[11] In the wake of the American collection of sources, research began in Germany into the history of quantum theory, and here too, Sommerfeld moved to the center of the research of historians of physics.[12]

The budding interest in Sommerfeld's contributions to quantum physics coincided with the wish of many physicists to observe Sommerfeld's 100th birthday in 1968 in a fitting manner. Bopp established a committee which prepared a four-volume edition of Sommerfeld's works commissioned by the Bavarian Academy of Sciences.[13] In September 1968, he organized a "double congress" at the University of Munich: an "Arnold Sommerfeld Centennial Memorial Meeting" and an "International Symposium on the Physics of the One- and Two-Electron Atoms."[14] Nor did Sommerfeld's textbooks lose their currency. "In deference to Prof. A. Sommerfeld's wish expressed shortly before his death, I have gladly assumed the task of working up Volume VI of his lectures for the present new edition," Fritz Sauer wrote in 1957 in the Introduction to the fourth edition of this volume. The fifth edition followed in 1961 and the sixth in 1965. In 1978 the Verlag Harri Deutsch took over the two volumes of *Atomic Structure and Spectral Lines* and the six volumes of Sommerfeld's *Lectures on Theoretical Physics*. Reprints of Volumes II and VI appeared as late as 1992.[15]

10 Kuhn/Heilbron/Forman/Allen, *Sources*, 1967. http://www.amphilsoc.org/guides/ahqp/index.htm (31January 2013).
11 Heilbron, *History*, 1964; Forman, *Environment*, 1967.
12 Hermann, *Diskussion*, 1967; Hermann, *Frühgeschichte*, 1969.
13 Sauter, ASGS, 1968.
14 Bopp/Kleinpoppen, *Physics*, 1969.
15 Sommerfeld, *Atombau*, 1992.

Given the extensive SHQP material, which includes numerous interviews with Sommerfeld's students, it was also possible to approach Sommerfeld biographically more intensively.[16] The contributions of Sommerfeld and his students to atomic theory now formed the subject of critical studies in the history of physics.[17] Sommerfeld's papers in the area of electron theory of metals also stirred great interest in the context of an international project begun in 1980 on the history of solid state physics. Like the SHQP Project, it took as its principal task the gathering and archiving of relevant sources.[18]

With these research initiatives, new source material came to light. Sommerfeld's estate proved so rich in material that it suggested an exhibition of selected documents to introduce broader circles to this scholarly life.[19] Sommerfeld and his "school" were also suited to the representation of the social context of theoretical physics in the first half of the twentieth century and the rise of this field to a science of the century.[20]

Herewith, a new phase of Sommerfeld's legacy opened up. His scientific correspondence proved a veritable treasure trove for the historical reconstruction of physical theories, giving impetus to extensive treatment.[21] Like the editions of the correspondence of Einstein, Bohr, and Pauli, the Sommerfeld correspondence also shows that modern theoretical physics is not the outcome merely of wrestling with ideas. It is, like other sciences, marked by social developments and historical trends, not discernible in the study of scientific publications, but which leave clear traces throughout the correspondence. The recent history of physics offers countless examples, also (not to say particularly) with respect to Sommerfeld.[22]

14.3 The Fine-Structure Constant

In the names wedding concepts in the natural sciences to their discoverers in near timeless eminence, we see a quite different form of legacy. The "Boltzmann constant," the "Planck quantum of action," and other constants and effects named for their discoverers immortalize their fame in the collective memory of physics. Apart from how Sommerfeld might otherwise be remembered, the "Sommerfeld fine-structure constant α" alone assures his name a lasting place in physics textbooks.

16 Benz, *Arnold Sommerfeld*, 1975; Forman/Hermann, *Sommerfeld*, 1975.

17 Heilbron, *Kossel-Sommerfeld Theory*, 1967; Nisio, *Formation*, 1973; Forman, *Doublet Riddle*, 1968; Forman, *Alfred Landé*, 1970; Cassidy, *Core Model*, 1979; Kragh, *Structure*, 1985.

18 Hoddeson/Baym/Eckert, *Development*, 1987; Eckert, *Propaganda*, 1987; Eckert, *Sommerfeld*, 1990.

19 Eckert/Pricha/Schubert/Torkar, *Geheimrat*, 1984.

20 Eckert, *Atomphysiker*, 1993.

21 Eckert/Märker, ASWB, 2000 and 2004.

22 Seth, *Crafting*, 2010.

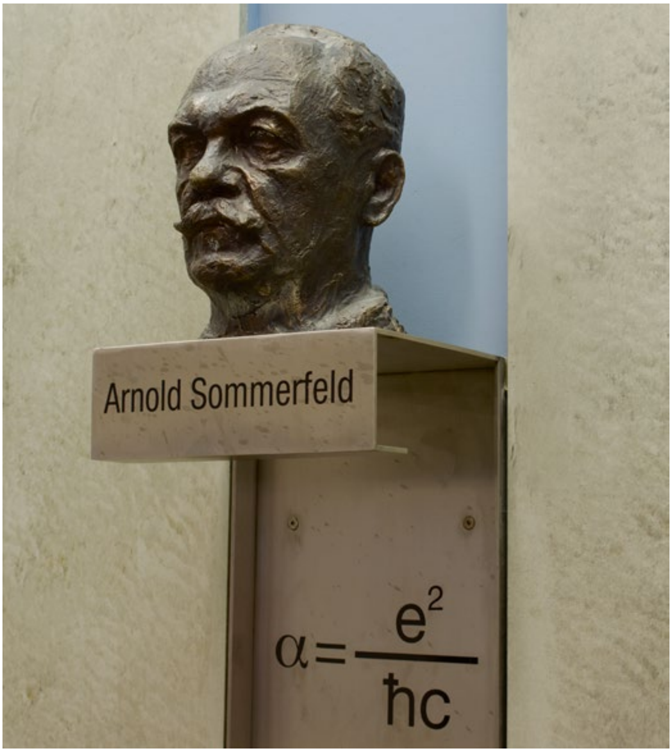

Fig. 35: With this bust outside the "Arnold Sommerfeld Lecture Hall," the physics faculty of the Ludwig Maximilian University in Munich memorializes the fine-structure constant, whose introduction in 1916 is seen as Sommerfeld's greatest achievement (Courtesy: Deutsches Museum, Munich, Archive).

At first, α was simply an abbreviation for a quantity assembled from other natural constants, similar to the "Bohr radius" or the "Bohr magneton." By contrast to these quantities, which can be thought of as an elementary length or an elementary magnetic moment, α did not correspond to any elementary physical unit, since α is dimensionless, a number whose value lies very close to 1/137. In 1916, in his fine-structure theory, Sommerfeld had introduced this number as the relation of the "relativistic boundary moment" $p_0 = e^2 / c$ of the electron in the hydrogen atom to the first of n "quantum moments" $p_n = nh / 2\pi$. Sommerfeld had argued that $\alpha = p_0 / p_1 = 2\pi e^2 / hc$ would "play an important role in all succeeding formulas," he had argued.[23] In 1916, he had gone no further than to suggest that more fundamental physical questions might be tied to this "relational quantity." In *Atomic Structure and Spectral Lines*, α was given a somewhat clearer interpretation as the relation of the orbital speed of an electron "in the first Bohr orbit" of the hydrogen atom, to the speed of light.[24]

23 Sommerfeld, *Quantentheorie*, 1916, p. 51.
24 Sommerfeld, *Atombau*, 1919, p. 244.

Not until the development of quantum electrodynamics did the deeper meaning of the fine-structure constant emerge.[25] The correspondence between Heisenberg and Pauli in the 1930s bears witness to the intense—though unsuccessful—efforts in this area. A "true understanding of the numeric value of your constant," Heisenberg wrote in a letter to Sommerfeld, "lies far in the future still—I have come no further with it."[26] With the Lamb-shift experiment (Sect. 13.3), the fine-structure constant moved once again to the center of efforts at theoretical interpretation. The miniscule shift of the energy level was based "on the smallness of the so-called fine structure constant," wrote Pauli in his article on Sommerfeld's eightieth birthday. "The theoretical interpretation of your numerical value is one of the most important still unsolved problems of atomic physics."[27]

With quantum electrodynamics, theorizing about the elementary processes in physics moved in a new direction.[28] The electrodynamic interaction was thought to be a process in which light quanta were exchanged between electrically charged particles, where the fine-structure constant was recognized as a measure of the force of this interaction. For each of the fundamental natural forces—in addition to the electrical (or magnetic) force in electrodynamics, gravity, and the weak and strong nuclear forces—there is in quantum field theory a characteristic exchange particle and a coupling constant which express the force of the interaction. In the fine-structure constant, the magnitude of the electrical elementary charge presented the primary riddle. The universal nature of the elementary charge was mirrored in a mysterious way in the fine-structure constant and extended to the entire domain of electromagnetic interaction. "It is not only the coupling of the electrons with the light quanta that is determined by the fine structure constant, but the coupling of any arbitrary elementary particle with the electromagnetic radiation field," Heisenberg wrote in 1968 in his article on the occasion of the Sommerfeld Centennial. "So long as one did not understand that all elementary particles have charges that are integer multiples of the elementary charge, one could really not hope to derive the Sommerfeld constant. So an understanding, at least a qualitative understanding of the entire spectrum of elementary particles, was the precondition. I spoke with Sommerfeld about this in the years following the last war, too."[29] The fine-structure constant became the great riddle of the physics of elementary particles.

For the theoreticians, though, Sommerfeld's constant was not just a great puzzle so far as the ultimate bases of physics were concerned but also a stroke of luck that gave wings to their practical work. It also appeared as a coupling constant of quantum electrodynamics in the calculation of various interactions. By means of an elegant graphic technique (the Feynman diagram), the integrals derived—thanks

25 Kragh, *Magic Number*, 2003.

26 From Heisenberg, June 14, 1935. DMA, HS 1977-28/A,136. Also in ASWB II.

27 Pauli, *Beiträge*, 1948, p. 132.

28 Schweber, *QED*, Kapitel 2.

29 Heisenberg, *Ausstrahlung*, 1968, p. 536.

to the smallness of α—could be calculated by approximation. With respect to the agreement of theory and experiment, quantum electrodynamics belongs to the most exact theories in physics. It is no coincidence that Richard Feynman (1918–1988), awarded the Nobel Prize for his contributions in this area, like Heisenberg and Pauli, sang an encomium to the fine-structure constant. "It is one of the greatest mysteries of physics," he reasoned at the end of his book on quantum electrodynamics, "a magical number that exceeds the human grasp, as though written by the 'hand of God.'"[30]

The puzzle took a new turn at the end of the twentieth century when astrophysicists found in the spectra of quasars not the value measured on earth of 1/137.03599976 but 1/137.037. The difference may seem tiny, but in light of the otherwise exact correspondence of theory to experiment, it gives theoreticians pause. Light from the quasars was emitted billions of years ago. Can it be—some theoretical physicists ask themselves—that the natural constants are not at all constant but change their value over the course of time? As bizarre as this conjecture based on a discrepancy so infinitesimal might appear, the question of the immutability of the natural constants remains unanswered—and solidly on the test bench of new experimentation and theories.[31]

14.4 The "Sommerfeld Puzzle"

The fine-structure theory of 1916 pertained to an electron orbiting elliptically around an atomic nucleus. Twelve years later, the same fine-structure formula for the energy level of hydrogen-like atoms was derived from Dirac's theory of the electron, "with negligible alterations of terms," as Sommerfeld wrote in the *Wave Mechanical Supplement*[32]:

$$E = E_0 \left\{ 1 + \frac{(Z\alpha)^2}{\left(n - k + \sqrt{k^2 - (Z\alpha)^2}\right)^2} \right\}^{-\frac{1}{2}}$$

This formula in the *Handbook of Physics* from the year 1933 already incorporates those "negligible alterations."[33] For the difference of the quantum numbers $n - k$, the Sommerfeld fine-structure formula used the radial quantum number n_r and, in place of k, the azimuthal n_φ. E_0 is the stationary energy of the electron, Z the nuclear charge number, and α the fine-structure constant. How could such different

30 Feynman, *QED*, 1990, p. 148.
31 Fritzsch, *Konstanten*, 2003.
32 Sommerfeld, *Ergänzungsband*, 1929, p. 337.
33 Bethe, *Quantenmechanik*, 1933, p. 316.

theories yield the same fine-structure formula? In the Dirac electron theory, elliptical orbits were not taken into account. The very concept of orbit makes no sense at all in this theory. With the concept of "spin" a new degree of freedom made its appearance to which nothing corresponded in Sommerfeld's conception. Was it ultimately just a lucky coincidence that the two theories came to the same result?

For Sommerfeld, this was not a coincidental agreement. Spin was, after all, "according to the Dirac theory, a consequence of the relativistic wave equation. This yielded my fine structure formula exactly," he wrote retrospectively in 1942, outlining the basis of the agreement. But this was no explanation, only an indication of his conviction that the two very different theories had their common roots in relativity theory. "It is amazing that Sommerfeld's original 1916 formula for the energy levels can be derived from this new theory which takes account of electron spin," Pauli wrote in 1948 in his essay on the occasion of Sommerfeld's eightieth birthday.[34] Twenty years later, in his contribution for his teacher's centennial, Heisenberg also expressed amazement over it. "But as it were miraculously, Sommerfeld's formula, calculated for a spherically symmetrical electron on the basis of the old, inadequate quantum theory, has also proved itself as the exact solution of the quantum mechanical relativistic theory of a spinning electron. It would be a stimulating project to explore whether this is truly a miracle, or whether perhaps the group theoretical structure of the problem underlying the formulations of both Sommerfeld and Dirac itself leads already to this formula."[35]

Curiously enough, neither Sommerfeld nor Pauli nor Heisenberg ever took up this "stimulating project." Other prominent theoreticians, on the other hand, have time and again puzzled over it. "Sommerfeld's derivation of the fine-structure formula provides only fortuitously the result demanded by experiment," wrote Schrödinger in 1956 to the authors of a book on mathematical procedures in quantum theory. The analysis presented therein led to the conclusion that this was a case of a quite special kind of coincidence. In the Dirac theory, one arrives at the fine-structure formula by means of wave mechanics and spin; neither of these played a role in Sommerfeld's work. Had Sommerfeld employed only the relativistic wave mechanics (as Schrödinger at first had attempted), he would have arrived at a different formula. "Sommerfeld's explanation was successful," the authors concluded, "because the neglect of wave mechanics and the neglect of spin happen to cancel each other in the case of the hydrogen atom."[36]

This judgment, however, was refuted several years later by Lawrence C. Biedenharn (1922–1996), who had made a name for himself in particular with the application of group theoretical methods in physics. For him, the "Sommerfeld Puzzle" had nothing to do with a chance agreement, but rather—as Heisenberg had

34 Pauli, *Beiträge*, 1948, p. 131.

35 Heisenberg, *Ausstrahlung*, 1968, p. 534.

36 Yourgrau/Mandelstam, *Variational Principles*, 1968, S. 113–115.

surmised—with a deeper symmetry. The differential equations used to describe the fine structure in all their variety exhibit a mathematical structure that leads to the same result in both theories, so far as the meaning of the magnitudes appearing in them is concerned. "It is this symmetry which produces the most remarkable and detailed correspondence between the Sommerfeld procedure and the quantal solution, as discussed at length above in our resolution of the Sommerfeld puzzle."[37]

However, that had still not solved the puzzle of Sommerfeld's fine-structure formula. Or more precisely, other explanations emerged when the problem was approached "semiclassically." Semiclassical quantization permitted approximative treatment of problems that were unsolvable with normal quantum mechanics. With the emergence of chaos theory in the 1970s, it attracted particular interest as the boundary between quantum mechanics and chaotic systems in classical mechanics ("quantum chaos") began to be explored. Semiclassical quantization consists in generalization of the Bohr-Sommerfeld quantization conditions of the type $\oint pdq = nh$ by the addition of another quantum number ("Maslov-index" μ), so that the quantization condition takes the form $\oint pdq = \left(n + \frac{\mu}{4}\right)h$. In order to derive the fine-structure formula by the semiclassical method, yet another quantum number for the spin must be added to the quantization formula. It thus appears that these additions exactly cancel each other out.[38] It remains an open question, however, whether this result corresponds to Biedenharn's analysis or whether simply the identical substance is being described in two different mathematical languages.

In the "Sommerfeld Puzzle," an essential and perennially astonishing characteristic of theoretical physics is manifested. Previous results are occasionally confirmed by new theories, even though the physical understanding and its related mathematical procedures have fundamentally altered. Other papers by Sommerfeld experienced a late rebirth in this way. In nonlinear dynamics, for example, phenomena arising from feedback in the energy exchange between vibrating systems are designated as "Sommerfeld effect" or "Sommerfeld-Kononenko effect."[39] That Sommerfeld's name should attach to such phenomena goes back to his paper from the year 1902 in which he analyzed the vibrations caused by a motor driving an unbalanced weight (Sect. 5.3). The "rocking table" phenomenon showed manifestly that under given conditions, energy transmitted to the motor resulted not in higher revolutions but in stronger vibrations of the table. Relating this phenomenon to the real world Sommerfeld wrote, "This experiment corresponds roughly to the case in which a factory owner has a machine set on a poor foundation running at 30 horsepower. He achieves an effective level of just 1/3, however, because only 10 horsepower are doing useful work, while 20 horsepower are transferred to the

37 Biedenharn, *Sommerfeld Puzzle*, 1983, p. 32.

38 Keppeler, *Phase*, 2004.

39 Kononenko, *Vibrating Systems*, 1969; Krasnopolskaya /Shvets, *Chaos*, 1993.

foundational masonry."[40] Its practical significance assured that serious consideration was given to the theoretical analysis as well. Decades later, when the study of nonlinear systems underwent a meteoric rise, the "rocking table" achieved new fame. The physical processes it occasioned, along with their mathematical description, bore implications far beyond the case of the "rocking table" described by Sommerfeld—had it not, the phenomenon would hardly have been given his name and dubbed the "Sommerfeld effect."[41]

14.5 From the Pacific Problem to Dark Matter

Quite a different Sommerfeld effect provides for discussion among astrophysicists. Considerably more matter must be present in the universe than what makes up the stars and cosmic gas clouds and is visible to astronomers by electromagnetic radiation through telescopes and radio telescopes. The prevailing view among astrophysicists is that so-called weakly interacting massive particles (WIMPs) constitute this invisible "dark matter." According to the theory, already a nanosecond after the big bang, these weakly interacting massive particles should have ceased interacting with each other, except gravitationally, if they are far apart from each other. If they happen to collide, they should annihilate each other by the weak interaction. In the process, they ought to emit a γ-ray, observable in principle, thus serving indirectly as evidence of dark matter. The radiation predicted by the first model calculations is spread so thin, however, that proof of it is practically impossible. Later model calculations, however, showed that in certain regions of the galaxy the radiation engendered by the annihilation of WIMPs is far greater than expected. In a research report on the subject from the year 2009, we read that "The unambiguous detection of Galactic dark matter annihilation would unravel one of the most outstanding puzzles in particle physics and cosmology. Recent observations have motivated models in which the annihilation rate is boosted by the Sommerfeld effect, a non-perturbative enhancement arising from a long range attractive force."[42]

How does it happen that more than half a century after his death, Sommerfeld has the distinction of having discovered an effect that might lead to the proof of dark matter? In the "Sommerfeld enhancement," as this effect is also known, two processes act together: In the one, the intensity of the radiation is proportional to the particle stream of the colliding WIMPs; in the other, it is dependent on the cross section of its effect. The latter can be visualized as a disc laid crosswise to the motion of the colliding particles, whose extension gives the distance at which

40 Sommerfeld, *Beiträge*, 1902, p. 393.
41 Eckert, *Sommerfeld-Effekt*, 1996.
42 KuhlenMadau/Silk, *Dark Matter*, 2009.

the particles still act on each other. Since the WIMPs attract each other before their mutual annihilation, the cross section of the effect as well as the particle stream is boosted since they become too concentrated.[43] In general, the calculation of such particle interactions is possible only with the perturbation theoretical methods of quantum field theory developed after World War II. But in his Pacific problem, Sommerfeld had considered the nonrelativistic limit in which the methods of the Schrödinger wave mechanics suffice. In his paper "On the Diffraction and Braking of the Electrons" from the year 1931, he had carried out this procedure for the calculation of the X-ray bremsstrahlung.[44] The interaction of two mutually attracting particles of small kinetic energy treated therein is exactly such a "nonrelativistic quantum effect," as is also supposed to occur in the colliding of the WIMPs and their mutual annihilation in certain regions of the galaxy. Sommerfeld was dealing with the braking of an electron flying by an atomic nucleus that is entailed in the emission of X-ray bremsstrahlung. In the case of dark matter, one has to imagine that the WIMPs react with each other similarly when they slowly (nonrelativistically) collide. Their interaction occurs—in the language of Feynman diagrams—in the form of a ladder diagram, in which the rungs of the ladder express that the mutual attractive force of the WIMPs is mediated by exchange particles (vector bosons), which because of their slow (nonrelativistic) mutual motion are exchanged many times between the WIMPs, before they mutually annihilate one another, and radiate their energy in the form of γ-rays.[45]

When Sommerfeld put his work on the radiation of X-ray bremsstrahlung to paper in 1931, there was no talk yet of Feynman diagrams or exchange particles. Nonetheless, Sommerfeld already saw himself confronted with a long problem history. In 1909, he had described for the first time how at the braking of an electron at the anticathode of an X-ray tube electromagnetic radiation was emitted that was bundled, depending on the velocity of the impacting electron, more or less in the direction of radiation of the electron. In 1911, at the first Solvay Congress, he had undertaken the attempt to determine the braking duration in this process quantum theoretically—and soon had to concede that the problem could not be handled in that way (Sect. 6.5). He encountered the subject again in 1929 in discussions with Yoshikatsu Sugiura, who devoted himself at RIKEN to X-ray bremsstrahlung. On the crossing from Japan to California, he tackled the problem by means of wave mechanics, without completely coming to grips with it (Sect. 10.6). The Pacific problem became a perennial challenge for him. Even with his comprehensive treatment in 1931 he saw the problem as not yet solved. He passed it along as a challenge to several of his students and devoted a comprehensive presentation to the subject in 1939 in the new edition of the *Wave Mechanical Supplement* (Sect. 11.5). It is no

43 Iengo, *Sommerfeld Enhancement*, 2009.
44 Sommerfeld, *Beugung*, 1931.
45 Lattanzi/Silk, *WIMP Annihilation*, 2009.

coincidence that even in the translation of the "Pacific problem" into the quantum field theoretical language of Feynman diagrams, traces of the Sommerfeld tradition are still perceptible.[46] Concealed behind the "Sommerfeld effect" in modern astrophysics and particle physics, then, is a problem history that reaches back through an entire century.

14.6 The Nobel Prize Denied

Since 1900, the highest honor bestowing on scientists a durable afterlife far above his colleagues has been the Nobel Prize. It was denied Sommerfeld. What he himself thought about this, he expressed in a heartfelt statement in December 1928, when once more he saw his hopes dashed. "It is gradually becoming a public scandal that I have still not received the Prize," he wrote at that time.[47] There is no final clarity as to the conjecture expressed in this letter that he had been passed over due to "rivalry with Bohr." All that emerges from documents in the Nobel archive is that from 1917, he had been nominated for the prize virtually every year, and in 1924 was on the short list. Why even that year he did not receive the prize must be inferred from the report of his Swedish colleague Carl Wilhelm Oseen (1879–1944), who spoke for physics within the Nobel Committee. "Given Sommerfeld's strong—and conscious—disinclination to systematic thought, it is natural that his achievements are often ephemeral," it reads.[48]

With each award of the Nobel Prize, a choice among many prize-worthy research achievements is faced that almost always raises the question whether this or another person is not more deserving of the Prize. The nuclear physicist Valentine Telegdi (1922–2006), who had played a decisive part in the discovery of parity violation in the weak interaction and other significant nuclear-physical discoveries and, thereby, himself had nearly won a Nobel Prize, on several occasions took up the question why Sommerfeld had never received it. He subjected the report in which Oseen in 1924 had classified Sommerfeld's elaboration of the Bohr atomic theory as insufficient for a Nobel Prize to critical analysis. To the charge that Sommerfeld's theory had been insufficiently systematic and soon revised in many details, Telegdi countered that Bohr's trilogy had likewise presented "an altogether not very logical edifice" and yet had been deemed worthy of the Prize. Oseen had "disparaged"

46 Elwert/Haug, *Calculation*, 1969.

47 To Wieland, December 13, 1928.DMA, NL 57. Also in ASWB II.

48 Protokollvid Kungl. Vetenskapsakademiens Sammankomster för behandling af ärenden rörande Nobelstiftelsen år 1924, here p. 29. Nobel Archives, Royal Swedish Academy of Sciences, Stockholm.

Sommerfeld, he wrote, and thereby sought "to deflect definitively all suspicion that he had been influenced by Bohr."[49]

Oseen was friends with Bohr, but to infer a conspiracy against Sommerfeld therefrom is pure speculation. The judgments in the realm of physics expressed by Oseen in the Nobel Committee caused annoyance also in other cases, where the idea of Bohr's having exerted influence can hardly be posited.[50] Whatever the forces at work in the background, it was incomprehensible already for Sommerfeld's contemporaries that this recognition was denied him. Millikan had proposed Sommerfeld for the Prize in 1925 and 1930.[51] He thought that *Atomic Structure and Spectral Lines* alone justified this honor. "It is outstanding work which should have brought you the Nobel Prize long ago," he wrote Sommerfeld in 1948 on the occasion of his 80th birthday.[52]

When an examination of the Nobel files many years after Sommerfeld's death brought to light how often he had been nominated for the Prize, historians of science were also astonished. In a survey of the first 50 years of the awarding of the Nobel, Sommerfeld is recognized as the holder of an unhappy record: Of all candidates for the physics Prize, he had received the greatest number of nominations. "Arnold Sommerfeld must be the unluckiest man in physics," the survey's author notes, for with 81 nominations between 1917 and 1950, he had "the dubious honor of being the most-nominated physicist in the period 1901–1950, never to win a Nobel Prize."[53]

49 Telegdi: "Why did Arnold Sommerfeld never get the Nobel prize?" lecture on March 7, 2002 to the Physics Colloquium at CalTech, http://www.pma.caltech.edu/PhysColl/PhysColl01-02.html (31 January 2013). I am grateful to Valentine Telegdi for access to the text of a parallel colloquium lecture at the University of Munich. See also, Lippincott, *Conversation*, 2008, p. 106.

50 Friedman, *Politics*, 2001.

51 Crawford, *Nobel Population*, 2002.

52 From Millikan, November 26, 1948. DMA, NL 89, 042. Also in ASWB II.

53 Crawford, *Nobel Population*, 2001.

15 Epilogue

"Planck was the authority, Einstein the genius, and Sommerfeld the teacher." Thus, one writer at once trenchantly and succinctly summed up the roles of the most important exponents of theoretical physics in its "golden age."[1] Another writer stressed the orientation towards *problems* that typified the theoretical physics of Sommerfeld and his school, in contrast to the physics of a Planck, an Einstein, or a Bohr, whose physics was focused on *principles*.[2] And Sommerfeld's biography does offer many instances of this approach. "How this comes about remains utterly obscure. But the consequences of what is postulated have to be thought through."[3] Thus, in 1927 had Sommerfeld deferred all foundational questions regarding the Fermi-Dirac statistics so that, unencumbered by them, he could attack a string of unsolved problems in the electron theory of metals with this new statistics (Sect. 9.3). One might well preface numerous other works by Sommerfeld with the identical dictum.

Nonetheless, the essence of a long and many-faceted life in science cannot be condensed in such simple formulas. "Sommerfeld, the teacher," spotlights only one aspect of his personality; similarly, the rubric "problem-oriented research" must not be understood in an exclusionary sense. As in the case of the *h*-hypothesis (Sect. 6.5), Sommerfeld was capable of exhibiting a quite pronounced orientation towards principle. When a problem is closely bound up with the principles fundamental to its formulation, principle orientation and problem orientation are not mutually exclusive alternatives.

In any case, with his often lapidary formulations, Sommerfeld himself contributed to this characterization of him as representing a decided antithesis to his "principle-oriented" colleagues. "I can only contribute to the technical aspect of quantum theory; you must devise its philosophy," he once had written to Einstein enunciating his position.[4] One should not infer from this a lack of interest in the philosophical and epistemological issues arising from the relativity and quantum theories, however. When challenged with contentious questions in natural philosophy, he adopted a clear-enough stance. That he should have declared himself decidedly a "dogmatist on the point of natural laws"[5] becomes very understandable in the context of his earlier debate with exponents of the Vienna Circle, in which he had issued a clear rejection of the Mach-inspired positivism (Sect. 10.7). He had expressed himself similarly on May 1, 1933, in a lecture in Edinburgh at the invitation

1 Hermann, *Max Planck*, 1973, p. 56.

2 Seth, *Crafting*, 2010.

3 Sommerfeld, *Elektronentheorie der Metalle*, 1927, p. 825.

4 To Einstein, January 11, 1922. AEA, Einstein. Also in ASWB II.

5 To Moritz Schlick, October 17, 1932. DMA, NL 89, 025.

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of the Royal Society.[6] His subject had been taken from a recently published book of speeches and lectures by Max Planck under the title "Paths to Physical Knowledge." Sommerfeld knew himself in agreement with Planck on many questions of natural philosophy. He referred also to Boltzmann, who, in the second volume of his *Electrodynamics*, had introduced Maxwell's equations with a familiar quotation from Goethe's *Faust*: "Was it a god who inscribed these symbols?" Belief in the harmony of the laws of nature was as deeply held a fundamental conviction for Sommerfeld as it was for Boltzmann, Planck, and Einstein. His philosophical statements may appear modest juxtaposed with those of Planck and Einstein, but they were nonetheless deeply felt (Sect. 13.7). Of all philosophical movements, he doubtless felt closest to the epistemology laid down by Kant, although he thought it in need of some revision in light of the general theory of relativity. "Certainly, it cannot remain in its original formulation," he wrote in 1948 about the Kantian conception that space and time are given "a priori." "Space and time acquire a physical structure a posteriori, stemming from the events playing out within them . . . A Kant of today would adjust his concepts to the doctrines of Einstein . . . Since Einstein, there is no longer any estrangement between physicists and philosophers. Physicists have become philosophers, while philosophers are careful not to conflict with physics."[7]

If in his scientific papers Sommerfeld broadly eschewed philosophical and ideological subjects, one should not judge this self-imposed reticence as a lack of interest in philosophy. But even the incorporation of philosophical convictions would not suffice to bring his activity as teacher and researcher to a common denominator. In the effort to compile a resume, music in particular must not be left out. His love of music formed a constant thread throughout his life, from his domestic private life to the convivial gatherings in the circle of his colleagues and, ultimately, to science itself. For him, atomic theory represented not just the challenge of solving problems and of thereby enhancing the careers in theoretical physics for a circle of his students. It was also (and perhaps primarily) "something aesthetic and harmonious that can be compared only to music." The number relations in the theory of spectral lines were "true quantum music," as he expressed it in 1924 in a lecture to the Prussian Academy of Sciences.[8]

Ultimately, all attempts to encapsulate Sommerfeld's life's work one way or another in a concise resume will fall short. One might rather recall a couplet from his favorite poet, Goethe:

"To find refreshment in the whole,
Seek the whole in the infinitesimal."[9]

6 Scott Lecture, May 1, 1933. Lecture text, Ms. in DMA, NL 89, 021, folder 9.9.

7 Sommerfeld, *Philosophie*, 1948, p. 98.

8 Sommerfeld, *Erforschung*, 1924, p. 875. Also in ASGS IV, p. 576.

9 "Willst du dich am Ganzen erquicken/So musst du das Ganze im Kleinsten erblicken."Goethe, Gedichte, 1827. *Werke*, 1981, p. 304 (I).

Abbreviations

AEA	Albert Einstein Archives, The Hebrew University of Jerusalem
AHQP	Archive for the History of Quantum Physics
AIP	American Institute of Physics, College Park, Maryland, USA
ASGS	Arnold Sommerfeld. Gesammelte Schriften. 4 Vols. Edited by F. Sauter and Bayerische Akademie der Wissenschaften. Braunschweig 1968
ASWB	Arnold Sommerfeld. Wissenschaftlicher Briefwechsel. Vol. I: 1892–1918; Vol. II: 1919–1951. Edited by Michael Eckert and Karl Märker. München, Berlin, Diepholz 2000 and 2004
BA	Bundesarchiv, Berlin
BA-MA	Bundesarchiv-Militärarchiv, Freiburg
BANL	Biblioteca dell'Accademia Nazionale dei Lincei e Corsiana, Rome
BayHStA	Bayerisches Hauptstaatsarchiv, München
BDM	Bund deutscher Mädchen
BSB	Bayerische Staatsbibliothek, München
CalTech	California Institute of Technology, Pasadena, California, USA
DMA	Deutsches Museum, Archiv, München
ESPC	Ecole superieure de physique et de chimie industrielles de la ville de Paris, Centre de ressources historiques, Paris
ETH	Eidgenössische Technische Hochschule, Zürich
Gestapo	Geheime Staatspolizei
GOAR	Göttingen, Archiv des Deutschen Zentrums für Luft- und Raumfahrt
GSA	Geheimes Staatsarchiv, Berlin
HJ	Hitlerjugend
KWKW	Kaiser-Wilhelm-Stiftung für Kriegstechnische Wissenschaft
MPGA	Max-Planck-Gesellschaft, Archiv, Berlin
NBA	Niels Bohr Archiv, Kopenhagen
NBCW	Niels Bohr Collected Works. 12 Vols. Amsterdam, New York, Oxford 1972–2006

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NSDAP	Nationalsozialistische Deutsche Arbeiterpartei
NSFK	Nationalsozialistisches Fliegerkorps
NVK	Nachrichtenmittel-Versuchskommando
Pg	Parteigenosse (member of NSDAP)
RANH	Rijksarchief in Noord-Holland in Haarlem
Ransom	Harry Ransom Humanities Research Center, The University of Texas at Austin
RIKEN	Rikagaku Kenkyūjo (Physical-Chemical Institute) in Tokio
SBPK	Staatsbibliothek zu Berlin – Preußischer Kulturbesitz, Handschriftenabteilung
SD	Sicherheitsdienst des Reichsführers der SS
SHQP	Sources for the History of Quantum Physics
SPGK	Schriften der physikalisch-ökonomischen Gesellschaft zu Königsberg in Preußen
SS	Schutzstaffel der NSDAP
StAM	Staatsarchiv München
SUB	Staats- und Universitätsbibliothek, Niedersachsen, Göttingen
UAB	Universitätsarchiv der Humboldt-Universität Berlin
UAG	Universitätsarchiv, Göttingen
UAM	Universitätsarchiv der Ludwig-Maximilians-Universität, München
WIMPs	Weakly Interacting Massive Particles
WPWB	Wolfgang Pauli: Wissenschaftlicher Briefwechsel, 4 Vols. Edited by Karl von Meyenn. New York, Berlin 1979–2005

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